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Physics and the 'marginalist revolution'

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The mathematician is an inventor, not a discoverer (Wittgenstein, 1978, I, 168).

1. Internal *versus* external histories of science

Interest in the origins of neoclassical theory has a number of motivations. The first is antiquarian: it is concerned with tracing the intellectual antecedents of a given innovation. The second is epistemological: the methods of great discoverers are held to provide an exemplar for currently accepted methods of research. The third is ontological: the occurrence of independent simultaneous discovery is used to suggest the substantiality and reality of the phenomenon identified. William Stanley Jevons, for instance, wrote that, 'The theory in question has in fact been independently discovered three or four times over and must be true' (Jevons, 1972, IV, p. 278). The fourth is practical: it provides a reservoir of metaphors and theoretical suggestions which might serve to prompt novel contemporary lines of inquiry which are obscured or slighted by modern theory. Confusion or doubt over the origins of modern neoclassical economic theory would introduce the possibility of serious historical, epistemological, ontological and practical confusions in its exposition.

At present, the most popular textbook of the history of economic thought attempts to dispose of the issue by absolving itself of any responsibility for discussing origins:

> Therefore, to try to explain the origin of the marginal utility revolution in the 1870s is doomed to failure: it was not a marginal utility revolution; it was not an abrupt change, but only a gradual transformation in which the old ideas were never definitively rejected; and it did not happen in the 1870s (Blaug, 1978, p. 322).

This text denies that there was any unified and self-conscious movement. In its stead, it portrays a haphazard and fragmented agglomeration of economic theorists, whose only common denominators were the twin notions of diminishing marginal utility and utility-determined prices. Since neither notion was particularly novel in the 1870s, it follows from this portrayal that there was no discontinuity in the economic thought of the period, and that economic theory has embodied one continuous discipline from Adam Smith until the present (see Bowley, 1973, ch. 4).

The thesis that innovations in economic theory in the 1870s and 1880s were unexcep-

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tional and merely a continuation of the unbroken threads of economic discourse in the preceding half century meets a number of difficulties. The first problem is that not all the major protagonists would have agreed with such an assessment. One cannot read the letters and published works of Stanley Jevons, Léon Walras, Francis Edgeworth, Irving Fisher, Vilfredo Pareto and others without repeatedly encountering assertions that their work represented a fundamental break with the economics of their time. Much of their professional lives was spent promoting the works of this small self-identified côterie. The second impediment to the gradualist view is the fact that the most discontinuous aspect of the 'marginalist revolution' was not the postulate of a utilitarian theory of value, but rather something no historian of economic thought has ever discussed in detail: the successful penetration of mathematical discourse into economic theory. In both their correspondence and in their published work, the early neoclassical economists recognised each other as *mathematical theorists* first and foremost; and when they proselytised for their works, it took the form of defending the 'mathematical method' in the context of economic theory. The third impediment to the gradualist view is the fact that all the major protagonists were concerned to differentiate their handiwork from previous political economy on the explicit ground that it was of a scientific character. While the claim that one's theory is 'scientific' (and therefore deserves respect) echoes throughout the last three centuries of social theory, in the case of Jevons *et al.* this claim assumes a very specific and narrow form, shared by all the principals. An understanding of these three points will lead inexorably to a re-evaluation of the significance of the rise of neoclassical economic theory.

The gradualist view of the genesis of neoclassical theory has generally been prefaced with some methodological remarks on the contrast between 'internalist' and 'externalist' intellectual histories (Blaug, 1978; Black, Coats and Goodwin, 1973). The internalist version, the one presently favoured by neoclassicals, assumes that all ideas are merely reactions to previous developments internal to the discipline under consideration. The job of an intellectual historian is to trace the descent of ideas from scientist to scientist through time, revealing how error was rooted out by the internal criticism of logical deduction and empirical testing, while scientific truths were preserved and nurtured. New insights and concepts are pioneered by key individuals, but the sources of those insights are not an important part of the historian's narrative (Popper, 1965). The historian may use sociological and other external considerations to explain adherence to superseded theories; but adherence to the successful theory is felt to need no other explanation other than its *prima facie* success (Bloor, 1976).

This view is in contrast to externalist intellectual history, which seeks the determinants of successful theories in the political, philosophical and/or social currents of the time. The externalist historian is satisfied to identify the link between an historical interlude and the construction and acceptance of a successful theory, without expending undue effort to trace the intellectual pedigree of its precursors within the science. Undoubtedly, much of the hostility of neoclassical economists to externalist explanations of the 'marginalist revolution' stems from the weak and unconvincing nature of the few attempts: Bukharin (1927) associated it with the rise of a new class of *rentiers* in *fin-de-siècle* Europe, whereas Stark (1944) saw it as a reflection of some general Kantian influences in conjunction with the assertion that the economy of mid-nineteenth century Europe was actually characterised by atomistic competition. It has been observed repeatedly that these portrayals are not historically accurate; nor do they describe correctly the milieu of the major protagonists (Blaug, 1978; Kauder, 1965).

The internalist/externalist dichotomy has itself impeded the understanding of the rise of neoclassical economic theory. It forces the student of history to choose between a tautology and a disdain for theory, which has rendered the history trivial for all present purposes. Further, recent philosophers of science have severely undermined the distinction (Bloor, 1976; Kuhn, 1970). It is particularly necessary for social theorists to be aware of both the social and intellectual parameters of their own practices.

2. An alternative thesis

Our first thesis may be stated simply and directly: there was a readily identifiable discontinuity in economic thought in the 1870s and 1880s which was the genesis of neoclassical theory; and both its timing and intellectual content can be explained by parallel developments in physics in the mid-nineteenth century. The evidence is drawn from (i) the published works of the first neoclassicists; (ii) an example from the physics of the time which reveals the parallels; and (iii), biographical information about the principals.

All the major protagonists of the 'marginalist revolution' explicitly stated in their *published* works the sources of the inspiration for their novel economic theories. Jevons (1970, pp. 144–147) wrote that his equation of exchange does '... not differ in general character from those which are really treated in many branches of physical science'. He then proceeds to compare the equality of the ratios of marginal utility of two goods and their inverted trading ratio to the law of the lever, where in equilibrium the point masses at each end are inversely proportional to the ratio of their respective distances from the fulcrum. Note at this stage that Jevons' exposition does not adequately support his statements in the text: since he does not derive the equilibrium of the lever from considerations of potential and kinetic energy, he fails to justify the parallel between the expression for physical equilibrium and his use of differential equations in his own equations of exchange (see further, Section 5).

Far from being an isolated and insignificant metaphor, this invocation of the physical realm is always present in Jevons' writings on price theory. For example, in his defence of the mathematical method before the Manchester Statistical Society, he insists that

> Utility only exists when there is on the one side the person wanting, and on the other the thing wanted... Just as the gravitating force of a material body depends not alone on the mass of that body, but upon the masses and relative positions and distances of the surrounding material bodies, so utility is an attraction between a wanting being and what is wanted (Jevons, 1981, VII, p. 80).

When one observes that more than half of Jevons' published work concerns the logic and philosophy of science, one begins to see that the metaphor of physical science was the unifying principle, and not merely a rhetorical flourish. In his major book, *The Principles of Science*, he suggests that the notion of the hierarchy of the sciences justifies '... a calculus of moral effects, a kind of physical astronomy investigating the mutual perturbations of individuals' (1905, pp. 759–760). The reduction of social processes to simple utilitarian considerations is compared to the reduction of meteorology to chemistry and thence to physics, implying that there is only one scientific methodology and one mode of explanation—that of physics—in all human experience.

Léon Walras was equally explicit concerning the motivation behind his published work. In his *Elements of Pure Economics* he claims that, 'the pure theory of economics is a science which resembles the physico-mathematical sciences in every respect' (1969,

p. 71). Walras explains in great detail his occupation with 'pure economics' in Lessons One to Four of the *Elements*. In his opinion, a pure science is only concerned with the relationships among things, the 'play of the blind and ineluctable forces of nature' which are independent of all human will. Walras insists that there exists a limited subset of economic phenomena which could be the objects of a pure scientific inquiry: they are the configurations of prices in a regime of 'perfect competition' (for further elaboration see Mirowski, 1981). Such 'pure' relationships justify and indeed, for Walras, *demand* the application of the *same* mathematical techniques as those deployed in mid-nineteenth century physics; other social phenomena tainted by the influence of human will would be relegated to studies employing non-scientific rhetorical techniques.

The proposed unity of technique in physics and economics is fully revealed in Walras's article of 1909, 'Économique et Mécanique' (reprinted in Walras, 1960). In this article he develops the two favourite metaphors of the early neoclassical economists, the rational mechanics of the equilibrium of the lever and the mathematical relations between celestial bodies; he also asserts that the 'physico-mathematical science' of his *Elements* uses *precisely* the identical mathematical formulae. He then proceeds to scold physicists who had expressed scepticism about the application of mathematics to utilitarian social theories on the ground that utility is not a measurable quantum; Walras retorts that the physicists themselves have been vague in their quantification of such basic terms as 'mass' and 'force'. The proposed connections between the terms of the sciences could not have been made more manifest: 'Aussi a-t-on déjà signalé celles des *forces* et des *raretés* comme *vecteurs*, d'une part, et celles des *énergies* et des *utilités* comme *quantités scalaires*, d'autre part' (Walras, 1960, p. 7).

Francis Ysidro Edgeworth was a third partisan of 'mathematical psychics' who was quite explicit about the wellsprings of the neoclassical movement. If only because of his extravagant and florid writing style, he is worth quoting directly:

> The application of mathematics to the world of the soul is countenanced by the hypothesis (agreeable to the general hypothesis that every psychical phenomenon is the concomitant, and in some sense the other side of a physical phenomenon), the particular hypothesis adopted in these pages, that Pleasure is the concomitant of Energy. *Energy* may be regarded as the central idea of Mathematical Psychics; *maximum energy* the object of the principal investigations in that science... 'Mecanique Sociale' may one day take her place along with 'Mecanique Celeste', throned each upon the double-sided height of one maximum principle, the supreme pinnacle of moral as of physical science. As the movements of each particle, constrained or loose, in a material cosmos are continually subordinated to one maximum sub-total of accumulated energy, so the movements of each soul whether selfishly isolated or linked sympathetically, may continually be realising the maximum of pleasure... (Edgeworth, 1881, pp. 9, 12).

Vilfredo Pareto, a fourth confederate of the marginalist cadre, adopted a much more pugnacious but essentially identical position:

> Strange disputes about predestination, about the efficacy of grace, etc, and in our day incoherent ramblings on solidarity show that men have not freed themselves from these daydreams which people have gotten rid of in the physical sciences, but which still burden the social sciences... Thanks to the use of mathematics, this entire theory, as we develop it in the Appendix, rests on no more than a fact of experience, that is, on the determination of the quantities of goods which constitute combinations between which the individual is indifferent. The theory of economic science thus acquires the rigor of rational mechanics... (Pareto, 1971B, pp. 36, 113).

In some ways, Pareto was the most ruthless proponent of the physical metaphor, and because of this, found himself the first of the neoclassicals to have to defend himself from attacks by mathematicians and physicists (Volterra, 1971, pp. 365–396).

Once one recognises these passages for the manifestos that they are, one sees that they are ubiquitous in the writings of early neoclassical economists. They can be found in Fisher (1892), Antonelli (1886), Laundhardt (1885) and Auspitz and Lieben (1889). In fact, the explicit appropriation of this specific physical metaphor is present in every major innovator of the marginalist revolution, with the single exception (discussed later) of the Austrian school of Carl Menger. The adoption of the 'energetics' metaphor and framework of mid-nineteenth century physics is the birthmark of neoclassical economics, the Ariadne's thread which ties the protagonists, and which can lead us to the fundamental meaning of the neoclassical research programme.

3. Physics and economics

Historians of economic thought, and many other economists as well, have long been aware that there are some close familial resemblances between physical concepts and neoclassical economic theory (see Sebba, 1953; Lowe, 1951; Knight, 1956; Weisskopf, 1979; Samuelson, 1972; Thoben, 1982). The reason why these observations have passed without notice is that the extent and significance of the linkage has not been chronicled from the viewpoint of physics. For example, it has become a cliché to refer to neoclassical economics as being 'Newtonian', perhaps bolstered by some offhand assertions that both are atomistic, both have resort to the language of frictions and equilibrium, and, depending upon the disposition of the commentator, perhaps inclusion of a pejorative comment that both are 'mechanistic'. Indeed, if those observations exhausted the sum total of the analogy, then it would merit no further serious consideration. However, recourse to the history of mathematics and physics shows that the characterisation of neoclassical economics as 'Newtonian' is both inept and misleading.

Historians of science are increasingly sceptical of the conventional wisdom that the history of physics consists of two discrete periods: one, stretching from the sequence Galileo–Descartes–Newton to roughly 1895, called Classical Physics; and the second, a twentieth century phenomenon based on quantum mechanics and relativity. To quote a recent textbook:

> The term 'Newtonian' as applied to 18th and 19th century physics implicitly conflates Newton's natural philosophy and the physics of this later period, and is hence a misleading description. The developments in theoretical mechanics in the 18th century show a significant departure from the mechanical and mathematical assumptions of Newton's natural philosophy; and the physics of imponderable 'fluids', active substances and the anomalous forms of matter current in the 18th century contrasts with Newton's theory of nature... Despite the dominance of the program of mechanical explanation . . . the term 'Newtonian' is misleading when applied to physics in the 19th century. The conceptual innovations of 19th century physics—energy conservation, the theory of the physical field, the theory of light as vibrations of an electromagnetic ether, and the concept of entropy—cannot be meaningfully be described as 'Newtonian' (Harman, 1982, pp. 10–11).

In point of fact, the word 'physics' was not generally used in English until the middle of the nineteenth century to refer to the united study of mechanics, light, heat, etc., both because of its Aristotelian connections (Cannon, 1978, p. 113, *et seq.*) and because there was no consensus on a unified theory of these phenomena until the rise of energetics in the middle of the century. Problems with Newtonian concepts in the nineteenth century with respect to light, heat and electricity led to the proliferation of types of postulated matter and their associated separate attractions and repulsions, which in turn led to contradictions inherent in the idea of more than one Newtonian force (Agassi, 1971; Harman, 1982). Energetics as a unifying principle was created by Helmholtz's famous 1847 paper 'On the Conservation of Force' (Kahl, 1971), drawing upon earlier study of the conceptualisation of *vis viva* (or 'living force') and the interconvertability of heat and mechanical work. This innovation induced substantial revision of many previous physical doctrines, and created the discipline of physics as the unified study of phenomena linked by energetic principles.

This watershed in physics altered not only the subject matter but the techniques of research and methodological prescriptions as well. It was linked to the mathematical supersession by French analytical methods and Leibniz's notation for the calculus of the English use of the Newtonian calculus of fluxions and the English fondness for geometrical argument (Bos, 1980). It was accompanied by changes in the acceptable standards of theory formation: these included an increasing refusal to specify the underlying nature of phenomena described mathematically; fewer concessions made to intuitive plausibility; increasing imperatives to measure quantitatively without being precise as to what it was that was being measured; and a predisposition to accept the 'usefulness' of a model as a form of proof (Heidelberger, in Jahnke and Otte, 1981; Harman, 1982).

Crucial in this revolution in thought concerning physical processes was the transformation of vague 'forces' into a Protean, unique, and yet ontologically undefined 'energy', which could only be discussed cogently through the intermediary of its mathematical eidolon. In this guise, energy did not characterise Newtonian particles, but rather processes. It shifted the description of motion itself away from vectors such as momentum and towards scalars encompassing the new 'energy'. Its divergence from Newtonian concepts became apparent when the conservation law was enunciated, because the conservation law provided the only means by which to identify an energetic system as in some sense the 'same' as it underwent various changes and transformations (Theobald, 1966; Meyerson, 1962).

Some familiarity with the history of physics, even one as sketchy as that provided above, is necessary for an understanding of the fact that neoclassical economics was not prompted by a Newtonian analogy. Classical economists made reference to the Newtonian analogy in non-essential contexts (see Blaug, 1980, pp. 57–58); but they could not reconcile the inverse square law, the calculus of fluxions and other Newtonian techniques with their overall conception of social processes. The rise of energetics in physical theory induced the invention of neoclassical economic theory, by providing the metaphor, the mathematical techniques, and the new attitudes toward theory construction. Neoclassical economic theory was appropriated wholesale from mid-nineteenth century physics; utility was redefined so as to be identical with energy.

An example may make this clearer for the modern reader.

Consider a point-mass displaced a distance $q(=AB)$ in a two-dimensional plane, as in Fig. 1, by a force vector $\mathbf{F}$. The force vector can be decomposed into its perpendicular components, $\mathbf{F}=\mathbf{i}F_x+\mathbf{j}F_y$ (where $\mathbf{i}$, $\mathbf{j}$ are the unit vectors along the appropriate axes). Similarly, the vector of displacement can also be decomposed into its components, $d\mathbf{q}=\mathbf{i}dx+\mathbf{j}dy$. The work done (i.e. the product of the force and distance moved along the path of its action) in the instance of this displacement is defined as the summation of the forces times the displacements, or:

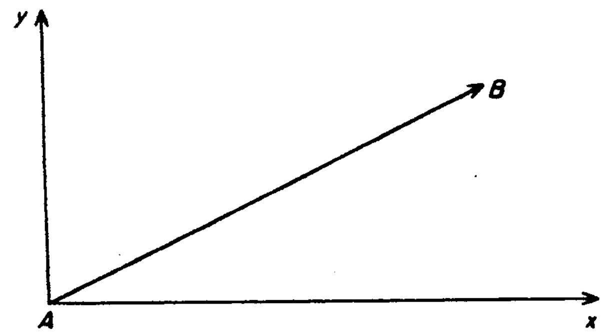

Fig. 1

$$T=\int_{A}^{B}(F_x dx+F_y dy)=\tfrac{1}{2}m|\mathbf{v}|_B^2-\tfrac{1}{2}m|\mathbf{v}|_A^2$$

Energetics redefined the change in mv^2 (i.e., the *vis viva* of the particle) to be the change in kinetic energy. The vector characterisation could then be translated into a single-valued scalar function, T. In the eighteenth century, there had been much controversy over whether the *vis viva* was conserved; this issue was clarified in energetics in the following manner. Suppose that the above expression in the parentheses were an exact differential; in other words, there existed a function U such that:

$$F_x=-\partial U/\partial x;\ F_y=-\partial U/\partial y;\ U=U(x,y).$$

This uniquely identified scalar function U was interpreted as the unobserved potential energy of the particle. Then it is the *total* energy of the particle, $T+U$, which is conserved through any motion of the particle. The postulate that total energy is conserved was significant, because it allows a rigorous specification of the 'principle of least action'. This principle, in its various forms, dated back to Maupertuis in the eighteenth century, who noted that the actual paths of motion traversed in many mechanical phenomena could be described mathematically as evincing the minimum of the particle's 'action'. William Hamilton in the 1830s pioneered 'the central conception of all modern theory in physics' (Schrödinger, quoted in Crowe, 1967, p. 17) by defining the action integral over time of the path of a particle as:

$$\int_{t_1,A}^{t_2,B}(T-U)\,dt.$$

The Hamiltonian principle of 'least action' asserts that the actual path of the particle from A to B will be the one which makes the action integral stationary. The path may be calculated by finding the constrained extrema, employing techniques of Lagrangean constrained maximisation/minimisation or, in more complicated cases, using directly the calculus of variations. In a conservative system, where $T+U=$ a constant, action is a function of position only, which implies that all motion is fully reversible, and exhibits no hysteresis (Kline, 1972, ch. 30).

To summarise: in the 1820s theoretical treatises in mechanics began to stress the work integral and its mathematical relationship to *vis viva* (Harman, 1982, p. 36). In the 1830s, Hamilton linked this framework to the mathematics of constrained extrema (Hankins, 1980). Starting in the 1840s, the interconvertability of mechanical energy and other energetic phenomena was postulated; by the 1860s, the mathematics of unobservable potentials and *constrained extrema* were extended to all physical phenomena.

Walras insisted that his *rareté* equations resembled those of the physical sciences in every respect. We may see now that he was very nearly correct. Simply redefine the variables of the earlier equations: let **F** be the vector of prices of a set of traded goods, and let q be the vector of the quantities of those goods purchased. The integral $\int \mathbf{F}.\mathrm{d}\mathbf{q} = T$ is then defined as the total expenditure on these goods. If the expression to be integrated is an exact differential, then it is possible to define a scalar function of the goods x and y of the form $U = U(x,y)$, which can then be interpreted as the 'utilities' of those goods. In exact parallel to the original concept of potential energy, these utilities are unobservable, and can only be inferred from theoretical linkage to other observable variables. Relative prices are equal to the ratios of the marginal utilities of the goods by construction: the 'potential field' of utility is defined as the locus of the set of constrained extrema, although the early marginalists reversed this logic in their expositions of the principle. Instead of treating utility as a derived phenomenon, they postulated the utility field as the fundamental exogenous data to which market transactions adjusted. The mathematics, however, are the same in both instances.

There *is* one major difference, however, between the mathematics of energetics and its transplanted version in neoclassical economics. The conservation principle in energetics does not translate directly into neoclassical theory: the sum of income and utility is not conserved, and is meaningless in the context of economic theory. Does this mean that neoclassical economics has managed to dispense with the artifice of a conservation principle? This may appear to be the case, because neither the progenitors of neoclassicism nor any of its modern adherents have ever seriously discussed this aspect of the physical metaphor (see Mirowski, 1984C). Yet to cast any problem in a constrained maximisation framework, the analyst *must* assume some sort of conservation principle. In physics, it is widely understood that the conservation principle is the means by which the system being considered retains its analytical identity.

In other words, the adoption of the energetics metaphor in economics has imposed an analytical regimen, the rigours of which have hitherto gone unnoticed. Neoclassical theorists, from the 1870s onwards, have surreptitiously assumed some form of conservation principle in their economic models. In the period of our present concern, the principle took two forms: (a) the income or endowment to be traded is given exogenously and, further, is assumed to be fully spent or traded; thus, for practical purposes, T is conserved; and/or (b) the transactors' estimation of the utility of the various goods is a datum not altered by the sequence of purchase, nor any other aspect of the trading or consuming process (or, as Marshall sheepishly admitted, desire was equated with satisfaction *by assumption*); so in effect the utility field U is conserved (see Mirowski, 1984C). In this case, the analogy between physics and economics would be as if physical theory had managed to preserve what has proved to be an anachronistic element: as if Hamilton had somehow managed to preserve the conservation of *vis viva* (kinetic energy) within the new mathematics of energetic extrema.

Once the parallels between mid-nineteenth century physics and neoclassical economic theory are outlined, and it is acknowledged that the progenitors themselves openly admitted them in their published writings, most would accept the thesis that the 'marginalist revolution' should be renamed the 'marginalist annexation'. Should doubts linger, however, the thesis should be clinched by an examination of the biographical particulars of the protagonists.

The most obvious and straightforward case is that of the most respected of neoclassical progenitors, Léon Walras. In his first effort to mathematicise his father's concept of *rareté*, Walras attempted to implement a Newtonian model of market relations, postulating that 'the price of things is in inverse ratio to the quantity offered and in direct ratio to the quantity demanded' (Walras, 1965, I, pp. 216–217). Dissatisfied with this model, Walras tinkered with various formulations, but none involved the constrained maximisation of utility until the late autumn of 1872. At that time, a professor of mechanics at the Academy of Lausanne, Antoine Paul Piccard, wrote a memo to Walras sketching the mathematics of the optimisation of an unobserved '*quantité de besoin*' (Walras, 1965, I, pp. 308–311) along the lines outlined above. Although Walras trained originally as an engineer at the École des Mines, he did not possess a deep understanding of the new energetics: this can be opbserved in his reactions to the letters of Charles Émile Picard (Walras, 1965, III, pp. 417–420) correcting his errors of interpretation and mathematical representation. While these letters did prompt him to write 'Économique et Mécanique', they did not prompt him to revise his *Elements* significantly. This suggests that Walras did not comprehend the real thrust of these letters, which question the appropriateness of various aspects of the physical metaphor. It was left for his successors Antonelli and Pareto to explore some of the *social* implications of the mathematics of energetics.

It is significant that all the earliest members of the Lausanne school were trained as engineers. Giovanni Antonelli was an Italian civil engineer whose monograph *On the Mathematical Theory of Political Economy* explicitly discusses utility theory in the manner described above (pp. 366–368) (Antonelli, 1886). He is now considered a pioneer in the problem of integrability, which here we interpret as an acknowledgement and extrapolation of the implications of conservation principles. The significance of this problem did not receive widespread attention until well into the twentieth century (Samuelson, 1950). Vilfredo Pareto was also trained as an engineer, and this expertise enabled him to explore the implications of the path-independence of the realisation of utility, a direct extrapolation of the path-independence of equilibrium energy states in rational mechanics and thermodynamics (Pareto, 1971A). This work was consigned to oblivion partly because Pareto and Antonelli gave up economic theory in later life, and partly because no one outside a very limited circle of engineers who had a working knowledge of the new economic theory could read it. The English-speaking world had to wait until the 1930s when an influx of physicists—and engineers—manqués into economics led to the revival of their work.

The biographical evidence in the case of Jevons is not as direct, but is substantial. Prompted by his father to become an engineer, Jevons studied chemistry and mathematics in London. He attended some of Michael Faraday's renowned public lectures at the Royal Institution, at which Faraday claimed that magnetic forces did not obey the Newtonian force rule (Jevons, 1972, I, p. 82). This is significant because in the land of Newton in the 1850s Faraday was one of the very few partisans of field theories and energetics: indeed, Jevons' letters make clear his enormous respect for Faraday. We also have evidence that Jevons was familiar with the writings of Thomson and Joule on the interconvertability of heat and mechanical work, writings which led to the enunciation of the theory of the conservation of energy (Jevons, 1972, II, p. 66). Later in his life Jevons remained conversant with the field of energetics, and even wrote to James Clerk Maxwell arguing a point of controversy in Fourier's theory of heat (Jevons, 1972, IV, pp. 207–208).

If there was a difference between Jevons and Walras, it was this: Walras did not evince any deep understanding of mid-nineteenth century physics, and applied the mathematical techniques and the metaphor in a mechanical and unimaginative manner, leaving it for others to draw out the logical and connotative implications of the physical metaphor. Jevons, on the other hand, was even less of a mathematician than Walras, but did dedicate his life's work to drawing out the meaning of the metaphor of energetics for the sphere of the economy. This point is not readily apparent, because Jevons' work is rarely considered as a whole. His major achievements were the *Theory of Political Economy*, *The Coal Question*, his work on sunspots and the business cycle, and *The Principles of Science*. The connection between the four can best be summarised in Jevons' own words, from his paper 'The Solar Influence on Commerce' (Jevons, 1972, VII, p. 97): 'Long ago George Stevenson acutely anticipated the results of subsequent scientific inquiry when he said that coal was sunshine bottled up; now it is among the mere commonplaces of science that all motions and energies of life . . . are directly or indirectly derived from the sun.' The maximisation of utility, the prediction that England was rapidly exhausting energy stocks in the form of coal, and the lifelong theme that economic crises must be caused by energy fluctuations exogenous to the social operation of the economy, are all direct extrapolations from the energetic movement of the mid-nineteenth century (Mirowski, 1984A). The last point gains credibility when one notes that Jevons recorded in his journal that Faraday explicitly discussed the periodicity of sunspots in his lectures of 1853 (Jevons, 1972, I, p. 82). As for the *Principles of Science*, it can be read as a plea for the unity of methodology in all sciences, in the face of the serious upheavals and discontinuities which erupted both in subject matter and in research methods in mid-nineteenth century physics. The fact that his own conception of scientific endeavor was highly coloured by the rise of energetics can be observed in the *Principle's* definition of science: 'Science is the detection of identity, and classification is the placing together, either in thought or in the proximity of space, those objects between which identity has been detected' (Jevons, 1905, pp. 673–674).

4. The Austrians were not neoclassicals

Those familiar with conventional histories of neoclassical economic theory must, by this point, be impatient to object: what about Menger and the Austrians? Do they fit the thesis which links the rise of neoclassical theory to the rise of energetics in physics?

Although it has become conventional wisdom to cite the triumvirate of the marginal revolution as Jevons, Walras and Menger, these three actors themselves did not accept this regimentation. Jevons did not mention Menger once in all his writings: a curious reticence in one so determined in later life to uncover all predecessors and fellow revolutionaries. Walras did correspond with Menger, but only to discover to his amazement that Menger did not recognise his contribution on account of its mathematical nature. This was sufficient for Walras to deny Menger's role in the revolution, writing in a letter to Bortkiewicz in 1887 that Menger's and Bohm-Bawerk's efforts to describe the theory of 'Grenznuten' in 'ordinary language' was unsuccessful, and even painful (Walras, 1965, II, p. 232). Walras viewed Menger's 1871 *Principles* as merely an attempt at *translation* of marginalist ideas into ordinary language, and a failed one at that: there was nothing novel or original there; he thus denied Menger any status as an equal. (Interestingly enough, this opinion seems to be shared by many modern neoclassical economists. In this regard, see Samuelson, 1952, p. 61.) Menger did not conform to

Walras's main criteria for a neoclassical theorist: he was not mathematical, he did not adhere to the norms of physical science, and therefore he was not 'scientific'.

In contrast, historians of economic thought are persistently perplexed by Menger's recalcitrance at being elevated to membership in the triumvirate. Howey, the most careful of these writers, notes:

> ... although Menger talked about the Austrian school, no one would gather from his words in any of his publications after 1871 down to his death that the Austrian School had the slightest connection with the Marginal Utility School. He either did not admit the connection, or wished to minimise it, or took it for granted. Menger never publicly admitted any kinship with Walras or with Jevons (Howey, 1960, p. 142).

There is much more here than petty squabbles over precedence or methodology, or personality clashes, or nationalistic insularity. There is the possibility that the Austrians, or at the very least Menger, were not part of the fledgling movement of neoclassical economic theory. This possibility has already been suggested by some Austrian economists, notably by Erich Streissler in a centenary collection of essays on the marginalist revolution (Black, Coats and Goodwin, 1973, pp. 160–175). Streissler points out that Menger's scales of successive marginal satisfaction, introduced in the middle of his *Grundsätze* (Menger, 1981, p. 127), were not at all central to his conception of economic theory. This contention is indirectly supported by Kauder (1965, p. 76), who reports that Menger crossed out this table in his author's copy of the book. Howey (1960, p. 40) notes that Menger's 'importance of satisfactions' cannot easily be translated into the language of utility because it did not vary in *quantity*. 'Satisfaction' never varied, but its subjective importance could be altered in a regular manner. Streissler maintains that Menger's major concerns—uncertainty, changes in the quality of goods, the absence of a notion of equilibrium, and hostility to the 'law of one price'—were motivated so fundamentally by his radical subjectivism that he could not be considered as promoting the same theory as Jevons and Walras. From our present perspective, we can find support for Streissler's thesis by examining Menger's relationship to physical theory.

After a personal visit, Bortkiewicz wrote to Walras that Menger did not have the least idea of mathematical analysis (Walras, 1965, II, p. 519). Perusal of his major works indicates that he was also unfamiliar with the physics of his time. Yet despite these inadequacies, Menger launched a scathing attack upon the German historicist school in his *Untersuchungen über die Methode*, mainly consisting of the contention that his opponents did not understand the nature of 'exact science' (Menger, 1963). In sharp contrast with Jevons' *Principles of Science*, Menger's weak and unconvincing claims that he was promoting the methods of 'exact research of a Newton, Lavoisier or Helmholtz' reveal an ignorance hastily camouflaged by bombast. He attempted to extend his radical subjectivism to physics without giving a single example from the physical sciences. He denigrated empiricism without being specific about the practices to which he objected. His conception of science was severely Aristotelian and he never addressed the fact that the scientists of his day had rejected this. He rather appropriated their names for credibility.

Menger cannot be considered a neoclassical economist because he rejected two basic pillars of that theory: the law of one price, which states that all generic goods in a market (however defined) must trade at the same price in equilibrium (see Dennis, 1982); and the concept that traded goods in some sense are related as equivalents in equilibrium (Menger, 1981, pp. 191–194). Absence of the first subverts any deterministic notion of equilibrium. Absence of the second explains Menger's hostility towards quantification. Absence of both effectively prevented the introduction of the physics analogy into economic theory. The mere postulation of a diminishing marginal utility is not sufficient to generate a neoclassical theory of price. In this respect Menger is no different from Dupuit (1952), who also recognised diminishing marginal utility, but also repudiated a single equilibrium price. Were it not for three historical accidents—first, the *Grundsätze* was first published in 1871; second, Menger's illustrious student Wieser promoted his claim to be a founder of neoclassical theory (and himself *did* adopt the new marginalist techniques from Laundhardt and Auspitz and Lieben); and third, Menger's works were largely unavailable outside the German-speaking world—Menger would not today be considered as one of the marginalist revolutionaries.

There has been much disagreement as to what constitutes the 'hard core' of neoclassical economic theory: the fundamental basis of the research programme which, if altered, would signal the substantive development of a non-neoclassical economic theory (Latsis, 1976; Boland, 1982). The core is not simply methodological individualism, nor is it utilitarianism, because both were active research strategies in social theory well before the rise of neoclassical theory, and because the Austrian and certain sociological research programmes also hold them as tenets. It is the second thesis of this paper that the hard core of neoclassical economic theory is the adoption of mid-nineteenth century physics as a rigid paradigm, a hard core it has preserved and nourished throughout the twentieth century, even after physics has moved onwards to new metaphors and new techniques. This thesis explains a number of issues which have eluded other attempts at locating the hard core of neoclassical theory.

First, it explains why neoclassical theory and mathematical formalism have been indissolubly wedded since the 1870s, even though a cogent defence of the necessity of the link has been notable by its absence. Second, it explains the success of neoclassicism in pre-empting other research programmes in economics by means of the forceful claim that it is scientific, even though standards of scientific discourse in the larger culture have changed periodically during the last hundred years. Third, it explains the preference for techniques of constrained maximisation over any other analytical techniques, which include input–output matrices, game theory, Markov chains, and a myriad of other techniques proposed over the last century (Samuelson, 1972). Fourth, it explains the persistent use of an unobservable and unmeasurable value determinant—utility—in textbooks and in applied research, despite protestations that utility is not 'needed' for neoclassical results (Wong, 1978). Fifth, it explains the modern controversy over the necessity for a 'microfoundation for macroeconomics', which can be interpreted as a complaint that Keynesian economics has not conformed to the hard core research strategy, and is therefore somehow illegitimate (Weintraub, 1979; Lucas, 1981). Sixth, it explains why neoclassicism links certain economic variables to particular exogenous variables, which are themselves 'naturally' determined and therefore analytically immutable and outside of the scope of economic theory. All these characteristics are borrowed from nineteenth century energetics.

5. Physical metaphors, organic metaphors and the role of Marshall

It is not unusual for a science to adopt the metaphors and/or analytical techniques of another discipline. The story of Darwin's appropriation of the concept of population pressure on resources from Malthus's *Essay on Population* is but one example of a pervasive phenomenon. Indeed, some historians of science attempt to explain the rise of energetics by the influence of German *Naturphilosophie* in mid-nineteenth century culture (Kuhn, 1977). What is unusual and noteworthy about the rise to preeminence of neoclassical economic theory is the lack of consciousness, and therefore the concomitant lack of any assessment or critique, of the sources of its analytical and technical inspiration. Newtonian action-at-a-distance came under severe scrutiny and criticism from philosophical perspectives in the eighteenth and nineteenth centuries. Darwinian natural selection has repeatedly been reconsidered at the level of the fundamental organising metaphor. The list could be extended indefinitely: many of the basic organising principles of physics have undergone criticism and revision over the past two hundred years. All these episodes reveal a willingness to reconsider theory at the level of the 'hard core', as opposed to revision of the 'protective belt'. In effect, the strength of physics lies in its openness to fundamental revision, and not, as the naïve conception has it, in its unwavering preservation of eternal verities.

Neoclassical economists, on the other hand, have often appealed to the dignity of the scientific endeavour, without understanding what it entails, or why they felt justified in claiming privileged scientific status for their paradigm. Until Georgescu-Roegen (1971), the extent of the dependence of modern neoclassical theory upon the physical metaphor had not even been surveyed seriously. What is still missing is a preliminary balance sheet of the gains and losses from adherence to this research strategy.

For early neoclassical theory, one can compile a condensed set of accounts. On the credit side, the main object of the early marginalists has been achieved: the abolition of the *anomie* and the lack of systematic theory of mid-nineteenth century political economy, and the creation in its place of a shared research programme with shared goals, as well as a well-defined set of research techniques. Attention moved away from broad and ill-defined growth and development issues to a much narrower set of concerns tethered to the notion of short-period equilibrium price (Garegnani, 1976). Systematic empiricism was encouraged by a shift in focus to certain easily quantifiable variables. The discipline of economics was divided up into a set of subfields, both theoretical and applied, which could provide researchers with a clearly defined expertise and thus identity. This played an important part in the growing professionalisation of economics in the later nineteenth century, guaranteeing it a secure place in the academic environment (Checkland, 1951). In other words, the appropriation of the physical metaphor effectively appropriated credibility for economics as a respected science.

The debit side of the account is more subtle, and so more contentious. Perhaps the major debit entry is the fact that the early neoclassicals themselves did not adequately understand the physical metaphor and the constraints which it imposed upon social theory. For example, Jevons did not explicitly derive the equilibrium of the lever from energetics principles in his *Theory of Political Economy*, thus leaving him open to ridicule by Marshall, who jeeringly suggested in a book review that he try to integrate his equation of exchange (Jevons, 1981, VII, p. 145). With the single exception of Marshall, all the early neoclassicals used the energetics metaphor; no other economists understood enough physics to discuss its implications and flaws.

Yet consider a short impressionistic list of these flaws. First, all energetics before the second law of thermodynamics (the entropy law) presumed that all phenomena were perfectly reversible, and thus equilibrium could not be time-dependent. In pre-entropy physics, history does not matter. The conservation principle is crucial in this respect, because it defines identity through time. When this metaphor is imported into the social sphere, it implies that in equilibrium byegones are byegones; thus one could practically ignore how a market actually functions in real time, paying attention only to putative 'eventual' outcomes. Hicks (1979) and Shackle (1967) are the latest in a long line of illustrious figures to complain about this issue; but their complaints have not made any substantive headway because they have not seen how deeply rooted this principle is in neoclassical techniques. Second, something must be conserved in order to apply the techniques of constrained extrema, the 'maximum principle'. When the physical metaphor is imported into the social sphere, neoclassicists were not at all precise about what the conserved entity was, and they have not yet been able to settle this issue (Mirowski, 1984C). If utility is conserved, then surprise and regret as psychological phenomena have analytically been ruled out of court. If income or endowments are conserved, then Say's Law is implicitly invoked, and there is no theory of output other than a psychological notion of 'virtual' production (Clower, 1970). Third, in energetics, all physical phenomena are fully and reversibly transformable into any other phenomena. When this idea is transported into the context of the economy, then all goods become fully and reversibly transformable into all other goods through trades. There is no requirement for a specific money commodity or set of financial institutions, because they would be redundant. The analogue of energetics is the barter economy. Fourth, equilibrium is identified with extremum principles in physics because they provide a concise method of summarising the actual path of particles in empirical experience. When the metaphor is imported into economics, the use of extremum principles is claimed to 'prove' the superior efficacy of a particular kind of economic organisation. Physics long ago renounced this teleological interpretation; economics has come to embrace it.

If contemporaries had understood what kind of economy the energetics metaphor described, then neoclassicism would have met substantial logical opposition. We may infer this from the fact that when the physics metaphor was explicitly introduced into the social sphere in other contexts, it met with strenuous opposition (Sorokin, 1956, ch. 1). But this is where economics is the anomaly in the history of social theory: because the 'inventors' did not understand energetics or the social metaphor with any great depth or subtlety, they rarely discussed the merits or demerits of the application of physical techniques and metaphors to social theory. No other economists understood enough physics to see its implications; nor were they induced to study physics by any of the writings of the early marginalists. Effectively, neoclassical economic theory was a *fait accompli* whose origins and fundamental bases were buried by historical accident, to the extent that the sources of inspiration of Jevons, Walras, Pareto, *et al.* could appear as a puzzle to their posterity.

It should not appear from my summary that the entire economics profession were sleepwalkers, stumbling unwittingly into a maze of energetics. Alfred Marshall, for one, certainly discussed some aspects of the adoption of physical metaphors (Marshall, 1898); and he clearly had some reservations. However, the case of Marshall is actually illuminated by an understanding of energetics.

Marshall's place in the history of economic thought has always been a curious one. He hinted, both privately and in print, that many of Jevons' ideas had been 'familiar truths' to him when they were published, thus intimating that somehow he also deserved 'discoverer' status. Since much of what appears in introductory and intermediate microeconomics texts as the theory of supply and demand is, in fact, the handiwork of Marshall, there is a grain of truth in his claim. However, once the actual sequence of events is uncovered, it appears that Marshall's major service in the marginalist revolution was as a populariser; and, like other popularisers, he altered the material which he promoted.

Recent study of Marshall's early unpublished writings, especially by Bharadwaj (1978), reveals that his early work was on the equilibrium of a supply curve with a phenomenological demand curve: he did not much care what lay behind his demand schedule. Implicitly, movements along the demand curve came from variations in the number of buyers, rather than a posited constrained maximisation by an individual buyer. 'The word "utility" itself was used only once in relation to Adam Smith, and not approvingly' (Bharadwaj, 1978, p. 367).

The saga of the journey between Marshall's early *Essay* and his *Principles* is the story of a decision to incorporate the innovations of the marginalist revolutionaries in order to shore up the foundations of the demand blade of the 'scissors', while preserving his original concerns with the underlying theories of the supply schedule. Unhappily, the superficial parallels between diminishing returns and diminishing marginal utility could not obscure the fact that the result was more like paper and stone rather than scissors. For example, much of Marshall's typology of markets involved altering of the timeframe of analysis and deriving its resulting effects upon the supply schedule. This method produced some embarrassment when applied to the demand side, either because the underlying demand determinants remained constant over time, revealing that the fundamental cause of price was an exogenous posited psychology, as Jevons had maintained, or because the demand curve would also be shifted in relatively arbitrary ways, undermining any claim that an equilibrium of demand and supply had been identified. Perhaps it was predictable that the attack would be pressed against the part of the system which Marshall originated (Sraffa, 1926), and that the ensuing retreat would vindicate Jevon's position.

Marshall sensed that his concerns could be overwhelmed by the zeal of his marginalist allies, and this partly explains why he does not conform in style to the characteristics of the marginalist cadre identified above. His defence of Ricardo *vis-à-vis* Jevons; his soft-pedalling of the mathematical method; his insistence on the basic continuity of economics from Adam Smith to his time; his persistent praise of organic metaphors: all these activities are attempts to incorporate energetics into economics while controlling or perhaps altering some of its more objectionable aspects. Many wave as a banner Marshall's claim that, 'The Mecca of the economist lies in economic biology', but few bother to quote the next sentence: 'But biological conceptions are more complex than those of mechanics; a volume on Foundations must therefore give a relatively large place to mechanical analogies . . .' (Marshall, 1920, p. xiv). However much he might protest, the fact remains that Marshall did render the energetics metaphor palatable for an English audience which would probably have resisted the brash revolution of a Jevons. Further, he fostered the illusion that 'The new doctrines have supplanted the older . . . but very seldom have subverted them' (Marshall, 1920, p. v).

It is important to appreciate that Marshall thought that the physical interpretation could be separated from the mathematical technique, and that his reservations lay in the interpretation rather than the technique. Those who happily quote Marshall's dictum to 'burn the mathematics' should read carefully the preface to the eighth edition of the *Principles*:

> The new analysis is endeavouring gradually and tentatively to bring over into economics, as far as the widely different nature of the material will allow, those methods of the science of small

increments (commonly called the differential calculus) to which man owes directly or indirectly the greater part of the control that he has obtained in recent times over physical nature. It is still in its infancy; it has no dogmas, and no standard of orthodoxy ... there is a remarkable harmony and agreement on essentials among those who are working constructively by the new method; and especially among such of them as have served an apprenticeship in the simpler and more definite, and therefore more advanced, problems of physics (Marshall, 1920, pp. xvi–xvii).

But of course there was dogma and a standard of orthodoxy: that was why agreement had been achieved relatively quickly by the mathematical workers; the standards and ideas had been appropriated during their apprenticeship in physics. The *Principles* is a book that touts the mathematical method while attempting to deny that the method could influence the content of what was being expressed. The clearest manifestation of this tension occurs in the Appendix to the *Principles*, where, in the midst of a series of abstruse notes concerning the application of constrained maximisation to utility, there is an incongruous discussion of the applications of Taylor's Theorem to the webbing between a duck's appendages (Marshall, 1920, pp. 841–842). The purpose of the digression is to suggest that the calculus was being borrowed from an organic evolutionary metaphor. Not only did Taylor's Theorem have nothing to do with the duck's webbing in Marshall's actual example; but the calculus of constrained maximisation was not employed by evolutionary theorists in Marshall's day.

6. The history of economic thought as an active generator of research programmes

The energetics metaphor can be found in every major neoclassical theorist of the nineteenth century and can be used to explain some controversies in the history of economic thought. It is a very neat pattern; perhaps too neat. Is it being too wise after the event in defining neoclassicism tautologically as coextensive with the introduction of the physics metaphor into social theory, and then brushing other authors aside? I do not think so. This article merely points out what has been there for all to see in published writings, biographies, and the history of science.

This paper has *not* specified why the energetics metaphor was so attractive to nineteenth century economic theorists, or discussed why the economics tail still is or is not wagged by the physics dog. Such omissions are due in part to prosaic reasons of space limitation, but also to the fact that such a discussion requires a much larger original content and a grounding in the philosophies of science and theory choice. The philosophy of science is so important because it indicates where to begin searching for acceptable explanations of the adoption of the physics metaphor. Should we look to the level of personal motivation or structural tendencies? Should we look to empirical inadequacies or logical flaws, or some less rigid intellectual influences? These questions give rise to a research project, which could be carried out at many different levels: the level of individual desires (e.g., Jevons' personal motivations (Mirowski, 1984A), that of individual influences (e.g. Edgeworth's family were friends of Hamilton), that of class interests, that of the sociology of professions (here the location of economists in universities), that of the cannons of empiricism (the rise of quantification as a preferred empirical technique), that of the status of alternative competing research programmes (say, the dilution of the Ricardian programme by Mill and the retreat of the labour theory of value), and that of metaphysical predispositions in the larger culture [e.g., the western tendency to see social relations as rooted in 'natural' processes (Levine, 1977)].

Another reason why modern philosophy of science is important is that it has highlighted the significance of the history of science. Discussion of the above issue can be cogently prosecuted only in conjunction with the study of the actual (as opposed to mythical) history of mathematics, physics, etc. Only then would we be able to extend the inquiries into the twentieth century with questions like: what is the relation between the penetration of input–output methods into economics and the preceding rise of matrix methods in quantum mechanics? What is the link between Niels Bohr's 'Correspondence Principle' and that of Paul Samuelson? Another question of interest concerns the relation of mathematical technique to model content. Did mathematical economic theorists before 1870 'fail' because they were inept, or for other more profound reasons (Mirowski, 1984B)?

Finally, we can clarify the issues broached at the outset. The antiquarian question has been settled: neoclassical economic theory is bowdlerised nineteenth century physics. The epistemological issue has been illuminated: present research techniques may be favoured *because* they were appropriated from physics. The ontological issue has been reinterpreted: neoclassicism was not 'simultaneously discovered' because it was 'true', as Jevons and others would have it; instead, the timing of its genesis is explained by the timing of the energetics revolution in physics, and by the fact that scientifically trained individuals in different Western European countries at that time had access to the same body of knowledge and techniques. The practical issue, however, has scarcely been addressed. One cannot predict where new theories will come from, but one can venture a broad inductive generalisation from past patterns: that a substantial non-neoclassical economic theory will distinguish itself by consciously repudiating the energetics metaphor.

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