The Economics of Science – A Case Study in the Contribution of Ontology

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Introduction

The capitalist 'global economy' continues to hobble along, especially in the global North, racked by recurrent crisis (financial, debt, fiscal…) and barely returning to economic growth. This contemporary crisis of capitalism has also unleashed a crisis of the economics mainstream – which spectacularly failed to foresee the looming problems – that may yet prove to be a singular opportunity for heterodox political economy. To date, any such change seems not to be occurring (e.g. Chakrabortty 2012) even as a slew of books has been published critiquing mainstream economics, economic debate has been invigorated by the growth of economic blogs (e.g. Keen vs. Krugman, Economist 2011) and a host of websites and listserve emails have emerged (e.g. Yves Smith's nakedcapitalism).

There are at least two elements to this challenge to mainstream economics. Politically, the multiple, overlapping crises – not just of capitalism, but also at least a 'triple crisis' of political economy, ecology/resources and knowledge production – have shifted the ground on which political economy must (and 'economics' *should*) be able to comment informatively; from the traditional focus on industry and trade to include also a diverse set of issues such as the commercialisation of science, innovation (including of novel ontological capabilities) in a 'knowledge-based' economy, socio-technical system transitions and the interaction of economy and 'nature' (often mediated by science and technology). These are all inter-related in complex and overlapping ways and so call for a research program that is capable of illuminating these connections and, preferably, ways to minimize the suffering associated with the triple crisis. These issues come together within an 'economics of science', but one assuming a broad, systemic perspective. Yet, epistemically, mainstream economics is incapable of furnishing an economics of science that is critical and explanatory, rather than axiomatic and ahistorical – asking the key questions of 'why these changes? now? here? with what consequences (for science, innovation, society, political economy)?' An economics of science is thus something of a test-case for economics and its transformation.

The pathological state of mainstream economics is no news for many, however, including the growing body of work in the philosophy of economics that has sought to introduce ontological concerns into the discipline. Most obviously, since the mid-90s at least, Tony Lawson (and then other critical realists) has been arguing for a fundamental 'reorientation' (2003) of economics so that it actually examines the economic reality it purports to illuminate. In this context, the economic and economics crisis should be a golden opportunity to make definitive strides towards this laudable goal. Yet there seems to have been a lack of commentary from critical realists regarding an alternative economic analysis both of how to respond to the crisis and its aetiology. Much, if not most, work in the 'ontological turn' of economic methodology is conducted at the level of philosophical argument alone. Perhaps the urgency of concrete, substantive economic issues are such that there has been little space for these more abstract concerns. Meanwhile, the critics of mainstream economics amassing the greatest attention are those directly 'debunking' its substantive theoretical positions, especially if they can make the self-congratulatory claim of having been one of the supposed few to have seen 'it' coming (e.g. Keen 2011). Nevertheless, confronted with this opportunity, a challenge for the ontological critique of economics arises with renewed insistence and urgency, namely: how can ontological analysis contribute to the construction of alternative perspectives that are compelling, both epistemically and politically? Or to phrase this more polemically, why bother with more time-wasting abstract discussion of ontology instead of directly challenging and changing economics (where this may make use of a rough-and-ready philosophical appeal to make economics more 'realistic')?

This paper takes a different approach regarding the ontological turn, namely to *demonstrate*, rather than merely argue for, the contribution of ontological attention to political economy.[1] We thus start with the substantive problem of developing an economics of science capable of illuminating the commercialisation of science and its interaction with and implications for broader social crises, arguing thence that ontological attention is a crucial step in this theoretical project. The inadequacies of mainstream economics for such a project show that an economics of science demands profound rethinking of the ontological presuppositions of 'economics'. Political economy also has much to learn from productive synthesis with disciplines that have engaged with these issues for many years, including evolutionary

[1] This paper is a summary of two recent volumes (Tyfield 2012a, 2012b).

economics of innovation and science & technology studies; issues, moreover, that are intimately and inextricably connected to ontological and epistemological reflection on the nature of 'techno-science' and its world-making powers. This yields the contours of a new research programme of a 'cultural political economy of research & innovation' (CPERI) (Tyfield 2012b). Yet such mutual engagement of political economy and STS also demands rethinking of the ontological (and epistemic) presuppositions of both disciplines, given that the former is presumptively realist in both epistemology and (social) ontology while the latter is foundationally anti-realist, constructivist and sceptical of social structures.

This therefore suggests a way in which ontological analysis can not only contribute significantly to the emergence of a powerful alternative economics but also thereby actually demonstrate the difference it makes to substantive economic understanding. Accordingly, this paper summarizes the argument for a CPERI, built on the productive synthesis of a relational Marxist political economy and empirical studies of science and innovation, while also highlighting the key role that critical realist ontological analysis plays in this research programme. Moreover, incorporating substantive concerns from the outset also affords illustration of genuinely dialectical development (in which ontological arguments are themselves honed through interaction with substantive research, rather than vice versa alone). Several major criticisms of ontological work's contribution to economics are thus tackled, regarding its alleged irrelevance to or unbridgeable remoteness from actual research, its arrogant *ex cathedra* pronouncements and its infallibility.

Economics and Ontology: An Alternative Approach

The argument for ontology arises in no small part from the nature and standing of the subject of critique, namely mainstream, neoclassical economics. Prior to the economic crisis, at least, this discipline could credibly (for many) claim that its uniquely high status in the social sciences was merely a reflection of its similarly unique scientific authority. In this context, a major motivation for the turn to questions of philosophy was in order to demonstrate that, far from standing on this epistemic higher ground, mainstream economics is itself without epistemic warrant. In short, we can seek instead the determinate negation of this dominant framework by altogether uprooting it. As Fine's (2010) discussion of 'zombienomics' makes clear, this cannot license any expectation of such a critique alone producing determinate negation *in practice*, but we may at least consider it a necessary if not a sufficient condition of this change in the discipline.

This critique of mainstream economics starts from the observation that it is a necessary condition of the intelligibility of each and every theory of knowledge (and thus each and every actual scientific theory) that reality is such that it can be thus described (e.g. Lawson 1997: 19). Furthermore, all theories are thus 'realist' in the sense of presupposing *some* conception of the nature of reality (ibid.: 19 & 48), i.e. *all* theories, and indeed all intentional actions that employ belief, have ontological presuppositions. We are therefore bound to admit that we are committed to particular understandings of the nature of reality and that these must also be uncovered and tested for consistency with our explicit views on such matters.

Examination of these ontological presuppositions is a strictly philosophical task, employing transcendental reasoning, which asks the question "what must be the case given that the premise (of the transcendental argument) is intelligible?" (see Tyfield 2012a: Ch.8; 2007). This is the primary focus and distinguishing feature of critical realism as a philosophical project: to counsel the need for such explicit consideration of our ontological presuppositions, or what I have called "ontological attention" (Tyfield 2012a). Such a "critical" philosophy can work by immanent critique that uses transcendental argument – working from premises that gain both their particular meaning and ontological purchase, through being genuinely believed to be true, from a socio-historically specific and pragmatically given critical context – to expose underlying contradictions in our given understanding. There is thus an irreducible role for philosophy (*qua* ontology) as "underlabourer" for the sciences (Lawson 1997: 45). As such, critical realism marks a wholesale break with much (if not most) modern mainstream philosophy of science in its turning *back* to ontology as the fundamental and distinctive task of philosophical argument, against the anti-metaphysical programme of the former.

From this starting point, the ontological presuppositions of mainstream economics are compared with those of the actual social practices that it purports to study, including pre-eminently the phenomenon of individual choice. This yields the conclusion that the world presupposed by mainstream economics is radically different from, and incompatible with, the open, dynamic, stratified economic reality. Mainstream economics is thus equally radically incapable of producing scientific knowledge that elucidates economic phenomena.

We will not focus on criticism of this particular argument, of which there has been much (e.g. Fullbrook 2009, Hausman 1998). As others have also noted, much of this criticism tends simply to misread Lawson's argument or restate it in terms that fundamentally distort the point and so render it absurd (Morgan 2012). Even so, in the utterly changed context of the present, when mainstream economics, while still supreme, is also facing intense public and academic scepticism, questions regarding the significance of the ontological critique ('the significance of the significance of realism' as Morgan (2012) puts it) are assuming renewed importance. This is no doubt partly because the compelling critical argument it offers no longer has the same motivation: mainstream economics *did* categorically fail to foresee anything like the financial/economic crisis and *everybody knows this*. The critical argument is thus reduced to the role of explaining *why* or *how* economics is so inadequate; again a task that is more immediately compelling at the level of substantive errors, absurdities, contradictions etc… as in Steve Keen's (2011) romp through all the holy cows of neoclassical economics (e.g. downward-sloping aggregate demand curves, upward-sloping aggregate supply curves, equilibrium of any kind etc…). In its place, therefore, the demands to show how ontology contributes to construction of a better alternative become commensurately louder. In this context, the repetitive and now familiar pattern of inadequate, partial or straw-man criticism of Lawson followed by restatement and clarification but no actual development is dissatisfying, to say the least (Morgan 2012).

Moreover, such positive guidelines for a 'realist' economics as have been formulated, namely the methodology based on contrastive demi-regularities (Lawson 2003), are not particularly compelling. For even if this methodology could work in various non-laboratory natural sciences, there are problems with the applicability of this conception to the social sciences, at least as the primary methodology. In short, the problems of under-determination, and hence judgemental relativism, for the social sciences that arise from the nature of their object as itself intentional are simply not adequately addressed by hypothetical retroduction from contrastive demi-regs (Tyfield 2012a).

Where then does this leave the contribution of ontology to a revitalised (political) economics? Instead of continual debate limited to the level of philosophy, an alternative route is actually to explore and so demonstrate what difference ontological attention makes to substantive economic enquiry. Given the intense contemporary importance of the subject matter, its theoretical novelty and the current incompatibility of mainstream economics with any such

project (see below), the 'economics of science' provides a very promising substantive project on which to pursue this goal. The remainder of the paper illustrates this alternative approach.

Before we turn to this, however, let us briefly consider the key contributions of ontological attention. First, as already mentioned, is the fact that ontological commitments are always presupposed and so must either be assessed explicitly or are likely to linger as festering contradictions. Conversely, the core ontological argument of critical realism affords the elaboration of a unique ontology that is both attentive to (rather than dismissive of) the concept and nature of 'reality' but also attuned to the constructivism constitutive of human or social realities (including diverse socio-natures) (e.g. Benton 2001). The ontology of critical realism thus conceives of 'reality' as simultaneously real and conditional, emergent and mediated, structural/systemic and relational.

Secondly, at the level of social scientific theory choice, assuming a critical realist perspective regarding the economics of science in particular sponsors a relational Marxist approach. Against the demi-reg method, therefore, ontological attention offers an alternative methodology for social sciences. This follows from the insight that the very intentionality of the object of study entails that it also comes with ontological presuppositions and these can themselves be subjected to transcendental analysis. This is therefore to propose a *critical* methodology on grounds of realism that employs a substantive transcendental argument, a key example of which is (a qualitative reading of) Marx's labour theory of value (LTV). Just like the philosophical transcendental arguments that characterise critical realism's transcendental realism (Bhaskar 2008) and Lawson's critique of mainstream economics, this involves an examination of conditions of possibility of given social phenomena; in this case the ubiquity of commodities characteristic of contemporary economic life (Marx 1999). This 'qualitative reading' is engaged in the "causal-explanatory" project of examining the "*causal mechanisms, social structures, powers and relations*" (Fleetwood 2001: 67, original emphasis) that govern the actual historical course of economic events.[2] In short, Marx's singular insight is to ask 'what are the ontological presuppositions of this two-fold nature of the commodity (as both exchange value and use value)?' *This is the specific concern of the LTV: 'what is presupposed by the existence of systemic markets that give rise to the fixed*

[2]This approach thus side-steps both the vast majority of objections to the LTV, including the neo-Sraffian ones, which (correctly) repudiate it as a *quantitative* theory of how the price of goods is determined, and the associated 'transformation problem'.

exchange values characteristic of commodities?' (Fine & Saad-Filho 2004: 17). This project "encourages an enquiry into the nature of (alienated) labouring activity and its form of appearance" (Fleetwood 2001: 78), i.e. an ontological reading that provides the theoretical terminology that refers to the social structures whose action we are then concerned to investigate empirically.

This is thus to advocate a 'relational Marxism', in which the categories of Marxian analysis emerge from examination of the presuppositions of given socio-historical understanding of economic activity. On this reading, therefore, it is arguably at least as, if not more, accurate to describe value theory as the 'value theory of labour' rather than the 'labour theory of value' (Elson 1979), i.e. the investigation of the implications of (socially ubiquitous) subjection of labour to the particular capitalist economic category of 'value'. Moreover, this in turn yields a series of key concepts including a constellation of (at least) twelve concepts (see Tyfield 2012b for more details). Six of them are at a relatively high level of abstraction, namely:

- Value;
- Labour (as source and substance of value);
- Fictitious commodities (of labour, but also land, money and, in particular, knowledge);
- Emergence of a real totality of the 'economy';
- The inherent improbability of capital accumulation; and hence,
- The *constitutive* role of 'extra-economic' factors in construction of the 'economy', including irreducible issues of politics and culture/semiosis.

The other six arise from meso-level theory, (mutually) informed by value theory, namely:

- Spatio-temporal fixes and a cluster of associated terms, including a political-economic (possibility) space or settlement;
- Hegemony (at nation-state and international level);
- Historic bloc;
- Financialization;
- A relational conception of the state; and
- Primitive accumulation or accumulation by dispossession.

This thereby directs the researcher to, and legitimizes, a broad swathe of neo-Gramscian work in international political economy.[3]

Finally, critical realism thereby provides the philosophical grounds on which to build productive synthesis of what are otherwise foundationally incompatible disciplines regarding the 'economy' and 'science', as is needed for an 'economics of science'. For whereas political economy is concerned with real social structures that are credible only in the context of a philosophical realism, the discipline of science & technology studies has been founded (and most famously and fruitfully developed) on an essential repudiation of realism, including of real social structures. Grounded in a critical realist philosophy, however, productive engagement becomes possible, opening up the reciprocal examination of the conditioning of techno-scientific trajectories by political economic structures, on the one hand, and the mediation of the construction of economic value and the regularisation of capital accumulation by techno-scientific innovations, imaginaries, institutions and materialities, on the other. A relational Marxist perspective thereby affords an analysis of scientific change that includes the reality of the social relation of capital as a key but neglected element of non-reductionistic explanation of these crucial, causally over-determined processes.

We therefore highlight three key ways in which ontological analysis has a unique contribution to make to the construction of an alternative economics:

1) Directly, via substantive transcendental argument and the relational Marxism that follows;
2) Indirectly, following this, by directing attention to and legitimating (hence 'underlabouring') specific substantive work for critical comparison and synthesis; and
3) Indirectly as ontological/epistemological ground for productive synthesis with empirical bodies of work that offer considerable insight into the contemporary science and/or innovation.

We focus here on illustrating these points regarding the evolutionary economics of innovation, though the argument applies equally and importantly to constructivist science & technology studies (Tyfield 2012b: Chs. 9-11).

[3] In particular, the "neo-Gramscian" school of Robert Cox, the Braudelian economic history of Giovanni Arrighi, and the "regulation approach" of Robert Boyer, Michel Aglietta and Alain Lipietz and, especially, Bob Jessop. For references, see Tyfield (2012b).

The Problem of the 'Economics of Science'

There has been a broad process of commercializing science for over 30 years now, most obviously in the US, but also across the global North and even recently in the emerging global powers of the BRIC countries. For example:

- Privatization of research funding:

Private funding of scientific research in the US has grown 3.8 times in real terms (8.7 in nominal terms) since 1980 as against increases of federal government funding of 1.5 times (3.5). Private funding of total R&D has thus grown to 65-70% of total national R&D expenditure in the decade from 1998-2008 from just under 50% in 1980 (NSF 2010), with most of this funding being directed to research itself conducted by private industry. At universities, too, commercial funding has increased dramatically.

- Commercial 'accountability' and 'relevance'/'impact' criteria in competitive public funding:

Public funding too has increasingly come with strings attached that test the commercial relevance of research; an "auditing culture" that seeks to quantify research achievements (e.g. Shore 2008).

- Growth in university-industry relations (UIRs) and direct incorporation of science into commerce:

These range from privately-funded centres or entire departments to smaller projects and collaborations and, especially in fields relating to hi-tech industries such as biotechnology or information technology, spin-off firms. At the most successful research universities, this has created a revolving door between university and commerce.

- Growth in patenting, especially at universities and especially in life sciences:

Since the early 1970s (*before* the passing of the US Bayh-Dole Act allowing patenting of publicly-funded research (Mowery *et al.* 2004)), patenting at universities, especially in the US, has grown rapidly. Moreover, this growth has been particularly marked in high-growth sectors of science-intensive high-technology, such as biotechnology.

- Commodification of higher education:

Science education has also become progressively privatised, with student fees an increasingly important source of revenue and transforming students into 'consumers' of higher education (Slaughter & Rhoades 2007, Jessop *et al.* 2008). In the US, in particular, there has also been a significant growth in for-profit higher education, rising some 59% in 3 years to 3.2 million students at 3000 colleges in 2008-9, and representing 11.7% of all American students (Economist 2010: 130).

- Strong, global intellectual property rights, especially for life sciences and ICT:

Finally, the 1995 'Trade-Related Aspects of Intellectual Property' agreement (TRIPs) instituted strong IPRs that would benefit only a handful of (largely US-based) transnational corporations, particularly those in the IP-sensitive industries of pharmaceuticals, agribusiness, ICTs and entertainment (e.g. Sell 2003, Drahos & Braithwaite 2002). This trumped historical precedent in which nation-states have tended gradually to strengthen IP law to reflect the strength of domestic industries dependent upon them (Chang 2002, May & Sell 2006).

In fact, there is no clear *ex ante* or philosophical reason why the increasing prevalence of scientific research done within or funded by private industry should be seen as a problematic phenomenon (Shapin 2008). For instance, it is increasingly difficult to maintain any neat correlation between the institutional location of scientific research and the kind or standard of research, with Nobel prize-winning science conducted in private laboratories and university teams working on 'applied' sciences. Nevertheless, together all these changes have raised serious concerns about the future of scientific research and its institutions. For instance, Radder (2010: 14) lists eight issues that have attracted critical comment:

1) The potentially undesirable influence of commercial interests on research methods and results;
2) Higher levels of secrecy as scientific findings are transformed into commercial secrets;
3) Downgrading of research disciplines not seen as relevant from the perspective of profitable economic activity;
4) A short-termism in research agendas, as commercial investment demands quick pay-off, to the detriment of longer-term 'basic' research or other socially beneficial projects;

5) Assorted objections (ethical, legal, philosophical, religious etc…) to the patentability of academic research, especially those associated with the life sciences;
6) Conflicts of interest and exploitation of public funds for private gain by entrepreneurial scientists;
7) Detrimental effects on public trust in science more generally and the (seemingly) 'disinterested' epistemic authority of scientific findings; and
8) General concerns regarding the "justifiability of the privatization and economic instrumentalization of public knowledge".

Moreover, as Kleinman (2010) stresses, these impacts need not be the effect only of direct private investment but may also, if not *primarily*, arise from a more pervasive and indirect transformation of academic research cultures.

These exceptionally broad and far-reaching changes (and on a global scale) have generated a pervading sense of crisis. They have also been implicated, via an investment strike due to over-propertisation of knowledge, in the global economic crisis (Pagano & Rossi 2009). It is no surprise, therefore, that there has been a proliferation of projects devoted to understanding the 'economics of science'. Yet none of the projects that have received widespread attention are concerned with exploring and explaining the changing economic underpinning of scientific research as a historical process with profound social repercussions. In other words, the crucial questions of 'why *these* changes in the economics of science, in *these* places and *now*?' are almost entirely elided by such work. Instead, these projects employ mainstream economic analysis to investigate the institutional conditions for the optimal allocation of resources in order to maximize output of scientific research. They are 'economics' not in the sense of exploring economic *aspects* of science but rather in employing forms of (mainstream) economic *analysis* (Sent, 1999).

This includes the high-profile work of David and Dasgupta (1994) regarding a 'new economics of science' (NES), which follows the standard procedure of exploring ways to rectify the market failure of socially optimal production of the public good of 'knowledge'. Even while acknowledging the importance of tacit knowledge as undermining to some extent the public good argument, the NES still treats 'knowledge' in its models as an uncomplicated commodity that is to be maximized (Mirowski & Sent 2002). Qualitative issues regarding the effect of funding upon *which* avenues of scientific enquiry are pursued and the even more

serious possibility that the very character of 'knowledge' itself may be dramatically debased by certain funding arrangements (consider, for instance, the possible reduction of 'science' to partisan corporate advertising) are simply overlooked (Mirowski 2011). No less importantly, this project has failed even on its own terms. Despite some fifteen years of intense research activity regarding related issues, there remains little apart the original papers to show for the development of a broader economics of science programme. As Mirowski (2009) puts it, "the landscape [of the mainstream economics of information], far from being crowded with monumental theorems and general models, is merely dotted with abandoned half-finished shells."

These problems hinge on the intrinsic difficulty, if not impossibility, of exploring the economics of science (or knowledge or information) using mainstream economic models built upon market exchange. Boyle (1996), for instance, notes that market models investigating the equilibrium of supply and demand for a commodity are built upon the assumption that individual agents have perfect information. When the commodity is *itself* information, therefore, models run up against intractable contradictions with their very assumptions. At the very least, as the access to information becomes more limited, the models become more complicated and less mathematically tractable, while such supposed mathematical rigour and analytical parsimony is a primary appeal of these models. Mirowski (2009: 138-9) too notes that:

> "You cannot paint the marketplace of ideas as a marvellously parsimonious and magnificently efficient model of cognition if you can't even demonstrate mathematically that the internal production of neoclassical market equilibrium does not bear the information requirements that outstrip any other known process."

Similarly, "the price of the marketplace of ideas leads to formally undecidable market prices." In short, there are good reasons, reflected in the actual, historical experience of the various projects attempting a (post-)neoclassical-based economics of information, to suppose it is an impossible task.

But similar conclusions may also be reached by consideration of the second half of this phrase, namely what is the economics of science an economics *of*? This question shifts our attention to a slightly different literature on the commercialisation of science. We have

already briefly seen the problems raised by assuming a mainstream economic perspective regarding this issue. For taking such a stance, and hence seeking to understand 'science' in terms of a market, necessarily demands that there be some 'thing' that is produced by science and which it is self-evidently a social good to maximize. From this starting point, it is extremely hard *not* to proffer models that reduce science to a familiar commodity, at least not without bringing the usefulness of this approach fundamentally into question.

Unfortunately, however, much of the literature on the commercialisation of science is just as problematic regarding its perspective on the nature of science and the interaction of 'science' and 'money'. Mirowski and Van Horn (2005) describe this literature in terms of a debate between "Economic Whigs" and "Mertonian Tories". The former are simply concerned with maximizing the productivity of 'science' and, true to their Whiggish (neo-) liberalism, tend to promote the commercialisation of science as a progressive development without any complications or problems for scientific research.[4]

Conversely, the latter adopt the Mertonian perspective of science being dependent upon social norms that leave it in splendid isolation from the corrosive influences of commerce and self-interest.[5] The commercialisation of science is thus treated as the catastrophic passing of a former Golden Age (i.e. the post-war period of the *trente glorieuses* of 1945-75) in which the state 'wisely' chose to fund science generously for the public good. Although this approach shows a much clearer concern regarding the interaction of changing economic arrangements and scientific research, it too is highly problematic. In particular, the notion of 'science' it employs systematically excludes socio-political concerns except insofar as they are distortions or corruptions of the scientific enterprise. The Mertonian perspective is thus merely the flipside of the errors of the Economic Whigs, both frameworks effectively ruling out the investigation of the actual effects of different funding arrangements on science, if for diametrically opposed reasons. In neither case, therefore, is there any need for (let alone possibility of) empirical investigation into the actual effects, both negative *and positive*, on science of changing economic arrangements because the answer is already known.

It is clear, therefore, that if we are interested in actually investigating questions such as 'how is/are science/s funded?', 'how and why has this changed?' and 'how have these changes

[4] See e.g. Etzkowitz *et al.* (2000), Etzkowitz & Peters (1991)
[5] See e.g. Boyle (2003), Krimsky (2003) and references in Mirowski & Sent (2008).

affected that/those science/s?' we must employ a completely different conception of science, just as we must employ a different economics. This perspective would not only recognize the variety of social practices designated 'science' – or, more accurately, research and innovation – and attend to their concrete particularities, but it would also acknowledge that science is itself *constituted* as an irreducibly socio-historical process, with all the economic, cultural and political 'thickness' this entails. This vision of science is closely akin to that developed with science and technology studies (STS) under the rubric of 'co-production' of science and 'society', i.e. the mutual constitution of relatively autonomous social phenomena (e.g. Jasanoff 2004).

Such a redefinition of 'science', however, also brings with it significant consequences for the form of economics that is capable of studying it in at least two obvious respects. First, the very subject matter of an economics that is relevant to the study of the economics of *science* (as opposed to the various reified definitions of Economic Whigs and Mertonian Tories) demands that we employ an economics that is capable of exploring *economic aspects* of an inseparably socio-political and cultural reality. The second challenge is methodological and arguably more profound. Since co-production posits a social ontology of science in which the very nature of science develops alongside that of its broader socio-economic context, it becomes epistemologically impossible to employ a framework that must first define what science is before proceeding to examine its economics. Whereas the co-production analysis is thus concerned to *develop* our understanding of the nature of research and innovation through analysis of its interactive development with is socioeconomic context – i.e. to stretch towards a 'definition' of science as its *conclusion* – the axiomatic and 'deductivist' structure of mainstream economics requires the 'science' it is investigating be defined *ex ante* and so reified.

Quite simply, then, *mainstream economics cannot illuminate the commercialisation of science and the knowledge economy more broadly*, and demand for just such understanding can only grow, especially in the context of crisis and discontent such as the present. Taken together, therefore, these two challenges illustrate how an economics of science offers a singular opportunity and motivation for a broader substantive project to develop an alternative economics that breaks with the mainstream discipline and addresses issues that will be at the very heart of economic concerns for future generations (*Cf* Sent 1999 for similar sentiments).

We now turn, therefore, to a brief illustration of the difference ontological attention does in fact make, regarding formulation of a critical, explanatory political economy of research and innovation through engagement with the evolutionary economics of innovation. In doing so, we will highlight the three contributions of ontological analysis listed above.

From EEI to CPERI

The school of thought that is variously called 'innovation studies' or (as we shall call it here) the 'evolutionary economics of innovation' (EEI) is without doubt the literature that has contributed most to current understanding of the processes of technological change and innovation. Its inclusion in any research programme on the 'economics of science' is thus hardly short of obligatory. From the perspective of a CPERI, five strengths, in particular, stand out.

First, EEI does indeed share a *prima facie* compatibility with neo-Gramscian political economy, affording comparison that brings out differences that are informative. Conversely, the latter has not paid the same due heed to issues of scientific and technological change as has EEI. Much of this connection may be explained by the foundational influence on EEI of the works of Schumpeter. For Schumpeter not only stands alone in the 'canon' of Great Economists in his emphasis upon the importance of an economic sociology (Swedberg 1987), hence calling for an economics that is more akin to Lawson's (1997) critical realist "economics as social theory". But he is also exceptional in the importance he placed upon the works of Marx and the Marxian argument for the quintessential dynamism of a capitalist economy and hence the central role of (technological) innovation (Bottomore 1992).

The second key strength of EEI is its empirical attention to actual processes of technical change and innovation, together with an attractive scepticism and reflexivity on how *little* is known about these phenomena (e.g. Freeman 1994: 473/4). Amongst the most important of these has been the growing body of work that has comprehensively dismantled the dominant idea of science and innovation policy of the 'linear model' of innovation (Kline & Rosenberg 1986, Mowery & Sampat 2006), which posits the sequence of basic science → applied science/ technology → innovation & diffusion → economic growth.

The third significant strength follows directly, regarding EEI's theoretically sophisticated and empirically grounded critique of the two key arms of the economic argument for the commercialisation of science and strong intellectual property rights, namely the linear model (as just discussed) and the 'public good argument'.[6] The core insight undercutting the latter is the increasingly undeniable evidence against the presumption of the non-rivalry and non-appropriability of knowledge. The conjunction of insights regarding the much greater importance of tacit knowledge together with the diverse roles and stages of the contribution of science to innovation leads to a much more complex picture regarding the economic incentives or lack thereof for private production of scientific knowledge, and hence the case for public subsidy. For instance, building up tacit knowledge may take a long time with only uncertain, prospective and medium-to-long term benefits promised in return, while the relevant personnel are not contributing to profitability in the meantime. It is also difficult to measure and so assess or purchase, and in many cases (e.g. encultured and embedded organizational or management 'knowledges') it adheres to the individual, not the firm.

The fourth reason that EEI is so important for a political economy of research and innovation resides in multiple fundamental insights that provide both (a) an expansive approach to innovation, as the process that mediates between scientific research and economic growth (insofar as there is such a link in a particular case) and (b) a broad set of factors that must be taken into account in any detailed research programme. These insights would include:

1) First and foremost, the fact that innovation and technical change is an evolutionary process, a "groping" (Nelson & Winter 2002), the outcomes of which are fundamentally uncertain and unpredictable (Rosenberg 1994, Dosi 1988). In what is essentially a realist move, this also leads EEI to significant methodological and theoretical conclusions regarding an economics that can take innovation seriously, let alone research it.

2) Secondly, therefore, EEI is also explicitly critical of the cursory interest mainstream economics shows in technological (and scientific) change, which, unable to be incorporated into its models, is merely "black-boxed" (Rosenberg 1982). Similarly, against the fetishism for mathematical modelling and deductive 'rigour', for EEI, study of actual technological and

[6] Though it must be noted that, in fact, it may be plausibly argued that EEI has been most influential in the corridors of power in just the *opposite* direction (Mirowski & Sent 2008, Godin 2006)

economic change demands a much greater (inter-disciplinary) role for history (Freeman & Louça 2002) and qualitative or 'appreciative' theories (Nelson & Winter 1982).

Fifth, EEI literature on long wave theories of the economics of techn(olog)ical change (ETC) (Perez 1983, 2002, Freeman and Perez 1988, Freeman and Louça 2002, Freeman 1994, Freeman and Soete 1997), following the seminal work of Kondratieff, is of particular interest for an economics of science since it provides a historical context for understanding changing economic conditions of, and demand for, research and innovation. ETC also has significant strengths. First, it too is an explicit critique of such neoclassical economics, notably New Endogenous Growth Theory (NEGT), which 'endogenizes' technological change (or rather productivity increase through modelling the R&D process) into its models, thereby reducing technological change to a one-dimensional variable of *rate* of change.[7]

Conversely, ETC uses (Kuhnian) concepts of "technoeconomic paradigm" and "technological revolution" (Perez 2002, Kuhn 1970) and the cycle associated with these conceptions to incorporate technological change seriously into economic explanation. These concepts posit a model of 'long waves' of such paradigms. For the duration of a paradigm, then, not only the rate but also the direction of the technological change is fairly straightforward for all to see, resulting in technological complementarities and path dependence. Technologies outside the paradigm, however, are excluded from development as not promising the same return on investment; the intrinsically uncertain nature of innovation privileges established patterns and processes that are known to be productive and to complement the parallel innovation of *others* on whom a given innovation's success significantly depends. The paradigm thus lends a cyclical temporality to innovation, in which there is first a surge of activity, then a steadying off to 'normal' growth (as *per* Kuhn's normal science) and then a gradual maturity and decline as the paradigm's technological fecundity approaches exhaustion. At that point, new technologies will be favoured instead, this in turn precipitating the next technological revolution; a turbulent Schumpeterian process that is at *no point* characterised by equilibrium.

Secondly, ETC also recognizes the crucial and singular role of finance in the growth of the economy (Perez 2002: 21, Orsenigo 1989: 26), which is left out of neoclassical accounts in which money is simply a more efficient means of exchange, a financial market is just another

[7] See e.g. Dosi & Nelson (1994), Verspagen (2006).

commodity market and finance does not have any emergent effects on the development of the economy (Bryan 1995, Evans 2004, Keen 2011).

Finally, ETC, like its close disciplinary cousin STS, has a much more sophisticated hold on the importance of social factors in the shaping of technologies and in the actual trajectories of successful technoeconomic paradigms. Indeed, these are "technoeconomic" paradigms for Perez, not merely "technological" ones, precisely in order to stress the overall social context and its interaction with technology.[8] One particularly important feature of this is the period of political and social turbulence that inevitably follows from the emergence of a new revolutionary technology (Freeman & Perez 1988: 59, Perez 2002: 4, 24-26).

The overall picture provided by ETC, therefore, is a credible and comprehensive systemic account of the coevolution of economic and technological change. Empirically, this yields a series of discrete paradigms or "ages" thus:

Table 1[9]

Five Successive Technological Revolutions, 1770s to 2000s

Year of Irruption	***Technological Revolution***	***Popular name for the period***	***Core country or countries***	***Big-bang initiating the revolution***
1771	FIRST	The 'Industrial Revolution'	Britain	Arkwright's mill opens in Cromford
1829	SECOND	Age of Steam & Railways	Britain (spreading to Continent and USA)	Test of the 'Rocket' steam engine for the Liverpool-Manchester railway
1875	THIRD	Age of Steel, Electricity & Heavy Engineering	USA and Germany forging ahead and overtaking Britain	The Carnegie Bessemer steel plant opens in Pittsburgh, Pennsylvania
1908	FOURTH	Age of Oil, the Automobile & Mass Production	USA (with Germany at first vying for world leadership), later spreading to Europe	First Model-T comes out of the Ford plant in Detroit, Michigan
1971	FIFTH	Age of Information & Telecommunications	USA (spreading to Europe and Asia)	The Intel microprocessor is announced in Santa Clara, California

Source: Perez (2002:11).

On this conception, then, the period of the contemporary commercialisation of science was one of "irruption", passing into "frenzy" (from 1987) in which the technological successes in the new paradigm, ICTs (information and communication technologies), led to a bubble of financial speculation built on the continuing growth of the revolutionary technology.

[8] Though compare Freeman & Perez (1988: 47) with Dosi (1982).
[9] Compare similar tables in Dosi (1982), Freeman & Louça (2002: 141), Freeman & Perez (1988) and Freeman & Soete (1997).

EEI thus conclusively shows that *there is, and can be, no such thing as a single, abstract 'economics of science'.* Moreover, regarding prospects for a vigorous and robust academic school, in the likes of Nelson, Freeman, Rosenberg and Pavitt, EEI is a school of unarguable stature and economic seriousness. As such, it has as good a chance as any body of economics to challenge and transform the notoriously, unapologetically unreconstructed 'zombieconomics' (Fine 2010) mainstream.

Nevertheless, for all the strengths of EEI, there are also significant differences to the neo-Gramscian perspective and significant weaknesses. A critical, explanatory economics of science bring these out particularly clearly. A key question here is 'how does the periodicity of techno-economic paradigms help explain the recent commercialization of science?' The neo-Gramscian perspective places politics (and culture) as *constitutive* of technological and economic change in the form of spatio-temporal fixes and modes of regularisation and societalization that constitute the (transitory, constructed) settlement of a political economic space. As argued in detail elsewhere (Tyfield 2012b), therefore, this leads to a compelling explanation for the commercialisation of science as the accumulation by dispossession of an as-yet-uncapitalised sphere of social activity, namely 'knowledge production', so that capital accumulation may continue. The timing is then explained in terms of a persistent overaccumulation crisis since the 1970s, leading to financialisation and the aggressive search for a new settlement of expanded capitalist relations of production. The commercialisation of science is thus an intensely political phenomenon, not just a techno-economic one associated with the emergence of a 'new (knowledge-based) economy'.

Perez also constantly stresses the importance of politics in the setting of technoeconomic trajectories and the "mutual shaping" of politics and technology (ibid.: 19). But when turning to the analysis itself ETC places technology at its centre and so treats politics as the epiphenomenon of the autonomous technoeconomic trajectory. Hence "each technology does then indeed lead to a 'new economy' [and]… technology is behind the transformations" (ibid.: 145 & 7, 24, 155). In particular, politics is seen either as the *ad hoc* context for particular technoeconomic developments (ibid.: 115, 123, 126) or as a functionalist safety valve that inevitably evinces the (generally socially progressive) change in regulatory framework necessary for the optimal exploitation of the technoeconomic paradigm (ibid.: 19, 99, 129-136). The irreducible contribution of willed political struggle in such progressive concessions is at the very least significantly downplayed. Moreover, the explicit

acknowledgement of the importance of politics is belied by its total absence in the explanation of the *creation* of the new economy, as opposed to the social response to it, and hence in the explanation of the cycle itself. As a result, the commercialisation of science (including e.g. TRIPs), must be understood – implausibly in both cases – either as the 'old economy's' political intransigence or the essentially progressive forces of the new economy.

These problems, however, point to deeper problems with the ETC framework, which revolve around its largely "neo-Kantian" ontological perspective (Bhaskar 2008: 9 & 25-26), in which the 'surplus' of theoretical terminology beyond empirical observation is construed as the idealized heuristics that best afford intelligible organisation of the empirical data. According to Bhaskar, such a stance can be distinguished from, on the one hand, a purely empiricist position and, on the other, a transcendental realist position for which theoretical terminology (when justified, may be legitimately, if defeasibly, understood to) refer(s) to real phenomena.

It is clear, first of all, that ETC *is* a neo-Kantian framework, with its explicit invocations of heuristic ideal types. This is equally apparent in its wholesale assumption of the Kuhnian conceptual apparatus of revolutions and paradigms (applied here to the "technoeconomy" instead of scientific knowledge). But it follows that all the familiar problems associated with Kuhn's argument are similarly taken on. In particular, ETC necessarily overstates the discontinuity, at the total expense of continuity, of one paradigm from the next in the exogenously given "technological revolutions" (Dosi 1982: 90, Perez 2002: 25, footnote 33).

Certainly, the timing of these shifts is integrated into the framework, on the basis that the maturity and decline of the existing paradigm sets up the conditions for the shift in investment to the next one. But this does not answer the fundamental problem of "whence the next technology?" because ETC's argument rests on a central circularity. On the one hand, finance is attracted to the new technology because of its promise of greater profitability (Perez 2002: 11). But, *ex hypothesi*, the new technology only takes off in a revolution *when finance is already investing heavily in it* (ibid.: 33), or else the timing remains a mystery. The only way to resolve this is to presume *ex ante* that there is a next technology "waiting in the wings" (ibid.: 32) and that all that is required is for one paradigm to end for another to begin. But this is to place technology as an exogenous given, the driving force of the whole economy, *pace* express statements against such technological determinism (ibid.: 22).

This fundamental problem, however, is inherited from ETC's other major inspiration, Schumpeter. For while it rightly focuses on the importance of technological innovation for the economy, and the resultant turbulent business cycle of economic growth, ETC, like Schumpeter, simply assumes that innovation will naturally occur (e.g. Fagerberg 2006: 1) and does not ask the question of "what are the social presuppositions of a systemic subjective drive, across economic life, to innovate?" The effects of a new technological revolution are thus explained in terms of how it "fires the imagination" of prospective entrepreneurs and engineers and produces a new business common sense (i.e. paradigm), completely neglecting the objective social context (Freeman & Perez 1988: 48, Perez 2002: 16). Similarly, ETC explanations are couched in terms of a subjective profit motive, but the objective (and exceptional) social context that is presupposed by this is not examined (Perez 1983: 358, 360 & 366).

As such, like Schumpeter, ETC overlooks the fact that even if innovation (as opposed to invention) is the *differentia specifica* of a capitalist economy, it has not itself emerged miraculously from nowhere in the modern world (Bottomore 1992). Conversely, a neo-Gramscian perspective highlights how a systemic imperative to innovate presupposes is the dominance of the social relations of production by the capital relation, which sets up the law of value that *forces* businesses to compete and innovate on pain of economic failure (Fine & Saad-Filho 2004, Arthur 2004).

This critical weakness is also evident regarding the key issue of the role of finance. Comparison with ontologically-underlaboured neo-Gramscian argument (e.g. Arrighi 1994, 2008) thus also allows a comparison regarding ETC's explanatory power of the historical record. Both the ETC and critical realist frameworks accord finance a crucial role in the transformation of the economy and the shift from one phase of growth to another. Both parties also agree that finance assumes dominance over the economy, in particular through "making money from money" (ibid.: 98, 100) or the "financialisation" of the economy (Arrighi 1994 & 2003, Blackburn 2006) respectively. Yet the significance of finance in the ETC schema is primarily its sponsorship of the *new technologies*, which will be at the heart of the emerging 'new economy' (Perez 2002: 33-35). Conversely, for the critical realist conception such investment is the result of a much broader shift in the balance of power from

productive to finance capital as the expansion of the former comes up against the limits of the existing political economic hegemonic settlement and its associated spatio-temporal fixes.

Two differences follow that are particularly striking. First, as we have seen, for the critical realist theory the pivotal role of finance revolves around its *power* to enforce a round of primitive accumulation in phases of financialisation (Arrighi 2008). Conversely, ETC sees no role for politics (beyond *ad hoc* national idiosyncrasy) in the *creation* of the new economy with repercussions for the role accorded to finance in the cycle of growth of the world economy. This is also clear in the bizarre decision to omit global wars (e.g. 1914-18, 1939-45) from the ETC schema of explanation (see Table 1), when it is surely obvious that such wars are both hugely significant geopolitically (and hence indirectly for innovation/ long waves) and periods of intense innovation *per se*.

Secondly, because technological change is placed at the heart of its analysis, ETC sees the growth of finance as dependent on new technologies. While it acknowledges that bubbles occur through the investment of finance in finance, it nevertheless places the new technology as the fundamental cause of financial dominance. Conversely, the critical realist theory treats the growth in finance as a *sui generis* phenomenon of which new technology investment is merely one consequence. And, taking the two points together, it focuses on the crucial interaction between finance and politics: the dependence of the former on political order, the political shift involved in its rise to dominance and its political effects.

These differences have conspicuous implications for explanation of actual events. Perez (2002: 77) is unequivocal that the end of the "frenzy" phase of a financial bubble economy is based on technology-based crashes:

> "There is one type of collapse, though, which is directly connected with technological revolutions. It is the crash – or series of mini-crashes – that tends to close the casino bubble at the end of the frenzy phase."

Furthermore, she is clear that just such a crash occurred in 2000 and that such crashes precipitate "prolonged recession" (ibid.: 7) that then triggers the political backlash and safety valve of painful reform to accommodate the new paradigm. Contrary to her analysis, however, following the dotcom bust (as well as the – geopolitical! – shock of 9/11 the

following year) the US (and world) economy did not collapse but GDP growth recovered: to 2.5% in 2003, 3.9% in 2004 and 3.2% in 2005 (data from US Bureau of Economic Analysis). Moreover, as Blackburn (2006) has shown, the subsequent growth was *still* based on continuing financialisation, if not its acceleration. Indeed, the continuing growth of such derivatives markets was a major factor in the subsequent Great Crash of 2008. In short, it is clear that the bursting of the technology bubble did not solve the underlying economic problems, nor chastened finance capital more generally and brought it to heel, nor placed financial regulation on the political table.

But on the critical realist conception, why should it? If it is acknowledged that the dominance of finance over the economy is dependent primarily on *political* support and stability then there is no reason why it should not be able to withstand any number of financial crashes and shocks so long as its political dominance is not fundamentally undermined.

An alternative strategy for Perez, of course, has arisen with the Great Crash of August/September 2008 and the subsequent economic crisis. Hence, in a recent update on her position, Perez (2009) seeks to square this circle by claiming that "this time" the technological and financial crashes were "two episodes rather than one". But she can only do so at the cost of the credibility of this central pillar of her argument that the power of finance and the developmental phase of the global economy itself are *primarily* dependent on the fortunes of paradigmatic technologies, in this case, ICT.

Moreover, and as *further* evidence for the neo-Gramscian theory and against Perez, even *this* almighty financial crash has not yet derailed the financialization of the global economy and the political dominance of neoliberalism. But this is because there has still not been the fundamental *political* uproar and movement against finance capital's dominance. Only such a political change will spell the end of financialization but this moment's emergence *cannot be predicted*, at least on the basis of such abstract models alone.

While many ETC scholars are not uncritical of new technological developments and their potential social repercussions (e.g. Perez 2002: 4, Freeman 2001), therefore, the ETC framework they employ *cannot* fundamentally question the likelihood, possibility or even benefit of an emerging paradigm, let alone that of the *cycle as a whole*. Instead, it focuses its

practical efforts on providing policy advice to minimise the economic problems caused by the inevitable birth; what Perez (2002: 113 & 158) calls "adaptive regulation" in order to "restrain the excesses" of these cycles. Conversely, on the critical realist conception, it remains an open question whether or not there will be an emerging paradigm and whether or not it, and *hence the cycle itself*, is good for all groups and classes, let alone for humanity as whole, or even fundamentally compatible with the ecological conditions of human survival; i.e. precisely the *open questions* of contemporary politics of science, technology and innovation that we have stylized above in terms of 'triple crisis'.

ETC thus carefully avoids the neo-classical errors of reifying economic *states* as static equilibria, but itself necessarily reifies the *processes and trajectories* it identifies. Conversely, the critical realist analysis identifies *real, transfactual tendencies* that *condition* but do not determine the *possible* courses of future *political* action (including but not limited to government policy) to *alter* or *construct* the course of history, not merely to adapt to it. And it does this not through identification of heuristic ideal types but through examination of the presuppositions of our actual, given understanding of the social world.

This illustration thus demonstrates the three ways in which ontological attention may contribute to an alternative economics appropriate to the challenges, political and epistemic, of the present in the form of a critical, explanatory economics of science. First, by providing a relational Marxism through substantive transcendental argument, it sets a theoretical and methodological position that allows the contextualization and strengthening (not abandonment) of the many insights of EEI, incorporating ETC. Secondly, by comparing its concrete explanations with those offered by substantive theories compatible with a 'value theory of labour' (in this case, neo-Gramscian IPE) this allows further criticism and development of explanations. Lastly, but by no means least, while EEI is fundamentally neo-Kantian in perspective, the development of a third philosophy that can incorporate and go beyond this perspective present the ontological and epistemic grounds on which to build a productive synthesis of this tradition of economics and a Marxian political economy that has to date paid insufficient attention to detailed empirical study of science and innovation.

Conclusion

Economics has been a key starting point for the critical realist programme in general – both in actual history (viz. Bhaskar, Lawson, Sayer, Jessop et al.'s early and/or abiding interest in

economic issues) and conceptually, regarding a turn to philosophy of science in order to open up alternative economic explanations. Yet in an age of multiple crisis, including of capitalist political economy and the mainstream economic discipline purporting to study it, it is palpably no longer adequate – notwithstanding the major contributions of these philosophical arguments to date – to continue responding to demands to show the difference ontological attention makes by pleading that such demands are to misunderstand the argument. This is so not just for epistemic reasons but also for political ones: in a rapidly changing and profoundly unstable social context that is now crying out for a different economics, critical realism must either 'show its hand' now in this regard or may forever be dismissed, robbed of its motivation.

Conversely, we have explored significant contributions, direct and indirect, of ontological attention to an economic project of considerable contemporary importance: a critical, explanatory economics of science or, more accurately, a cultural political economy of research and innovation. This, thus, begins to furnish a fitting response to those who would argue such analysis is simply a diversion from the real work of constructing such an alternative economics. For it highlights how, far from being a distraction, ontological attention is a crucial parallel project and one that yields major gains at the level of substantive understanding of our contemporary situation.

But this approach also responds to two other criticisms of critical realism that are not entirely unfounded. First, against the argument that the philosophical project of establishing an ontology necessarily involves arrogant *ex cathedra* pronouncements that are expected to be 'applied', engaging directly with an economics of science – and showing the pivotal role ontological analysis nevertheless plays in such a project – illustrates how this is fundamentally not the case. Instead, we have a programme that actually *is*, and does not just *call for* others to be, engaged in parallel, inseparable and messy projects that have lessons in *both directions*. This, in turn, also tackles head on (over-stated) criticisms regarding the supposed infallibility of critical realist ontological analysis. For instance, concerted engagement with empirical studies of research and innovation force critical realist philosophy to take more seriously the ontological openness of an intensely techno-scientific world and the intrinsic epistemic limits this entails.[10] In short, a more concerted engagement *from*

[10] This point is even more clearly brought out in engagement with constructivist STS (see Tyfield 2012b).

critical realism with substantive issues offers considerable benefits to critical realism itself as well. It is hoped that, in this way, the major contributions ontological attention has to make are indeed realized.

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