Finnish Economic Papers – Volume 5 – Number 1 – Spring 1992

DO ECONOMISTS SUFFER FROM PHYSICS ENVY?

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This paper summarizes and expands upon the thesis found in the author's More Heat Than Light (1989) that the origins and rise to dominance of the neoclassical orthodoxy has been intimately linked to the history of physics. Problems of »physics envy» include a certain contempt for the history of economics, a tendency towards the uncritical appropriation of a limited range of mathematical formalisms, and constant intrusions by physical scientists seeking to upgrade the scientific status of the discipline.

1. Introduction

Pity, if you will (or if you can), the poor modern economist. It is commonplace to deride them for their obscurity in expression, their ambidextrousness (on the one hand... on the other hand...) and their failures of prediction and policy control. And worse, each new ratification of their professional legitimacy only seems to make them more nervous and captious. If they manage to achieve a certain level of homogeneity and self-assurance in their theoretical discourse, then esteemed members of the profession complain of an empirical sterility in their house journals.[1] If they are endowed with their own Nobel Prize, then some early Nobel laurates assert the prize has had a vile and pernicious influence upon the discipline.[2] If special governmental status is conferred upon them, such as, say, the U.S. Council of Economic Advisors, then many other economists assert that such blandishments should be renounced in the same language as preachers denounce temptations of the flesh. If they are closeted together with various physicists with the intention of producing some joint theoretical innovations, such as recently happened at the *Santa Fe Institute*, much time is frittered away invidiously comparing whom should be regarded as the more mathematically rigorous.[3] And when a recent study of graduate education in economics concluded that the next generation seemed beset with cynicism, combining »a loss of faith in science (with) continuing commitment to modernist expressions» a committee constituted by the American Economics Association to address the

[1] *Wassily Leontief, Science 217, 104 (1982).*

[2] *Gunnar Myrdal, the 1974 winner of the Nobel Prize in Economics, often said before his death that the prize was a curse upon economics, and toyed with the idea of renouncing it.*

[3] *Robert Pool, Science, 245, 701 (1989). While level of mathematical prowess is the main focus of physics envy amongst economists, it can also be discovered in curiosity about the relative length and magnitude of citations of Nobel winners. See Robert Tollison and Brian Goff, Journal of Institutional and Theoretical Economics, 142, 581, (1986).*

problem could not manage to come up with any substantive response.[4]

How should one come to understand this neurotic labile behavior? While the psychobiographer might want to get these specimens of the species *homo economicus* to lie down on the couch and submit to protracted psychoanalysis, there is no need in this instance for Freudian categories or Jungian archetypes to clear up the confusion. Tentatively we should like to suggest that it will be sufficient to instead repair to the history of economics, and to the history of science in general[5] to reveal that the source of many of these quirks that beset economists are rooted in a pervasive physics envy. This observation may be useful to recall the next time some physicist vents their frustrations with regard to those unrecusant economists and their unscientific predilections.[6]

2. The Origins of Neoclassical Economics

Ignoring for our present purposes other rival schools of economic thought such as the Marxians, the Institutionalists and the German Historicists, the dominant lineage of the modern profession is often traced from what has been called »classical» economics in the 18th and 19th centuries through to the modern incarnation of »neoclassical» economics, dating from the 1870s to the present. The signal characteristic of classical economics was that it conceived of value in exchange as being intimately linked to the labor expended in the production of goods, whereas realized price might also reflect short-run accidents of market fluctuations or external disturbances. While the classical economists were most desirous of putting their inquiry upon a »scientific» footing, it is very important to note that theirs was a rather earlier conception of science, more akin to what we would now call »natural philosophy.»[7] This meant, for instance, that there are no mathematical models to be found in the classical economists (contrary to the attempts of *Samuelson, Brems* and others to assert their presence in a latent state); nor, indeed, were there any serious systematic attempts at what we would now regard as empirical inquiry or data gathering.[8]

The validity and legitimacy of classical economics suffered severe erosion in the second half of the 19th century. One symptom amongst many of this fall from grace was the attempt by Francis Galton to oust Section F (Political Economy and Statistics) from the *British Association for the Advancement of Science* in 1877, on the grounds that the denizens of that section did not hew to commonly recognized scientific methods. Still another was what is now called in retrospect the *Methodenstreit* between the *German Historicist school* and the *Austrian subjectivists* in the late 19th century. A third was the protracted controversy over the proper role of the economics profession conducted in *Science* in the year 1886. While this decline had numerous causes, from the resurgence of neo-Kantian philosophy to the new impetus towards professionalization in the burgeoning system of universities,[9] the prime motive force was the

[4] Arjo Klamer and David Colander, The Making of an Economist (Westview Press, Boulder, 1990), 183.

[5] This article should therefore be considered a sequel to Stephen Brush's »Should the History of Science be Rated X?», Science 183, 1164 (1974).

[6] »Global security is too important to be left to the politicians, just as economics is too important to be left to the classical economists.» David Pines, quoted in Pool, ibid, 245, 703 (1989). Parenthetically, I do not mean to imply here that »physics envy» might not itself be susceptible to further theoretical analysis; rather, I doubt that it has a methodologically individualist basis. On this, see my forthcoming Markets Read in Tooth and Claw (Cambridge University Press).

[7] Evidence documenting the relationship between economics and the various images of science may be found in Philip Mirowski More Heat Than Light: Economics as Social Physics, Physics as Nature's Economics (Cambridge University Press, New York, 1989). Earlier work on the classical economists can be found in Vernard Foley, The Social Physics of Adam Smith, (Purdue University Press, West Layfayette, Ind., 1976).

[8] On Smith's attitudes towards empiricism, see Philip Mirowski, Against Mechanism (Rowman & Littlefield, Totawa, N.J., 1988) chap. 11. An empiricist/inductivist reaction to classical economics identified with William Whewell, Thomas Robert Malthus and Richard Jones did not succeed in establishing a viable rival research program in the early 19th century. On this episode, see Neil de Marchi and R.P. Sturges, »Mathus and Ricardo's Inductivist Critics» Economica, XX, 379 (1973).

[9] An analysis of the impact of the nascent professionalization movement upon British economics is John Maloney; Marshall, Orthodoxy and the Professionaliza-

profound change in the very notion of science prevalent in the later 19th century.[10] Quite simply, the writings of the classical economists no longer resembled what an inhabitant of that culture in that era would recognize as science in any of its myriad manifestations. Of course, controlled experiments were out of the question in classical economics; but worse, the discursive essays of the political economists did not resemble the *genre* of new journal literature characteristic of the natural sciences. Mathematical expression was largely absent, as was a sense of cumulative theoretical achievement. Yet the change in the image of science ran deeper, as exemplified by the displacement of astronomy by physics as the new king of the sciences. The conceptual shift responsible for this alteration in the image of science revolved primarily around the novel concept of energy.

It is difficult for modern readers to appreciate the extent of the impact that the innovation of the energy concept had across the broad gamut of intellectual endeavor in the later 19th century.[11] While first achieving the consolidation of previously disparate physical phenomena such as heat, light, magnetism, electricity and the previously well-developed rational mechanics, it was soon asserted that the energy formalism might encompass the science of life itself under physical law; and from there it would proceed to encompass psychology, sociology and even aesthetics, thus unifying the whole of science. This »Energetics» movement was championed in Germany by *Wilhelm Ostwald* and *Georg Helm;* there developed a Swiss variant at Lausanne; *Pierre Duhem* and *Henri le Châtelier* were at times counted in its French ranks; it found a patron in Belgium in *Ernest Solvay* (of the famous Solvay Conferences); was given a peculiarly British accent by *Herbert Spencer;* and found late advocacy in America by *Henry Carey, Frederick Taylor* and the Technocracy movement.[12] Anyone who contemplated striving for scientific status in the late 19th century context had to confront the energy concept. It was in this particular regard, perhaps more than any of the others we have mentioned, that classical economics was found wanting.

Curiously enough, it also explains the origins of the modern orthodoxy of neoclassical economics. In the decades of the 1870s and 80s, a wide range of individuals in different European contexts, but all sharing a grounding in the new energy physics, proposed to mathematize economics by taking the basic model from physics and changing the names of the relevant variables: potential energy became »utility»; kinetic energy (with the added proviso of something later known as the »law of one price») became the budget; space was transformed into »commodity space»; forces were transformed into prices, and so on.[13] The reason this has not been blatantly obvious to all concerned is that the addition of one non-physical relation — the law of one price — rendered the critical energy integral an energy summation instead in a transformed commodity space. If we start with a conservative irrotational vector field:

$$(1) \quad \oint F\,ds = 0$$

then we can associate with it a scalar potential field U

$$(2) \quad F = \text{grad } U = \left(\frac{\partial U}{\partial x}, \frac{\partial U}{\partial y}, \frac{\partial U}{\partial z}\right)$$

tion of Economics (Cambridge University Press, Cambridge, 1985); while a good source on the American context is Mary Furner, Advocacy and Objectivity (University of Kentucky Press, Lexington KY, 1975); the 1886 controversy in Science is discussed therein on pp. 92 – 106.

[10] *This shift is noted in the history of physics literature in P.M. Harman, Energy, Force and Matter (Cambridge University Press, Cambridge, 1982), M. Heidelberger in H. Jahnke & M. Otte, eds., Epistemological and Social Problems in the Sciences of the Early 19th Century (Reidel, Boston, 1981), and Susan F. Cannon, Science in Culture (Science History Pub., New York, 1978). Its impact upon economics is discussed in further detail in Mirowski, More Heat, ibid. chap. 5.*

[11] *One recent work which gives a flavor of this enthusiasm is Crosbie Smith & M. Wise, Energy and Empire (Cambridge University Press, Cambridge, 1989); another is Patrick Bratlinger, ed., Energy and Entropy (Indiana University Press, Bloomington, 1989). A popular history of the attendant reconceptualization of the human body is Anson Rabinbach, The Human Motor (Basic Books, New York, 1990).*

[12] *The best work on the history of energetics is R. Deltete The Energetics Controversy in Late 19th Century Germany (unpub. Ph. D. thesis, Yale University, 1983). See also Mirowski, More Heat, ibid, 265 – 70.*

[13] *The isomorphism is discussed in detail in Mirowski, Against, ibid. chap. 1 and More Heat, ibid, chap. 5.*

This was the primal inspiration behind the neoclassical doctrine that prices are proportional to marginal untilities in equilibrium, but allowed trades of any specific commodity {x, y, z} to take place at different prices for the same commodity, an idea the early neoclassicals could not reconcile with their notions of competition and arbitrage. Hence they posited that each unit of every commodity must trade for the identical price in equilibrium which reduced equation (1) to:

$$(3) \quad \Sigma F_x dx + F_y dy + F_z dz + \ldots$$

which became the familiar budget constraint. When this constraint was included in a revised commodity coordinate system, the simple symmetry conditions of the irrotational field became the Slutsky or integrability conditions of modern neoclassical doctrine. Here (contrary to *Varian,* 1991) the symmetry of the Slutsky matrix implies that compensated demand functions, and not inverse demand functions, constitute the conservative vector field, though it took neoclassicals in general until the 1930s to figure out this twist on the physical model.

While various ideosyncracies in the respective understandings of the energy model would result in differences in the proposed economic model, eventually *William Stanley Jevons, Léon Walras, Vilfredo Pareto, Francis Ysidro Edgeworth, Giovanni Antonelli, Maffeo Pantaleoni, Irving Fisher,* and a whole host of lesser writers came to acknowledge each other as toilers in the same vineyard, primarily by stressing the analogy between the extremum principles in rational mechanics and what became known as the »maximization of utility» in their novel economic doctrine. Constrained optimization, which had much earlier been employed to argue the efficiency of God's creation in natural theology, now was turned around to define »rationality» in the economic sphere. Whereas the classical economists had previously discussed the health or reproduction of the economy, the neoclassicals now related every economic issue to the mechanical notion of »equilibrium».

While the appropriation of a mathematical model from physics provided a ready-made shared language and template of legitimate explanation for the nascent school of economics and hastened its mathematical elaboration, one should not presume that progress was effortless, nor that other schools of thought passively acquiesced in the novel definition of economic science. Indeed, neoclassical theory made but paltry headway in the economics profession in the period up till roughly 1930.[14] The reasons for this retarded progress are again numerous, ranging from various disanalogies present in the proto-energetics model to hostility towards the abstract mathematical character of the new discourse, but the primary obstacle was located within the structure of the discipline. Graduate education in the nascent profession was not able to recruit people with the scientific backgrounds similar to those of the pioneer generation of neoclassicals mentioned above, nor was it able to unilaterally impose the sort of curriculum which would enable novices to recognize and manipulate the formal energy model. Further, some minor skirmishes with mathematicians and physicists who questioned the wisdom of the appropriation of the energy model, such as *Joseph Bertrand, Hermann Laurent* and *Vito Volterra,* prompted subsequent neoclassicals to shy away from explicit acknowledgement of their imitation of energy physics.[15] The decline of the Energetics movement within physics after *Boltzmann* and *Planck*'s attack on *Ostwald* and *Helm* in 1895 also encouraged the dissociation.[16] Hence a whole

[14] The two exceptions to this rule are the previously-mentioned Lausanne school and the bailiwick of Alfred Marshall at Cambridge University in England. The latter special case is discussed in Philip Mirowski, »Smooth Operator: How Marshall's Curves of Supply and Demand Made Neoclassicism Safe for Public Consumption but Unfit for Science» in R. Tullberg, ed. Alfred Marshall in Retrospect (Edward Elgar, Cheltenham, 1990). The former is covered in Claude Menard, »The Lausanne Tradition» in Klaus Hennings and Warren Samuels, eds., Neoclassical Economic Theory, 1870 to 1930 (Kluwer Academic, Boston, 1990). Evidence for the retarded advance of neoclassicism is presented in Philip Mirowski, »The How, the When and the Why of Mathematical Expression in the History of Economic Analysis,» Journal of Economic Perspectives, 5, 148 (1991).

[15] The problems of early graduate education are discussed in Maloney, ibid. and in William Barber, ed., Breaking the Academic Mould (Wesleyan University Press, Middletown, CT, 1989). The criticisms of Bertrand et al. are described in Mirowski, More Heat, ibid, pp. 241–53.

[16] C. Jungnickel & R. McCormmach, Intellectual Mastery of Nature (University of Chicago Press, Chicago,

sequence of historical accidents served to repress the physics origins of neoclassical economics, while much of its structure was preserved in a chrysalis-state in the mathematics.

Many historians, such as *Samuel Hollander, Don Walker, Jan van Daal* and *Donald McCloskey*, seem to have difficulty with this last statement. Instead, they claim that the use of mathematics was somehow »separable» from its physical instantiations. While it is true that in the later 20th century mathematics has largely severed the bonds of its physical origins, this was not the case in the period under discussion. Virtually everyone who had competent training in applied mathematics as late as the turn of the century imbibed their curriculum from the physical sciences, and in particular, rational mechanics, and therefore would be structurally predisposed to favor neoclassical economics. The point is simply illustrated by the fact that if anyone tried to employ graph theory or group theory or number theory to discuss the economy instead of the calculus, they simply would find themselves isolated, without an audience. In such a millieu, mathematics was plural but science was singular.

3. *Twentieth Century Physics Envy*

All of this borrowing across disciplines might not have mattered much, if indeed it were only the initial phase of a sustained internal critique of the analogy combined with further emendation to bring the model more in line with problems generic to the economy. After all, it is quite common amongst modern historians and philosophers of science to appreciate the role of transfer of external analogy and metaphor in the construction of scientific theories.[17] But that is not what happened in the case of neoclassical economics. Instead, the period roughly 1890–1930 witnessed a rather muddled and inconclusive discussion of the meaning of such fundamental theoretical terms as potential (utility), statics, dynamics and equilibrium without the benefit of experience with their usage at their point of origin. The tenor of the meagre mathematical discussion ranged from rudimentary to abysmal, and quantitative empiricism was not taken seriously. In this period, neoclassical theory became confused with the Marshallian apparatus of demand and supply curves, which misrepresented underlying structure of the energetics model, and due to its own internal logical inconsistencies came under increasingly harsh criticism in the 1920s.[18] In view of these events, the neoclassical program was not widely viewed as inherently more »scientific» than its rivals.

The Great Depression of the 1930s changed all that. First, it lent an urgency to the discussion of economic problems which transcended more detached attitudes of the preceeding period. But it also had the unanticipated consequence of throwing large numbers of scientifically trained personnel out of employment, as well as the extremely indirect ripple effect of forcing waves of intellectuals to flee the subsequent political chaos. Simultaneously, philanthropic organizations such as the Cowles and Rockefeller Foundations intervened in this situation to try and nudge economics in what they considered to be a more scientific direction in both Europe and Ameri-

1986), v.II, pp. 218–27; Erwin Hiebert, »The Energetics Controversy» in D. Roller, ed., Perspectives in the History of Science (University of Oklahoma Press, Norman, 1971); Deltete, ibid.

[17] *»The history of physics shows us that the searching for analogies between two distinct categories of phenomena has perhaps been the surest and most fruitful method of all the procedures put into play in the construction of physical theories.» Pierre Duhem, The Aim and Structure of Physical Theory, trans. P. Wiener (Atheneum, New York, 1977), 95–96; Mary Hesse, Models and Analogies in Science (University of Notre Dame Press, Notre Dame, 1966); Anthony Ortony, ed., Metaphor and Thought (Cambridge University Press, New York, 1979); Margaret Masterson, »Braithwaite and Kuhn: Analogy-clusters within and without hypothetico-deductive systems in science,» in D.H. Mellor, ed., Science, Belief and Behavior (Cambridge University Press, Cambridge, 1980).*

[18] *On the misuse of terms such as »dynamics», see E. Roy Weintraub, Stabilizing Dynamics (Cambridge University Press, New York, 1991) and More Heat, ibid, chaps 5 and 6. On the absence of mathematical discourse, see »The How, . . .» ibid. On the lack of empirical work, see Philip Mirowski, »The Probabalistic Counter-revolution», Oxford Economic Papers, 41, 217 (1989) and »Problems in the Paternity of Econometrics» History of Political Economy 22, (1990). For the Mashallian controversy, see Krishna Bharadwaj & Bertram Schefold, eds., Essays on Piero Sraffa (Unwin Hyman, London, 1990) and »Smooth Operator», ibid.*

ca.[19] What this all portended was an unprecedented influx of natural scientists into economics, especially but not exclusively in the United States, in a period of pervasive crisis in the discipline. I will let one of these scientists who later won a Nobel Prize describe what was in broad outlines a fairly common experience:

»Why did I leave physics at the end of 1933? In the depths of the world-wide economic depression I felt that the physical sciences were far ahead of the social and economic sciences. What had held me back was the completely different, mostly verbal, and to me almost indigestible style of writing in the social sciences. Then I learned from a friend that there was a field called mathematical economics, and that Jan Tinbergen, a former student of Paul Ehrenfest, had left physics to devote himself to economics. Tinbergen received me cordially and guided me into the field in his own inimitable way. The transition was not easy. I found that I benefitted more from sitting in and listening to discussions of problems of economic policy than from reading the tomes. Also, because of my reading block, I chose problems that, by their nature, or because of the mathematical tools required, have similarity with physics.[20]

These peripatetic natural scientists often had little time or patience to plow through *Adam Smith* or *Karl Marx* or even *Francis Ysidro Edgeworth,* but they did recognize the rough outlines of the energy model when they saw it written in its truncated and outdated format; they also were aware of many mathematical techniques which could be used to bring it »up to date.» Further, precisely because of all the political disruption and dislocation they had endured, they sought to elevate economic discussion to a less ideologically contentious plane, and believed that the emulation of science was the best mode of achieving that end. The net result was that the neoclassical program enjoyed a tremendous rejuvenation, an unplanned shot in the arm of mathematical rigor and formalist reconstruction. First in the United States, and then after World War II in Europe, the mathematical program of neoclassical theory succeeded in displacing all rival research programs in economics, to the extent that it is today the world standard in graduate economic education.

Here we should note that the story does get more complicated than this simple narrative would portend and that we need more histories like (*Ingrao & Israel,* 1990) to help us straighten it out. Some of this new generation like *Jacob Marschak, Tjalling Koopmans, Maurice Allais* and *Paul Samuelson* were more concerned to tap physical science metaphors for model improvements; while others who fit the above pattern less well such as *Kenneth Arrow, John von Neumann* and *Gerard Debreu* were more interested in mathematically loosening the assumptions away from their initial physical configurations; but in each and every case the only »legitimate» model was deemed to be that which treated individual »preferences» as a stable vector field in an independently given commodity space, which of course is the core of energy physics, although in economics it travels under the rubric of »Walrasian». Furthermore, one must not presume that advocacy of the neoclassical model was a function of any particular political attitude towards the Depression: it was the urgency of the problems, and not dilatory dispositions or political bias which rendered the vast previous accretion of economic literature irrelevant for this generation.

But this brings us back to the issue of physics envy. It should be plain to anyone who makes the effort to become familiar with this history that any simplistic explanation of the rise to dominance of neoclassical theory, such as unvarnished assertions that it »works» or is »more scientific» or »more rigorous» than its rivals, will surely meet insuperable logical obstacles. For instance, sophisticated theorists of neoclassical general equilibrium will readily admit that their models exhibit profound conceptual difficulties when it comes to such is-

[19] *Earlene Craver, »Patronage and the Direction of Research in Economics», Minerva, 24, 205 (1986); Craver and Axel Leijonhufvud, »Economics in America: the Continental Influence», History of Political Economy, 19, 173 (1987).*

[20] *Tjalling Koopmans, »Experiences in Moving from Physics to Economics», unpublished talk delivered to the American Physical Association, New York, 29 Jan. 1979. Copy deposited in Koopmans papers, Sterling Library Archives, Yale University, Box 18, folder 333.*

sues as the uniqueness and stability of equilibrium, the treatment of human knowledge and uncertainty, a plausible scenario of dynamics, adequate specification of the role of money, and so forth.[21] Thus modern general equilibrium theory cannot be said to explain how the market does or does not work, nor to explain the failure of socialism, or even, as (*Ingrao & Israel*, 1990) argue, provide any basis for the widespread belief that posited characteristics of individual actors place any restrictions upon the resulting Walrasian equilibria. Indeed, over its century-long history neoclassical theory has been used both to justify state planning and laissez-faire policies, methodological individualism and methodological collectivism, just as it has been used to argue both sides of most contentious political issues. As for its putative scientific character, that is the root of the problem of physics envy.

Since the Energetics movement failed within physics, and because subsequent neoclassical economic theory does not draw any of its fundamental precepts directly from known natural laws,[22] the impression that economics has attained unblemished scientific status rests almost entirely upon superficial points of resemblance between physics and economics. It was at one time common to assert that economics and the social sciences shared a common »scientific method», but this hope has grown increasingly forlorn and dim as historians and philosophers of science have become more skeptical about such trans-historical and trans-disciplinary criteria.[23] Now it has become more common to simply note the structural parallels between physics and economics, and to cite this as indication of the generic nature of all science or rationality.[24] Yet as simple deterministic rational mechanics recedes into distant memory, and profound reconceptualizations of natural law are persistently produced in the 20th century, the resemblances grow less and less marked, and the neoclassical economists grow more and more nervous about the precise character of their scientific status. It is this quandry, and not deficient toilet training, which accounts for periodic modern outbreaks of physics envy amongst economists.

Physics envy is not a pleasant condition. It renders the sufferer exceedingly sensitive to fine distinctions in the level of perceived mathematical rigor of others, while losing sight of the ultimate purpose of the mathematics. It induces endless lament about the dearth of high quality empirical endeavor, while acknowledging that no substantial controversy has ever been settled in the history of the orthodox discipline by an empirical paper. It frets about the problem of replicability and reliability of research results, without once being able to analyze why such a goal might be unsuitable for the social sciences.[25] It fosters a graduate curriculum where accelerated escalation of technique is the main instrument of socialization and domination of the next generation of economists. And worst of all, it only hops on an analytical bandwagon once it is coupled to the engine of physics, but ignores much indigenous and ingenious innovation in its own backyard.

One example of this last phenomenon is the treatment of *Benoit Mandelbrot* over the last two decades. In the 1960s and early 1970s, Mandelbrot produced some very original work in the economics of random phenomena, but since it was not in harmony with the deterministic notions of explanation then preva-

[21] Franklin Fisher, Disequilibrium Foundations of Equilibrium Economics, (Cambridge University Press, New York, 1983); B. Ingrao & G. Israel, »General Equilibrium Theory — A History of Ineffectual Paradigm Shifts,» Fundamenta Scientae, 2, 1 (1985); Philip Mirowski, »The Rise and Fall of the Equilibrium Concept», Recherches Economiques de Louvain, 55, 447 (1989).

[22] Even »production functions», which are often asserted to have been derived from physical or engineering specifications, regularly violate physical laws. On this, see Mirowski, More Heat, ibid., chap. 6.

[23] A good introduction to the state of this controversy can be found in R.C. Olby et al., eds. A Companion to the History of Science (Routledge, London, 1990). An introduction to the methodological problems with this position from the economics vantage point is Bruce Caldwell, Beyond Positivism (Allen & Unwin, London, 1982).

[24] Robert Bordley, International Journal of Theoretical Physics 22, 803 (1983); Leonid Hurwicz and Marcel Richter, Journal of Mathematical Economics, 6, 1 (1979).

[25] W. Dewald, J. Thursby & R. Anderson, American Economic Review, 76, 587 (1986); Harry Collins, Changing Order, (Sage, London, 1985); Philip Mirowski & Steven Sklivas, »Why Econometricians Don't Replicate», Review of Political Economy, 3, 146 (1991); Philip Mirowski, »What Could Replication Mean in Econometrics?», paper presented to Conference on Testing, Tilburg Univ., December 1991.

lent (and traceable to the origins within 19th century energetics), his work was roundly ignored. However, now that fractals have found an application in the theory of turbulence and in other physical problems, numerous neoclassicals have rushed to import chaos theory into economics, all the while continuing to ignore the earlier Mandelbrot work.[26] When you've got physics envy and got it bad, no model will ever gain substantial allegiance in neoclassical economics until it has first earnerd its spurs in physics.[27] While by no means a globally necessary condition, it has certainly been historically sufficient.

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[26] *Philip Mirowski, »From Mandelbrot to Chaos in Economic Theory,» Southern Economic Journal (1990).*

[27] *Another example of this phenomenon is discussed in Philip Mirowski, Studies in the History and Philosophy of Science, 20, 175 (1989).*