

## **More ideology than heat – Mirowski’s “More Heat Than Light” revisited**

The core argument of the paper is that neo-classical general equilibrium theory (GET) is not a “bad” scientific theory in the form of physics envy, it is an ideology serving the interests of economic elites in a capitalist society. The paper critically examines another view that GET is a form of Physics envy – the core argument of Philip Mirowski’s book from 1989 with the suggestive title “*More Heat Than Light: Economics as Social Physics, Physics as Nature’s Economics*”. Mirowski’s book was the first to argue this “natural science envy” view of main-stream economic theory. More recently Tony Lawson (1997, 2003, 2006) have consistently argued that main-stream economics is obsessed with math. Lawson explicitly argued against viewing GET as an ideology. Mirowski does not. The question is outside his radar “More Heat than Light” (“More Heat than Light”). The way I want to substantiate my view is to show that Mirowski himself deliver the most forcible argument against his own thesis. It is no coincidence that the penultimate chapter is entitled “The ironies of physics envy”, because the “envy” hypothesis clearly does not fit that well to the facts. Mirowski’s core argument is that neo-classical economics became controlled by a principle in early physics – the principle of conservation of energy. Mirowski’ shows in detail how the early neo-classical marginalists borrowed metaphors and mathematical tools from late nineteenth century physics. This happened both because in most peoples opinion physics is *the* role model of science and since economics ought to be(come) a science it had consequently to – not exactly imitate – but be modelled on physics.

Mirowski’s thesis is that this early “proto-energetic” model of physics has determined to a very large degree neo-classical economic theory from the marginalist founding fathers (Walras, Jevons, Edgworth) and down to the present – a prime post-WWII example being Paul Samuelson. In Mirowski’s opinion economics suffers from century long physics envy. Mirowski’s problem is that neo-classical theory – as he himself is the first to point out – did not and do not slavishly imitate – or creatively apply – neither the conceptual or mathematical developments of physics. This is obvious from the very beginning, that is last decades of the nineteenth century, but the split is becoming more and more obvious as relativity theory, entropy, path dependency, chaos theory become important in modern physics – as Mirowski points out with great detail and clarity.

Not only that, the founding fathers of the neoclassical school, Walras and Pareto, did get friendly warnings and critique from contemporary physicist that their conceptual parallels between energy and value (utility) were inadequate if not directly misleading, that their static equilibrium models could not explain the dynamic phenomenon that a capitalist market economy is. In my opinion

Mirowski is not able to handle this as some stubborn resistance by the marginalists to this – in most cases – friendly – advice. This is because this resistance runs counter to Mirowski’s fundamental envy/imitation thesis. The resistance remains unexplained because Mirowski do not contemplate that the neo-classicals had another agenda which was more important namely to create an alternative to the labour theory of value and to create a theory that proved some for them very important – and by now very well-known - “results”. The marginalists wanted to prove that any interference with “perfect competition” created inefficiency – so no unions, no progressive taxation, no protective tariffs, the least possible of health, safety, stock market regulations. So although Mirowski is highly critical of neo-

classical economics as scientific theory he does not discuss its *political* use over the last hundred years.

Let it clear from the start that I do not contest that the marginalists did look to physics, did borrow. Mirowski proves that beyond any doubt. The point is that it is a big difference between borrowing methods and concepts out of naïve or misguided admiration and to borrow in order to pose as a science. In my opinion the latter is clearly the case. The prime example – as Mirowski shows – being Paul Samuelson. Having now outlined the essence of my critique of Mirowski let's turn to the facts, in the form of quotations from "More Heat than Light".

### **The first phase – the physics and the marginalist revolution**

In the first chapter Mirowski outlines the start of the whole story:

'The Marginalists appropriated the mathematical formalisms of mid-nineteenth-century energy physics, which for convenience we shall refer to as "proto-energetics", made them their own by changing the labels on the variables, and then trumpeted the triumph of a truly "scientific economics". Utility became the analogue of potential energy; the budget constraint became the slightly altered analogue of kinetic energy; and the Marginalists Revolutionaries marched off to do battle with classical, Historicist and Marxian economics' (p. 9)<sup>1</sup>

Already here we get a clear statement that the Marginalists knew whom – and which *value* theory they were fighting against – the labour theory of value. But as Mirowski immediately points out:

'Unfortunately, there had been one little oversight: The neoclassicals had neglected to appropriate the most important part of the [physics] formalism ... namely the conservation of energy. This little blunder rendered the neoclassical heuristic essentially incoherent; but heedless of that fact, the Marginalists triumphed under their banner of Science.' (p. 9)

But as we shall see below, there was more than one little blunder, there was a stubborn resistance to be really scientific as Mirowski points out – as he does throughout "More Heat than Light":

'Physicists have in the past displayed a dynamism and flexibility with regard to the meaning of their metaphors for which they can rightfully be proud. But with economics<sup>2</sup> it is another story. Economists have consistently lagged behind physics in developing and elaborating metaphors; they have freeloaded of physicists for their inspiration, and appropriated it in a shoddy slipshod manner.' (p. 108)

The unavoidable question becomes – why were the neoclassical economists so shoddy and slipshod? Could it really be that generation after generation of neoclassical economists did not grasp physics? That none of them never "really understood the conservation principles"? (p. 10). After all Mirowski understands them, and give an elaborate résumé in chapter 2. But before we go on to answer that – and related questions, let's have a restatement of the physics envy thesis in light of value theory. Mirowski poses three questions as the starting point for a value theory:

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<sup>1</sup> All page numbers refer to "More Heat than Light", Mirowski (1990), if not otherwise stated.

<sup>2</sup> Mirowski also uses "economics" as a shorthand for main-stream/neoclassical economics.

- 1) What is it that renders commodities commensurable in a market system?
- 2) What are the conservation principles that formalize the response to 1), permitting quantitative and causal analysis?
- 3) How are the conservation principles in 2) united with the a larger metaphorical simplex of body/motion/value – which provides the principles with their justification. (p. 141)

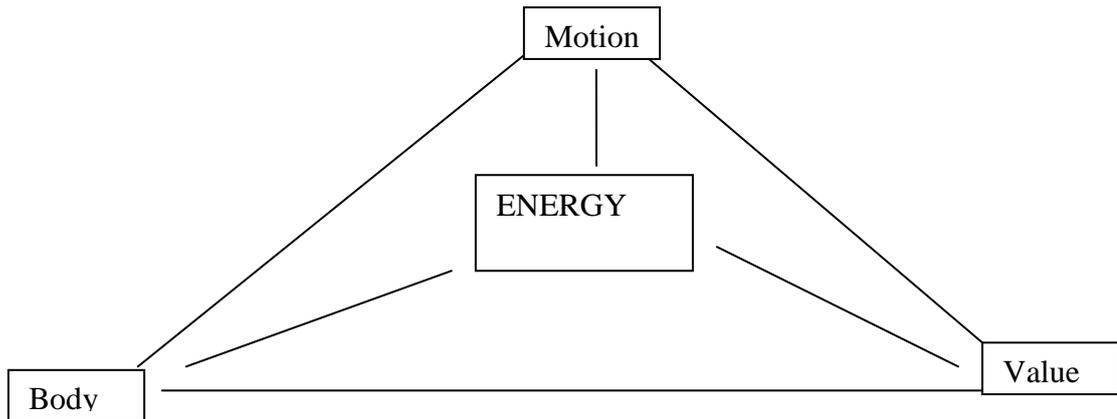
Mirowski hits bull's eye when he writes:

‘I would venture to think that the majority of economists, upon hearing the topic “the theory of value”, would regard it as the province of endless nattering metaphysical speculations upon the ultimate nature of the economy. These economists, out of frustration, or perhaps a disdain for philosophy, have sought to pass over these issues as rapidly as possible, in order to get down to the “real work” of economics. This attitude has been nowhere as prevalent as in the United States, where in the post-war period an important book could be titled *The<sup>3</sup> Theory of Value* (Debreu 1959) and yet be devoid of any explicit discussion of the above three questions.

After Debreu, citations of value theory tend to use it as a synonym for price theory. Value theory is indeed concerned with prices and the mathematical expression of chrematistical relationships, but it is a mistake to regard that as exhausting the purview of value theory. Of course value theory also evokes overtones of morality and social norms, but that doesn't get to the heart of the matter either. The only way to fully comprehend value theory in economics is to situate it within the pyramid, the metaphorical simplex of energy, motion, body and value and to regard it as part and parcel of the same structures that undergrid Western physics. The payoff to this reconceptualization of value theory is a clarification of the entire history of economic thought. In the realm of value theory, the concepts of the discipline called economics have persistently been dominated by somewhat prior developments with regard to concepts of motion in physics.’ (p. 141 - 142)

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<sup>3</sup> The full and correct title is “Theory of Value An Axiomatic Analysis of Economic Equilibrium”



While I completely agree with Mirowski's description of the average economist's attitude to value theory, I definitely do not agree regarding what is the "heart of the matter" – and I do not think most people will. To me economic theory, the core of which is value theory - is important precisely because it attempts to answer questions like: are the bonuses on Wall street legitimate incomes? The fact that an CEO in a big firm might earn ten times more than the firms best engineers – is that a fair remuneration? When I see a hand-made carpet which it has taken hundreds, if not thousands of hours to make – and I buy it for a sum of money representing two weeks pay, eighty hours of work – is that fair? Is the market then "perfect", does it give us the best possible of all possible worlds? Why is it that labour unions always have a rough, non-mathematical labour theory of value and that stock broker's like theories that in essence says that what the "Market" says is always a correct judgement on what share of the pie the various individuals and social groups should get? When women with the same characteristics as men, like age, education, tasks, bur have significantly different wages – is that according to your value theory fair, could be fair, or unjust?

I think that to "situate" value theory in the triangle of body, motion and value – encapsulating "energy" rightly is to be dismissed as metaphysical in the negative sense of that word – and as I will try to show it will not make sense of the history of economic thought. A much more promising approach is to see various economic theories as more or less affiliated with various social groups. One should write a "reception history" of various theories. Why is the labour movement spontaneously more Keynesian than neo-liberal?

If we take Mirowski's discussion of Marx as an example of if "substance" versus "field" theories of value can throw light on the problems of Marxian value theory – the answer is not much. Mirowski claims that although Marx could not foresee the development of theories,

concepts and world views of physics from 1870 onwards ‘yet, it can be argued that the scientific community’s transition from substance to field had some influence on his understanding of the labour theory of value, in that there ended up being not one but *two* Marxian labour theories of value: the first rooted in the older substance theories, the other sporting resemblances to nascent field theories of physics.’ (p. 176)

The reader is left totally in the dark how the “substance” version can be “rooted” in concepts Marx could not now when he formulated his theory – not to speak about Smith, Ricardo and other adherents of some version of a labour theory of value. And what is meant by “sporting resemblances” is far from clear. Indeed a quick Google search for that expression gives only a handful of hits- the only meaningful are references to this paragraph in Mirowski. But if one replaces “substance” with “static equilibrium” and “field” with “dynamic” Mirowski has several good points regarding how antagonistic those two interpretations/theories are, that is the static and the dynamic one<sup>4</sup>. A discussion of which one is Marx’, which one is validated by the stylized facts of capitalism is beyond the scope of this article. But the fundamental difference between static and dynamic models in their ability to grasp the essence of such a dynamic system as capitalism is actually a central theme of Mirowski's book although he mostly uses concepts like substance versus field, discusses the “integrability”, the (ir)reversibility of process etc.

### **Mirowski's first problem – the very selective imitation**

Mirowski's first “lemma” is that “To put it bluntly, economics finally attained its objective to become a science through a wholesale appropriation of the mid-nineteenth-century physics of energy... The seemingly simultaneous discovery [of neoclassical theory] was the direct result of the preceding watershed in nineteenth-century physical theory, and the fact that all of the progenitors of neoclassicism were trained in engineering level physics and subject to particular philosophical trends of the time”. (p. 196-197)

This lemma would be easy to accept, if it was not for the fact that this “appropriation” was of a very peculiar kind, or as Mirowski puts it: ‘this increased dependence on science has a number of perverse side effects. The first was that the more fervent the invocation of science by political economists, the correspondingly lesser were their efforts in delineating precisely what those methods consisted, or in finding out what it was that contemporary scientists actually did.’ (p. 198)

But this ‘invocation of science’ without caring about it – is not that very close to using it as mere rhetoric rather than “far from” that being the case as Mirowski argues (p. 198). What real scientists do is actually no mystery. They create theories to explain observations, and – as Mirowski describes in his book at length – changes those theories when new facts obviously are not explained well by the theories. But this is precisely what neoclassical economist *do*

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<sup>4</sup> In my opinion Marx had a fundamentally dynamic vision of capitalism, but very often he wanted to prove his major results within a classical framework, where long term (dynamic equilibrium), prices as centres of gravitation etc. are core concepts. But there is an inherent danger in focussing only on equilibrium states when studying a system that actually never reaches this equilibrium due to its *endogenous* forces. Competition drives technological change and disrupts a (neo-)classical equilibrium all the time. Due to frequent use of equilibrium reasoning, Marx theory could be forced into completely static models – and of course then ending up being inconsistent. Mirowski also sees this problem: ‘The ways in which the tensions between these metaphors [substance/field] play themselves out in the three volumes of *CAPITAL* are extremely subtle... (p. 175)

*not do-* and have never done. The Great Depression, the last financial crisis – it is outside the theoretical framework *static* equilibrium theory. Twenty, thirty percent unemployment – always voluntary of course – is also outside this model. It is no great news that neoclassical economics is immune against facts, as argued at length in Lawson (2003).

This in my opinion ideologically based hostility to using the ordinary methods of both natural and non-ideological social sciences is well documented in "More Heat than Light". Mirowski describes how Walras in an article boast that his new "physico-mathematical science of the *Elements* uses *precisely* the identical mathematical formulas [as physics]. Walras then proceeded to scold physicists who had expressed scepticism about the application of mathematics to utilitarian social theories on he ground that utility is not a measurable quantum...' (p.220) Already here we meet the problem with the way the early neoclassicals imitatate physics – "scolding" the leading scientist of the discipline you try to imitate.

As Mirowski points out the ambitions of Pareto was if possible even greater. Pareto argued that "Thanks to the use of mathematics..." and the "fact of experience" of which combinations of goods the individual are indifferent... "The theory of economic science acquires the rigor of rational mechanics." (p. 221). As we will see below, Mirowski is not impressed by Pareto's grasp of physics, be it rational mechanics or more modern theories.

But let us first take a look at the one person that clearly had all the mathematical training needed to get things right, Irving Fisher. Mirowski goes detailed through the effort of Fisher to make a close analogy between mechanics and economics, where space in mechanics is commodity in economics, force is "marginal utility or disutility" etc. (p. 224). After describing what Fisher tried to do Mirowski is neither impressed nor amused:

'Fisher, the most sophisticated scientist among the nineteenth-century neoclassical economists, still displayed an inadequate comprehension of the formalism of the energy concept and, as a corollary, lack of appreciation of the metaphorical dissonances involved.' (p. 228). Mirowski then goes through all of Fishers mistakes, which to a modern reader seems rather obvious: 'At the top of the list, the identification of a particle with an individual is incorrect...' 'Fisher's next mistake is the conflation of to incompatible meanings of the term "work".' (p. 229). But not only what Fisher wrote does not impress Mirowski, "the gaps in his lexicon of correspondences are more serious than that." (p. 230). Again we see this extraordinary "amateurish" imitation even by people with superb mathematical skills. Is there really any other explanation that the neoclassical school wanted their "results" to be accepted as natural laws, not to be opposed by any human, and especially not by any movement of humans like the labour movement; or as Mirowski formulates it:

'The question of the "measurability of utility", which has dogged the neoclassical research program since its inception, was not due to some tempest in a teapot over cardinal versus ordinal utility, contrary to modern histories of the doctrine. To the early neoclassicals, on the contrary, it represented the goal of the final attainment of the status of a science on par with physics. Such status was doubly desired because it would then dissociate economics from all the contentious and non-scientific speculations to be found in the low-rent program of psychology.' (p. 235).

In my opinion the fact that no neoclassical economist managed to appropriate the concepts and implications of the physics of their time because they lacked the intellectual prerequisites

do to so, is a rather weak hypothesis. It is much more reasonable that they wanted neoclassical economics be (seen as a science, but that same time they wanted this science to prove the well-know results about the optimality of “free” markets and the other side of the same coin: the loss of efficiency by any intervention in these “free” markets by democratic government, taxes, tariffs, unions etc. We know by now which of the desires turned out to be the strongest.

### **Mirowski’s second problem – the reaction to critique from the physicists**

Again it is best to leave the floor to Mirowski, whose sharp, satirical prose is a pleasure to read:

‘It would have been extraordinary for so many economists to mangle and misrepresent the energy model so frequently without eventually calling down the wrath of physicists upon their labours. Indeed one of the skeletons in the neoclassical closet is that around the turn of the century, quite a number of physicists turned their attention to this species of upstart proto-energetics and pronounced it wanting’ (p. 241)

The reader probably already can guess what was the reaction of the neoclassical core cadre:

‘Time and again they [the neoclassicals] met these inquisitions with hurt incomprehension, bluster, farrago, protests the physics was irrelevant, and finally, a feeling of betrayal: How did it come to pass that those in the forefront of trying to make economics a science should be so abused by those whom they were trying to emulate?’ (p. 241)

The last question is rather easily answered – the physicists were serious scientists, not ideologists, so they cared about conceptual consistency, mathematical rigour, empirical relevance as will be amply demonstrated by Mirowski. The first man out is Joseph Bertrand, “specialist in the mathematics of rational mechanics and the editor of the third edition of Lagrange’s *Mecanique Analytique*. Bertrand reviewed Cournot’s *Recherces* and Walras’ *Théorie Mathématique de la Richesse Sociale* and Mirowski sums it up:

‘Most of his review centered ont the thesis that the neglect of mathematical political economy in the French academy had been deserved, because the existing attempts had been devoid of *any serious empirical content, not to mention their numerous mathematical and conceptual errors*. ... Bertrand observed that in general there would exist what in the modern literature is called false trading – namely, some exchanges are conducted at non-equilibrium prices in the process of trying to discover the market-clearing price. Bertrand pointed out, quite correctly, that the mere existence of tales trading, or indeed any mercantile speculation, would obviate the determinacy of Walras’ general equilibrium’ (p. 242, my italics)

We could now test a 0-hypothesis about Walras’ reaction to the criticism by Bertrand. That is what would a real scientist do, faced with such criticisms? The answer is: ‘Walras never directly answered the critisims’ (p. 242)

Next scientist out is Willard Gibbs, Irving Fishers thesis advisor. He asked ‘why Fisher’s indifference lines should be able to be integrated into utility surfaces. Far from being a minor technical point, Gibbs probably tried to make Fisher aware that the absence of integrability

would necessarily mean that there could exist no such quantity as total utility, and path-independence of equilibrium would be compromised. ... Fisher's stock reaction to the problem of conservative entities within his economic theories was *simply to deny the possibility of any serious dynamic theory*' ... We can date the collective neoclassical neurosis with regard to the physics metaphor from this point. (p. 243, my italics).

As Mirowski puts it: 'The third instance of a scientist harassing the new economic science ...' came in 1889 when Herman Laurent, mathematician at the Ecole Polytechnique and author of a textbook on rational mechanics wrote to Walras about 'the appropriate unit of value' ... and how did Walras respond? In Mirowski's words: 'Walras, after trying to fob him [Laurent] off with compliments, responded by repeating ... that it is not proper to speak of a unit of value, only an arbitrary *numéraire*. Laurent, a little perturbed by being patronized, wrote back that he was asking about *dynamics* and the essential role of time, but that Walras had only responded with a static argument.' (p. 243 – 244). But there are more episodes in this story: 'Laurent, by this time was beginning to wonder whether Walras was just playing dumb, was being obstreperous, or perhaps simply did not understand the physics (letter 1380)... the correspondence on value theory then cooled for a while, but upon a friendly letter from Walras a year later, Laurent decided to try one more time. This letter of 13<sup>th</sup> of May 1900 (letter 1452) is a *miracle of compression and lucidity*.' (p. 244, my italics)

And guess what happened: 'This threw Walras into a tizzy' (p. 245). I shall not go into the actual mathematical arguments on integrability and the need for a standard or measure of utility, but Mirowski leaves no doubt that Walras did not answer the questions this time around either, and that: 'Amazingly, Laurent doggedly tried one more time...' (p. 245). As we now expect: 'Walras entirely ignored the question about the exact differential and responded by shifting his premises... From this point on Walras started suggesting to others that Laurent was part of a plot against him.' (p. 245)

But the story does not end there:

'Because of his avid devotion to get the content of the physical metaphor [conservation of energy etc.] correctly specified, Laurent should be considered one of the unsung heroes of neoclassical economic theory. Strange as it may seem, Laurent thought of himself as a supporter of the Lusanne school of economics throughout the entire episode... So just when the Walras correspondence looked to him [Laurent] as it was going nowhere, he decided to try one of the neoclassicals who might possess a little better comprehension of the issues involved. In an effort clearly above the call of duty, early in 1899 he [Laurent] composed a number of letters to Vilfredo Pareto, essentially posing the same questions. Pareto's first response was to praise Laurent's mathematical textbooks...' (p. 245-246).

Again I leave out Mirowski's résumé of the economics and mathematics involved and go directly to Mirowski's description of the next scene in this drama: 'Laurent must have been perplexed by this further indication of erratic behaviour the part of the avatars of economic mechanics and rational economic man. Not only was Pareto's letter internally inconsistent, it resembled Walras' letter only in its cavalier assertion that the problem was insignificant; yet their respective understandings of the problem had no relation one to another, *and worse, both had no connection to the obvious physics metaphor*. Laurent chose to press the inquiry with greater insistence, to which Pareto replied *with a wholly different defence*. (p. 246, my italics).

By now the fact that Mirowski is not impressed by Pareto's defence comes as no surprise... followed a thoroughly awkward comparison of economics with geometry... realising belatedly that this analogy wanders into a cul-de-sac, Pareto sought refuge in another non-sequitur... (p. 246). We are now approaching the end of this story: 'At this point one is at a loss as to why Laurent wanted to pursue the matter any further, but as Pareto undoubtedly would say, *chacun a son goût*. Pareto's next letter is a little testy, and at one stage he writes:

"What you say about the habits of pure mathematicians doesn't bother me". To this statement by Pareto Mirowski adds the following comment: 'We remind the reader that at this same time, Pareto was attempting to intimidate other social theorists, such as Croce, with the supposed precision of his mathematical science.' (p. 246). Again I will leave out Mirowski's description of the substantial issues and go directly to Mirowski's conclusion: "The explanation of this failure of communication is straightforward: Laurent understood the physics, and Pareto did not.' (p. 247).

Mirowski counts as the "fourth instance of a scientist taking a neoclassical economist to task" was when Vito Volterra in 1906 reviewed Pareto's *Manuale di Economica Politica* and no surprise at this advanced stage of the story. Volterra's 'complaint was the same: The mathematics of fields should be used in economics only with caution... when there are more than two goods, Pareto's expression, which was identical to Laurent's equation (1) would not be integrable...' (p. 248).

Mirowski is very clear in his summary: 'The history of the integrability problem in neoclassical economics is an extremely peculiar interlude. Around the turn of the century, some major figures in neoclassical thought were challenged by some even greater luminaries in the scientific community, and, to a man, they acquitted themselves abysmally'. Later on the same page Mirowski returns to the conclusion again in more detail:

### **Bad science or ideology?**

Mirowski's hypothesis is that the founding fathers of the neoclassical school did not understand the physics involved. But to me that seems very far fetched given the intellectual resources and training of the founding fathers. It might have been true if the founding fathers had been full of naïve optimism – and nobody had tried to tell them what the physics were about. But Fisher got clear advice from his thesis advisor – and did not "understand" it. Not only that – he never answered it. As Mirowski points out: 'In his retrospective manuscript "My Economic Endeavours" Fisher crossed out the section containing this comment, leaving out of the text all discussion of integrability. Is this how scientist should work?

Walras and Pareto got letters from fellow scientist that according to Mirowski were miracles of compression and lucidity – and still they did not catch it. Pareto even did get it twice – first from Laurent and then some years later from Volterra.

When ordinary scientists get comments on their drafts from colleagues, from peer-review telling them that they do not get the maths right – would they not start studying it and until they could prove that they were right – be rather humble? But not the founding fathers, they show no restraint in browbeating and hoodwinking other schools of thought.

Even if these individuals, the founding fathers were too "dumb" to understand the physics, why didn't some other neoclassical theoretician come up and show both an understanding of the physics and how it might be a basis for the neoclassical paradigm?

But isn't pretty clear by now that there is "something" in that paradigm that is incompatible with real science – natural as well as social – and that of course is the *absolute static* character of the neoclassical paradigm. In equilibrium nothing changes, not prices, not technology, not preferences and initial endowments. This should be called "perfect stagnation" since nothing changes – not perfect competition, since real life competition is all about changing technology by innovation, changing preferences by using billions on advertising etc. Most social scientists, including many economists do not understand that there in a GE-model cannot be trading outside of equilibrium, so that prices cannot be "moving" towards equilibrium. That would be what in GE-jargon is called "false trading" – which is a no-no since it changes the "initial endowments", that is the budget constraint and "constrained optimisation" model cannot be solved. The same goes for increasing returns to scale, a truly *pervasive* fact of life, especially of industrial production, and in particular for software, where the first unit produced has almost all the cost since the marginal costs of a copy is very close to zero. With falling unit costs – the Walras model crashes before it have even started to take of.

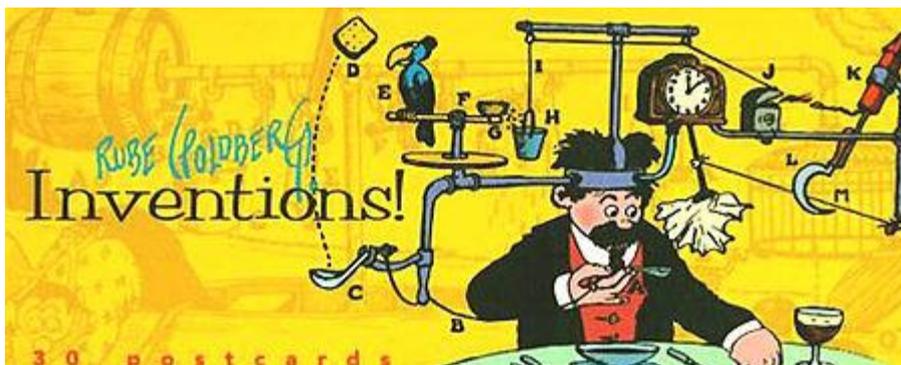
The problem with making the Walras/GE-model dynamic is that all the "optimal" results cannot be proven any more. You can no longer prove that taxes, unions, tariffs, environmental and stock market regulation create inefficiency. It becomes an empirical, facts based question not an indisputable, a priori, physical law – which you cannot dispute without being against math and logic. The Achilles' heel of the GE-model is its lack of dynamics, the undisputable fact that the *stability* has not been proven. But the neoclassical – when not under direct attack by critics, will always give you the impression that this is the case, or at least no problem, a minor extension of the model etc<sup>5</sup> so that they could have the authority of a real science, based on rigorous maths when they wanted to use the GE-model and its results as the basis for neoliberal economic policies.

Mirowski formulates the static-dynamic divide in the following way:

'The question of the extension of the physics metaphor to encompass Hamiltonian dynamics merged all the critical issues of time, process, conservation principles and integrability into a single, seemingly technical issue. A genuinely rigorous response to this question would by its very nature need to incorporate an evaluation of the energetics metaphor to describe social processes. *For whatever reasons*, neoclassicals have avoided this extension and its attendant evaluation whenever possible. Instead they have gone out of their way to concoct jerry-built scenarios of dynamic movements between static equilibria identified by the primitive physics model. Jevons invented a black box, called a "trading body", which magically performed all the dynamic functions of coordination in an *unspecified manner*. Walras posited his famous autineer who prevented all trading activity while transactors resorted to hypothetical questions... Others attempted a pseudo-dynamics predicated upon the difference between demand and supply functions, piling one Rube Golberg contraption atop another. The purpose of all these contrived schemes was to circumvent the dynamics constructed by the physicists within the logic of the appropriate model. (p. 251 - 252)

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<sup>5</sup> See Haavelmo (1974) for a short, mathematics free, very insightful discussion of statics, dynamics and stability. The title of the article is the question "What can static equilibrium models tell us?" and the answer is – nothing interesting about a real dynamic market economy.



Source: Wikipedia. [http://en.wikipedia.org/wiki/Rube\\_Goldberg](http://en.wikipedia.org/wiki/Rube_Goldberg)

### For whatever reasons...

As Mirowski points out – based on Walker (1987)– the critique from the physicists did have some impact on Walras. The first three editions of Walras’ *Elements* attempted:

‘...to construct a model of economic dynamic where purchases of inputs and production of commodities actually occur through time as part of a mechanism of equilibration...in the (fourth) edition of 1900 – that is after Bertrand’s blast and Laurent’s needling – Walras switched to a different model of *bons* or “pledges”, one in which everything is coordinated on paper prior to any and all economic activity: Everything is irredeemably static... this version, of course, is the progenitor of the twentieth-century Arrow-Debreu model. The one unfortunate aspect of all this is that Walras himself never really understood that the problem was one of path-dependence and thus violation of the original energy metaphor; he therefore merely forced the pledges model into the structure of the *Elements* without eliminating or revising the older theorizing that contradicted it. As a consequence, generations of neoclassical theorists have felt free to assert anything they pleased about the dynamics of their model, without understanding that their freedom was constrained’ (p. 252).

The problem here is not Mirowski’s description of the theoretical issues involved, but with the motivation for creating black box’es, auctioneers, Rube Goldberg contraptions, because “for whatever reasons” is no answer to why they undertook all these strange manoeuvres, it just begs the question.

There is one piece of evidence that might give us a hint. There was one person who possessed the rare combination of a complete understanding of the physics and being an adherent of the neoclassical school – that is the “unsung hero” Laurent. As Mirowski writes: ‘Perhaps the most incongruous aspect of this particular episode is that Laurent persisted in seeing himself as a partisan of the Lausanne school of mathematical economics. In 1902 he published his *Petite traité d’économie politique mathématique*, which was little more than a pastiche of brief observations on a sequence of mathematical models; nevertheless it was written with the intention of defending the Walrasian program. Curiously enough, the section on price theory merely recapitulates the contents of his letters cited in this section [of *More Heat than Light*], *minus the parts questioning why equation (1) is an exact differential*. His questions along those lines were never adequately answered by the protagonists, and so it appears he just passed them by in silence in his own treatise.’ (p. 247, my italics).

But why should he do that, why did Laurent not make a new “Social Mechanics”, that is a dynamic model for a dynamic phenomenon? To me there is only one obvious reason for this passing by in silence: the core neoliberal results of the neoclassical model cannot be proven in a dynamic setting, so Laurent – like Walras had to choose between empirical realism and ... ideology. You could not prove your “most cherished results” (“free” markets are an optimal allocation mechanism and everything following from that) in a scientific manner so something had to give –and for all adherents of the neoclassical school inclusive Laurent – science was thrown overboard.

### **Modern physics – modern economics**

Mirowski has an analysis of the development of modern physics, i.e. twentieth century physics and how that development was (not) reflected in modern economics and comes very close to saying that the relation between neoclassical economics and the “energetics” movement was never fundamental, it was just a case of the neoclassicals using that jargon to pose as a science. What happened was that as physics developed, as a result of empirical observation (the constancy of the speed of light etc.) the old rational mechanics paradigm went out of business. The energetics movement came under attack from the leading edge scientists like Max Planck and Ludwig Boltzmann. As Mirowski puts it: ‘Both complained that the energetics did not sufficiently understand the distinction between a state function like energy and path-dependent quantities, such as physical work.’ (p. 268) Another aspect of the critique of Planck and Boltzmann is that an aspect the two paradigms have in common – intensive use of tautologies hit by Boltzmann’s and Planck’s critique:

‘Finally, in the realm of philosophy, Planck shrewdly observed that ‘energetics achieves the apparent and surprising simplicity of its proofs by the simple process of pushing the contents of the laws to be demonstrated (which always must be known in advance) back to their definitions. Planck’s indictment of energetics deserves to be read today because ... it is (unintentionally) one of the most cogent and concise critiques of neoclassical economic theory.’ (p. 269).

While Planck’s critique “killed” the energetics paradigm in physics, that did not hurt the neoclassicals much, because ‘neoclassical economics (unintentionally?) managed to segregate itself from the larger program of energetics... The first generation of neoclassical economists never completely explored the structural physics metaphor, because their understanding of it was so deficient, and hence never ‘joined forces’ with the energetics movement.’ (p. 269). Another – and more plausible explanation was that the neoclassicals understood the energetics well enough, they just cherry-picked some rhetoric in order to get a more scientific image. I will return to this question below when discussing Mirowski’s analysis of Samuelson’s use of physics metaphors.

### **Mirowski and neoclassical production theory**

Mirowski devotes a whole chapter to neoclassical production theory:

‘A thoroughgoing symmetry between production and consumption theory dictated that production should be portrayed as instantaneous, virtual, static, and path-independent, or more prosaically, fully reversible in time. This conception offended common sense images of

production; even worse, it violated and subverted the very message of the proto-energetics metaphor on the side of consumer theory – namely it made a joke of the primal parable of neoclassical theory, the stern moral tale of scarcity and constraint. If production were instantaneous and fully reversible, it is difficult to see how there could ever be a shortage of anything. Silk purses could be made from sow's ears, but if we then conceived a craving for pork, we could just temporarily take the silk purse from the closet and presto! Pork chops. ' (p. 320). One might add that not only did this view of production violate common sense, it also violates any scientific approach to production; or as Mirowski puts it:

'The idea of a path-independent transformation of one set of physical objects into another violates so many physical laws that one can only marvel at the audacity of those who wrap themselves in the banner of physics before marching off to do battle with the opponents of production functions' (p. 237).

Mirowski also discusses the small group of neoclassical economist that seemingly was interested in real-life production processes, the post-war concept of the 'engineering production function'. One of the leading cadres of this current, Chenery, gets the following verdict from Mirowski:

'Chenery is assuming away anything that would make the process path-dependent. In other words, he is chopping off every physical phenomenon that cannot be represented by a field formalism. This includes banishing the second law of thermodynamics [increasing entropy], something an engineer would never be allowed to even contemplate without risking his diploma. Just as in any other instance, it is the physics itself which is forced to lie upon the bed of the neoclassical metaphor, and not vice versa.' (p. 330)

The point is here is not if Mirowski's critique of neoclassical production theory to the point or not, but that there is a strong tendency that Mirowski when he looks at the theories of neoclassical economists in detail – he in strong terms claims that they do not understand any version of physics, late eighteenth century, mid-nineteenth-century or twentieth-century physics. But when Mirowski operates on a grand historical scale things are turned on their head. Mirowski claims that neoclassical economics is "the slavish imitation of physics. (p. 356)

### **Mirowski and "The ironies of physics envy"**

To decide on what is really the role of physics I think it is more useful to take a look at a "case" than to try to counter pose and interpret even more quotations. Mirowski's conclusion on this stated in passages like: 'But in the final analysis, however coy and ambivalent neoclassicals may appear to be about their physics metaphor, it cannot be seriously be repudiated or relinquished, because *there is nothing else that can hold the neoclassical research programme together.* (p. 368).

Is there really nothing else – what about subjective value theory as opposed to a labour theories of value? If that is the case then physics has a function of mere scientific window-dressing and does not guide or inspire the development of economic theory. The case I will look at is Paul Samuelson. Under the subtitle "Paul Samuelson Mirowski devotes no less than 7 pages to discuss Samuelson's role in economics and relation to physics. According to

Mirowski it was first and foremost Samuelson ‘who by word and deed was responsible for the twentieth century self-image of the neoclassical economist as scientist’ (p. 378). Mirowski points out one aspect of Samuelson’s work that had not ‘drawn any comment in the numerous *Festschriften* and evaluative volumes dealing with Samuelson's career’ and that was ‘Samuelson's habit of making reference to modern physics in superficial and rhetorical ways while persistently misrepresenting both its content and its relationship to neoclassical theory.’ (p. 379 – 379). One example being Samuelson's “correspondence principle” (CP). The term was taken from Bohr where the principle said that in some context one could use predictions from pre-quantum mechanics as a fairly good approximation. The stated purpose of Samuelson's CP was to ‘suggest that dynamic stability analysis in a neoclassical context could lend some structure to the comparatively static results in neoclassical price theory.’ (p. 379). But as Mirowski correctly points out ‘At the most prosaic level, Samuelson's mathematical model had no connection with those of Bohr, either in the old or the post-1925 quantum mechanics ... Even from the most Olympian heights there is no plausible analogies between Bohr’s and Samuelson's Correspondence Principles.’ (p. 379). Another example is the Le Châtelier principle – which I shall not try to explain, but Mirowski's conclusion is clear enough: ‘Again the reference to a physical principle was *not* being seriously used as a heuristic research device.’ (p. 382)

Mirowski’s global view of Samuelson’s track record is quite harsh: ‘Throughout his career, Samuelson has been the master of scientific rhetoric, continuously and consciously hinting at parallels between neoclassical theory and twentieth-century physics, and just as consciously denying them, usually in the very same article.’ (p. 382). Mirowski goes on to quote Samuelson’s Nobel Prize lecture where Samuelson said that: ‘*There is really nothing more pathetic than to have an economist or an retired engineer try to force analogies between the concepts of physics and the concepts of economics. How many dreary papers have I had to referee in which the author is looking for something that corresponds to entropy or to one or another form of energy.*’ (p. 382).

Mirowski’s more detailed comments are equally harsh:

‘Nevertheless Samuelson's *Collected Scientific Papers* are chock-full of titles such as “A Quantum Theory Model of Economics”, “The Law of the Conservation of the Capital-Output Ratio”, “Two Conservation Laws in Theoretical Economics” and “A Biological Least-Action Principle”. Papers with less misleading titles such as “Causality and Teleology in Economics” and “Maximum Principles in Analytical Economics” contain at least as many explicit references to physics as they do to economics. Nevertheless, the single most salient aspect of those papers is that their scientific is entirely decoupled from their paltry substantive physics content. The “Quantum Theory” paper has nothing whatsoever to do with quantum mechanics; the “Conservation Laws” paper never actually comes to grips with the problem of what neoclassical theory actually assumes to be conserved in its proto-energetics model...’<sup>6</sup>

One might argue that the fact that one leading economist was trying to impress his audience with a misuse of physics metaphors does not matter very much for the neoclassical school, but as Mirowski points out:

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<sup>6</sup> References to Samuelson's papers left out for reasons of readability.

‘Once Samuelson has set the tone, the floodgates were opened to a multitude of misleading references to Lyapunov (Buasor 1987), the Noether theorem (Sato 1981, 1985), chaos theory (Day 1983), catastrophe theory (Varian 1979), and so on. This wretched excess of emulation of technique essentially served to hide the fact that neoclassical economists were maintaining a cool distance from the profound innovations of twentieth century physics...’ (p. 385).

Again we confronted with the problem of how to interpret this, are the neoclassicals really imitating, that is taking methods and mathematical techniques which they really believe can throw light on a market economy or are they just using physics metaphors to pose as scientists? I think that an important argument for the latter conclusion is that they consequently from Walras onwards did not take on board the dynamic mathematical techniques – starting with the Hamiltonian and ending with chaos theory and Mandelbrot fractals<sup>7</sup>. These methods should obviously be much more adequate to grasp a dynamic phenomenon such as capitalism. So even if they did not get it right from the start – why did neoclassical economics not get on to the right dynamic track?

But the fact is that neoclassical economics are further away from physics now than ever before – and since physics is not as “hot” not such a role model for science, for mans mastery over nature (= everything) as in the Samuelsonian decades after the war – the neoclassical need to use physics rhetoric is reduced. As Mirowski points out:

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<sup>7</sup> Mirowski gives a good description of how Mandelbrot originally developed his idea for econommics, but the idea was not taken up by the neoclassicals, it was the physcists that saw its potentail. (p. 386 – 387).

‘... as we have argued in Chapter 2, the vintage of physics appropriated by neoclassical theory is that of the most rigid and implacable determinism, the physics of the Laplacian dream. In the ensuing century physics has largely repudiated this image of explanation. New forms of explanation have taken their [rational mechanics] place: stochastic forms, forms allowing interaction with the observer, forms allowing for emergent novelty arising out of apparent chaos, forms allowing “something for nothing”. In such a cultural climate, neoclassical economics must appear atavistic, if not irrelevant’ (p. 400 – 401).

Yes, from a scientific point, irrelevant – it just cannot explain the current financial crisis, but as Mirowski also points out:

‘Nevertheless, the chosen strategy of slavish imitation of physics certainly was not a total failure. It is a matter of historical record that neoclassical economic theory has indeed managed to displace all rival schools of economic thought, with the single exception of Marxism. (p. 393)<sup>8</sup>

But how are we to reconcile:

- a) The fact that from the start the imitation was neither “slavish” nor creative, it was very selective. The fact that neoclassical economics – although with obvious weaknesses from a scientific point of view, pointed out by the physicist from the start.
- b) The fact that the only physicist according to Mirowski who did understand the physics – Laurant – but was also an adherent of the neoclassical school – passed over all the problems of “imitation” in silence?
- c) The fact that no later neoclassical economist took physics seriously, but – like Samuelson – just misused physics metaphors to impress his audience?

As already stated several times – only another agenda than “slavish imitation of physics can explain that ensemble of facts. That is a political-ideological agenda can explain this unscientific behaviour. But this possible political-ideological explanation is outside Mirowski's focus, entirely outside his radar.

Other observers have been more open to this type of explanation. Stiglitz for example in his Nobel lecture with the telling title “*Information and the Change in the Paradigm of Economics*” wrote: ‘...one cannot ignore the possibility that the survival of the [neoclassical] paradigm was partly because the belief in that paradigm, and the policy prescriptions, *has served certain interests.*’ Stiglitz, (2002)

Recently Paul Krugman made a similar point in his New York Times column regarding the lessons for economic theories from the present financial crisis:

‘In the end, those of us who expected the crisis to provide a teachable moment were right, but not in the way we expected. Never mind relearning the case for bank regulation; what we learned, instead, is what happens when an ideology backed by vast wealth and immense power confronts inconvenient facts. And the answer is, the facts lose. (Krugman 2010)

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<sup>8</sup> Remember that this was written in 1989, before the rise of the neo-Scumpeterian, evolutionary school during the last two decades. Other heterodox currents have also emerged since then.

It is this factor of “vast wealth and immense power” that Mirowski overlooks in his otherwise excellent book. One cannot always find the explanation of the development of science or ideologies posing as science by looking only inside science.

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