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JERRY RAVETZ

## **Economics as an elite folk science: the suppression of uncertainty**

In this essay I offer a new approach to the analysis of the reasons why mainstream economics has survived and indeed flourished in recent decades in spite of its many trenchant critics. I use the concept “elite folk science” to explain how a discipline can have functions other than those of the increase of positive knowledge, or the improvement of practice. Such other functions can be in the ideological sphere, providing reassurance for a general world view and also a justification and ostensible guidance for practice. When these other functions become dominant, then the credibility of the discipline can persist for some time in spite of the absence of any confirmations of its pretensions to scientific status. This condition is most clearly manifested in the way that the discipline manages uncertainty; although this theme has largely been ignored in the official philosophy of science (to the detriment of all those who have wanted their own disciplines to be “scientific”), it provides a good symptom of the degree to which the discipline relates to an independent external reality rather than to its ideological functions. Mainstream academic economics has, I argue, flourished in recent decades largely as such an elite folk science; and I offer some suggestions of how its sudden change to that distorted condition occurred. But that condition may be temporary, and a discussion of the relation of elite with popular folk sciences can indicate possible trends in the future.

### **“Elite folk science”**

The term “elite folk science” would seem to be an oxymoron; but its paradoxical appearance will help us to grasp the special character of its

The author is Director of the Research Methods Consultancy, Ltd., London. An earlier version of this paper was presented at the conference on Keynes, Knowledge and Uncertainty, held at Leeds in March 1993, and is to appear in its *Proceedings* (Dow and Hillard, 1994). The author is grateful to Roger Backhouse for his very useful criticisms of an earlier draft and for the materials he provided; and also to Hugh Miser for his assistance, including the essay by Parker.

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subject. The term “folk science” has already been used occasionally, as an indication of the social *location* of a science—namely, among the “folk” as distinct from the educated classes. In that case, its formal theoretical content is necessarily slight, and its reality testing is informal. For examples of contemporary popular folk sciences, we have practical crafts like cookery, needlework, fitness, beauty therapy, or DIY. Before the spread of mass education and professionalism, ordinary people were helped to get through life by a variety of popular folk sciences of this sort, notably in the medical field, provided by practitioners who were not too far removed from them socially. There is another sort of traditional folk science, overlapping with this practical sort, that had greater theoretical pretensions, and whose functions were correspondingly more sophisticated. Typical among these are those that in recent centuries have been relegated to the status of “superstitions,” such as astrology, numerology, and palmistry. These have to a great extent been displaced by folk sciences with a respectable, scientific pedigree, like popular psychology in its various branches including coping with children, sex, stress, and old age. There is, however, evidence that some of the older sciences of an enchanted world have been making a recovery as part of “post-sixties” culture.

These examples remind us that folk sciences need not be devoid of genuine content, either scientific or practical. Also, as the example of psychology shows, their function is not restricted to assistance in the performance of practical tasks. Equally important, their doctrines explain phenomena that are puzzling or threatening, and also provide assurance in various ways. This is partly accomplished by the very act of explanation, for thereby the phenomena themselves are tamed. Also, the explanation fits them into a pattern that includes a confirmation of the lawful and generally beneficent workings of the world as a whole. And the doctrine is also believed to provide guidance for some relevant body of practice. In such ways, these folk sciences provide *security*; and it is in respect of this function that I use the term. The “folk” here is the particular clientele, with its particular world view that needs to be buttressed by the body of learning in question. I first found this use of the term in one of Joseph Needham’s writings about ancient Chinese natural philosophy; it was in this “functionalist” fashion that he explained the persistence of doctrines that to us seem to have lacked any reliable applicability to the control of the natural world.

Such a provision of security is not an illegitimate function for a body of knowledge. Indeed, it is necessary that someone, somewhere, *be-*

believes in a subject as having an intrinsic value, related to their world view. This is essential for the preservation of the core of commitment and morale that are necessary for the maintenance of recruitment, support, and above all quality control on the work. (I discussed this topic at great length in my earlier book, *Scientific Knowledge and its Social Problems* [Ravetz, 1971]; and my views have more recently been corroborated by the Deming school of Total Quality Control in industry [Neave, 1990]). It may seem surprising that elites, particularly those that define themselves by their intellectual accomplishments, should be a “folk” in this sense of needing sciences for reassurance. After all, the replacement of religion by science, and hence of belief by facts, was explicitly intended to accomplish the removal of those false props to security. But in recent decades a great variety of critics have shown that our modern world view, and indeed its core in science, necessarily have many of the functions of traditional beliefs; and in that sense, *independently of their character and quality as sources of positive knowledge*, they function as folk sciences for our modern elites.

There are a variety of other functions for bodies of knowledge, including self-increase (through research), and application of various sorts; but in addition there are training, education, and edification for a broader public. There are also ideological functions, which I shall discuss more below; these relate directly to elite folk sciences. How a particular subject is developed in any given milieu can be considered as a design exercise for optimizing among all these possible functions; some or all of them will be present in some degree, and there will always be a tension among them. When one of those functions becomes dominant over the others, then it is correct to describe that science in that milieu by that term. Hence, when I speak of “elite folk science,” I refer to a learned discipline in which that particular function of security for its elite clientele is dominant; and in that case the dominant criteria of quality are defined by the folk science function rather than by others. The concept “folk science” enables us to avoid a false dichotomy between a science being simply “real” (or “matured” or “positive”), or being simply “ideological” (and therefore spurious). My point is that the shape of the science, and its dominant criteria of quality, will be determined by the attribute or attributes considered most important by those who govern the social activity of the discipline.

One sign of a discipline having become purely an elite folk science is that the criteria of quality corresponding to its other functions are weakened. To make the point by means of another example, if the

function of self-increase becomes dominant, research may excel but then teaching and popularization will tend to suffer, and the long-term health of the field may be affected. Such a phenomenon has been noticed in the case of the natural sciences in universities. Alternatively, if, say, the provision of jobs through patronage is paramount, then the criteria of quality of research will be adjusted accordingly. I am *not* arguing against the folk science aspect of learning, only observing that it is one element of a design. Such an analysis opens the question of how the design of any discipline is established at any time; and this is the subject of the history and sociology of learned disciplines. For American social science in general, such a study has been made by Ross (1991). Although she does not use the term folk science, her thesis is about just that; thus, "I believe that American social science owes its distinctive character to its involvement with the national ideology of American exceptionalism, the idea that America occupies an exceptional place in history, based on her republican government and economic opportunity"; and also, "The distinctive character of American social science has necessarily had a profound effect on social practice and social thought in the United States" (pp. xiv, xiii).

It could be that the term "folk science" should be replaced by "reassurance-science," or, perhaps for an American audience, "Linus-blanket science," after the immortal Peanuts character. Perhaps I have even retained the term "elite folk science" precisely because of the slight air of paradox generated by this less intuitively obvious interpretation of it: according to my theory, even the elites need their folk sciences. With this expansion of the concept to include "elite folk sciences," some obvious candidates come to mind, notably theology. But there are other well-attested examples, such as linguistics, which has been used for the resurrection or re-creation of old or neglected languages in the cause of re-creating or even creating a national or folk consciousness. Even musicology has served such a function, to say nothing of history and geography. In certain countries in the nineteenth century, history *in general* was such an elite folk science; hence the later attack on "historicism" led by Popper. A most significant elite folk science was classics, which provided the formation for the Platonic Guardians sent out from Jowett's Oxford to administer the British empire competently and justly. The rise and fall of lesser academic disciplines can be understood in terms of their varying fortunes as elite folk sciences. My own career in "history and philosophy of science" was made possible by a sense of worry about

the Bomb among leading scientists, leading to a supportive atmosphere in which resources flowed freely.

For a time, physics functioned as an elite folk science for the Pentagon; its “Project Jason” gathered leading physicists in conclave, in order to tap their collective wisdom. The underlying logic seemed to have been that anyone smart enough to have made the Bomb, would know how to lick the Vietcong. But that was only one manifestation of faith in mathematical-physical science as the means to victory; the conduct of the Vietnam War as if it were a Harvard Business School exercise—including such means as a reliance on patently spurious statistics of body counts—exemplifies the scientific and practical vacuity that can follow from the dominance of the folk science aspects of a technical or scientific field. It is clear that the functions of such elite folk sciences are different from those of the “popular” sort; their service is mainly (though not exclusively) on the ideological front, to elite social groups in their various endeavors and struggles. Even on the basis of these few examples, one could imagine a history of the elite folk sciences, explaining their rise and fall in terms of particular groups and their projects.

When I discussed this topic at length in my earlier book (Ravetz, 1971), one of my principal concerns was with an image of “Science” (modeled on natural science) as an elite folk science in the ideological tradition deriving from the Enlightenment. I drew some illustrative examples from economics, using mainly the writings of Ely Devons and Oskar Morgenstern, to show how the ideological functions dominated over the ostensibly scientific ones. These examples were noticed by some friendly readers, and perhaps by some unfriendly ones too; and so my views on this topic are known to be of long standing. I did then refer to economics as an “immature folk science” (p. 396, n. 38), thereby combining two separate ideas in a somewhat confusing fashion; but the proper development of those insights has waited until this present occasion. I hope that the idea has now been introduced and explained sufficiently for the purposes of the argument.

The relationship between elite and popular folk sciences is one of mutual influence and conditioning. An elite folk science may have an avowedly political function, serving to legitimate some cause around which the masses can be rallied (notable here are history, linguistics, and geography, to say nothing of religion). On the other hand, a successful elite folk science must also cohere with the presuppositions of a broader audience in some respects, lest it lose the resources and protection that it needs from its patrons among the elite and by extension

from society in general. Relations can be quite nuanced; thus in management there are several strands, including the “airport terminal” literature, which is practical, aphoristic, and frequently inspirational, alongside the academic teaching, which is strongly conditioned by the mathematical ideals of economics. Another important example is medicine, where the “complementary” tendency may be seen as the continuation of a popular folk tradition that is now being adopted by a section of the elite as part of their new, green cosmology. Through its history, economics has exemplified these various relationships. Its recent developments can be explained thereby, and its future prospects may be gauged in their terms.

### **Uncertainty and science**

The awareness of uncertainty in science has been increasing at a dramatic rate over the past decade. On one hand, we may interpret this as a welcome recognition of the new character of the problems of the global environment, in which it is now becoming recognized that our knowledge is frequently swamped by our ignorance. On the other hand, we may see it as the decline of an official image of certainty and exactness within science, which is as old as modern science itself. The possibility of a mathematical science with no bounds to its penetration of natural phenomena was proclaimed in the seventeenth century by Descartes and Galileo, and repeated for the nineteenth century by Laplace. Even the quantum-level uncertainty discovered by Heisenberg left much of this ideology untouched; that is why “chaos theory” has recently caused such a philosophical stir. This image of certainty has of course had great plausibility, but it has always been contradictory to the practice of scientific research and debate. Its dominance can be understood partly in terms of its convenience for teaching, but also as a result of the political functions of “Science” as an elite folk science. It was promoted as a competing paradigm of genuine knowledge by anticlerical forces in their long battle with the churches, and in that role had to pretend to total certainty.

In view of this long unchallenged image of the “exactness” of real science, it could be considered paradoxical that actual research in the quantitative natural sciences involves above all the control of *in*-exactnesses along with other forms of uncertainty. This is the lesson learned by any perceptive student in a good course in a matured experimental science. But this knowledge, which is largely intuitive and



craft-based, is something of a craft secret shared among skilled practitioners. Such an understanding makes no contribution to the elite folk science of "Science"; indeed, it undermines its pretensions to certainty, and so it gains no audience. Hardly any philosophers of science have recognized it, and those who have done so have been largely ignored (I count N.R. Campbell [1928] and also myself [Ravetz 1971] among those). Hence, those on the outside who have wanted or needed to imitate the natural sciences took the appearance of certainty and exactness as the reality, and were then launched on a path of illusion and futility. Any success they had could not be based on a deepening theoretical interaction with data representing an external reality, but occurred only because their functions as an elite folk science were effectively dominant over all the others.

These are fairly bold statements to make, partly because they involve a claim of a "false consciousness" among certain scientists as a social group. They might also be considered to have relativist implications, along the lines of some notorious recent anthropological critiques of science (Latour, 1987). That is not my intention; the successes of science in providing particular sorts of knowledge about the natural world, and particular sorts of power over it, are not in question. But there is no doubt that, until quite recently, the history of science was devoted to showing that scientists progressively kept on getting the right answers; and the philosophy of science, to showing how that would happen infallibly. Uncertainty and ignorance were, in these scholarly traditions, simply negatives whose only role was to be located and then vanquished. The lacunae in successful scientific theories (as in those of Harvey or Darwin), and the actual errors (as in Newton's theory of light and Lavoisier's theory of acid-formation through "oxygen") were generally suppressed in standard histories; and failed research programs, such as those of the Laplace school in Napoleonic France, were handled with tact. So it is small wonder that those possessed of "physics-envy" would take an idealized, indeed fantasized, image of science for their model. One such discipline was, notoriously, behavioristic psychology; and another, as Mirowski (1990) has argued, was early neoclassical economics.

In such circumstances of ignorance and delusion about the management of uncertainty in empirical science, it was only to be expected that large-scale vacuity, in natural science and technology as well as in social sciences, should sometimes occur. The criteria of quality that had previously been implemented as skills in a craft, partly tacit fashion for the maintenance of good work in matured scientific disciplines, were



only imperfectly transferred to newer fields. Lapses of quality control in novel and complex technologies such as civil nuclear power and space exploration led eventually to their downfall. Some very influential technologies were quite vacuous; “Star Wars” was an egregious example, but that could occur only because of the self-destructive contradictions of previous theories of nuclear strategy (Ravetz, 1990a); and these were related to a series of formal, pseudo-mathematical sciences of “decision” and “management.” In these circumstances, one can speak of a new and significant sort of pseudo-science of our times, one defined not by its cosmology (as astrology or numerology) but by its methodology. I call this a “GIGO-science,” referring to the American acronym, “Garbage In, Garbage Out” which defines the limits of possible improvement of input data by a computer program. I have defined such a science as one where uncertainties in inputs must be systematically suppressed, lest the outputs become indeterminate. Recently, Andrew Stirling (1993) has provided a simple test: If the precision of expression goes up as the accuracy of measurement goes down, the science is likely to be GIGO, or vacuously pseudo-quantified.

In recent years increasing attention has been paid to uncertainty and ignorance in science; indeed, there was a session at the meeting of the American Association for the Advancement of Science in 1993 devoted to this theme, at which I made a contribution (1994). With my colleague S.O. Funtowicz, I have attempted to clarify the question in a fashion useful for practice (1990). We have devised a notational system in which the three sorts of uncertainty affecting any quantitative statement are distinguished. We call these spread, assessment, and pedigree; they roughly correspond to precision, accuracy, and state of the art; and philosophically are at the technical, methodological, and epistemological levels. The system is named by the acronym NUSAP, the first two letters standing for numeral and unit, respectively. Distinctions such as these are familiar to modelers; they know about data uncertainties, which can be controlled to some extent, and also about parameter uncertainties within the model, but they are now becoming aware of “model uncertainties” relating to the open (and perhaps unanswerable) question of whether the model has any relation at all to the outside world.

### Uncertainty and economics

Others have commented on the need of mainstream economics to ignore theoretical uncertainty. Brian Arthur (1993) tells of his gradual disen-

chantment with the assumptions of equilibrium and “diminishing returns” (which supported the elite folk science that recommended a “hands-off” policy by governments). His theory of “increasing returns” yielded history-dependence, unpredictability, and a number of other real-world properties. He then recalled the alarm of economists of previous generations about unpredictability. In 1939 John Hicks was quite explicit about the consequences of incorporating unpredictability into mainstream economics, if the (patently unrealistic) assumption of perfect competition were to be abandoned. According to Hicks, “Under monopoly the stability conditions become indeterminate; and the basis on which economic laws can be constructed is therefore shorn away. . . . The threatened wreckage is that of the greater part of economics” (quoted in Backhouse, 1985, p. 145). Economics then had to choose between an uncertain reality and an unreal certainty, and opted for the latter. According to Arthur, the tools for the former option have now become available, through the work of himself and his colleagues at the Santa Fe Institute.

My own experience of the suppression of uncertainty in mainstream economics was more on the empirical side. I had not noticed this tendency when studying economics as an undergraduate, perhaps because my teachers at Swarthmore College stood out against it. I still recall an inspiring public lecture by Clair Wilcox, a former Roosevelt “brains-truster,” about the pitfalls in aggregated social statistics. Indeed, it was many years later, when my friend Alan Coddington encountered econometrics, as an ex-physicist recruited to the quantitative social sciences, that I learned the horrid truth from him. For he had naively asked his colleagues about the “error bars” on their calculated quantities, and he had been given short shrift. It seemed to him then that the whole subject of econometrics depended on techniques that would get a student thrown out of any respectable first-year course in physics. For a while afterwards, he cried “Emperor’s clothes,” and then turned to high theory and Shackle.

A conviction that Coddington was right and his many colleagues wrong came through my discovery of Oskar Morgenstern’s neglected classic, *On the Accuracy of Economic Observations* (1963). There he reminded his colleagues of all the complexities and uncertainties in such basic variables as “price.” He also told the cautionary tale of the 1920 census of the Bulgarian pigs, where a change in calendar produced a false conclusion of the doubling of the stock; I was told later that this bogus statistic had been accepted as quite significant for a while in the

1920s. (I leave it as an exercise to students of social statistics, what sort of pitfall was created by the calendar change.) I was privileged to meet him, very shortly before his death; he told me that he could never understand why his colleagues failed to appreciate his point about quality of data. He was present at a seminar on pseudo-quantification that I gave at the Institute for Advanced Study in 1977. None of the economists there dared to contradict me in the presence of the master, but *as soon as* I was out in the corridor, I was seized, and assured fervently that real economists know all about uncertainty and teach it to their students; it is just those wicked journalists who purvey spuriously precise numbers to the public. Later I tried to discern what influence Morgenstern's book had had; and in his *Festschrift* (1967), I found just one single essay, by Shubik, which touched indirectly on the theme of uncertainty and quality of data; all the others were orthodox.

Even when mainstream economists attempt sincerely to understand and control the real world, their methodological heritage can betray them. A notorious recent example of this is the affair of the "Fisher equation,"  $MV \equiv PT$ , where  $M$  is money supply,  $V$  is velocity of circulation,  $P$  is price level, and  $T$  is number of transactions. This was seen as a sort of "synthetic *a priori*" proposition, embodying a necessary truth about the real economy (hence the three lines in the equality sign); and so monetarist policy for the control of inflation was based on a rearrangement of terms to  $P \equiv MV/T$ . (The similarity to a basic law of the physics of gases,  $PV = RT$ , should not be overlooked). Perhaps the economists were the victims of the doctrines they learned from the philosophy of science, which concentrated on abstract problems of validation of theories while ignoring the principles of measurement. For the uncertainties in the interpretation of the Fisher equation are very deep. It is not even clear whether the equation is really an identity, in the case of an aggregate of real transactions with a variety of real prices, and "price level" is well known to be partly an artifact of the definition of indices. Worse, the uncertainties in the practical definition of "money supply" turned out to be extreme; indeed, bitter experience produced "Goodhart's Law," that "any monetary aggregate which the authorities try to control automatically becomes subject to distortions which render such control difficult in the extreme" (Hodgson, 1993). After a succession of failed definitions of "money supply" through the 1980s, which rendered economics more risible than ever in the public view in Britain, the whole strategy was abandoned.

Of course, not all economists ignore the uncertainties in their quanti-

tative arguments. Some take explicit account of them, and handle them with an appearance of great care. But even there, the appearance can be deceptive. A case in point is W.D. Nordhaus, who has been writing on the theme of “a little bit of global warming could be good for you.” Analyzing one of his recent papers (1991) in an essay written with my colleague (Funtowicz and Ravetz, 1993), we observed how he manages, or rather manipulates, uncertainty in his quantitative arguments. In a table of eleven entries (Table 1), five have numerical values (in billions of dollars) around  $-1$  or  $-2$ , expressed to *three* significant digits, and five are unquantified. By contrast, the first entry, for impact on farms, is a large interval,  $-10.6$  to  $+9.7$ . The uncertainty in this swamps all the other entries, rendering their precision (as well as that of its own endpoints) quite meaningless. If this “error-bar” interval were carried through the subsequent calculation, it would all be patently vacuous. But by ignoring this gross uncertainty and focusing on smaller uncertainties, Nordhaus plausibly massages his calculated quantities, making various “ad hoc” adjustments, based on “hunch,” so that his final figure, though nearly meaningless scientifically, is presented as quite weighty for policy. A good name for such a methodology might be “meta-GIGO.”

On the theoretical side, the revisions by Ingrao and Israel (1990) to the well-known thesis of Mirowski (see Backhouse, 1991) also fit my analysis. They describe a change in the theoretical economists’ paradigm around 1910 from mechanics to abstract mathematics; this resulted from a recognition of the failure of the previous research program. This move also cohered with the ideals of the “modern” style that was just then being created, in the aesthetic and scientific spheres alike (Funtowicz and Ravetz, 1992). In that way, an axiomatic economics, forever searching for rigor at the price of losing all hope of contact with the empirical world, functioned as a folk science for the very rarefied elite that inhabited the most hallowed of the groves of academe.

Where does all this get us? In his classic methodological statement of 1953, Milton Friedman argued that it is all right, indeed laudable, for economics to have assumptions that are “wildly inaccurate descriptions of reality” so long as it predicts well (Backhouse, 1985, p. 278). Since that positive attribute is generally conspicuous by its absence, the justification of mainstream economics is reduced to its internal consistency alone. In this respect it is methodologically similar to the modern interpretations of some of its distinguished predecessors in the mathematical human sciences, such as astrology and numerology. They rested on an assumption (which we now reject) that the cosmos is sentient and

**Table 1**  
**Impact estimates for different sectors, for doubling of CO<sub>2</sub>, U.S.**  
**(positive number indicates gain; negative number loss)**

Sectors	Billions (1981\$)
<b>Severely impacted sectors</b>	
Farms	
Impact of greenhouse warming and CO <sub>2</sub> fertilization	-10.6 to +9.7
Forestry, fisheries, other	Small + or -
<b>Moderately impacted sectors</b>	
Construction	
Water transportation	?
<b>Energy and utilities</b>	
Energy (electric, gas, oil)	
Energy demand	-1.65
Nonelectric space heating	1.16
Water and sanitary	-?
<b>Real estate</b>	
Land-rent component	
Estimate of damage from sea-level rise	
Loss of land	-1.55
Protection of sheltered areas	-0.90
Protection of open coasts	-2.84
Hotels, lodging, recreation	?
<b>Total</b>	
Central estimate	
Billions, 1981 level of national income	-6.23
Percentage of national income	-0.26

Sources: Nordhaus 1991, Table 6, p. 932. Underlying data on impacts are summarized in EPA (1988). Translation into national-income accounts by author. Details are available on request.

rational. The detailed assumptions of mainstream microeconomics are at least as bizarre in detail (all-wise traders, instantly clearing markets), but its cosmology fits our *common sense* better. Indeed, it is only as an elite folk science that we can understand the power of mainstream economics up to now; and perhaps also we can thereby assess its points of vulnerability.

**Economics as an elite folk science**

My introduction to this section is simplicity itself; I need only quote the words of the master (with thanks to John Hillard):

[A]s living human beings, we are forced to act. Peace and comfort of mind require that we should hide from ourselves how little we foresee. Yet we must be guided by some hypothesis. [Keynes, vol. 15, p. 124]

What better general explanation of the prevalence of folk sciences, even elite ones, could be found? At that point, Keynes was discussing a philosophical conundrum—namely, the assumption that the future will resemble the past. Hence, his remarks were in the domain of generalized human psychology. Shifting our focus somewhat, from people in general to social groups, we are ready to consider mainstream economics as an elite folk science.

For this we might make use of an analogy, derived from a story told to me long ago in Poland. This was not one of the classic anti-Soviet jokes that were an unintended but highly valued byproduct of socialism. Rather, this seems to have been a true story, about a sympathetic American who came to the State Planning Institute, and was surprised to find econometrics of a rather “bourgeois” sort being used unself-consciously. After a while he asked whether he might talk to some Marxist economists. “Ah, our Marxist colleagues! Of course,” came the answer. “We’ll make an appointment for you with them at the University, where they teach the young. We are too busy just keeping the economy running.” For one of the many tragic ironies of socialism was that Marx had spent his life analyzing *capitalism*, and offered precious little help for the building of socialism. Yet the socialist system needed its ideological legitimation in Marxism, and so Marxist political economy became an elite folk science for the apparatus, providing formulas and clichés that were retold and memorized in varying degrees in various institutional settings.

Could there be an analogy here with free-enterprise, quick-buck capitalism? Certainly, no one who operates on a real market behaves remotely like the model actor of microeconomics. Brian Arthur has made a computer simulation of a real market, and shown that agents can indeed manage its inherent uncertainties successfully; but the mainstream assumptions about markets are quite irrelevant (Arthur, 1992). But we do live in an economic world largely defined by “possessive individualism”; and the dominant social practice comes closer to that ideal (if one might use the term) in the United States than anywhere else. Hence, neoclassical economics “feels right” there. With its borrowed prestige as a branch of the elite folk science of Science, it legitimates short-termism, union-busting, junk-bond finance, and perhaps also the Trumps, Boeskys, and Millkens of that world.

Some economists might be shocked at this imputation of covert political messages in their austere doctrine. But economics has always had a commitment to policy, which means politics; and its leading theoret-

ical problems in any given milieu can be understood in terms of the societal challenges to which responsible economists responded. In this way its functions as an elite folk science were open and proper. As Parker (1993) has shown, the shaping of economics by politics in the United States has a long and open history. The mathematical way was distinctly unpopular in the States through all the decades down to the 1950s, as American economics traditionally had a strong ethical, sometimes even a populist, tinge. Also, during all the debates between opposed tendencies, explicitly involving the folk science aspects (including the appeal to Science), there was considerable attention to the characteristic uncertainties of the discipline. Arguments centered on which style, theoretical or empirical, was the best way of coping with them (Yonay, 1994).

In the light of this previous history, it becomes a challenging and important problem to elucidate the sudden rise to total dominance of the ostensibly apolitical mathematical style in American economics in the late 1940s and early 1950s. From being the pursuit of a small handful of brilliant outsiders, it became the ruling paradigm in academic economics within a few years. It was all over by 1957; then Kenneth Boulding said, "there are only a few economists today who would call themselves institutionalists, . . . and there is not anything which would be called an institutionalist movement"; and the Marshallian, the genuine neoclassicals, were eliminated at the same time (Yonay, 1992). This change was not merely the substitution of one ruling tendency by another, for (as Yonay has argued) the character of the discipline changed radically. All the tendencies to mathematization that had been developing over the decades, as described by Mirowski and by Ingrao and Israel, suddenly became hegemonic in the academic profession of economics particularly in the United States. This was not because of great successes in new theoretical syntheses; rather to the contrary, for the most important theoretical advance of that period was Arrow's Theorem, which proved the impossibility of an important aspect of the mathematical program. Once established, this paradigm became exclusive in many ways, partly by the normal processes of academic preference conducted with characteristically American ruthlessness, but also by its construction of an "iron cage" (Yonay, 1992). This operated by a "socially constructed ignorance"; if students had no acquaintance with anything outside the arcane technicalities of their subject, and in particular were deprived of acquaintance with the history of economic thought, then the dominance of this style would have strong tendencies



to self-perpetuation. And then the elite folk science aspects of the discipline could swamp all the others, and so academic economics earned its reputation of splendid indifference to the brute realities of the economy, and also its style of equally splendid contempt for the uncertainties that pervaded its arguments.

Historical research on this very important transition is just beginning; but I cannot resist some speculations of my own. Certainly, "scientism" in the social sciences was then running strongly in many quarters. Thus, a prominent social scientist at the London School of Economics advocated abolishing the study of the history of those disciplines, as it was an impediment to their becoming truly scientific (Wootton, 1950). And during the 1950s the field of geography in Great Britain experienced a temporary takeover by enthusiastic mathematizers. These and other significant phenomena, such as the professional popularity of "modern architecture," can be seen as manifestations of the rise of a general style of "modernism." This had its inception very early in the twentieth century, and by midcentury had spread from the visual arts, music, and mathematics to many fields of activity. Its ruling assumption was that the "classical" style was incorrect in believing that reality could be directly apprehended; rather, an abstract, formal analysis was necessary for elucidating its deep structure (Funtowicz and Ravetz, 1992). Such general currents were doubtless important in the triumph of the mathematizing tendency in economics; but to explain the very sudden rise to hegemony in one place of a tendency in social science which, after all, goes back more or less continuously to Hobbes, one tends to seek for more special circumstances.

Could it possibly have had something to do with the McCarthyite witch hunt in the American universities? Anyone who lived through that period will remember the victimization and humiliation of distinguished scholars in all fields, the summary expulsion of those younger faculty who lacked tenure, the fishing expeditions of the Federal Bureau of Investigation, and the atmosphere of real terror engendered by the various state un-American committees. This was the period when many nonCommunist liberals and radicals became fiercely anti-Communist in order to avoid extermination by association. It was after all the son of a Jewish socialist immigrant, named after the radical poet Walt Whitman (and whose brother was named after the great socialist Eugene V. Debs) who promulgated the "Non-Communist Manifesto" for world economic development (Rostow, 1960), and then proceeded to mastermind the Vietnam debacle. It is then *not* a paradox that the founders of

the new, abstracted economics were generally liberals, and their institutionalist opponents conservative. We might even speculate that for more than a decade a rigorously abstracted mathematics was the only professionally viable alternative to virulent and vacuous free-enterprise propaganda in American academic economics. And by the time the McCarthyite miasma had begun to clear, the continuity with past liberal traditions was broken. The abstract style became adopted as the form for the new elite folk science of economics, in which any political liberalism, covert or overt, was conspicuous by its absence. There was then no institutionally effective challenge to the teaching that eventually produced "the Chicago boys" with their openly political agenda, tested during the restabilization of Chile.

In the context of such a gross politicization of an ostensibly pure academic discipline, it could even be that its counterintuitive, abstract doctrines are an asset. If the standard research paradigm included any features relating closely to problems of the ordinary world of economic affairs, then apprentice scholars might be led into the temptation of including those features in their analyses, and thereby harm their careers by challenging the necessarily banal assumptions of the paradigmatic model. But when patently unreal mathematical exercises are the sole permitted fare of aspiring economists, then their function as a pure *rite de passage* is established. (I am indebted to Neva Goodwin for discussion of these themes.) Those who emerge in later years as distinguished scholars, and who are thereby entitled to pontificate on the economy, may *then* speak the same language as politicians, journalists, and ordinary people. They too juggle with the patent uncertainties of such macro variables as inflation, unemployment, interest rate, and balance of payments. But they have earned the right to do so, in the eyes of their own elders and controllers, by demonstrating their unswerving loyalty to the party line. So the analogy goes, as Marxism was an elite folk science for the nomenclatura of socialism, so is mainstream economics for the contemporary robber barons of the Anglo-American variety of capitalist society.

And there is a most important difference. As time wore on, Marxism lost more and more, and eventually all, of its plausibility. Capitalists and proletarians were either absent from socialism, rendering Marxism irrelevant, or they were present in new forms, rendering it subversive. As an elite folk science under socialism, with functions becoming ever more purely ideological, Marxism became hollow, rotten, and dead.

By contrast, to the extent that a modern economy is based on masses

of individualistic profit-maximizers, large and small, then mainstream economics retains its plausibility; and then it can perform its elite folk science functions unchallenged. It functions by proxy as a popular folk science too; its relations to the aspirations of ordinary folk might be compared to those of theology for the religious. They do not need to understand it in detail, and still less to be involved in its disputes and uncertainties. All they need to know is that something is *there*, providing the answers for those on whom they rely to know and to keep control. Once so firmly established, the elite folk science can be pervasive through all policy debates, providing the ultimate rhetorical justification for any and all ill-founded partisan arguments. Thus, for example, multilateral “free trade” and deregulated financial markets will claim “competitive equilibrium” for their pedigree, in spite of its obvious inapplicability in both cases.

So it would seem to be almost a circular argument, that a discipline functions as an elite folk science so long as there is some elite folk that needs it for a science. But as things work out in history, the circle eventually becomes a spiral. Elite folk sciences come and go, and are argued for and against on the whole spectrum of reasons. Theology has not been the queen of the sciences for a very long time; and more recently the sun has set on the British empire, eventually rendering Oxford classics redundant. Even Science has been taking a hammering, on the political and philosophical fronts, perhaps partly because it has lost the internal *élan* and external protection that came with its status as the leading anticlerical elite folk science. What of economics, in this perspective?

Among the most important determinants of the success of an elite folk science is its plausibility—that is, its coherence with the rest of the socially constructed common-sense reality inhabited by its clientele and audiences. If it is firmly embedded in that cultural matrix, then it is invulnerable against competitors, even if its presuppositions echo the most banal elements of that culture, and its policy conclusions do not go much further. In the absence of effective alternatives, its wider supportive audience will stick with it as the best thing available. One of the greatest shifts in choice of elite folk science in our culture occurred in the seventeenth century, when the modernized, Christianized Aristotelian world system was displaced by a collection of “corpuscular” philosophies, all mutually inconsistent and each internally incoherent, and all lying rather too close for comfort to some notorious atheistic sources. Yet, in ways that are too complex to describe here, there was a

basic shift in educated common sense. At the beginning of the century, the “atomists” comprised a small, motley crew, many of whom were put down as heretical. By the end, students were imbibing Gassendi, Descartes, and Galileo, ladies were reading about the plurality of worlds, and manners and morals were not noticeably corrupted thereby. Through that century, popular common sense was also changing, as plausibility ebbed out of the ancient arts, as alchemy and astrology, and (much more complex) witchcraft also. The decline of the elite Aristotelian humanized cosmos went roughly parallel with the decline of the popular enchanted cosmos. The story of that momentous epoch is still only imperfectly understood, but it serves as an important example of the interaction of elite and popular folk sciences, of their associated common-sense universes, and of the possibility of quite radical changes (Ravetz, 1990b).

It could now be that we are coming into such a shift in plausibility (though on a smaller scale) that could affect economics as an elite folk science more rapidly than we might expect. In popular consciousness, we all live on “spaceship earth,” on only one planet, with limited sources and limited sinks. The world of global environmental policy now incorporates two assumptions as basic: nothing is certain, and nothing is “external.” Any science that assumes certainty and relegates the most urgent problems to “externalities” will seem increasingly irrelevant and bizarre. Environmental accounting, even with all its enormous methodological problems, is a recognition and implementation of the new emerging folk science, operating at both popular and elite levels, of “ecology.” Ecological economists are now beginning to work with uncertainty, both in the applied and theoretical domains (Costanza and Cornwell, 1992; Drepper and Månsson, 1993).

Parallel to the rise of the new environmental consciousness is the decline of modernism. This tendency can go in several directions, perhaps to the “green,” valuing sentiment as an indicator of truth; or “postmodern,” dismissing reality altogether; or “postnormal,” establishing new roots in the practice of dialogue (Funtowicz and Ravetz, 1993). Whatever the outcome, the totem of Science, as a foundation for practical wisdom, loses its allure. All over the Western world, the traditional abstracted and mathematicized natural sciences, whose image formed the model for mainstream economics, suffer from widespread disillusion and from the disinterest of their intended recruits. In economics, there is evidence that in the United States the mainstream has also passed its peak, in both quantitative growth and political

influence (Parker, 1993). It now becomes newsworthy that real decision makers use their intuition rather than the unreliable economic indexes (Bradsher, 1994). Constructive criticisms of mainstream economics are emerging, in which some genuine empirical content of important regularities is salvaged. But these studies are conducted in explicit opposition to the axiomatic assumptions of mainstream economics (including the independence of economic activity from institutions, and the perfect wisdom of decision makers), and also through the demolition of its pretensions to intellectual coherence of any sort (Ormerod, 1994).

One of the great challenges of the craft of the intellectual historians is to imagine how educated people could once invest so much commitment and passion in issues that now seem quite meaningless. What these historians do (in the terms of this analysis) is to see how the peculiar doctrines could have had a vivid meaning as an elite folk science. And now that the special functions of mainstream economics as an elite folk science are in decline, it may occur, even sooner than we expect, that individuals who had been conditioned to perceive reality in terms of its abstruse technicalities might look back upon their own earlier endeavors, and wonder how it could all have happened.

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