

# ***Economic Thought***

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## **Table of Contents**

### **Editorial Introduction**

*John Latsis, Alejandro Nadal, and Annalisa Roselli* 1

### **Mathematical Modelling and Ideology in the Economics Academy: Competing explanations of the failings of the modern discipline?**

*Tony Lawson* 3

### **Economics and Research Assessment Systems**

*Donald Gillies* 23

### **Richard Cantillon's Early Monetary Views?**

*Richard van den Berg* 48

### **Different Approaches to the Financial Crisis**

*Sheila C. Dow* 80

### **On the Limits of Rational Choice Theory**

*Geoffrey M. Hodgson* 94

### **An Evolutionary Efficiency Alternative to the Notion of Pareto Efficiency**

*Irene van Staveren* 109





## Editorial Introduction to the First Issue of *Economic Thought*

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In August 2011 Edward Fullbrook, the editor of the *Real World Economics Review* and the driving force behind the World Economics Association (WEA), asked the three of us to take on editorial roles in the newly established philosophy, methodology and history of thought journal of the association. The project was, to say the least, rather daunting. We had a document outlining the scope and editorial policies of the would-be journal written by Grazia Letto-Gillies. Grazia had also developed the proposed Open Peer Discussion Forum (OPD), based on her research into similar peer review systems used within the natural sciences. However, we had never met each other nor worked together and we lacked administrative and technical support. In addition, scheduling editorial meetings between Rome, Mexico City and Oxford proved rather challenging! Nevertheless, a combination of belief in the cause of the WEA and foolhardiness led us all to accept. The first issue of the journal is the fruit of our collective labours as well as those of our authors. In addition, both Jake McMurchie (the association's IT expert) and Jane Spurr (our production and layout expert) have been instrumental in the process of transforming a vibrant open peer discussion into six conventional journal articles.

Before introducing those articles and providing a brief outline of their content, the project of *Economic Thought* is worth explaining and justifying in a little more depth. The journal and its sister publication the *World Economic Review* are sponsored by the WEA, which was launched on May 16<sup>th</sup> 2011 by an eminent group of dissident economists. The practical and intellectual impetus for creating the WEA came from the parochialism and narrowness of the majority of the dominant associations of economists and the perceived failings of the discipline in the face of the ongoing economic crisis, to which those associations had not provided a satisfactory response. In a press release published one week after the launch of the association, the founding members evinced a commitment to increase 'the relevance, breadth and depth of economic thought', in addition to supporting theoretical pluralism, broadening geographical focus, and providing transparent and democratic governance mechanisms. The two journals are a key element of the WEA's attempt to open up economics to different voices from within and outside academic institutions and from different intellectual and disciplinary backgrounds. The journal is also a proud part of the open access publishing movement that is gaining ground at the time of writing. All our issues will be provided free of subscription charges on our Open Journal Systems website and the costs of the journal will be absorbed by the association.

As a journal that focuses on the history, philosophy and methodology of economics, *Economic Thought* enters a field in which the contributions of heterodox and critical scholars already loom large due (in part) to the relative lack of interest from mainstream economists in these topics. The troubled state of the mainstream gives further impetus to the critical analysis of the genesis and evolution of contemporary theory. This is why the history of economic thought, as well as philosophy and methodology, should be seen as active and important research programmes rather than visits to the museum of old or obsolete doctrines. We hope that the addition of a high-quality publication outlet to the small list of existing titles in this area will be welcomed by the academic community. As editors, we also believe that *Economic Thought* has three distinct advantages over competing and complementary journals in the field. First,

thanks to the phenomenal success of the WEA we have a subscription base that far exceeds most journals: 10,251 and counting at the time of writing this introduction. Second, our open peer review process provides an unprecedented level of transparency and fairness in the refereeing process. Our authors are provided with advice, commentary and constructive criticism in a public arena designed to address the weaknesses of the current peer review system. Whilst this does not mean that all papers will be published, we hope that all our authors will emerge from the OPD with interesting feedback and new insights, provided in a timely fashion. Third and finally, *Economic Thought* does not simply pay lip service to the idea of pluralism. As our first issue clearly demonstrates, we are committed to publishing on all topics that fall within our scope and we encourage interdisciplinary contributions as well as contributions engaging with a broad range of authors and traditions.

Finally, we should write a few words about the six papers that make up the first issue of the journal. We were delighted when six such eminent scholars agreed to submit articles to our fledgling journal – including four members of our editorial board. We were also pleased to see the numerous thoughtful and constructive responses to those papers on the open peer discussion forum and the role that those responses played in the revision of the original manuscripts. There was no overall theme, so the first issue is eclectic, as we expect the journal to continue to be.

Our first two articles are methodological contributions, touching on themes that are close to the interests of the WEA membership. Tony Lawson's 'Mathematical Modelling and Ideology in the Economics Academy', develops themes from the author's research in constructing a critical response to the oft-cited claim that mainstream economics is in some sense 'ideological'. Lawson rejects the dominant arguments in favour of an ideological interpretation of the mainstream, but draws our attention to the uncontested and uncontestable status of mathematics in the mainstream. Donald Gillies selects another important topic by focussing on the impact of national research assessment systems on the output of academic economists. His paper is part of a larger project looking at the epistemological implications of research assessment for the production of scientific knowledge. His conclusions run counter to the conventional wisdom that quantitative measurement and accountability in research produce better results.

Richard van den Berg provides the only purely historical paper in the first issue of *Economic Thought*. His article suggests an intriguing reply to the time-honoured question of the relation between Richard Cantillon and David Hume. It presents a novel interpretation of how Cantillon's theories developed over time, drawing on the writings of his cousin Philip as evidence. We return to methodological concerns in the fourth article in which Sheila Dow connects the theoretical approach adopted by the mainstream to analyse the financial crisis to the perceived failings of that analysis. Using two case studies – the treatment of market sentiment and the notion of moral hazard – Dow shows how the adoption of mainstream theoretical approaches translates into policy-making.

The last two articles provide stimulating and critical discussions of the philosophical foundations of economics. Geoffrey Hodgson provides a critical deconstruction of the underpinnings of rational choice theory and, in particular, its reliance on maximisation assumptions. He argues that whether they maximise manifest payoffs or utility, the agents of rational choice models are unlikely to explain real human behaviour. The latter can only be done through the construction of a more sociologically and psychologically grounded theory of human action. In her contribution, Irene van Staveren develops a critique of the mainstream notion of Pareto efficiency. Showing how the normative underpinnings of the latter lead to a static conception of efficiency that cannot cope with the flow of events in actual economies, she proposes a dynamic, evolutionary alternative.

We hope the readership of *Economic Thought* and the WEA membership more generally enjoy reading these papers as much as we did and that the many lively discussions initiated in the OPD continue in print in the pages of this journal and others for many years to come.

# Mathematical Modelling and Ideology in the Economics Academy: competing explanations of the failings of the modern discipline?

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## Abstract

The widespread and long-lived failings of academic economics are due to an over-reliance on largely inappropriate mathematical methods of analysis. This is an assessment I have long maintained. Many heterodox economists, however, appear to hold instead that the central problem is a form of political-economic ideology. Specifically, it is widely contended in heterodox circles that the discipline goes astray just because so many economists are committed to a portrayal of the market economy as a smoothly or efficiently functioning system or some such, a portrayal that, whether sincerely held or otherwise, is inconsistent with the workings of social reality. Here I critically examine the contention that a form of political-economic ideology of this sort is the primary problem and assess its explanatory power. I conclude that the contention does not fare very well. I do not, though, deny that ideology of some sort has a major impact on the output of the modern economics academy. However it is of a different nature to the form typically discussed, and works in somewhat indirect and complex ways. Having raised the question of the impact of ideology I take the opportunity to explore its play in the economics academy more generally.

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## Introduction

One positive consequence of the ongoing economic crisis is that the intellectual malaise of the modern academic discipline of economics is becoming ever more widely recognised. Economics is a discipline that is marked by significant explanatory failure stemming from wildly unrealistic formulations, and has been for many years now (see Lawson 2003, chapter 1).

The reference to specifically academic economics here is not incidental; I want to stress that throughout the discussion my concern is not economics in all its forms or manifestations but economics as it is pursued within the modern academy, which after all is the site from which most strands of the subject emanate.

Elsewhere I have put forward an explanation of the noted academic malaise that draws significantly on ontological theorising, i.e., on theorising the *nature* of (social) reality. Because my focus here is with examining the power of, and support for, an alternative explanation I do not want to rehearse my own position at length. But in brief, the explanation I elsewhere maintain is that the fundamental problem of modern economics is that methods are repeatedly applied in conditions for which they are not appropriate (see Lawson, 1997; 2003). Specifically, modern academic economics is dominated by a mainstream tradition whose defining characteristic is an *insistence* that certain methods of mathematical modelling be more or less always employed in the analysis of economic phenomena, and are so in conditions for which they are not suitable.

Fundamental to my argument is an assessment that the application of mathematics involves more than merely the introduction of a formal language. Of relevance here is recognition that mathematical methods and techniques are essentially tools. And as with any other tools (pencils,

hammers, drills, scissors), so the sorts of mathematical methods which economists wield (functional relations, forms of calculus, etc.) are useful under some sets of conditions and not others.

The specific conditions required for the sorts of mathematical methods that economists continually wield to be generally applicable, I have shown, are a ubiquity of (deterministic or stochastic) closed systems. A closed system is simply one in which an event regularity occurs. Notice that these closures are as much presupposed or required by the 'newer' approaches to mathematical economics, those often referred to as non-linear modelling, complexity modelling, agent-based modelling, model simulations, and so on (including those developed under the head of behavioural or neuro-economics), as they are by the more traditional forms of micro, macro and econometric modelling.

The most obvious scenario in which a prevalence of such closures would be expected is a world 1) populated by sets of atomistic individuals or entities (an atom here being an entity that exercises its own separate, independent, and invariable effect, whatever the context); where 2) the atoms of interest exist in relative isolation (so allowing the effects of the atoms of interest to be deducible/predictable by barring the effects of potentially interfering factors). Not surprisingly the latter two (ontological) presuppositions are easily shown to be implicit in almost all contemporary economic modelling contributions (see Lawson, 2003, chapter 1).

However, explicit, systemic and sustained (ontological) analysis of the nature of social reality reveals the social domain *not* to be everywhere composed of closed systems of sets of isolated atoms. Rather social reality is found to be an open, structured realm of emergent phenomena that, amongst other things, are processual (being constantly reproduced and transformed through the human practices on which they depend), highly internally related (meaning constituted though [and not merely linked by] their relations with each other – e.g., employer/employee or teacher/ student relations), value-laden and meaningful, amongst much else (see Lawson, 2003 chapter 2).

Clearly if social phenomena are highly internally related they do *not* each exist in isolation. And if they are processual in nature, being continually transformed through practice, they are *not* atomistic. So the emphasis on the sorts of mathematical modelling methods that economists employ necessarily entails the construction of economic narratives – including the sorts of axioms and assumptions made and hypotheses entertained – that, at best, are always but highly distorted accounts of the complex phenomena of the real open social system (for lengthy elaborations of all this see e.g., Lawson, 1997; 2003; Edward Fulbrook, 2009). It is thus not at all surprising that mainstream contributions are found continually to be so unrealistic and explanatorily limited.

Employing the term *deductivism* to denote the thesis that closed systems are essential to social scientific explanation (whether the event regularities, correlations, uniformities, laws, etc., are either a *prior* constructions or a *posterior* observations), I conclude that *the fundamental source of the discipline's numerous, widespread and long lived problems and failings is precisely the emphasis placed upon forms of mathematical deductivist reasoning*<sup>1</sup>.

So much for my own assessment. Many heterodox economists clearly demur, and most of these seemingly hold to the view that a superior explanation of the state of modern economics is provided by focusing on the prevalence of a form of political-economic ideology. The real source of all the failings of the mainstream academic project, according to advocates of this view, is that ideology about how the economic system works gets in the way of explanatorily successful or realistic analysis.

Ideology is a term I have rarely employed, as various heterodox critics have pointed out<sup>2</sup>. Here I address the claim that ideology provides a better, or even an alternative, explanation of the modern

<sup>1</sup> This critique of deductivism of course is not a denial that deductive reasoning can be appropriately employed alongside numerous other forms of logic, reasoning and analysis. In what follows I may occasionally refer not to deductivism but just to mathematical modelling or some such. For a discussion of deductivism as a form of scientific explanation see especially Lawson, 1997, chapter 2; 2003, chapter 1.

<sup>2</sup> Brian O'Boyle and Terrence McDonough (2011) for example suggest that a "lack of an ideological-critique is perhaps the most glaring lacunae for the Lawsonian project, in light of its own philosophical project of critiquing mainstream economics". They add that "the Lawsonian project is unable (or unwilling) to entertain the possibility of an ideological function for mainstream economics"

academic economics' malaise to the one I have advanced and have briefly sketched above. In so doing I take the opportunity to explore the role of ideology in the practices and output of the modern economics academy more generally.

## Ideology

The term *ideology* was seemingly coined by Count Destutt de Tracy in the late 18th century to define a "science of ideas". Few interpret the category in this way today, however. Almost universally, the current understanding is that the term ideology refers *not* to the study, but rather to the content, of certain sets of ideas, along with their consequences.

Thereafter, however, interpretations diverge, with very many alternatives to be found in the contemporary literature<sup>3</sup>. Even so, with a little systematising I think most interpretations can be seen either to presuppose, or to reduce to special cases of, two broad systemic conceptions. These are:

- 1) Ideology<sub>1</sub>: a relatively unchallenged set of (possibly distorted or misleading) background ideas that every society or community possesses which forms the basis of, or significantly informs, general opinion or 'common sense', a basis that remains somewhat invisible to most of its members, appearing as 'neutral', resting on preconceptions that are largely unexamined. A consequence is that viewpoints significantly out of line with these background beliefs are intuitively seen as radical, nonsensical or extreme no matter what may be the actual content of their vision.
- 2) Ideology<sub>2</sub>: a set of ideas designed, or anyway intentionally employed, in order to justify, preserve or reinforce some existing state of affairs, where this state of affairs is preferred, perhaps because it facilitates or legitimates various advantages for some dominant or privileged group, and where these ideas mostly work in the manner described by way of intentionally masking or misrepresenting the nature of reality.

Whether or not these two conceptions are sufficiently comprehensive with regard to the literature on ideology in general, as far I can discern they do cover the interpretations found in the economics literature (and are clearly apparent in the contributions of those who emphasise my apparent neglect of the role of ideology in modern economics). They also cover the viewpoints of many that never mention the term ideology. I should note, though, that those who adopt such a position, including those who explicitly emphasise ideology frequently refer to (what I am calling) modern mainstream economics as neoclassical. The latter is again a term I rarely use, though I take it for the purposes of the current paper that both expressions are intended to refer to the same project, even if there is disagreement as to its nature.

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and, worse, that "to follow Lawson in his particular critique of the mainstream is to relinquish the very tools that are needed to uncover 'the anatomy and ideological function of orthodoxy' or indeed its contribution to neoliberal ideology". Other such critics include for example Kanth, 1999, and Guerrien, 2004 (see below).

<sup>3</sup> Terry Eagleton (1991, p.2), for example, lists the following interpretations found in the recent literature: a process of production of meanings, signs and value in social life; a body of ideas characteristic of a particular social group or class; ideas which help to legitimate a dominant political power; false ideas which help to legitimate a dominant political power; systematically distorted communication; that which offers a position for a subject; forms of thought motivated by social interests; identity thinking; socially necessary illusion; the conjuncture of discourse and power; the medium in which conscious social actors make sense of their world; action-oriented sets of beliefs; the confusion of linguistic and phenomenal reality; semiotic closure; the indispensable medium in which individuals live out their relation to a social structure; the process whereby said life is converted to a natural reality.

## The ideology theorists

According to those in effect employing the first conception of ideology, economic theory is found wanting just because, or where, its proponents act upon, but fail seriously to reflect upon, or to challenge, or to treat as other than a given, a set of background intuitions/ideas of *efficiently functioning markets* or some such. It is the latter intuitions/ideas that constitute the content of the ideology in question. Mainstream theories, it is held, are grounded in such intuitions; and it is just because they are inadequate to real world processes but so unquestioningly and almost unthinkingly maintained, that economics goes so wrong.

According to those instead employing the second interpretation, mainstream economists are viewed as the agents (rather than subjects) of ideology, advancing it with the purpose of sustaining the underlying economic system – so that the proposed *explanation* of the form of the mainstream lack of explanatory insight and so forth is just this desire to perpetuate the economic *status quo* using theories that are knowingly inappropriate to it.

These two contentions, though connected, are clearly not identical. In the first case, mainstream economists are viewed as the hapless dupes of the prevailing ideology. In the second case, these same economists knowingly and indeed conspiratorially provide, maintain and promote the ideology. Let me briefly give examples of each interpretation.

### *Ideology*<sub>1</sub>

A version of the former mainstream-as-manifestation-of-background-ideology interpretation is advanced for example by Bernard Guerrien (2004). Criticising my own contributions in particular, Guerrien writes:

“I am only going to consider Lawson’s main criticism of neoclassical economics: its “lack of *realism*”. I think that it is not the appropriate objection: *all* theories lack realism, as they take into consideration only some aspects of reality. Everyone agrees on this, even neoclassical economists. The real problem with neoclassical theory is not its’ “lack of *realism*” but the “ideology” (a word Lawson never uses) that it smuggles in and carries with it”

Having suggested that the real problem is not a lack of realism or (a better term) realisticness, Guerrien proceeds by agreeing that a lack of realisticness nevertheless *is* a problem, albeit, he supposes, mostly one that lies in the mainstream formulation of structure rather than its accounts of human agency. Guerrien understandably finds the formulations that abound to be somewhat absurd; in fact he goes as far as to talk of ‘irrelevant’ and ‘stupid’ models. His main concern, though, is not with the lack of realisticness *per se* but with the extreme nature of it; with why anyone – and specifically intelligent academics – would choose to reproduce and study constructions that can only be regarded as ‘stupid’:

“The question is [...]: how such intelligent people can propose – and endlessly study – such *stupid* models?”

Guerrien answers his own question as follows:

“I only see one reason for that: *ideology* (intuitive beliefs which render them blind)”

This is Guerrien’s only elaboration of his understanding of the term. But it does suggest that Guerrien means something like a certain set of beliefs held by the community that are so intuitive in their appeal that their holders do rarely reflect upon them and are mostly blinded to the possibility that the beliefs in question might be wrong or even open to valid criticism<sup>4</sup>.

<sup>4</sup> Of course, when or where most people in a community think alike about certain matters, and even ‘forget’ that there are alternatives to the current state of affairs, we also arrive at Antonio Gramsci’s concept of *Hegemony*.



What is this set of intuitive beliefs that Guerrien has in mind? Following his brief outline of what he means by ideology Guerrien writes:

“Here, the belief alluded to is that “market mechanisms” [...] produce “efficient” results – if you abstract from “frictions”, “failures”, etc. (ignoring these “imperfections” being, for neoclassical theorists, the principal reason of “lack of realism”). As there is a strong link between competitive equilibrium (that is, with auctioneer, etc.) and efficient states – link given by the two Welfare Theorems – then competitive equilibrium *must be* identified with “perfect market” (as both are supposed to be efficient). In some books (especially those on growth, in the “macro” mood – as those of Romer and Barro and Sala-i-Martin), perfect competition and an “omniscient” “representative agent” (or planner) choice are presented as giving the same results. How can a normal person make any sense of this?”

And Guerrien’s answer to the question posed in the last sentence, in effect, is that we make sense of this by seeing mainstream economists as essentially cultural or economic dupes, unable to rise sufficiently above or beyond the prevailing ideology of their times.

### *Ideology<sub>2</sub>*

A rather more conspiratorial view of mainstream theory, as itself a form of ideology, is perhaps more common. The self-consciously heterodox *Real World Economics Review* (formerly the *Post-Autistics Review*) and its associated *Real World Economics Review Blog* are replete with contributions where the term ideology is used in this way. An illustrative example is provided by Peter Söderbaum (2009), who describes ideology explicitly as theory serving as a ‘means’ to achieve a political end:

“‘Ideology’ stands for a ‘means-ends philosophy’ and is not limited to more or less established political ideologies like socialism, social democracy, social liberalism or neo-liberalism. In this sense, neoclassical economics clearly qualifies as an ideology and as such is more specific and precise than the political ideologies mentioned.

Neoclassical economics tells us about the relevant actors in the economy (consumers, firms and government); about how to understand markets (supply and demand of commodities and of factors of production); about decision-making (optimization) and efficiency (usually a monetary concept or at best cost-efficiency). This way of understanding economics is clearly not neutral but specific in ideological terms” (p. 9)

Elsewhere a colourful statement of the more conspiratorial interpretation of economics as ideology is provided by Rajani Kanth (1999). Kanth, indeed, is quite explicit in the view that (mainstream) “economics is the ruling ideology of the capitalist system” (p. 191). Writing of (mainstream or ‘neoclassical’) economics as “this crown jewel of capitalist ideology” (p. 191); of “the inherent charlatanism of economic ideology” (p. 189), and so forth, Kanth elaborates as follows:

“To state the moral: *the entire enterprise of neo-classical economics is rigged to show that laissez-faire produces optimal outcomes*, but for the disruptive operation of the odd externality (a belated correction) here and there (Kanth, 1999, pp. 191-2, emphasis in the original)”.

How is this rigging achieved? One component of the strategy is everywhere to stipulate that human beings are rational (meaning optimising) atomistic individuals. A second is the construction of theoretical set-ups or models specified to ensure that (typically unique) optimal outcomes are attainable.

There are indeed very many economists who do adopt the individualist framework and assumption that individual behaviour is optimal (in the sense of always deriving from optimising decisions in conditions where optimal outcomes are to be had). Perhaps most do. But this is not yet enough to show that the overall economic system is itself optimal in any way. If the claim is that mainstream economists seek to defend the economic system *per se*, something more is required to guarantee the result that the system is in a sense 'optimal'. This, it is supposed, is achieved by the commonplace construction of an equilibrium framework; the latter being so specified that the actions of isolated optimising individuals somehow (tend to) work to bring an equilibrium position about. Thus Kanth, for example, refers to the "economic science of capitalism" as

"simply *irrelevant* for being a fantasy world of an ideal rational, capitalism where all motions are mutually equilibrating, in a Newtonian co-ordination of the elements" (Kanth, 1999, p. 194)

In sum, two competing interpretations of the nature of ideology and how it connects to the mainstream tradition of modern economics can be found. The first supposes that mainstream economics is the more or less unrecognised product of ideology, the second sees mainstream economics as itself the ideology perhaps intentionally promoting deception.

If the two interpretations are radically different in their orientation, they do have something significant in common. They both characterise the mainstream project as primarily concerned with producing theories that express capitalism as an efficiently functioning, or otherwise desirable or optimal, system. Thus, according to both sets of accounts the lack of realisticness and so forth of mainstream economics arises just through that project's portrayal of the economy as an efficiently functioning or otherwise desirable system, a portrayal that is regarded as being inconsistent with the way the real world really is.

In this manner the two groups ultimately provide somewhat similar explanations of the enduring failings of the modern mainstream, even if their accounts of the intentions of economists and manner in which ideology impacts are quite different.

### **An assessment of the political-economic ideology explanations**

My own view is that neither of these explanations of the state of modern economics is sustainable. Remember the phenomenon before us is the generalised explanatory failure and lack of realisticness of formulations across the totality of the mainstream (macro, micro and econometric) output over a large number of years (again see e.g. Lawson, 2003, chapter 1).

It is one thing to suggest that mainstream economists mostly suppose that capitalism, as a market centred system, is somehow natural or normal or the best that can be achieved; but it is quite another thing to suppose that much of the output of these economists is even mainly concerned with such issues of political economy. It is a further step still to suppose that mainstream economists in their modelling endeavours everywhere either take it as a matter of unquestioning belief, or are motivated to demonstrate, that the social world in which we live is not merely defensible, but characterised by equilibrium or efficient markets or perfect competition, and so forth. In truth, the social system that is capitalism is, *qua* social system, barely ever even considered.

Moreover in those cases where economists do focus on questions of market or competitive equilibrium etc., the formulators of the models in question are often careful to stress that their theorising has little connection with the real world anyway and should not be used to draw conclusions about the latter, whether in terms of efficiency or for policy or whatever. Thus, Frank Hahn, a major contributor in this field, writes:

"[...] it cannot be denied that there is something scandalous in the spectacle of so many people refining the analyses of economic [equilibrium] states which they give no reason to suppose will ever, or have ever, come about. It probably is also dangerous. Equilibrium economics [...] is easily convertible into an apologia for existing economic arrangements and it is frequently so converted" (1970, pp. 88-9).

Elsewhere, Hahn reveals in rather dramatic fashion what he feels should happen if people contemplate using such models for policy:

"When policy conclusions are drawn from such models, it is time to reach for one's gun"  
(Hahn, 1982, p. 29).

In truth in those cases where mainstream assumptions and categories are couched in terms of economic systems as a whole they are mainly designed to achieve consistency at the level of modelling rather than coherence with the world in which we live.

This concern for a notion of consistency in modelling practice is true for example of the recently fashionable rational expectations hypothesis, originally formulated by John Muth (1961), and widely employed by those that do focus on system level outcomes. The hypothesis proposes that predictions attributed to agents (being theorised about) are treated as being essentially the same as (consistent with) those generated by the economic model within which the same agents are theorised<sup>5</sup>. As such the proposal is clearly no more than a technique for (consistency in) modelling, albeit a bizarre one. Significantly any assertion that the expectations held (and so model in which they are imposed) are essentially correct, is a step that is *additional* to assuming rational expectations.

It is a form of modelling consistency (albeit a different one) that underpins the notion of equilibrium itself. In modern mainstream economics the category equilibrium has nothing to do with the features of the real economy (say with the balance of supply and demand -- see e.g., Lawson, 2005; 2006). Economic models often comprise not single, but sets of, equations, each of which is notoriously found to have little relation to what happens in the real world. One question that nevertheless keeps economists occupied with such unrealistic models is whether the equations formulated are mutually consistent in the sense that there 'exists' a vector of values of some variable, say one labelled 'prices', that is consistent with *each and all* the equations. Such a model 'solution' is precisely the meaning of *equilibrium* in this context. As such the notion is not at all a claim about the world but merely a (possible) property that a set of equations may or may not be found to possess. The mainstream economists Huw Dixon gets it right when he summarises matters as follows: "At its most general, we can say that 'equilibrium' is a method of solving economic models. At a superficial level, an equilibrium is simply a solution to a set of equations" (Dixon, 1990, p. 356). In short, when mainstream economists question whether an equilibrium 'exists' they merely enquire as to whether a set of equations has a solution.

More to the point, however, the substantive content of mainstream theorising is far wider and more dynamic than a fixed focus on market mechanisms or on conceptions of competitive equilibrium and claims that the market mechanisms lead to efficiency, and such like.

In fact, most mainstream economists, as I say, have never concerned themselves much with the workings of the economic system as a whole (whether via an equilibrium framework or otherwise). The dominant concern, rather, has been, and remains, with highly specific or partial analyses of some highly restricted sectors or forms of behaviour. Very often the focus is on 'micro' decision making or 'behaviour'. Even (or perhaps especially) here though the contributions have more or less always been unrealistic and have rarely if ever generated (as opposed to imported) insight.

<sup>5</sup> As Muth himself puts it: "expectations, since they are informed predictions of future events, are essentially the same as the predictions of the relevant economic theory [...] The hypothesis can be rephrased a little more precisely as follows: that expectations of firms (or, more generally, the subjective probability distribution of outcomes) tend to be distributed, for the same information set, about the prediction of the theory (or the "objective" probability distributions of outcomes)" (Muth, 1961, p. 316)

To the extent that it has ever been meaningful for the various disparate results or theorems of these economists to be considered as a whole, the clearest conclusion that can be drawn is that they are mostly wildly inconsistent with each other. So long as the assumptions are tractable mainstream theorists are free to posit anything they want no matter how unrealistic. Competing hypotheses abound, even by the same contributors in different contributions.

If we focus on empirical contributions, specifically, it is clear that there are few attempts to repeat the results of others, progress the results of others, or even acknowledge the results of others. Even econometricians using identical, or almost identical, data sets are regularly found to produce quite contrasting conclusions, usually with little attempt at explanation. The systematic result here, as the econometrician Edward Leamer (1983) observes, is that: "hardly anyone takes anyone else's data analysis seriously" (p. 37).

Furthermore, far from being a conspiracy or a uniformly misled project, mainstream economics lacks agreement even as to the project's purpose or direction. As one of its leading practitioners Ariel Rubinstein acknowledged more than a decade ago:

"The issue of interpreting economic theory is [...] the most serious problem now facing economic theorists. The feeling among many of us can be summarized as follows. Economic theory should deal with the real world. It is not a branch of abstract mathematics even though it utilises mathematical tools. Since it is about the real world, people expect the theory to prove useful in achieving practical goals. But economic theory has not delivered the goods. Predictions from economic theory are not nearly as accurate as those by the natural sciences, and the link between economic theory and practical problems [...] is tenuous at best. Economic theory lacks a consensus as to its purpose and interpretation. Again and again, we find ourselves asking the question 'where does it lead?' " (Rubinstein, 1995, p. 12).

In short, the modern mainstream is not a project whose emphases and explanatory failures are mainly direct manifestations either of intentions to maintain attachment to the existing economic system, or of a blindness to its real nature. At the level of substantive theory, there is far more heterogeneity within mainstream theorising than Guerrien, Kanth and others allow, with relatively little attention focussed on the economic system as a whole, let alone given over to the theorising of its optimality. Indeed at the level of substantive theory the project is marked by overall incoherence and lack of cohesion amongst its various strands, and with significant uncertainty even as to what is worth pursuing.

In fact some recent close critical observers of the economic content of mainstream theories not only do not characterise the mainstream as ideological at the substantive level, but actually portray the mainstream project at this level as pluralist (see for example David Colander *et al.*, 2004 or John Davis 2005). In truth I think this a polite way of saying that the projects flits from fad to fashion in the hope of achieving explanatory successes somewhere. But that aside, at least these authors recognise the obvious heterogeneity and change that characterises the project<sup>6</sup>.

The one (and as far as I can see only) feature that has persistently and comprehensively marked all mainstream contributions, and continues to dominate, is the insistence that methods of mathematical modelling always be employed. And, significantly, *this emphasis is by itself sufficient to explain the mainstream deficiencies at all levels including that of economic content*. Specifically, and unlike explanations that seek to make ideology at the level of economic content the central factor, explanations in terms of the misplaced emphasis on mathematical modelling can account for mainstream inadequacies

<sup>6</sup> Thus, in contradistinction to Guerrien and others, Colander *et al.* (2004) call attention to what they see as the "changing face of mainstream economics" and criticise heterodox economists for failing to notice such ongoing developments. Specifically, these authors criticise heterodox contributors for adopting an overly "static view of the profession" (p. 486); for referring to the current mainstream as neoclassical; and for missing the "diversity that exists within the profession, and the many new ideas that are being tried out" (p. 487). In fact, Colander *et al.* insist that "Mainstream economics is a complex system of evolving ideas" (p. 489), and refer to the "multiple dimensionalities that we see in the mainstream profession" (p. 489).

whether couched in terms of explanatory failure, unrealistic formulations, or the project's lack of direction; whether the economic focus is the system as a whole or very partial 'micro' situations; whether the modellers are or are not supporters of the *status quo*; whether or not data are employed; whatever the substance of the latest fad and fashion to preoccupy, and so on. In consequence, I see little reason in all of this to reject yet the assessment that the misplaced emphasis on mathematical deductivist reasoning provides the better explanation of the rather unhappy state of modern economics.

### **An alternative conception of ideology in the economics academy**

I have considered the two conceptions of ideology that are most often associated with modern academic economists and I have argued that, in the political-economy garb in which these ideologies are usually presented by opponents of the mainstream, they do not serve easily to account for the explanatory failings of the discipline.

However in so concluding I must stress that it by no means follows that I suppose either that ideology is entirely absent from the modern economics academy, and specifically mainstream economics, or that questions of ideology have no bearing on the latter project's lack of explanatory power. Indeed I am of the definite view that ideology is rife and with significant consequences for mainstream explanatory performance. There are various issues to consider though.

First and foremost, I want briefly to indicate an alternative ideology, a version of ideology<sub>1</sub> ( a set of background views manifest unquestioningly as if normal or neutral) that I believe does pervade the economics academy, one that is extremely widespread and indeed plays a significant contributory role in the failings of the discipline. But this is a set of beliefs that bears *not* directly upon the nature of the underlying economic system at all. Rather it is *precisely the doctrine that all serious economics must take the form of mathematical modelling*.

This ideology, as I am suggesting it to be, usually involves a presumption of an event-regularity-seeking (and so prediction-oriented) conception of science along with the complementary belief that mathematics is closely aligned with, and indeed essential to, such a science.

I must stress that the contention I am advancing here is not an arbitrary add-on adopted just because the focus of discussion is ideology. If I am right that the problems of modern economics stem first and foremost from the misplaced attachment to mathematical reasoning despite the record of failure so far, there must be a reason for this emphasis. That reason, I want to suggest, is, in large part at least, the unquestioning, uncritical, taken on trust as normal, blinkered orientation to employing mathematical techniques characteristic of most of those who pursue it. It is blindness to, or an unreasoned dismissive resistance to any suggestion of, any alternative to the idea that wielding mathematical techniques is both essential to science and compulsory if economics is to make a proper contribution. Such a stance amounts precisely to a system of beliefs that is itself a form of ideology (ideology<sub>1</sub>).

Let me emphasise that I am quite aware that there are forms of mathematical systems, methods and techniques beyond those currently and/or traditionally prosecuted by economists. Deductivist forms, presupposing closures of the sort defined above, just happen to be those that economists (so far) find easiest to wield or otherwise most convenient. I do not argue that all forms of mathematical method (including those yet to be invented) must necessarily be inappropriate to social analysis – though I strongly doubt that any will ever prove of general use for addressing/illuminating the sort of capitalist social system in which we currently live. But this is not the issue. My point, rather, is that far from even contemplating, let alone exploring, alternative non-mathematical approaches – where various available alternatives are easily shown to be relevant to, and fruitful for, illuminating the real social system (for illustrations see for example Lawson, 2009a; 2003, chapter 4; 1997, chapter 18) – modern mainstream economists continually seek out mathematical techniques of some kind. It is, to repeat, this unwillingness or apparent inability seriously to contemplate the idea that a serious, fruitful and explanatorily successful

(science) of economics might be developed that does not rely upon the application of mathematical methods and techniques of some form that indicates the dominant form of the actually prevalent ideology.

### *Ideology in action*

It can be easily seen that the unquestioned emphasis on mathematical modelling is indeed a form of ideological blindness if a brief examination is made of cases where, for whatever reason, mainstream economists, whether in the form of a few critical contributors, or as a collective body, have come to critically address the explanatory failings of their project. For it is evident that the one feature that is almost never addressed in such endeavour is precisely the emphasis on the employment of mathematical techniques. Even in the very few cases that mainstream contributors recognise or acknowledge that criticisms of the mathematical-modelling emphasis have been made from outside their project, the latter criticisms tend to be summarily dismissed either without serious consideration, or at most with some vague referencing of alternative more appropriate mathematical techniques yet to be developed.

Let me first briefly consider examples of the latter, cases where the issue of the mathematical emphasis itself is at least broached in published mainstream commentaries. Amongst the very few examples I can find are by the 'theorists' Alan Kirman and Frank Hahn, surely amongst the most open-minded and reflexive of mainstream economists. However, even in such cases any notion that the mathematical emphasis might be the problem is quickly dismissed rather than seriously entertained.

Kirman, for example, in critically examining the nature and poor performance of mainstream theorising focuses on the individualist emphasis of the enterprise. But in suggesting alternative ways for the mainstream to proceed it is clear that he regards methods of mathematical modelling *per se* as effectively indispensable. Thus, he writes:

“The argument that the root of the problem [...] [is] that we are confined by a mathematical strait jacket which allows us no escape, does not seem very persuasive. That the mathematical frameworks that we have used made the task of changing or at least modifying our paradigm hard, is undeniable but it is difficult to believe that had a clear well-formulated new approach been suggested then we would not have adopted the appropriate mathematical tools” (Kirman, 1989, p. 137).

I noted above that Frank Hahn does not hesitate to acknowledge that equilibrium theory tells us little about economic reality; and he is especially ill-disposed towards those that would seek to use mainstream models to draw policy conclusions or as a guide to practical action. But the idea that mathematics might be dispensable to economic thinking, or anyway overly emphasised, or commanding too high a proportion of the resources available for economic research, is not afforded serious reflection. To the contrary, he dismisses any suggestion that the modern emphasis on mathematical modelling may be misplaced as “a view surely not worth discussing” (Hahn, 1985, p. 18). And in a major speech to the Royal Economic Society he even counsels that we “avoid discussions of ‘mathematics in economics’ like the plague” (Hahn, 1992a; see also Hahn, 1992b).

If mainstream economists tend to be blind to, or to ignore, or at best to dismiss, external critiques of the mathematical emphasis how do they react in situations where they at least acknowledge explanatory failure? The ongoing economic crisis provides perhaps a unique insight to this. Recently, possibly the largest numbers of mainstream self-critics ever have been driven to reassess their own practices as a result the ongoing economic crisis and widespread suspicion of, and concern for, if not outright condemnation of, academic economic output. Notably even amongst mainstream economists there have been increased calls for change and numerous claims advanced that changes have indeed been, or are being, made. The situation thus affords an unusual opportunity to assess which parts of academic economic

practice are most readily regarded as open to transformation and which treated, consciously or subconsciously, as beyond, or anyway not in need, of critique.

A useful concentration or grouping of this unusually large set of mainstream critical reflections on the nature of modern academic economics is provided by the various contributions to the inaugural (2010) conference (held in Kings College Cambridge) of the *Institute for New Economic Thinking* (INET), the latter being an organisation set up precisely to transform economics in the light of the failings of the economics discipline to provide much understanding of the crisis.

Very many economists attended the conference, all apparently concerned critically to reconsider the nature of academic economics. It is in such a forum if anywhere that we might hope to find mainstream economists challenging all but the most obviously acceptable aspects of their theories, approaches and activities.

Although George Soros, who sponsors the Institute, shows some awareness that the reliance upon mathematics may at least be something to question (see e.g. Soros, 2009; Lawson, 2010), for most of his close associates the idea that there might be something problematic about the emphasis on forms of mathematical technique does not appear even to cross their minds (all the numerous contributions are posted on the INET website or can be found on Youtube<sup>7</sup>).

Consider for example the presentation by Joseph Stiglitz (2010<sup>8</sup>), a central contributor in the Institute. Stiglitz quickly focuses on the failure of most models to predict the crisis, he emphasises the need for predictive accuracy, and then he proposes a strategy of experimenting with different model assumptions, most of which are accepted as unrealistic, in order to come up with a best model in terms of forecast accuracy. Or rather Stiglitz makes reference to a “standard model” and declares that the critical question for “research strategy” is to decide which of the model’s “many unrealistic assumptions” we “want to drop”. Indeed, emphasising the need for pluralism Stiglitz suggests that we “investigate a number of different models where different assumptions are dropped”. Stiglitz stresses that, in his view, dynamic models and stochastic models and general equilibrium models remain important for economics, even if it is necessary to revise some of their specifications. His overall preference is seemingly for analytic models or more complex stochastic models that can at least in principle accommodate human interactions (though other preferences for model specifications are listed).

Of relevance here is that, in all this, Stiglitz is seemingly open to entertaining an array of different modelling assumptions (albeit mostly unrealistic ones<sup>9</sup>), and so is also open to a degree about policy stances, etc., that might be adopted. The one issue that is not even hinted at, however, is that we might also question the very emphasis on mathematical modelling itself. Indeed the discussion throughout his presentation is only and continually about how economists should go about finding ‘better’ mathematical models. And this uncritical stance typifies the presentations made throughout the conference, as recorded on INET’s website.<sup>10</sup>

Of course, mainstream economists like Stiglitz rarely, and perhaps have little opportunity to, explore in a sustained, serious or systematic way the issues of philosophy/methodology on which they sometimes pronounce. But the same reaction to the crisis is found even by methodologists of the mainstream that

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<sup>7</sup> See for example <http://ineteconomics.org/initiatives/conferences/kings-college> or <http://www.youtube.com/watch?v=SdZgD1DCNq4>

<sup>8</sup> See <http://ineteconomics.org/video/conference-kings/agenda-reforming-economic-theory-joseph-stiglitz>

<sup>9</sup> Of course, the willingness to entertain unrealistic assumptions makes the whole project rather pointless (see Lawson, 2009a). If I am allowed to make unrealistic assumptions then after the event I can predict anything you want [if the outcome to be so ‘forecasted’ is X, then I can simply (or complexly) assume 1) Y implies X, and 2) Y]. Before the event then, in an open world, successful prediction, if it occurs (involving timing, - like earthquakes we all know crises can and will occur, and usually why) is mostly a matter of luck.

<sup>10</sup> Coincidentally, as I was writing these lines I received via email a paper from INET written by Harald Uhlig (2011), based on his 2010 INET presentation, and entitled ‘Economics and Reality’ (the same title as my 1997 book). It is a philosophical paper so I thought this might be an exception in at least exploring the relevance and grounding of mainstream mathematical modelling. The author does, in a footnote on page 1, reference my book, and suggest that his piece may, in its philosophical orientation, be “more reminiscent of Lawson (1997)”. But the author quickly adds, notably without any argument or explanation, “though I sharply disagree with his [Lawson’s] rejection of formal, mathematical models to address the social reality of economics [...]”

might be expected to look deeper and further. Thus in an influential paper entitled “The Financial Crisis and the Systemic Failure of Academic Economics”, David Colander, Hans Föllmer, Armin Hass, Michael Goldberg, Katerina Juselius, Alan Kirman, Thomus Lux, and Brigitte Sloth (2008), provide an assessment that again questions anything and everything except the emphasis on formalistic mathematical modelling (see Lawson 2009b; 2009c).

### *Heterodoxy and mathematical modelling*

In fact so apparently compelling is the belief system in question (that mathematical modelling is the proper way to do economics) that many heterodox economists too seemingly fall under its sway. Although heterodox modellers do not follow the mainstream in dogmatically insisting that we all everywhere adopt a mathematical orientation, it remains the case that many heterodox economists fail to recognise that the conceptions they find to be inadequate in mainstream theory owe something to the mainstream modelling emphasis; and these heterodox economists continue excessively (and often exclusively) to explore alternative mathematical models and forms of mathematical reasoning in the face of explanatory failures and unrealistic formulations.

The heterodox *Real-World Economics Review* has even seen fit to create a *Revere Award for Economics* given in large part for success in predicting the Global Financial Collapse, a move that may well further encourage the uncritical reliance on formalistic models and the predictive criterion of success<sup>11</sup>. And whilst the same journal and its associated blog do carry articles and comments that critically focus on the use of mathematics in economics, the dominant emphasis seems to be the exploration of novel, or currently non-standard, forms of mathematical technique<sup>12</sup>.

Of course, I do not wish to (and indeed do not – see e.g. Fullbrook, 2009, especially chapter 12) discourage all experimentation with mathematical models and methods in all contexts, and certainly cannot insist that the use of methods of mathematical- modelling must always be devoid of insight, even if the arguments I have laid out lead me to believe that the current emphasis is largely wasteful of resources (again see Lawson, 1997; 2003). My point rather is that the reliance on mathematical modelling and on criteria like predictive accuracy, goes almost unquestioned throughout much of the economics academy, certainly within the mainstream and sometimes even beyond. And this is so despite the unrealistic concoctions that so far have almost always resulted from this endeavour along with the severe limitation of

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<sup>11</sup> Of course, I accept (indeed I continually argue) that (using the criteria of *explanatory power*) we can come to understand the workings of causal processes and mechanisms (see Lawson, 2009a; or 2003 chapter 4). And where we understand such mechanisms we can conditionally predict (contingent) *tendencies*, meaning the impacts of causal forces acting in certain ways or directions *whatever the actual outcomes*. Thus I predict that should planet earth survive without catastrophe into tomorrow then over large parts of the planet there will be gravitational tendencies in play working to ‘pull’ leaves to the ground, *tendencies that will be operative even as leaves fly over roof tops and chimneys*. But, excepting a lucky accident, only in a closure can we predict any actual outcome (in the case of the path of a specific leaf we must absent aerodynamic and thermodynamic tendencies, the wind, and so forth).

I acknowledge, too, that because some tendencies are so powerful (relative to countervailing forces) we can even predict certain sorts of outcomes that will eventually occur. Thus given our understanding of the mechanisms behind earthquakes we can reasonably expect that the latter will continue to occur now and again on planet earth. And it is not difficult to understand that capitalism, being an inherently contradictory system, will repeatedly manifest crises. The specific forms and timing though are something else (in such cases the inability to predict outcomes matters little anyway, unless our one goal is to get rich through forms of gambling. Rather what we *need* to know is how best to locate and construct buildings that can withstand earthquakes; and how to best support social systems that are free of the sorts of contradictions that generate instabilities and crises; and so on. But these matters are not my focus here).

Of course, as with all forms of gambling there is often some forecaster that picks a winner. As I write these lines (end of 2011) there are groups of economists predicting that the euro will collapse, though there is a range of predicted timings of this event; and there are others who expect the euro to survive. There are also anticipations of intermediate paths and outcomes. Each group or commentator is knowledgeably analysing the nature of operative causal mechanisms. But the actual outcomes depend on so many contingent developments including factors yet to be determined. Of course, whatever happens, someone forecasting today will be found after the event to have been closest in their forecasting. But unless they possess the power to effect the result, such ‘success’ will inevitably be far more luck than judgement.

<sup>12</sup> Though it seems to be something of an easy retort it is surely not wholly without significance, or totally unfair of me, to note that whilst the current paper was posted on the *Economic Thought* website the majority of comments it received advance the idea that the solutions to the problems I raise lie in continuing the search for more appropriate techniques of mathematical modelling.



explanatory insight generated by (as opposed to being tagged on to) such activities. And it is so despite the ongoing crises leading economists of all persuasions to critically reflect upon the now-all-too-apparent failings of the academic discipline. All such features I suggest are strongly supportive of the contention that the belief/conviction that ‘the only proper economics is a mathematical economics’ is a prevailing form of ideology.

### Explaining the ideology of mathematical technique in modern economics

If I am right about all this then the ideology I am identifying itself requires an explanation. This is easy enough to provide. But notice first that a feature (I believe it to be a strength) of the explanation of the state of modern economics that I am advancing is that I do not need to invoke a conspiratorial view of ideology (ideology<sub>2</sub>); and specifically I do not at all view the mainstream as a body of dishonest individuals setting out to pull the wool over the eyes of anyone.

Nor of course do Guerrien and others who also emphasise the first interpretation of ideology (ideology<sub>1</sub>), but who suppose that mainstream economists are ‘blind’ to the unrealisticness of their presumption that the political economic system is an efficiently functioning one. But unlike these contributors I also do not need to suppose that mainstream economists or anyone else continue to be blind to the increasingly all-too-apparent crisis-ridden state of capitalist economies.

Still I need to give some reason why mainstream economists may indeed be blind to the possibility that their methods of mathematical modelling are inappropriate to social analysis.

A large part of the explanation, I suggest, is simply that mathematics has been so successful in the history of human endeavour, and especially within (non-social) natural science, that its centrality to all science and serious and systematic investigation is, throughout wide sections of society, taken as an article of faith. Certainly a perception that, especially where measurable quantities are involved, all serious research requires a mathematical form has been widely in evidence since the Enlightenment. And economics is, more than any other social discipline, commonly interpreted as being concerned with measurable quantities (quantity of money, prices, output<sup>13</sup>). As such it is not surprising to find many economists both supposing that forms of mathematics are essential to economic science, and optimistic that economics can equally achieve significant mathematical successes eventually.

Thus, whilst Guerrien posits a blindness of mainstream ‘intelligent’ contributors even when the content of their models is regarded by him as so obviously ‘stupid’, I only posit a blindness in a situation where it may be understandably difficult for many to see clearly. With the neglect of ontological reasoning, recognition that the current emphasis on formal mathematical modelling in economics is misguided is not so obvious. This is especially the case in the light of the just noted and often emphasised post Enlightenment historical record of mathematical success throughout the various disciplines.

In fact, an acceptance of the idea that mathematics is essential to grounded knowledge has been a factor in sections of popular culture for rather longer even than the post-Enlightenment period. In the interests of brevity let me recall how Morris Kline sums up the introduction to his majestic *History of Mathematics in Western Culture*:

"In this book we shall survey mathematics primarily to show how its ideas have helped to mould twentieth-century life and thought. The ideas will be in historical order so that our material will range from the beginnings in Babylonia and Egypt to the modern theory of relativity. Some people may question the pertinence of material belonging to earlier historical periods. Modern culture, however, is the accumulation and synthesis of

<sup>13</sup> As early as 1871 we find Jevons writing “My theory of Economics, however, is purely mathematical in character [...] To me it seems that our science must be mathematical simply because it deals with quantities” (p. 3)

contributions made by many preceding civilisations. The Greeks, who first appreciated the power of mathematical reasoning, graciously allowing the gods to use it in designing the universe, and then urging man to uncover the pattern of this design, not only gave mathematics a major place in their civilisation but initiated patterns of thought that are basic in our own. As succeeding civilisations passed on their gifts to modern times, they handed on new and increasingly more significant roles for mathematics. Many of these functions and influences of mathematics are now deeply imbedded in our culture" (Kline, 1964).

The influence of mathematics is now so deeply ingrained within our culture, indeed, that many people (especially non-professional scientists) appear to suppose that anything stated in mathematics must be correct, whilst for things to be correct, reliable, insightful or scientific (or at least conferring of scientific status), they must be stated in mathematics<sup>14</sup>. For so many people it seems to be simply an unquestioned and seemingly unquestionable matter of faith that if a field of study is to be scientific or accorded status as a knowledge-producing activity, or otherwise regarded as serious, it must take a mathematical form.

Of course, the Enlightenment did give an important boost to this long-in-evidence perception. And in fact ever since Newton succeeded in uniting heaven and earth in equations, and Kant announced that the study of social phenomena requires its own Newton, the programme of mathematising economics has been underway (see Lawson, 2003, chapter 10).

Interestingly, John Henry (1997) recently observes that following the publication of Newton's *Principia*, readers "took for granted the validity of mathematics for understanding the working of the world". And he stresses that "although his book met with some fierce criticism, not a murmur was raised against it in [...] regard [to its emphasis on mathematical reasoning]" (p.21)

Actually, from the point of view of understanding the acceptance of the emphasis on mathematisation as ideology, even more interesting is a critical response to Henry provided by Yves Gingras (2001) in his wide-ranging commentary on the history of mathematics. Addressing Henry's assessment that 'murmurs' of dissent against the mathematical formalism were absent from *Principia*, Gingras writes:

"As we will see, this was far from being the case but to recover these murmurs, one must look at actors who are now unknown precisely because they rejected the mathematization of physics and were thus excluded from the field (and its history) as it evolved in the eighteenth and nineteenth centuries" (Gingras, p. 385)

The point, then, is that a belief that mathematics is central to all science and serious study is a widespread cultural norm of long standing, one that emerged in the face of, and has been continually reinforced through, successes with mathematics throughout the disciplines. If the successes of mathematics has grounded a society-wide cultural belief in the general relevance of, and indeed need for, mathematics for scientific and all serious study, it is not surprising that economists enamoured of the idea of pursuing a serious and scientific economics have too fallen under its sway.

So my contention, in sum, is that the modelling emphasis of modern mainstream economics is explained by a largely unquestioning society-wide conviction (a form of ideology<sub>1</sub>) that mathematics is fundamental to all science, a conviction or ideology that itself is in large part explained by the successes of mathematics in so many other domains.

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<sup>14</sup> Of course this view means turning a blind eye to (or forgetting) the clear scientific successes of the largely non-mathematical disciplines of chemistry and biology in the nineteenth century and indeed much of modern chemistry and bio-medical research (on all this and much that relates to the current paper see Donald Gillies, 2004).

## The modern mainstream and the economic system

I have argued that the malaise of modern economics is not primarily due to ideology at the level of substantive political economy. I have also argued that ideology is present in the economics academy nonetheless, albeit at the level of methodology. Finally, I want to stress that in maintaining these views I do not at all suppose there is no relation between the mainstream stance (that is the insistence that mathematical techniques be everywhere employed) and the underlying economic system. Specifically, and despite the foregoing discussion, I do not at all deny that the mainstream practices can serve the purposes of sustaining the workings of the economic system. But let me be clear on how I think the two do relate.

First, I do not doubt that political-economic and cultural ideological factors in the sense of prevailing background beliefs (ideology<sub>1</sub>) are in play in all societies or communities, and are manifest in the contributions and practices of us all. We are all situated and products of our time, place and culture etc. I do not doubt that such background ideology of this sort bears on the sorts of questions we ask, the orientations we adopt, the assumptions, including absences, we take for granted, the states of affairs we regard as 'normal', and so on. However, I do not think the very real ideological beliefs that prevail regarding the benefits or 'normality' of the existing political-economy system are primarily or even significantly responsible for the perpetual failings and acknowledged fictions of contemporary academic economics. This is just to repeat the foregoing. Most economists as academic researchers do not even seem interested in the economy as such anyway. Far more important is their prowess in manipulating mathematical models and such like. The dominant ideology in the economics academy, I am maintaining, is precisely the extraordinarily widespread and long-lasting belief that mathematical modelling is somehow neutral at the level of content or form, but an essential method for science, underpinning any proper or serious economics.

Second, and following on from the above, the scandal, of modern economics is *not* that it gets so many things *wrong*, but that it is so largely *irrelevant*. However in being irrelevant, and yet using significant resources that could have been used for research into the way the economy really works, then, at a time that the economy is in crisis and proving largely dysfunctional, the mainstream modelling orientation cannot but serve to deflect criticism from the nature of the *status quo* at the level of the economy and thereby work to sustain it (and would do so whatever that status quo happened to be). As Leamer, (already noted above) observes, no one takes anyone else's data analysis seriously. In truth, few people take any mainstream analyses seriously, except in economics faculties' promotion exercises.

Third, if however anyone *were* to pay much attention to mainstream analysis, it would serve to sustain, if not reinforce, the *status quo* in an additional way. The point here is that the emphasis on event regularities (necessitated by a reliance on forms of mathematical modelling), and so attachment to an implicit ontology of closure and atomism, entails that any references to social relationality, and so to (relational) issues of power, discrimination, domination, oppression, and conflict generally, are effectively masked over or hidden, or at best trivialised. For the framework is ill-equipped even to allow such categories to be seriously considered. Thus the very emphasis on mathematical modelling renders analyses of real conflict, power relations and social transformation effectively if inadvertently precluded.

### *The persistence of mathematical reasoning*

I am accepting, then, that although the generalised malaise of modern economics does not reflect any ideological attachment to specific economic theories, the persistent irrelevance of academic mainstream economics, resulting from the emphasis on formal modelling, is nonetheless inhibiting of analysis capable of constituting meaningful constructive criticism of existing political-economic states of affairs. Moreover this consequence, which, I stress, I take to be largely unintended, does, I believe, contribute to explaining how the mathematical project persists in maintaining institutional power in the face of, and despite, its repeated

failures and fictitious constructions. For government sponsored funding bodies and the like are actually less likely to withdraw funds from a project that provides no serious criticism of the government's actions.

Parenthetically, I believe this inability of the mathematical modelling project to challenge anything seriously or convincingly is a major factor in understanding how the project has survived as long as it has in the economics academy, and even how it originally came to dominate. Elsewhere, I address at length the reasons for the rise and persistence of a mainstream tradition that so clearly lacks a history of explanatory successes<sup>15</sup> (see especially chapter 10 of Lawson, 2003).

I stress once more, however, that in all this I am not suggesting that those who contribute to the mathematising project in economics do so, in the main, opportunistically. Rather I merely point out that whatever its advocates' intentions, the mainstream project (with its emphasis on mathematical deductivist modelling, and lack of criticality) may appear, and at times has indeed proven to be (see Lawson, 2003, chapter 10), conducive to those, especially outside the academy, seeking, for whatever reason, to deflective or minimise intellectual challenges to the underlying economic system. As such, this mathematical project, itself underpinned by methodological ideology, and not formulated by those pushing, or duped by, a political-economy ideology, might nevertheless be said to contribute ultimately to sustaining the *status quo*.

## Conclusion

My contention, in short, is that contemporary *academic* mainstream economics is indeed often underpinned by ideology. But this ideology is first of all methodological in nature, being in effect the widespread cultural view that mathematics is essential to science. Incidentally I argue elsewhere not only that this ideology covers a false view in that successful natural science does *not* actually rest on the application of mathematics, but also that a nonmathematical economics can actually yet be a science in the sense of the successful natural sciences (see Lawson, 1997; 2003 and especially 2012).

I have suggested too that a possible, and indeed likely, reason the mathematical emphasis of the mainstream project does not come under more critical scrutiny from outside the academy is that the project's continuing irrelevance actually renders it harmless to political defenders of any prevailing *status quo* who might otherwise be drawn to more critical considerations in connection with funding and the like.

Although I have attempted here to explain the continuing misplaced emphasis on techniques of mathematical modelling in the economics academy I might add that I do not suggest that there are not yet factors at play additional to those I have identified. Amongst other things, the longish recent history of

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<sup>15</sup> In the context of the last 200 years of economic thinking, the fortunes of the project of seeking to mathematise economics have notably waxed and waned in line with changes in the relevant background academic environment, not least in responses to developments in forms of mathematics itself (again see Lawson, 2003, chapter 10).

But certainly no less important were changes in the political environment both inside and outside the academy. One significant feature of the latter was the influence of the political environment in the US following the Second World War. The situation in the US in this period is especially important to understanding the subsequent path of modern economics, just because the resources of the US in the early post-war period allowed it to dominate much of the post world war II international academic scene (in economics as in many other disciplines).

A very significant feature of the early Post World War II political environment was the emergence in the US of McCarthyite witch-hunts in the face of the Cold War. In this climate, the nature of the output of economics faculties became a particularly sensitive matter. And in such a context, the project of seeking to mathematise economics proved to be especially appealing. For although it carried scientific pretensions it was significantly devoid of any necessary empirical content (especially when carried out in the spirit of the Bourbaki approach – see Lawson, 2003).

The group most feared or resented by the McCarthyites were the intellectuals (see e.g., Erik Reinert, 2000). The formalising project with an emphasis on mathematical structure to the exclusion of almost any critical or reflexive content was clearly extremely attractive to those caught up in the situation. This was especially the case not just for insecure or fearful university administrators but also for the funding agencies of US social scientific research (who were especially important in this period - see for example, Bob Coats, 1992; Crawford Goodwin, 1998; Yuval Yonay, 1998).

In fact, historians of the US have long argued that McCarthyism and the Cold War were decisive in the growth of anti-intellectualism in the US in the twentieth Century. (see e.g., Richard Hofstadter's [1963] *Anti-Intellectualism in American Life*; or Alan Bloom's [1987] *The Closing of the American Mind*). My point here is simply that this environment impacted on the economics faculties as elsewhere, and was doubtless conducive to the spread of economics as merely a form of technicist manipulation, with little attachment paid to, or with little consistency in, economic content (see Reinert, 2000 for a similar conclusion<sup>15</sup>). On all this see Lawson, 2003, chapter 10.

mainstream perseverance with the deductivist modelling techniques in the face of repeated failure has suggested to some that there may also be something pathological to what is going on, and that a psychological explanation is likely also of some relevance. Also, the pattern of behaviour in question seems to be gendered, with the mathematical modelling and prediction activities being relentlessly pursued largely by gendered males. This too seems to warrant explanation (again see Lawson, 2003, chapter 10). These are issues that are currently receiving attention elsewhere and in illuminating ways<sup>16</sup>. But that, as they say, is another story.

For now it does seem safe to conclude that the primary explanation of the numerous, long lived and continuing failings of modern academic economics is the (misplaced) emphasis on mathematical modelling. It is an emphasis underpinned by the cultural belief that a reliance on mathematical technique in science is somehow so normal or neutral or natural that any questioning of this emphasis can be ignored or swiftly dismissed as obviously far too radical if not nonsensical.

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<sup>16</sup> See for example Vinca Bigo, 2008.

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# Economics and Research Assessment Systems

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## Abstract

This paper seeks to analyse the effects on Economics of Research Assessment Systems, such as the Research Assessment Exercise (or RAE) which was carried out in the UK between 1986 and 2008. The paper begins by pointing out that, in the 2008 RAE, economics turned out to be the research area which was accorded the highest valuation of any subject in the UK, even though economists were then under attack for failing to predict the global financial crash which had occurred a few months earlier. One aim of the paper is to explain this economics anomaly in research assessment. The paper goes on to point out a key difference between economics and the natural sciences. Most areas of the natural sciences are dominated for most of the time by a single, generally accepted, paradigm, whereas there are always in economics different schools of thought which have different and highly conflicting paradigms. Given this situation, it is argued that the effect of research assessment systems in economics is to strengthen the majority school in the subject (whatever that is), and weaken the minority schools. This conclusion is supported by empirical data collected by Frederic Lee for the UK. It is then shown that the greater the dominance of the majority school, the higher the overall valuation of the subject is likely to be, and this is used to explain the anomaly noted earlier. It is argued that research in economics flourishes better in a situation in which there are a number of different schools treated equally, than in one in which a single school dominates. The conclusion is that research assessment systems have a negative effect on research in economics and give misleading results. Instead of such systems, an attempt should be made to encourage pluralism in the subject.

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## 1. Introduction. Research assessment systems in general

Research Assessment Systems began with the Research Assessment Exercise (or RAE) which was introduced into the UK by Thatcher in 1986 and continued by Blair. For many years, the RAE was the only Research Assessment System, but recently the situation has become more complicated. After the RAE had been in existence for more than 20 years, the UK government decided that it was unsatisfactory and should be replaced by a new type of Research Assessment System to be called the Research Excellence Framework (or REF). The UK government accordingly announced that the Research Assessment Exercise to be held in 2008 (RAE 2008) would be the last RAE. They also gave an outline of how the new REF would be conducted. The initial plans for the REF, however, gave rise to much debate, and have been altered several times. They may not even be fixed now. Meantime further complications have arisen, since some other countries, notably Italy, have decided to introduce Research Assessment Systems. These will be to some extent modelled on the UK RAE and REF, but are likely to differ in detail from both the RAE and REF. We are thus in a complex situation which involves several Research Assessment Systems which differ from each other in detail.

Despite these complications, I think it is possible to give a general definition of a *Research Assessment System (or RAS)* which captures the essential features in common to the various existing systems, while abstracting from the details. The definition runs as follows:

A Research Assessment System (or RAS) is a system in which groups of researchers are assessed at intervals. If the assessment is good, the group retains its funding or gets more, while, if the assessment is bad, the group's funds are reduced or perhaps removed altogether.

Obviously the central problem for any RAS is how the assessment of the value of the work of the group of researchers should be carried out. Two basic methods have been suggested. The first of these is *peer review*. This means that the value of a researcher's work is judged by a set of researchers working in the same field – the 'peers' of the given researcher. The original RAE was based entirely on peer review. The second method of assessment has arisen out of the development of computer technology which has created vast data-bases containing information about research being carried out. This suggests that instead of getting a panel of humans to judge the value of the research output of a group, one might be able to do it by extracting information from these data-bases, and introducing various measurement formulas. Such formulas are known as *bibliometrics*, or *metrics* for short. A simple example of a metric is a citation index. It works like this. The value of a paper is assessed by counting the number of times that paper is referred to by other papers in the field. Naturally a large number of different, and often much more complicated, metrics have been proposed for research assessment.<sup>1</sup>

The UK's original Research Assessment Exercise turned out to be very expensive to conduct. Papers and books had to be collected from departments all over the country, and then read and assessed by panels of experts. This involved the employment of many extra administrative staff, while the time of academic staff was diverted away from the productive activities of teaching and research to the non-productive activities of preparing for the RAE, or carrying out the research assessment. The UK government saw the metrics approach as a way to avoid all this expense. The original idea of the REF, which was designed to replace the RAE, was that it should be based entirely on metrics. Thus the assessment could be carried out automatically by computers and would involve little cost. Unfortunately for the UK government, it turned out that an assessment based entirely on metrics is not feasible, and the present plans for the REF involve a curious mixture of peer review and metrics. The Italian Research Assessment System will differ from both the RAE and REF, and will, no doubt, have special Italian features.

I have already developed a series of criticisms of Research Assessment Systems in 3 papers (Gillies, 2006; 2007; 2009), and a book (Gillies, 2008) whose appearance was designed to coincide with the publication of the results of RAE 2008. Two of these works (2008 and 2009) sketch an alternative approach to research organisation which does not involve the use of Research Assessment Systems. The main thesis of these works is that Research Assessment Systems have the effect of lowering the quality of the research produced rather than increasing it. To illustrate this thesis, I give a variety of examples of research from different fields. There are examples from mathematics, the natural sciences, and medicine, and I also include some examples from research in the humanities – particularly philosophy. However, I do not discuss in detail any examples from economics. In this paper I hope to redress the balance by focussing on the effect of Research Assessment Systems on Economics. The thesis will be that Research Assessment Systems have an

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<sup>1</sup> For an excellent recent survey and critical account of these, see Baccini, 2010.

even more negative effect on research in Economics than they do on research in the Natural Sciences. This is owing to the special features which distinguish Economics from the Natural Sciences, and which are described in Section 3 of the paper. These are that Economics is divided into a number of different schools and these schools are associated with political ideologies. I will try to show in section 4 that the negative effects of Research Assessment Systems on Economics do not depend on the precise details of the Research Assessment System employed. They occur whether the assessment is carried out using peer review or using metrics or using some combination of the two. My claims then apply to Research Assessment Systems in general, but still it will be useful to begin with a specific example. In the next section, I will give the results for Economics of the UK Research Assessment Exercise carried out in 2008 (RAE 2008). This will bring to light a curious anomaly in the RAE results for Economics, which I will refer to as the Economics Anomaly in Research Assessment (or EARA). Later in Section 4 I will try to explain how this anomaly came about.

## 2. The results of RAE 2008 for economics, given with some historical context

The results of RAE 2008 were published in the *Times Higher Education (THE)* on 18 December 2008.<sup>2</sup> Before I give the results for Economics, however, it will be useful to provide some historical context by describing some relevant events which occurred in the preceding three months of September, October and November.

On 15 September 2008, 3 months and 3 days before the results of RAE 2008 were published, Lehman Brothers filed for bankruptcy. This was the largest bankruptcy in the history of the USA with Lehman holding over \$600 billion in assets. This major event soon led to a global financial crash, the biggest since the Wall Street crash of 1929, and this in turn brought about a recession in most countries. Of course the consequences of these dramatic events are not yet played out, and it looks as if, in the West at least, the next decade is going to be a period of economic difficulty similar to the 1930s.

On 5 November 2008, the Queen and the Duke of Edinburgh visited the London School of Economics (LSE) to open the New Academic Building for economics. At the opening ceremony, the Queen asked what has come to be known as 'The Queen's Question'. The general sense of what the Queen said is clear enough, but different sources report her words somewhat differently. According to the Daily Telegraph, 2008, after hearing about the Global Financial Crash, she asked: "Why did nobody notice it?", and went on to describe the turbulence of the markets as "awful". According to LSE Website, 2009, what she said was: "If these things were so large, how come everybody missed them?" Now strictly speaking the presuppositions of the Queen's Question, in either version, are not correct. As we shall see later, some people did notice that a Global Financial Crisis was in the offing. However, the Queen was certainly right that the majority of economists had failed to foresee the coming financial crisis. If it is not *lèse-majesté*, I propose to modify the Queen's Question to the following: "Why did the majority of economists fail to foresee the Global Financial Crash of 2008?" For the rest of the paper, I will consider the Queen's question in this form, and I will also try to answer it later on. The presuppositions of this modified form of the question are correct. Indeed many leading

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<sup>2</sup> An article by Zoë Corbyn about my book criticizing the RAE was also published in the same issue of the *THE*. See Corbyn, 2008.

economists had denied that there was any risk of a serious financial crisis developing. I will here give two examples by way of illustration.

The first is Robert E. Lucas, who is a leading member of the Chicago School of Economists. He won the Bank of Sweden Prize for Economics<sup>3</sup> in 1995, and, according to Wikipedia, “is consistently indexed among the top 10 economists in the Research Papers in Economics rankings.” He is famous for criticizing Keynesian economics, and developing the alternative ‘Rational Expectations’ approach. On 19 September 2007, he wrote a letter to the Wall Street Journal, in which he made the following statement (see Lucas, 2007):

“ ... I am skeptical about the argument that the subprime mortgage problem will contaminate the whole mortgage market, that housing construction will come to a halt, and that the economy will slip into a recession. Every step in this chain is questionable and none has been quantified. If we have learned anything from the past 20 years it is that there is a lot of stability built into the real economy.”

There can be little doubt that Robert E. Lucas failed to foresee the financial crash of 2008.

Robert E. Lucas is in the heart of the American economics establishment, and for my second example I have chosen a figure who has a similar position in the British economics establishment. This is Richard Portes, Professor of Economics at the London Business School since 1995. He has been (1992-2008) the longest serving Secretary-General of the Royal Economic Society since John Maynard Keynes, and was decorated as Commander of the British Empire (CBE) in the Queen’s New Year Honours List 2003. In 2006, there was some turmoil in financial markets, and, as a result, doubts began to arise about the soundness of the Icelandic banks. They were given lower ratings than some of their Nordic peers, and a higher risk premium was placed on their debt. Richard Portes was asked to investigate the situation of the Icelandic financial system, which he did with an Icelandic economist Fridrik Mar Baldursson. Their report entitled: ‘The Internationalisation of Iceland’s Financial Sector’ was published in November 2007. Section 4.1 of the concluding chapter of this report is entitled: ‘The banks: successful and resilient’. The authors write (2007, 63):

“The internationalisation of the Icelandic financial sector proceeded from market liberalisation, ..., and privatisation, on the base of ... an exceptionally healthy institutional framework. The banks have been highly entrepreneurial without taking unsupportable risks; good supervision and regulation have contributed to that, ... Icelandic banks come out well in a comparison with Nordic peers – and their overall and core profitability is higher. ... They are well hedged against volatility in the krona. Stress tests by the FSA indicate that the banks can withstand quite extreme movements in market variables specific to Iceland. The banks have negligible exposure to the US subprime market, structured finance products, and related financial

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<sup>3</sup> The full title of this prize is ‘The Bank of Sweden Prize in Economic Sciences in Memory of Alfred Nobel’. This is usually abbreviated to ‘The Nobel Prize for Economics’, but this abbreviation has been recently questioned by the Nobel family (see the statement on their behalf made by Peter Nobel on 11 October 2010). Peter Nobel points out that, in his original will, Alfred Nobel did not specify that there should be a prize in economics, and that the economics prize was funded at a much later date by the Bank of Sweden. He goes on to say (Nobel, 2010): “What has happened is an unparalleled example of successful trademark infringement.”, and adds: “I can imagine Alfred Nobel’s sarcastic comments if he were able to hear about these prize winners. Above all else, he wanted his prizes to go to those who have been most beneficial to humankind, all of humankind!” The objections of the Nobel family seem fair to me, and I will therefore use the abbreviation: “The Bank of Sweden Prize for Economics”.

vehicles that have hit many financial institutions hard recently. Most fundamental, the banks exploit strong competitive advantage, arising from their entrepreneurial management, flat management structures, and unusual business models.”

But what about the doubts regarding the solvency of the Icelandic banks expressed by some rating agencies? Portes and Baldursson state very clearly that these doubts are unjustified (2007, 63):

“Yet in spite of their strong performance, Icelandic Banks have lower ratings than their Nordic peers, and a much higher risk premium is being placed on their debt during the present turmoil. *We see no justification for this in their risk exposure.*” (My italics – D.G.)

Portes and Baldursson conclude their ‘executive summary’ as follows (2007, 4):

“Overall, the internationalisation of the Icelandic financial sector is a remarkable success story that the markets should better acknowledge.”

Less than a year after the publication of the report of Portes and Baldursson, all the major Icelandic banks collapsed. They were among the first casualties of the Global Financial Crash initiated by the bankruptcy of Lehman Brothers on 15 September 2008. Glitnir, Landisbanki and Kaupthing were placed into receivership in late September and early October 2008. Before the crash, the Icelandic banks held external debt of about €40 billion, which can be compared to Iceland’s 2007 GDP of about €8.5 billion. In particular about £2.3 billion was owed to British individuals and institutions who had placed their money in Icelandic banks. This figure includes about £1 billion deposited by British local councils, and about £120 million by charities.

It is clear enough that Richard Portes did not foresee what was about to happen. The dramatic events of the Global Financial Crisis and the Queen’s Question form the historical backdrop to the publication of the results of RAE 2008 on 18 December 2008. Let us next examine how economics fared in this Research Assessment Exercise.

The principal aim of the RAE was, for each subject, to grade the various departments in that subject throughout the UK. However, by combining the results in a different way, it was also possible to grade the various research subjects. The scale used ran from 4 (the highest possible grade) to 0. In this way an order of the various subjects such as Physics, Chemistry, Philosophy, History, and so on was produced in which the subject in which UK research was better, according to the valuations of the RAE, appeared higher. Where did economics appear in this order? The answer is given by the THE as follows (RAE 2008: The Results, 27):

“Of all the subjects assessed, it is ‘economics and econometrics’ that shines as the UK’s top scorer, nationally averaging more than three (on a scale of 0 to 4).”

In fact ‘economics and econometrics’ (which I will refer to henceforth simply as ‘economics’) scored 3.01. It was the only subject in RAE 2008 which scored above 3. For comparison, the next two subjects in the research ranking were Cancer Studies which scored 2.88, and Chemical Engineering which scored 2.86. These are averages over all the departments in the country which research into the subject in question. However, it is also interesting to examine the rating of the top institution in each of the subjects. For economics, the top institution was LSE, which scored an amazing 3.55. For Cancer Studies, it was Manchester which scored a much lower 3.20, while for Chemical Engineering the best institution was Cambridge which scored 3.15.

This result is very surprising indeed. On 5 November, the Queen must have embarrassed the economists at LSE by asking her Question, which presupposed that all of them had failed to foresee

the biggest Global Financial Crisis since 1929. The Queen's Question clearly implied that something had gone seriously wrong with research in economics. Yet in the results of the Research Assessment Exercise, published the next month, research in economics was declared to be, by some margin, the best research in any subject in the UK. Moreover research in economics at LSE was given a grade of 3.55 out of 4. Who was right in their judgement? The Queen or the RAE committee? It seems clear to me that the Queen was correct, and the RAE gradings were very seriously mistaken.

Now although I was very sceptical about the value of the RAE gradings, and had even published a book against the RAE, I still found the extent of the error in the case of economics very surprising. Here was a subject in which recent events had shown that current research was very much mistaken, and yet the RAE claimed that research in this area was not only good, but the very best research in the whole of the UK. How could such an erroneous assessment have occurred? After all, the RAE committee was composed of experts in the field, and they no doubt performed their allotted task carefully and conscientiously. Yet an outsider, such as the Queen, who presumably does not know a great deal about economics, could tell them in a few minutes that their judgment was completely wrong. I will describe this situation as the *Economics Anomaly in Research Assessment* (or EARA). Anyone interested in Research Assessment should, in my view, try to explain why this Economics Anomaly (or EARA) came about.

This section has provided us with two related questions which are both in need of answers. The first of these is the Queen's Question. The second is the question of how the Economics Anomaly in Research Assessment (or EARA) came about. In order to answer these questions, however, it will be necessary to examine a little more closely the nature of economics as a subject, and, in particular, to show how it differs from a natural science such as physics. These matters will occupy us in the next section.

## **2. How economics differs from the natural sciences. The existence of competing schools of thought within economics**

Kuhn in his 1962 *The Structure of Scientific Revolutions* presents a view of the natural sciences which has become very well-known and quite widely accepted. According to Kuhn, mature natural sciences develop for the most part in a manner which he describes as 'normal science'. During a period of normal science, all the scientists working in the field accept the same framework of assumptions which Kuhn calls a 'paradigm'. However, these periods of normal science are, from time to time, interrupted by scientific revolutions in which the dominant paradigm of the field is overthrown and replaced by a new paradigm. This model is proposed by Kuhn for the natural sciences, and indeed Kuhn, who was originally trained as a physicist, worked almost exclusively on the history and philosophy of the natural sciences. Still, in the Preface of his 1962, he makes some remarks about the social sciences, which, despite their brevity, contain, in my view, a very important insight.

Kuhn writes as follows (1962, ix-x):

"The final stage in the development of this monograph began with an invitation to spend the year 1958-59 at the Center for Advanced Studies in the Behavioral Sciences. ... spending the year in a community composed predominantly of social scientists confronted me with unanticipated problems about the differences between such communities and those of the natural scientists among whom I had been trained.

Particularly, I was struck by the number and extent of the overt disagreements between social scientists about the nature of the legitimate scientific problems and methods. ... Attempting to discover the source of that difference led me to recognize the role in scientific research of what I have since called 'paradigms'."

So Kuhn developed the theory that natural scientists normally agree on the same paradigm by observing that this was not the case for social scientists. At first Kuhn seems to have thought that the term 'paradigm' should be restricted to fields where there was a unique paradigm, so that the social sciences could be characterised as having a number of competing schools of thought but no shared paradigm. Later, however, he reached the conclusion that each of these competing schools had its own paradigm. As he says (1962, xi):

"Each of the schools ... is guided by something like a paradigm."

I will adopt Kuhn's second position here, so that the difference between the natural sciences and the social sciences can be put as follows. In the natural sciences, outside revolutionary periods, all the scientists accept the same paradigm. In the social sciences, however, social scientists are divided into competing schools. Each school has its own paradigm, but these paradigms are often very different from each other. The contrast is between a single paradigm and a multi-paradigm situation.

As far as the natural sciences are concerned, we can illustrate this with the example of theoretical physics. In this field, all scientists accept a paradigm whose core consists of relativity theory and quantum mechanics. It is not that contemporary theoretical physicists are excessively dogmatic. Most of them probably think that, at some time in the future, there will be another revolution in theoretical physics which will replace relativity and quantum mechanics by some new, and perhaps even stranger, theories. However, they would argue, relativity and quantum mechanics work very well, and so it is sensible to accept them for the time being. If we turn now to economics we find a very different situation.

Economics is a social science, and Kuhn's analysis proves to be quite correct. The economics community is divided into different schools. The members of each of these schools may indeed share a paradigm, but the paradigm of one school can be very different from that of another. Moreover the members of one school are often extremely critical of the views of members of another school.

The school of economics which has the most adherents at present is neoclassical economics. The majority of economists are neoclassical, and this approach can justly be referred to as the mainstream. Indeed Weintraub in his 1992 says, 1:

"When it comes to broad economic theory, most economists agree. ... 'We're all neoclassicals now, even the Keynesians,' because what is taught to students, what is mainstream economics today, is neoclassical economics."

There is some truth in what Weintraub says here, and yet he also exaggerates in some respects. While most economists are indeed neoclassicals, there is a small, but very vocal, minority who reject the neoclassical approach completely. They are known as heterodox economists. Weintraub is also correct to say that some Keynesians do accept neoclassical economics. Versions of Keynes' original theory have been produced which fit in with the neoclassical framework. This is known as the neoclassical synthesis. However, Keynes himself did not accept neoclassical economics, and many Keynesians both in the past and today have been sharply critical of neoclassical economics.

Of course Weintraub is aware of this, and he goes on to say (1992, 2-3):

“Some have argued that there are several schools of thought in present-day economics. They identify (neo)-Marxian economics, neo-Austrian economics, post-Keynesian economics, or (neo)-institutional economics as alternative metatheoretical frameworks for constructing economic theories. To be sure, societies and journals promulgate the ideas associated with these perspectives. ... But to the extent these schools reject the core building blocks of neoclassical economics ... they are regarded by mainstream neoclassical economists as defenders of lost causes or as kooks, misguided critics, and antiscientific oddballs. The status of non-neoclassical economists in the economics departments in English-speaking universities is similar to that of flat-earthers in geography departments: it is safer to voice such opinions after one has tenure, if at all.”

One can certainly agree with Weintraub that it is difficult for heterodox economists to obtain permanent posts in universities, and that, even if they do obtain such a post, they may well be treated badly by their neoclassical colleagues. However, despite these handicaps, there still remain a significant number of heterodox economists who are active in the academic world. They are divided into a number of schools. Leaving out some of the ‘neos’, Weintraub mentions: Marxist, Austrian, Post-Keynesian, and Institutional economist, and one could add some more. There are Sraffian, or neo-Ricardian economists, who are followers of the Italian economist Sraffa who worked at Cambridge with Keynes, but developed his own system. There are also evolutionary economists and economists who use complexity theory.

Weintraub states correctly that neoclassical economists have a low opinion of heterodox economists, but equally most heterodox economists have a low opinion of neoclassical economics. Every few years a book appears by one or more heterodox economists denouncing neoclassical economics as intellectual rubbish. A well-known example of this genre is Keen 2001 *Debunking Economics. The Naked Emperor of the Social Science*. Steve Keen is a Sraffian economist. The economics which he debunks is neoclassical economics. According to him it is like the naked emperor of Hans Christian Andersen’s fairy tale. Another more recent example is Edward Fullbrook (ed.) 2004 *A Guide to What’s Wrong with Economics*. This is a collection of papers by contributors most of whom criticize neoclassical economics very sharply. The general scene in economics then, with its different schools which criticize each other harshly, is very different from that in theoretical physics. There just is no group of heterodox physicists who spend their time denouncing relativity theory and quantum mechanics as valueless theories.

Indeed what is striking is the harshness of the comments which the members of one school of thought in economics make about the other schools of thought. We remarked earlier that a synthesis has been formed between Keynesianism and Neoclassical Economics. Yet some neoclassical economists regard even this very moderate variety of Keynesianism as worthless. This attitude was held, for example, by Robert E. Lucas whom I quoted in section 2. According to Krugman 2009, Robert E. Lucas of the University of Chicago declared in 1980: “At research seminars, people don’t take Keynesian theorising seriously any more; the audience starts to whisper and giggle to one another.” Another nice example of this phenomenon is provided by the Japanese economist Morishima who wrote in his 1973, 1:

“In Japan, for example, Marxian economists have formed an association called Keizai Riron Gakkai (Economic Theoretical Association) in opposition to the Riron Keizai



Gakkai (Theoretical Economic Association) of non-Marxists. In spite of the similarity of the names of the societies, no fruitful conversation has ever been held between them. They are at daggers drawn and describe each other as a society for reactionaries and a society for economists with lower I.Q.s.”

Now it is a general tendency among academic researchers to have a high opinion of their own approach to a subject, and a much lower opinion of alternative approaches. However, this tendency is much more pronounced in economics than it is elsewhere. To explain why this is so, we must introduce a new consideration.

Kuhn claimed correctly that economics is divided into competing schools, each with its own paradigm. However, we can add another point. These different schools are associated with different political ideologies. To examine this question, let us confine ourselves for simplicity to the following schools: Neoclassical Economics, the various versions of Keynesianism, and Marxist Economics. It is easy to see that these schools are arranged on a political spectrum running from the right to the left. There are in fact two compelling arguments for this conclusion.

The first of these arguments looks at the main features of the paradigms adopted by the various schools. Let us begin with Marxist Economics. Perhaps the central claim of the Marxist paradigm is that the profits made by capitalists arise out of the exploitation of workers. The political message of this point of view is clear enough. No one wants to be exploited. So, if the Marxist claim is correct, then workers should seek to abolish capitalism and replace it by socialism. This is just what the Marxists advocate. Here the connection between economic theory and political ideology is quite transparent and is acknowledged by both Marxists and non-Marxists. Let us next turn to Neoclassical Economics. The core of the neo-classical paradigm is equilibrium theory. In a neoclassical equilibrium model, we have firms which arrange their production in order to maximize their profits, given the existing technology; and households which arrange their consumption in order to maximize their utility, given their income. It is then shown that, if there is a market with free competition, this behaviour leads to an equilibrium which is Pareto-optimal. Pareto-optimality means that no redistribution of goods or productive resources can improve the position of one individual without making at least one other individual worse off. The political implications of this are again clear. A market with free competition delivers the best (the Pareto-optimal) results for everyone. Hence politicians, in the interest of society as a whole, should to use the phrase of Portes and Baldusson (2007, 63) introduce “market liberalisation ... and privatisation”. Indeed this is just what politicians the world over have been doing in recent decades, justifying their actions by an appeal to neoclassical economics.<sup>4</sup> Keynesians hold a position intermediate between Marxist and neoclassical economists. Keynesians would not agree that markets with free competition always deliver the best results for society. Keynes himself reached this conclusion during the 1930s when the free market seemed to have delivered the Great Depression with massive unemployment and under-utilisation of capacity. In his economic theory Keynes showed how markets could lead to this sub-optimality situation, and also how government intervention could correct the flaws of the market. However, Keynes did not go as far as the Marxists in advocating the complete abolition of capitalism and the market. He argued for a limited degree of government intervention which would correct the defects of the market while leaving a great deal of economic activity to the market. Naturally this kind of compromise position can occur in

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<sup>4</sup> This is of course only a very concise sketch of the ideological implications of neoclassical economics. A much more detailed account is to be found in Keen (2001, 163). Here Keen shows why neoclassical economics leads to opposition to minimum wage legislation and social security, and support for anti-union laws.

different forms depending on how much government intervention is seen as necessary. The right wing of the Keynesians, those who support the Neoclassical Synthesis, advocate rather little government intervention; while the more left-wing Keynesians, the so-called Post-Keynesians, support more radical government interventions in the economy. Indeed the left wing of the Post-Keynesians overlaps to some extent with the Marxist school. We see from all this the very close links between economic theory and political ideology. These links are further confirmed by our second argument.

This second argument points to the close correlation between the political regime in power and the type of economics taught in the country. This was obvious during the Cold War era when economists in the Soviet Union were all, except for a few dissidents, members of the Marxist school, while economists in the U.S.A. were all, except for a perhaps somewhat larger number of dissidents, members of the Neoclassical school. The contrast between economics and physics is here very striking, since physicists in both the Soviet Union and the U.S.A. adopted exactly the same theories of physics and used those theories in the construction of nuclear weapons. Another striking example is provided by the situation in Britain since the Second World War. Politically speaking we can distinguish two different periods (1945-75) and (1980-2010) with a brief interregnum in 1975-80. In both periods the Conservative party was more to the right of the Labour party. However in the late 1970s, there was a general shift to the right which affected both parties. In 1945-75, the political debate was Tory 'wets' versus old Labour. In 1980-2010, the debate was between Tory Thatcherites, and new Labour. New Labour was to the right of old Labour, and Thatcherites were to the right of Tory 'wets'. This significant shift to the right in the political spectrum was accompanied by a change in the character of the economics taught in British universities. Keynesianism had been very important in British universities in the period 1945-75, but in 1980-2010 the neoclassical economists ousted the Keynesians to a large extent.

The connection between the various schools of economics and political ideologies explains the harsh judgements which economists of one school make of economists of other schools. These are quite similar to the harsh judgements which politicians of one party make about politicians of another party. There is a striking contrast here with subjects, such as physics, which are, at the moment at least, relatively ideology free. In physics, the various schools of thought do not attack each other with the bitterness to be found in economics. Now the bitterness of the feuds between economists is very relevant to the question of the assessment of research in economics. We will show what effect it has in the next section (section 4), but before turning to this, we will close the present section by trying to answer the Queen's question, since the discussion of this section puts us in a position to do so.

We earlier stated our modified version of the Queen's question as follows: "Why did the majority of economists fail to foresee the Global Financial Crash of 2008?" We can now answer this question quite simply as follows. The majority of economists were members of the Neoclassical School. They therefore accepted the core of the neoclassical paradigm, namely equilibrium theory. According to equilibrium theory, a market governed by free competition moves into a Pareto-optimal equilibrium. Now the financial markets throughout the world had in the decades before 2008 been de-regulated and so made to approximate to a freely competitive market. Hence neoclassical economists deduced from their economic paradigm that these financial markets would move towards equilibrium rather than crashing in a catastrophic fashion.

What we see here is a feature of paradigms which applies just as much in the natural sciences as it does in economics. The original empiricists thought that reality could be observed without any theoretical presuppositions, but this is not the case. We always see reality in terms of a framework

provided by a paradigm. Often the paradigm acts like a magnifying glass and brings to light features of reality which we would not otherwise have noticed. However, sometimes the paradigm acts like a set of blinkers, and makes us overlook features of reality which contradict the assertions of the paradigm.

Kuhn gives a wonderful example of a paradigm in astronomy blinding scientists to phenomena which they might otherwise have observed. According to the Aristotelian-Ptolemaic paradigm, which dominated astronomy from Ancient Greek times up to the beginning of the Copernican revolution in the 16<sup>th</sup> century, the heavens were perfect and unchanging. Heavenly bodies such as stars and planets did not change in any way, but continued to carry out their uniform circular motions through the centuries in exactly the same fashion. During this period, European astronomers did not notice any of the changes, such as exploding stars, which in fact did occur in the heavens. However, changes of this sort were noticed and recorded by Chinese astronomers who were not working within the Aristotelian-Ptolemaic paradigm. As Kuhn puts it (1962, 115):

“Can it conceivably be an accident, for example, that Western astronomers first saw change in the previously immutable heavens during the half-century after Copernicus’ new paradigm was first proposed? The Chinese, whose cosmological beliefs did not preclude celestial change, had recorded the appearance of many new stars in the heavens at a much earlier date. Also, even without the aid of a telescope, the Chinese had systematically recorded the appearance of sunspots centuries before these were seen by Galileo and his contemporaries.”

The situation was just the same for neoclassical economists. They failed to observe very dangerous imbalances building up in financial markets because their paradigm told them that these would just be fluctuations which would disappear as market forces brought the system back to equilibrium. This then is my proposed answer to the Queen’s question. The majority of economist failed to notice the coming financial crash because they were looking at economic reality through the glasses provided by the neoclassical paradigm. I will support this answer later on by showing that those economists who did correctly foresee the financial crash were members of economic schools different from the neo-classical school, and so were looking at economic reality through the glasses provided by paradigms which differed from the neoclassical one.

#### **4. The effect of research assessment systems on economics**

Our examination of the community of researchers in economics has led us to the following picture. This community is divided into a number of different schools of thought A, B, C, ..., each with its own paradigm. The members of each school have a very low opinion of the research work produced by the other schools. Now if a Research Assessment System (or RAS) is applied to such a community, what result will it give? My claim is that the research work of the members of whichever school has the largest number of members will receive the highest valuation. So if school A is in the majority, the members of school A will receive the highest valuation. If school B is in the majority, then the members of school B will receive the highest valuation, and so on. The valuation received by the members of a particular school X will be roughly proportional to the number of members of that school.

To argue for this claim, let us first suppose that the RAS is conducted (like the UK RAE) entirely by peer review. I am supposing throughout that the RAS is conducted fairly, so that the

committee which reviews research has representatives from the various schools in exactly the same proportion in which members of these schools occur in the research community as a whole. Now let us suppose that a piece of research work by a member of school A is judged by the committee. Those members of school A on the committee are likely on average to take a favourable view of the piece, while those members of the other schools will take an unfavourable view. I am assuming, of course, that the members of the committee are completely honest in their judgements, and also the rule, for which I have provided very strong evidence in the preceding section, that members of each school in economics have a very low opinion of the research work of other schools. On average then, a piece of research work from school X, will receive favourable judgements from members of school X, and unfavourable judgements from members of the other school. It follows immediately that the valuation received by members of a particular school X will be roughly proportional to the numbers of members of that school.<sup>5</sup> Members of minority schools with few members will on average receive low ratings, while members of whichever school is in the majority will receive on average the highest ratings.

Let us next suppose that the RAS is conducted using bibliometrics instead of peer review. The simplest bibliometric is the citation index in which the value of a piece of research is evaluated by counting the number of references made to it in other published pieces of research. Now the members of each school of economics refer, most frequently, to the papers and books of members of the same school. It follows that if a school has a large number of members, the chance of a piece of research work by a member of that school getting a large number of references is much higher than the chance of a piece of research work by a member of a school which has few members. So, once again, using citation indices, the average evaluation receive by the members of a particular school X is going to be roughly proportional to the number of members of that school. In particular, the members of whichever school is in the majority will, on average, receive the highest valuations.<sup>6</sup> Now, of course, there are a whole variety of different metrics, and a RAS can combine the use of metrics with peer review in complicated ways. However, it is easy to check that, whatever the complicated formula employed, the result will turn out to be the same.

These then are my theoretical reasons for the claim that the valuations of the members of a particular school of economics produced by a RAS will be roughly proportional to the number of members of that school. However, it is always as well to check theoretical arguments against

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<sup>5</sup> David Corfield pointed out to me that in many evaluation procedures the discrimination in favour of the majority school is more than proportional to the number of members of that school. He wrote (private communication): "Take a very simple model for publishing in a top journal where there are two schools A and B, and 2/3 of researchers belong to A, and 1/3 to B. If to be accepted you need two yes votes, and let's imagine ... that reviewers from one school accept papers if and only if they're from their school, then the acceptance rates will be in the ratio 4:1 not 2:1, assuming referees are assigned at random from the population." In what follows, I will continue to assume the 'rough proportionality' model, but, in view of Corfield's point, it should be remembered that this underestimates the discrimination in favour of the majority school and against minority schools.

<sup>6</sup> This was queried by Alberto Baccini (private communication). Baccini agreed that all the citation indices currently in use do increase in value with the number of citations obtained by a paper. This would seem to favour the majority school. However, against this he points out that members of a minority school could cite each other's work more than frequently than members of the majority school, which would act as a countervailing tendency. I am not sure that members of minority schools usually cite each other more frequently than members of the majority school, and, even if they do in some cases, this is unlikely to have a significant effect on the general tendency for members of the majority school to score more highly on citation indices than members of a minority school. In an independent comment (private communication), Frederic Lee argued in the opposite direction. His point was that heterodox economists frequently cite mainstream economists (in order to criticize them), whereas mainstream economists ignore heterodox economists and their criticisms, not citing them at all. Therefore "citations to mainstream economists will be more than proportional to their number", and citation indices correspondingly biased.

empirical data about what happens in practice. Here there is some good information about what happened in a research assessment of economics conducted in Italy in 2005. This assessment was carried out as a trial run for the introduction of a full research assessment system for Italy. Such a system is indeed now (2010-11) being introduced by the Berlusconi government. The 2005 assessment was carried out by the CIVR, or Comitato di Indirizzo per la Valutazione della Ricerca (Committee for the Purpose of Assessing Research). I will refer to the results of this assessment as regards economics as CIVR, 2005. The committee were able to agree in a valuation for two thirds of the research items they considered; but, for the remaining third, a consensus valuation proved to be impossible. In particular one member of the committee (Luigi Pasinetti) strongly criticized some of the valuations of the other members of the committee. The report on the deliberations of the committee was written by its coordinator (Guido Tabellini), but Pasinetti wrote an appendix (Number 4) in which he expressed his points of dissent. Tabellini then replied to this in Appendix 5.

Now Pasinetti is well-known as a leading critic of mainstream neoclassical economics. He has based his own approach to economics on a development of the tradition of Keynes and Sraffa. We would expect on our analysis that Pasinetti's judgments would differ sharply from those of mainstream economists and so it proved to be. Pasinetti and his supporters on the committee objected, according to the report (CIVR, 2005, 9) that

“The valuations given have results on average better ... for items with a high mathematical content and of mainstream theory, than for those of the history of economic thought, economic history, institutional economics, and heterodox theory.”

This is exactly in accordance with our claim.

So far I have considered the effect of a RAS in a single evaluation. I will now turn to the question of the long term effect of a RAS, if such a system is in operation for many years. This was the case with the UK RAE which ran for twenty two years from 1986 to 2008. To discuss this issue it is useful to distinguish between *monistic* and *pluralistic* research communities in economics. Of course, as we have argued, research communities in economics are always to some extent pluralistic, since it is never the case that one paradigm becomes acceptable to all researchers. However, there are undoubtedly different degrees of pluralism. The highest degree of pluralism occurs when researchers of each of the various schools are treated on a par. Members of every school are to be found in the best institutions, on important governmental advisory and decision-making bodies, and so on. The opposite situation, which I will refer to as monism, occurs when members of the majority school hold virtually all the places in the best institutions and have virtually all the good research grants; when governmental advisory and decision-making bodies are composed almost exclusively of members of the majority school; and when the very few remaining members of minority schools of thought lead a beleaguered existence in low grade institutions with poor research facilities. My thesis is that if a RAS operates for a long time, it will push the community of researchers in economics away from pluralism and towards monism.

To see why this is so, let us consider what happens once an evaluation has been carried out. Those groups which have low evaluations have some or all of their research funds removed, while those with high evaluations keep their research funds or have them increased. As far as economics is concerned, this means that members of the majority school have their research funding increased, while the members of minority schools will on average have their research funding reduced. In the specific case of the UK RAE, those departments which had a large proportion of members of minority

schools would receive low ratings and so have their research funding cut. This would have the effect of reducing the research time of the members of those departments and forcing them to do more teaching and administration. Meanwhile their rivals in the majority school would have better conditions and more time for research. However, this is the effect of just one evaluation. Once such evaluations become regular, every department will seek to gain a high evaluation and so more research funds. The best strategy to achieve this is, whenever a post becomes available, to appoint someone who belongs to the majority school. Thus the numbers in the majority will tend to increase while those in the minority schools will tend to diminish, leading in turn to even worse evaluations of their work in the RAS. This trend is likely to be further reinforced by the attitudes of graduate students hoping to get a post as a researcher in economics. The best strategy for them to achieve their goal is obviously to join the majority school. The better institutions have a wider choice of candidates for their posts, and so the percentage of members of the majority school is likely to be highest in those institutions, while, conversely, the few remaining members of minority schools will be forced into accepting posts in low grade institutions where the time and facilities available for research are very limited. Even if a member of a minority school does retain a post at a high grade institution, he or she is less likely to get promoted, since he or she will get a lower rating for his or her research than a rival from the majority school. Note that all these effects will occur whichever school is in the majority at the time that the RAS is introduced. If, for example, the Post-Keynesians had been the majority school in economics in the UK when the RAE was introduced in 1986 (which was clearly not the case), the workings of the RAE would have made them the overwhelming majority. We are facing here something which is a feature of many developmental or evolutionary processes. What starts as a small initial advantage is amplified by the working of the process until it becomes an enormous advantage.

This then is the proposed model of the long term effects of a RAS on a community of researchers in economics. If it is correct, it would lead us to expect that in the period from 1986 to 2008 when the RAE was operating in the UK, the dominance of the majority school in economics (i.e. neoclassical economics) would become stronger and stronger in the UK. This conclusion is supported by the empirical findings presented by Frederic S. Lee in his excellent 2007 paper. Lee begins his paper as follows (2007, 309):

“Previous research ( ... Lee and Harley, 1998) on the impact of the Research Assessment Exercise (RAE) on heterodox economics and heterodox economists in the UK arrived at a discouraging set of conclusions and predictions. ... we concluded that the RAE would continue to drive economic departments to discriminate positively in terms of their hiring, promotion and research strategies in favour of mainstream economists, and their research in order to maintain or improve their ranking (and hence their research funding). As a consequence, we predicted there would, in time, be no or only a token presence of heterodox economists in an increasing number of departments. And, in turn, the near absence of heterodox economists in many economic departments would result in undergraduate, post-graduate and research students only being taught mainstream economics and writing neoclassical doctoral dissertations.”

Lee's 2007 paper, written about a decade after his original predictions, shows them to be amply confirmed.

In his 2007, Lee analyses the results of the 2001 Research Assessment Exercise, and comes up with an interesting result which is shown in our Table 1, which is an extract from Lee's Table 1 given on p. 312 of his 2007.

**Table 1**

Departmental Grades	5*	5	4	3
Percentage of Diamond List publications in total RAE publications	42.57	29.18	23.18	11.81
Diamond List publications per active research staff	1.67	1.16	0.87	0.45

The RAE 2001 was supposed to be conducted by Peer Review, that is to say the committee were supposed to read all the items submitted and grade them. However, Lee shows that the grades given to the departments could have been obtained by a very simple metric, namely calculating the percentage of the total RAE publications which appeared in a particular set of journals, known as the 'Diamond List'. This is seen clearly in Table 1 above. The first line gives the departmental grades which in RAE 2001 were given on the scale 5\*, 5, 4, 3. It will be seen that this grading exactly corresponds to the grading obtained in the second line by calculating the percentage of diamond list publications in total RAE publications of the department. The third line gives an alternative, but equivalent, metric, which consists of calculating the number of diamond list publications per active research staff.

Now what is this 'Diamond List' of economics journals. It consists of 27 journals which are given in Lee, 2007, 311, fn 1. As we would expect, they are journals which publish almost exclusively mainstream economics.

Now what of heterodox economists? Obviously they do not publish in diamond list journals which would not accept their papers. However, they do publish their research in the form of books, or in collections and journals which specialise in heterodox economics. Lee in his 2007 examines how this research was judged in RAE 2001 by introducing a category of H-HET-M publications. H-HET-M stands for 'Heterodox, History of Economic Thought, and Methodology'. Lee gives his results in his Table 3 on p. 313, an extract from which constitutes our Table 2 below.

**Table 2**

Departmental Grades	5*	5	4	3
Percentage of H-HET-M publications in total RAE publications	1.79	2.71	4.75	6.83
H-HET-M publications per active research staff	0.07	0.11	0.17	0.26

Once again a simple metric would produce the departmental grades of RAE 2001. This time the metric consists of calculating the percentage of H-HET-M publications in total RAE publications, but, in contrast to the previous case, the inverse of the metric has to be used for the grading, that is to say the higher this metric, the lower the grade given to the department.

Tables 1 and 2 provide, so I would claim, very strong empirical support for the model I presented earlier. According to this model, in a RAS the members of the school which constitutes the majority will receive on average the highest grades. This is clearly the case for RAE 2001. On average researchers who publish work in the majority school (neoclassical economics) received much higher grades than those who published work in a minority school. It is also striking that the grading in RAE 2001 was in accordance with the principle to which Pasinetti objected in his dissident appendix 4 to CIVR, 2005. He there says (CIVR, 2005, Appendix 4, 6):

“Cases like the following was the first cause of my denying consensus (without any effect, being always in the minority). Quality of the product: ‘This paper is published in a top field journal, the IF (Impact Factor – D.G.) of the journal is high, hence the paper is excellent.’ Or conversely (always on the quality of the paper) ‘this paper is published in my opinion in a non serious journal [in the specific case of the quotation it was the *Journal of Post Keynesian Economics*], hence the quality is ‘limited.’”

This is exactly the principle which was operating in the RAE 2001. If a work was published in the ‘Diamond List’ of mainstream economic journals it received a high rating. If it was a H-HET-M publication, it received a low rating. Naturally Pasinetti objected to this kind of valuation, but his protests were “without any effect” since he was “always in the minority”. This shows clearly the extent to which a RAS in economics consists in the oppression of minorities by the majority.

Given the results of RAE 2001 just analysed, it was clearly in the interest of any economics department in the UK at that time to appoint someone who published papers in the Diamond List, i.e. a mainstream neoclassical economist, and to avoid appointing anyone who produced H-HET-M publications, i.e. a heterodox economist of some kind. This indeed would have been the only sensible strategy for improving the RAE rating of the department and hence obtaining more research funding. One would therefore expect there to be very few heterodox economists in the UK in 2007, and the data given by Lee bears this out. Here is what he says (2007, 321):

“ ... we find that over 60% of British economics departments and 68% of the ranked departments have none or only one heterodox economist on their staff; and in contrast,



less than 16% of the departments and 12% of the ranked departments have a sustained presence of four or more heterodox economists. ... we find that over 77% of the departments and 88% of the ranked departments include only mainstream economics in their course aims and objectives, and that 63% of economic students and 76% of students in ranked departments reside in departments with no or one heterodox economists and which include only mainstream economics in their course aims and objectives. Again in contrast, 5% of economics students and 3% of students in ranked departments reside in departments with four or more heterodox economists and which include both heterodox and mainstream economics in their aims and objectives. These starkly contrasting figures fully support the conclusions and predictions Harley and I made.”

This passage shows how much the economics scene in the UK has changed from how it was in the 1960s and 1970s. At that time, the UK economics research community was very pluralistic. There were many neoclassical economists, but the UK was also famous for the Cambridge school of economists, most of whom were sharp critics of neoclassical economics, and supported radical versions of Keynesianism or the ideas of Sraffa. The position of most members of this school would now be classified as ‘heterodox’, and anyone supporting or trying to develop such a point of view would probably be having a beleaguered existence in some university low down in the academic hierarchy. *Sic transit gloria mundi*. What is surprising, in view of all this, is that groups of heterodox economists, which are small in size, but nonetheless vigorous and active, do continue to exist in the UK. This is a remarkable triumph of independent thought in unfavourable circumstances.

The next consequence which I want to draw from the model presented is the following. If a group of economists gets a very high score on a RAS, this must mean that it is almost entirely monistic in character. It is clear that this must be so, since the presence of any non-mainstream economists in the group will inevitably lower the overall score of the group. We saw earlier that the department of economics at the LSE obtained the remarkably high rating of 3.55 out of 4 in RAE 2008. If our theory is correct, this must mean that that in the period covered by the valuation, the LSE department of economics consisted almost entirely of mainstream neoclassical economists. This is an empirical consequence of the theory which could be checked, though I have not done so.<sup>7</sup>

I will now attempt to use our model to explain the Economics Anomaly in Research Assessment (or EARA). EARA it will be remembered arose because RAE 2008 gave economics the highest research rating of any subject in the UK. Indeed economics was the only subject in the UK to

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<sup>7</sup> I do, however, have a piece of anecdotal evidence which supports this conclusion. For many years, I gave a Master’s course on the *Foundations of Probability* at LSE. I was not actually a member of staff of LSE, but rather of King’s College London. However, the course in question was part of a MSc in Philosophy and History of Science which was run jointly by LSE and King’s College London. Now Keynes’s first piece of research before he turned definitely to economics was in the philosophy of probability. In 1921 he published his *A Treatise on Probability*, which gives a classic account of the logical interpretation of probability. I naturally covered the logical interpretation and Keynes’s *Treatise* in my course. At that time, there was a great deal of discussion of Keynes’s views of probability by the Post-Keynesian school of economists. This school argued that the standard ‘neoclassical synthesis’ version of Keynesianism differed from Keynes’s own views because it omitted any considerations of uncertainty and probability, whereas uncertainty and probability played an important role in Keynes’s *The General Theory*. In fact the Post-Keynesian school thought that Keynes’s early work on the foundations of probability had a profound influence on his later economics, though there were disagreements among its members as to the nature of this influence. I discussed some of the Post-Keynesian work on this topic and published a paper on this subject [Gillies (2003)]. I was once chatting with a well-known economist at LSE and mentioned to him that I discussed the Post-Keynesians’ views in my course on *Foundations of Probability*. He looked at me with some surprise, and said that he had never heard of the Post-Keynesians.

score over 3 (out of a maximum of 4). However, at the time the results of RAE 2008 were announced, it had become obvious to any outsider (such as the Queen) that there was something seriously wrong with economics research in the UK. The preceding few months had witnessed the worst global financial crash since 1929, and yet the great majority of UK economists had altogether failed to foresee, or give any warning about the possibility of such a crash. The anomaly was that a carefully conducted valuation of research such as the RAE should give a result which any outsider could see was obviously wrong. The analysis of this section provides an explanation of EARA. The very high rating for the economics research community given by RAE 2008 actually meant that the economics community of the UK was very monistic. Because of the nature of economics, a highly monistic research community can go seriously wrong. Everyone is looking at economic reality through the spectacles of the same paradigm, and so everyone can miss some important developments which are concealed by the nature of the paradigm. In a more pluralistic community, there would be several representatives of other schools, operating with other paradigms. The dangerous developments might be rendered more visible by these other paradigms, and so the community would get some warning of approaching disaster, and so could take steps to avoid it. But in a monistic community, where a single school dominates, and the representatives of other schools are ignored, no warning is given, or, if it is given, no one pays any attention. In this section I have argued informally, but it is possible to produce a mathematical version of the model being used. This is done in the Appendix for those who like a mathematical approach.

## 5. Arguments for Pluralism

The first and most important argument for pluralism is that history shows that often minority schools give correct results while mainstream schools give wrong results. Hence banning or marginalising minority schools is never a good strategy for developing reliable knowledge. This historical phenomenon of the frequent success of minority schools applies just as much in mathematics and the natural sciences, as it does in economics. In my 2008 (33), I give a typical recent example, taken from research in the bio-medical sciences.

In 2008, Harald zur Hausen was awarded the Nobel prize for the discovery that a form of cervical cancer is caused by a preceding infection by the papilloma virus. In the research which led to the discovery, however, the majority of researchers favoured the view that the causal agent for cervical cancer was a herpes virus and not a papilloma virus. Zur Hausen and his group were the only ones who favoured the papilloma virus.

One of the reasons why the research community supported the idea that a herpes virus was the cause of cervical cancer was that it had been shown that a herpes virus, the Epstein-Barr virus, was the cause of another cancer: Burkitt's Lymphoma. The dominance of the herpes virus approach is shown by the fact that, in December 1972, there was an international conference of researchers in the area at Key Biscayne in Florida, which had the title: Herpesvirus and Cervical Cancer. Zur Hausen attended this conference and made some criticisms of the herpes virus approach. He said that he believed that the results indicate at least a basic difference in the reaction of herpes simplex virus type 2 with cervical cancer cells, as compared to another herpes virus, Epstein-Barr virus. In Burkitt's lymphomas and nasopharyngeal carcinomas, the tumor cells seem to be loaded with viral genomes, and obviously the complete viral genomes are present in those cells. Thus a basic difference seems to exist between these 2 systems. (cf. Goodheart, 1973, 1417). It is reported that

the audience listened to zur Hausen in stony silence (Mcintyre, 2005, 35). The summary of the conference written by George Klein (Klein, 1973) does not mention zur Hausen. At that time zur Hausen's position seemed eccentric and unjustified. Yet zur Hausen and his group continued their research on the papilloma virus, and, after more than twenty years, the research results did convince the majority that the papilloma virus was indeed the cause of many forms of cervical cancer. This has most important consequences since a simple vaccination against infection by the papilloma virus gives protection against cervical cancer.<sup>8</sup>

What happened in the case of research into the viral causation of cervical cancer, also happened in the case of economic research in the decade or so before the great financial crash of 2008. As we have seen, the economists in the mainstream neoclassical paradigm failed to foresee this crash, but, by contrast, several economists working in minority paradigms did successfully predict the crash. In 2010, The Real-World Economics Review decided to give a prize to be called the *Revere Award for Economics*.<sup>9</sup> It was named in honour of Paul Revere and his famous ride through the night to warn the Americans of the approaching British army. Participants in the Real-World Economics Blog were asked to vote for "the three economists who first **and** most clearly anticipated **and** gave public warning of the Global Financial Collapse **and** whose work is most likely to prevent another GFC in the future." 2,500 people voted and the winner by a considerable margin was Steve Keen. As we have seen, Steve Keen is a member of the minority Sraffian school of economics, and has written a book criticizing the mainstream neoclassical paradigm. Steve Keen shared the Revere Prize with Nouriel Roubini and Dean Baker, and there were several other finalists who were judged to have predicted the Global Financial Collapse. An analysis of these finalists shows that they were all, like Steve Keen, critics of neoclassical economics, and belonged to schools which opposed the mainstream. It would take too much time to analyse every economist who was a finalist for the Revere award, but I will here discuss one more example: George Soros, and will mention some recent work of another of the finalists (Ann Pettifor) later in the section.

George Soros has developed a boom/bust model of how financial markets work, which he has used in his own financial activities. This model is based on what he calls 'reflexivity'. Soros criticizes neoclassical economics for failing to recognise reflexivity, and for consequently producing a theoretical construction with little relevance to the real world. Whereas according to neoclassical economics, free markets have a built in tendency to move towards equilibrium, Soros denies that there is any such tendency, and even goes so far as to argue that markets tend towards excess and disequilibrium.

In 1998, Soros published a book entitled 'The Crisis of Global Capitalism' in which he applied his model to the financial sector as a whole. He reached the alarming conclusion that global capitalism was heading towards a very serious crisis. In the introduction, Soros makes clear his objectives in writing the book (1998, xxviii-xxix):

"I want to make it clear that I do not want to abolish capitalism. In spite of its shortcomings, it is better than the alternatives. Instead, I want to prevent the global capitalist system from destroying itself."

Soros argues that, in the global financial system, there is a prevailing bias which he calls 'market fundamentalism' defined as follows (1998, 126-7):

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<sup>8</sup> This very brief sketch of zur Hausen's work is based on the much more detailed account to be found in Clarke (2011).

<sup>9</sup> Further details about the award can be found on <http://rwer.wordpress.com/2010/05/13/keen-roubini-and-baker-win-revere-award-for-economics>.

“The global capitalist system is supported by an ideology rooted in the theory of perfect competition. According to this theory, markets tend toward equilibrium and the equilibrium position represents the most efficient allocation of resources. Any constraints on free competition interfere with the efficiency of the market mechanism; therefore they should be resisted. In previous discussion, I described this as the *laissez faire* ideology but market fundamentalism is a better term.”

Soros was writing his book at a problematic time. There was the Asian crisis, the Russian melt-down, and the collapse of Long Term Capital Management. This was an investment firm so large that its demise put the whole global financial system at risk. Despite all these negative events, Soros pointed out that we might only be at the ‘period of testing’ in his boom/bust model. If the global financial system survived testing by the Asian crisis and its aftermath, the boom would, according to his model, continue in a more exaggerated fashion than ever, leading eventually to a much bigger crash some years down the line. This, of course, is exactly what happened. It should be stressed, however, that this prediction of Soros’ was conditional on the system remaining as it was. It could, he goes on to argue, be corrected in a way which would reduce the chances of a major crash. As he says (1998, 134):

“I have no hesitation, however, in asserting that the global capitalist system will succumb to its defects, if not on this occasion then on the next one – unless we recognize that it is defective and act in time to correct the deficiencies.”

Soros goes on in chapter 8 of his book, entitled: ‘How to Prevent Collapse’ to suggest a number of reforms which he thinks might prevent the crash which he has predicted. Needless to say, these reforms were not adopted, and the predicted crash duly occurred.

As we have seen, monism in economics had a very negative effect regarding the advice which politicians and policy makers received in the decade leading up to the great financial crash of 2008. Naturally politicians and policy makers chose as their advisers, those economists who were the most prestigious under the current system of valuation. This meant that they did *not* hear the views of any members of a minority school, and so were not given any warning of the dangers of a financial collapse. If, instead, they had chosen advisers from each of the minority schools, as well as advisers from the mainstream, they would at least have heard these warnings.

At present we are faced with the problem of how to deal with the consequences of the crash of 2008, and, in particular, with the high levels of public debt which have resulted from governments bailing out the banks. Mainstream economic models claim that to reduce public debt, it is necessary to cut public expenditure. However, it will hardly come as a surprise that these models of the majority school in economics are being sharply criticized by some of the minority schools in economics. Perhaps the most striking such criticism is to be found in Chick and Pettifor (2011). Victoria Chick is one of the leaders of the Post-Keynesian school of economics in the UK; while Ann Pettifor was one of the heterodox economists who correctly predicted the great financial crash of 2008. She was one of the finalists for the Revere prize. In their 2011 paper, Chick and Pettifor argue, using Keynes’s multiplier theory, that cuts in government expenditure will increase rather than decrease government debt, and that to reduce government debt it would be necessary to increase government expenditure. A remarkable feature of their paper is that it supports this conclusion by a detailed consideration of statistics of the UK economy from 1909 to 2009. From these statistics, the authors conclude (2011, 3):

“ ... there is a very strong negative association between government expenditure and the government debt, excluding the two outliers for the World Wars. As public expenditure increases, public debt falls, and *vice-versa*. A simple regression (excluding the World Wars) shows an  $R^2$  of -0.98 ...”

Apart from their overall analysis of the statistics, the authors give a detailed analysis of particular episodes. For example, Chick and Pettifor, write (2011, 7):

“ ... between 1931 and 1933, government expenditure was cut by about 10 per cent. Nominal GDP fell by 2.3 per cent, and government debt rose from 173 to 183 percent of GDP.”

After 1933, however, the UK government was persuaded by Keynes and other economists to replace the cuts policy by one of increasing government expenditure. This policy was implemented from 1934 until the outbreak of the Second World War in 1939. Chick and Pettifor comment (2011, 8):

“The extent of this expansion (in public expenditure from 1933 to 1939 – D.G.), from 12 to 23 per cent of GDP, is not widely appreciated, ... . The economy recovered: real GDP rose by an average annual rate of 4 per cent, the unemployment rate was halved and the public debt fell from 183 to 141 per cent of GDP.”

It is not of course my aim here to compare the merits of Chick and Pettifor’s approach as opposed to the models based on mainstream economics on which the various political parties in the West are relying at the moment. A study of which approach is better confirmed by the data is a job for economists. The question I am raising is the following much simpler one. In seeking advice about the economy, would the government get better advice if they consider only the models based on mainstream economics (monistic strategy), or if they consider not just those models but also the approaches of some heterodox economists such as Chick and Pettifor (pluralist strategy)? This question is of course easy to answer for it is obvious that the pluralist strategy is likely to produce better results.

## 7. Conclusions

I have argued that the effect of a research assessment system on economics is to drive the economics community away from pluralism and towards monism. I have also argued that a pluralistic economics community is much more likely to produce good research than a monistic one. Putting these two claims together, the conclusion is that a research assessment will make research in economics worse rather than better. Hence there is a strong case for abolishing research assessment systems in economics where they exist, and not introducing any new ones.

One result which emerged in the course of the paper was that, if an economics community gets a very high score in a research assessment system, this should be interpreted as meaning that the community is very monistic, and hence in a problematic state as far as research in the subject is concerned. So a high score for an economics group in a research assessment system does not mean that that group is producing a lot of good research – rather the opposite. The fact that scores in a research assessment system can be so misleading is another reason for eliminating such systems.

Of course there are many ways of organising research which do not involve research assessment systems. In my 2008 book, I sketch one possible approach (Gillies, 2008, Part 3,

Chapters 7-11, 65-129). This 2008 book is mainly concerned with research in mathematics, the natural sciences, and the humanities. In the case of economics, an additional point needs to be stressed. The most important factor in designing a system of research organisation for economics is to ensure that pluralism is protected and encouraged. Members of minority schools should have the same chance as members of the majority school of obtaining posts in leading institutions, promotions, research grants, and time and facilities for research. On the teaching side, all economics degree courses should present the views of the minority schools as well as those of the majority school; while committees of economists who advise the government should always include representatives of minority schools as well as representatives of the majority school. In short, pluralism should be encouraged in every context.

### Appendix A. Mathematical model

It is possible to construct a mathematical model which shows the relation between the valuation which a group of economists receives in a RAS and the degree of monism/pluralism of the group. This model, which I call *the MPS model*, is based on simplified assumptions, but nonetheless gives results which I believe to be qualitatively correct. The first simplification is to suppose that there are only two schools of economics – School A (Marxism) and School B (Neoclassical Economics). I suppose that a proportion  $x$  of the economists in the group being evaluated belong to School A, where  $0 \leq x \leq 1$ , so that a proportion  $1-x$  of the economists belong to School B. Let us further suppose that when a research assessment takes place, each economist of the group submits the same number of items for evaluation and that the total number of such items is  $n$ . A committee is formed to carry out the evaluation. We suppose that it contains  $m$  members, and that the proportion of members of the committee belonging to School A on the evaluation committee is the same as the proportion in the group being evaluated, namely  $x$ . We further suppose that the members of the evaluation committee belonging to School A give 1 to each item of research produced by a member of School A, and 0 to each item of research produced by a member of School B; while the members of the evaluation committee belonging to School B give 1 to each item of research produced by a member of School B and 0 to each item of research produced by a member of School A. Naturally this is a simplification, but our earlier discussion shows that it is qualitatively on the right lines.

It is now easy to calculate the total score which the  $n$  items of research produced by the group obtain. There are  $nx$  items produced by School A, and  $mx$  members of School A on the committee. So these  $nx$  items score  $mnx^2$ . Similarly there are  $n(1-x)$  items of research produced by School B, and  $m(1-x)$  members of School B on the committee. So these  $n(1-x)$  items score  $mn(1-x)^2$ . Hence the total score obtained by the  $n$  items of research is

$$mnx^2 + mn(1-x)^2$$

Let us suppose that to obtain the RAS valuation of the group of economists ( $y$  say), we normalize by dividing through by  $mn$  to produce a scale which runs from 0 to 1. We then obtain

$$\begin{aligned} y &= x^2 + (1-x)^2 \\ &= 2x^2 - 2x + 1 \end{aligned}$$

The point P (Pluralism) is the point which represents the maximum degree of pluralism when the two schools A and B are equally represented in the group. It corresponds to the lowest value of the RAS evaluation of the group  $y$ , namely  $y = \frac{1}{2}$ . If the group moves away from pluralism by an increase in the value of  $x$ , i.e. an increase in the proportion of the members of School A in the group, then the RAS valuation  $y$  steadily increases, reaching its maximum of 1 at the point S (Stalinism), when all the members of the group belong to School A. If the group moves away from pluralism by a decrease in the value of  $x$ , i.e. an increase in the proportion of the members of School B in the group, the RAS valuation steadily increases, reaching its maximum of 1 at the point M (McCarthyism), when all the members of the group belong to School B.

I argued in section 5 of the paper that a pluralist group of researchers in economics is likely to produce better results than a monistic group. If we accept this assumption, then the MPS model has a remarkable consequence, namely that to judge the research potential of a research group in economics, we should use not the RAS valuation  $y$ , but its inverse  $1/y$ . This is because  $y$  reaches its minimum value at the maximum of pluralism, where the potential for good research is highest; while it reaches its maximum value at the maximum of monism, where the potential for good research is lowest.  $1/y$  behaves in the opposite fashion, and so gives a better measure of the research potential of a group of economists.

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# Richard Cantillon's Early Monetary Views?

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## 1. Introduction

In April 1759, soon after the publication of Philip Cantillon's book *The Analysis of Trade*, a review appeared in the *Monthly Review*. The reviewer, one William Kenrick, was unimpressed with the literary merits of the work. He perceived a 'puerility of method and poverty of language' evinced by many 'unnecessary and self-evident propositions' (Kenrick, 1759: 309, 310). In addition he accused Cantillon of a lack of originality. The author, the reviewer argued, had in fact reproduced a number of theories that could be found in the *Political Discourses* of 'that masterly writer' David Hume, whom Cantillon frequently quoted 'in justification of his own sentiments; but does not appear always to comprehend' (*ibid.* 309). While Kenrick's criticism was rather too severe,<sup>1</sup> in a number of places in the *Analysis* verbatim quotations and paraphrases of passages from Hume's *Discourses* did indeed occur. Thus, this reviewer's suspicion that the novel monetary ideas in Cantillon's book were in fact borrowed from Hume, was not unreasonable.

Of course we know that this was not the whole story. In addition to adopting some of Hume's ideas, Philip Cantillon (17??-1772) was in fact also, and to a much greater extent, indebted to the writings of his cousin, the banker Richard Cantillon (1687?-1734), who had been murdered a quarter of a century earlier.<sup>2</sup> The latter's great work, the *Essai sur la nature de Commerce en général* had belatedly been published in France, four years before the appearance of the *Analysis*. Kenrick can be excused for not having been aware of this, because by the late 1750s extremely few people in Britain appear to have known the French *Essai*.<sup>3</sup> Modern commentators, following Jevons (1881) have often assumed that Philip Cantillon must have been one of the very few Englishmen at the time who were familiar with that French publication. Indeed, the *Analysis* has generally been thought to be a partial and poor translation of the *Essai* of 1755.

In this paper it is argued that this assumption is incorrect. There is no convincing evidence that Philip Cantillon was even aware of the existence of the French publication. On the title page of the *Analysis* he asserted that his work was '[t]aken chiefly from a Manuscript of a very ingenious Gentleman deceas'd, and adapted to the present Situation of our Trade and Commerce'. It will be shown that this statement, despite having been dismissed by later commentators, is likely to be true.

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<sup>1</sup> Fieser (1994) shows that Kenrick fervently championed Hume on other occasions, accusing other authors too of plagiarising the Scotsman.

<sup>2</sup> Antoin Murphy, by far the most reliable biographer of Richard Cantillon, establishes that he was born sometime between 1680 and 1690 (Murphy, 1986: 10). The more precise dating of his birth to 1687 is based on Murphy's conjecture that the Irishman was naturalized a Frenchman in 1708 upon turning 21 (Murphy, 2011: 2-3). Murphy (1986: 287) identifies Philip as his cousin.

<sup>3</sup> This statement is based on two kinds of evidence. First, there are scarcely any traces of copies of the *Essai* in private libraries in Britain in the third quarter of the 18<sup>th</sup> century. A rare, but of course significant, exception was the presence of a copy in Adam Smith's library. Smith had been in France in the period that the *Essai* was en vogue in Paris and probably picked up his copy there. Second, no British author of the same period quotes or discusses the *Essai*, apart again from Smith's one reference to 'Cantillon' in the *Wealth of Nations*, which was the Cantillon of the *Essai*. Some references to the *Essai* did appear in works that were translated from French to English. See Vivant de Mezaque (1766), Mably (1758, 1766, 1784). Ironically, these French authors assumed the *Essai* to be well-known in Britain and America, believing it had originally been written in English, by an 'English merchant'. In his letters to John Adams, after commenting at some length on Cantillon's views about the rise and decline in the prosperity of nations, Mably stopped himself: 'I will add no more, Sir, as I think Cantillon's work must already have crossed the Atlantic' (Mably, 1784: 90). The *philosophe* may have thought wrong.

One reason why the different source of Philip's borrowing is important, is that the monetary ideas found in the *Analysis* show marked differences from the ones found in the *Essai*. Not only are they frequently less sophisticated when compared to those in the French text, they are also in a number of respects closer to the monetary views of David Hume. There are two ways to explain this curious fact. On the one hand it may be argued that Philip Cantillon, in an ill-conceived attempt to make the argument of his book more conform to the notions expressed in Hume's successful *Discourses*, must have severely cut and simplified the manuscript text at his disposal. Alternatively, that manuscript may have contained the early, more rudimentary monetary ideas of the murdered economist. Since no actual manuscript text has been found it is not possible to decide with absolute certainty which of the two explanations is the right one. But if the second explanation is likely to be correct, as will be argued in this paper, it would make the *Analysis* a far more important text than has previously been recognised.

In discussing the two possibilities this paper addresses two questions. First, what can reasonably be inferred from the differences between the *Analysis* and the *Essai* about the evolution of Richard Cantillon's monetary ideas? Second, what are the implications of a re-examination of the content of the *Analysis* for the longstanding issue of the historical and analytical connections between the economic writings of Richard Cantillon and David Hume? The paper will proceed in the following way. To provide a context to what is to follow, section 2 gives a brief historical overview of the perceived relations between the economic writings of Cantillon and Hume. In section 3, evidence is presented to support the claim that the *Analysis* was based on a different, earlier manuscript version of the *Essay*. In section 4, the 'early' monetary ideas of Cantillon are compared both to his 'mature' ideas and to Hume's views. Section 5 is a conclusion.

## 2. Cantillon and Hume: a long association

The first thing to note about the link that has frequently been made between the economic writings of Hume and Cantillon is that it is not merely a retrospective association made by later generations of historians of economic thought. Commentators both in Britain and France made the connection practically as soon as the writings of the two men appeared in print. The story is somewhat divergent in the two countries due to the different early publication history of the works of Hume and Cantillon. Hume's *Political Discourses* were a publishing success almost immediately on both sides of the Channel. A first French translation appeared in 1752, followed by two other translations, which became widely read and commented upon.<sup>4</sup> Due to this there was never any serious issue in either country of who had access to what version of Hume's economic writings. With Cantillon the situation was quite different. While, as already noted, in Britain the French *Essai* was hardly known, in France the *Analysis of Trade* seems to have gone completely unnoticed.<sup>5</sup> But this did not prevent commentators in either country from perceiving a link between the writings of 'Cantillon' and Hume. In Britain, not only did Kendrick perceive a relation between the views of Hume and Cantillon of the *Analysis*, the same point was made in another piece in Smollett's *Critical Review*.<sup>6</sup> However, unlike Hume's *Discourses*, the *Analysis* had little impact on

<sup>4</sup> See Rochedieu (1948: 161-2). The first translation is ascribed to Mme. De la Chaux (editions published in 1752, 1766 and 1767), the second was by the *abbé* Jean-Bernard Le Blanc (editions in 1754 and 1755) and the third by Eléazar Mauvillon (editions in 1754 and 1761). Cf. n. 8 below.

<sup>5</sup> There does not appear to be a single reference to the *Analysis* in the French economic literature of the third quarter of the 18<sup>th</sup> century. Neither does it seem that any Frenchmen who wrote on economic subjects owned copies of the work. It does not feature in the *Catalogue d'une Bibliothèque d'Économie Politique* of the anglophile economist *abbé* Morellet or the records of the libraries of Turgot, Abeille, Forbonnais, Plumard de Dangeul, or Grasin (all authors who knew or knew of the *Essai*). Neither was it, as far as can be established, in the libraries of Silhouette or Butel-Dumont. I thank Arnaud Orain, of Université Paris 8, for providing me with information about the libraries of the last six men (private correspondence 19 October 2011).

<sup>6</sup> See *The Critical Review* for the Month of March 1759, pp. 241-9.

subsequent British authors, with the notable exception of Sir James Steuart who in his *An Inquiry into the Principles of Political Economy* of 1767 named the work twice.<sup>7</sup>

In France, during the second half of the 1750s, when partly due to the activities of Gournay and his circle, economic theory and policy aroused much public interest, the Cantillon of the *Essai* had a greater impact than the British incarnation. One of its first reviewers, Fréron, hailed the book as ‘one of the best that have been written on the subject of trade’ and noted similarities with Hume’s ideas (Fréron 1755: 68, 72). In *L’ami des hommes* (1756: 85), a work that also quoted Hume in several places, the marquis de Mirabeau agreed, calling Cantillon ‘the most able man’ to have written on the theory of commerce. Similarly, Mably (1757), soon to be a critic of the physiocrats, styled the *Essai* ‘the best work that has been written on the subject’. As a result of such endorsements the *Journal de Commerce* soon referred to the author as ‘le célèbre Cantillon’ and mentioned the *Essays* of Hume and Cantillon in the same breath as ‘les plus profonds Ouvrages des Anglois sur le Commerce’ (Jan. 1760: 69; Jan. 1759: 44-5). Significantly, in 1756 Cantillon’s book was actually reprinted as part of an edition of Hume’s writings, thereby suggesting that the economic writings of the two men were complementary.<sup>8</sup> After two decades or so the fame of Cantillon in France gradually faded and certainly after the Revolution the *Essai* suffered the same fate that had much sooner met the *Analysis* in Britain: it was largely forgotten.

It is a remarkable fact that, due to the separate reception history of the Cantillons in France and Britain, for over a century nobody appears to have noticed any possible connection between the *Essai* and the *Analysis*. McCulloch, for instance, in his influential *Literature of Political Economy* only included a reference to the *Analysis*. Echoing Kendrick’s review, he noted that ‘the author adopts several of the views of Hume, whose *Political Essays* were published in 1752’ (McCulloch 1845:52). Perhaps it was Karl Marx who was the first to puzzle about the relation between the two books, when he applied himself to the study of Cantillon’s ideas in 1863.<sup>9</sup> Presumably unaware that the *Essai* had not been written by Philip Cantillon, he concluded that the *Analysis* ‘proves by its contents that it is a later and revised edition: e.g., in the French edition [*i.e.* the *Essai*], Hume is not yet mentioned, whilst in the English, on the other hand Petty hardly figures any longer’ (Marx [1887] 1977 I, 520, n.2). By observing that ‘the English edition is theoretically less important’ (*ibid.*), Marx implied that the ideas found in that work were more rudimentary, without offering an explanation why that would be the case if it was a later edition.<sup>10</sup>

In his famous article on Cantillon, William Stanley Jevons (1881) provided such an explanation. Having discovered that Philip and Richard had been different individuals, he concluded that the former had simply produced a ‘horribly garbled’ translation of the latter’s French masterpiece (Jevons, [1881] 1931: 335). It is a view that has not been seriously questioned until the present day. Importantly, Jevons’ rediscovery of the *Essai* also involved a reassessment of the relation between the economic writings of Cantillon and Hume. He was scathing about McCulloch’s suggestion that Hume would have influenced Cantillon (*ibid.* 333-4). Not only was this impossible due to the Irishman’s early death, also when the content of the *Essai* was compared to Hume’s *Political Discourses*, it was evident who the superior

<sup>7</sup> Judging by its frequent presence in libraries that were catalogued in the second half of the 18<sup>th</sup> century, the *Analysis* had sold quite well. More notable authors who owned a copy were Malachy Postlethwayt (1768, item 7065) and the ‘Scottish physiocrat’ John Gray (see van den Berg, 2010).

<sup>8</sup> The *Essai* was included in book III (pp. 151-434) of a collection of *Discours Politiques* by Eléazar Mauvillon. Hume’s *Discourses* had appeared as book I in 1754. Book III was published in 1756, ‘à Amsterdam, chez J. Schreuder et Pierre Mortier le jeune’. In 1762 (March, p. 105) the *Journal de Commerce* recommended that ‘the young merchant will perfect his understanding of the theory [of trade] by a reading of Mr. Hume’s discourses on trade and the Essay on the nature of Trade in general of Mr. Cantillon’.

<sup>9</sup> Having already referred to Cantillon in notes dating from 1843-5, Marx made a deeper study of Cantillon in 1863, probably in June. 70 pages of excerpts from the Mauvillon edition of the *Essai* are part of one of the last notebooks of Marx’s Manuscript 1861-63. In the somewhat later *Beihefte*, on the last pages of notebook G, he compared the English (1759) and French (1756) versions of the works ascribed to Cantillon. This comparison formed the basis for his judgment in *Capital* cited above. I thank Regina Roth at the Marx-Engels-Gesamtausgabe (Berlin-Brandenburgische Akademie der Wissenschaften) for supplying me with this information (private correspondence 19 July 2011).

<sup>10</sup> He implicitly rejected the possibility that the English version could have been based on a different source, discounting the statement on the title page that the book was based on a manuscript as ‘a pure fiction, very customary at that time’ (*ibid.*).

economic theorist was. To express his reassessment of the relative merit of the two 18<sup>th</sup> century founders of the discipline of political economy, Jevons took the sobriquet 'cradle of political economy', which Hume's biographer Burton had earlier used to describe the *Political Discourses*, and bestowed it instead upon Cantillon's *Essai* (*ibid.* 333, 359).

After this 'cradle snatching' move, it was also logical to reverse the direction of the question of a possible influence of one man on the other: if Richard Cantillon could not have known Hume's writings, might the Scottish philosopher have been influenced by the Irish banker? Credibility was given to this possibility by Friedrich Hayek ([1931] 1985: 28; cf. 1935: 9) who argued, that 'from a comparison of Hume's monetary theory with that of Cantillon one gets the inescapable impression that Hume must in fact have known Cantillon'. A similar opinion has since been expressed by a number of commentators.

Since by the late 1740s, when Hume started to write his economic discourses, the *Essai* had not yet been published, it is not a trivial question how he could have familiarised himself with Cantillon's work. Two explanations have been proposed. On the one hand, Hume may have been able to lay his hands on a manuscript version of the *Essai*. Thornton (2007), who provides the most recent and lengthy discussion of this possible route of influence, is unable to turn up conclusive evidence.<sup>11</sup> Still, this does not preclude the possibility that Hume did in fact get acquainted with Cantillon's work through his reading of a manuscript. What can safely be discounted, though, is the other route that has been suggested (see e.g. Hayek, [1931] 1985: 28; Fanfani, [1952] 1997: xiii; Brewer, 1992: 186; Wennerlind, 2005: 227 n.3), namely that Hume would have learned of Cantillon's ideas through the earlier, partial publication of the Irishman's writings in a number of works by Malachy Postlethwayt. Even though it is true that a long excerpt of Cantillon's writings appears as early as 1749 in Postlethwayt's *Dissertation*, it is not a fragment in which ideas occur that remind of Hume's *Political Discourses*.<sup>12</sup> With regards to the many fragments Postlethwayt plagiarised from Cantillon in his *Universal Dictionary of Trade and Commerce* (1751-5), all but one were in fact published after the *Political Discourses* appeared.<sup>13</sup> It is therefore simply not possible that any of Postlethwayt's publications could have been the source for Hume's knowledge of Cantillon's ideas.

Despite the fact that no incontrovertible external evidence has been found for a direct influence of Cantillon on Hume, it is nevertheless undeniable that a number of intriguing parallels exist between the

<sup>11</sup> Thornton's case is a further investigation of two points made by Hayek [1931] 1985: 28. First, he argues that it is reasonable to assume Hume would have had access to a manuscript version of the *Essai* during his time in France, partly because, presumably, copies of the Irishman's work had quite a wide circulation there (something posited by others too, see e.g. Schumpeter (1954), Brian and Théré (1997), but without hard evidence) and partly because Hume knew people who had also known Cantillon. Second, it is argued that some of Hume's surviving notes suggest that he made extracts from Cantillon. There may, however, be an over-interpretation of the evidence here. One note is about infanticide in China. Cantillon does write about this as well, but states his source to be 'the *Relations*'. This is very probably Eusèbe Renaudot's *Anciennes Relations des Indes et de la Chine [Aḥbār al-Šīn wa-l-Hind] de deux voyageurs mahométans [Sulaimān et Abū Zaid Ḥasan ibn Yazīd], qui y allèrent dans le neuvième siècle; traduites d'arabe: avec des Remarques sur les principaux endroits de ces relations* (1718). There is no reason why Hume could not have had access to this original, quite well known, source, rather than have learned about it indirectly through Cantillon. In another note Hume observes that steel may increase 10,000 fold in value when it is worked up by labour. Thornton, following Hayek, argues that this is suspiciously similar to Cantillon's observation that the ratio of the value of steel to labour in a watch is 1 to 1 million. The problem with this argument is that it is not Cantillon's proportion but one invented by Postlethwayt, put in his *Universal Dictionary*, and subsequently adopted by Higgs in his English translation of the *Essai*. See van den Berg 2011 n. 17. As explained in note 13 below, it is not credible to argue that Hume would have read the Postlethwayt version.

<sup>12</sup> This long fragment corresponds to parts of the first three chapters of *Essai* part III. It deals primarily with the exchange of currencies.

<sup>13</sup> The publication dates 1751-1755 are somewhat confusing because they have been taken to mean that the first volume of the *Universal Dictionary* appeared in 1751 and the second in 1755. In fact, however, the first volume was only complete by the autumn of 1753. Prior to that, publication of roughly weekly issues had commenced from 1 November 1751. The earliest entry that contained some very limited fragments of Cantillon's writings was 'Arbitration in matters of Foreign Exchange'. It was contained in issue 8, distributed around the beginning of January 1752 (this date can be deduced from a Bill of Complaint against Postlethwayt from 1757 [National Archives C12/2353/64]). Only that entry can be said to predate the publication of Hume's *Political Discourses* in England in the first week of March 1752. However, Hume's work had already been printed in Scotland by the end of 1751 and the *Monthly Review* for January already carried 'an Account of these Discourses, with a large Abstract of them' (*London Daily Advertiser*, 12 Feb. 1752).

writings on monetary issues of the two men. Three topics in particular have attracted attention: the effects of increases in the money supply on prices and production, the price-specie-flow mechanism and the determination of the rate of interest.<sup>14</sup> With respect to each of these topics the Cantillon of the *Essai* offered an analysis that is more refined than what Hume put forward in his *Political Discourses*. Murphy (1985: 203-4) notes: '[...] even if Hume did read Cantillon's *Essai* in manuscript form prior to writing the *Political Discourses* he obviously did not read the *Essai* in depth. If he had, his monetary theory would have been far more sophisticated than that advanced in the *Political Discourses*'. It is an opinion shared by commentators as far apart as Frederick Engels ([1877] 1947: 289) and Murray Rothbard (1995: 426-7).

This is an interesting point in connection with the reassessment of the content of the *Analysis* offered in the next two sections. We will see that the monetary views found in Philip Cantillon's work are in some cases closer to Hume than the views in the *Essai*. My explanation for the differences between the 1759 and 1755 publications is that most of the monetary views found in the *Analysis* were adopted from an early draft of Richard Cantillon's work. The more developed theories found in the *Essai*, though published somewhat earlier, appear to derive from a later manuscript. It is a separate question whether the fact that some of Cantillon's early monetary ideas are closer to Hume's positions also means that the latter would have been familiar with an earlier version of the former's writings. This is a less important issue about which some further remarks will be made about this in the final section of this paper. First, however, it is necessary to present evidence for the main contention that the more rudimentary monetary ideas found in the *Analysis* were not simply a poor rendering by Philip Cantillon of his illustrious cousin's more sophisticated reasoning.

### 3. The fragments on monetary theory in the *Analysis*

*The Analysis of Trade* has always had a bad press. As was noted in the previous section, the 'puerility of method and poverty of language', noted by its earliest reviewer, was explained by Jevons by means of his supposition that the work was in fact a 'horribly garbled translation' of the French *Essai* of 1755. Subsequent commentators have taken the same view calling the *Analysis* a 'very mediocre work' (Hayek [1931] 1985: 218), which 'mutilates the *Essai* badly' (Higgs, 1931: 378), or describing it as a 'bowdlerised inferior version' of that French work (Murphy, 1985: 203). To some extent these judgments are understandable given the fact that the economic theories presented in Philip Cantillon's work are indeed in a number of respects inferior to those found in the *Essai sur la nature de Commerce en général*.

Nevertheless, I have argued in a recent paper (van den Berg, 2011) that earlier assessments of the *Analysis* have been too hasty. It is in fact a much more interesting and significant text than it has been given credit for. This opinion is based on a paragraph-by-paragraph comparison between not only the *Analysis* and the *Essai*, but also a third published version of the writings of Richard Cantillon, namely the numerous fragments that occur in Malachy Postlethwayt's *Universal Dictionary*, first published in instalments between 1751 and 1755.

Since the present paper is concerned with the monetary theory that can be found in the *Analysis*, the focus is here on the chapters XII to XVII, pages 25-68, of that work. When the structure and content of these chapters are compared to the corresponding parts of the *Essai* and of the *Dictionary* one can draw some remarkable conclusions. If one first only compares chapters XII to XVII of Philip Cantillon's

<sup>14</sup> Hayek, for instance, discusses each of these topics. Hayek (1935: 8-9) concentrates on the similarities between the two men's views about the quantity theory of money, while Hayek (1931) focuses on the price-specie flow theory and similarities in their views about the determination of the rate of interest.

work with the *Essai*, and notes which precise passages the two text have in common, then one finds relatively little similarity.<sup>15</sup> An impression of this is given in Figure 1.

The two horizontal bands in the figure represent the text of the *Analysis* (top) and the *Essai* (bottom). The borders dividing up the bands indicate the different chapters (chapter numbers are given above and below the bands respectively). As noted, the comparison is at a paragraph-by-paragraph level (the numbers inside the lower band refer to paragraphs in the *Essai*; the numbers in the upper band refer to page numbers in the *Analysis*). It can be seen that, with the exception of paragraphs 1, 2, 4 and 6 of part I, chapter xvii of the *Essai*, all counterparts for this part of the *Analysis* are found in part II of the French work.<sup>16</sup> The correspondence with part II of the *Essai* is not at all extensive. There are in fact only 8 paragraphs (indicated in the figure with a darker shading), out of a total of 159 paragraphs in part II of the *Essai*, that have counterparts in Philip Cantillon's text.<sup>17</sup> The content of several chapters of the French work (part II, chapters i, iv, v, vii, and viii) is completely missing from the *Analysis*.

Conversely, the larger part of the content of the chapters XII to XVII of the *Analysis* is missing from the *Essai*, and therefore on the basis of a comparison with that French text alone, cannot be positively identified as having been written by Richard Cantillon. These doubtful parts have been indicated in figure 1 by means of dotted areas. While one frequently gets the impression that the arguments in these passages "sound like" Richard Cantillon, since no direct correspondence with the *Essai* is found, for all we know Philip Cantillon here 'mutilated' his cousin's writings beyond easy recognition.

This impression is radically transformed, however, if one extends the comparison to fragments found in Malachy Postlethwayt's *Universal Dictionary of Trade and Commerce* (1751-5). This work has long been known to contain extensive plagiarised fragments of Cantillon's writings (see especially Higgs, 1931: 384). However, one crucial thing has gone unnoticed: the Postlethwayt version deviates in many places from the French text, universally recognised as the authoritative version.<sup>18</sup> A close comparison reveals that throughout the Postlethwayt text, but especially in the part under consideration (corresponding to chapters XII to XVII of the *Analysis*; and to part II of the *Essai*) there are passages where only the two English versions agree. The extent of the correspondence is shown in Figure 2.

<sup>15</sup> A comparison between the full texts of the *Essai* and the *Analysis* and the relevant parts of the *Dictionary* is presented in van den Berg (2011). It should be noted that compared to the very limited correspondences between Philip Cantillon's text and part II of the French publication, correspondences with parts I and III are much more extensive. This *may* indicate that Richard Cantillon's monetary theories, developed in the *Essai* in part II, underwent a greater evolution and developed later than many of his other ideas.

<sup>16</sup> Those paragraphs of the final chapter of part I of the *Essai* correspond to *Analysis* p. 25 to 27. On page 28, where chapter XII continues without anything more than a paragraph break, the *Analysis* picks up the argument of *Essai* II, ii, 2.

<sup>17</sup> This conclusion largely agrees with the only other detailed comparison between the contents of the *Essai* and the *Analysis* made by Groenewegen (2001: 185-8). Where Groenewegen differs is that he sees a correspondence between *Analysis*, pp. 28-30, and *Essai* II, i (rather than II, ii) and he sees an additional correspondence between *Essai* II, viii, 5 and an expression in *Analysis* p. 126. I agree with the latter.

<sup>18</sup> For a fuller discussion see van den Berg (2011).

	XII		XIII	XIV	XV	XVI	XVII	
Analysis	25	28	33	42	43-45	51	62-63	65-66
			34-42		44	51	62	66
						52-54-62	63	67
Essai	1, 2	2, 3, 4		20		8	6	3
	I, XVII	II, II		II, III		II, VI	II, IX	II, X

Figure 1



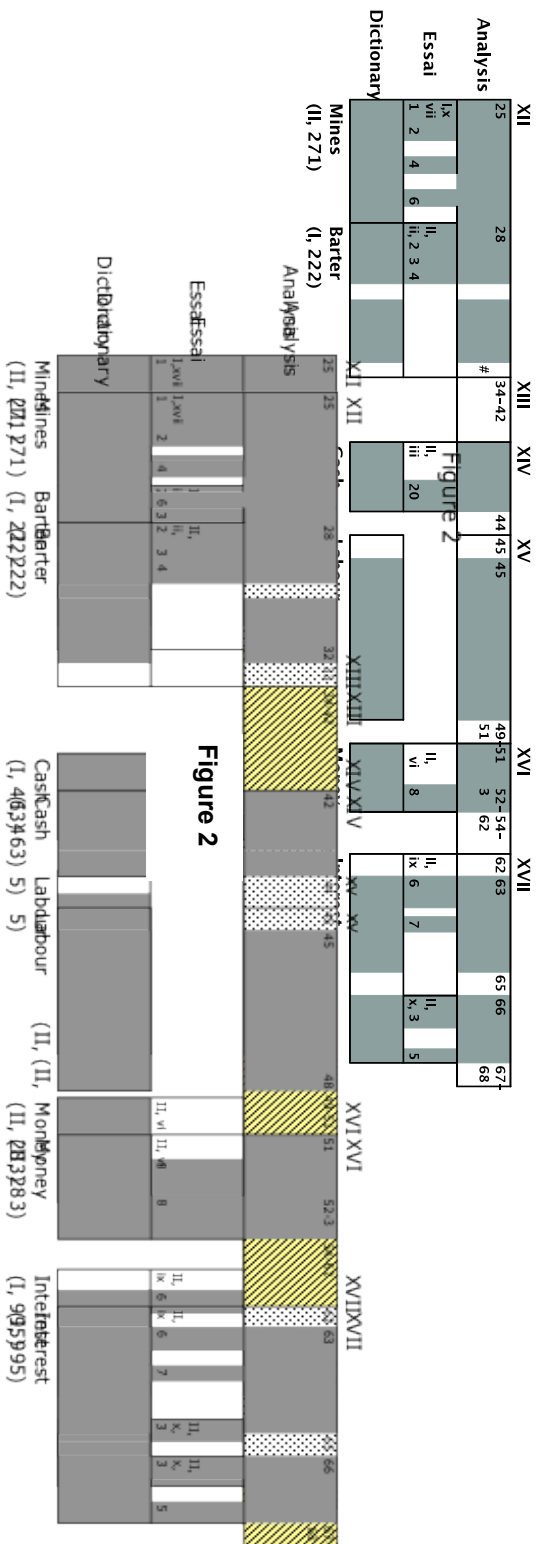


Figure 2

The figure shows that when parts of the content of entries in Postlethwayt's *Dictionary* (the names of the entries are given below the added band) are compared to the *Analysis*, many more passages in the latter text turn out to have counterparts. The more extensive darker shaded areas in the top band of Figure 2, as compared to figure 1, indicate this. This surprising finding has two implications. First, there are simply too many divergences between the two English versions on the one hand and the *Essai* on the other for it to be plausible that the French publication of 1755 was the source for Philip Cantillon's work (as has often been assumed). Second, the findings portrayed in Figure 2 severely limit the extent to which Philip Cantillon may have 'mutilated' the text that he did use: due to the correspondences with a publication that was published several years earlier, it is not possible that Philip essentially concocted the monetary ideas found in those particular passages. Admittedly, he may well have exercised an influence on precise formulations.<sup>19</sup> For this reason we cannot be sure that the text of the *Analysis* gives us the monetary views of Richard Cantillon in the *exact words* used in the manuscript that passed into the hands of his cousin. Nevertheless the somewhat lesser claim that these passages (not found in the *Essai*, but present in the *Dictionary*) were adopted from a pre-existing source appears pretty safe.

At the same time, it is unlikely that this source was Postlethwayt's *Universal Dictionary*. There are a number of reasons to reject this possibility, but the three most important are: a) it would have been exceedingly difficult for Philip to identify the relevant fragments in Postlethwayt's massive work; b) the monetary views in the Postlethwayt text are often more sophisticated or formulated differently; c) the Postlethwayt text exhibits many further correspondences with part II of the *Essai* that are not found in the *Analysis*; To illustrate the last point, Figure 3 shows the full correspondences between the parts under consideration of the three texts.<sup>20</sup>

The large gaps that appear in the top band of Figure 3 illustrate the considerable extent to which content that is present in the *Essai* (middle band) and/or the *Dictionary* (lower band) is absent from the *Analysis*.

To return to Figure 2, the exercise of comparing the texts also allows one to pinpoint more reliably which parts of the *Analysis* chapters under consideration are likely to have been additions by Philip Cantillon. Two kinds of markings have been used in Figure 2 to distinguish the remaining parts for which counterparts are found in neither the *Essai* nor the *Dictionary*. The relatively few short passages that are represented by light dotted areas are more likely to derive from a manuscript of the murdered economist. The reason for thinking this is that the content of these passages generally fits seamlessly with the preceding and/or subsequent argument and there is nothing that suggests a dating after the early 1730s.

<sup>19</sup> Especially if it is true that the parts of the text that derive from Richard Cantillon are a translation from a manuscript that was written in French. On this point see van den Berg (2011)

<sup>20</sup> This figure is cut in half simply to make it fit on a single page. It corresponds to the middle part of figure 1 in van den Berg (2011) with this difference that the middle and bottom bands are swapped around. Note that since in this figure the *Essai* is used as the base text, the order of presentation of the *Analysis* text is somewhat changed: chapter XV is placed after chapter XVI (for this reason this part is marked with horizontal lines). The content of this chapter is discussed in section 4.2.

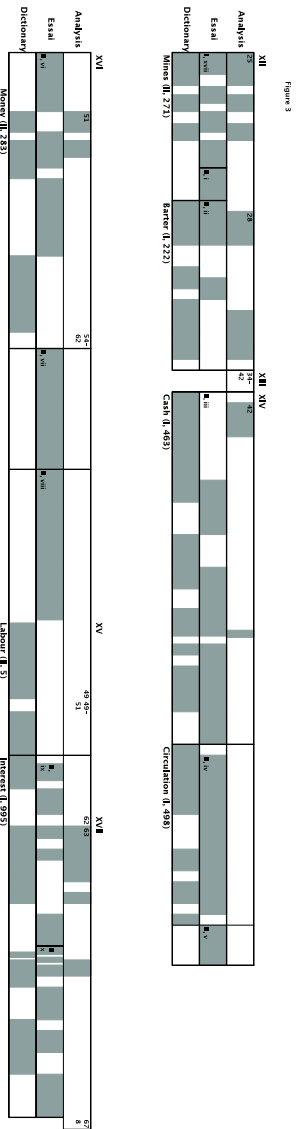


Figure 3

On the other hand, the areas in yellow denote parts where Philip supplemented the content of that text with writing of his own and that of other authors. The author that he used most extensively is David Hume. There are three places in the *Analysis* where the name of the famous Scotsman is used: on pages 34, 49 and 186. The last mention of '[t]hat profound and ingenious Reasoner, on the Subject of Money, Trade, and Commerce *Dauid* [sic] *Hume*, Esq.' (Cantillon, 1759: 186), precedes a long verbatim quotation from the essay 'Of Money' (pp. 186-193).<sup>21</sup> While this quote is extensive, its use is arguably less interesting than the three other places where Philip Cantillon relies on Hume. The first instance of this is in chapter XIII, with the heading *Of Money and its Value* (pp. 34-42). While Hume's name occurs only at the beginning, Philip Cantillon in fact makes surreptitious use of 'Of Money' throughout that chapter and intermixes it with views that appear to derive from Richard Cantillon. The second mention of Hume, on page 49, is followed by paraphrased passages, on pp. 49-50, from his essay 'Of Public Credit'. Finally, there are several passages again reminiscent of 'Of Money' at pages 54-5, even though Hume's name is not mentioned. Again the argument seems to be combined with Richard Cantillon's views. The content of these three fragments will be examined in more detail in Section 4 below.

#### 4. Richard Cantillon's Early Monetary Views?

The comparison presented in the previous section suggests that the middle part of the *Analysis* (chapters XII to XVII) was, to an important extent, based on a manuscript that was less developed than the manuscripts that were the source for respectively, the *Essai* of 1755 and the several entries in Postlethwayt's *Dictionary*. If the content of the *Analysis* was indeed 'taken chiefly from' an early draft manuscript, then a comparison between the monetary ideas found in that work and those in the *Essai* and in Postlethwayt's *Dictionary* can provide us with valuable insights into how the Irish banker's ideas evolved during the process of composing his ground-breaking work. This comparison is attempted in this section. The discussion will focus on three topics in monetary theory for which both Richard Cantillon and David Hume are famous, *i.e.* the quantity theory of money, the price-specie flow mechanism, and the determination of the interest rate.

##### 4.1. The Quantity Theory of Money

Most modern students of Cantillon's work have argued that he did not in fact subscribe to the quantity theory of money. That is to say, Cantillon '[...] was not prepared to accept a crude monetarist view that, if the money supply increased, prices would rise proportionally' (Murphy 2009: 85). It is indeed true that the Cantillon of the *Essai*, as we for our purposes will have to call him, makes conscious efforts to distance himself from earlier formulations of the quantity theory, specifically those found in John Locke.<sup>22</sup> He does this by examining in detail the channels through which additional quantities of money enter the circular flow. From this much-admired analysis he concludes:

that by doubling the quantity of money in a State the prices of products and merchandise are not always doubled. A River which runs and winds about in its bed will not flow with double the speed when the amount of its water is doubled

(translation Higgs 1931:177).

<sup>21</sup> In three places sentences that appear in Hume's essay are omitted, and occasionally a word is changed.

<sup>22</sup> I do not consider here the anachronistic question whether John Locke can be considered a monetarist. For a discussion see Eltis (1996).

One might debate whether this conclusion implies merely a qualification of the quantity theory or a fundamental rejection, but it clearly does *not* endorse a simple view of a proportional relation between money supply and the price level.

Curiously, this simple view *is* found in *The Analysis*. The first statement of the quantity theory of money in that work is in chapter XII, *Of Mines and Barter* (Cantillon, 1759: 25-33). This chapter commences the analysis of money by considering metals, like copper and silver, as products of mining activity similar to other commodities like lead, coal, and tin (*ibid.* 25-6): their 'value' is determined by their cost of production, *i.e.* the 'Value of Land and Labour made use of', while their 'price' varies with market demand, or more precisely 'according to the Taste, Luxury and Will of the Proprietors of Land' (*ibid.* 26).<sup>23</sup> Subsequently it is supposed that in a small state or community, with mines, but 'without foreign Trade or Intercourse with its Neighbours', copper is introduced as money:

Suppose that part of this Metal is ordered to Pass for Money; it is clear that this new Use for it, would cause a greater Demand, and a greater Quantity would be taken from the Mines, than formerly, in order to supply this new Demand (*ibid.* 27).<sup>24</sup>

While this perhaps suggests a conception of the money supply as an 'endogenous' quantity, *i.e.* produced in response to the demand for it, in subsequent passages of chapter XII money tends to be considered as a given quantity. Once metal (primarily silver and gold) came to be used as money, then 'by its Quantity at Market, [it] finds its Proportion, by passing there at a fixed Value in Exchange for the Land's produce'.<sup>25</sup> After explaining further 'how Money operates in bringing about this Proportion', in a passage that exhibits clear parallels with the famous example in the *Essai* (II, ii, 2-4) of the determination of the market price of peas,<sup>26</sup> the *Analysis* considers the effects of a sudden increase in the money supply. In particular there is this striking statement of the quantity theory of money (left-hand column):

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23 The corresponding paragraphs in the *Essai* have a similar distinction but use somewhat different terms. There the distinction is between '*la valeur réelle ou intrinsèque des Metaux*' (I, xvii, 2) and '*la valeur des Metaux au Marché*' (I, xvii, 3). The Postlethwayt text in turn juxtaposes the notions of 'value' and 'price' in the same way as the *Analysis* (see Postlethwayt, 1751-5, II, 271).

24 In a slight elaboration the Postlethwayt text says that the additional demand for copper when it starts being used as money, will first raise its price and so stimulate a greater production: 'this additional demand of copper will make it dearer, and encourage the digging out of the mines more of it than usual' (Postlethwayt, 1751-55, II, 271).

<sup>25</sup> The Postlethwayt text expresses this same idea more fully: 'Money alone (I do not here consider what is used for money) naturally finds out this proportion, and the quantity of money which is brought to market to barter for each kind of commodity, readily fixes the proportion of value that is between them all, *Quae eadem uni tertio sunt eadem inter se* ['What are identical with a third thing are identical with one another']. Every body who brings money to market knows what money he has to lay out for the expence of that day'. The French text does not have a direct counterpart to these passages.

<sup>26</sup> The Postlethwayt text has corresponding passages to the two other versions in the entry 'Barter' (Postlethwayt, 1751-5, I, 222). One difference is that while the *Essai* and the *Dictionary* use the market for peas as an example, the *Analysis* uses wheat.

Table 1

*Analysis* (p.31)

Let us suppose a Town, and the Lands about it, that has no Communication with its Neighbours, and that the Money necessary in Circulation, in such a Place is 100,000*l.* to carry on all its Wants; and let it be supposed that this Sum by one Accident or other, increased to 200,000*l.* so that such as had twenty Shillings, would by this increase have forty Shillings: this Town and its Districts about it, considered in itself, would not be richer, happier, or more powerful than it was before this Acquisition; but the Prices of every Thing would be double to what it was before: and this I shall attempt to make clear, by what I shall say of the consequential Effects of the Increase and Decrease of Money to a Community.

*Universal Dictionary* (I, 223)

If the money which carries on the barter of a city (which, at present, we will consider as if there was no other in the world) be 100,000 ounces of silver, that is to say, if all the proportions of the values of all goods and commodities in the said city, be measured by the 100,000 ounces; or, what still comes to the same thing, if these 100,000 ounces pass for pledges, and keep the accounts of the pretensions of all barterers in the said city. And if, in these circumstances, the said city receives 100,000 ounces more, so distributed that every one who has had an ounce of silver, has now two ounces, and that the quantity of money in circulation becomes 200,000 ounces of silver; this city, considered in itself, is not in any respect richer or happier than before: it will only happen that all goods and commodities will grow twice as dear as they were. - Though this consequence seems mighty plain, yet I shall endeavour to set it in a clearer light under the article MONEY, when I come to consider particularly the effects of the increase and decrease of the real quantity of money in a state.

It is worth noting a few things about this statement. First, a kind of thought experiment is proposed whereby the money supply of an economy, explicitly considered in isolation, is suddenly doubled for some *exogenous* reason ('one Accident or other'). Second, it is argued that, as a consequence, only the general price level would be affected (indeed, it is doubled), leaving the volume of output unaffected (that is, the economy would not be 'richer, happier, or more powerful' than before the change). This bold statement of what in modern economics is called the 'neutrality of money'<sup>27</sup> is of course quite different from the much more guarded positions known from Cantillon of the *Essai*: in particular a) there is no attempt to explain any transmission mechanisms between money and prices, b) the injection is assumed (for simplicity?) to be such that all individuals, in the same moment, see their money holdings doubled,<sup>28</sup>

<sup>27</sup> Patinkin (1987) explains that the phrase 'neutrality of money' came in use in the 1930s to indicate the proposition that money and absolute prices vary proportionally in the long run leaving relative prices and the level of real output unaffected. But as Blaug (1996a: 29-30) points out this was 'a familiar quantity-theory proposition all through the nineteenth century long before a memorable name for it had been invented'.

<sup>28</sup> Note that the same assumption is made, more explicitly, in the Postlethwayt text. One gets the impression that in that version the assumption serves as an initial case that can subsequently be varied. The *Analysis* does not clearly offer a discussion of these variations but the *Dictionary*, like the *Essai*, does. The *Essai*, in turn, does not consider the "proportional distribution case" from the outset. Only in the concluding remarks of the relevant discussion there is an acknowledgement of the case as an exceptional situation in which a proportional increase in the general price level will indeed occur: 'the dearness caused by this [increase in] money does not affect equally all kinds of products and merchandise, proportionably to the quantity of money, [...] unless those who offer in the Market one ounce of silver be the same and only ones who now offer two ounces when the amount of money in circulation is doubled in quantity, and that is hardly ever the case' (*Essai* II, vii, 10, translation Higgs 1931:181; emphases added). Thus all versions agree on the point that if the increase in the money supply would be distributed equally to all participants in proportion to their previous money holdings, the effect on prices would be "neutral". But they differ with regards to the importance given to this case.

and c) there is no distinction between the immediate ('short run') and eventual ('long run') effects on prices and output.

In fact, the passage is sufficiently unlike what we find in the *Essai* to entertain reasonable doubt whether Richard Cantillon could have written it. Could Philip Cantillon instead have borrowed it from an author other than his cousin? David Hume would appear to be a likely candidate, especially because it is evident that Philip knew his *Political Discourses*. We will see in a moment that the latter does indeed link the views discussed above to Hume's positions. Nevertheless, the conclusion that they are no more than paraphrases of Hume is unwarranted: comparing the *Analysis* passage of page 31 with the corresponding one in Postlethwayt's *Dictionary* (see the right-hand column in table 1), it is quite clear that the structure of the argument, though not the actual wording, is very similar. This strongly suggests, first, that Philip Cantillon did not write this passage, or the preceding and subsequent passages, in imitation of Hume.<sup>29</sup> Second, since the entry 'Barter' of the *Dictionary* was first published in the spring of 1752, it is highly unlikely that Postlethwayt had in his turn based those passages on Hume.<sup>30</sup> Something more interesting is likely to have been the case: Postlethwayt and Philip Cantillon appear to have relied on manuscripts of a third author in which some ideas were expressed that resembled the later views of Hume.

This impression is confirmed by an examination of the subsequent fragments referred to in each of the final sentence of the two passages in table 1. In the *Analysis* it is a reference to chapter XVI (pp. 51-61), which has the precise title *Of the consequential Effect, which the Increase and Decrease of the current Coin of a Country, has on the Community*; in the *Universal Dictionary* the discussion is found in the entry 'Money', under the subheading *Of the INCREASE and DECREASE of the ACTUAL QUANTITY OF REAL OR HARD MONEY in a STATE*' (Postlethwayt, 1751-5, II 283). Some of the passages in those sections in turn correspond with *Essai* II, vi, which has the strikingly similar title: *De l'augmentation & de la diminution de la quantité d'argent effectif dans un Etat*.<sup>31</sup>

It is precisely in this chapter vi of part II of the *Essai* where Cantillon commences his celebrated discussion of the effects of increases in the money supply, analysed under various assumptions with

<sup>29</sup> Figure 2 gives an impression of the extent of correspondence between *Analysis* chapter XII and Postlethwayt's entries 'Mines' and 'Barter'. Examples of textual comparisons of passages in this part of the texts are given in notes 26 to 29 above. Further comparisons cannot be given here apart from that between the paragraphs that immediately follow the ones cited in table 1. Both texts discuss an alleged historical case of a sudden increase in the money supply, namely the moment Augustus returned to Rome with a great treasure after defeating Anthony and Cleopatra, something that is said to have doubled prices in that city. (Actually, the source of this story Cassius Dio *Roman History*, book 51, chapter 21, only mentions a rise in prices, not a doubling). There are some interesting differences between the corresponding paragraphs. In particular, the Postlethwayt version is much more careful to distinguish between the effects of a sudden and a gradual increase in the money supply:

*Analysis* (p.31):

'History informs us, that after the Defeat of Anthony and Cleopatra, by Augustus Cæsar, the immense Riches brought by him to Rome, and there dispersed in Circulation, raised the Price of Provisions and Merchandize to double what they were before;'

*Dictionary* (I, 223):

'When Augustus returned to Rome, after the defeat of Mark Anthony and Cleopatra, he brought with him so great a quantity of money, that all goods and commodities sold immediately for double the value they sold before, as Dion Cassius tells us. If all the money he brought to Rome had been laid up in the treasury, it would not have had this effect; for it would have entered but slowly into circulation and barter: but he distributed it among his soldiers, whom he was not able to pay after the battle of Actium, by which means it came quickly into circulation.'

<sup>30</sup> 'Barter' was first published in issue 19 of the *Universal Dictionary* from which it can be deduced that it appeared around March 1752. As was noted (n.13 above) Hume's *Political Discourses* was published in England in the first week of March 1752. It is improbable that Postlethwayt would have had the time to digest the content of Hume's work by this time. More importantly, the correspondences between the entry 'Barter' and *Essai* II, ii are simply too great for there not to be a strong link with Richard Cantillon. To be sure, Hume's *Political Discourses* are quoted in Postlethwayt's *Dictionary*, but only in entries that first appeared some time later. The first entry that contains a long quotation from Hume's 'Of Public Credit' is 'Credit, or public credit' (Postlethwayt, 1751-5, I, 576-81; see p. 580 to end). This entry first appeared in issue 49, which was published in the first few months of 1753.

<sup>31</sup> Higgs (1931: 159) translates the term 'argent effectif' with 'hard money' possibly relying on Postlethwayt. Saucier and Thornton (2010) simply leave the adjective out.

regards to the source and point of entry into circulation of the additional quantity of money. In the French text the whole discussion is presented as a critical development of John Locke:

[Locke] has clearly seen that the abundance of money makes everything dear, but he has not considered how it does so. The great difficulty of this question consists in knowing in what way and in what proportion the increase of money raises prices  
(*Essai* II, vi, 4; translation Higgs, 1931:161).

While the *Essai* starts the discussion of this question with the assumption of an increase in the money supply due to a discovery of silver or goldmines, the considerably shorter discussion of the *Analysis* again assumes 'an Accident'<sup>32</sup>:

Suppose fifteen Millions of Specie sufficient to circulate the Trade of *England*; this Money would roll backwards and forwards amongst the Individuals of Society, in Barter and Exchange of the Land's Produce, and in its several Windings and Turnings, it fixes the Rents of Land, as also the Hire of Labour and Artizans.

Let it be again supposed, that these fifteen Millions of Money, by some Accident or other, was all of a sudden doubled and put in the Channels of Trade; the Money then circulating in *England* would be thirty Millions; (Cantillon, 1759: 51-2)

The account of what would happen subsequently attempts to sketch a transmission mechanism and suggests some development beyond a mere restatement of the simple quantity theory. I quote it in full<sup>33</sup>:

[...] this sudden Increase of Cash, would increase a proportional Demand for Merchandize, as also for Labour and Industry, and consequently enhance their Price, as also that of all Sorts of Goods and Merchandize; and whenever Things got up double in Price to what they formerly went at, properly speaking, no one would suffer, but the Landholders; the Farmers would be Gainers during the Term of their Leases; but when these expired, the Landholders would double their Rents, and justly, as the Land's Produce sold at double to what it formerly yielded.

This sudden Increase of Species would encourage Merchants and Manufacturers, to embark a-new in fresh Undertakings and Adventures, and from the Increase of their Money, they would naturally be induced to indulge themselves in better Living, to keep better Tables, and consequently a proportional Demand would arise both for the Necessaries of Life, and for Labourers and Artizans; and the Proprietors of Land would receive double Income to what they formerly received; and this Increase of Money being thus thrown into Circulation, and thereby the Prices of all Things becoming in proportion dearer, yet the Merchants not being able to get a proportional Profit on their new Undertakings and Adventures, they would in this Case, if guided by Prudence to themselves and Families, be necessitated to retrench their new Method of Living, and return to the Old; otherwise the Consequence must be Bankruptcy: and others, who lived with greater Oeconomy, and saved their Profits, would rise in their Place; *all Things in the Course of Time, would return to their former Situation*; for let who will suffer Money and Labour, by degrees, will at last find their due Proportion; because if it is certain that the Quantity of coined Money in the Channels of Trade, and the Credit there given, forms and fixes from time to time the Prices of all Things at Market

<sup>32</sup> These two short paragraphs only have counterparts in the *Dictionary* (Postlethwayt, 1751-5, II: 283, 'Money').

<sup>33</sup> These two paragraphs show correspondences with both *Essai* II, vi, 8 and with the *Dictionary* text (Postlethwayt, 1751-5, II: 283, 'Money'). In fact, the latter version exhibits most correspondences with the other two and appears to combine the argument.



(Cantillon 1759: 52-4; emphasis added).

The case considered shows similarities with the earlier statement (quoted in table 1 above), in the sense that a sudden 'exogenous' doubling of the money supply is considered, and that foreign trade is abstracted from. However, the argument is more developed, because a) a transmission mechanism is considered, b) there is some suggestion that not all prices, and rents especially, will rise at the same time, and c) a *temporary* expansion of economic activity, coupled with increases in consumption, is acknowledged. However, compared to the other two versions, in the *Analysis* the assumption of who gets the additional money first and what are the subsequent relative price and output effects is far less distinct.<sup>34</sup> What is more, this is where the discussion that appears to be based on Richard Cantillon's manuscript ends, while the two other versions offer extensive further discussions.<sup>35</sup>

It has to be said that the similarities between the views expressed in the various passages identified here (chiefly pp. 31-3 and 51-4 of the *Analysis*) and Hume's essay 'Of Money' are striking. That this did not escape Philip Cantillon is clear from the fact that in the remainder of chapter XVI he paraphrases 'Of Money' (pp. 54-5), before digressing on the trade of Holland and Ireland. A very similar thing occurs somewhat earlier in the text where Philip appears to have inserted a whole chapter of his own, XIII *Of Money and its Value* (pp. 34-42; cf. figure 2), which contains more extensive quotes and paraphrases of Hume's essay 'Of Money'.<sup>36</sup> In one place Philip Cantillon makes a direct link between the passage quoted and Hume's views:

*I have already observed [i.e. p.31, see table 1] that in considering one Country in itself, without Trade or Connection with its Neighbours, the greater or less Quantity of Money is of little Consequence to such a State or Community: 'tis only those Nations and Kingdoms who have Connections and Commerce with their Neighbours, that reap the Advantage of the superior Figure, Power, and Influence which Money gives them over other States and Monarchies* (Cantillon 1759: 37-8; emphasis added).

The fact that part of the first sentence is found almost verbatim in 'Of Money' shows that Philip is here borrowing from Hume.<sup>37</sup> The next two pages in the *Analysis* follow the Scotsman's argument and include his well-known proviso that when an increase in the money supply results from a positive balance of trade it can *temporarily* have positive effects on output<sup>38</sup>:

<sup>34</sup> Note that even though the *Analysis* passage does not clearly specify a social group that is assumed to receive the increase quantity of money at first, there is a suggestion that 'Merchants and Manufacturers' are early recipients. In the *Essai* it is the people who are most directly involved in the mining of precious metal. In the Postlethwayt version, on the other hand, the assumption is that the additional money, 2000 ounces of silver, is initially received 'for a present from Americans' by the 'undertakers' of a country (Postlethwayt, 1751-5, II, 283). It is important to note that by supposing 2000 ounces of silver to be in circulation previously, the Postlethwayt text continues a discussion of the circulation of money elsewhere in the *Dictionary* (*ibid.* 'Cash' I, 463), which offers a more extended version of *Essai* II, iii.

<sup>35</sup> The *Essai* does of course continue with the consideration of the effects of various other injections of money into circulation. The same is true for the Postlethwayt text, although there are fascinating differences between that text and the *Essai*, including a very clear statement of indirect transmission effects through the interest rate and a more rigorous distinction between the effects of an increased money supply in a closed and an open economy. This *may* indicate that the Postlethwayt version was actually based on a manuscript that in this place was *more* developed than the French text. This intriguing possibility cannot be dealt with in greater detail here because of our focus on Cantillon's early monetary thought.

<sup>36</sup> Those passages are intermixed with ideas that sound more like Richard Cantillon, but for which direct counterparts are found in neither the *Essai* nor the *Dictionary*.

<sup>37</sup> The sentence in 'Of Money' reads: 'If we consider any kingdom by itself, it is evident, that the greater or less plenty of money is of no consequence' (Hume, [1952] 1955: 33).

<sup>38</sup> A variety of views exist about the meaning of Hume's proviso to the 'notorious proportionality theorem' (Blaug, 1996a: 29). Most common is the interpretation that Hume allows for the non-neutrality of money in the short run only. Alternatively Wennerlind (2005) argues that Hume allows for positive effects on real output only if the increase in the money supply is 'endogenous', *i.e.* a result of a positive balance of trade. Yet other commentators, e.g. Rotwein (1955: lx-lxvi), Rashid (1984), Murphy (2009: 101) have warned against attempts to read too much consistency into Hume's contradictory statements. Without going over the differences in these views, it may be said that Philip Cantillon's rendering of Hume's proviso is consistent with more than one interpretation of the latter's position.

[...] but in a Country in which real Money begins to flow, from the Consequence of a well-regulated [foreign] Commerce, Industry and Labour gain new Life; tho' the high Price of Goods is a natural Consequence of the Increase of Bullion, yet this Consequence does not immediately follow. Money acquired by Trade takes Time before it circulates; its Effect is produced by degrees, and until it arrives at its height, all Degrees of People in the Nation are whetted by a laudable Ambition and Application to acquire some Part of this Money flowing in upon them. Money thus becomes dispersed into many Hands, enters into every Transaction and Contract, and encourages Labour and Industry  
(Cantillon 1759:39).

This passage also paraphrases, not very well but undeniably, Hume's 'Of Money'.<sup>39</sup> As we saw, the idea of temporary real effects caused by increases in the money supply also occurs in a passage that appears to have been taken from an earlier draft of Richard Cantillon's writings. Here the similarities with Hume, including the view that 'all Things in the Course of Time, would return to their former Situation' (Cantillon 1759: 53), are indeed striking. However, these similarities cannot simply be explained by an effort of Philip to make his cousin's ideas resemble those of Hume more closely.

If the various passages identified here (chiefly pp. 31-3 and 51-4), dealing with the effects of increases in the money supply, were indeed taken from the manuscript of 'a very ingenious Gentleman deceas'd', something which must be considered probable, then one is tempted to sum up Richard Cantillon's early views on this topic as follows. He appears to have started from a more or less traditional position with regards to the effects of increases in the money supply, as can be found in earlier authors such as Jean Bodin, William Potter and John Locke.<sup>40</sup> His depiction of the transmission mechanisms from increases in money to increases in prices was quite rudimentary, there is some consideration of temporary output effects, but the statement of eventual proportionality is rather dogmatic.<sup>41</sup> There was little of the later awareness of the importance of the source or point of entry of the additional money into the circular flow and no careful analysis of relative price effects. In a word, was this how Richard Cantillon thought before he hit upon, well, the 'Cantillon effect'?<sup>42</sup>

What may be significant here as well is that there is hardly any counterpart in the *Analysis* to *Essai* II, chapters iii to v, in which detailed analysis is offered of the determinants of the demands for, and hence velocity of, money (see figure 3). Only one single paragraph indicates some consideration of these topics.<sup>43</sup> Could this mean that the early Cantillon had not yet developed his distinct ideas about

<sup>39</sup> In particular, the following phrase in Hume ([1752] 1955: 37-8) is evidently paraphrased: 'though the high price of commodities be a necessary consequence of the encrease of gold and silver, yet it follows not immediately upon that encrease; but some time is required before the money circulates through the whole state, and makes its effect be felt on all ranks of people'.

<sup>40</sup> On Jean Bodin see O'Brien (2000), on Potter see Viner (1937: 40-5) and on Locke see Eltis (1996).

<sup>41</sup> At a later point in the *Essai* (III, v, 15) Cantillon refers the reader to 'the principles I have established that abundance and scarcity of money in a State raises or lowers all prices proportionably [*à proportion*]' (translation Higgs 1931: 295). This is curious given the fact that in *Essai* part II the proportionality assumption is severely qualified. However, if Cantillon originally did subscribe to this assumption more fully then this statement does not appear so odd, because it may be explained as having survived later revisions by the author.

<sup>42</sup> The actual term 'Cantillon Effect' appears to have been coined by Mark Blaug in the second edition of his *Economic Theory in Retrospect* (1968: 151). It refers to the insight that the effects of increases in the money supply on prices of individual products vary, depending on the source and point of entry into circulation of the additional money. It is worth noting that modern elaborations and perceived implications of the 'Cantillon effect', especially in Austrian economics, require additional theoretical assumptions that are absent from the Irishman's writings. For a good discussion see Hagemann and Trautwein (1998).

<sup>43</sup> This paragraph appears in the short chapter XIV of the *Analysis* (pp. 42-4), entitled 'Of the Circulation of Money'. Whilst it is the only paragraph of that chapter that has a clear counterpart in the *Essai* (II, iii, 20), there is a full counterpart to the French chapter in the *Dictionary* entry 'Cash', except for the final paragraph. The paragraph that has a counterpart in all three texts reads in the *Analysis* version: 'The Payments made by Tenants to Landlords, Farmers to Labourers, and Tradesmen to Farmers for the Produce of the Land, Merchants to Manufacturers, the Proprietors of Lands to Tradesmen and Artists; and lastly, by the Prince to his Courtiers and Pensioners, is what is called Circulation of Money; the oftner these Payments are made, the quicker Money shifts Hands, and goes round; and it is calculated that the Coin which conducts the Circulation of a Kingdom, is about the ninth Part of the Value of the Produce of the Land, and the Value of the Labour necessarily had to form it into Use' (Cantillon, 1759: 43-4). Especially

circulation, especially the varied speeds at which the 'three rents' are circulated within the economy?<sup>44</sup> Clear ideas as to the circulation of money and the nature of the demands for money would appear logically prerequisite to the more sophisticated analysis of the effects of an *increase* in the money supply that is found in both the *Essai* and the *Dictionary*. Admittedly, this impression of the development of the Irishman's ideas with regards to the effects of increases in the supply of money (in a closed economy) is something of a conjecture. Nevertheless one may want to consider the plausibility of a remarkable transformation in the ideas of this brilliant early theorist, which *if made by a modern economist* would be tantamount to moving from a Monetarist to a more Keynesian or Austrian position.<sup>45</sup>

## 4.2 The Price-Specie-Flow Mechanism

Another, but of course related<sup>46</sup>, topic is that of the connections between inflows and outflows of precious metals, due to imbalances in foreign trade, and the general price level and production of a nation. As with the analysis of exogenous changes in the money supply within an essentially closed economy, Hume's theory of the self-adjusting process of the international of specie flows and price levels, has been interpreted as a less sophisticated version of Cantillon's analysis (see Murphy, 2009: 105-6). Again, the discussions of this topic that are found in the *Analysis* throw some fascinating light on the matter.

The principal discussion of this topic in Philip Cantillon's work is found in chapter XV, pages 45-51, which has the title *Of the Ways and Means by which real Species Increase and Decrease in a Kingdom*. There is no clear counterpart for this chapter in the *Essai*, although both the title and subject matter of part II chapter viii of that work show similarities. However, the Postlethwayt version has a fragment (Postlethwayt 1751-5, II: 5, entry Labour) that offers close counterparts to the *Analysis* chapter. In fact, the *Dictionary* fragment is a more extended and sophisticated version of essentially the same argument.<sup>47</sup> The two versions are presented in parallel in the appendix to this paper. Since both English versions have these passages, it is argued that here too we are looking at fragments written by Richard Cantillon, despite the considerable discrepancies with the generally recognised text of the *Essai*.

These, hitherto unnoticed, English versions offer a fascinating description of what can perhaps best be described as a long trade cycle of a country, namely France. Developments are analysed from the moment when, as the result of a civil war, the country suffers a great decline in production, population and the general price level, as well as a substantial outflow of precious metals (see appendix paragraphs 2-4). Once the civil war finishes the country enjoys an extended period of expansion in economic activity, due to the cheap availability of land, frugal living of the rich, coupled with an increasing consumption of a growing population and a great demand for exports (appendix paragraph 5). The latter leads to a

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important is the estimate that the quantity of money in circulation is a ninth of the annual output of the economy. It also appears in the two other versions.

<sup>44</sup> To be sure, the 'three rents' theory does briefly figure in chapter XXI of the *Analysis* (pages 114-8). This chapter occurs out of sequence compared to the *Essai*, and corresponds to part I, chapter xii of the latter work. What is missing from the *Analysis*, however, is the application of the three rents theory to the analysis of the circulation of money, as found in *Essai* II, iii and the *Dictionary* entry Cash (Postlethwayt 1751-5, I, 463-4). Moreover, William Petty, who in the *Essai* and *Dictionary* is quoted prominently as an earlier writer who had tried to estimate the required size of the money stock, is not mentioned anywhere in the *Analysis*.

<sup>45</sup> Of course, Cantillon was neither a monetarist, nor a Keynesian, nor an Austrian and by likening his monetary views to modern theories there is the danger that his writings are given an anachronistic reading, especially where perceived policy recommendations are concerned. Nevertheless, these comparisons are to some extent enlightening. Seeing Cantillon as a clear forerunner of Austrian economics, Thornton (2006: 47-50) argues that the Irishman's main aim in the *Essai* was to demonstrate the non-neutrality of money. Alternatively, Shackle (1982: 772-7), noting non-neutral effects of changes in amongst other things the money supply, points out a number of parallels between the analyses of money of Cantillon and Keynes.

<sup>46</sup> Sekine (1973: 278) observes that historically ideas about tendencies towards international monetary equilibrium arose 'as a direct consequence of the quantity theory of money'. It is only as a consequence of Cantillon's characteristic method of first considering simpler cases ('a Town that has no Communication with its Neighbours') that the two topics are distinct in his work (and that in the simpler case the direction of causation from money to prices is unambiguous).

<sup>47</sup> The Postlethwayt version also exhibits greater similarities with *Essai* II, viii than the *Analysis* version.

persistent inflow of precious metal and subsequently to rising prices. Eventually a kind of turning point is reached: after it arrives at its 'highest Point of Power and Riches' (paragraph 7, Cantillon, 1759: 48)<sup>48</sup> the balance of trade turns against the country and a net outflow of precious metals will commence.

It is worth noting that both English versions offer a number of explanations for the reversal in the balance of trade (see paragraph 7). Both mention the price effect, *i.e.* the argument that foreign countries will start producing their own manufactures when the prices of French manufactures increase as a result of the inflow of money into the latter country. Both also note a kind of cash balance effect, namely the argument that merchants, who have amassed wealth and landlords who have higher incomes, will consume more imports and once a taste for the consumption of foreign luxuries is established it is not easily changed. An additional argument which only figures in the *Analysis* is the suggestion that imports increase once a ceiling to agricultural production capacity is reached: 'the Increase of Coin would increase a Demand for more than the Land could produce; and consequently this Demand must be supplied by foreign Produce'. Also only in the *Analysis* the doubtful observation is made that, as a result of these effects, 'in a Course of Years the Nation would be drained of all its Coin' (paragraph 7, Cantillon, 1759: 48).<sup>49</sup>

An important difference between the two English versions is that in the *Analysis* the description of the trade cycle is presented as a hypothetical case; the reader is invited to imagine that a civil war broke out in France 'in the Year 1740' (paragraph 2; Cantillon 1759: 45). It seems most likely that Philip, rather than Richard, Cantillon came up with this date.<sup>50</sup> In contrast, the Postlethwayt text explicitly presents the case as a historical example; the civil war mentioned is in fact the Fronde, which ended in 1652, and the high point of economic might is said to be in the early 1680s, after which a gradual decline set in until about 1715. This allows one to say that the length of the cycle from trough to trough is in this account supposed to be about 60 years with a peak roughly in the middle (paragraphs 8 and 9). This periodisation is similar to that found in the *Essai*.<sup>51</sup>

Another difference is the clear manner in which the Postlethwayt text states that the historical example of France is merely an instance of a *general* process:

The rise and decline of all other kingdoms, naturally and abstractedly from wars and conquests, are owing to causes of the like nature; and, when a nation gets a great plenty of money, and increases exorbitantly in it's [*sic*] paper circulation, it naturally tends to decline, by the dearness that happens of land, labour, and commodities [...] where things go on in their natural course, the too great plenty of money, or paper credit, by enhancing the price of things, gives other rival nations an opportunity to take the trade into their hands, and to get the money along with it

(paragraph 10; Postlethwayt 1751-5, II, 5).

In the Postlethwayt version it is argued that policies of 'a legislator' can only hasten or slow down this 'natural course' of the fortunes of a nation. Poor policies such as the expulsion of the Huguenots had hastened the decline of France (paragraph 9), though it would have occurred anyway; good policies, such as reducing the velocity of money circulation and removing precious metals from circulation when the

<sup>48</sup> In the Postlethwayt version the high point is called the moment France is 'in its acmé'. This term expresses the interesting notion of an achievement that carries within it the seeds of its own destruction.

<sup>49</sup> Recall that Hume ([1752] 1955: 61) specifically rejects the view, which he ascribes to Joshua Gee, that this would be possible.

<sup>50</sup> In addition to his wish to bring his cousin's writings, literally, up to date, one can imagine that at a time when anti-French sentiments were running high, due to the seven-year war, Philip Cantillon would have thought it a pleasing exercise for his British readers to imagine the enemy experiencing new civil strife.

<sup>51</sup> In *Essai* II, viii, 7 it is stated that the power of France has been on the increase since 1646, when manufactures of cloths were set up and that it reached its peak in 1684, when the Huguenots were expelled. No clear date is given for the next trough. From this one could reasonably conclude that the period from trough to trough in the *Essai* is closer to 80 years (see Thornton, 2006: 52-3). Nevertheless the turning point in the mid-1680s is similar to the Postlethwayt version.

competiveness of the nation was affected by inflation, could merely slow down, not stop, the decline (paragraph 10).<sup>52</sup>

Interestingly, the last date in the historical account in the Postlethwayt version is 1715, just before the ascendancy of John Law. In keeping with the foregoing analysis one would not have been surprised to find an advocacy at this juncture of a policy of extending the money supply and paper credit, because, as the French version has it, '*un habile Ministre est toujours en état de lui [i.e. the economy] faire recommencer ce cercle*' (*Essai* II, viii, 16). However, the historical account in the Postlethwayt version stops just at the beginning of Law's System.<sup>53</sup>

The *Analysis* version, on the other hand, does mention the bubble year 1720, but the authorship of the passage is questionable because halfway through the paragraph (see paragraph 10), Philip Cantillon starts paraphrasing Hume's 'Of Public Credit' about the potentially disastrous effects of a large public debt.<sup>54</sup> This topic does not follow easily from the earlier discussion and it appears that Philip failed to see that the discourse of Hume of which the subject matter is much more closely related is 'Of the Balance of Trade'.

In that discourse one passage in particular reminds of the sequence of events that in the *Analysis* version commences at the moment the civil war subsides, although in Hume the depletion of the money stock is instead the result of an unexplained event:

Suppose four-fifths of all the money in Great Britain to be annihilated in one night, and the nation reduced to the same condition, with regard to specie, as in the reigns of the Harrys and Edwards, what would be the consequence? Must not the price of all labour and commodities sink in proportion, and every thing be sold as cheap as they were in those ages? What nation could then dispute with us in any foreign market, or pretend to navigate or to sell manufactures at the same price, which to us would afford sufficient profit? In how little time, therefore must this bring back the money which we had lost and raise us to the level of all the neighbouring nations? Where, after we have arrived, we immediately lose the advantage of the cheapness of labour and commodities; and the farther flowing in of money is stopped by our fullness and repletion

(Hume [1752] 1955: 62-3).

Despite the similarities of this passage with Cantillon (1759: 46-8; paras 5 to 7 in the appendix) it has to be said that Hume appears to envisage a much faster working balancing effect of specie flows on prices and vice-versa. In addition, as various commentators have pointed out, Hume's analysis is very largely limited to relative price effects only (see e.g. Sekine, 1973: 278-83; Rothbard, 1995: 427; Murphy, 2009: 105-8). As was already noted, in the *Analysis* other effects, such as the cash balance effect are used as additional explanations of why the inflow of specie would lead to a deteriorating balance of trade.

However, in the *Essai* these effects are elaborated in more detail. In particular, there is a fairly clear understanding in that French publication that the transmission effect of specie inflow on prices is not so straightforward, due to the 'law of one price' of internationally traded goods. Hence additional effects,

<sup>52</sup> According to Thornton (2006: 53) in the *Essai* state policies are not merely additional factors that may retard or hasten the 'natural course' but actually the root causes responsible for the cycle. If correct, this would suggest a substantial difference with the Postlethwayt version. In fact, however, in the *Essai* too phrases are used like 'the ordinary course of things' [*le cours ordinaire des choses*] to describe the cycle and that the downturn occurs '*insensiblement, mais naturellement*' (*Essai* II, viii, 5). Such phrases suggest that the cycle is a phenomenon inherent to the economies of trading nations. Therefore, perhaps the difference between the two versions on this point is mostly one of emphasis, and the interpretation that in the *Essai* state interventions cause the cycle too Austrian a reading.

<sup>53</sup> However, elsewhere in this version, as in the French text, the use of paper money and other credit-based instruments are discussed in favourable terms provided they are well managed.

<sup>54</sup> The paraphrases are on the second half of page 49 and almost the whole of p. 50 (not included in the appendix). These passages correspond to the content of Hume ([1752] 1955: 104-6).

such as the cash balance effect would seem to take on a greater importance. The chapter in which this argument is made, *Essai* II, vii, has no counterpart whatsoever in the *Analysis*. Perhaps this indicates that Cantillon added this chapter only in a later draft of his work.<sup>55</sup> This interpretation also implies that the Irish banker substantially rewrote *Essai* II, viii. However, perhaps this speculation requires further investigation before it can be accepted.

### 4.3 The Determination of the Interest Rate

The final topic of monetary theory is the phenomenon of interest payments. In the *Essai* the last two chapters of part II deal with this issue. To be precise, chapter ix introduces the nature of interest and 'its causes' and chapter x discusses the causes of changes in the rate of interest in a nation. In the *Analysis* the same topics are discussed in a single, short chapter, number XVII, entitled *Of the Interest of Money* (pp. 62-68). To be exact, pages 62-5 cover matters similar to *Essai* II, ix and pages 66-7 resemble arguments found in *Essai* II, x.<sup>56</sup> As can be seen from figure 1, the actual concordance between the *Analysis* chapter and what we find in the *Essai* is very limited. Again, however, the correspondence with the Postlethwayt text is more extensive (see figure 2), suggesting that we are not simply looking at a concoction by Philip Cantillon.

Generally speaking, most of the arguments made in the *Analysis* chapter are also found in the two other versions. For instance, a clear link is established in all three texts between the rates of profit and interest. The link is established in a sequence whereby first the profits are considered of an 'undertaker' who provides his own money capital and second, the interest paid by a new entrepreneur who has to borrow to set up his business<sup>57</sup>:

**Table 2**

<i>Analysis</i> p. 63-4	<i>Essai</i> II, ix, 6 and 7	<i>Dictionary</i> 1, 996
But Merchants and Undertakers in Manufacture, whom I consider in my Supposition as the Proprietors of Money, acquire Subsistence from its Income, by employing it to the Use of the Labourer	D'un autre côté, un maître Chapelier, qui a du fond pour conduire sa Manufacture de chapeaux soit pour louer une maison, acheter des castors, des laines, de la teinture, &c., soit pour paier toutes les semaines, la subsistance de ses Ouvriers, doit non-seulement trouver son entretien dans cette entreprise, mais encore un profit semblable à celui du Fermier, qui a la	A master hatter, who sets up for an undertaker, hires a workhouse of a proprietor for his work, buys wool, poil de castor, &c. buys utensils and instruments fit for the work, hires several journeymen for daily wages, and makes all advances of money necessary in that business: as he corresponds to the overseer of slaves, and is the master and inspector of his

<sup>55</sup> As can be seen from figure 3, in the *Dictionary* there is no counterpart to *Essai* II, vii either. This poses a problem for my interpretation that the manuscript that formed the basis for the Postlethwayt version may be of a later date than the one from which the *Essai* derived. Cf. notes 37 and 61. One possible explanation for the absence of this, and other, *Essai* chapters from the *Dictionary* could simply be that Postlethwayt decided not to use them. But that is perhaps too convenient an explanation in this case. Cf. van den Berg (2011).

<sup>56</sup> The bottom of p. 67 and p. 68 until the end of the chapter mainly consist of a quotation from Locke that has nothing to do with the foregoing discussion.

<sup>57</sup> The paragraphs in the *Essai* and *Dictionary* that immediately precede the ones cited in table 2 show significant differences between each other. These variations in particular raise the issue whether Cantillon was groping towards a conception of profit as a return on capital advanced. That conception is certainly absent from the *Analysis* and is for that reason not discussed further here. See instead van den Berg (2012).

and Artizan: this Profit is proportioned to the Demand the Public has to support or supply the Taste and Luxury of the Proprietors of Land, and the Quantity of Money in several Hands to be advanced. A young Tradesman who has not wherewith to establish himself, is obliged to act as a Journeyman for Wages with some Master, Artist, or Manufacturer, the more Money the Master has to buy his Materials to supply the Calls of the Public, the more Men he is enabled to employ; by which he has the better Chance of enriching himself and Family, by his Care and Industry. [...]\*

troisieme partie pour lui. Cet entretien, de même que ce profit, doit se trouver dans la vente des chapeaux, dont le prix doit païer non-seulement les matériaux, mais aussi l'entretien du Chapelier & ses Ouvriers, & encore le profit en question.

Mais un Compagnon Chapelier entendu, mais sans fond, peut entreprendre la même Manufacture, en empruntant de l'argent & des matériaux, & en abandonnant l'article du profit à quiconque voudra lui prêter de l'argent, ou à quiconque voudra lui confier du castor, de la laine, &c., qu'il ne paiera qu'à long terme & lorsqu'il aura vendu ses chapeaux. Si à l'expiration du terme de ses billets le Prêteur d'argent redemande son capital, ou si le Marchand de laine & les autres Prêteurs ne veulent plus s'y fier, il faut qu'il quitte son entreprise; auquel cas il aimera peut-être mieux faire banqueroute. Mais s'il est sage & industriel, il pourra faire voir à ses créanciers qu'il a en argent ou en chapeaux la valeur du fond qu'il a emprunté à-peu-près, & ils aimeront mieux probablement continue à s'y fier & se contenter, pour le présent, de leur intérêt ou du profit. Au moïen dequoi il continuera, & peut-être amassera-t'il par degrés quelque fond en se frustrant un peu de son nécessaire. Avec ce secours il aura tous les ans moins à emprunter, & lorsqu'il aura amassé un fond suffisant pour conduire sa Manufacture qui sera toujours proportionnée au débit qu'il en a, l'article du profit demeurera en entier, & il s'enrichira s'il n'augmente pas sa dépense.

journeymen; as he lays out his money at an uncertainty, and runs the hazard of losing it; he must get, in the price of his hats, a profit proportionable to his risque, expence, and situation; and so he commonly does, lives pretty well, and maintains a family, and breeds up children; and, if he computes what his advances of money amount to, and what money he has by his profits, which he spends in his family, he will find that he has made 30 or 40 per cent. of his money; he will have sold 10,000 hats, or more, in a year, to a haberdasher of hats, who has paid him money for them, which reimburses his advance, and leaves the said profit, and enables him to go on with his employment, and advance for the ensuing year. [...]\*

Now, if any one who has saved a sum of money, offers to lend it to a journeyman hatter, who earns but his small daily wages, by which the said journeyman may be enabled to set up for a master hatter, and turn undertaker, he would gladly promise him a share of his profits; for, though he would not to clear so much as the master hatter above mentioned, who had money of his own to set up with, yet it would mend his condition to be an undertaker; and a little experience would determine how much this journeyman, now master hatter, might well allow out of his profits to the person who lends the money, and enables him to set up; and his share of profit would be proportionable to the sum lent, and be called INTEREST of the money.

\* Both English versions have an intervening paragraph

There are some variations between these paragraphs in the three versions that are more noteworthy than the fact that the *Analysis* talks about 'Undertakers' in general while the other two use the example of entrepreneurs in the hat trade. First, in the *Essai* version the point is made, somewhat casually, that there is no essential difference between having capital advanced in the form of money, borrowed at interest, or as goods supplied at a higher price, payable '*à long terme*'. In the two English versions, on the other hand, this point is made at greater length in the subsequent paragraphs, where it is asserted that the latter kind of lending, *i.e.* commercial credit, is the 'source and original cause of interest in a state'.<sup>58</sup>

In the *Analysis* in particular, the theoretical importance given to commercial credit appears to be connected to a more prominent statement of the view that 'the several Profits made from Hand to Hand, down to the Consumer, must be paid by the Landed Gentry, who are the great Consumers of rich Manufactures' (Cantillon 1759: 63): entrepreneurs can only pay an advanced price for the materials obtained 'at Time and Credit' because they are able to charge their final consumers even higher prices and hence still make a profit.<sup>59</sup>

A second, perhaps minor, variation between the three passages in table 2 is that only in the *Analysis* the person from whom the 'young Undertaker' borrows is explicitly identified as 'a Wholesale Dealer [who] finds himself in good Circumstances, [and who is] naturally [...] inclined to Ease and Retirement' (Cantillon 1759:64). The reason to note this is that it is reminiscent of Hume's argument in 'Of Interest' that it are specifically older merchants who have made their fortune, who often "switch sides" and become lenders to newer entrants into trade: 'Where merchants posses great stocks, [...], when they either become tired of business, or leave heirs unwilling or unfit to engage in commerce, a great proportion of these riches naturally seeks an annual and secure revenue' (Hume, [1752] 1955: 54). However, this striking similarity between 'Of Interest' and the *Analysis* in identifying lenders as established rich merchants might well simply reflect the reality of the times, rather than a direct influence.

A third and most significant variation is the statement, missing from the *Essai* and *Dictionary* versions, that 'Profit is proportioned to the Demand the Public has to support or supply the Taste and Luxury of the Proprietors of Land, and the Quantity of Money in several Hands to be advanced' (Cantillon 1759: 63; emphasis added). The formulation here is also found in a discussion earlier in the *Analysis* of

<sup>58</sup> These subsequent paragraphs, absent from the *Essai*, are the following (note the greater analytical clarity in the *Dictionary* version):

*Analysis* 64-5:

|| If this young Adventurer by his Assiduity and Industry can find a quick Sale for his Goods, he will be encouraged to increase his Undertakings; and for the Purchase of Materials he will apply to borrow more Money, which if he cannot find Lenders at Market to supply him with, he will be obliged to purchase the Materials of his Business at Time and Credit, and give an advanced Price for Want of Money to pay down.

|| This is, I apprehend, the Source of the Interest of Money, and is determined, as to its Quantum per Cent. by the several Adventurers in Commerce, who, from the Nature of their Business, judge how much of their Profits they can afford to divide with the Proprietors of these precious Metals.

*Dictionary* I, 996:

||If this new master hatter, by his skill, industry, and assiduity, works himself into good business, and has many customers, he will be able to increase and augment his undertakings; he will borrow more money to carry them on, out of which he will give a share of profit, or an interest; or, if he can buy wool and other materials, payable at a long term, he will give a higher price for them than the current, which is, in effect, to give a share of his profit, or an interest.

|| This seems to be the source and original cause of interest in a state. The wool merchant, gets an interest for the price of his wool from the hatter, in the term he gives him for payment; he himself borrows money, at a smaller interest, from some richer undertaker, and takes also time for payment; and this undertaker again gives a smaller interest to the monied-man, who commonly lends it to the most solvable and considerable undertakers.

<sup>59</sup> On pages 66-7 the conclusion is drawn that a high rate of interest in a country is due to the great luxury consumption of 'Gentlemen of Estates' and the fact that some of them end up mortgaging their properties when they want to continue to 'live above their Incomes'. Similar arguments are found in *Essai* II, x, 5 and in *Dictionary* I, 996.



the way prices of goods are determined 'by the Quantity of Money at Market' relative to the quantities of goods offered (pp.28-30; cf. section 4.1 above).<sup>60</sup> Reading these passages together one is tempted to conclude that in the *Analysis* the following causal link is made between the quantity of money in circulation and the level of profit: market prices are the result of the confrontation between quantities of goods supplied and the demand (money available and consumer credit) for them; hence the level of market prices determines profit margins received by entrepreneurs (a kind of 'profit upon alienation'). What is remarkable about this argument is that in the *Analysis* it is subsequently extended to serve as an explanation for the rate interest. This can be seen in the following passage where the rate of interest is explicitly related to the quantity of money in circulation:

Land is lett to Farmers, to spare the Proprietors thereof the Trouble of cultivating it; and the Proprietor of Money disposes thereof at *Interest*, to avoid the Risk and Trouble annexed to Trade; and its Income fixes itself, by the Demand the public has for Labour and Industry, and *is proportioned by the Quantity of Specie in Circulation*: In a Nation where there is no Trade or Industry, the greater or lesser Quantity of Money is of no Use; it is the hard Hand of Labour and Industry which gives it a Value, and the Agreement and Convention of the Society has fixed its Standard as a Measure in Exchange and Barter for the Produce of Land and Labour

(Cantillon, 1759: 65-6; emphases added).

What is significant about this *Analysis* passage is that it suggests a very different explanation of how the interest rate is determined than the other two versions. Admittedly, the idea expressed in the first sentence, *i.e.* that the proprietor of money, like the proprietor of land, farms out his assets 'to avoid the Risk and Trouble' of employing it himself, can also be found in the *Dictionary* and the *Essai*.<sup>61</sup> However, the subsequent statement that the rate at which money is lent 'fixes itself, by the Demand the public has for Labour and Industry, and is proportioned by the Quantity of Specie in Circulation' is contradicted in the other two versions.

As is well known, the Cantillon of the *Essai* explicitly rejects the 'common idea, received of all those who have written on trade', that there would be a 'necessary connection' between the 'plenty or scarcity of money in a State' and 'the rate of interest' (*Essai* II, x,1 and 3; Higgs translation 213-4). Instead, he presents the novel theory that the rate of interest is 'settled by the proportionate number of Lenders and Borrowers'.<sup>62</sup> This theory that the rate of interest is determined by the supply of and demand for 'loanable funds' was seminal. More precise variants would later be developed not only by Hume, but also by Turgot and Smith and would generally find wide acceptance in classical political economy.

But is it possible that Richard Cantillon initially accepted something like the 'commonly received' view, only to develop his own theory in the process of writing more advanced versions of his work? In the *Essai* and the *Dictionary* instead of a statement directly linking the quantity of money in circulation and the interest rate one finds the opinion that '[t]hough money passes for a pledge in exchange it does not

<sup>60</sup> The conclusion of that earlier discussion has a very similar wording: 'From what is here said, may be perceived the Reasons why the Prices of Goods daily vary at Market, where the Quantity of Money, and the Necessity of exchanging it against a certain Quantity of Goods, bring about these Alterations. The Demand of the Public to supply the Taste, Luxury and Manner of living of the Rich is that which occasions these Alterations [...]' (Cantillon, 1759:30).

<sup>61</sup> In the *Dictionary* there is the following statement, out of sequence with the *Analysis* text: 'As the proprietor of land sets and farms out his land, so the proprietor of money farms out his money, to avoid the trouble of managing it himself, and turning undertaker' (Postlethwayt, 1751-5, I, 996). In the *Essai* there is not such a clear statement, but the connection between the returns on renting out land and lending money is made in a number of places (II, ix, 3-4, x, 14).

<sup>62</sup> In the *Dictionary* the same views are found: 'the greater or lesser quantity of money is not the essential cause of the fall or rise of interest, according to the notion commonly received'; instead 'in [the] several channels of loans upon interest the price always rises and falls in proportion to the number of lenders and borrowers' (Postlethwayt, 1751-5, I, 996-7).

multiply itself or beget an interest in simple circulation' (*Essai* II, ix, 2; translation Higgs 1931:199).<sup>63</sup> From this one is tempted to conclude that, upon further reflection, Cantillon started to appreciate more fully the importance of loanable funds, instead of simply the amount of money in circulation, for the determination of interest rates.

## 5. Conclusion

When W. Stanley Jevons unearthed the *Essai sur la nature du Commerce en général* after a century of neglect, he marvelled at the remarkable completeness of his find: 'It is a systematic and connected treatise, going over in a concise manner nearly the whole field of economics, with the exception of taxation. It is thus, more than any other books I know, *the first treatise on economics*' (Jevons [1881] 1931: 342; emphasis in original). The systematic nature and high analytical quality of the *Essai*, which commentators have attested to ever since,<sup>64</sup> is indeed one of many mysteries that have always surrounded Richard Cantillon. How did this adventurous banker and speculator, this man of practice, arrive at his highly sophisticated, wide-ranging and largely coherent economic theories?

The fact that the *Essai* appeared to have been Cantillon's sole surviving theoretical economic work has made it difficult to address questions of the possible development of his ideas. Of course, the validity of the interpretations offered here relies to a large extent on a revisionist reading of Philip Cantillon's *The Analysis of Trade*. While this work has often been dismissed as an inferior product compared to the *Essai* of 1755, it has been argued here that the reason for this inferiority has not been correctly understood. It is particularly the comparison with fragments of Postlethwayt's *Universal Dictionary* which suggests that parts of the *Analysis* that differ from the *Essai* were indeed, as Philip claimed, 'taken chiefly from a Manuscript' authored by his cousin.

How can the evolution of Richard Cantillon's monetary theories be summed up? Generally speaking, the monetary views in the *Analysis* are not only more rudimentary compared to those found in the *Essai*, they are also closer to the commonly received opinions of the day. The changes in and further specifications of his monetary ideas give us an impression of the intellectual struggle Cantillon faced to emancipate himself from the views of (named) authorities like John Locke and (unnamed) adversaries like John Law. In the process Cantillon went far beyond the monetary views of any of his contemporaries in terms of coherence and analytical precision.

Of course David Hume made similar advances twenty years later. As we saw some of the Scotsman's views appear to show a greater resemblance with the 'early' ideas as found in the *Analysis*. As discussed in Section 2, the possibility that the Scotsman knew the *Essai* has been seriously considered by a number of commentators. By this they have meant a manuscript version of the French text that was subsequently published in 1755. But there is no strong reason why, *if* Hume knew a manuscript, this could not have been a draft that was more similar to the one on which Philip Cantillon was to base his *Analysis of Trade*. In favour of this possibility is that some notions in the *Analysis*, in particular not only the 'short term' but also the 'long term' effects of an increase in the money supply on prices and output, are strikingly similar to ones found in the *Political Discourses* (see Section 4.1).

<sup>63</sup> 'Quoique l'argent passe pour gages dans le troc, cependant il ne se multiplie point, & ne produit point un intérêt dans la simple circulation' (Cantillon 1755, II, ix, 2). The corresponding sentence in the Postlethwayt text reads: 'It does not appear that money begets an interest by passing for a pledge in barter, nor that its exchange for other commodities produces a great quantity of it in a state'.

<sup>64</sup> To give but a few examples, Schumpeter (1954: 217, n.4; 223) called the *Essai* a 'great work', that in some places was 'a brilliant performance'; Shackle (1982: 779) called it an 'imperishable master piece'; Blaug (1996b: 21) deemed it 'the most systematic, the most lucid, and at the same time the most original of all the statements of economic principles before the *Wealth of Nations*'; finally, the editors of the recent new English translation go so far as to call the French work 'one of the most important books ever published' (Saucier and Thornton, 2010:13).

However, with regards to the other topics considered, the price-specie flow mechanism and the determination of the interest rate (Sections 4.2. and 4.3) the similarities are often less striking. More importantly, in order for this possibility to be more than a fascinating speculation, one would want to see some external evidence. There does not appear to be any.

The more likely explanation is that the similarities between Hume's monetary theories and the views found in both the *Analysis* and the *Essai* are coincidental and probably due to the fact that both men developed their views in response to a similar body of received economic ideas. The extent to which some of Hume's ideas are closer to the *Analysis* is then merely a further indication of how the Irish banker eventually went beyond the elegantly stated but analytically often less sophisticated views of the Scottish philosopher. As long as no original manuscripts are discovered one cannot be certain about the intellectual process by which he arrived at his highly original monetary ideas. But, as I hope to have shown, a careful use of the different versions of his writings that were published in the 1750s can help us make some plausible conjectures.

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## Appendix

### *The Analysis of Trade (1759)*

[45] CHAP. XV. *Of the Ways and Means by which real Species Increase and Decrease in a Kingdom*

1 In all trading States where there are not Mines of Gold, or Silver, the only natural Way of acquiring these precious Metals, is by foreign Commerce; and this by a very few Words is explained, by the Acquisition of the surplus Value of our Exports, more than that of our Imports; this surplus Value must be paid us in Bullion, which brings what is called the general Ballance of Trade on our Side; and it is this Ballance which increases our real Specie, and consequently if this Ballance is against us, that is to say, if the Amount of what we import, exceeds that of what we export, the Excess must be paid in Bullion, and consequently will decrease our real Specie.

2 In order to illustrate this Matter, let us suppose, *France* in the State in which it was in the Year 1740, the Land tollerably well tilled, Rents well paid, but that the Calamity of a civil War broke out: the great Proprietors of Land would take some one Side, some [46] the other; all Ways and Means to raise Money by Mortgage and Credit would be practised, in order to support their respective Parties; the Land in general would be uncultivated; Manufacturers and Undertakers in Trade would be disconcerted and at a loss what to do, consequently very cautious how they engaged in Business; Countries, Towns, Cities, and Villages, would be pillaged and plundered. The Farmer could not bring to

### *The Universal Dictionary (1753)*

[Entry 'Labour' II, 5] *Of the natural causes of the rise and decay of nations in wealth and power, with regard to the price of labour*

|| Let us suppose *France* in a middling state, the land pretty well cultivated, and the proprietors rents pretty well paid; if in these circumstances there happens a civil war, the proprietors will take party, some on one side and some on the other; they will engage and mortgage their estates, to lend money to the chief of their faction to support his quarrel, since, if the opposite chief prevails, their lands and estates will be confiscated, the undertakers will be disheartened, the country rifled, the magazines and warehouses plundered, and labour will be discouraged; so the land will not produce wherewithal to maintain the inhabitants, and to supply necessaries for the armies. The chiefs of the parties will be obliged to get stores and other

Market the Produce of his Land; in this unhappy Situation of Affairs, a Necessity must arise to import from foreign Countries the Materials necessary to support this domestic Contention; Money must be had for this Purpose, which would carry away the current Coin of the Country,

necessaries from the neighbouring states which are in peace, and consequently they will send money out of France to pay for them.

- 3 and consequently occasion a great Scarcity; many of the Inhabitants would quit the Country,

||This will gradually create a scarcity of money in France; besides that great sums will be buried, and that all barter in evaluation and credit will be diminished, the uncertainty of the event of war will hinder marriages and multiplication, and the mortality in the war will diminish the inhabitants.

- 4 the general Face of all Things would wear the Complexion of Blood, Confusion, Poverty, and Want; and the Country would be in Danger of being invaded by some of its ambitious Neighbours.

||In this situation France will be in a deplorable condition, and in danger of being oppressed by a foreign power. A general plague in France will occasion much the same mischiefs.

- 5 || Now let us suppose this Plague of civil Contention happily composed; the Consequence would naturally be, that the Landholders receiving little or no Money from [47] their Lands, would be necessitated to lett them at low Rents; and Money being scarce, the Land's Produce would be cheap, the Landlords would be obliged to live in a proportionable Compass; few or no foreign Goods would be imported, being too dear, with respect to the Poverty of the Country, where its own native Produce would be so cheap as to encourage foreign Nations, to come and purchase them: this would bring in a Flow of Money; the Cheapness of Rents to the Farmers would produce Plenty; the Country People finding easy Means of subsisting would be induced to marry, and bring up a Stock of Children; valuable Manufactures would be established; the Cheapness of Merchandize would introduce a large Demand for foreign Exportation, all Things being in Peace and Tranquillity, Money would insensibly increase in Circulation, and get into the Channels of Trade; the Inhabitants would become numerous, the Prices of all Things would, in proportion to the Increase of Money, insensibly augmt;

||Now let us suppose the civil war ended, the proprietors who received little or no rent during the troubles, and whose lands lay waste and uncultivated, will now farm them out at a small rent, as well because of the scarcity of money, which makes all commodities cheap, as because they must encourage the farmers, in regard to the decrease of the inhabitants. As the rents are small, they will live without luxury, and consume little or no foreign commodities which will be dear, since more money circulates on this hypothesis in the neighbouring states, than in France. The labourers and peasants, by reason of the thinness of the inhabitants, will be encouraged, and, as they will consequently find it easy to subsist, they will breed up a great number of children, and so France will become again very populous. The scarcity of money in France will make their commodities so cheap, that they will export great quantities of them, particularly if valuable manufactures are set up in France. So that France will in this case get a yearly ballance, and fall naturally into the channels of trade. This will gradually bring great sums of money into France, where it's plenty will begin to raise the price of all things, and where several undertakers will have amassed good sums of money.

- 6 Merchants and Tradesmen would by their Savings grow rich; the Landholders would increase their Rents, Luxury and Profusion would soon take the Place of Oeconomy and Industry; the landed Gentry, [48] who before lived with great Care and Parsimony, would now keep great Tables well covered with Delicacies; Merchants and Traders becoming rich, would launch out into Expencc, and would consume more of the Land's Produce than before.
- || Now, since the prices of all things are risen insensibly, the proprietors will raise the price of their estates, the increase of the inhabitants will make them offer to work for less sustenance than at first; and, as there is plenty of money in circulation, foreign commodities will come at a cheap price, the exportation of commodities will slacken because of their dearness, and the neighbouring nations will be able to set up cheaper manufactures; and, as the business decreases in France, several French tradesmen will go into foreign parts, where there is work for them, and improve the manufactures there. The quantities of money amassed by the French undertakers in the course of their business, while France gained the ballance of trade, will encourage to spend more money, and consume more foreign commodities, than usual, as they are now cheaper; and the proprietors, with their additional rent, will do the same, and so luxury will come into fashion.
- 7 And in this Situation of Luxury and Expencc, *France* would be looked upon by its Neighbours in the highest Point of Power and Riches; and the Country possessing more Money than its Neighbours, every thing would proportionably be dearer; Foreigners would cease buying, and would attempt supplying themselves by their own Manufactures; the Channels of Commerce would be turned into different Courses; the Increase of Coin would increase a Demand for more than the Land could produce; and consequently this Demand must be supplied by foreign Produce, which would carry the Ballance of Trade against *France*; Manufacturers, Artizans and Merchants would quit the Kingdom, and in a Course of Years the Nation would be drained of all its Coin. Luxury is seldom introduced into a Country or Community, but when it is believed that it can always be supported; but once begun, People generally ruin themselves before they are prevailed upon to quit it.
- In the beginning of this turn of affairs, the balance of trade will be pretty equal, France being not yet quite beaten out of the channels of trade, but only beginning to lose some branches of it. In this situation France is in it's acmé, or highest state of power, having more ready money than the neighbouring states, and consequently the king can raise greater sums from his subjects than at any time. But, as the increase of expence and luxury has taken root, 'tis remarkable those who begin it seldom lay it aside till they are undone; this will cause a continuance of the expence of foreign commodities, and, the exportation slackening and decreasing in proportion, the ballance of trade will turn against France, and their money will be sent out annually in payment of the surplus of those foreign commodities they consume: and thus France will decline in it's wealth and power, by the decrease of the quantity of actual money, and the thinning of it's inhabitants, which it's luxury and decay of trade will necessarily occasion.



- 8 || This example of France is historical. After the composing of the civil war there about 1652, the prime minister of the finances, Mr Colbert, set up and encouraged fine manufactures there, and France lived several years without luxury, and few coaches were seen at Paris.
- 9 || They gained greatly in the balance of trade, and Lewis the XIVth grew very powerful: money grew very plenty, and about 1680, the ballance of trade grew pretty equal, and luxury began; and then it would have naturally turned against France, which must have necessarily declined in process of time, if that operation had not been hastened by the expulsion of the Hugonots [sic], which, by the money and inhabitants sent out of the kingdom, hastened the decline of France; which nevertheless did not happen visibly, till about the year 1715, when she was in peace.
- 10 [49] || By these Explanations, the Causes which enrich and impoverish trading Nations are easily traced. Kingdoms after public Calamities, provided they are but tolerably well governed and protected, grow rapidly wealthy; those who survive the Calamities of their Country become wise sober and industrious. Had the Calamities introduced by the Schemes of the Year 1720 been wisely applied, and Labour and Industry eased of its oppressive Taxes, the Community in general had long since been restored to its Senses, from the Madness and Folly which the Schemes of that Year possessed us with, and still seem to govern us by; and it had not been verified what was then introduced (to our Scandal and Reproach) as a Maxim, that every Man in Great Britain was to be purchased for his Price. The learned Mr. Hume observes, that Mankind are such Dupes, that notwithstanding any violent Shock to the Community, yet it would not be long before Credit would again revive; and though Men are commonly governed by what they have seen more than by what they foresee; yet Promises, fair Appearances with the Allurements of Gain, are powerful Temptations which few are able to resist. || The rise and decline of all other kingdoms, naturally and abstractedly from wars and conquests, are owing to causes of the like nature; and, when a nation gets a great plenty of money, and increases exorbitantly in it's paper circulation, it naturally tends to decline, by the dearness that happens of land, labour, and commodities; and the greatest prudence of a legislator seems to be, when money and paper circulation are rising to that plenty, to take methods to clog their circulation, and, if possible, to lock up great sums of money gradually and insensibly, to encourage the use of plate, and take any other methods than those that naturally and commonly happen, which is to send it again to foreign parts, in payment of jewels, pictures and other ornaments of luxury. The next [6] essential thing to be done, also, is the gradual annihilation of their paper debts, and the taxes thereby occasioned. If this could be effected, a state would continue, by a reasonable price of it's commodities, to keep up the channels of trade and exportation; but, where things go on in their natural course, the too great plenty of money, or paper credit, by enhancing the price of things, gives other rival nations an opportunity to take the trade into their hands, and to get the money along with it.

# Different Approaches to the Financial Crisis

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## Abstract

The economic crisis has exposed shortcomings in standard economic theory and provided an impetus for new economic thinking. But the theoretical debate in the wake of the crisis has been unduly constrained by the terms of the mainstream approach to economic theory. Like any approach, it is characterised by a way of framing reality, giving meaning to terms and setting criteria for good argument. It also determines how any economic theory is understood, whether from the history of economic thought or from the contemporary literature. But there are other approaches to economics which would open up the field to a much wider range of possibilities for new economic thinking. Addressing the challenge that any reader bases her understanding on her own approach, the purpose of this paper is to attempt to explain what it means to consider different approaches and why it matters for policy. This is done by discussing two features of the financial crisis which pose particular problems for economic theory. These are the role of changing market sentiment in driving asset prices on the one hand and the breakdown of trust relationships in banking on the other (the moral hazard issue). We will see how these are addressed by mainstream theory and by alternative approaches. First, market sentiment is discussed within the mainstream rational-optimising framework, where risk is quantifiable, and compared with the Keynesian approach based on the general uncertainty of knowledge, where reason, evidence and sentiment are integrated. The moral hazard issue is then discussed in its mainstream form in terms of rational opportunism and in its institutionalist form in terms of the foundation of social relations (including relations between institutions) in trust. It is shown that different ways of approaching theorising in each case imply different policy measures. It is argued further that an exclusively deductive mathematical approach to analysis of market sentiment and trust is unduly limiting and that a more pluralist approach would more fully address the issues.

**Keywords:** methodological approach; market sentiment; trust; banking

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## Introduction

The financial crisis which began in 2007 has sparked an unusual degree of reflection on the state of economics. For many economists, it was challenging to explain the crisis. Mainstream theory had been founded on the presumption of underlying stabilising tendencies in competitive markets. But the authorities were required to act without waiting until the dust had settled in academic economics. In the process reference was made to theories outside the mainstream which addressed problems arising from markets not equilibrating at the full employment level and to figures from the history of economic thought. Thus Keynes was invoked in support of expansionary fiscal policy, while Minsky's theory of financial instability was invoked in support of the supply of liquidity to the financial system. Reference was also made to other older literatures for alternative explanations of the crisis, notably the Hayekian literature which focused on interest rates having been held below the natural rate.

Now a range of explanations for the crisis has been developed within the mainstream which retains the foundation of a presumption of equilibrating competitive markets. These explanations refer to factors which inhibited this equilibrating process, particularly information asymmetries, irrational behaviour and state

interference in the form of the lender-of-last-resort facility. The policy solutions follow directly in the form of removing these factors to prevent a recurrence of crisis – that is, making reality more like the standard model. The policy of fiscal expansion has generally been reversed, the focus returning to the supply side, while concerns are raised about expected inflationary effects of quantitative easing.

Much of the public debate about reform, as expressed for example in the pages of the *Financial Times*, has taken a wider purview and indeed at times has actively questioned the conventional mainstream approach to economics. Yet there has been little coverage of what would actually be entailed by opening up the question of other ways of approaching economics. The purpose of this paper is to draw attention to the possibility of re-examining not just theory but also theoretical approach as a way of addressing policy in the light of the crisis and to consider how that might impact on policy advice. To consider the question at this level of approach is justified by the challenges posed by the crisis, but is justified further by the fact that theory within some non-mainstream approaches had anticipated the crisis and were able to explain it as it evolved. Some wider reflection on economics is called for.<sup>1</sup>

The mainstream approach is characterised by an insistence that arguments be expressed (or capable of expression) in terms of formal deductivist mathematical logic. But this is not the only possibility; there is a range of alternatives, some of which employ some form of formal mathematical expression to some degree. But the critical difference is that mathematical formulation does not fully define these approaches. It is not a matter of abstraction or not: any theory and any theoretical approach inevitably require abstraction. Theory abstracts from variables thought to be less important to the question at hand. Theoretical approach goes much further. It employs particular ways of understanding, and therefore categorising, the subject matter, giving particular meanings to terms (such as ‘rationality’), and specifying the range of acceptable forms of argument. Thus any approach is based on a process of framing the subject matter, where framing involves ‘selection, emphasis, exclusion and elaboration’ (Weaver, 2007, p. 143). Not only does this abstraction provide the terms within which theory currently develops, but it also provides the terms in which older ideas are understood. There is no sense in which any approach can replicate reality; each approach to knowledge is an abstraction from reality and therefore incomplete in a variety of ways. Yet, as we shall see, different approaches involve a different approach to abstraction itself, and therefore a different relation with reality.

For any economist, deciding on one approach or another is necessary for knowledge to be developed, to inform policy. The choice to adopt one approach involves putting higher value on what that approach allows relative to what it precludes. But, while reasoned justifications can be made, there is no absolute basis for choosing one approach over another. Choice for economists, as for economic agents (on which more below), requires the exercise of judgement.<sup>2</sup> Study of the history of economics provides basic material for developing the capacity for judgement, showing how different ideas have developed in different contexts in order to address particular problems. But it also provides material on different approaches to economics. Without understanding what is involved in difference of approach, ideas from the history of economics can become distorted by modern frameworks. Thus, for example, only Keynes’s fiscal stimulus policy, Minsky’s idea of systemic risk through interconnectedness of portfolios and Hayek’s idea about interest rates have been picked up following the crisis, out of their much broader theories. But the different approaches within which these particular ideas were embedded have been ignored since they *do not make any sense* within a mainstream framework (because they are not compatible with the mainstream deductivist methodology).

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<sup>1</sup> Dow (2012a) provides foundational material for such a reflection.

<sup>2</sup> Of course such choices are not entirely ‘free’; any discipline involves its own institutions and educational frameworks which encourage particular approaches to the discipline.

There is an inevitably reflexive element in analysing the framing of the crisis in this paper, since, as an analyst, I have my own framing. This framing inevitably pitches the argument in favour of the alternative approaches which I will explore below as better addressing the problems posed by the crisis than the mainstream approach. It is important therefore to reiterate that all approaches have their limitations since they each abstract in some way or another. Nor do I want to suggest that these alternative approaches are complete in their own terms. Indeed according to these approaches, theory is provisional in the face of an evolving subject matter. While enumerating some of these limitations would require further exploration from other perspectives, the aim here is the more limited one of illustrating what is entailed in taking different approaches, albeit from one meta-methodological approach.

While it could be argued that any analysis should start with reality (framed in some way) if it is to be useful for policy-makers, this itself involves some circularity. First, this in itself involves an approach which relies more on induction than the more deductivist methodology of mainstream economics. By this is meant, not pure induction, since understanding of evidence is conditioned by theory, but that theory is grounded in evidence and is regarded as provisional in the face of future evidence, or evidence from different contexts. Further, reality will be understood differently by different groupings of economists (as well as policy-makers, and indeed political parties). Nevertheless it seems the best place to start, even if (as with Debreu, 1991) the conclusion is that reality is best addressed by abstract theory for universal application. The motivation is to open up the discussion beyond the mainstream approach, which involves starting with a particular theoretical framework, seeing how to adapt it to current circumstances and ultimately considering policy to make behaviour and market structure more consistent with the framework. The issue of different ways of understanding reality (different ontologies) will be addressed in the course of the analysis below.

Broadening the discussion to encompass different approaches could be understood in terms of widening the choice from which some new standard theory and theoretical approach are selected. But that in itself would reflect a particular approach. Methodological pluralism is a different approach which argues for actively fostering diversity of approach on the grounds that, as in biology, the capacity to adapt in the face of further environmental threats is enhanced. This is often misunderstood as 'anything goes'. But to consider pluralism in this way is to continue in the dualistic (either/or) approach of mainstream economics: there is only one best approach (Dow, 1990). Pluralism in its own non-dualistic terms rather involves opening up the possibility of a range of approaches. The range of possibilities is structured according to the conventions adopted by different schools of thought in economics (Dow, 2004).

The strong implication of methodological pluralism is that economists should stand ready to justify whatever approach they take. Where new thinking is constrained to fall within the mainstream approach, the implicit assumption is being made that economics is defined by that approach and thus it requires no justification. It is normal for non-mainstream economists to justify their approach in relation to their understanding of the subject matter and in relation to an alternative approach – normally mainstream economics – i.e. they are naturally pluralist to some degree. But it would be a major step forward were mainstream economists to attempt to justify their deductivist formalism, and in particular the exclusivity of this methodological position, in relation to the subject matter and in relation to some non-mainstream alternative(s). In particular this would require mainstream economists to address the implication of their methodology that the economy is a closed system (Lawson, 2009).

In order to explain the nature and implications of different possible approaches, we will focus here on two aspects of the financial crisis which require a policy response but which pose particular challenges for theory:

- a. *The scope for changes in market sentiment to drive asset prices.* While the long boom in asset markets had been taken to reflect rising ‘true’ value, this assumption was increasingly questioned as the weak basis for risk assessment in particular markets came under scrutiny. At the worst points in the crisis markets found it extremely difficult to assign values and banks chose as liquid a stance as possible, to the extent that the interbank market actually froze. On reflection, market sentiment was seen to have driven asset prices up and then drove them down. How can swings in market sentiment be explained theoretically and what is the policy scope for taming these swings?
- b. *The central importance of trust between the state, the banks and the public.* The banking crisis emerged as banks lost trust and then depositors lost trust, in particular in banks (and implicitly in the central banks’ support of these institutions), while central banks lost trust in banks’ willingness to behave prudently. The payments system, and thus the social fabric, were threatened. An important challenge therefore is to restore that trust. What theoretical approach can inform such policy?

We will consider different methodological approaches to considering each of these issues and the very different policy prescriptions which follow. In both cases we compare the mainstream approach with that of non-mainstream schools of thought. We compare it mainly to the (Post) Keynesian approach in the case of market sentiment and mainly to the institutionalist approach in the case of trust and moral hazard.

### **The role of market sentiment**

Greenspan (*Financial Times*, 27 March 2009) expresses the theoretical challenge posed by market sentiment as follows: ‘We can model the euphoria and the fear stage of the business cycle. Their parameters are quite different. We have never successfully modelled the transition from euphoria to fear.’ The crisis was the outcome of ever-increasing leveraging on the part of all sectors on the basis of confident expectations of continuing rises in asset prices, what Greenspan here refers to as ‘euphoria’, and of continuing financial stability. When these expectations were not met for some assets, asset sales and defaults (due to high leveraging) added fuel to the reversal in asset prices and to the increasing reluctance of banks to supply liquidity. This was exacerbated by the banks themselves now holding and trading in assets whose prices were reversing. Market sentiment changed from euphoria to fear. Before considering how this transition might be modelled, we need to consider how it may be conceptualised first, then theorised and then, possibly, modelled. Forecasting turning-points is widely regarded as a major challenge, but for many outside the mainstream the goal of formally modelling market sentiment is itself misplaced in any case.

The terms Greenspan used are psychological, and behavioural finance has done much to introduce conceptualisations from psychology into the analysis of financial markets. But, because the stated aim, or at least the outcome, has been to incorporate psychology into the existing formal mainstream framework of rational choice theory (see e.g. Kahneman, 2003; DellaVigna, 2008), the conceptualisation has necessarily been constrained in a particular way, either as cognitive limitations or as unconventional preferences (see further Dow, 2011). Rationality is defined as the logical pursuit of given goals, such that anything which falls outside such behaviour is defined as irrational (and to be limited or discouraged by policy). The benchmark is full information, including information about objective risk, so there is a concern with cognitive limitations which limit the absorption of information and thus estimation of risk, on which rational decisions are based.

The more activity is dominated by professional players in financial markets, with the fullest information and the least distraction by unconventional preferences and irrational behaviour, the better the chance of markets not deviating from their equilibrium path.

But recent experience suggests that market players themselves can find it difficult to price assets; indeed this is the normal pattern when markets undergo structural change, as evidenced earlier by the collapse of Long-Term Capital Management. To contemplate an objective risk measure, which markets are to identify, is to presume that the future is knowable, at least stochastically, as presumed by mainstream theory. Unpredicted structural change challenges such a presumption. The more general case is rather some degree of fundamental uncertainty, or unquantifiable risk, which looms large particularly when current conventions of risk assessment are challenged by events.

Keynes (1921; 1937) provided a theory of behaviour under uncertainty to explain, not only how we (as agents or as theorists) cope with uncertainty but also how we are able to take positive action under uncertainty. He pointed out that it would not be rational (in the strict mainstream sense) to make any positive decision to invest under uncertainty. While, rationally, we draw on theory and evidence based on past experience as far as possible, this cannot be sufficient to guide action with respect to an uncertain future. Further, deductive reasoning cannot explain why a set of expectations could change from Greenspan's euphoria to his fear. We make up the gap left by uncertainty by drawing on conventional judgements (Davis, 1994) and by exercising (or not) animal spirits (Dow and Dow, 2011). Neither of the latter is grounded in rational choice theory as defined by mainstream economics and indeed would be classified as irrational.

But to accept that classification in terms of rationality is to accept the bounds of that approach to theory. For Keynes, as for Hume and Smith, and indeed for much of the psychology literature, cognition and sentiment are not a mutually-exclusive dual, but rather are interdependent (Dow, 2011). Thus reason requires a foundation in conventional belief (just as the Bourbaki project found that deductive mathematics cannot be constructed as a self-sufficient system) and must be combined with the exercise of the imagination, along with emotion, to motivate behaviour. Far from being something necessarily to be discouraged, some sentiment (or emotion) is necessary for decision making.

Animal spirits are necessary for firms' investment decisions, given uncertainty, and also for market leaders who trigger changes in market sentiment by making bold moves against the market. But for most market players it is (informed) conventional judgement which is most important. While individuals are the unit of mainstream analysis (with possible, though logistically limited, modification to incorporate other-regarding behaviour), other approaches understand individual identity in relation to society (Davis, 2003). Rather than a basically selfish atomistic individual constrained by society, Smith (1759) in the *Theory of Moral Sentiments* analysed individuals whose behaviour is in reference to society's judgements or an imagined impartial spectator who judges behaviour. (This does not presume unselfish behaviour, but rather behaviour which is aware of the consequences for others, and takes this into account in varying degrees.) In the absence of certain knowledge, a successful society therefore evolves in such a way as to enable action in spite of uncertainty. Institutions are formed and conventions established which provide a stable foundation for decision making (van der Lecq, 1998).

Conventions may be challenged by events – they too evolve – and this is particularly the case for conventional judgements. In Keynes' terms, confidence in the conventional low assessment of risk increased as markets followed a relatively stable path up to 2007. This psychological state had real consequences in employment, production and expenditure. Conventional judgements were part of the reality, in turn affecting the reality, and reinforcing themselves reflexively as asset prices continued to rise (Soros, 2008). Market players framed the reality in terms of mainstream theory, which suggested that rational market behaviour was expected to produce the pricing of assets in line with true risk and the best outcome for society (or at least

this framing was used rhetorically). But conventional risk assessment was thrown into disarray with the crisis and it took some time for new, more wary, conventions to become established.

Let us now consider the implications of this way of understanding the nature of behaviour in financial markets, first for approach to theory and then for policy. As far as theoretical approach is concerned, questions arise about the scope for deductive logic (which relies on the certain, or certain-equivalent, knowledge as to the truth-value of premises). If the nature of the economic system is such that it does not behave in a law-like way which allows confidence in quantification of risk, then uncertainty is the general case. To focus on law-like behaviour and quantifiable risk is therefore to focus on what for Post Keynesians is a special case, with uncertain scope as to application. In particular, the mainstream approach is to attempt to capture behaviour in a deductive mathematical system. This approach has the advantage of clarity and consistency within itself, where the aspects of reality under consideration are made commensurate by mathematics, but at the cost of limiting what can be considered (Chick and Dow, 2001).

Much follows from the centrality of the concept of rationality, by its special definition, in mainstream economics. Just as economists are seen as rationally constructing deductive models of stochastic relationships, so economic agents rationally optimise on the basis of risk assessments based on stochastic relationships. But if in fact behaviour is based on conventional judgements, eg about risk, which are subject to non-deterministic (but not stochastic) shifts, then the case is strong for theory to address the factors underlying those conventions and shifts in the conventions. Just as Keynes argued that, in society, our behaviour is based on knowledge derived in a plurality of ways from a plurality of sources (with input from emotions), so also the analyst may usefully draw knowledge in a pluralist way. Mathematical models play a part, as a way of expressing partial arguments in a clear way. But because uncertainty, conventions and emotions, as well as non-deterministic evolution of institutions, cannot be modelled in the conventional deductivist way, any argument based on a formal, closed model is inevitably partial and requires putting together with other lines of argument and different forms of evidence, in order to increase weight of argument (Lawson, 2009). It is worthwhile to consider that, while Keynes referred to the usefulness of formal models, he nevertheless warned about the importance of keeping in mind the closures which models require, but which need to be relaxed for application of the model's conclusions:

[I]n ordinary discourse, where we are not blindly manipulating but know all the time what we are doing and what the words mean, we can keep 'at the back of our heads' the necessary reserves and qualifications and the adjustments which we shall have to make later on, in a way in which we cannot keep complicated partial differentials 'at the back' of several pages of algebra which assume they all vanish. (Keynes, 1936, pp 297–8)

From a mainstream perspective, which effectively defines the subject by what can be dealt with by means of deductive (mathematical) logic (see e.g. Blaug, 1999), anything else falls outside the discipline. This parallels the conclusion that anything which falls outside the particular definition of rationality is irrational and therefore to be avoided. But the argument for taking a broader purview of possible methodological approaches (pluralism, as outlined above in the Introduction) is particularly strong for policy makers who are required, not just to analyse, but to take positive action. Central bank publications have been addressing uncertainty increasingly frequently (see e.g. Aikman *et al.*, 2010 for a recent discussion). While policy-makers need to decide on their own overall approach and thus range of methods, there would also be benefit in increased awareness of what other approaches can offer. Each approach has its own strengths and weaknesses, and unanticipated developments might call for guidance from alternative approaches. As Keynes (1921) argued, confidence in judgement is higher the more different types of evidence (and

reasoning) support that judgement. Further, the Monetary Policy Committee (MPC) of the Bank of England (1999) has explicitly referred to the pluralism they employ in the sense of a range of methods (see further Downward and Mearman 2008).

But, while the MPC had access to a range of evidence on market sentiment, based on different methods, the potential for crisis was either not picked up or not sufficiently highlighted. What was required instead was that market sentiment be taken seriously. From the mainstream perspective, market sentiment is either a form of short-cut rationality or else something to be ignored or eliminated as irrationality. The only policy response on market sentiment has been to make markets more transparent, with fewer incentives and constraints distorting market behaviour, to allow markets to be more efficient. The Post Keynesian approach is rather to understand market sentiment as the normal mechanism for market judgement in the face of uncertainty. Theory used to understand developments in financial markets should therefore include analysis of decision-making under uncertainty, including any changes in the institutional environment which might alter the process of arriving at, and perpetuating, judgements. This would suggest input from 'old' institutionalist theory (Rutherford, 1994; Hodgson, 1999) and 'old' behavioural theory (Earl, 1989; Sent, 2004) which (unlike the 'new' versions of this theory) are not constrained to analyse behaviour in terms of rational optimisation by atomistic individuals.

These other theoretical approaches would aid understanding of market sentiment and what causes it to change, but also point to possible policy intervention in order to stabilise markets. Conventions may depart from what the authorities regard as reasonable (rather than narrowly rational) judgement, and psychological theory can inform the analysis (see e.g. Tuckett, 2011). This implies the need for mechanisms for monitoring market sentiment and for designing monetary policy (especially communication of monetary policy) to moderating market sentiment when it is judged to be lacking a grounding in reality. What will be required will depend on particular circumstances since market sentiment does not lend itself to universal theorising. But to take the current circumstances (in late 2011) as an example, the authorities are trying to calm volatile market sentiment by reassurances that fiscal austerity packages will resolve budgetary problems, rendering sovereign debt instruments secure (as mainstream theory would suggest). While these policies may have been encouraged by markets' own initial framing of a budgetary deficit problem, governments could have attempted to put that framing into historical perspective, calming markets and reducing pressure for austerity measures. But if the real outcome of austerity measures at a time of weak economic conditions amid efforts by the private sector to draw down debt in fact turns out to be low growth and worsening budgetary conditions, accepting the markets' framing will have proved counterproductive. While market sentiment makes up for insufficient evidence and understanding, evidence and understanding nevertheless do influence market behaviour. If market sentiment is simply one, integral, part of the cognitive process, then it is not purely psychological.

### **The role of trust**

The Keynesian theory of knowledge under uncertainty outlined above emphasises the role of (socially) conventional knowledge. But the functioning of the economy in general, and of monetary policy and financial sector reform in particular, require the presence of a key social convention: trust. A major challenge posed by the banking crisis has been how to address the general breakdown of trust between the central bank, the banks and the public.

Trust has been the subject of new literature within the mainstream, particularly in the form of trust games, where the responses of other parties to incentives are not known with certainty (Berg, *et al.*, 1995).



But, as Hughes (2011) shows, conceptually trust in this approach either collapses into rational optimising behaviour (within these circumstances) or it is irrational (and thus to be discouraged). At best, other-regarding behaviour can be incorporated by placing the pay-offs to others in each others' utility functions in a calculative way. But this is different from trust. Here again we see the choice of approach determining the scope of theory.

The Hume/Smith/Keynes approach outlined above takes other-regarding behaviour as a starting-point rather than a modification. Indeed according to this approach, market economies could not function without social conventions, the most important of which is trust. Rather than the calculative trust of the game theory approach, this conceptualisation sees trust as an alternative to calculation (where calculation would not be possible, given uncertainty). Hughes (2011) argues that trust refers to expectations with respect to agency (the actions of identifiable agents or organisations). Confidence rather refers to the successful build-up of trust with respect to the structure of organisations. But when confidence in structure is challenged, as during the crisis, the issue reverts to one of trust, and thus agency. What is at issue now is the agency of central bankers, bank CEOs and the borrowing and investing public.

Since confidence and trust are built up as a result of extensive periods of experience, and this is evidently the case with banking, a historical approach can contribute to our understanding. As Chick (1986; 1993; 2008) demonstrates within her stages-of-banking-development framework, fractional reserve banking emerged as a result of the convention emerging of using bank liabilities for payments, a convention which relies on confidence in the banks managing their assets prudently. From a narrow rationalistic perspective, fractional reserve banking should not work, since it relies on what cannot be strictly rational expectations as to risk of bank collapse. Instead it relies on a socio-psychological convention; the more confidence builds up, the less the possibility of bank failure is contemplated.

Central banking develops as the potential for instability in banking becomes recognised as a threat to the maintenance of confidence and thus to the successful working of the system. Central banks use banking regulation, and supervision and monitoring with respect to this regulation, to promote prudent bank behaviour. In addition, the central bank stands ready to supply liquidity to any bank in trouble through the lender-of-last-resort facility. The existence of this facility encourages confidence which in turn reduces the need for it to be brought into play. The banks are therefore providing a public good in the form of the liquidity of their liabilities, with the support of the central bank.

But there are tensions between the profit-seeking behaviour of banks and the central banks' need for them to behave prudently. Maintaining a balance between these tensions, which might in the past have rested on personal relations between bank Governors, was challenged in recent decades by the growth in scale and complexity of the banking sector. Banking now included a much wider range of functions than deposit-taking, direct lending and safe investments. Banks had been given more latitude to pursue profits in the 1980s with deregulation. But restrictive reregulation in the form of capital adequacy requirements had the unintended consequence of encouraging banks to seek profits off balance-sheet by securitisation and by activities in derivatives markets which were important ingredients in the build-up to the crisis. While banks continued to supply the bulk of society's means of payment, with the lender-of-last-resort facility still in place, they were exposing themselves to increasing degrees of risk. With growing awareness of that risk (and the weakness of knowledge as to the extent of risk) trust between banks, as expressed by inter-bank lending, broke down and so the confidence in the market's capacity to supply liquidity broke down. The public's confidence in some banks broke down (amid general uncertainty about deposit insurance protection) leading to bank runs which led to a contagious lack of confidence in banks more widely. Both banks and the public in the initial crucial stages were unsure as to whether the central bank would use the lender-of-last-resort facility, further damaging confidence. Unlike the systemic risk which arises from interconnectedness of highly-

leveraged portfolios, the systemic risk here refers to the loss of confidence in one bank spreading to others exposed to similar forces, something which does not lend itself to capture by deductive reasoning.

The mainstream approach to theory suggests that the resulting policy issue be addressed in terms of moral hazard: the unintended effect of insurance as encouraging the taking on of increased risk (where there is some limit on the scope for monitoring that risk) (see further Dow, 2012b). In spite of the term 'moral', the issue is one of rational optimising behaviour, under asymmetric information. Because such behaviour is not other-regarding, it is opportunism. It may be regarded implicitly as immoral because, by impeding markets from finding the social optimum, the outcome is a reduction in social welfare; but because this outcome is an unintended consequence, it may not be regarded as immoral. In any case, morality is equated with rationality in this approach; the impartial spectator, which Smith discussed as a mechanism for promoting moral standards, is discussed by behavioural economists as a mechanism for ensuring rational choice (see Ashraf *et al.*, 2005).

The policy implications of this theoretical approach are, first, that opportunities for moral hazard be limited by regulation, hence the proposal to limit banks to their traditional functions to limit the scope for opportunistic behaviour. Second, in the spirit of calculative rational behaviour, financial incentives (bonuses etc) would be regulated in such a way as to incentivise more prudent behaviour on the part of bank management and employees. Third, the scope for irrational behaviour among borrowers from banks would be promoted by 'nudging', as a substitute for the impartial spectator (Thaler and Sunstein, 2009). Trust between central banks, the banks and the public would be restored, ie it would be seen to serve calculative self-interest to trust.

But if we go back to the more general theory of knowledge under uncertainty, where social conventions, including trust, are essential building blocks for market activity, some important elements have been excluded from the mainstream theoretical approach. First, alternative approaches suggest that important influences on behaviour are non-calculative and thus not amenable to modelling as optimising behaviour. In particular, behaviour which observes moral norms with respect to trust, and then the breakdown of such behaviour and the breakdown of trust, are difficult to capture fully in a deductive framework. Indeed confidence entails quite the opposite of calculation, reducing the need even to pay attention to the possibility of bank failure.

Theories as to social conventions, and the nature and role of trust, have been explored by 'old' institutionalist theory, while the role of confidence in the development of banking has also, as we have seen, been analysed within the evolutionary approach. Finally, since some social conventions involve moral judgement, e.g. as to standards of fairness, it is important for economic theory also to be able to address such considerations. Notions of fairness effectively fall outside the realm of rationality in the mainstream framework (Akerlof and Shiller, 2009). Nevertheless, much of the public policy discourse surrounding the crisis has focused on issues of fairness. This is evidence of the other-regarding behaviour analysed by Adam Smith. Fairness issues may be raised for selfish or unselfish reasons (reflecting concern over one's own relative position, or that of others). The point is that it is an issue for individuals understood as members of society. Similarly, in the financial sector, employees may respond to bonuses as relative indications of standing, rather than being incentivised by absolute amounts. Since such considerations are important to the internal running of organisations, as well as to relations of trust between central banks, banks and the public, a theoretical approach is needed which can address them, in order to inform policy. Indeed, since corporate culture and issues of governance have arisen as sources of problems within financial institutions which gave rise to the crisis, a theoretical approach is required which focuses on institutions too in terms other than incentives based on (narrow) rational, fully-informed calculation. Behaviour within and between organisations, as between individuals, involves social empathy and uncertainty.

According to this alternative approach, moral hazard involves a wider range of issues surrounding the breaking of trust than the mainstream definition. If the banks had risked the trust of the central bank (as well as other financial institutions) by their opportunistic behaviour, the central bank also risked the trust of the banks by not clearly standing by the lender-of-last-resort facility from the start. Where trust is the outcome of conventional judgements with respect to long experience, it is not calculative, but nevertheless an important element in relations within the economy. Breaking with the conventional behaviour which underpins trust bears the serious risk of breaking trust, requiring new prolonged experience for trust to be restored.

A return to traditional banking is being considered as a response to the moral hazard and fiscal problems associated with the lender-of-last-resort facility being provided to large banks. Since it is the deposits of retail banks which perform the vast bulk of money functions and therefore it is retail banking which requires central bank support, the mainstream proposal to separate retail banking off from investment banking is shared by this non-mainstream approach. But it would be important in addition, from an evolutionary perspective, for a clear commitment to be made to continuing to make the facility available to these narrower banks. In principle, if these traditional banks were to fail, deposit insurance would protect depositors. But, given uncertainties over the insurance process, exacerbated by differences in national regulation and practices in a global banking environment, it is hard to see how confidence would in fact be restored without such a commitment. Given uncertainty, particularly in the kind of circumstances where a bank might fail, rational calculation would not in fact justify trust. Rather, as the evolutionary approach demonstrates, it takes time, reassurance and experience for society to restore a (non-calculable) convention of trust.

The more general policy implication, that efforts be made to rebuild trust between central banks, banks and society at large, is difficult to tie down further as a general principle (rather than with respect to particular local circumstances, including institutional history). But this does not invalidate it if the goal is not to seek universal policy prescriptions. Further this alternative approach requires a change of mindset from basing policy on financial incentives and constraints as they affect the individual and the individual firm and turning to addressing issues of fairness and wellbeing at a societal level.

## **Conclusion**

The aim here has been to point out that there are different possible approaches to economics which can inform policy (not just different theories within one approach). Each starts from its own view of the nature of the economy, categorises it accordingly, and established criteria for good argument. It has been suggested that a deductivist approach dominates mainstream economics and mainstream economic policy (in spite of challenges from evidence). But this should not be regarded as the only option. For all its attractions, this approach limits coverage of important issues which have arisen with the crisis. The starting point of rational optimising individual behaviour limits the scope for understanding market sentiment (indeed any sentiment, e.g. with respect to fairness), and how it may change. It also limits the scope for analysing trust, and considering how it may be restored. All approaches inevitably are limited by the very nature of theoretical abstraction (far less framing). But, just as non-mainstream economists actively justify their methodologies, so should mainstream economists. We have attempted here to illustrate ways in which two alternative approaches address these limitations.

To argue for consideration of different approaches is to argue for methodological pluralism. This is not at all to advocate that 'anything goes', but rather that reasoned judgement be applied to considering which is most useful among the range of possibilities within which different sets of economic theory have

been developed. (These approaches each represent a set of conventions among groups of economists as to how to build knowledge.) We have illustrated the meaning and significance of difference of approach in terms of policy to address the crisis. For policy-makers, judgement is required in considering the applicability of particular, inevitably partial theories (Dow, 2012c). But it is the duty of economists to explain these theories in terms of the approaches which have generated them and to justify the approaches as well as the theories. In particular, the *onus* is on mainstream economists to justify their theories in relation to what is being assumed about the nature of the subject matter.

Since the mainstream approach prioritises argument expressed in deductive mathematics, methodological pluralism also refers to the possibility of different types of argument (plurality of method); deductive mathematical reasoning itself precludes a wide range of subject matter which can more readily be analysed using a range of other methods (possibly alongside partial mathematical models). The issue is whether a deductive mathematical model can be sufficient argument in itself, or whether it can only yield partial arguments for input with other forms of argument. If the latter is the case, then the role of judgement, in choosing strands of argument addressed to a particular context, and considering how to put them together, becomes central.

As suggested earlier, the best place to start in exercising judgement is an account of the reality to be analysed. On this basis, emphasis was placed here on the significance of fundamental uncertainty, for agents and for economists, which society addresses by developing conventions. But the urge to action requires animal spirits in spite of uncertainty. It is to be hoped that the extreme circumstances of the crisis may fire up the animal spirits of economists to reconsider and challenge their own conventions in a constructive way.

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# On the Limits of Rational Choice Theory

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## Abstract

The value of rational choice theory for the social sciences has long been contested. It is argued here that, in the debate over its role, it is necessary to distinguish between claims that people maximise manifest payoffs, and claims that people maximise their utility. The former version has been falsified. The latter is unfalsifiable, because utility cannot be observed. In principle, utility maximisation can be adapted to fit any form of behaviour, including the behaviour of non-human organisms. Allegedly 'inconsistent' behaviour is also impossible to establish without qualification. This utility-maximising version of rational choice theory has the character of a universal 'explanation' that can be made to 'fit' any set of events. This is a sign of weakness rather than strength. In its excessive quest for generality, utility-maximising rational choice theory fails to focus on the historically and geographically specific features of socio-economic systems. As long as such theory is confined to ahistorical generalities, then it will remain highly limited in dealing with the real world. Instead we have to consider the real social and psychological determinants of human behaviour.

**Keywords:** rationality, utility, preferences, experimental economics, falsifiability, historical specificity

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Much time has been spent by economists and critics on the pervasive but elusive concept of rationality.<sup>1</sup> This essay will not end this output, but it will hopefully divert some of the wasted energy into more useful occupations. There are multiple prominent versions of rationality, not one (Sen 1987). One version upholds that rationality is essentially about consistency of behaviour. Another sees rationality as the maximization of explicit (typically pecuniary) rewards. So much empirical and experimental evidence has been marshalled against the second (more restrictive) version that a significant number of economists have now abandoned the idea, and at least six critics of rationality (Friedrich Hayek, Gunnar Myrdal, Herbert Simon, Ronald Coase, Amartya Sen and Daniel Kahneman) have been awarded Nobel prizes in economics. Whether the existence of altruistic behaviour challenges the idea of rationality depends on the definition that is adopted. Definitions of rationality that accommodate altruism end up being unfalsifiable.

This essay is divided into three sections. Section one discusses possible meanings of rationality and distinguishes between payoff maximisation and utility maximisation. Payoff maximisation has been refuted by many experiments. Section two shows that the prominent notion of rationality as utility-maximisation is strictly unfalsifiable and does not offer a viable causal explanation of behaviour. Section three selectively reviews some prominent criticisms and defences of the rationality assumption in the light of the unfalsifiability of utility maximisation. Section four concludes the essay.

## 1. The slippery concept of rationality

One popular notion of rationality is *thoughtful deliberation*. This is not the meaning that is associated with the 'choice' concept of Lionel Robbins (1932) or the 'as if' methodology of Milton Friedman (1953). But when Herbert Simon (1957) argued that rationality was 'bounded,' he sometimes used this term to refer to limited computational and deliberative capacity.

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<sup>1</sup> This essay uses some material from Hodgson (2013). The ideas have benefitted from numerous discussions with many scholars.



A similar meaning of rationality is *acting for reasons*. Although etymologically accurate, this is alternatively described as the *rationalist concept of action*, and criticized in a different manner (Hindess, 1977; Hodgson, 1988). The use of different meanings by economists has confused much of the debate surrounding the concept of rationality.

Another prominent notion of rationality is that people try to do the best they can in their circumstances. An important corollary is that agents respond to incentives. There is nothing wrong with this idea. But it fails to tell us how people interpret their situation or identify 'the best' goal. It does not acknowledge that different interpretations of situations and hence different goals are often possible. The problem with this 'doing one's best' notion of rationality is that it lacks the necessary explanatory detail concerning agent cognition and goal-formation. It might also be used to buttress self-interested notions of 'the best' that are in fact undermined by the evidence. The 'doing one's best' notion of rationality is not entirely vacuous, because it does point to the necessity of problem-solving. But it lacks vital detail. To avoid confusion, 'doing one's best' notion would better be described as *following incentives* or *adaptation to circumstances*. But more specific detail would also be required.

We now consider notions of rationality that dominate modern mainstream economics. I define some additional terms. A payoff is a reward in a game that *has an explicit expected worth* (such as a declared monetary reward) *that is known to the analysts of the game and to all of its players*.<sup>2</sup> By payoff rationality or payoff maximization, I mean the maximization of such explicit payoffs by players, given the information available to them plus their assumption that other players are also payoff maximisers. If one is committed to the axioms of payoff rationality, then logically one is also committed to the idea that rationality involves consistent behaviour. But utility maximization is not necessarily payoff maximization, unless there is a monotonic relation between utilities and payoffs.

Vernon Smith (1982) and others have addressed the problem of the possible absence of a monotonic relation or 'parallelism' between overall utility and monetary payoffs. To relate payoffs to utilities, the possibility of additional, subjective utilities that are unrelated to one's own monetary payoffs, such as utility derived from the satisfaction of others or from taking risks, has to be substantially diminished. The player's own money payoffs have to 'dominate' their decisions. In order to make experiments 'work' in this sense, Smith proposes a number of 'precepts' of experimental assumption and design constituting an 'induced value procedure.' These precepts include nonsatiation, sufficiently large and obvious rewards, restriction of communication between subjects, and so on. But Smith (p. 929) himself is the first to admit that these precepts cannot *guarantee* any monotonic correspondence between observable monetary rewards and preferences, which are 'not directly observable.' In fact, we can never know if the precept has been effectively applied. The idea that Smith's precepts 'work' is an article of faith, placed so far under surprisingly little methodological scrutiny.<sup>3</sup>

Despite the headway made by its critics, some economists retain a notion of payoff rationality. They argue that while there are empirical deviations from its norms, if pecuniary rewards are sufficiently large and agents are given long enough to learn the game, then payoff maximization will become established as an approximate behavioural rule (Harrison, 1989; Binmore, 1994; 1998; 1999; Binmore and Shaked, 2010).

By contrast, others follow 'behavioural economists' and argue that the evidence is sufficient to undermine payoff rationality. Behavioural economics has now spread to the mainstream, and is evident in some of the most prestigious journals in the discipline.<sup>4</sup> Payoff rationality and self-interest were regarded as articles of faith among mainstream economists from the 1950s to the 1990s, and to question them was

<sup>2</sup> Note that the definition of payoff includes those formulated in probabilistic terms. For simplicity I shall ignore games where possible payoffs are known to some players but not others. Including this possibility would not change the principal conclusions below. In fact, it would make the concept of rationality more difficult to define straightforwardly, as in some types of game theory (Sugden, 1991).

<sup>3</sup> For a critical discussion of Smith's precept of parallelism see Siakantaris (2000).

<sup>4</sup> See Earl (2010) for a critical comparison of mainstream and Simonian behavioural economics.

enough to lose one's credentials as an economist. Subsequently, in the face of massive, accumulating, evidence of agents who do not maximize pecuniary rewards, economics has changed.

It is impossible to review all the evidence here.<sup>5</sup> There are 'framing effects' when rankings of options change when equivalent choices are presented in different terms. Although payoff rationality means that bygones should be ignored, people often take them into account. Individuals are presented with two gambles – one with a certainty of winning a modest sum of money and the other with a low probability of winning a large sum of money. Even when the expected value of the risky option is greater, people often prefer the certain reward (Slovic and Lichtenstein, 1983). In the face of such evidence, strict payoff rationality has been abandoned by many.<sup>6</sup>

Although the axioms of payoff rationality imply consistency of behaviour, the reverse is not true. Without logical contradiction, one can abandon payoff rationality and still uphold that behaviour is consistent, and even utility-maximizing. Herbert Gintis (2007; 2009) is an exponent of this position.

Gintis is a co-author of a fascinating set of cross-cultural studies that show that players rarely reach a Nash payoff solution in ultimatum games (Henrich *et al.*, 2001; 2004). One of two players in an ultimatum game is asked to divide an amount of money between herself and the other player. If the second player rejects the division, then both players get nothing; but if he accepts, then they each receive their allocated amounts. If the second player is a payoff maximiser, then he will accept the lowest possible positive allocation when it is offered: payoff maximisers always prefer something to nothing. This is a subgame perfect Nash equilibrium; it gives the best expected payoff outcome for both players, each assuming that the other player is also a payoff maximiser. Consequently, no player has anything to gain by unilaterally changing strategy on their own. But experiments often do not lead to this Nash payoff outcome: players do not always maximize payoffs in this way. Instead, their behaviour is consistent with taking additional, intangible, non-pecuniary factors into account, such as honour, custom and fairness, even when they cannot bargain with one another and the game is not repeated. The cross-cultural studies of Joseph Henrich and his colleagues also showed that the actual pattern of play can vary significantly from one cultural setting to another.

While abandoning payoff rationality, Gintis defends a broader concept of rationality, defined as consistency of behaviour. Given behavioural consistency (or transitivity) – along with the other standard assumptions such as independence and continuity (von Neumann and Morgenstern, 1944, Fishburn, 1970) – it is possible to construct a standard ordinal utility function where behaviour is consistent with expected utility maximization. Gintis (2007; 2009) considers much of the experimental evidence and points out that the absence of payoff maximization does not mean that these players are behaving inconsistently or failing to maximize utility.<sup>7</sup> For Gintis (2006, p. 17) this behavioural consistency is rooted in genetically rooted instincts and drives that have evolved over the millennia and dispose us to respond in specific ways to specific cues. Consequently, Gintis (2006, p. 7) argues that 'utility maximization should be a central tool in analysing human behaviour, even if humans are not self-regarding.'<sup>8</sup>

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<sup>5</sup> See the extensive works of Nobel Laureate Daniel Kahneman and his colleagues (Kahneman, 1994; 2003a; 2003b; Kahneman *et al.*, 1982; 1986a; 1986b). Bowles and Gintis (2011) provide an excellent overview of the evidence.

<sup>6</sup> But controversy has not ended. For example, different results – sometimes closer to payoff maximization – are obtained from some field experiments (Gneezy and List, 2006). The contrasting outcomes of laboratory and field experiments show that the social and institutional context matters, and individuals cannot be taken in isolation (List, 2006).

<sup>7</sup> There is experimental evidence that people are not very good at logical problems, especially when they are posed in abstract terms or involve probabilities. Gintis (2007, pp. 11-12) brushes this evidence aside with the questionable conclusion that 'most individuals do not appear to have difficulty making and understanding logical arguments everyday life.' The evidence of Wason (1983), Cosmides (1989), and Cosmides and Tooby (1994a; 1994b) suggests otherwise. Even from Gintis's standpoint, this argument seems somewhat superfluous, because if an individual made the same logical errors over and over again, then he or she might be behaving consistently, and might be 'rational' by Gintis's criterion.

<sup>8</sup> Gintis distinguishes between 'self-regarding' and 'self-interested' preferences. With 'self-regarding' preferences one takes account of one's own situation only. Hence for Gintis a charitable act is not 'self-regarding' but it may be 'self-interested' because of increased utility gained by the 'warm glow' or satisfaction of giving. By contrast, it is argued here that notions of utility-maximization or 'self-interested' behaviour are in principle unfalsifiable, and of little use.

## 2. Fitting everything and explaining nothing

Gintis does not acknowledge the following key difficulty. When the young Paul Samuelson (1937, p. 156) discussed utility maximization, he understood that ‘all types of observable behaviour might conceivably result from such an assumption.’ Because utility is unobservable, all kinds of behaviour can be ‘explained’ in terms of the idea, without fear of refutation. As Sidney Winter (1964, pp. 309, 315) and Lawrence Boland (1981) have also remarked, no evidence can possibly refute the theory that agents are maximizing some hidden or unknown variable (such as utility). Amartya Sen (1977, p. 325) has similarly pointed to the circularity of explaining behaviour ‘in terms of preferences, which are in turn defined only by behaviour.’ Sen (1987, p. 73) notes elsewhere that the description of choices in terms of utility ‘does not give any independent evidence on what the person is aiming to do or trying to achieve.’

Defending ‘self-interest, rightly understood’ against its critics, Teppo Felin and Nicolai Foss (2009, p. 622) say it is consistent with ‘cooperation, organization, community-building, trust, or for that matter, any other individual, relational, or organizational virtue.’ Rather than selling the assumption, this rather gives the game away. An assumption that is consistent with everything describes little and delimits nothing.<sup>9</sup>

If experiments show that some consumers appear to prefer a monetary reward that is less than the expected outcome, or appear to have intransitive preference orderings, or defy the independence axiom, then we can always get round these problems, and make the evidence consistent with utility maximization, by introducing other explanatory variables.<sup>10</sup>

For example, preference reversals can be regarded as consistent with expected utility theory. Assume that a subject is faced with a choice between \$10 with certainty, and \$1,000 with a probability of 2 per cent. Experiments with real subjects indicate that in such situations the \$10 option is sometimes chosen, despite the fact that the expected value of the second option is higher at \$20 (Slovic and Lichtenstein 1983). But preference reversals also fail to falsify expected utility theory, once we accept that (expected) utility is not necessarily measured in terms of the monetary payoffs in the experiment. If we assume an added disutility associated with involvement in a risky and low probability choice, then the theory that people are maximizing their utility is not overturned by these experiments. A risk-averse actor may not maximize expected monetary value but still be maximizing expected utility. By appropriate functional manipulation, the choice of \$10 can be made perfectly consistent with the maximization of expected utility, rather than the maximization of the expected monetary value of the payoff.

Gintis and others might respond that inconsistent behaviour would refute utility maximization. The problem here is one of identifying inconsistent behaviour in empirical terms. Note that the utility maximand is unobservable. For example, if an experiment shows that option *A* with an expected value of \$4 is preferred to option *B* with an expected value of \$5 then we can simply assume that there are additional attributes of option *A* (for example, we may enjoy losing or gain pleasure from seeing others win) that are consistent with the view that it yields higher overall expected utility for the subject.

On repeated visits to the same restaurant, we may prefer steak to fish one day, and fish to steak on another. Is this behaviour inconsistent? Maybe. Maybe not. We may discover that the steak is not as good as expected. Or we may have seen an alarming television report about mad cow disease that causes us to switch to fish. The two choice occasions were different in terms of circumstances and knowledge. Hence they do not necessarily imply inconsistency.

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<sup>9</sup> Significantly, Felin and Foss (2009, p. 622) continue: ‘the type of ‘enlightened’ self-interest we have in mind should be completely decoupled from ethics.’ In contrast to their defence of the self-interest assumption, their critique of social constructivist and performativity arguments in their article is much more robust.

<sup>10</sup> Hausman (1992, ch. 13) documents several attempts to explain the apparent anomalies that have been revealed by the experimenters, notably by pointing to other possible sources of utility. But in some of these cases the independence axiom is abandoned in attempts to rescue the idea of utility maximization.

The empirical detection of preference intransitivity is also problematic. An experiment may seem to reveal preference intransitivity, by showing that while  $X$  is preferred to  $Y$ , and  $Y$  is preferred to  $Z$ ,  $Z$  is preferred to  $X$ . But this result can be explained away by showing that the three pairwise comparisons did not take place under identical conditions, or were separated in time or space. Extraneous factors may account for the apparent intransitivity. All we have to do is indicate in some way that the two  $Z$ s in the above comparisons are not quite identical. The two  $Z$ s could be slightly different in timing, substance, or their informational or other contexts. We then get the result:  $X$  is preferred to  $Y$ ,  $Y$  is preferred to  $Z_1$ , and  $Z_2$  is preferred to  $X$ . In these circumstances, transitivity is no longer violated. The defender of utility maximization may conflate  $Z_1$  with  $Z_2$ , whereas they were in fact different.

It may be objected that if preferences are assumed stable, then evidence on revealed preference could reveal inconsistent preferences. But this would not be the case if utility depended on other factors in the environment. Consider the utility function  $U = f(X, E)$ , where  $X$  is a vector of consumption inputs and  $E$  is a vector of environmental or contextual conditions. Assume the function  $U$  is perfectly stable. But  $E$  can never be strictly held constant. Some part of the environment, however remotely or slightly, will inevitably alter. Hence, in practice, intransitivity (or intertemporal inconsistency) in the rankings of the elements of vector  $X$  alone would not reveal preference inconsistency because some elements in the vector  $E$  would also have changed, even by the tiniest amount. Strictly, the environment is never constant. Consequently, because we cannot strictly and identically replicate the  $E$  conditions, intransitivity or inconsistency of  $X$  choices can never falsify the assumption of fixed preferences.<sup>11</sup>

Given that we can never in principle demonstrate that some unobserved variable (like utility) is not being maximized, then the theory is invulnerable to any empirical attack. No amount of evidence can establish non-existence. Hence the standard core of expected utility theory is *unfalsifiable*.<sup>12</sup>

The utility-maximization assumption is unfalsifiable, but it is not a tautology in the logical sense because it is *conceivably false*.<sup>13</sup> Logical tautologies – such as a triangle has three sides – are true by definition. By contrast, it might be the case that individuals are not maximizing anything. But we can never establish this on the basis of empirical evidence.

This does not necessarily mean that the utility maximization framework is useless or wrong. We do not have to uphold falsifiability as the mark of science – a criterion attributed to Karl Popper, who in fact adopted a more nuanced position (Ackerman, 1976). Neither tautological nor non-falsifiable statements are necessarily meaningless or unscientific.<sup>14</sup>

A key problem with utility maximization is that it is so general that it can explain anything; consequently its explanatory power in specific instances is dramatically diminished. Its explanatory success is an illusion. Close inspection of its proclaimed achievements reveal that the results always depend on additional assumptions. For example, Gary Becker (1976; 1991; 1996) contends that standard rationality assumptions generate a number of testable predictions concerning human behaviour. But all of Becker's 'predictions' depend on assumptions *additional* to his core axioms of utility maximization. Indeed, because it is difficult to conceive of evidence that falsifies these axioms, such models must depend on auxiliary assumptions to generate specific results. As Mark Blaug (1992, p. 232) puts it: 'The

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<sup>11</sup> There is a standard argument that intransitive preferences would involve a 'money pump' – an agent with intransitive preferences would accept a series of trade offers that leaves her worse off to the benefit of the other trader. But given the sequential separation of each choice in time, strict intransitivity may never apply. This becomes evident when we leave the timeless world of neoclassical economics to the real world with historical time.

<sup>12</sup> This argument is redolent of the so-called Duhem-Quine thesis, which claims that it is generally impossible to falsify any single hypothesis because we always have to adopt additional hypotheses in the analysis of any set of observations (Harding 1976). Consequently, we can never be sure that the main hypothesis is being targeted and tested on its own, and that other auxiliary hypotheses are not complicating the picture.

<sup>13</sup> Several important authors, from Simon (1986, p. S222) to Field (2001, p. 6), mistakenly confuse tautological with non-falsifiable propositions.

<sup>14</sup> Indeed, it is widely accepted in the philosophy of science – including by Popper – that some unfalsifiable propositions are necessary for science itself. These include the principle of determinancy (every event has a cause) and the assumption of the uniformity of nature. Without these prior assumptions, science is impossible.

rationality hypothesis by itself is rather weak. To make it yield interesting implications, we need to add auxiliary assumptions.'

The notion of utility maximization is so capacious that it goes beyond the parameters of human decision. Experimental work with rats and other animals (Kagel *et al.*, 1981; 1995) has 'revealed' that animals have downward-sloping demand curves, supposedly just like humans. Becker (1991, p. 307) proposes that: 'Economic analysis is a powerful tool not only in understanding human behaviour but also in understanding the behavior of other species.' Similarly, Gordon Tullock (1994) has claimed that organisms – from bacteria to bears – can be treated as if they have the same general type of utility function that is attributed to humans in the microeconomics textbooks. Utility maximization is applied to humans in all forms of society since the origin of our species, and to a large portion of the animal kingdom as well. Seemingly, we now have 'evidence' of the 'rationality' of everything in evolution from the amoeba onwards. As a consequence such assumptions are telling us very little about what is specific to human nature and human society.

Arguably, human societies are partly differentiated from other animals in terms of developed institutions and cultures. The authors cited in the preceding paragraph thus demonstrate that these distinctive elements are effectively separated from the utility-maximizing picture of 'rational economic man.' Consequently 'rational economic man' bears no mark of any specifically human culture or institution. The causal mechanisms through which culture and institutions mould and constrain human agents remain unexplored in their paradigm. Human psychology is likewise neglected. Essentially, there is no adequate and substantial theory of *human* agency at the core of the standard theory. It tells us nothing of significance that is specific to the human psyche, human interaction, human nature, or human society. With respect to specifically human characteristics it is causally vacuous. Its very weakness, when applied to the human domain, stems from its excessive scope.

The non-falsifiability of the concept of rationality-as-behavioural-consistency-or-utility-maximization sustains an *epistemic* critique. It does not clinch the matter. One has also to consider the *theoretical* limitations of this stance. Here rationality-as-behavioural-consistency-or-utility-maximisation falls down for at least two reasons. First it neglects the problem of *explaining the causes* of behaviour. Second it fudges the question of the individual *development* of capacities and dispositions.

In a prominent defence of rationality, Richard Posner makes his neglect of psychological or other causes of behaviour explicit. Posner (1980, p. 5) sees the 'rationality of 'economic man'' as 'a matter of consequences, not states of mind.' In discussing 'economically rational' human agents he declines 'any statement about their conscious state ... behavior to an economist is a matter of consequences rather than intentions' (p. 53 n.). Here the problem of explaining behaviour, by reference to psychology or other matters, is openly abandoned.

For related reasons, claims that there is an evolutionary basis for utility maximization (Robson, 2001; Gintis, 2006, p. 17) do not pass muster. It is insufficient to show that the behavioural outcomes of evolution are consistent with some utility function. Ultimately this claim is trivially true, because one can always find a function that fits. One has to show that utility maximization is useful causal account of behavioural motivation. This is problematic, for reasons elaborated below.

Indeed, it is rather odd to claim simultaneously that evolution has produced individuals that maximize utility and are also capable of altruism, as a consequence of inclusive fitness or whatever. Altruism is typically defined as costly for the individual concerned but beneficial for others. This sits uneasily with a utilitarian framework, and consequently the definition of altruism is out-of-equilibrium and has to be constantly clarified (e.g. Bowles and Gintis, 2011). Utilitarians working in an evolutionary framework might awkwardly depict altruism as simultaneously involving a fitness cost and a utility gain for the agent, to preserve the near-vacuous dogma that all agents are utility maximisers.

Other defenders of rational economic man – notably Becker (1996) – treat the individual 'as if' she is born with a sophisticated but fixed meta-preference function. The process of human development is then regarded as a matter of gradually acquiring information about underlying 'true' tastes. Sure enough,

some such meta-preference function can always be stretched and twisted to fit the data. But as an account of the developmental process it is untenable. The fixed meta-preference function has no place in our current understanding of the neural system. Although many dispositions are inherited biologically, our further development from birth depends on the formation of many neural structures and connections, which are contingent on our environment and our past development. Although there is dispute concerning the details, psychologists and neuroscientists agree that there is considerable flexibility and plasticity in the developing brain (Penn and Shatz, 1997; Marcus, 2004; Sarnecki, 2007). This neural flexibility and plasticity goes against the idea of an entirely inherited and fixed meta-preference function.

Placed within some versions of modern game theory, the 'as if' argument is stretched beyond the limits of credulity. It is not simply assumed that agents act 'as if' they are rational, but also that they act 'as if' they consider the rationality of others, and 'as if' others respond rationally with such common knowledge (and so on ...), somehow without necessarily making any assumptions about their deliberative behaviour. Retaining the 'as if' argument in this context requires us to treat individuals as capable of emulating incredible super-calculators with unbounded cognitive capacities, without any consideration of how they would manage to do this.

Past economists have tolerated the 'as if' neglect of real phenomena, but it no longer satisfies scholars in this new age of exploration for evolutionary understandings of origin and development. We are interested specifically in the human mind and human social organization. We obtain little insight in this respect from overly-capacious and unfalsifiable principles that apply to any organism or behavioural entity.

On the basis of experimental evidence, some neuroeconomists (Platt and Glimcher, 1999; Glimcher *et al.*, 2005) make the strong claim that the utility function exists as a physiological reality inside the brain. This claim is scrutinized by Jack Vromen (2010), who argues that at best the neurological evidence exhibits consistency with the predictions of expected utility theory. There is no evidence of actual computation of utility. Given the argument here that any observed outcomes can be made consistent with some utility function, the consistency claim is hardly powerful or surprising. But existence claims are unsupported. After an extensive review of the evidence, Colin F. Camerer, George Loewenstein and Drazen Prelec (2005, pp. 54-55) are also sceptical of the claim that neuroscience supports a standard model of rational choice. The evidence that carries some weight relates to simple decisions only, not the 'abstract, complex, long-term tradeoffs which are the traditional province of economic theory.'

Overall, the long debate over whether behaviour is 'rational' has generated more heat than light. Sometimes the antagonists have misunderstood one another, particularly by confusing the falsifiable notion of payoff maximization with the unfalsifiable propositions of utility maximization or behavioural consistency. Since 1990, many leading members of the economics profession have abandoned payoff maximization. Yet the credo of rationality is preserved in the empty mantra of utility. In these terms it tells us very little. Faced with this explanatory agenda, 'rationality' in the broader sense of utility maximization is but a word of little consequence. By contrast, payoff rationality is more meaningful. But it turns out to be wrong.

The important task is to understand the nature and evolutionary origins of our human dispositions. Both genetic inheritance and cultural transmission are relevant to this quest. To understand the motion of the planets or the nature of matter is to comprehend the structures and forces that lie behind events, not to imagine spirits or gods that create every eventuality. To understand human nature and society is to appreciate human dispositions and interactions, not to fit all observations of behaviour to imagined mathematical functions of ever-expandable correlative capacity. Rationality in the broader sense serves an ex-post rationalization – rather than a materially-grounded causal explanation. A utility function may serve a limited purpose as a formalized preference ordering. Such formal constructions have some benefits. They can be useful shortcuts for modelling or explanatory purposes. But they do not enhance

our understanding of human motivation. Utility theory is an elegant way of summarizing what we don't know about human psychology.

Q: Why did the chicken cross the road? A: To maximize its utility. Some economists may be satisfied with this answer. But it tells us nothing about chickens, roads, specific motives, developmental histories, or detailed causal mechanisms. We should also be dissatisfied with summarizing all the complexities of human motivation in terms of a relatively simple preference function. As Sen (1977, pp. 335-6) has famously argued:

A person is given a preference ordering [that] is supposed to reflect his interests, represent his welfare, summarize his idea of what should be done, and describe his actual choices and behaviour. Can one preference ordering do all these things? A person thus described may be 'rational' in the limited sense of revealing no inconsistencies in his choice behaviour, but if he has no use for these distinctions between different concepts, he must be a bit of a fool.

In sum, a problem with the standard rationality assumption is not that it lacks empirical correlation, but that they could cover every conceivable decision situation and every possible causal mechanism underlying choice. Insofar as there may be common features of every decision situation then it may be possible to extract universal and meaningful propositions. Nevertheless, some important and specific features or causal mechanisms may be excluded by concentrating solely on the common features of every decision situation. In fact, the degree of universality involved is so great that it goes beyond the parameters of mere human decision.

For the neoclassical economist, the fact that utility theory can 'explain' a wide variety of types of economic behaviour is regarded as a strong vindication of this general approach. I take a different view. First, the sheer generality of a theory tells us nothing of its explanatory power. We could conceive different general theories, such as that we all are programmed by aliens from outer space, or that we are all pawns of God. These would be quite general in their scope and could be applied in principle to any behavioural manifestation. But we would rightly be sceptical of their explanatory value. A theory does not explain anything unless it points to an underlying causal mechanism. In the case of individual behaviour, explanations must thus relate to the known mechanisms of the human psyche and human interaction and draw upon psychology, anthropology, sociology and other disciplines. This is precisely what the neoclassical advocates of utility theory refuse to do. They take the utility functions as given and give the job of grounding them theoretically to somebody else. By this refusal they indicate that utility theory itself cannot provide a real explanation.

Arguably, human societies are partly differentiated from other animals in terms of developed institutions and cultures. If utility maximising behaviour not confined to humanity, then these differentiating elements are effectively absent from the universal picture. Whether true or false, this picture can tell us little of importance about historically specific human cultures or institutions. That is the unintended achievement of the exponents of ubiquitous rationality and economic imperialism. The causal mechanisms through which culture and institutions mould and constrain human agents remain unexplored in this paradigm. Essentially, there is no adequate and substantial theory of human agency at the core of the standard theory. It tells us nothing of significance that is specifically about the human psyche or about human interaction. Outside the realm of the universal, no particular causal mechanism is identified by the theory. With respect to specifically human characteristics and specific human societies, it is causally vacuous. Its very weakness stems from its excessive universality. Indeed, to attain the status of universality it has to be evacuated of much of its real content.

### 3. Weak criticism and false approval

But many critics of mainstream economics have taken a different line of attack. For example, the Post Keynesian economist Alfred Eichner (1983, p. 211), complained that the core assumptions of mainstream economic theory 'have yet to be empirically validated' and that they have 'no empirical counterpart in the observable world'. However, the problem with these assumptions is *not* primarily their lack of empirical corroboration. It is that they are vessels into which *any* empirical content can be filled. The problem with the theory is *not* that it lacks empirical validation but that *any* conceivable fact about behaviour, from church attendance to suicide, can be fitted into the theory.<sup>15</sup>

Just as the critics of neoclassical theory wrongly claim that its basic postulates have been falsified, its exponents misleadingly claim that they have been rigorously confirmed. Jack Hirshleifer (1985, p. 59) went so far as to write: 'Ultimately we must be ready to abandon the rationality paradigm to the extent that it fails to fit the evidence about human behavior.' But this apparent concession to empirical confirmation in fact conceals a methodological misunderstanding. Hirshleifer did not have to worry, because no conceivable evidence can 'fail to fit' some version of the theory. Both Hirshleifer and the critics of the rationality paradigm share the flawed supposition: that evidence can in principle refute the theory. Both supporters and critics of neoclassical theory have perpetuated the myth that it is susceptible to decisive empirical testing.

As a result, the mainstream theory is not wrong because it is empirically inaccurate. It is not unrealistic in the sense that it fails to fit the data. Any data can be fitted into it. Hence no data can refute the theory. It cannot be displaced simply by an appeal to the evidence. The experimental evidence of preference reversals and other choice 'anomalies' may lead us to search for a different and better theory, but it does not in principle refute the old version based on utility and rational choice.<sup>16</sup>

Critics such as Eichner (1983) based their criticism on an untenable and empiricist view of science that denies that some non-falsifiable and 'metaphysical' assumptions are *essential to any science*. In fact, all sciences depend upon some propositions that are untestable. No theory can be composed entirely of empirically validated elements. Prior concepts are required to make sense of any fact. These prior concepts cannot all be 'tested' empirically. In any case, any 'test' itself relies on prior concepts or categories. As a result, all sciences must unavoidably make extensive use of some untestable and metaphysical assumptions.

For this reason, the empiricist criticism of mainstream economics is untenable. In practice, their denial of the essential role of non-falsifiable assumptions in any theory would disable any of their own attempts at theoretical construction. Given that it is practically impossible to test all assumptions, any theoretical construction would reveal hidden, 'ad hoc' assumptions, privileged to lie beyond empirical test. For reasons outlined above, every theory must involve some untestable assumptions. Hence any theory built on the claim of complete testability would be highly vulnerable to critique by its own canon.

But this does not mean that 'anything goes' and that all criticisms are disabled. There are powerful theoretical criticisms of the rationality assumption. Essentially, the theory lacks adequate theoretical concepts to discriminate, understand and properly explain key phenomena. A problem with the standard assumptions of rationality and expected utility maximisation is their lack of specific theoretical and conceptual content, pertaining to specific causal mechanisms involved in the human psyche and in the structures of specific real world economic institutions.

To repeat: the empirical evidence is valuable and important, but it cannot be used to show that the theory is false. In recent years, there have been attempts to apply models of rational, utility maximising behaviour to a wide variety of phenomena, even beyond the sphere of commerce and markets. Models of utility-maximising behaviour have been applied to politics, marriage, religion, suicide, and much else.

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<sup>15</sup> See Azzi and Ehrenberg (1975) and Hammermesh and Soss (1974).

<sup>16</sup> I am not arguing that evidence is unimportant. Although evidence cannot falsify the theory, the accumulated evidence may provide a context in which the theory is more readily questioned. See Loomes (1998, pp. 485-6).



Such attempts have been widely resisted. Many tried to defend their academic discipline from the 'economic imperialism' of rational choice models. However, the widespread failure to recognise the non-falsifiability of 'rational' maximising behaviour has weakened many such counter-arguments. They appealed to evidence: it was mistakenly argued that rational choice models did not fit the facts. On the contrary, models of utility-maximising behaviour can always be adjusted to fit the facts. The attempt to resist the incursions of rational choice theory by claiming otherwise was bound to fail. In this instance, appeals to evidence cannot win.

In development economics, for example, there was a debate in the 1970s over whether peasants were or were not 'rational'. Critics of this idea appealed to 'evidence' of 'non-rational' behaviour, without realising that no evidence can strictly falsify the theory. With opponents weakened by their own theoretical position and methodological misunderstandings, the rational choice theorists seemed to win the argument (Popkin, 1979). Similarly weak defences were evident in sociology and political science, as they too were invaded by rational choice theorists. Again and again an attempt was made to resist the incursions of utility and rational choice, on the grounds that its assumptions are not 'realistic'. Such attempted defences against the invasion of rational choice theory are methodologically flawed and ultimately doomed.

The moral here is that mistaken claims concerning the testability of rational choice theory led its opponents to attack it with weak arguments. It would have been much more fruitful if both sides had admitted that the theory was falsifiable and then debated its explanatory value in specific circumstances. Instead, these controversies were entirely confined to claims and counter claims concerning empirical validation. At that primitive level the issue is simple: the assumptions of utility theory cannot be falsified.

#### 4. Conclusion

But ironically, it has become fashionable for mainstream economists to question some of these core assumptions. Perhaps because mainstream economists have lost the capacity to police their own disciplinary boundaries, in search of a new separate identity they have begun to question their own *raison d'être*. As Kyriakos Kontopoulos (1993, p. 90) has pointed out: 'Ironically, economists become less economic at a time when sociologists seem to become enamored with rational choice theory.' Accordingly, some economists are now deconstructing rational economic man. As economist Robert Sugden (1991, p. 783) put it:

There was a time, not long ago, when the foundations of rational-choice theory appeared firm, and when the job of the economic theorist seemed to be one of drawing out the often complex implications of a fairly simple and uncontroversial system of axioms. But it is increasingly becoming clear that these foundations are less secure than we thought, and that they need to be examined and perhaps rebuilt.

One reason for this change of heart is the rise of game theory. In certain types of game the very definition of rationality becomes problematic. Nevertheless, the response of mainstream economists to these problems has largely to become immersed in the technicalities, rather than to give the economic agents at the core of the theory of human behaviour some real institutional and cultural flesh and blood. Some still cling tenaciously to the principles of rationality, in a manner that is reminiscent of Ptolemaic astronomers, fitting the evidence of the apparent circular movements of the stars into complicated models (Koestler, 1959). Others are not inclined simply to 'save appearances'; they express their misgivings but seem unaware where to look to find an alternative paradigm.

For some, the move to game theory has led to the questioning of core assumptions. For others it has reinforced the idea that economics itself is a formal game, with little connection to reality. If a theory makes no claim outside a single domain, then there is no aim to use the theory to explain other real world

phenomena. The interest in the theory is typically in its mathematical content, rather than its usefulness to help understand reality. Accordingly, there is a move away from former attempts to build a universal theory (which turned out to be unfalsifiable), to the building of exemplifying theories that are designed with limited empirical scrutiny in mind.

Rational choice theory has invaded social sciences such as sociology with its devotees declaring triumphantly that it can 'explain' nuanced social phenomena such as altruism, honour, trust and duty. The point argued here is that it is ineffective to counter these allegations with denials of their *empirical* validity. This is a weak response because, in principle, any manifest behaviour can be fitted into the rational choice framework. However, once the unfalsifiable nature of rational choice theory is understood, we can meet the triumphant claims with the polite response: 'So what. We know *in advance* that any behaviour can fit the theory.' We are then able to move on to the more important question, concerning claims of explanation and their derivation. Explanation, we insist, is much more than empirical correlation. And a perfect empirical fit does not necessarily imply any explanation of the causal processes underlying behaviour. The debate then moves on to the vital question as to how such causal explanations can be obtained.

Economic and social theory can advance only if it takes account of historical, cultural and institutional specificities (Hodgson 2001). The crusade for rational choice theory in modern social science is part of a wider project to develop a universal theory of all social phenomena. The problem with such a theory is that, in its excessive quest for generality, it will fail to focus on the historically and geographically specific features of the socio-economic systems that we wish to study and understand. As long as social theory is confined to generalities it will remain highly limited in dealing with any specific socio-economic system, including the one in which we live. What is needed is a theory that is far more sensitive in this regard.

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# An Evolutionary Efficiency Alternative to the Notion of Pareto Efficiency

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## Abstract

The paper argues that the notion of Pareto efficiency builds on two normative assumptions: the more general consequentialist norm of any efficiency criterion, and the strong no-harm principle of the prohibition of any redistribution during the economic process that hurts at least one person. These normative concerns lead to a constrained and static notion of efficiency in mainstream economics, ignoring dynamic efficiency gains from more equal allocations of resources. The paper argues that a weak no-harm principle instead provides an endogenous efficiency criterion, which shifts attention away from equilibrium analysis in hypothetically perfect markets towards an evolutionary analysis of efficiency in real-world, non-equilibrium markets. Moreover, such an evolutionary notion of efficiency would be less normative than the Paretian concept.

**Keywords:** evolutionary efficiency, Pareto efficiency, equity, redistribution

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## Introduction

The transition from neoclassical economics to the mainstream economics of today may be perceived in a variety of ways. One way, admittedly a simplified one, is by focusing on the major shifts that have occurred in micro and macro economics over the past three decades as well as the emergence of the meso level in economic analysis connecting the micro and macro level, and, so important in evolutionary economics. For macro economics, the shift may be summarized under the broad label of the New Growth Theory, involving concepts such as endogenous growth (Lucas, 1988; Aghion, Caroli, and García-Peñalosa, 1999), increasing returns (Romer, 1986; Acemoglu, 1996; Rima, 2004), and the increasingly shared recognition that the fallacy of aggregation seriously challenges the construct of a representative agent (Colander et al., 2008). Hence, in the era of new growth thinking, the old neoclassical assumptions of constant returns to scale, a necessary trade-off between equity and efficiency on a well-defined possibilities frontier, and free markets as a precondition for an efficiency optimum, are now increasingly recognized as no longer necessarily valid (Bénabou, 1996). Or, to put it stronger in the words of Samuel Bowles and Herbert Gintis (2000: 1425) these Walrasian assumptions “should be shelved in the museum of utopian designs”.

In micro economics, the move beyond neoclassical economics is driven to a large extent by developments in behavioural and experimental economics (Smith, 1991; Kahneman, 2003), which in turn have spurred the recognition of evolutionary economics and game theory (Hodgson, 2002; Witt, 2003; Gintis et al., 2005). The insights from these developments have proven the old assumptions underlying welfare economics to be mistaken. Economic agents do not always pursue their self-interest (Fehr and Gächter, 2000), they appear to care for efficiency as well as equity, even helping the worse-off at own cost (Charness and Rabin, 2002; Engelman and Strobel, 2004; 2006), attaching intrinsic value to equality (Lutz, 2001) and

treating a particular level of rewards as entitlements (Falk et al., 2006). Their behaviour is influenced by various psychological and social effects which give their actions meaning (Klamer and McCloskey, 1995; Akerlof and Kranton, 2008), while incomplete contracts, bounded rationality and principle-agent problems lead to impacts of uncertainty, interactions, transaction costs and power on efficiency (Bardhan, Bowles and Gintis, 2000; Witt, 2003; Bandiera et al., 2005; Smith et al., 2006), and agents appear to behave along various ethical routes next to utility maximization, in particular following the principled morality of deontology (White, 2004) as well as the contextual morality of virtue ethics (McCloskey, 2006; van Staveren, 2007).

Finally, the emerging field of meso economic analysis, in particular in the areas of technological change, global value chains, networks, clusters, and dynamic poverty analysis has shown the importance of evolutionary economics with concepts such as endogenous preferences, path-dependence, and endogenous change (Witt, 1994; Bowles, 1998; Gereffi, 1998).

Given this transition from a narrow neoclassical to a pluralist mainstream economics, in which many old assumptions of welfare economics have been shown to be empirically mistaken and theoretically inconsistent, it is surprising to see that the major economic evaluative criterion of neoclassical economics – Pareto efficiency – is still the dominant criterion of efficiency in most of economic research, assessing strictly normatively whether no one can be made better off without making anyone else worse off. Whereas in evolutionary economics, Pareto efficiency is explicitly rejected and replaced by a much more general view of efficiency as emerging in a dynamic process of innovation and adaptation but as not completely achievable due to inherent limitations to the economic process such as uncertainty, bounded rationality, path dependence, and norms that express cultural traditions and power relations. This evolutionary view of efficiency needs further elaboration. Valuable attempts have been done to operationalize an evolutionary efficiency concept in relation to communication (Dudley, 1999; Dolfma, 2005). I would like to take this up more generally but first like to argue that such elaboration must not ignore the normative foundations of efficiency notions in general and the Pareto criterion in particular. Amartya Sen already recognized that the widespread belief that efficiency is a value-neutral evaluation criterion has resulted in a reductionist efficiency analysis limited to the Pareto criterion, which, he argues, is “a very restrictive informational basis for welfare economics” (Sen, 2008: 623)<sup>1</sup>. The extensive use of the Paretian criterion may be explained by its intuitive attractiveness: as long as it is possible to improve the situation of one or more persons without affecting the situation of anyone else, resources have apparently not been used to their full potential. But this is a static view, belonging to the Walrasian approach to economics (Hodgson, 1993). Bowles and Gintis (2000) have concluded their critique of Walrasian economics by reminding their readers of recent research indicating that some redistributive policies are likely to increase allocative efficiency in a large variety of markets. As a consequence, they have pointed out, “the canonical efficiency equity trade-off – whose ineluctable logic is given prominent place in most introductory texts – may be up for reconsideration” (Bowles and Gintis, 2000: 1427).

In this paper, I would like to follow up on this point, arguing, first, that the seemingly neutral criterion of Pareto Efficiency is unable to reflect a social optimum and to select among multiple equilibria, precisely because it relies upon a strong no-harm principle. Second, I would like to attempt to provide some flesh to the bones of the evolutionary idea of adaptive efficiency, as an imperfect and endogenously emerging optimum. I do recognize, however, that this is a preliminary attempt and still far from a well-developed evaluative criterion. This alternative relies on a weak no-harm principle, which will allow for a selection of the least inefficient equilibrium through analysing the nonlinear dynamic relationship between the distribution of factor

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<sup>1</sup> For example, the *Journal of Economic Literature* classification system distinguishes code D63 for “equity, justice, inequality, and other normative criteria and measurement” (emphasis is mine), from code D61 “allocative efficiency; cost-benefit analysis”, implying that efficiency would be a non-normative evaluation criterion as compared to the criteria referred to in code D63.



inputs on the one hand and efficiency gains on the other hand. In this way, efficiency becomes endogenously determined without a preference for the status quo distribution or any need for a prohibition to compare outcomes between individuals. The argument will be methodological rather than technical or empirical, in order to show that the most urgent problem is not so much the definition of the optimum equilibrium (see for an example of this, Rehme, 2007) or empirical support (see for an example of this, Banerjee and Duflo, 2003), but the commonly adhered to view that Pareto efficiency is a value neutral evaluation criterion that has common sense logic and which therefore requires no alternative<sup>2</sup>.

The section below will explain the normative foundations of the Paretian criterion. The next section will argue why this leads to a flawed efficiency criterion, whereas the following section will discuss the example of the efficiency of land productivity to illustrate my argument. Finally, the paper will argue that from an evolutionary perspective, efficiency may be defined in an endogenous way relying on a weak moral norm rather than the strong moral norm that so much constrains the Paretian criterion.

### **The normative foundations of Pareto efficiency**

Here, I will not discuss the well-known literature pointing out that the conditions for competitive markets are almost never met in real world markets. Indeed, real world markets tend to exhibit economies of scale, externalities, barriers to entry, imperfect and asymmetric information, incomplete contracts, principle-agent problems, as well as transaction costs, uncertainty, market power and asymmetric bargaining power, and last but not least, a variety of formal and informal institutions. Hence, competitive markets are not likely to reach Pareto efficient solutions without emerging institutions and intervening policies. Nor will I review the literature that has critiqued the Pareto criterion for ignoring equity as a valuable evaluation criterion on its own. The point that a Pareto optimum may well allow for dramatically unequal outcomes has been well made by others and is now generally accepted (Sen, 1987; Lutz, 1999; Schultz, 2001).

Instead, I would like to argue that the problem with Pareto Efficiency is not merely that it is so difficult to reach or that it can occur at extreme levels of inequality, but rather that the Pareto criterion takes on a particularly strong normative position while pretending that it is a neutral criterion, as opposed to evaluations of equity, and that this prevents it from selecting the most efficient equilibrium among multiple equilibria. The generally accepted trade-off tends to reinforce the criterion's presumed neutral status, shifting efficiency to the 'positive' side and equity to the 'normative' side of welfare economics. However, the placement of Pareto efficiency on the 'positive' side of economic evaluation hides two normative foundations.

### **Consequentialism**

The first normative foundation of the Paretian evaluation criterion is concerned with its location of the good as lying in an outcome. This is an expression of consequentialism, which is an ethic in which one evaluates the good not by principles or processes but by outcomes. As David Hume already recognized, any type of consequentialism implies an 'ought' position, and Hume therefore rightly located consequentialism on the normative side of the Cartesian positive/normative dichotomy.

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<sup>2</sup> I fully agree, though, that more work needs to be done on theoretical proof, modeling, and empirical testing of the nonlinear relationship between factor inputs and efficiency measures such as factor productivity or GDP growth.

The ethics of consequentialism necessarily follows instrumental reasoning, as Jean Hampton (1992) has explained. Instrumental reasoning, 'do X in order to achieve Y', implies a consequentialist norm. For the Paretian criterion, this norm is expressed in its definition: *allocate resources in such a way (X) that no one can be made better off without making anyone else worse off (Y)*. Now, what is crucial to instrumental reasoning, Hampton has argued, is that it cannot deduce consequentialist norms from non-normative foundations: its norms are inescapably founded on moral reasoning: "... we must conclude that one cannot reduce normative statements to non-normative elements: or alternatively, we cannot hope to build authoritative prescriptions from entirely natural components – unless, of course, the natural is understood to include the normative, as those who deny the fact/value distinction insist. But for those who maintain that distinction, trying to 'naturalize the normative' is impossible" (Hampton, 1992: 234).

Is such normativity indeed the case for Pareto efficiency? If not, what might be possible non-normative consequentialist norms for Pareto Efficiency? Here, I will briefly consider two possible candidates: a natural norm and a norm arising from logic. A natural norm might be found in the condition of scarcity. Although scarcity is a relative notion in economics, it is generally regarded as ultimately driving all economic behaviour. Hence, scarcity seems a *natural* ground from which rationality, competition, and innovation emerge. Pareto efficiency, therefore, seems to follow from the natural condition of scarcity: given the nature of resources as being limited, efficiency appears a *natural objective* to pursue. But scarcity is not so natural as it seems. Scarcity is for a large part constructed by human behaviour, through the endogenous creation of wants leading to neighbourhood effects and bandwagon effects, as Thorstein Veblen (1931) recognized; through strategic behaviour leading to collusion and market power as analyzed by Joan Robinson (1969); and through manipulation of relative scarcity by the accumulation of intangible resources, such as information, strategic skills and the power to control (Webster, 1999). As a consequence, scarcity is not so much a purely natural phenomenon but to a considerable extent a *social and cultural construct*, as economic anthropologists have pointed out for decades (see, for example, Mary Douglas and Baron Isherwood, 1996, on moral goods). Moreover, even if scarcity was given by nature, it is surely not the only condition to which economic behaviour is subjected: economic agents also have to counter uncertainty, control risk, process more information than they can handle, seek cooperation, etc. Singling out, without justification, scarcity from the varied list of conditions that all provide key parameters for economic behaviour, implies a normative stance. For these two reasons, scarcity cannot serve as a non-normative consequentialist norm for Pareto Efficiency.

Another possible candidate for a non-normative norm for Pareto efficiency is logic. For example, in a simple equation of positive real numbers,  $a + b = c$ , in which  $a < c$ , logic informs us that  $b > 0$ . Hence,  $b$  is the *necessary* element to complement  $a$  in order to get  $c$ . However, logic is a form of reasoning and not of justification. It may just as well be that not  $c$  but  $a$  is put on the right-hand side, so that the equation now reads  $c - b = a$ . Now, a particular economic action,  $a$ , becomes the right-hand side variable, or desired outcome, which may be attained by reducing efficient outcome  $c$ , by the size of  $b$ . In other words, logic cannot form a non-normative basis for the Pareto criterion because it is not reason but judgment that assigns whether  $a$ ,  $b$ , or  $c$  should be placed on the right hand side of the equation as the desired outcome.

Nature and logic, two possible candidates for a non-normative consequentialist norm for Pareto efficiency, appear not to be neutral at all but are biased by power and judgment. This brings us to the intuitive ground for any efficiency criterion, namely some form of prudent resource-use, as opposed to a waste of resources. This, of course, is a normative criterion. The consequentialist norm of the Paretian criterion therefore is indeed a *moral* norm, because it judges less waste of resources to be more desirable than more waste of resources. This moral norm of the minimization of waste underlies every concept of efficiency – the Paretian definition is no exception to this. The way in which this norm is operationalized, however, is not the

same for all possible concepts of efficiency. In the Paretian criterion, the norm of minimization of waste is defined in terms of maximizing total utility without redistribution, while the Arrow-Debreu proof fills in a particular way in which resources may be allocated to reach this, namely through free markets. The consequentialist norm underlying Pareto efficiency, hence, is narrowed down to a situation of (X) as a free market equilibrium and (Y) as maximum total utility without redistribution. Obviously, this leaves open the possibility that some Pareto Efficient solutions come about not through free markets but through government intervention, collective action, or the exercise of power. The Pareto efficient free market equilibria, however, often imply multiple equilibria so that  $(x_i, y_i) \rightarrow n$  for  $i = 1, \dots, n$ , which are all efficient given a particular initial distribution, and the Paretian criterion is not in itself capable of selecting the optimum among these. In fact, all these free market equilibria represent what Roland Bénabou and Jean Tirole (2006) have labelled “a belief in a just world”, the belief that people generally get what they want through effort and hard work when they are free to provide for themselves. Hence, the Arrow-Debreu set of Pareto efficient equilibria seem to imply, inadvertently, yet another form of consequentialism, namely the belief of justice being done when agents are free to choose.

## Deontology

The second normative foundation of the Paretian criterion derives from a very different ethic than consequentialism, namely deontology. Deontology is a principled ethic in which the good is defined not by outcomes but by rights, duties, laws, and other principles. The deontological principle of Pareto Efficiency is expressed by restriction (Y) that no one should be made better off *at the expense of anyone else*. This particular deontological principle is a strong no-harm principle. It strictly forbids any redistribution between individuals after the initial distribution of endowments, even when doing so is likely to improve total outcome, as Pigou already noted in 1929. So, deontological restriction (Y) limits consequentialist allocation (X) to the sub-set of non-redistributive allocations, that is, to free market allocations.

Lionel Robbins' (1952) positivist insistence in 1935 that individual utility is purely subjective and cannot be compared between individuals has blocked the grounds for redistribution in efficiency analysis in neoclassical economics. Happiness economics, however, provides a response to the information problem of incomparability by providing an inter-subjective measure that allows for a comparison of individual satisfactions (Frey and Stutzer, 2002). This, in turn, allows for redistribution of marginal satisfactions until these are equal, while increasing total satisfaction, and hence, efficiency. However, such a purely subjective approach does not deal with the problem of perverse preferences, adaptive preferences, and other problems related to the reliance on purely subjective wellbeing information, so that an efficient allocation equalizing marginal happiness may still not be the most efficient allocation possible.

The strong no-harm principle is often ascribed to John Stuart Mill, but he did not clearly distinguish between a strong and a weak form of the no-harm principle. On the one hand, he defended individual liberty against interference by the state, whereas on the other hand he supported redistribution of resources to the poor and to women, for example in his support for the Poor Laws (1917: 754-7). Others, like Isaiah Berlin (1969) and Sen (2002), have discussed the difference between the strong and the weak no-harm principle in terms of negative liberty ('freedom from' interference) versus positive liberty (enabling disadvantaged groups to provide for themselves, to acquire 'freedom to'). The debate about negative and positive freedom and the strong and weak no-harm principle has not yet been resolved, while philosophers have proposed further distinctions of the no-harm principle (Ripstein, 2006). For this paper, the basic distinction between a strong and a weak no-harm principle is sufficient: a strong no-harm principle does not allow, as a matter of principle,

any redistribution even when it would lead to a net aggregate welfare increase, while a weak no-harm principle allows redistribution up to a certain point, such as the equalization of marginal utilities in the original theory of cardinal utility of Bentham.

Pareto Efficiency, hence, appears to take a clear normative position. On the one hand it expresses, like any other efficiency criterion, a consequentialist norm that judges waste as undesirable, while on the other hand it relies on a strong deontological norm that opposes any redistribution after the initial distribution, even when that would appear to increase efficiency in the dynamics of the evolving economic process.

### **The inefficiency of the Pareto criterion**

Some economists may be disturbed by the implication that Pareto efficiency does not appear to lie on the neutral side of a neat efficiency/equity trade-off, and that there is actually no neutral side at all to efficiency. On the other hand, most economists will perhaps not really feel troubled as long as the criterion does what it promises to do: evaluating states of economic affairs according to their relative efficiency. The problem, however, is that it does not perform this function very well, as evolutionary economists know so well. In the next section, I will show that the normative foundations of Pareto efficiency are part and parcel of this problem, so that the reasons why Pareto efficiency cannot be achieved in real world economies are not only practical – due to, for example, path dependency and bounded rationality – but also normative. This recognition is important for the development of an evolutionary alternative notion of efficiency, as I will argue.

### **Utility maximization: Desire fulfilment versus efficient resource-use**

Utilitarianism is a form of consequentialism, as utility maximization through Bentham's consequentialist norm of 'the greatest happiness for the greatest number' reflects a concern with outcomes. The choice for individual utility as the unit of measurement in welfare economics implies that it is not resource-use which forms the measure for evaluating efficiency. Rather, the assumption is that when utility is maximized, this can only mean that resources must have been used to their maximum, otherwise someone's utility could have been increased without hurting anyone else.

This assumption, however, is debatable because there is no one-to-one relationship between utility and resources. Mainstream economics has recognized that preferences may include psychological desires, relying on feelings of jealousy and other emotions, or on status, leading to the consumption of positional goods as well as non-rival, non-excludable goods that are produced in households (warm glow feelings), communities (social capital) or by nature (a beautiful sunset). In other words, the space in which Pareto efficiency is measured is not the space of resources, but that of desire fulfilment, including desires that are only partly related or even completely unrelated to resource-use (enjoying listening to birdsong or taking pleasure in humiliating one's employees) as well as desires that are highly resource-intensive or even wasteful (status symbols and other positional goods) or preferences that are harmful for oneself but indulged in because of myopia, limited information, or weakness of will (from smoking to over-eating – also referred to as preference pollution by David George, 2001). As a consequence of the weak, irregular, or sometimes even absent relationship between resource-use and utility, utility maximization, like happiness maximization, does *not necessarily* imply that resources are being used in their most efficient way, not at the individual level, nor in the aggregate.

Considering, in addition, that interpersonal utility comparisons are not allowed but that only the sum total of utility matters with a *given initial distribution* of endowments, efficiency gains from redistribution away from those with low marginal utilities (the ‘haves’) to those with high marginal utilities (the ‘have-nots’) are ignored. Such redistributions, if allowed, are not only likely to increase total utility but also to increase the efficiency with which resources are used. This is because the ‘have-nots’ are likely to use resources in a more productive way (more needs-oriented) than the ‘haves’, who are more likely to waste resources on positional goods (more want-oriented)<sup>3</sup>. In agricultural economics this has become known as the inverse farm-size productivity relationship, in which small plots of land generate higher productivity than large plots due to very intense labour use, which outweighs any economies of scale of large plots. In other words, redistribution implies not only marginal utility gains but is also likely to induce more efficient resource use because progress can only be made through intensifying the only available non-fixed resource (labour).

### **Strong no-harm principle: Voluntary exchange versus efficient resource-use**

The belief in a just world, an expression of libertarianism, assumes that the status quo distribution of endowments is just, as long as individuals have acquired their endowments through voluntary acts such as exchange. This implies that when agents have agreed to an exchange, each of them must have made a gain, otherwise they would not have agreed to the transaction. Moreover, libertarianism implies that competition in free markets provides individual agents with the incentives to use resources efficiently thanks to the opportunity to make gainful exchanges. This link between incentives and free exchange is probably the reason why economists so widely support the narrow interpretation of the Arrow-Debreu proof. In this libertarian view, interference with agents’ free choices will generate disincentives, which in turn will reduce the efficient use of resources. More precisely, disincentives would induce the rich to reduce their production until their marginal earnings would equal the marginal tax rate they face, while the poor would reduce their production with the size of the subsidy they would receive. Hence, efficiency – measured as total production with available resources, rather than in utility terms – would go down with interference in free exchange. Unless, of course, redistribution would be neutral to the incentive structure, as proposed by the Kaldor-Hicks compensation in welfare economics through which winners compensate losers and still receive a net gain. But when losers have low bargaining power, which may occur even under democracy and competitive markets for those on the short side of markets (Bowles and Gintis, 2000; Walsh, 2003), it is very likely that only limited compensation, or no compensation at all will be made. So, the Kaldor-Hicks compensation is generally not feasible given an unequal distribution of endowments to begin with.

A deeper problem with the libertarian belief that free exchange ensures justice by providing the best incentive structure for efficiency to occur, is that it ignores the real world situation in which quite often some agents lack the endowments for any beneficial exchange – even in the absence of market imperfections. In other words, libertarianism assumes that exchange is by definition voluntary when not forced or constrained from outside. But voluntary exchange may also involve involuntary losses when there is too much imbalance

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<sup>3</sup> When using a social welfare function or applying a Kaldor-Hicks compensation in Paretian analysis, which allows for complementing equity concerns with the efficiency side of the criterion, shifts along the efficiency frontier will allow for redistribution, at the same level of efficiency. But such a utilitarian win-win situation is highly arbitrary: the shape of the social welfare function cannot be derived from individual utilities, as these are highly subjective and face the problem of what Sen called the Paretian liberal. And the Kaldor-Hicks compensation has its own problems, as it is not likely that redistribution will actually happen, given the power differences between winners and losers. The implication for efficiency remains problematic. How can we know that such redistributions in utilitarian terms will lead to resource efficiency? A social welfare function is likely to reflect political power, norms of fairness, or other social and political forces, which are not necessarily dominated by an objective to minimize the waste of resources. Hence, as long as Paretian analysis and social welfare functions remain in utility space, there is no convincing way to ensure that Pareto efficiency coincides with resource efficiency.

in endowments and opportunities, and hence, inequality in bargaining power between market parties. That is why genuine voluntary exchange can only exist when there is a feasible non-exchange option (Sen, 1981c; Walsh, 2003). Without such a fall-back, exchange of one's last resource or even of non-economic goods such as one's children or bodily integrity, will not be voluntary, but simply the only option available for short-term survival. So, paradoxically, voluntary exchange will only be voluntary with what Sen (1981c) has labelled a feasible option for autarky. Distress sales or underinvestment may be regarded by libertarians as voluntary in a static sense, but they undermine an agent's resource base, and hence, crowd out productive capacity in the long run. This is clearly not voluntarily chosen by agents while it is neither efficient in a dynamic sense, making people dependent on others or the state. Distress sales or underinvestment can only be prevented by trade-independent security, deriving from resources such as savings, wealth, community care, access to commons, public goods or welfare support. Most people who experience a disadvantaged exchange position have very few resources to provide for themselves, except their labour power. And even this may not be in demand, as it may be only potential rather than actual labour power, due to lack of nutrition and health (Dasgupta, 1993), or it may not earn sufficient market value to survive (Kurien, 1996), or a combination of factors including lack of aggregate demand keeping the demand for labour low at any wage rate (Walsh, 1996).

The libertarian strong no-harm principle, hence, will not necessarily result in efficient resource-use because free markets provide no guarantee for trade-independent security, without which incentives may be distorted. Agents with very limited endowments may sell their last assets, crowding out their productive potential for own use or exchange, or disabling their children's human capital formation, while those with abundant endowments may acquire factors of production in excess to what can be put to its most productive use. While at the same time, the thus acquired surplus by advantaged groups may serve to fulfil wasteful desires as well as enable them to accumulate power, which would further distort an efficient allocation of resources. That is why Aghion, Caroli and García-Peñalosa (1999: 1656-1657) have argued that efficiency requires not just a one-time redistribution, but sustained redistribution, calling for "...permanent redistribution policies in order both to control the level of inequality and to foster social mobility and growth." Therefore, only an institutional setting of markets that acknowledges basic entitlements or other mechanisms that prevent inequality-inducing accumulation will be able to reflect genuine free trade. Sen's capability approach moves in this direction but has not yet delivered an efficiency criterion that goes beyond the Paretian criterion (Sen, 2002)<sup>4</sup>.

In conclusion, the narrow interpretation of the consequentialist norm of efficiency and the strong no-harm principle have provided the Pareto efficiency criterion with a rigidity that not only reflects a clear normative position, but also, and partly because of this normative position, ignores many forms of inefficient resource-use. Hence, an alternative efficiency notion should break with these normative foundations in order to be able to incorporate a wide variety of dynamic sources of efficiency. This would lead to an evolutionary notion of efficiency. But before discussing that, I would like to go through an example of the dynamics of the efficiency of land productivity, which serves as an illustration of the multiple sources of evolutionary efficiency in which efficiency and equity are intertwined.

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<sup>4</sup> Sen (2002) has adapted the Pareto criterion to capabilities defined as 'weak efficiency of opportunity-freedom': "a state of affairs is weakly efficient in terms of opportunity-freedom if there is no alternative feasible state in which everyone's opportunity-freedom is surely unworsened and at least one person's opportunity-freedom is surely expanded" (Sen, 2002: 518). But this criterion keeps the strong no-harm principle in tact, and thereby its problems.

## An Example of the efficiency of land productivity

The following example draws on the literature on determinants of land productivity and serves to illustrate the various inefficiencies implied in the Pareto criterion. In this example, we assume a big landowner A and landless rural labour  $B_i$ , with  $i = 1 \dots n$ , a proportion  $\alpha$  ( $0 < \alpha < 1$ ) of whom are hired on-and-off as day labourers or seasonal workers, depending on agricultural production cycles and market demand for agricultural products. The rural wage rate is at subsistence level while labour supply exceeds labour demand, resulting in unemployment of the size of  $(1-\alpha)$ . A mainly grows cash crops for exports and buys part of the food for his own consumption on the market, which includes imported food. The labourers consume local and imported food depending on what is available on the market for their wages. The example now focuses on the use of the last hectare of A's land, which I will refer to as the marginal land. There is a free land market and the current situation of production by A, with the use of labour of  $\alpha B_i$ , is Pareto efficient in the sense that no one can be made better off without hurting either A or  $B_i$ . There may be, however, various sources of inefficiency that are not captured by the Pareto criterion in this situation. Some imply that the theoretical conditions for Pareto efficiency have not been met (in particular the presence of market failures), while others go beyond these and even hold under the restrictive assumptions for Pareto efficiency. The four examples below will only reflect the latter sources of inefficiency.

- a) *A may keep the marginal land unused because his marginal utility from signalling richness is higher than the marginal utility from additional earnings derived from the sales of crops, even when the marginal value of production exceeds the market price. This under-use of land arises from a preference based on status in a rural social context in which status is signalled by owning surplus land.*

Inefficiency arises from underproduction of crops due to informal institutions that result in wasteful preferences. This source of inefficiency was signalled several decades ago by the agricultural economist Margaret Reid (1943), who referred to decadent and luxury consumer patterns as implying waste of productive resources such as land. More recent research on psychological utility and endogenous preferences suggests that such unproductive uses of resources in traditional and modern societies are not uncommon (Bowles, 1998).

- b) *There is a principal-agent problem which induces A to supervise  $B_i$ , which leads to transaction costs.*

Labour productivity of  $B_i$  would be higher if workers would own the land, because they would have full control over the surplus value of their labour (Sen, 1981a; b; Taslim, 1989). Hence, the productivity of the marginal land is sub-optimal, *not* (only) due to lower investment in technology or material inputs such as fertilizer, but due to sub-optimal labour productivity and costs of supervision.

- c) *A may not hire additional  $B_i$  even though  $B_i$  is offering labour at or below the going wage rate, because A's capital/labour ratio is set in response to subsidized prices for fertilizer, irrigation, tractors and other investment goods. Hence, labour is under-used and the proportion  $(1-\alpha)$  of  $B_i$  that is not hired by A lacks the entitlements to provide themselves with a basic livelihood, let alone to invest in their and their children's human capital.*

The problem here is the state's agricultural policy that is biased by lobbying activities of big landowners. The capital subsidies are generally not available at the same favourable conditions to small scale producers, as van den Brink et al. (2006) have argued in a World Bank overview study on countries such as South Africa, India, and Brazil. "Large farmers are often well-organized and well-connected, and are able to lobby

governments for special tax breaks, subsidies, and other special distortions. The consequence of these distortions is invariably that they face lower effective capital costs relative to labour costs, and therefore over-invest in more machines that replace labour than they would have had they not been able to obtain the tax breaks, subsidies and cheap credit” (van den Brink et. al, 2006: 21).

- d) *Even without state subsidies, A's capital/labour ratio may be relatively high so that not all  $B_i$  available at market wages will be hired due to a choice of capital investment based on average land conditions, not the conditions of the marginal land. Marginal land of big landowners is likely to include forest, hillsides, dry areas, etc. which require a much higher labour intensity than is optimal given the average capital/labour ratio for A.*

The problem here is underproduction of crops in relation to the underused labour available in the labour market. The origin is not market failure but the size and heterogeneity of the land, combined with asymmetric bargaining power in factor markets, which induces a higher average capital/labour ratio than is socially optimal.

In all four cases, redistributive land reform would generate efficiency gains, even though making A worse off, by taking away a hectare of land (with compensation). If the marginal land would be redistributed over landless labour  $B_i$ , the total production of crops would increase, underemployment and unemployment would decrease, the average capital/labour ratio would come closer to its social optimum, with capital productivity, land productivity and labour productivity all increasing. The literature on the inverse farm-size productivity relationship points out that small scale farms are more productive than large scale farms through the high labour productivity on such family farms (van den Brink et al., 2006). Labour is used to such a high extent that some even speak of self-exploitation (Barrett, 1996) – but when the reservation wage is very low and current and future livelihood is insecure due to uncertainties around food prices, child survival, non-farm employment and migration, maximizing labour productivity by using all available farm labour to its physical limits is a rational strategy at the individual level<sup>5</sup>. But such behaviour it is not an optimal strategy, as evolutionary economists have recognized for similar situations (see, for example, Pagano, 2000).

In conclusion, the example illustrates that although market failures often play a role in the inefficiency of large scale production, market failure is not necessarily the main cause. The underlying problem can well be a highly unequal distribution of land and its impacts on innovation and productivity – precisely the issue that the Pareto criterion excludes from efficiency evaluations through its strong no-harm principle.

### **e notion of efficiency**

The consequentialist norm of efficiency as the minimization of waste was recognized already by Adam Smith, Karl Marx and Thorstein Veblen. Vivian Walsh (2000: 21) reminds us that Smith “is savage when he sees the surplus being squandered by the profusion of the great” and he also reminds us that Marx’ concept of exploitation included the recognition of waste of the surplus on luxury when it is shifted from labour to capital. While Thorstein Veblen (1931: 126) has criticized the waste of conspicuous leisure and consumption, arguing that “the utility of both alike for the purposes of reputability lies in the element of waste that is common to both. In the one case it is a waste of time and effort, in the other it is a waste of goods.” The explicit definition

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<sup>5</sup> An important note to this is that in most parts of the developing world, the majority of such farmers are female, who on top of limited access to land also tend to be discriminated against in access to human capital, technology, and other resources. Following a traditional gender division of labour in households, female farmers tend to specialize in food production to feed their families, while trying to produce a surplus for local markets or export in order to generate the necessary cash for investment in technology, non-food consumer goods and services for the household, and investment in the human capital of their children.



of efficiency as the minimization of waste was given only years later, after the Paretian criterion had been introduced, namely by Margaret Reid in the 1930s (1934; 1943). She referred to waste in *consumption* when the rich consume far more than the poor; waste in the *production* of goods that have negative externalities (giving the example of tobacco); waste through inefficient *methods of production* (partially related to economies of scale); and waste through *market equilibria* allowing for the under-use and under-investment of production factors (leading to sub-optimal land-use and unemployment). In her work in agricultural economics, Reid (1943) argued that the partial production for own use among US family farmers was rational in a dynamic perspective in a context of uncertainty about yields and world market prices, and therefore efficient for the US food sector. The production for own use protected family farmers from food insecurity and distress sales in bad times and provided a buffer against too high market volatility. Hence, Reid's understanding of efficiency was a pragmatic one, rejecting the welfare theoretic assumptions of utility maximization, perfect markets, constant returns to scale, and absence of power, while recognizing that real world economies are influenced by uncertainty, dynamics, power relations and asymmetric institutions. Whereas Veblen already recognized that these imperfect conditions of markets require a shift away from efficiency as a static criterion of evaluation – the evaluation of an equilibrium – towards a dynamic criterion, evaluating waste in the economic *process*, rather than in an idealized market *outcome*, as Mark Blaug (2001) has formulated it so aptly.

Efficiency as the minimization of waste will only achieve such minimization over time when the strong no-harm principle will be released, allowing for re-allocations of resources in order to increase efficiency. This, in turn, requires a move away from a purely subjective measurement of outcomes, as in utility maximization and happiness studies<sup>6</sup>. Moreover, a closer link between resource-use and the measurement of wellbeing also enables closer monitoring of how economic processes affect the environment, and how agreed environmental standards, for example in relation to global warming, may be reached most efficiently (van den Bergh et al., 2006). Since efficiency implies necessarily a relationship between means and ends and not just some absolute minimum, the strong no-harm principle cannot simply be dropped but needs to be replaced by another no-harm principle relating means and ends. Following my argument of a substantive relationship between efficiency and equity, I propose to replace the strong no-harm principle by a weak no-harm principle which recognizes that redistribution of resources can crowd in production, investment and productivity, up to a certain point E (see the optimum at level 2.5 in the figure below). After point E, more redistribution of resources is likely to generate net disincentives, whereas before E, there would be net incentives through the crowding in effect of resources. Up to E, it is also likely that disincentives for the rich may be actually rather limited due to their support for equity values in society, either out of a belief in fairness or out of fear of social unrest (Henrich, et al., 2004 for evidence on such motives in 15 traditional societies, and Biancotti and D'Alessio, 2008, who find support for both motives in a study of 23 European countries). Hence, the weak normative principle implies a nonlinear dynamic relationship between efficiency and equity, precisely what is implied in the efficiency view shared in the evolutionary economics literature (Hodgson, 1993; Nootboom, 2002; Witt, 2003). Of course, it is important to note that not all forms of equity enhance efficiency – some social norms of equity go against efficiency as Philippe Platteau (2000) has shown. Taking this into account, the efficiency-equity relationship may be formulated as:

<sup>6</sup> This would bring us back to Smith's wellbeing concept that incorporates objective measures relating to minimum standards. Smith, in the *Wealth of Nations*, recognized two objectives for the economy: "first, to provide a plentiful revenue or subsistence for the people, or more properly to enable them to provide such a revenue or subsistence for themselves; and secondly, to supply the state or commonwealth with a revenue sufficient for the public services" (Adam Smith, (1981) [1776] Book IV. Introduction: 428). Moreover, he made a plea for sufficient wages: "... in order to bring up a family, the labour of the husband and wife together must, even in the lowest species of common labour, be able to earn something more than what is precisely necessary for their own maintenance"(ibid, Book I. VIII: 85-6).

$$E = \alpha + \beta_1 \text{gini}L + \beta_2 \text{gini} L^2 + \beta_3 \text{gini} H + \beta_3 \text{gini} H^2 + \beta_4 \text{gini} K + \beta_5 \text{gini}K^2 + \beta_6 \text{norm}$$

In which E = a measure of efficiency (which may be net agricultural yields per unit of input, total factor productivity, long run GDP growth, or a human development index for a long period of time),  $\alpha$  = a constant, gini stands for the gini-coefficient of inequality for the distribution of three resources: L = land, H = human capital, and K = capital, which are all included in a standard and in quadratic form (see also Figure 1.), and norm refers to the extent a fairness norm is supported in a community. This quadratic form is a simplified form of a general nonlinear formulation as suggested by Banerjee and Duflo (2003), while it parallels the quadratic form of the inequality-growth equation presented by Cornia, Addison, and Kiiski (2004), which, however, relates growth to income inequality rather than to inequality in the allocation of production factors<sup>7</sup>.

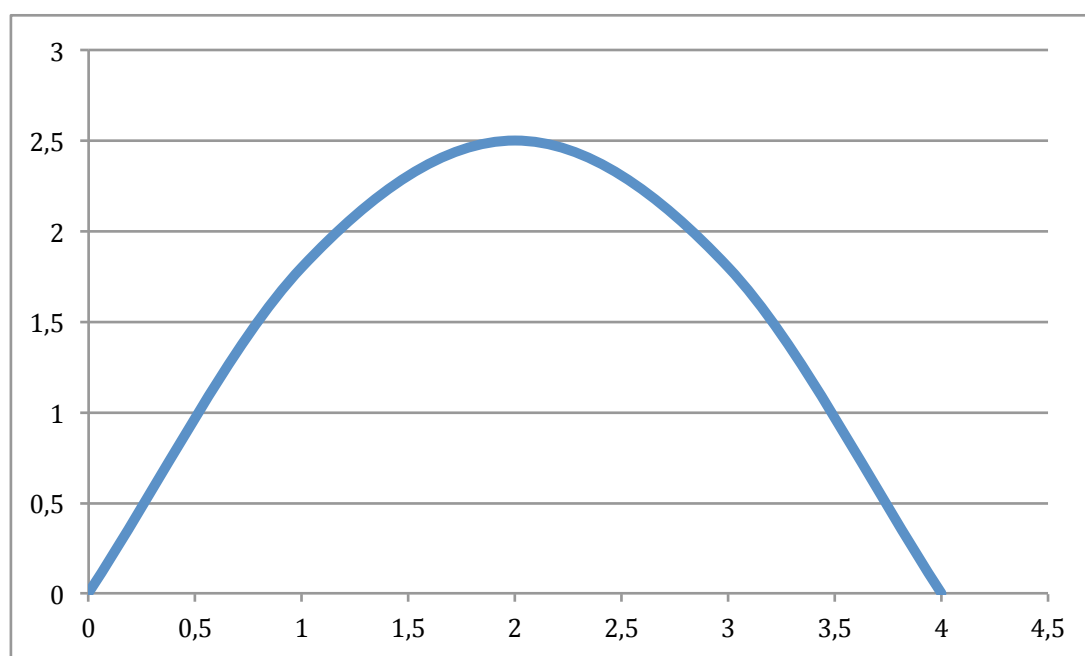


Figure 1. The relationship between equity (X-axis) and efficiency (Y-axis).

<sup>7</sup> Due to lack of sufficient data for a relevant period of time, it appeared not possible to empirically test this equation. For each of the dependent variables, some data does exist: for land distribution from the 1990 FAO agricultural surveys, for education from the World Bank Development Indicators for several decades, and for finance only very recent data from a dataset as part of the report *Finance for All?* (World Bank, 2008). The land and finance data are for a relatively small number of countries, which are for a large part not overlapping. Moreover there is no older finance data available which would be needed for a medium or long term growth estimation with production factors.

## Conclusion

In this paper, I have argued for an evolutionary efficiency criterion in which both equity and efficiency refer to the allocation of resources, not to the distribution of utility or income. Hence, the endogenous relationship between equity and efficiency based on the weak no-harm principle is quite different from the trade-off of equity and efficiency in the Paretian criterion, where equity implies a redistribution of the gains from a Pareto improvement, hence, exogenously derived. Or, in terms of the Kaldor-Hicks compensation, a redistribution of gains in order to make a Pareto improvement politically feasible. Instead, an evolutionary efficiency criterion is an endogenous efficiency measure, reflecting a nonlinear dynamic relationship between efficiency and equity in the allocation of resources, in which the most efficient allocation reflects a relatively equal allocation of resources, where the crowding in effect of production, innovation, investment and productivity is equal to the combined disincentive effect of reduced property rights and lower economies of scale for the rich. Such an evolutionary efficiency criterion, which is genuinely endogenous, shifts the attention away from equilibrium analysis in hypothetically perfect markets towards a process analysis in real-world, imperfect and highly dynamic markets. To be more concrete, the weak no-harm principle in this efficiency criterion reflects the minimization of waste of resources, a genuine, real-world criterion of efficiency, rather than the imagined and interpersonally incomparable unit of utility. This is an empirically assessable criterion, for which one can establish on the basis of historical data on inputs and outputs or on the basis of a productivity experiment for example, the point at which a more equal distribution of resources will no longer crowd-in more production. But it is not a mechanical measure of efficiency because the extent to which a community adheres to a particular fairness norm matters as well, for its feasibility. And also other contextual factors matter, such as the capabilities for innovation of entrepreneurs when they get access to more capital or training, for example. Hence, the evolutionary notion of efficiency that I have proposed here, as adaptive efficiency along the part of the equity-efficiency relationship where these are positively related, can only become meaningful when applied within a particular context, taking relevant context variables into account.

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