The Economics of Research – the Contribution of Critical Realism
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Introduction
The capitalist 'global economy' continues to hobble along, especially in the global North, which remains racked by recurrent crisis (financial, debt, fiscal) Nevertheless, the crisis of mainstream economics – which spectacularly failed to foresee the looming problems – may yet prove to be a major opportunity for heterodox political economy. Unfortunately, to date, an alternative economics remains embryonic at best (e.g. Chakrabortty 2012), despite the slew of books critiquing mainstream economics and the invigoration of economic debate, as in the surge of economic blogs, websites and listserve emails (Economist 2011).

There are at least two elements to this challenge to mainstream economics. First, there is a political challenge. The multiple, overlapping crises – 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. This shift has been away from the traditional focus on industry and trade towards a more diverse set of issues in a 'knowledge-based' economy (in crisis), such as innovation *per se* and its broader socio-economic 'rhythms' and conditions, the commercialisation of research, , 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 as well as, preferably, ways to minimize the suffering associated with the triple crisis. These issues come together within an 'economics of research', that assumes a broad, systemic perspective. Second, there is an epistemological challenge. Mainstream economics is incapable of furnishing an economics of research that is critical and explanatory, rather than axiomatic and ahistorical. It therefore fails to ask key questions such as: 'Why these changes?; Why now? Why here? And what are the consequences (for science, innovation, society, political economy)?' The patterns of funding of research, based on questionable assumptions of 'good' science, are thus a test-case for economics and its transformation.

The pathological state of mainstream economics is not news for many. Since the mid-90s at least, Tony Lawson and other critical realists have argued for a fundamental 'reorientation' of economics so that it actually examines the economic reality it purports to illuminate in an 'ontological turn' (Lawson, 2003). 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. By contrast, there has been little space for these more abstract concerns in post-crisis debates about an alternative economics, perhaps due to the urgency of concrete, substantive economic issues. Meanwhile, the critics of mainstream economics amassing the greatest attention are those who debunk its substantive theoretical positions. This is especially true of authors who can claim to have predicted the crisis (e.g. Keen 2011). Nevertheless, confronted with this opportunity, we might ask: how can critical realism contribute to the construction of alternative perspectives that are compelling, both epistemically and politically? Or to phrase this more polemically, why waste time with abstract discussion of ontology if we could directly challenge and change economics?

This chapter therefore aims to *demonstrate*, rather than merely argue for, the contribution of critical realism to political economy.[1] We thus start with the substantive problem of developing an economics of research capable of illuminating the commercialisation of research and its interaction with and implications for broader social crises; and showing how critical realism is a crucial component in this theoretical project. The inadequacies of mainstream economics for such a project show that an economics of research demands profound rethinking of the science of 'economics'. Political economy also has much to learn from a productive synthesis with disciplines that have engaged with these issues for many years, including evolutionary economics of innovation.
We therefore highlight three key ways in which critical realist analysis makes a unique contribution to the development of an alternative economics:

1) Directly, via substantive transcendental argument (retroduction) 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 (elimination of competing theories via judgemental rationality); 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.

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The Problem of the 'Economics of Research'
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:

- <u>Privatization of research funding</u>:

Private funding of scientific research in the US has grown 3.8 times in real terms (8.7 in nominal terms) from 1980 to 2010 as against increases of federal government funding of 1.5 times (3.5). Private funding of total R&D grew 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.

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

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).

- <u>Growth in university-industry relations (UIRs) and direct incorporation of scientific research into commerce</u>:

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

[1] For more detail, see (Tyfield 2012a, 2012b).

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). 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 (Sell 2003). 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 (May & Sell 2006).

Some may argue that 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: Nobel prize-winning science is conducted in private laboratories while universities conduct important applied research. 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 in the global economic crisis, via an investment strike due to over-propertisation of knowledge (Pagano & Rossi 2009). It is no surprise, therefore, that there has been a proliferation of projects devoted to understanding the ‘economics of research’. 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 research, 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).

Yet such a project has failed even on its own terms,despite some fifteen years of intense research activity regarding related issues. 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 research (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(Mirowski 2009: 138-9)

But similar conclusions may also be reached by consideration of the second half of this phrase, namely what is the economics of research an economics *of*? This question shifts our attention to a slightly different literature on the commercialisation of research. Assuming a mainstream economic perspective, 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 research 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 research as a progressive development without any complications or problems for scientific research.[2]

[2] See e.g. Etzkowitz *et al.* (2000)

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.[3] The commercialisation of research 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 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 research (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 research and the knowledge economy more broadly*, yet 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 research offers a

[3] See e.g. Krimsky (2003).

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).[4]

We now turn, therefore, to a brief illustration of the difference critical realism makes, 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 critical realist analysis listed above.

From EEI to critical realist economics of research

The school of thought that is variously called ‘innovation studies’ or (as we shall call it here) the ‘evolutionary economics of innovation’ (EEI) has contributed most to current understanding of the processes of technological change and innovation. Its inclusion in any research programme on the ‘economics of research’ is thus hardly short of obligatory. From the perspective of a critical realist economics, 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 research and strong intellectual property rights, namely the linear model (as just discussed) and the ‘public good argument’.[5] 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

[4] For a collection of the growing body of work taking this issue seriously, see (Tyfield et al., forthcoming).

[5] 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)

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, innovation and technical change are evolutionary processes, a "groping" (Nelson & Winter 2002), and their outcomes are fundamentally uncertain and unpredictable (Rosenberg 1994, Dosi 1988).
2) Secondly, therefore, EEI is also explicitly critical of the cursory way mainstream economics "black-boxes" technological (and scientific) change, since it cannot incorporate such change into its models (Rosenberg 1982). Similarly, against the fetishism for mathematical modelling and deductive 'rigour', for EEI, study of actual technological and economic change demands a much greater (inter-disciplinary) role for history (Freeman & Louça 2002) and qualitative or 'appreciative' theories (Nelson & Winter 1982).

The fifth reason concerns the 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. This work is of particular interest for an economics of research since it provides a meso-level historical context for understanding changing economic conditions of, and demand for, research and innovation. ETC, like EEI, also has significant strengths. First, it too is an explicit critique of 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.[6] 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.

[6] See e.g. Verspagen (2006).

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 commodity market and finance does not have any emergent effects on the development of the economy (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.[7] 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[8]

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 research coincided with 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.

EEI thus conclusively shows that *there is, and can be, no such thing as a single, abstract 'economics of research'*. 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

[7] Though compare Freeman & Perez (1988: 47) with Dosi (1982).

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

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 research 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 research 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 research is thus an intensely political phenomenon, not just a techno-economic one associated with the emergence of a ‘new (knowledge-based) economy’.

Perez also 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 thus 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 research (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).

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 2003). 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 did it chasten finance capital more generally and bring it to heel, nor did it place meaningful 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 cyclical 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 critical realism 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 research. 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 critical realism 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 now 'show its hand' in this respect.

Here, we have explored significant contributions, direct and indirect, of critical realism to an economic project of considerable contemporary importance: a critical, explanatory economics of research or, more accurately, a cultural political economy of research and innovation (Tyfield 2012a, b). 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, critical realism is a crucial parallel project and one that yields major gains at the level of substantive understanding of our contemporary situation – and thus the significant contribution it could make to a charting a course beyond the 'triple crises'.

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