



## ‘From Political Economy to Economics’ and Beyond

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### Abstract

Ben Fine and Dimitris Milonakis have done political economy a great service by drawing attention to the insights lost in the twists, turns and reductions in the transition from political economy to economics. These two volumes constitute a solid foundation upon which a new generation can build a political economy for the future. This review presses some of their meta-theoretical arguments a little further than they actually do in an attempt to ‘toughen-up’ the new political economy and make it more able to carry the fight to economics.

### Keywords

abstraction, agency, critical realism, idealisation, habits, mathematisation, meta-theory, ontology, positivism, rigour, rules, structure

### Introduction

These two books, *From Political Economy to Economics*<sup>1</sup> and *From Economics Imperialism to Freakonomics*,<sup>2</sup> constitute an impressive history of economic thought. Their purpose is to highlight the insights lost in the transition from political economy to economics and, thereby, encourage a new generation of political economists (many of whom will not be aware of ideas presented herein) to build a political economy for the future. It is a timely and extremely important project, and one that I fully support.

*From Political Economy to Economics* shows how, from the eighteenth century, political economy was gradually, via a series of twists and turns, reduced in scope until it became what we would understand as (mainstream, orthodox or neoclassical) economics<sup>3</sup> by the mid-twentieth century. The twists and turns

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1. Cited as Milonakis and Fine 2009.

2. Cited as Fine and Milonakis 2009.

3. I follow Milonakis and Fine and use the term ‘economics’ to refer to mainstream, orthodox, or neoclassical economics only – i.e. not political economy or heterodox economics. I also use the term ‘social science’ to exclude economics.

involved several schools of thought and key individuals. The reductions were not only in methods, techniques, concepts (crucially, the concepts of the human being and the market as synonymous with supply and demand) and objectives, but also in the elimination of historical and social phenomena. *From Economics Imperialism to Freakonomics* builds on this, showing how economics, with increased momentum after the information-theoretic turn, is currently colonising other social sciences in an imperialistic march.

Milonakis and Fine are keen to point out that these twists, turns and reductions on the road from political economy to economics are all considered in ‘a methodological context’.<sup>4</sup> Indeed, methodology is stated as one of the main themes of *From Political Economy to Economics*; the term ‘method’ appears in the book’s sub-title; and methodology is widely mentioned in *From Economics Imperialism to Freakonomics*. And methodology (or meta-theory<sup>5</sup> as I prefer to call it) will be the theme upon which I base this review. To be more specific, I intend to press some of Milonakis and Fine’s meta-theoretical arguments a little further than they actually do, in order to ‘toughen up’ the new political economy they seek to encourage, thereby making it more able to carry the fight to economics. But let me begin by reproducing some of the key ideas on meta-theory presented by Milonakis and Fine in *From Political Economy to Economics* and *From Economics Imperialism to Freakonomics* respectively.

### Key meta-theoretical ideas in *From Political Economy to Economics*

Milonakis and Fine show how discussion of methodology has been dominated by a division between deductive and inductive methods. The exact meaning of these terms is never clarified, and Mitchell<sup>6</sup> is probably correct to suggest we ought to banish the ‘superficial jargon’ of deduction and induction. By the late nineteenth century, however, many political economists knew that the success of natural science was due to its method (even if this method had no specific name at that time). Whilst some advocated its use in political economy, others were hostile to this view – e.g. Leslie and Veblen.

Let us start with the classical period. Marx commented on methodology, and I will not expand any further as readers of *Historical Materialism* will be

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4. Milonakis and Fine 2009, p. 2 and *passim*.

5. The term ‘meta-theory’ encapsulates philosophy of science, ontology, aetiology, epistemology and methodology. Quite often we use the term ‘methodology’ as an umbrella term, when we are not actually discussing method *per se* but, say, causality or ontology.

6. Milonakis and Fine 2009, p. 203.

familiar with Marx's work, but perhaps less so with the other political economists of the period. Mill was aware of Comte's positivism.<sup>7</sup> Walras referred to the 'physico-mathematical sciences'.<sup>8</sup> Jevons argued that since economics 'deals with quantities, it must be a mathematical science', adding that 'all economic writers must be mathematical so far as they are scientific at all'. He also suggested that the 'deductive method of Economics must be verified and rendered useful by the purely empirical science of Statistics'.<sup>9</sup> Menger's objective was to 'provide the scientific foundations for economic theory'<sup>10</sup> and suggested that the 'deductive approach is the only truly scientific method'.<sup>11</sup> He is also one of the first to refer to the 'method of isolation'.<sup>12</sup> Bohm-Bawerk agreed,<sup>13</sup> as did Weiser. 'Being a mathematical concept', as Milonakis and Fine put it, the concept of marginal utility 'gave a great impetus to the mathematisation of the subject'.<sup>14</sup> In Marshall's time, as noted by Keynes, 'mathematisation of economics was in the air of both the natural and the moral sciences'.<sup>15</sup> Marshall was 'committed to the use of mathematics, but only when it was grounded in reality'.<sup>16</sup> The laws of supply and demand, and the law of substitution, key Marshallian ideas, placed functional relations at the heart of economic theory.

In the post-classical period the Institutionalists welcomed developments in quantification, suggesting that 'economics will become a quantitative science'.<sup>17</sup> Weber felt it necessary to comment, negatively, upon the emergence of the hypothetico-deductive method<sup>18</sup> by rejecting the idea that the purpose of economics is the 'discovery of "a complex of regularities" in the form of "*lawlike relations*" . . . since "these generalizations would have no causal status"'.<sup>19</sup> For Pareto, according to Milonakis and Fine, 'in typical positivist fashion, and guided by the methods of the physical sciences, the only royal road to true scientific knowledge in social science is through what he called the logical-experimental method, which is firmly anchored in reality and involves

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7. Milonakis and Fine 2009, p. 30.

8. Milonakis and Fine 2009, p. 91.

9. Milonakis and Fine 2009, pp. 96–7.

10. Milonakis and Fine 2009, p. 102.

11. Milonakis and Fine 2009, p. 107.

12. Milonakis and Fine 2009, p. 108.

13. *Ibid.*

14. Milonakis and Fine 2009, p. 98.

15. Milonakis and Fine 2009, p. 127.

16. Milonakis and Fine 2009, p. 129.

17. Milonakis and Fine 2009, p. 184.

18. Milonakis and Fine 2009, p. 193.

19. Milonakis and Fine 2009, p. 194.

observation, experience, and logical inferences from experience'.<sup>20</sup> Menger exposes epistemological and methodological errors of the then-prevailing philosophical doctrines such as logical positivism, and criticises those who 'confuse the methods of the natural and social sciences'.<sup>21</sup> 'Natural phenomena', he argues, 'exhibit regularities that can be ascribed the status of law, and which can be arrived at through laboratory experiments', something not possible in the social sciences. Mises too attacked the 'method espoused by positivism' which, according to Milonakis and Fine, is suitable for natural but not social science. One key issue in the *Methodenstreit* was, according to Milonakis and Fine, epistemological, namely the notion of causality. In contrasting Menger's Aristotelian essentialism with Schmoller's nominalism or empiricism, Milonakis and Fine note the latter's use of a 'descriptive type of causality where, following David Hume, the explanation of the causal relation [is perceived] as merely uniformity in succession'.<sup>22</sup> This is, of course, a reference to causality as event regularity or constant conjunction of events, and underlies the regularity-conception of laws.

Finally, let us consider the mid-twentieth century. Keynes's comments on the (mis)use of mathematics and statistics are not only recognised by Milonakis and Fine, they are so well known I refrain from discussing them. According to Cooter, 'In the process of absorbing Newton's mathematics, which began in the 1880s and was completed by the time Samuelson published the *Foundations of Economics* in 1947, economics had gained technical superiority over other social sciences'.<sup>23</sup> With the consolidation of general equilibrium theory, mathematics had taken centre stage of economic theorising. As Debreu puts it: 'An axiomized theory first selects its primitive concepts and represents each one of them as a mathematical object. . . . Next assumptions on the objects . . . are specified, and consequences are mathematically derived from them'.<sup>24</sup> By the first quarter of the twentieth century this method had been reflected upon and systematised under the labels of logical positivism, logical empiricism and the hypothetico-deductive method. Although this (now systematised) account of methods was introduced, 'officially' as it were, to economists in the 1930s via the likes of Hutchison, the fundamentals of this method were well known before this.

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20. Milonakis and Fine 2009, p. 222.

21. Milonakis and Fine 2009, p. 257.

22. Milonakis and Fine 2009, p. 112.

23. Milonakis and Fine 2009, p. 179.

24. Milonakis and Fine 2009, p. 281.

### Key methodological ideas from *From Economics Imperialism to Freakonomics*

The term 'rigour' emerged in *From Economics Imperialism to Freakonomics* and is used throughout. It has now become a euphemism for the use of quantification and mathematics. As Milonakis and Fine put it: anything that does not conform to the approach favoured by economists is 'dismissed as lacking "rigour" and "science", terms that are well known within economics as a superficial code for policing anything that does not ultimately rest on a mathematical model, or mathematical testing'.<sup>25</sup> A 'lack of science or rigour, [are] the by-words for conformity to orthodoxy's methods, theory and technical apparatus'.<sup>26</sup>

Consider Milonakis and Fine's comments on New Institutional Economics (NIE). Paraphrasing Furboten's and Bardhan's comments on NIE, Milonakis and Fine note the 'continued reliance on formalism and model building'<sup>27</sup> and the way NIE achieved 'greater rigour', at the expense of the old Institutionalism. Toye even notes that 'unless transaction costs are quantified, they are not being taken into account properly'.<sup>28</sup> According to Moe, the advantage of using the kind of simple analytical framework beloved of NIE is that '[i]ssues can be cast in a clear and rigorous manner that allows for the application of conventional economic methods. A corresponding disadvantage, however, is that such a framework sometimes encourages highly complex mathematical treatment of trivial problems'.<sup>29</sup>

Consider Milonakis and Fine's comments on economic history. According to Fogel: 'The methodological hallmarks of the new economic history are its emphasis on measurement and its recognition of the intimate relationship between measurement and theory'. This, according to Milonakis and Fine, amounted to the 'official introduction of positivism into economic history'.<sup>30</sup>

On 'freakonomics', Milonakis and Fine note Levitt and Dubner's recognition of the 'endless math' that appears to bore the creative spark out of a generation of grad students<sup>31</sup> and their claim that 'Economics is above all a science of measurement'.<sup>32</sup> Milonakis and Fine comment on Blaug's report of a survey showing 'lack of interest in the real world on the part of elite economics

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25. Fine and Milonakis 2009, p. 125.

26. Fine and Milonakis 2009, p. 162.

27. Fine and Milonakis 2009, p. 83.

28. Fine and Milonakis 2009, p. 85.

29. Fine and Milonakis 2009, p. 86.

30. Fine and Milonakis 2009, p. 96.

31. Fine and Milonakis 2009, p. 107.

32. *Ibid.*

graduates as opposed to honing their skills in the latest econometric and mathematical economics'.<sup>33</sup>

Milonakis and Fine are keenly aware of the mathematisation of economics – although it is unclear whether they endorse its use or not. They recognise that 'mathematical formalisation of the theory has both reflected its reduced analytical content and precluded wider considerations'.<sup>34</sup> They cite the authority of Friedman, who observed that: 'Economics has become an increasingly arcane branch of mathematics rather than dealing with real economic problems'.<sup>35</sup> They also cite the authority of Leontief who bemoans the fact that '[p]age after page of professional economic journals are filled with mathematical formulas leading the reader from sets of more or less plausible but entirely arbitrary assumptions to precisely stated but irrelevant theoretical conclusions'.<sup>36</sup> They also cite Lazear, for whom

The power of economics lies in its rigour. Economics, he goes on, 'is scientific: it follows the scientific method of stating a formal refutable theory, testing the theory, and revising the theory based on the evidence. Economics succeeds where other social sciences fail because economists are willing to abstract . . . Economists are not alone among social scientists in following this method, but this form of enquiry has become standard for economic research'.<sup>37</sup>

### **Key methodological ideas: a summary**

Let me try to pull this selection of methodological comments together. As political economy evolved into economics, it abandoned some meta-theoretical concepts, introduced and elaborated upon others, and ended up with a meta-theoretical approach that appears to have the following characteristics:

- It deals with (allegedly) quantifiable, measurable phenomena such as quantities supplied and demanded, prices, profits, rents, labour, capital, utility and so on.
- Being (allegedly) quantitative makes it amenable to mathematics and statistics, especially the key mathematical device, the function, and raises the possibility of law-like relations where attaining the status of a law depends upon the existence of event regularities.

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33. Fine and Milonakis 2009, p. 137.

34. Fine and Milonakis 2009, p. 131.

35. Fine and Milonakis 2009, p. 130.

36. Fine and Milonakis 2009, p. 147.

37. Fine and Milonakis 2009, p. 145.

- Being quantitative, mathematical, statistical, and making use of functions and law-like relations does not necessarily mean being empirical. Pure theory, that is, purely algebraic models that are never confronted with empirical data are still quantitative, mathematical, and use functions and law-like relations.
- Being quantitative, mathematical and statistical raises the possibility of making predictions in the form of hypotheses, that can be tested (or subject to an attempt to falsify them) via empirical data. This makes the meta-theory appear to be similar to that allegedly used in natural sciences.
- This meta-theoretical approach makes extensive use of processes variously described as abstraction, isolation, formalisation, idealisation and ideal-typification.
- This approach presupposes a very specific notion of causality: causality as event regularity.

What, one might ask, do we call this meta-theoretical approach? Whilst it is almost never named, defined, or stated unambiguously, and whilst different economists make use of different aspects of it, it appears to consist of some ill-conceived and *ad hoc* jumble of logical positivism, logical empiricism, the deductive nomological model, the hypothetico-deductive model, the inductive-statistical, and/or the covering-law model, often with a nod towards falsification. Precisely because this approach is an ill-conceived and *ad hoc* jumble of meta-theoretical concepts, there is little point in trying to define it precisely. Indeed, to do so would present this meta-theory with a degree of systematicity and legitimacy that it neither has nor deserves. I suggest we refer to this meta-theoretical approach as ‘scientism’ – defined in the following section. The deliberately ambiguous term ‘scientism’ works because it expresses the highly ambiguous nature of the approach.

Having sketched Milonakis and Fine’s impressive account showing that the various twists, turns and reductions in political economy and economics are rooted in meta-theory, let me change tack and press some of Milonakis and Fine’s meta-theoretical arguments further than they actually do.

### **Scientism and economics imperialism**

Economists have monopolised the term ‘science’ and ‘scientific’, and these terms have become associated with ‘rigour’ and ‘rigorousness’. Terms like this play an important ideological role. By presenting economics as using a ‘scientific’ meta-theory, both the meta-theory and the discipline become associated with rigorousness and understood as ‘like’ natural science which,

after all, can (rightly) claim many success stories. The ideological message is that whilst economics is 'scientific', 'rigorous', and 'successful', social science is 'unscientific', 'non-rigorous' and, thereby, 'unsuccessful'. This gives a wholly unwarranted legitimacy to economics and its scientific meta-theory. So what is 'scientism'?

The *Collins Dictionary of Sociology*<sup>38</sup> defines scientism as 'any doctrine or approach held to involve oversimplified conceptions and unreal expectations of science, and to misapply "natural science" methods to the social sciences'. Hughes and Sharrock define scientism as 'those philosophies such as positivism, which seek to present themselves as having a close affiliation with the sciences and to speak in their name, and which then go on to fetishise the so called scientific standpoint'.<sup>39</sup> Scientism, then, refers to the employment of meta-theory and research techniques that look similar to (some branches of) natural science, without actually specifying what they are. By closely resembling some under-elaborated notion of (natural) 'science' and by using scientific language and concepts (e.g. prediction, replication, quantification, hypothesis testing, mathematics and statistics) scientism gains a wholly unwarranted veneer of scientificity. Scientism looks like genuine science, but is not.

In recognising the influential role of discourse, postmodernists and poststructuralists have been quite correct in pointing out the socially constructed nature of (much) scientific knowledge. By using the discourse of 'science' (more accurately described as 'scientism') economics is better able to carry out its imperialist project. It does this in two ways.

First, whilst economics is meta-theoretically weak, we should not overlook the fact that a great deal of social science is (relatively speaking) not much better. A glance at virtually any methodology of social science textbook reveals an unresolved tension between positivists on the one hand and postmodernists and poststructuralists on the other, with *inter alia* phenomenologists, hermeneuticists, ethnomethodologists, post-positivists and critical realists floating about somewhere in the middle. This bewildering array of perspectives leaves many social scientists, especially empirical researchers, lacking clear meta-theoretical foundations and encourages the (understandable) desire to avoid seemingly arcane debates and just get on with 'doing' social science. Unfortunately, however, 'doing' this often defaults to ill-conceived forms of scientism where weak theory is used 'to suggest statistical relations between variables with little more than guilt by association through reference to some

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38. Jary and Jary (eds.) 1995.

39. Hughes and Sharrock 1997, p. 208.



theory or other'<sup>40</sup> – i.e. forms of crude or naive empiricism, ultra-empiricism or measurement without theory.

Being (relatively) meta-theoretically weak makes social science extremely vulnerable to colonisation from economics. Why? Because economics, unencumbered by seemingly arcane meta-theoretical debates, and with what looks like crystal-clear meta-theory, steps in proclaiming that the economic approach, being 'scientific', is the answer to social science's meta-theoretical woes. And social scientists, being unsure about their own meta-theory, are not always sure how to respond. The example of Labour Economics is instructive. The 'scientific' approach has, advocates claim, allowed modern Labour Economists to throw off its previous flirtation with Institutionalism and Industrial Relations. Fallon and Verry articulate the point:

Firstly, what used to be a largely descriptive and institutional subject, often virtually synonymous with industrial relations, has become more analytical. Secondly, the subject has become more quantitative. Some very advanced econometric techniques are now commonly used in labour economics research... these subjects are now treated much more rigorously.<sup>41</sup>

Almost two decades later, McConnell, Brue and Macpherson repeat a similar sentiment:

Economists have achieved important analytical breakthroughs in studying labor markets and labor problems. As a result, economic analysis has crowded out historical, institutional, legal, and anecdotal material. Labor economics increasingly became applied micro and macro theory.<sup>42</sup>

In the hands of Labour Economists, who have used scientism to crowd out historical, institutional and legal material, this discipline has now become devoid of significant insight; a highly mathematical exercise in irrelevance.<sup>43</sup>

Second, the ideological power of 'scientific-looking' discourse is hard to underestimate. This can be exemplified via the New Economics of Personnel, which is currently colonising the disciplines of Human Resource Management and Employee Relations. Consider the following passage:

In order to test the indirect effect, we used Preacher and Leonardelli's interactive programme to calculate the critical z ratio. The values used in this interactive

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40. Fine and Milonakis 2009, p. 108.

41. Fallon and Verry 1988, p. ix.

42. McConnell, Brue and Macpherson 2006, p. 3.

43. Fleetwood 1999.

programme are taken from table 7 where the non-significant variables are withdrawn from the relationships through backward regression, in order to get better estimates. The values used are  $b = 1.046$ ,  $s_b = 0.060$ ,  $b_2 = 0.335$  and  $s_{b_2} = 0.066$ . The z values obtained are  $z_{\text{Sobel}} = 4.873$  ( $p = 0.000$ ),  $z_{\text{Goodman(I)}} = 4.866$  ( $p = 0.000$ ) and  $z_{\text{Goodman(II)}} = 4.881$  ( $p = 0.000$ ). These z values indicate that the indirect effect of HRM systems on organizational performance is significantly different from zero.<sup>44</sup>

Faced with statistical language like this, social scientists untrained in econometrics are at a distinct disadvantage. Many will shift uneasily in their seats embarrassed at their ignorance – even if they have some (possibly unclear) reason to doubt the appropriateness of these techniques. Others will genuflect, marvelling at the technical abilities of economists, cursing their own lack of training in these areas and the lack of rigour of their own discipline. Economics imperialism is handed an easy victory.

In short, then, the new generation of political economists should oppose this ideological manoeuvre by refusing to allow economics any association with (genuine) science. Referring to the meta-theory used by economists as ‘scientism’ goes some way to exposing this ideology.

### Scientism’s harmful effects on a new generation of political economists

It is, arguably, a mistake, although a common one, for heterodox economists to presume that the problems besetting economics are theoretical, not meta-theoretical. This encourages the idea that it is possible to retain the scientific approach (especially the commitment to quantification, mathematics and statistics, use of functional relations, search for law-like regularities and so on) but merely change the theoretical components and technical apparatus. Out go things such as scarcity, utility, rationality, maximisation, individualism, production and supply-and-demand functions, indifference maps, efficiency, perfect competition etc. In come whatever theoretical components and technical apparatus are the focus of the heterodox school in question: aggregate demand; quantities of labour embodied; empirical estimates of declining profit rates etc. Good examples of this are Sraffian economics and Rational Choice Marxism (RCM). Sraffians and RTMs use the same meta-theory as economists, whilst using a different set of theoretical components and technical apparatus. The result is logically consistent, but hopelessly unrealistic models – an issue elaborated upon below.

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44. Katou and Budhwar 2006, p. 1243. Note that the technical ability of Katou and Budhwar is not being criticised here.

Space does not permit me to elaborate upon all the harmful effects of scientism, so I have opted to focus on abstraction, isolation, idealisation, fictionalisation and ideal types. Let us consider some of Milonakis and Fine's comments on these concepts.

With his labour theory of value, Smith deploys 'an abstract causal category'. Ricardo is engaged in 'deliberately and totally abstracting from empirical reality'<sup>45</sup> and making 'necessary abstractions and simplifying assumptions'.<sup>46</sup> Marx criticises Ricardo because he 'omits some essential links'.<sup>47</sup> Himmelweit and Mohun criticise Ricardo for employing a 'model built on assumptions rather than the theorization of a real world process by means of abstraction. Assumptions are thought constructs which have no real existence but are invented in order to simplify... the analysis'.<sup>48</sup> Marx was 'firmly in favour of abstract theorising',<sup>49</sup> but in place of Ricardo's formal abstractions Marx's are 'real, concrete, rational abstractions, in accordance with material reality'.<sup>50</sup> According to Bridel, Walras's tatonnement mechanism is nothing more than (or not even) 'an idealized representation of a virtual market process'.<sup>51</sup> The Historical school insisted upon 'realism of abstraction'.<sup>52</sup> According to Schmoller, Menger employs a 'process of abstraction from the individual phenomena of the empirical world to discover their essences, to isolate them, and then to use the simplest elements so obtained to deduce how more complicated phenomena develop from the simplest part'.<sup>53</sup> Commons states that: 'the economist must abstract from the empirical data of history only so much as is needed... to construct an all-round ideal type for the particular phase of history which, as an economist, he is concerned with'.<sup>54</sup> Weber is, of course, the thinker most associated with ideal types, and whilst Milonakis and Fine cite him<sup>55</sup> it remains unclear whether ideal types are abstractions or what I will refer to below as fictionalisations. Wieser is interesting for his explicit use of what later came to be known as the method of successive approximation. Whilst on a superficial reading this might be mistaken for Marx's *modus operandi*, it is not. Weiser advocates a process of isolation and idealisation.

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45. Milonakis and Fine 2009, p. 16.

46. Milonakis and Fine 2009, p. 22.

47. Milonakis and Fine 2009, p. 35.

48. Milonakis and Fine 2009, p. 36.

49. *Ibid.*

50. Milonakis and Fine 2009, p. 37.

51. Milonakis and Fine 2009, p. 283.

52. Milonakis and Fine 2009, p. 86.

53. Milonakis and Fine 2009, p. 112.

54. Milonakis and Fine 2009, p. 179.

55. Milonakis and Fine 2009, p. 199.

Milonakis and Fine describe Weiser's method thus: 'Starting from the most abstract isolating and idealizing assumptions, the theorist builds up step by step a system of decreasing abstraction by rendering his assumptions more concrete and multiform'.<sup>56</sup>

There is, however, a degree of ambiguity in Milonakis and Fine's references to abstraction, isolation, idealisation, fictionalisation and ideal types. Please note my awareness of Milonakis and Fine's awareness of the problems that arise when economic theories and models fail to reflect, express, grasp, correspond to (or whatever verb is most appropriate) the phenomena they purport to be theories and models of. Nonetheless, Milonakis and Fine do not always differentiate between abstraction, isolation, formalisation, idealisation, ideal types, and, in places, seem to use the terms interchangeably. Unfortunately, however, anything less than crystal clarity in using terms like these lets economists off the hook on an issue where political economists have something radically distinct to offer. Consider two examples where the impression is given that economics engages in abstraction:

At the same time, the use of marginal analysis (an essentially mathematical tool) and the concept of equilibrium (borrowed from statistical mechanics) made economics more susceptible to mathematical analysis, pushing economic science further down the road of abstraction and formalism.<sup>57</sup>

Second, the economy as market relations now constituted a distinct object of study, with the discipline of economics to undertake this task, by focusing on the economic aspects of behaviour in abstraction from any other social influences.<sup>58</sup>

Let me try and persuade you to avoid claiming, hinting, or inadvertently implying that economists engage in a process of abstraction. First, let me deal with a red herring. No-one doubts that if theory did not abstract from the phenomenon it sought to theorise, then theory would be a kind of one-to-one description of that phenomenon. From here on, though, doubt emerges. To abstract is (minimally) to leave things out of the analysis. The result of this process of abstraction we refer to as an abstraction. Marxists have done more than most to try and create abstractions that reflect, express, grasp, or correspond to the phenomena under investigation. I will call this a commitment to realism. This commitment means that those things which are left out are not those that would damage the theory's realism. But abstraction demands more than leaving out. It also demands that what is left in is theorised

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<sup>56</sup> Milonakis and Fine 2009, p. 252.

<sup>57</sup> Milonakis and Fine 2009, p. 109.

<sup>58</sup> Fine and Milonakis 2009, p. 4.

in (realistic) ways that allow the categories to unfold dialectically. Perhaps the best example of this is Marx's derivation of money from the category of the commodity in *Capital*, Volume 1. Note that nothing Marx introduces at later stages in his presentation impact 'backwards', as it were, and renders earlier categories unrealistic. Abstraction relates the domains of the epistemic and the ontic so that theory reflects, expresses, grasps, or corresponds to its object. If Marx ends up at any stage with unrealistic categories, then he will have made a mistake somewhere.<sup>59</sup>

Second, and this really is the point of the argument, the process of abstraction I have just (briefly) described has nothing whatsoever to do with what economists often misleadingly refer to as 'abstraction' – and I will use scare quotation-marks to refer to this process as (mis)understood by economists. Consider the following comments from information-theoretic economists.

Suppose that output from a group of identical workers is some function of each worker's effort. . . . To motivate the analysis we introduce a 'peer pressure' function

$$\text{peer pressure} = P(e_i; e_j, \dots, e_N, a_i, a_j, \dots, a_N)$$

The peer pressure that worker *i* feels depends generally on his own effort,  $e_i$ ; on the effort of his peers  $e_j, \dots, e_N$  and on the actions that he and his peers may take  $a_i, \dots, a_N$ . . . . The peer pressure function is an attempt to formalize the discussion of tastes. By making explicit assumptions about  $P(\ )$ , we clarify the exact nature of the tastes required to explain a particular behavior.

Suppose that the world consisted of two types of workers: the social for whom  $P_1 < 0$ , and the independent for whom  $P = 0$ . . . . If one's type is known by the individual himself, does a separating equilibrium exist in which each type of worker prefers firms of his own kind?<sup>60</sup>

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59. It is often thought that Marx's dialectical method of presentation (which builds upon the method of analysis), as it moves from the abstract to the concrete, is a method of successive approximation. Much clearly depends upon how these concepts are understood and defined. Economists claim to use a method of successive approximation, where assumptions are gradually relaxed and the theory moves, via successive increments, to ever closer approximations to reality. In truth, however, these incremental shifts never become closer approximations to reality – indeed, it is usually the case that more (fictitious) assumptions are added in as the theory gets more complex. I have described this elsewhere as better understood as a method of *successive closures* or of *successive fictionalisations* (Fleetwood 2001, reprinted as Fleetwood 2002). Marx's method, by contrast, does not rely on unrealistic assumptions at any stage of the process, has no 'gap' to close, with successive approximations. Indeed, the move from the abstract to the concrete is not one from irrationality to reality: the abstract is realistic.

60. Kandel and Lazear 1992, pp. 803–14.

Kandel and Lazear also ask us to '[s]uppose that, in addition to exerting effort, workers can monitor each other at a cost. Workers who are caught shirking can be penalized by their partners [by] mental or physical harassment'. They then define 'the expected penalty associated with being caught shirking' in functional terms and assume that 'since all workers are *ex ante* identical, the choice of monitoring level  $k$  will be identical. Each worker chooses a "monitoring level" and "puts forth monitoring efforts" because he believes that other workers will increase their effort as a response'.

Let us be in no doubt: Kandel and Lazear are not engaging in a process of abstraction, and the categories they use are not abstractions. They are engaging in a process of fictionalisation. Many of these claims and assumptions are not simply unrealistic, they are fictitious, and known to be fictitious by those who use them. The kind of theories exemplified above are populated by agents known to be fictitious, inhabiting environments known to be fictitious, undertaking forms of behaviour known to be fictitious, and doing so for reasons known to be fictitious. But it gets worse when we enquire into what is excluded. By leaving out absolutely crucial categories (e.g. class, power, shop-floor management regimes) theory ends up being fictitious by omission. I will not waste time demonstrating that real workers undertaking real tasks, for real reasons in real workplaces are not like this, because this is freely admitted by most advocates of this kind of theorising. Claiming, implying, or inadvertently giving the impression that this fictionalisation has anything in common with abstraction is dangerous. Economists make no attempt whatsoever to be realistic; to relate the domains of the epistemic and the ontic; or to present a dialectical unfolding of the categories. The process of fictionalisation is carried out for one purpose: to make theories and models mathematisable or formalisable – and 'formalisation' is a euphemism for mathematisation.

Now, sometimes this process of fictionalisation is referred to using the term 'idealisation'. I think this is misleading and that we should avoid it. The term itself is undefined and ambiguous. Idealisation is (a) sometimes used by political economists in a pejorative sense to refer to 'abstraction', where 'abstraction' is equivalent to idealisation; (b) sometimes used by political economists in a non-pejorative sense to refer to abstraction, where abstraction is equivalent to idealisation; and (c) sometimes used in a non-pejorative sense by economists to refer to what they call 'abstraction', where 'abstraction' is equivalent to idealisation. In case (a) there is little or no confusion. In cases (b) and (c), however, confusion is almost impossible to avoid. In these cases, when political economists use the term 'idealisation' they have in mind a (legitimate) process of abstraction; but when economists see the term, they have in mind an (illegitimate) process of 'abstraction'. This ambiguous use of the term

‘idealisation’, then, has the effect of making it difficult to differentiate between (legitimate) abstraction and (illegitimate) abstraction or fictionalisation.

In short, to avoid scientism’s harmful effects, the new generation of political economists must know exactly what scientism is, why it should be abandoned and what the alternatives to it are. I do find it a little disconcerting that, of the ten questions for political economy appearing in the final chapter of *From Economics Imperialism to Freakonomics*, not one refers to meta-theory.

### Scientism and the information-theoretic turn

In *From Economics Imperialism to Freakonomics*, Milonakis and Fine show that twenty-first century economics has turned from an economics based upon perfect competition, perfect information and complete markets, to one based upon imperfect competition, imperfect information and incomplete markets. Modern economics, therefore, uses concepts such as information asymmetry, uncertainty, moral hazard, adverse selection, efficiency-wages and the endogenisation of things that were previously taken as exogenous. Whilst this is undoubtedly correct, Milonakis and Fine seem to offer two interpretations of information-theoretic (I-T) economics. One interpretation makes it fairly clear that this turn does not represent a complete break with previous meta-theory. The other interpretation, which I should point out is less clear, suggests that I-T economics has made significant advances over its predecessor. The following comments are evidence of both interpretations:

What is more fundamentally innovative within the new microeconomics of informational asymmetry is its ability to examine social structure, institutions and customs, albeit on the continuing basis of the peculiar form taken by methodological individualism...it also extends the scope of the analysis more or less indefinitely across the social sciences. And it does so in a way that is more palatable to them. For the new phase of economics imperialism prides itself on not treating the non-economic as if it were a market but positively promotes itself by declaring that institutions, customs, habits and history matter.<sup>61</sup>

[T]he new approach is able to explain structures or corresponding macro-economic outcomes despite continuing to be based on the optimising behaviour of individual agents in response to asymmetric information...it is remarkable for its success in endogenising economic structure where previously such structures had to be taken as exogenously given constraints...and due to rigidities, institutions, or whatever, possibly to be accounted for by other social

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61. Fine and Milonakis 2009, p. 58.

sciences. . . . Consequently, institutions in general can be understood as the non-market response to market imperfections.<sup>62</sup>

Culture, customs, norms, habits and trust, for example, are no longer taken to be exogenous (as for institutions previously) or non rational. Rather such behaviour can be explained as the rational or optimising response to market imperfections.<sup>63</sup>

[I]t still remains true that in all other respects the new information school remains strongly attached to the neoclassical paradigm. All essential features of the neoclassical world, including methodological individualism, instrumental rationality, equilibrium price theory, marginalism and stable preferences remain intact.<sup>64</sup>

[All the above] strengthened the technical nature of economics as a discipline in terms of its use of mathematical modelling; . . . symbolises the professionalization of economics in terms of its core method, theory, axioms and techniques . . . [thereby] continuing the traditional unworldliness and technicism of economics.<sup>65</sup>

In the rest of this section I will argue, unambiguously, against any possible interpretation that suggests that I-T economics has made significant advances over its predecessor.

As Milonakis and Fine themselves point out, I-T economics remains strongly attached to neoclassical economic theory, its fundamental concepts, its use of mathematics, and, I would add, its use of scientism. Take, for example, the instrumental rationality of *homo economicus*. Only a few economists (e.g. Becker and his followers) really believe *homo economicus* is a realistic approximation of human beings, the rest knowingly use this assumption as a convenient fiction. The moment a theory or model makes use of a known fiction such as *homo economicus* (even if it does allow for the possibility that he acts instrumentally-rationally but without complete information) the theory or model immediately becomes fictitious and unrealistic. And I am not sure how a fictitious and unrealistic theory or model can be said to be an advance.

Take, as another example, the introduction of efficiency-wage concepts into models of labour markets. These models rely on theoretical devices rooted in assumptions of perfect competition, even if they do extend into (theoretical) ‘environments’ that are less than perfect. True, the outcome is a non-market clearing wage, but ask yourself what an efficient wage mark-up is a mark-up on?

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62. Fine and Milonakis 2009, p. 65.

63. *Ibid.*

64. Fine and Milonakis 2009, p. 63.

65. Fine and Milonakis 2009, pp. 60–3.



Actually, it is a mark-up on the market clearing wage – a concept rooted in perfect competition. I-T concepts, then, often have ‘one foot’ as it were in its predecessor.

Might this be legitimised by pointing out that, by comparing the non-market clearing wage to the market clearing wage, we can gain important insights? As Milonakis and Fine put it, I-T economics offers an ‘important understanding of the ways in which markets do or do not work perfectly, especially by way of departure from the model of perfectly working markets’.<sup>66</sup> I once heard Frank Hahn defend economics along these lines via the following metaphor: This is like a doctor comparing a faulty human heart to a healthy one and then using the difference to further our understanding of heart disease. This metaphor is, however, misleading. This is actually like a doctor comparing a faulty heart to a (fictitious and unrealistic) model of a heart. Comparing non-market clearing wages to market clearing wages (or any similar moves) is more like comparing a ‘bit of a fiction’ to a ‘complete fiction’ and then suggesting that comparing two relative fictions informs us about the non-fictitious world.

Let me now consider Milonakis and Fine’s suggestion that I-T economics is capable of ‘explaining’ a whole range of phenomena that were previously impossible to explain, such as social structure, institutions, customs, habits and so on. This suggestion sits uneasily with their own social theory elaborated in Chapter 8 of *From Economics Imperialism to Freakonomics*:

[I]n the form of methodological individualism adopted by neoclassical economics, the individual becomes the basic unit of analysis. . . . Even, however, within new institutional economics, everything from the existence of institutions to structural change is seen as a result of the (rational) action of individuals. Individual agency takes precedence over structural factors and the latter are either treated as the results of individual action . . . or else are taken as exogenously given and, as such, are not explicable from within the model. Once in place, institutions influence behaviour by acting as constraints on individual choice.<sup>67</sup>

If this observation is correct, and I think it is, it is difficult to hold on to the suggestion that I-T economics can explain social structure, institutions, customs and habits. In I-T economics, structures, institutions, customs and habits are ‘either (a) treated as the results of individual action or else (b) are taken as exogenously given and, as such, are not explicable from within the model’. I agree fully, and here is why. If, as in (b) these things are assumed exogenous, it is obvious that they are beyond explanation in terms of economics,

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66. Fine and Milonakis 2009, p. 64.

67. Fine and Milonakis 2009, p. 253.

so this is clearly not an advance. The other claim (a) is a little more complex, so allow me to elaborate.

Just like its predecessor, I-T economics presupposes (ontological, epistemological and methodological) individualism and, thereby, a social world consisting of nothing more than individuals and the outcomes of individuals' ideas and actions. Non-agential things like structures, institutions, customs and habits are treated in a reductionist manner. As a result, I-T economists cannot (and I mean this quite literally) even begin to conceptualise, categorise, theorise, research or indeed explain anything that is not reducible to agents' ideas and actions. Milonakis and Fine know this, and their comment on North's (failed) attempt to define class<sup>68</sup> is simply one example amongst several.

This individualism is in complete contrast to methodological structuralists such as Milonakis and Fine; critical realists such as myself; and Institutionalists such as Hodgson. For these (and others) structures, institutions, customs, habits, norms, rules and so on, exist independently of agents, that is to say they exist objectively.<sup>69</sup> Moreover, they can be explained without reducing them to the ideas and actions of agents. The fact that I-T economists refer to 'institutions' and 'customs' (they rarely use terms such as 'structures' and 'habits', but sometimes refer to 'rules' and 'norms'), or their recognition that these things play a role in economic activity does not mean they are able to provide a plausible explanation of them.

In sum, because I-T economics is, quite literally, incapable of 'explaining' social structures, institutions, customs, habits, norms, rules and so on, it cannot be interpreted as an advance over its predecessor. Making this clear might prevent the new generation of political economists from getting blown off course by the suspicion that I-T economics might have something to offer. It is the antithesis of political economy and we should have no truck with it.

### Methodological structurism

In Chapter 8 of *From Economics Imperialism to Freakonomics*, Milonakis and Fine advance what they refer to as methodological structurism. Let me say immediately that I largely agree with Milonakis and Fine's proposal to employ something like methodological structurism – although I think it is better described as a social ontology than a method. Unfortunately, however, they overlook a significant body of contemporary social theory that is readily

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68. Fine and Milonakis 2009, p. 156.

69. Fine and Milonakis 2009, p. 154.

available for the new generation of political economists to draw upon. I have in mind critical realism. As a keen advocate of critical realism, I should tell the reader explicitly that I am hardly a disinterested commentator on this matter. Nonetheless, many readers (critical realists or otherwise) will be astonished to find that Milonakis and Fine could write a section outlining an appropriate methodology for political economy and hardly mention critical realism – although it does get the odd mention in their two volumes.

Political economists cannot, and I would argue should not, ignore critical realism, because it is, arguably, the most complete alternative to scientism, if for no other reason than its ability to deal with philosophy of science, ontology, aetiology, epistemology, methodology as well as notions of agency and structure. Whilst methodological structurism has something to offer vis-à-vis agency and structure, what does it offer vis-à-vis philosophy of science, ontology, aetiology, epistemology and methodology? Does it, for example, accept (a<sub>1</sub>) the notion of causality as regularity; (b<sub>1</sub>) the hypothetico-deductive method; and (c<sub>1</sub>) the ‘flat’ ontology of empirical realism, all *pace* scientism? Or does it accept (a<sub>2</sub>) the notion of causality as powers; (b<sub>2</sub>) the causal-explanatory method; or (c<sub>2</sub>) a ‘layered’ and ‘transformational’ ontology, all *pace* critical realism? A new meta-theory to underpin a new political economy must be able to deal with these (and many other) concepts. Whilst critical realism cannot claim to be the last word on meta-theory, it provides a good place to start.

Furthermore, the agency-structure apparatus that Milonakis and Fine advocate (which incidentally is compatible with critical realism’s ‘transformational’ social ontology) has, in the hands of several critical-realist social theorists, undergone many significant advances<sup>70</sup> since Giddens first penned structuration theory in the late 1970s and 80s. Ignoring these advances is unlikely to assist the building of a new political economy for the future.

## Conclusion

Milonakis and Fine have done political economy a great service by drawing attention to the insights lost in the twists, turns and reductions in the transition from political economy to economics. These two volumes constitute a solid foundation upon which a new generation can build a political economy for the future. By pressing some of Milonakis and Fine’s meta-theoretical arguments a little further than they actually do, I hope to have ‘toughened up’ the new political economy, thereby making it more able to carry the fight to economics.

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70. If I were to single one such critical realist theorist for special attention, it would have to be Margaret Archer (Archer 1995, 2000, and 2003).

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