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MAINSTREAMING. REFLECTIONS ON THE ORIGINS AND FATE OF MAINSTREAM PLURALISM

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**MAINSTREAMING.
REFLECTIONS ON THE ORIGINS AND FATE
OF MAINSTREAM PLURALISM**

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Abstract

There is considerable discussion on the so-called “mainstream pluralism”, which stems from the growth and coexistence of new research programs in economics that significantly deviate from the neoclassical core. Other disciplines have actively contributed to the birth of such programs, that are carried on by different, often separated communities of researchers. Although “mainstream pluralism” is not the pluralism heterodox economists and students groups have sought for in the recent decades, its persistence over time might provide a possible precondition for the advent of pluralism in economics. While the literature tends to regard mainstream pluralism as a transitory state towards a new, post-neoclassical, mainstream, this paper contributes to the debate by bringing in a different perspective, focusing on economics’ fragmentation and the necessity of specialization. We adopt a “late Kuhnian” framework (derived from Kuhn’s late works on specialization), considering not scientific revolutions but specialization as key engine of progress in science, and interpret mainstream pluralism as the result of economics’ recent growth in size and diversity. To account for the necessity of specialization in economics, we employ Ronald Heiner’s work on the competence-difficulty gap, as well as the evidence offered in some recent studies about the impact of the “burden” of previously accumulated knowledge on innovative behaviour. After a bird’s eye view on the recent history of economics in relation to other disciplines (and an analysis of Herbert Gintis’s “unity of behavioral sciences” proposal as possible new mainstream), we discuss the possibility that today’s “mainstream pluralism” might persist over time.

Keywords: Pluralism; Mainstream economics; Specialization; Burden of knowledge; Economics in relation to other disciplines

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“With much reluctance I have increasingly come to feel that this process of specialization, with its consequent limitation on communication and community, is inescapable, a consequence of first principles. Specialization and the narrowing of the range of expertise now look to me like the necessary price of increasingly powerful cognitive tools”
(Kuhn 2000, 98)

“Not for them [our successors] the grand unifying theory of particle physics which seems to beckon physicists. Not for them [...] the pleasures of theorems and proof. Instead the uncertain embrace of history and sociology and biology”
(Hahn 1991, 50)

o. Introduction

There is considerable discussion, in current economics, on the possible future scenarios of the discipline. Historians and methodologists, in particular, are debating on the so-called “mainstream pluralism” (Davis 2006, 2008), which stems from the growth and coexistence of new research programs in economics that significantly deviate from the neoclassical core. Other disciplines have actively contributed to the birth of such programs (evolutionary game theory, behavioural, cognitive and experimental economics, experimental economics, neuroeconomics, and agent-based complexity economics), that are carried on by different, often separated communities of researchers. True, economics has never been an entirely cohesive discipline. Still, the contrast between today’s “mainstream pluralism” and the decades when many leading mainstream economists were praising the virtue of the “imperial” attitude of their discipline (built upon the relative strength of the neoclassical core) is quite evident. In the last two decades, a constellation of not necessarily interconnected criticisms of neoclassical economics has produced, in effect, a noticeable number of niches, each trying to solve specific scientific puzzles by the use of distinct theories and methods. And although “mainstream pluralism” is not the pluralism various non-mainstream economists and students groups have sought for in the recent decades, its persistence over time might at least provide a possible precondition for the advent of (true) pluralism in economics.

One might mention the weakness of the (neoclassical) approach (see e.g. Colander 2000, Elsner 2013) as possible explanation of the proliferation of niches, or adopt a sociological perspective, and claim that the creation of niches can help to develop a successful academic career (Ben-David and Collins 1991). Still, economics *did* have a strong paradigm, to the extent that the current discussion about mainstream economics presupposes that its pluralism cannot last indefinitely. The (often implicit) use of a

Kuhnian (but see below, section 1) perspective easily generates the idea that current pluralism is but a transitory state towards the advent of a new, post-neoclassical, mainstream, exploiting overlaps and shared concerns between today's different research programs. This new mainstream might rest for instance, it is argued, on the "vision" of complexity sciences, bringing to completion the "revolution" of complexity (Colander, Holt and Rosser 2010); or on economists' contribution to an encompassing framework making behavioural disciplines finally compatible with each other, as in Gintis's (2007) proposal.

This paper wants to contribute to the discussion by bringing in a different perspective. In particular, to discuss the current fragmented state of economics, we adopt a "late Kuhnian" framework (derived from Kuhn's – 2000 – late works on specialization), considering not scientific revolutions but specialization as key engine of progress in science. We therefore suggest elements for an interpretation of current mainstream pluralism as the result of economics' recent growth in size and diversity, and therefore as an indirect by-product of specialization. To account for the necessity of specialization in economics, we employ Heiner's (1983) work on the competence-difficulty gap (and, in general, the emergence of regularity in contexts shaped by uncertainty and complexity), as well as the evidence offered in some recent studies about the impact of the "burden" of previously accumulated knowledge on innovative behaviour (e.g. Jones 2009). We thus point at specialization as an economizing (and sustainable) way of reaching the frontier of economics by reducing both the competence-difficulty gap and the potentially paralyzing burden of knowledge, and highlight its potential significance for today's mainstream pluralism. Moreover, the paper offers a bird's eye view on the recent history of economics in relation to other disciplines, with the specific aim of highlighting the significance, for both the future of economics and the changing pattern of relationships between economics and contiguous disciplines, of Gintis's "unity of behavioral sciences" proposal as possible new mainstream in economics.

1. Pluralism and "mainstream pluralism"

Pluralism in economics is now a hotly debated issue. It is now usual to consider Hodgson, Mäki and McCloskey's "plea for a pluralistic and rigorous economics", published as advertisement in volume 82, issue 2 of the *American Economic Review*, in 1992, and signed by forty-four leading economists, as the birth of the recent discussion around the issue.

The initiative was funded by the Foundation for European Economic Development. A year later, the International Confederation of Associations for Pluralism in Economics was established, then followed by the Post-Autistic Economics movement, that stemmed from French economics students' petition, in summer 2000, for broadband approaches to economic teaching. The issue was raised again in 2001 by the "Cambridge 27" group of 27 PhD candidates at Cambridge UK ("Opening Up Economics" was the title of their manifesto), as well as by the students from 17 countries, in that same year, who released an "International Open Letter" asking for a reform of economics education. Due to the crisis, which brought new interest to the issue, new requests appeared. The Foundation for European Economic Development launched a new appeal, in November 2008, to "revitalize" economics; along the same lines, in 2012, Krugman and Layard proposed a "Manifesto for Economic Common Sense". In 2013, the Institute for New Economic Thinking has launched the CORE (Curriculum in Open-Access Resources in Economics) to develop a new economics curriculum. Then, much attention has been given to the Post-Crash Economics Society, founded in 2014 by some students of the University of Manchester, calling for a revision of the economic syllabus used in that same university. Finally, an International Student Initiative for Pluralist Economics was established in early 2014, on the initiative of various groups of students from different countries. Pluralism has finally become the topic of many recent non-mainstream academic conferences.

As Sent wrote on reviewing pre-2003 pleas for pluralisms, "implicit in all these appeals is the observation that economics lacks pluralism" (Sent 2003), and the same holds for more recent initiatives. Still, curiously enough, there seems to be a consensus, in the non-mainstream literature, that neoclassical economics is "dead", as Colander (2000) would say, or however "exhausted" (Elsner 2013), and that the mainstream is "changing face" (Hodgson 2007, Colander, Holt and Rosser 2010). Economics is in fact experiencing a flourishing of new research programmes that differ, more or less starkly, from standard neoclassical approach, and vary considerably in their perspectives and aims. In brief, and as a result, we would be possibly witnessing the passing "from neoclassical dominance to mainstream pluralism" (Davis 2006).

Such pluralism "is everywhere evident" in the practice of economics (Caldwell 2013, 758). It is quite obvious that "mainstream pluralism" is not what the above-mentioned non-mainstream academics and students have sought for in the recent decades. On one side, neoclassical economics continue to dominate pedagogy (Davis

2008), to the extent that the growth of interest in (and literature on) economic pluralism has itself been a sign of the unprecedented control of the neoclassical paradigm on economic thought (Dobusch and Kapeller 2012). On the other, the development of plurality in orthodox economics goes along with adherence to a considerable degree of monism with respect to methodology (Dow 2008). Still, there are opposite tendencies to hold into adequate consideration.

On one hand, the appeal of pluralism has been reinforced by the discontent that the unforeseen 2008 crisis and the subsequent economic downturn have instilled within the profession and in policy-makers. In Elsner's (2013) Keynesian allegoric account, there are cracks in the orthodoxy and mainstream citadel: heterodox economics is getting nearer to the walls to besiege them. This does not mean that future economics will necessarily be pluralistic. The replacement of neoclassical economics by one of the various paradigmatic alternatives from the heterodox scene (post Keynesian, Marxian, institutionalists, and so on) would paradoxically provide continuity with the defeated former dominant paradigm in imposing an equally monist (stand-alone) alternative. This was in essence the aim of "first-wave pluralists", who have fostered pluralism in the special sense of having attempted at making method contestable in economics (Garnett, Olsen and Starr 2010). On the contrary, "second-wave pluralists", since Hodgson, Mäki and McCloskey's 1992 *AER* petition, call for "a spirit of pluralism", implying the possibility of "critical conversation and tolerant communication between different approaches", as the petition reads. If Hodgson is right in claiming that the changing face of mainstream is visible from the appearance of some institutionalist features, such as evolutionary ideas, "previously the longstanding preserve of mavericks and dissidents" and now "commonplace" (Hodgson 2007, 7), this might be an indication of a willingness to reach pluralism by enlarging a formerly reductionist perspective.

Yet, on the other, "mainstream pluralism" might simply reflect a plurality of theories without concern for pluralism, that is, if we accept Mäki's (1997, 38) definition, without involving a "theory or principle that justifies or legitimizes or prescribes the plurality" of approaches. It is certainly true, as Dow (2008, 77) writes, that this plurality shaping the mainstream landscape, "is being unified by the shared purpose of a general systematization of agents' rational behavior under certainty and uncertainty conditions, including interactive behavior", as well as by formalism and general (negative) attitude towards methodological alternatives (see Dutt 2014 on the lack of clarity about the

meaning of –mainstream – “pluralism”). In this perspective, even complexity economics, one of the most serious challenge to the dominance of neoclassical economics, has a problem with the “exclusivity” of the methodology it promotes. Still, although it does represent a new, alternative “framework for economic thought” (Arthur 2014), and as such, that is as a stand-alone approach with its own theories and methods (Fontana 2010), a potential enemy for the (once?) dominant paradigm, it is currently concurring to impress a pluralist turn to mainstream economics.

The example of complexity economics is highly significant: Colander, Holt and Rosser (2004, 496) define complexity as the “defining factor of the new work at the edge of economics” that is changing the mainstream, as well as how the mainstream sees itself. The focus is not so much, therefore, complexity economics, since the mainstream has already accepted many of its methods and approaches. Rather, it is the “broader vision” of complexity (497), one that few economists would possess, that brings together the revolutionary but specific perspectives adopted by the different research programs of today’s mainstream. Evolutionary game theory brings in institutions; ecological economics imposes consideration for the interrelations between nature and economy; psychological economics provides a new way of looking at rationality; computer simulations redefine models; experimental economics modifies empirical work, and complexity theory transforms equilibrium in a complex notion. “As work at the edge progresses and accumulates”, Colander, Holt and Rosser (2004, 497) write, “it shifts the center of the economist’s approach, and ... eventually will create a new orthodoxy centered on a broader complexity vision”.

In sum, even after leaving to experts in this topic the task of forecasting its possible impact on the future of pluralism itself, “mainstream pluralism” is and should be, in any case, a phenomenon of interest. Still, the relevant literature tends somehow to undervalue its conceptual autonomy. It is held, directly or implicitly, that mainstream pluralism matters because it can play a role in helping the discipline to definitely depart from the formerly dominant neoclassical core, and in shaping the future of economics itself. In Colander, Holt and Rosser’s (2004) view of changes in economics – evolution, not revolution, owing to the accumulation of evolutionary changes –, mainstream pluralism is but a transitory state, moving in the direction of progress. A misnomer, in truth, for the big picture behind is the advent of the complexity era: this is what future historians of economic thought will remember of our epoch (Holt, Rosser and Colander 2011). Davis

(2008) sees the current mainstream pluralism in terms of competition between new research programmes, but notes that despite the heterogeneity of the landscape, stemming from separate origins in different disciplines, mainly outside economics, and their being implemented by distinct communities of researchers, it is becoming evident that such programmes share common concerns and fields of inquiry. This creates, in Davis' (2008, 350) view, "the possibility of a new general research programme for economics that would abandon much of neoclassicism". Similarly, Hodgson (2007) believes that the final result of today's pluralism might be the advent of a new mainstream, evolutionary and institutional economics replacing the neoclassical approach as its core. Davis (2008, 350) again: "the proliferation of new approaches in economics may reflect a transitional state of affairs, which may give way to new orthodoxy and a new mainstream in the future rather than a more pluralistic economics".

Davis, however, assigns a precise role to the current mainstream pluralism. For this latter would be one of the two phases of a cycle, that shaped by the succession of periods of dominance of single approaches and of periods of pluralism of approaches. The decaying of dominant approaches is replaced by pluralism, and from pluralism a new orthodoxy emerges, this is what the history of economics tends to suggest. One of us (Fontana 2010) has recently provided an explanation potentially able to reconcile Colander, Holt and Rosser's (2004) approach and Davis' "cycle" theory. The neoclassical attempt to subsume the complexity approach under the Neoclassical/Samuelson paradigm is a good illustration of a strategy that can be defined as "oil spot dynamic", whereby neoclassical economics ensures its survival by encompassing criticisms emanating from competing approaches. This requires an elastic paradigm, well captured by the metaphor of an oil spot that however loses depth while growing in extension (Fontana 2010).

Note, however, that this dynamic does not necessarily require the advent of a new dominant paradigm once the neoclassical one has become a crust, one too thin and precarious. Mainstream pluralism may show a more lasting nature, and eventually even "degenerate" into a state of pluralism tout court in economics. Remarkably, Davis refers to an article by Gintis (2007) to suggest the idea that "debate has now ... begun over possible components of a post-neoclassical single mainstream approach in economics" (Davis 2008, 351). This provides another reason to consider the current mainstream pluralism as an interesting research question. For in that contribution, Gintis explicitly attacks

behavioural disciplines (believing it “scandalous”, 2007, 15) for failure to develop, throughout the twentieth century, a “common underlying model” of human behaviour, and provides the bases of a theoretical framework expressly aiming at unifying behavioural sciences. The project rests on two main pillars, namely evolutionary thinking and (evolutionary) game theory, but in truth, many research programmes of current mainstream pluralism – behavioural economics, complexity economics, and so on – are called upon to contribute to the new framework, having created, according to Gintis, the preconditions “for rendering coherent the areas of overlap of the various behavioural disciplines” (2007, 1).

The interesting aspect of Davis’s remark is that he associates Gintis’s “unity of science” proposal, explicitly a rebuttal, and a condemnation of social-sciences pluralism, with the likely development of a new, post-neoclassical mainstream *in economics*, arising out of a synthesis of different, non-neoclassical research programmes, which however would have been constructed with the aim of overcoming disciplinary boundaries. The least one can say is that the pathway towards a new orthodoxy might not be as linear as the literature on mainstream pluralism would suggest. An inquiry into the (possible) origins of such pluralism and the causes of its emergence might throw light on the conditions that might favour its persistence, especially those most clearly related to economics’ status as one living in a *milieu* of social sciences.

2. Evolution and revolutions in economics, the narrative(s)

To an external observer, economics might look like a system of celestial bodies gravitating around the neoclassical planet. The metaphor seems to work well, in that the relationships between the coexisting views of mainstream economics is regulated by a non-trivial set of attracting and repelling forces. Actually, either implicitly or explicitly, a substantial part of the literature employs a metaphor of this kind to discuss the current configuration of the discipline of economics (see Dobusch and Kapeller 2012, Davis 2012, Fontana 2010).

The history of complexity economics provides a fitting example of this kind of relationship. Born out of the Economics Program hosted by Santa Fe Institute for the Study of Complex Systems in the period 1998-2004, the “complexity perspective” had been initiated with the explicit task of overcoming the limitations of the neoclassical theories. Kenneth Arrow, who chaired the foundational workshop, showed considerable openness

towards the possibility of complementing the standard view with tools “imported” from other disciplines, especially physics (Arrow 1988, 280-281). Certainly, the purport of the complexity-economics to-be was not meant as “revolutionary”: the contributions of physics would have helped in solving specific problems, such as those created by multiple equilibria and chaos in time series. Neoclassical theory was “bolting on” a new armoury in order to accommodate a set of phenomena that escaped treatment with traditional tools. However, the interdisciplinary research team that was in charge of the program soon trespassed the initial border. In the mid Nineties, Arthur – at the time director of the Economics Program -- stated that standard-equilibrium economics was to be used mainly as a benchmark, and that complexity economics rejected the main neoclassical postulates (equilibrium analysis, rational expectations and Olympian rationality): consequently, it was to be considered as an autonomous approach (Arthur 2003). Starting from 1997, the breadth of the Program went through a further twist. The research team, finally displaying essentially economists and physicists, progressively but significantly reduced its heterogeneity, and the Program changed attitude towards the neoclassical approach – far less belligerent. In the introduction to the proceeding of the last workshop of the Economics Program explicitly dedicated to complexity economics, L. Blume and S. Durlauf (2006) argued that complexity models “do not represent any sort of rejection of the neoclassical approach” and that “[neoclassical] theory was able to absorb SFI-type advances without changing its fundamental nature”.

Complexity economics is currently identified with the set of ideas developed at the apex of the innovative strength of the Economics Program, and is counted by one of his leading figure (Arthur 2014) as one of the many research programs that are part of the discipline. The way in which its role inside (or outside) the mainstream has been interpreted and discussed is representative of the terms of the current debate on mainstream pluralism. The first noticeable issue is that the plurality of research programs is usually considered under the light of the now consolidated classification of orthodox, heterodox, and mainstream economics (see Dequech 2007, Colander, Holt and Rosser 2004, Davis 2008). The terms “heterodoxy” and “orthodoxy” are intellectual categories, whereas “mainstream” refers to the social dimension of the profession. Orthodoxy “generally refers to what historians of economic thought have classified as the most recently dominant ‘school of thought’, which has been long recognized as being ‘neoclassical economics’” (Colander, Holt and Rosser 2004, 490), while “heterodoxy”

implies making a departure from the orthodox theories and standards. “Mainstream” economics

consists of the ideas that are held by those individuals who are dominant in the leading academic institutions, organizations, and journals at any given time, especially the leading graduate research institutions ... mainstream economics usually represents a broader and more eclectic approach to economics than is characterized as the recent orthodoxy of the profession (ibid.)

Mainstream economics is thus generally wider and more variegated than orthodoxy, and while the latter tends to be more stable in time, the former exhibits more permeable boundaries. In this sense, it becomes possible to interpret complexity economics as born at the border of orthodoxy and mainstream economics, and to claim that it has subsequently moved to more heterodox positions (Fontana and Corsatea 2013).

It is worth noting that economics is probably the only social science that heavily relies on this terminology. In line with the sociology of scientific knowledge, Davis (2008) explains this distinctive trait with the necessity of defending the autonomy of the discipline both from neighbouring endeavours and from the hard sciences. The idea of having a strong consensus within the discipline would be functional to the defence of the domain from other similar communities of scholars that might invade those research areas that economics has identified as its domain. Moreover, the distinction between orthodoxy – with the attached meaning of best scientific approach – and heterodoxy – with the attached meaning of unscientific – allows economics to be seen from the outside as a somehow reliable and cohesive discipline in the ambit of policy. For what concerns the hard sciences, the divide serves the purpose of hiding the fact that economics has a plurality of views and values inspiring research and therefore that it is not as ‘pure’ as, say, physics. Hiding or minimizing value judgments behind research in a domain with strong empirical vocation is fundamental: as (the dismissal as unscientific of) a heterodoxy is needed to strengthen the “scientific” character of economics (and its orthodoxy) itself, pluralism would make value judgment simply too apparent, and thereby disrupt the image of economics as a science.

Secondly, the relation among the three categories has often been described by adapting concepts and terms employed by Thomas Kuhn in his *The Structure of Scientific Revolutions*. A loose Kuhnian framework thus structures the debate, even though Kuhn never mentioned orthodoxy and heterodoxy in his account of scientific revolutions.

According to this view, the epistemic progress of economics takes places through a series of paradigm shifts preceded by spells of pluralism. Orthodoxy and mainstream embody the prevailing paradigm at a given moment in time. This is forged out of competition among concurrent approaches, and the prevailing paradigm wins thanks to the novelty of its finding, the openness of its ends, as well as the ability it displays in attracting a stable community of scholars from other approaches (Kuhn 1970[1962], 10). The juxtaposition between orthodoxy and heterodoxy is therefore inherently dynamic: the composite economic heterodoxy is the cauldron from which the new paradigm/orthodoxy will emerge. More importantly, the relevance of the vocabulary used in the debate seems to go well beyond the mere narrative device: in this view, it would be the (more or less explicit) motivation to become the “next paradigm” to drive the research agendas of heterodoxy. It is the so-called “paradigmism”: in the post war period, “leading theorists in each of these groups [Institutionalists, Post-Keynesians, Marxian and Sraffian economists] aspired for their approach to become the new master framework, the new “general theory”, to which other theories would be subsumed as special cases” (Garnett 2007, 524). The “complexity approach”, again, would fit well with this reasoning: “in this context”, writes Arthur (2003, 11) referring to the Economics Program under his own direction, “standard-equilibrium economics became a special case”.

Symmetrically, the existence of orthodoxy would be necessary to the survival of heterodoxy. In order to develop the new general theory, this latter needs in fact a cohesive and well-defined oppositional paradigm. This attitude has the peculiar effect of revitalizing the same paradigm that heterodoxy is trying to supersede. In fact, the vision of the Neoclassical paradigm is often more vivid in the mind of its opponents than in the perception of its members. In other words, the neoclassical paradigm could have lost its intellectual binding power – the orthodoxy no longer guides research – (Blaug 2003) and allowed the development of a multifaceted and somehow pluralistic mainstream. This is not kept together by the neoclassical theoretical and methodological tenets, rather it resembles to a community whose ties are based on reputation and reciprocal recognition but in scholars operate in quite distinct and autonomous ways. If this is the case, and there are reasons to believe that it is, one can reasonably argue, as a first approximation, that economics nowadays is profoundly pluralistic both in its heterodox (Davis 2008) and, more surprisingly (but see Holcombe 2008), mainstream components.

The attempt at disentangling the descriptive and normative aspects of the general

issue of pluralism looks particularly difficult and intriguing. On the one hand, economics is, as a matter of fact, more pluralistic than it had been in the past. On the other hand, heterodox scholars and students are currently and deliberately invoking pluralism. An “interested”, rather than “selfish” (pluralism is accepted only as transitional solution before the advent of a paradigm shaped by the preferred tradition) or “disinterested” (genuinely inspired to tolerance for the coexistence of different traditions, see Dobusch and Kapeller 2012, 1043) pluralism.

On the top of these facts and aspirations, however, the idea that a new “paradigmatic mainstream” might emerge at the horizon is unsurprisingly alive. After all, as Nickles (2013) argues, “somewhat ironically, Kuhn’s attempt to revolutionize the epistemology of science has had a wider social impact than many scientific revolutions themselves”. The traditional Kuhnian interpretation of pluralism sees the development of science strictly connected to the existence of a paradigm: even if there can be “a sort of scientific research without paradigms”, these latter have a priority in that they allow the accumulation of knowledge (Kuhn 1970[1962], 12). Therefore, pluralism is of relatively little importance for what concerns both phenomena of interest and methods of research.

In the *Theory of Scientific Revolutions*, the emergence of a plurality of views is a revealing sign of an approaching revolution. According to Kuhn, in fact, normal science does not encourage the exploration of new phenomena. Quite the contrary: those phenomena that are not within the boundaries traced by the paradigm or cannot be analysed with the tools it provides are ignored (Kuhn 1970[1962]). When normal science faces anomalies, facts that cannot be accommodated within the paradigm, it produces a “proliferation of divergent articulations (more frequently the will come to be described as ad hoc adjustments” (*ibid.*, 83) that results in a progressive loss of coherence and cohesion of normal science itself, up to the point at which the paradigm implodes.

Heterodoxy can be seen as emerging from the attempt at dealing with those anomalies in ways that are not allowed in normal science. As a consequence, an increasing number and strength of heterodox approaches would reveal the crisis of the paradigm: as Colander, Holt and Rosser (2004, 487) observe, “a large variance in acceptable views, such has emerged in the profession over recent decades, signals that changes are likely in the future”. The very existence of the lively debate on the future of economics would be a symptom of an approaching revolution since normal science does not need “written rules”, as long as paradigm and models are perceived as secure. The moment when such

perception vanishes – namely, the pre-paradigmatic period, characterized by the coexistence of competing theories – “is regularly marked by frequent and deep debates over legitimate methods problems and standard of solutions, though these serve rather to define schools than to produce agreement” (Kuhn 1970[1962], 48).

To a large extent, the idea of a natural alternation between pluralism and paradigm resonates in comments and analyses of the current state of economics. An emerging corpus of literature is discussing the end of neoclassical dominance. Reasons adduced for this decay include the incorporation of anomalies into the paradigm, diluting and perverting its core and thereby fostering competition (Fontana 2010, Bronk 2011; see also Palley 2013. For a criticism of this argument, see Elsner 2013). This echoes Kuhn’s account – “the assimilation of all new theories and of all sorts of new phenomena has in fact demanded the destruction of a prior paradigm and a consequent conflict between competing schools of scientific thought” (Kuhn 1970[1962], 96). A more detailed explanation is provided by Davis (2006), who suggests three (possibly concurrent) causes for the end of the neoclassical era: the “breakdown view”, the “outside takeover view”, and the “maturity view”.

The breakdown view refers to the post-war dominance of the axiomatic approach, that while providing unity to the discipline through the analytical language, has simultaneously erected insurmountable walls around the object of research. The discovery of flaws in the fundamental propositions of general equilibrium theory has deprived the paradigm of one of its constitutive elements: the openness of ends. Impossibility theorems - mainly Arrow’s, Sen’s and Mantel’s, Sonnenschein’s, and Debreu’s – have shown that neoclassical economics could not deal with crucial questions such as social choice and that some of its pillars, namely uniqueness and stability of equilibrium in general equilibrium models, were unreliable. Similar findings spread the idea that only minor progresses, mainly in the form of refinements, would have been possible within the neoclassical framework, too few to make it appealing to the new generation of scholars. This way, in Kuhnian terms (1970[1962], 10), the paradigm would have lost another of its necessary property: the ability to attract a stable community of scholars from other approaches.

The “maturity view” is essentially a nuance of the previous one, with less emphasis on “flaws”. Each approach, it is rather argued, has inherent limits: given its theoretical premises and related tools, only a determined set of phenomena can be explained. Once

all these phenomena unfolded, the paradigm dies of natural death, having served its purpose (Colander 2000, 2001). An agonizing paradigm is obviously not attractive for scholars that rather move towards the borders and often cross the frontier of the research programme they are working within. Arrow's talk at the inaugurating workshop of the "complexity approach" is very much along this line:

The general perspective of mainstream (the so-called neoclassical) economic theory had certainly had some empirical success (...) But it is clear that many empirical phenomena are not covered well by either the theoretical or the empirical analyses based on linear stochastic systems, sometimes not by either (Arrow 1988, 278).

However, the solution he envisaged leads to consider the complicated relation that economics keeps up with other endeavours. Arrow asked other participants to the workshop – mainly physicists – to provide the missing tesserae to repair the neoclassical mosaic. Paradoxically, Arrow's intention at the revitalizing general equilibrium theory turned evil, and gave birth to the (irreconcilable) tenets of complexity economics. In more general terms, the mechanism is depicted in the "outside takeover view". The latter shares with the "breakdown view" the idea that the neoclassical paradigm has crumbled due to the inclusion of "irreconcilable" concepts. However, Davis directs here his attention towards conceptual imports from other disciplines. Although a benefit for well-structured framework such as the neoclassical one, the accumulation of such extra-tribal contributions could have "transformative effects" in the long run. For instance, the massive injection of mathematics within the discipline has turned the neoclassical approach into the "contemporary formalist mainstream" (Davis 2006, 15) that does not strictly resemble its ancestor.

As said (the following sections provide direct evidence), Kuhn's 1962 account of scientific revolutions is probably the most widely used still nowadays. At a closer inspection, however, the literature on "recent" economics and mainstream pluralism employs a Lakatosian interpretation of scientific progress. To allow for the emergence of new research programs in mainstream economics, departing from the neoclassical core but also different from traditional heterodox programs, Davis (2008, 2012; see Boumans and Davis 2010) uses a core-periphery model that expressly "transfers the general idea behind Lakatos's hard core/protective belt account of individual research programs to entire field or disciplines" (Davis 2012, 212). According to Lakatos's (1970) (see Backhouse 2004 for a discussion of Lakatos's methodology of research programmes as applied to

economics), each research programme consists of a “core”, that is a set of assumptions that are considered as irrefutable (i.e., without which the programme itself would come to an end), and a “protective belt” made of assumptions, procedures, and testable theories, ensuring the possibility to apply the “core” to specific problems. These latter assumptions are dispensable, i.e. they can be modified when “anomalies” (to use Kuhn’s term) are discovered that render their subsequent use problematic. The main reason why a Lakatosian framework is adopted when dealing with today’s mainstream pluralism, and despite the various criticisms Lakatos’s methodology has received (see Backhouse 2004, Drakopoulos and Karayannidis 2005), is that it explicitly admits the possibility of different, simultaneously competing research programmes (Boumans and Davis 2010). Lakatos’ attention, however, went to the problem of how to choose between such programmes, and the very concepts of “progressive” or “degenerating” (the former are able to predict new facts) programmes were introduced to this end (for an application to economics, see De Marchi and Blaug 1991).

The literature on “recent economics” applies Lakatos’s methodology to distinguish between the subset of research programmes constituting the “core” of the discipline and another subset that represents its “periphery”. Still, this periphery is not to be intended as “protective belt”: rather, the research programmes belonging to the “periphery” simply occupy a marginal position, with respect to the core. In other words, “they pursue questions and issues removed from core concerns and often at odds with core assumptions” (Davis 2012, 213): up to the Eighties, taken together, such programmes represented the “heterodoxy”. And it would be here that economics can meet other disciplines, sharing with them assumptions and theoretical frameworks; an interaction that may even result in the creation of new research fields. This has noteworthy implications for economics: on one side, it becomes possible to argue that economics can be significantly affected by such encounters with other disciplines, and can even undergo a process of transformation (Davis 2008). On the other, as opposed to a purely Kuhnian framework, the Lakatosian methodology allows for a more flexible perspective on the evolution of economics. While identifying various different strains of contemporary economics as forebears of the “new” era, the complexity era (Holt, Colander and Rosser 2011), Colander, Holt and Rosser (2004, 488) argue that changes “occur within the mainstream of the profession in a way that is not apparent to the mainstream. These changes do not lead to sudden paradigm shifts, but instead lead to cumulative

evolutionary changes that ultimately will be recognized as a revolutionary change". Therefore, the use of a Lakatosian methodology is also justified by the less restrictive assumptions it makes about the origins of the expected (ex post recognizable as) revolution: this latter can "come from within and will not be noticed for years" (489).

In this light, today's "mainstream pluralism" itself becomes a solid motivation to discard Kuhn's methodology (advocates of pluralism in economics employ similar arguments when criticizing the "paradigmism" of first-wave pluralism; see Garnett 2007). Yet, the adoption of Lakatos's "gradualism" does not discourage today's scholars from embracing a cyclical model to explain the evolution of economics. To the possibility that economics might "simply become more pluralistic for the indefinite future", Davis (2008, 350) opposes in fact a reading of the history of economics wherein the play between orthodoxy and heterodoxy creates an alternation between phases of dominance and situations of pluralism: from dominance comes pluralism, and from pluralism comes dominance. He identifies five "pluralistic environments" that later gave way to dominance: first, "the transition from classical to neoclassical economics in nineteenth-century Britain". Second, "the *Methodenstreit* between the German Historical school and the early Austrians"; third, "the multiple approaches to labour and monetary economics in post-Marshall Cambridge". Fourth, "the interwar competition in the USA between institutional and neoclassical economics"; and finally "the 1970s debate between proponents of monetary and fiscal policy in the ISLM framework" (350-51).

Thus, while Kuhn's is substituted for by (a modified version of) Lakatos's methodology, the vision remains cyclical. Exactly like, Kuhn's critics would remark (see Weinberg 2001), Kuhn's own theory, wherein a revolution, after a period of pluralistic turbulence, creates the conditions for the advent of a paradigm and the development of "normal" science (there is therefore temporary pluralism; or pluralism about extraordinary research, but not about normal science, see Mäki 1997). Moreover, when Davis, among others, postulates the implausibility of a future non-monistic mainstream despite the coexistence of different research programmes in today's mainstream, he is somehow coming close to a Kuhnian characterization of the state of contemporary economics as "pre-paradigmatic": "the return of orthodoxy" will put an end to the disorder of today's (mainstream) pluralism.

3. Mainstream pluralism: a suggested explanation

If mainstream economics can be reasonably defined as pluralistic, this means that it is fragmented, as the literature notes. Fragmentation refers both to the “technically-driven specialisms” produced by the dominance of formalism (Hodgson 2007, 11) and to the coexistence of heterogeneous research programs in the mainstream, all significantly deviating from the neoclassical core, as in Holt, Rosser and Colander’s (2011) perspective. When claiming that a “complexity” era in economics is replacing the “neoclassical” one, Holt, Rosser and Colander take for granted that modern economics has finally accepted that the economy is complex. Remarkably, their conception of the “complexity vision” as driver of change in economics rests on a *vision d’ensemble* of the various simultaneous “redefinitions” of the main tenets of the neoclassical approach operated by the various, heterogeneous research programmes of today’s mainstream economics. Here follows the catalogue:

evolutionary game theory is redefining how institutions are integrated into the analysis; ecological economics is redefining how nature and the economy are viewed as interrelating in a transdisciplinary formulation; behavioral economics is redefining how rationality is treated; econometric work dealing with the limitations of classical statistics is redefining how economists think of empirical proof; complexity theory is offering a way of redefining how we conceive of general equilibrium and economic dynamics more broadly; agent based computational economic (ACE) analysis is providing an alternative to analytic modeling; experimental economics is changing the way economists think about empirical work, with this being the principal method by which behavioral economics is studied.

Holt, Rosser and Colander believe that such changes are “ongoing” and “have, in varying degrees entered into the mainstream. As that has happened, there have been a broader set of changes in how mainstream economics sees itself”. This would signal “a new openness to ideas from other disciplines, making it a more transdisciplinary field”. Current mainstream pluralism, and the fragmentation of the discipline, would testify, to put it differently, that “modern economics is ... far more willing to question the special status of economics over the other fields of inquiry and to integrate the methods of other disciplines into their methods” (*ibid.*, 363).

If mainstream pluralism is so decisive in shaping the future of the discipline (as the recalled perspectives induce to believe), that of detecting its origins becomes an ineluctable task. With the relevant exception of Dow (2008), the recent literature has given little consideration to a crucial distinctive trait of current mainstream pluralism and a long-standing issue in economics – Schumpeter (1954, 22) drew attention to the

“indefinite number of specialties” in economics in his *History of Economic Analysis* of 1954 –, that is fragmentation. Still, this latter was reputed a relevant issue for the future of economics already in 1991, when a “philosophical” symposium of the *Economic Journal* was published, gathering reflections on “The Next Hundred Years” of the discipline. In his article, Pencavel (1991) bet on the continuing growth of economics in both size and diversity. One on side, economists had fully demonstrated their usefulness and necessity, in (modern) societies highly relying upon the competencies and authority of expert systems; hence a future, for the discipline, of increased size. Moreover, the possibility of expressing mathematically its arguments would not secure to economics any longer, in the future, the same advantages it had enjoyed in the past. Hence increased “diversity”: economists would have increasingly exploited concepts, ideas and modes of reasoning from other disciplines.

After projecting into the future some trends of the past, Pencavel (1991, 81) argued that

The enterprise of economics will become bigger and more varied ... economic science will become an even more competitive activity than it is now. A rough pyramidal hierarchy will persist, but there will be a much wider base with many minarets representing local confluences of authority.

But the originality of Pencavel’s (1991, 86) predictions lies in stressing the essential role that specialization plays in the process. In his view, the main problem of “this fragmented world of specialization” is its “intractability”, one that “will present itself to each individual of being informed of ongoing research” (*ibid.*). Specialized in “very detailed subcategories” within the “broad categories of economics” (“specialization will attain new heights”), scholars will encounter great difficulties in maintaining an awareness of developments beyond their subfield:

It will be simply impossible for economists to be at all well-informed of developments in more than a few narrow fields of the subject. An attempt to make substantive contributions to many fields of economics is likely to result in an embarrassing display of ignorance (*ibid.*).

In a similar vein, Turnovsky (1991, 143) noted in his contribution to the symposium that economists are “ignoring, and being happy to ignore, other areas of the discipline”. This derives, in his view, from the maturing of economics:

as progress is made into understanding the various branches and processes of economics, more detailed knowledge and expertise is required. This involves investment on the part of the individual in certain analytical techniques, necessitating his specialisation to that subarea (*ibid.*).

But then, “where no one can be well versed in ongoing research in more than a few fields, the profession will assume a more pluralistic character” (Pencavel 1991, 86). This induces significant changes in the development of the discipline, but (for the same reasons) practitioners, seeing “minute change upon minute change”, can fail to mature “a perception of the aggregate of the changes” (Colander, Holt and Rosser 2004, 486).

In a nutshell: while growing in size, economics is destined to grow also in diversity. Economists specialize in specific areas of the discipline, and specialization produces decentralization: scholars simply cannot be abreast of theoretical developments occurring outside their specialty. This results in enhanced pluralism. Is this line of reasoning helpful in approaching the current “mainstream pluralism”? In other words, is it sufficiently grounded on theoretical foundations and available empirical evidence, to deserve attention as possible (part of the) explanation for the persistence over time of this pluralistic pattern? To answer these questions, we employ, respectively, the theoretical framework proposed by Ronald Heiner (1983) in order to investigate the origins of predictable behaviour, and the evidence offered in some recent studies (see Jones 2009) about the impact of the “burden of knowledge” on innovative behaviour.

In *The Origin of Predictable Behavior*, Heiner (1983) suggests that rules and institutions evolve in contexts characterized by uncertainty that prevents agents from complying with the standard neoclassical assumption of maximizing behaviour. Contrary to the received view, Heiner argues that it is “uncertainty in distinguishing preferred from less-preferred behavior” that induces agents to recur to rule-based behaviours, that are mechanisms inhibiting the flexibility to choose potential actions. Heiner locates therefore the origins of predictable behaviour as far away as possible from maximizing, and rather places them there where uncertainty prevails. The standard theory assumes no C-D gap, to use his own terms, that is no mismatch between one’s “competence” and “difficulty” in selecting most preferred alternatives. Conversely, in Heiner’s framework, uncertainty generates, and makes larger, this gap. In so doing, it also lowers the tolerance limit of the “reliability condition” that determines when the selection of a new action is sufficiently reliable for an agent to benefit from enhanced flexibility and the possibility itself to select that action. In other words, by lowering the probability of selecting the right action at the right time, uncertainty causes the agent to reduce the repertoire of possible actions among which to choose, favouring the adoption of behavioural rules (which are by definition more predictable). Among the illustrations of predictable behaviours one can explain by

making use of this framework is the dynamics of scientific inquiry. In particular, Heiner (1983, 575-576) states that:

The work of Thomas Kuhn (1962) (see also Popper, 1969; Lakatos and Musgrave, 1970) has emphasized a systematic pattern of resistance in the behavior of scientists to quick and sensitive reaction to new ideas and theories. Yet, when sufficient anomalies and awkwardly interpreted evidence about a previous theory build up, a major shift in ideas (a "scientific revolution") will relatively quickly occur. ... The Reliability Condition also implies other features in the behavior of scientists, such as: (a) resistance to accepting or using several competing theories unless there also exist easy to decipher (and reliable) criteria of when to switch between them; (b) similar resistance to incorporating new concepts or variables into accepted theories unless reliable criteria on how to use them are available (consider an economist's reaction to incorporating sociological variables into economic models); (c) differences in accepting and rewarding (salary, promotion, etc.) theoretical vs. empirical research in different fields depending on the reliability of observable data studied in those fields (for example, see Leijonhufvud's 1973 parody about "Life Among the Econ").

Now, in a Kuhnian perspective, Heiner claims that scientists tend to resist exploring the possibilities opened up by the introduction of new ideas and theories within the walls of an established paradigm. Uncertainty arising out of the complex environment of scientific research leads scholars to postpone the need for a change of paradigm. At least until, at the end of a "punctuated dynamics", the accumulation of anomalies has convinced them to consider the possibility of breaking with the paradigm, and finally to select this same "behavior".

Still, Heiner's framework can also be applied to the analysis of research specialization. Once the concept of "competence-difficulty gap" is made the main theoretical tool for a "cognitive" analysis of specialization, two opposite tendencies become observable. First, scholars may be biased in their perspective when specializing in a specific subfield of economics. Economics instruction and economics research, as Davis (2006, 4) maintains, are "the two main practices responsible for the replication of economics over time", and neoclassicism remains by far the main approach in instruction. As Elsner (2013, 293) notes, all "complexity issues" enter modern textbooks as peculiarities of, or add-ons to the basics of the "perfect market economy", an ideal they cannot affect by definition (for an exception see Elsner, Heinrich and Schwardt 2014). This suggests that scholars can find in the neoclassical core of the discipline a loyal ally helping them to constrain the C-D gap within acceptable bounds. This tendency is exacerbated by

the nature itself of neoclassicism as evolved in recent decades. Historically, the neoclassical world has in fact been extremely rigid, as shown by the “futility thesis” (Mirowski 2001) it tends to employ when proclaiming insignificant or, rather, non-existent, any fact, concept or construction that might make its assumptions appear questionable. More recently, on the contrary, due to the accumulation of “anomalies”, and – at least in part – as a necessary direct by-product of its “imperialist” attitude towards other social sciences, the elasticity of neoclassicism has greatly increased to allow the framework to encompass criticisms that come from alternative approaches (Fontana 2010). In general, however, mainstream economics would show a considerable degree of cohesion as regards methods, leading Dow (2008, 74) to assert that orthodox economics is “monistic in terms of methodology (one method: mathematical formalization)”. As a practical illustration of the relevance of such aspects, one can look at Palley’s (2013, 193) reconstruction of mainstream economics in the aftermath of the 2007-8 crisis. Even when compelled to admit the existence of “errors and omissions” imputable to the neoclassical theory, “Gattopardo economics takes on board ideas developed by critics of mainstream economics, but it does so in a way that ignores the thrust of the original critique and leaves mainstream analysis unchanged”.

In short, to restrict the argument to the cognitive dimension: if confronted with a sufficiently high “competence-difficulty” gap – itself arguably the result of a “growing” economics –, scholars might have strong incentives to limit the range of possible approaches and turn to the orthodox perspective. Holcombe’s (2008, 61) *excusatio non petita* – “mainstream economists are not rejecting heterodox ideas; they just see no reason to become familiar with them” – provides indirect confirmation of this bias. Now, this should naturally produce a more compact mainstream. Yet, the “obvious” strategy to cope with the C-D gap is necessarily less so when economics’ growth in diversity, not only in size, is considered, or rather, when it becomes evident that increasing size increases diversity. A less obvious but equally strong, and even stronger tendency is towards specialization. While growing size tends to introduce a bias in favour of mainstream as against the variety of alternative approaches, it also encourages economists to specialize in specific subfields.

The general problem faced by scholars might be defined as the “burden of knowledge”, as one of the most promising theories of the so-called new literature on the nexus between knowledge, age and great scientific achievements (creativity) is called. As

Jones (2009: 284) observes introducing an investigation of the effects of knowledge accumulation on technological progress, “if one is to stand on the shoulders of giants, one must first climb up their backs, and the greater the body of knowledge, the harder this climb becomes”. If s/he wants to “innovate”, that is to contribute significantly to deepen the understanding of a specific phenomenon, innovators (who “are not born at the frontier of knowledge”; *ibid.*, 283) must in fact reach the “frontier of knowledge”, and the burden of knowledge corresponds to the difficulty of reaching the frontier itself. The validity of this general remark is not limited to “technological” innovators: innovators in sciences are confronted with similar difficulties. Longer educational periods would therefore come consequently. In effect, the growing literature on life-cycle creativity shows, in general, that “age at great achievement” (a proxy for educational attainment) has significantly risen, among scientists, over the last century (see Tilghman et al. 1998; Jones, Reedy and Weinberg 2014). This result is consistent with longer duration of doctoral programmes, rising frequency of post-doctorates in the life sciences since the 1960s, and increasing age at doctorate for Nobel Prize winners over the twentieth century (Jones 2010; see Weinberg and Galenson 2005 for a study of the life cycles of Nobel laureates in economics).

Now, the lens provided by Heiner make it possible to visualize specialization as an effect of the competence-difficulty gap, that is, at least at an aggregate level, as an almost obliged by-product of the increasing sophistication of core economic theory. While the “mainstream bias” limits the range of possible approaches to the potentially infinite (after Robbins’ definition of economics as the science of choice) set of phenomena susceptible of economic analysis, specialization allows scholars to reduce the “uncertainty” intrinsic to any attempt to address the potential shortcomings of the established “paradigm”, or to originally contribute to its strengthening. Specialization would therefore emerge as part of a strategy to reduce the strength of the requirements posed by the reliability condition, which Heiner (1983, 566) aptly defines, in informal words, as a “potentially complex set of relationships between an agent’s repertoire and the structure of the environment”. Yet specialization is not only an active, pragmatic and essentially individual response to the widening of the competence-difficulty gap. It is also, as Jones (2009) maintains, an effective way of bypassing the problem of the “burden of knowledge”. Any innovation increases the burden of knowledge: to “innovate”, economists are left with the question of how to deal with increased knowledge without surrendering to its burden. To reduce the

C-D gap, they can therefore either learn more or narrow expertise; this latter might be the favourite option, owing to the not negligible costs involved in attaining broader education. It is to be noted that two effects – increasing educational attainment and increasing specialization – are two sides of the same coin, that is the “burden of knowledge”: “if the distance to the frontier were not increasing, then increasing education should be associated with broader individual knowledge, not narrowing expertise” (Jones 2009, 310).

It is not a difficult task to document growing specialization in economics. Suggestive evidence comes from the general and rapid increase in the overall number of academic journals, and in the growing importance of specialized periodicals – “the specialization of journals”, wrote Stigler, Stigler and Friedland (1995, 334), “will follow that of the scholars or professional practitioners”. Decreasing importance of generalist journals further corroborate the hypothesis: within top journals, only the *American Economic Review*, the *Journal of Political Economy* and *Econometrica* have hold top-ranking positions over the last three decades, while other prestigious journals such as the *Review of Economics and Statistics*, *Economica* and the *Economic Journal* have lost positions (Goel and Faria 2005, 538).

While narrowing expertise, economists see their individual capabilities reduced, and are incentivized to work in teams. These are the results of the study conducted by Jones (2009) on a rich patent data set, showing robust evidence for increasing specialization and team size, with similar trends in academic research. In this latter, after Wuchty, Jones and Uzzi’s 2007 study on the whole body of research articles (19.9 million) included in the Institute for Scientific Information Web of Science database, one can clearly observe a rapid increase in co-authorship, and a spectacular rise, within social sciences, of economics. In truth, Jones (2010; see also Jones, Reedy and Weinberg 2014) finds that the average “age at first invention” does not show remarkable differences across fields – chemistry, medicine, physics and economics –, while the incidence of co-authorship in economics has been constantly lower than that in biology, throughout the twentieth century, as shown in the comparison made by Laband and Tollison (2000), drawing upon articles published since the Fifties in three leading journals in each of the two disciplines. But as the authors show, co-authorship was almost nil, in economics, before the Thirties, and there has rather been a sharp increase since the early Fifties. As pointed out by McDowell and Melvin (1983), the growing size of economics have

substantially expanded the gains from specialization: hence the rise in co-authorship. And the gap between economics and biology has greatly reduced since the Eighties (that is, since the launch of the empirical revolution in economics and of some pioneering programmes of “reverse imperialism”): formal intellectual collaboration raised spectacularly in economics while declining in biology. Finally, Laband and Tollison demonstrate that “informal collaboration” – and therefore the “social construction” of knowledge, measured by collegial commentary on published papers in a leading journal in the discipline – is much higher in economics than in biology.

Now, for surprising as it may be, it was Kuhn himself, in the end – implicitly replying to criticisms made by sociologists of science to his early account of scientific revolutions (consider Mulkay’s 1975 “branching” model of scientific development) –, to emphasize the relevance of specialization as driver of scientific progress. In his latest works – quite neglected, at least until Wray’s (2011) essay on *Kuhn’s Evolutionary Social Epistemology* –, Kuhn (2000) insists on the possibility to substitute the creation of new specialties for scientific revolutions as most effective way of coping with the excessive burden of cumulative anomalies, discrediting the dominant paradigm. “What replaces the one big mind-independent world about which scientists were once said to discover the truth is the variety of niches within which the practitioners of the various specialties practice their trade” (2000, 120). Whenever the currently prevailing theories and methods fail to explain a given phenomenon, scientists tend to concentrate on that specific anomaly, a narrower sub-set of their problem of interest, and to develop models and methods having the precise aim of solving the particular puzzle under consideration. In the same vein, Kuhn explained that a theory meant to accommodate an anomaly is often too particular to be relevant to the whole field and transforms itself into a new (sub)field or specialty; therefore, new specialties do not always discard old theories, which can rather survive in narrower domains. This way, a large collection of (new) specialties can (though not always) gravitate around the same set of scientific puzzles.

This picture resembles closely the current state of economics, where a number of specialties coexists making more or less stark departures from the received theory. As Davis (2008) puts it, economics is now characterized by approaches – read specialties – with different “structural” features. There are, in fact, synchronic and diachronic approaches that focus, respectively, on outcomes generated by short-run interactions (i.e. game theory, experimental economics), and on long-run interactions and their

transformational effects on the phenomenon under study (e.g. complexity agent-based models economics, evolutionary economics). Using a different perspective, it is also possible to distinguish between approaches that try to grasp the actual functioning of decision making (experimental, cognitive and neuro-economics), and others that, while still focusing on the individual, are more keen to represent intra-individual interactions through the stylized behaviour of ideal, textbook situations (game theory). Whereas diachronic approaches end up with adopting a system-wide perspective rather than focusing on real-world individual behaviour. Agent-based simulations, for instance, place very limited demands on individual rationality and, usually, consider very simple behavioural rules.

This very classification throws light on the fragmentation of the discipline: each approach there included is also a specialty, or a special domain of interest within the discipline. Specialization seems thus very tightly connected to pluralism. In fact, economics currently shows both patterns of specialization, topic specialization – scientists specialize in different contexts, e.g. microeconomics, macroeconomics, growth theory – and theory specialization - different views (and theories) on material under consideration coexist and develop within each topic, thereby implying a more radical form of pluralism than topic specialization. With the emphasis it lays on the actual practices of modern social science, the sociology of scientific knowledge is in a privileged position to discuss the “specialization view”, as we may call it, of pluralism. A key idea of the sociology of scientific knowledge, in this regard, is that the creation of specialties has to do with the organization of science. For instance, Ben-David and Collins (1966) pictured specialization as an attempt to escape overcrowding within a discipline: scientists create new niches in order to enhance carrier possibilities. De Solla Price (1986) suggested rather that specialization is primarily a reaction to an ever-growing corpus of knowledge that scientists have to master, despite cognitive limitations. It is through specialization that scientists come to select manageable bodies of literature out of the whole corpus of available knowledge (Wray 2011).

According to the late Kuhn, and contrary to the above-recalled purely sociological account of specialization, this latter might well have to do with rewarding careers, but it also serves the purpose of advancing scientific knowledge (defined in terms of accuracy, that is the ability to manipulate the world in predictable ways) through conceptual innovation (Wray 2011, 125). This consideration is simply crucial, given the possibilities it

opens up of explaining the long run dynamics of scientific development. The advent of specialization easily leads to a proliferation of niche-specific lexicons, models, and instruments, with the resulting incommensurability of theories and results. In turn, incommensurability creates barriers in inter-specialty communication, which however, far from constituting a problem, force scholars into (sort of) protected areas where they can focus but on a restricted set of problems, and create particular theories to investigate upon them. Communication barriers also shield scholars from the impinging influence of dominant thoughts, thus opening research to innovative thinking. The resulting conceptual innovations are “both the *cause* [emphasis in original] of barriers between specialties and require barriers between specialties if they are to develop” (Wray 2011, 127).

The proliferation of new specialties in economics can be seen as peculiar branching process generating new perspectives out of a prior unified, prevalent view of economic issues. Within each specialty, the orthodoxy/mainstream approach is bound (compelled by the fragmentation inherent to specialization) to progressively decline in importance and influence, thereby allowing not only for specialised domains but also for more disruptive innovation. The sociological dimension of this apparently purely conceptual dynamics is quite evident. Carrier opportunities strongly depend on the overall coherence of specific research programmes with the received body of thought, when a monistic mainstream avows its ambition of providing a unifying theoretical foundation for work in the field. When newly born specialties are allowed to create their own associations, journals and schools, without being (excessively?) starved of research funds, however, being tuned with the mainstream is not a necessary requirement for professional advancement any longer.

To sum up, the future state of (mainstream) economics will also depend on the strength of the forces that push the discipline toward a (more) pluralistic configuration, also in reason of the self-reinforcing nature of the described mechanism. Increasing specialization calls for further specialization as soon as niche-specific anomalies are discovered, professional opportunities decline and more and more specialized lexicons, tools and models have been developed, laying down, as a whole, the pre-conditions for the birth of new specialties.

The argument that economics is not a datum, but rather the aggregation of efforts of scholars that belong to different times and places and that experience, and deal with an ever-changing landscape of theories, is an obvious antidote to a paradigmatic view of the

history of the discipline. But then, the historical perspectives popularized by the philosophy of science suggest rejection of any rigid law presumably at work in the actual evolution of economics, from the strong “cumulative view” of the history of economic thought in terms of a “progressive rise to ever higher levels of understanding of economic reality” (Roncaglia 2005, 2), to the somehow weaker assumption that economics proceeds by alternating phases of monism and periods of pluralism. Rather, economics can change, and transform itself (see Davis 2006), as adaptive systems do. Mainstream pluralism may be there to stay, in other words, the fragmentation of economics being “the price we must pay for the depth of understanding afforded by specialization” (Wray 2011, 127).

3. On the possible evolution of economics: Imperialisms, pluralism and the (new) mainstream of the discipline

It is now commonplace to refer to the concept of “imperialism” when dealing with the relationship between economics and adjacent social disciplines. Economics, wrote Stigler in 1984 (311), “is an imperial science: it has been aggressive in addressing central problems in a considerable number of neighbouring social disciplines and without any invitations”. The imperialist programme of economics dates back to the late Fifties, when Gary Becker (1957), the leading figure in expanding economics outside the historical borders of the discipline, published *The Economics of Discrimination*. Adopting Mäki’s (2009, 360) terminology (see also Davis 2012), “economics imperialism” is a form of “economics expansionism” – economic theory is persistently applied to new classes of phenomena, thus increasing the degree of explanatory unification it provides – “where the new types of explanandum phenomena are located in territories that are occupied by disciplines other than economics”.

Economics imperialism has been the rule, in the last decades of mainstream economics, “proudly adopted by the imperialist themselves with the purpose of celebrating it” (Mäki 2009, 352). As Lazear notes, in that following Hirshleifer (1985), the “expanding domain of economics” (to use Hirshleifer’s own account) owed to the “rigorous language that allows complicated concepts to be written in relatively simple, abstract terms. The language permits economists to strip away complexity”, which “may add to the richness of description”, but “prevents the analyst from seeing what is essential” (Lazear 2000, 99–100). Lazear’s celebration of economics as “the premier social science” (*ibid.*, 99) configures a case of economics *hubris*, writes Mäki: the Stanford economist

ends up with defending the peculiar form of economics imperialism wherewith “economics presents itself hegemonically as being in possession of superior theories and methods” (Mäki 2009, 374). Although Lazear does not directly “exclude rival theories and approaches from consideration”, as conversely “bad” economics imperialism does, he believes that other social sciences are quite good at identifying questions to which economics – in possession of a superior method and analytical capacities – can provide “well-reasoned answers” (Lazear 2000, 103).

Lazear claims, not without reasons, that the story of economics imperialism has been one of success. Compelling evidences in this regard are provided by the extent to which other disciplines have adopted the economic approach to analyse issues that were traditionally of interest in their fields. Moreover, economists have sometimes replaced outsiders in the analysis of non-economic issues or destroyed consolidated monopolies. “Formalist” anthropology (see Marchionatti 2012) and the economics of unselfishness (see Fontaine 2007, 2012) are crystalline examples of this inclination. Most relevant for our purposes are the reasons why economics imperialism has easily managed to conquer the mainstream of the discipline. “While economics has been expanding horizontally”, Hirshleifer (1985, 64) observed, “a simultaneous invasion has been taking place *vertically* as evolutionary biology has asserted a claim to be the foundation of all the social sciences”. As Fontaine shows in relation to the economics of altruism, Wilson’s (1975) sociobiology has clearly played a role in promoting economics imperialism as against other behavioural disciplines, exactly because of the need, for economics, to address and counteract the expansionist ambitions of natural sciences.

As Davis (2012, 210) maintains, economics – like all social sciences – is not a “monolithic system distinct in nature from other fields, but is rather made up of a collection of heterogeneous elements or even relatively independent research programs, some originating in economics and some developing within economics by drawing on other fields and disciplines”. This makes it difficult to argue that – or when – “economics as a whole is imperialist” (*ibid.*), exactly because other disciplines seem to have had, and to have, a profound impact on economics. Yet the entry itself of economics imperialism into the mainstream of the discipline marks a turning point in this story. Fontaine’s explanation (2012, 205) of the gradual attenuation of economics imperialism in the study of unselfish behaviour – “the need for economists to assert their expertise over social phenomena and seemingly unselfish behavior became less pressing than before. The

interests of economists turned to the more general study of the relationships between economics and biology, and in particular to evolutionary theory” – might be generalized. On one side, imperialist economic programmes have shown – with the passing of time, the enlargement of the neoclassical paradigm, and the subsumption of new perspectives (once it has become de facto impossible to continue apply the “futility thesis” to arguments that cannot fit the neoclassical core of the discipline) decreasing returns (see Hirschleifer 1985, Frey and Benz 2005, Fine and Milonakis 2009, Marchionatti 2012). After all, economics imperialism rested (also) on the promise of remedying unsatisfactory explanations offered by other disciplines as regards issues traditionally included in their own domains (Davis 2006); the diminishing returns of economics imperialism testify that this promise was not maintained. On the other, the entry of economics imperialism into mainstream economics signalled that economics had effectively reacted to the competing imperialism of sociobiology, and actively participated in a competition to define the specific form of the would-be “only” socio-natural science.

At the same time, the concomitant acceptance of the rights but also of the duties pertaining to the imperial status of economics may have been at the origins of the reverse imperialisms” characterizing the “recent” (that is, since the Eighties, the date around which many of today’s research programs in mainstream economics began, see Davis 2006) evolution of the discipline. Here too, as Frey and Benz (2004) maintain, the turning point in the “from imperialism to inspiration” dynamics shaping, in general, the relationship between economics and disciplines contributing to deepen the criticism of the *homo oeconomicus* and more in particular that between economics and psychology, is not so much the birth of new specialties (e.g. behavioral economics) but in its acceptance within the citadel of mainstream economics. By establishing “limited intellectual ‘colonies’” (Mäki 2013, 336) into mainstream economics, “other” disciplines have certified that economics cannot face the diminishing returns of its “mainstream” imperialism, unless its practitioners are prepared to repudiate imperialism itself or to develop more subtle versions of the discipline’s imperial attitude. Growing awareness, after “an initial phase of easy successes” (Hirshleifer’s 1985, 54) in imposing the postulate of rational self-interested behaviour, of the impossibility to apply this latter to some conquered territories will cause economists to realize “how constraining has been their tunnel vision about the nature of man and social interactions” (*ibid.*, 53).

That the end of (traditional) economics imperialism and the advent of

mainstream pluralism (with its somehow “social” view of the relationships between disciplines) can be regarded as a first attempt to solve the long-standing problem economics imperialism has with the “social” (see also Hands 1997) is evidently not a coincidence. The utopia of economics as “universal grammar of social science” (Hirschleifer 1985, 53) clashes against “the obstacles that economics imperialism has to overcome, not only from within its own logic, but in terms of its acceptability to others”, but also, and significantly, “the inability of marginalism to address the ‘social’ in a satisfactory way” (Fine and Milonakis 2009, 11-12). The revolution of rational expectations and micro-foundations in macroeconomics – a “reductionist methodology” (McCombie and Negru 2014, 58; see King 2012, Skouras and Kitromilides 2014) making no attempt “to determine directly how sound [these microfoundations] are” (*ibid.*, 62) – can be read as sort of by-product of the inability to address the “social” by achieving a monism of theories.

Thus, Sent (2003, 93) can claim that one preliminary way of strengthening the case for pluralism is to exhibit the failures of monism(s) in the history of economics, in particular (for our purposes) of the recent project of microfoundations. Such failures have led economists “in the direction of exploring cognitive limitations on the part of the agents who populate their model”. As regards macroeconomics, “incorporating bounded rationality could modify or take the edge off the very sharp no-trade theorems ... for game theorists, absence of a fully rational treatment of knowledge may circumvent no-trade theorems by allowing speculative trade”. Economics has not succeeded in eliminating the tensions it has with the notion of “social”, as demonstrated, for instance, by Fine’s (2000, 14) criticism of the “information-theoretic approach” developed and popularized by Akerlof and Stiglitz, which he describes as a kind of “new” imperialism. One that has the merit of not taking the social as given, but ends up with considering it as “the rational response to informationally imperfect market relations”. Nevertheless, writes Sent, the effort itself “to reduce other fields to microeconomics” (Sent 2006, 84) is responsible for the more pluralistic environment wherein various mainstream research programmes can flourish.

Still, while first-wave pluralism directly attacked mainstream economics for considering alternative theories as simply unscientific, second-wave pluralism “stood atop a wide raft of post-positivist work on economic ontology and epistemology that had flourished in the 1980s” (Garnett, Olsen and Starr 2009, 2), to condemn “the notion of

science as empire building or paradigmatic one-upmanship” (*ibid.*). Second-wave pluralism is thus a rebuttal of “Kuhnian school-of-thought-ism” (*ibid.*). There is therefore an intriguing parallel between heterodox, or non-mainstream (see Lee 2009) second-wave pluralism, and what in this paper has been referred to as “mainstream pluralism”. Pluralism (emerged out of the ashes of an attempted major “monist” revolution) is truly shaping the discipline’s horizon, inducing for instance Holcombe (2008) to turn the heterodox criticism of the mainstream on heterodoxy itself. This latter, writes Holcombe, has not yet, in truth, succeeded to emancipate itself from “Kuhnian school-of-thought-ism”, whereas in mainstream economics, the two competing approaches of “methodological positivism” and “axiomatic general equilibrium analysis” coexist with the “empirical” approach and accept the challenge of this and other competitors.

A remarkable feature, and a fundamental novelty (if compared to these antecedents), of the possible future mainstream, is the contribution that other social sciences are expected to offer in shaping it. The heterodox literature speculating on the future traits of mainstream economics focuses on the collaboration between the formerly imperial science and other disciplines, either as the solution for a radically different, and desirable mainstream (Hodgson 2007), or as the *fait accompli* that is producing a new era in economics (Colander, Holt and Rosser 2010). For sure, other social disciplines play a decisive role in fostering the current “mainstream pluralism”. This latter may be viewed as the result of a peculiar process of “social-scienciation” (as Bögenhold 2010, 1585 has defined it) of economics, prompted by both the failure of economics imperialism and the growing number of “colonies” established by other social sciences into economics. A process that might therefore depend on the state of fragmentation that currently defines economics, having in specialization – as previously hinted at – its main engine. By narrowing their expertise, in fact, economists likely find themselves more dependent on that of specialists in other fields (Wray 2011).

Heiner’s (1983, 576) framework thus provides the bases of a seductive story. As a way of coping with the growing size and diversity of economics, specialization *cum* interdisciplinarity may contribute to address the problem of finding those “easy to decipher (and reliable) criteria on when to switch” (*ibid.*) between competing theories, and help to understand how to use “new concepts or variables into accepted theories” (*ibid.*). Such criteria are those in whose absence, Heiner observed, Kuhnian patterns of “normal” science would persist over time. But it is exactly such persistence – and the

consequent accumulation of anomalies – that, for paradoxical that it may appear, enhances, over time, the likelihood of a scientific revolution, whereas the combination of specialization with interdisciplinarity would introduce gradualism in changes. The reasoning can be easily extended to interdisciplinarity *tout court*: the creation of subfields and niches, usually needed to investigate – and eventually accommodate – persistent anomalies with respect to the received theory, restricts not only the set of problems to be analysed, but also the possibility to analyse them by the use of a single perspective. In other words, the explanatory power of economics is weakened, when the relevance of a however reduced subset of puzzles derives its importance exactly from being a tell-tale sign of the need to revisit the conventional view. Specialization, therefore, somehow (and paradoxically, at first sight) requires *external expertise*, coming from other disciplines. In part, this is the effect of ignorance (as defined by Turnovsky) caused by the growing size and diversity of economics. Economists who are specialized in a specific subfield of their discipline ignore (being also unable to understand) what is going on in other areas of economics, and consequently find in other disciplines the powerful required tool to broaden an otherwise too thin perspective, as well as a source of legitimacy for their concerns. Needless to say, expertise coming from other disciplines not only provides, but is itself a reliable criterion on how to reform received theories by allowing new perspectives to enter and shape the analysis.

To put it differently, specialization permits to cope effectively with the competence-difficulty gap enlarged by the progressive removals of the “futility theses” typical of the era of economics imperialism. Narrower expertise and, consequently, greater reliance on the contributions of other disciplines – a joint byproduct is multidisciplinary team working – help reduce the burden of knowledge of “mature” economic science. The burden of knowledge seems a peculiar characteristic of the age of “reverse imperialisms” and mainstream pluralism. The literature on age and scientific genius (Galenson 2001, Galenson and Weinberg 2000, 2001, Weinberg and Galenson 2005; see Jones, Reedy and Weinberg 2014) distinguishes between the abstract and theoretical work of “conceptual” economists and the concrete and empirical work of “experimental” economists, the former solving precise problems deductively, the latter solving broader questions inductively. There seems to be two distinct life cycles of scholarly creativity: conceptual work (innovations typically consisting in significant departures from the received paradigm) is in fact done mostly at the onset of a career, while great achievements in

experimental work come much later (see Jones, Reedy and Weinberg 2014).

For our purposes, the important conclusion this literature arrives at by analysing conceptual and empirical innovators in economics, is that it is inductive work to suffer more from a problem of “burden of knowledge”, exactly because applied empirical work draws on accumulated knowledge and experience. Weinberg and Galenson (2005) find in fact that “conceptual” Nobel laureates in economics – innovators by definition, so to speak – made their most important and pioneering contributions, in average, at 32.5 years of age, whereas the age of scientific breakthroughs of their “experimental” colleagues is 53.2. The literature employs such data to fortify a fully “early” Kuhnian perspective – whereby a new theoretical framework, that is a revolution later imposing normal science, accommodates anomalies (with respect to the previous paradigm) discovered through empirical work (see Jones, Reedy and Weinberg 2014). The burden of knowledge is said to affect also the direction of scientific progress: raising “training” time, with enhanced educational burden, means diminished “creative” time. And training “truncates” early life-cycle innovative capacity, with the result of reducing (increasing) the chances of conceptual (experimental) achievements. In short, “contributions may become increasingly biased against deep, conceptual knowledge.

Now, there are sufficient reasons for interpreting the recent evolution of the discipline from “economics imperialism” to “reverse imperialism” as one from theoretical and conceptual to concrete and experimental work. The age of economics imperialism was one of “pure economics”, “a program of abstractness that had problems when confronted with competing empirical material since pure economics was related to an economy in a vacuum” (Bögenhold 2010, 1569). With Bögenhold (1570), one may observe that “theoretical economics became increasingly an abstract science during the 20th century, trying to bring home the complexity of economic life into formulas”. Economics “became applied” after the 1970s, as Backhouse and Cherrier (2014) argue, before that date, there was widespread consensus on the primacy of economic theory over empirical work. “It was generally accepted”, they write, “that economics was based on a common core of economic theory centered on mathematical modeling of maximizing agents”. This very idea was “strengthened by the extension of models based on maximizing behavior to fields that had long resisted it”, *in primis* those traditionally considered as belonging to other disciplines – “the phenomenon often described as economics imperialism” (Backhouse and Cherrier 2014, 10). While heterodox economists were critical of the methodological

monism implicit in this view, mainstream scholars began, in the early Seventies, to insist on the unfortunate state of economic theoretical models having little relevance for the actual world. Backhouse and Cherrier's analysis of John Bates Clark medals winners suggests that the "empirical revolution" of the 1990s, representing "a significant departure from the now disparaged over-theoretical orientations of the 1970s and 1980s" (Fourcade, Ollion, Algan 2015, 92), was favoured by the shift from theoretical towards applied economics. The resulting continuous creation and institutionalization of new fields lies at the origins of "a process of fragmentation of the discipline" (Backhouse and Cherrier 2014, 13) dating back, in truth, to the Seventies.

To restate the argument: a formerly conceptual and essentially theoretical discipline, economics has progressively expanded its boundaries and scope of inquiry, and the resulting growth in size of the discipline understandably threw light on the decreasing returns of economics imperialism. It became a relatively easy task, for mainstream critics and reformers, to associate the theoretical limitations of the neoclassical core with the difficulties of satisfying the mounting demand for diversity produced by the growth of economics itself. The shift towards applied economics and the empirical revolution that followed exacerbated the tendency towards fragmentation. Engine of change, fuelled by the growth in size and diversity of economics, specialization also enters this picture as powerful defensive strategy. On one side, specialization limits the necessarily increasing gap between scholars' competencies and the difficulties they face in keeping up with the requirements and tasks of an imperial science. On the other, it helps to reduce the burden of accumulated knowledge, by narrowing one's expertise while contributing, this notwithstanding, to a more and more demanding "applied" science.

Yet, all this might come to an end, with the arrival of a new orthodoxy, and a less pluralistic mainstream. For reasons outlined above, Davis (2008) can argue that "the turn in recent economics [will give way to the] return of orthodoxy". Designed to analyse the dynamic laws governing the rise of dominant approaches from periods of pluralism, Davis's (2006, 2008) core-periphery model classifies "heterodox" research programmes of recent economics according to their "origin stories" (how they came to acquire the status of being heterodox) and "primary orientations", or final aim – either to revise the core, or to contribute to a more radical transformation of the discipline, "by challenging its boundaries" (Davis 2008, 356). Contrary to "traditional heterodoxy", the "new mainstream heterodoxy", positing "fundamental assumptions at odds with neoclassical orthodoxy"

(ibid., 354), has its origins outside economics. Its “heterodoxy” has clearly to do with these external origins, and aims at “redirecting”, or reforming, the core of the discipline. Taking into account sociological factors, Davis believes it more likely that the new orthodoxy will result from a combination of the research programmes of today’s “new mainstream heterodoxy”, and emerge from a “conservative” pathway than a “transformational” one. That is, contra the atomism of the neoclassical core, it will emphasize individuals’ social embeddedness and the mutual influence exercised by individuals and social structures, but it will only mildly adopt an evolutionary rather than mechanical perspective of processes, thereby reducing the innovative potential of the reform.

Curiously enough, Davis (2006, 18) was ready to acknowledge that the (now) strong impact of other disciplines on economics could make a difference in the cyclical alternation of monism and pluralism, of orthodoxy and heterodoxy. The birth of new research programmes having their origins outside economics is itself a challenge to the received view on economics imperialism. “We are neither easily able to say that economics on the whole is imperialist when we add in other fields’ reverse imperialism toward economics, nor are we able to say that agreed-upon episodes of imperialism are evidence of ‘economics imperialism’ when the reference to that expression comprehends evidently non-imperialist research programs. What this then implies is that we need to explain interdisciplinarity and imperialism in particular at a lower level of aggregation than entire fields or sciences” (Davis 2012, 212). This is what Davis (2013) himself has attempt at doing by analysing the impact of psychology on a specific sub-field of economics, that is behavioral development economics. The result of his investigations is quite revealing: behavioral development economics makes visible a sort of “economics-plus psychology imperialism”, or a “social science imperialism under the leadership of economics” (Davis 2013, 12). On one side, economics selectively appropriates contents from psychology (with little if any concern for debates in the discipline, and excluding contents that cannot be easily accommodated), then domesticates it “in ways that somehow maintain economics’ separate identity as science” (Davis 2013, 1). In other cases, economics can be victim, so to speak, of “imperial” (as just defined) attitudes from other sciences that can frame its material as desired. On the other, economics manages to reinforce (hence the idea of its “leadership”) the priority accorded to “economic explanations of the world” (ibid., 13).

Albeit without developing the argument, Davis considers Gintis’s (2007) proposal of “A framework for the unification of the behavioral sciences” as a serious candidate for

becoming the post-neoclassical mainstream arising out of the “heterodoxy” of the current mainstream pluralism. In Gintis’s view, recent laboratory (Fehr and Gächter 2000) and field (Henrich *et al.* 2005) behavioral research (in general, see Camerer 2001, and Gintis, Bowles, Boyd and Fehr 2005) on the plausibility of the *homo oeconomicus* representation have demonstrated the need to revise the “canonical model” of individual choice behaviour. Individuals systematically violate the assumption of self-interested material payoff-maximizing individuals: cooperation and (strong) reciprocity do matter, and preferences cannot be considered as exogenous. The concept itself of preference consistency is shown to have biological-evolutionary roots. Gintis’s model for unifying behavioral sciences rests on five “conceptual units”: first, the evolutionary-biological perspective of gene-culture coevolution – “the application of sociobiology” (Gintis 2009, 224). Second, a “sociopsychological theory of norms”, aiming at solving “the contradictions between the sociological and economic models of social cooperation, retaining the analytical clarity of game theory and the rational actor model while incorporating the collective, normative, and cultural characteristics stressed in psychosocial models of norm compliance” (*ibid.*, 233). Third, game theory, as “logical extension of evolutionary theory” (*ibid.*, 238), raised however to the status of “universal lexicon of life” (Gintis 2007, 8), including classical, behavioral, epistemic, and above all evolutionary game theory (“allowing biological and cultural evolution to be mathematically modelled”, Gintis 2009, 222). Fourth, “the most important analytical construct in the behavioral sciences operating at the level of the individual” (*ibid.*), that is a rational actor model, called “beliefs, preferences and constraints” model, based on choice consistency, to represent behaviour. Fifth, complexity theory, that Gintis included in the list, oddly enough, only after criticisms to his target 2007 article.

Most relevant, for our purposes, is that this possible new economic mainstream is presented as a potential revolution in behavioral sciences. Gintis’s proposal is to construct a new theoretical framework integrating natural and social sciences, with the explicit aim of “rendering coherent the areas of overlap of the various behavioral disciplines” (1). The rationale underlying the proposal is that although each behavioral discipline “contributes strongly to understanding human behavior”, when “taken separately and at face value”, behavioral disciplines “offer partial, conflicting, and incompatible models. From a scientific point of view, it is scandalous that this situation was tolerated throughout most of the twentieth century” (15). In this view, incompatibility is said to derive from

disciplinary boundaries that “have been determined historically, rather than conforming to some consistent scientific logic” (16), with the result that behavioral disciplines would live in “semifeudal” (Gintis 2009, 247) conditions. On the contrary, there would be now “a strong current of unification, based on both mathematical models and common methodological principles for gathering empirical data on human behavior and human nature” (*ibid.*, 15), allowing the possibility of a general, transdisciplinary research project.

Transdisciplinary – to employ Alvargonzález’s (2011) definition –, since the project use laboratory and field experiments as providers of material continuity between disciplines, and evolutionary thinking and game theory as common principles for the unified framework, sciences. Remarkably, Gintis assigns to biology in particular, and evidently to the “new” economics, the task of leading the reform plan: the proposal *de facto* revives (see Getty 2007) Edward O. Wilson’s ambition to include the “last branches of biology”, that is (recalcitrant) social disciplines, “in the modern Synthesis” of sociobiology. At the same time, Gintis evidently agrees with Hirschleifer (1985, 53) on the impossibility “to carve off a distinct territory for economics, bordering upon but separated from other social disciplines. Economics interpenetrates them all, and is reciprocally penetrated by them”. And although Gintis’s proposal includes an implicit condemnation of economics imperialism, the lexical affinity might be revealing (note also that the three epigraphs of Gintis 2009 chapter on the reunification of behavioral sciences are quotes from Wilson, Becker, and Hirschleifer). “There is only one science”, wrote Hirschleifer, with economics as its “universal grammar” (*ibid.*). In short, contrary to what Gintis states in response to the criticisms received by his target article (see in particular Clarke’s 2007 discussion of the “unity of science” character of the proposal; on the general issue, see the symposium on *Perspectives on Science*, 3(7), 1999), the proposal might truly aim at establishing the conditions for a “unified alternative” for all the behavioral disciplines, not simply a “unifying bridge” (Gintis 2007, 46) linking them. But the most relevant aspect of the proposal, for our purposes, is its attempt to “repair” (1) a state of “fragmentation” (1), a “scandalous” one (15).

It is almost impossible, while reading Gintis’s article, to overestimate the symbolic and practical significance of the similarity between the “fragmentation” of behavioral sciences and that of today’s mainstream economics. As implicit in Davis’s remark, while pointing the finger at the incompatibility of behavioral disciplines’ different models, Gintis is trying to convey a crucial message about the revolutionary changes mainstream

economics is undergoing under the influence of economists' interdisciplinary work with specialists from biology, anthropology and sociology. In other words, Gintis is first and foremost trying to reconstruct an economic mainstream on different (post-neoclassical) bases, after the "disintegration" – in the literary sense – of the discipline's core into a number of increasingly autonomous streams. After all, Gintis's framework configures itself as a collage of many, if not all, today's mainstream research programmes: from behavioral economics to evolutionary game theory, from the new institutional economics to complexity economics, and even rehabilitates, opportunely revised, the rational actor model of more conventional economics. Reformulating Vromen's (2009) argument about Lazear's reasoning (which would prove, only, that economics imperialism has been successful in economics), one could say that the only attainable result Gintis can hope for is to be successful... in economics, that is, to persuade his fellow economist of the emergence of a new mainstream in the discipline. An operation implicit in Gintis's proposal would be the attempt to bring economists working in one of the various research programmes of today's pluralistic mainstream to develop awareness, whether grounded or not, of being part of a coherent whole, thereby remedying the fragmentation brought about by specialization and, at the same time, scholars' inability to perceive unity *e pluribus*.

It is as if, after various reverse imperialisms have established "social sciences colonies", a more and more fragmented economics, growing in size and diversity, could not inaugurate a new era of "normal dominance" arising out of a supposedly temporary state of mainstream pluralism unless by devising and leading, jointly with its former competitor (biology), a process of reunification of behavioral sciences (in which these latter are all called to participate). It is by projecting the problem of specialization, heterogeneity and even incompatibility at a higher level, that a post-neoclassical mainstream economics can aspire to acquire the permanent status of a "dominant" approach, as an emergent property of the set of different research programs of today's mainstream pluralism. Gintis gathers in a unitary framework behavioral sciences – either the origins of reverse imperialisms (psychology, sociology) or currently involved in the development of new mainstream research programmes (biology, sociology, anthropology) – still "waiting to be included in the Modern Synthesis" of sociobiology. And he makes them cooperate with the scientific branches (mathematics, physics) that have had the highest impact on recent and less recent economics. Remarkably, at an epoch of "social-

scienciation” of economics, as opposed to the era of economics imperialism, social sciences are taken as sources of legitimacy for a new economic mainstream able – contrary to the current, pluralistic one – to perceive itself as the core research programme of the discipline.

Curiously enough, Clarke (2007, 22) notes that Gintis’s “unified behavioral science could be expected to have many of the characteristics of a Kuhnian paradigm”. In other words, it would discourage researchers from conducting work that explicitly aims at investigating the plausibility of the unified framework’s assumptions. Building upon these remarks, one might conclude that a “unity of (behavioral) science” perspective seems, in other words, necessary first and foremost to economics. Victim, so to speak, of a process of “social-scienciation”, specialization having evidently played a major role in promoting the shift, economics may be losing, while accepting the reverse imperialisms, however defined, of other disciplines, exactly those features that most contributed, in the past, to its characterization as a *sui generis* social science. One that has been keeping a strong demarcation between an orthodoxy and various heterodox programmes exactly in order to defend its scientificity and special status as (apparently) value-free, authoritative, and ready-to-use science of government, or that internal (apparent) unity upon which economics justified its imperial attitude towards disciplines (e.g. political science) that lacked it (see Mäki 2013). The remarkable aspect of this dynamics is that specialization, among other factors, contributes to propel it, and therefore that the dilution of historically rigid boundaries between mainstream and non-mainstream economics may be the result, at least in part, of an unintended evolution.

In any case, it seems reasonable to interpret Gintis’s work as a reaction to a sort of “disintegration” of the mainstream of economics, to the loosening of its compactness, with the concomitant attempt at reorganizing the mainstream itself by exploiting the contribution that other social sciences can offer in this regard. Remarkably, the leading scholars of two of the most important research programmes of today’s mainstream pluralism – complexity economics and institutional economics, respectively, clearly originating from outside the neoclassical core once dominating the mainstream –, Colander and Hodgson, similarly invoke a “transdisciplinary social science” (Colander 2014), or “the reorganization [of social sciences] on different lines” (Hodgson 2007, 20) as main possible driver of the desired revolution in economics. It seems safe to argue that the expected and preferred outcome of this change is not the same for Colander, Hodgson and

Gintis. But in at least one important sense, their perspectives overlap. Interdisciplinarity, or even transdisciplinarity (Hodgson explicitly agrees with Gintis on the ambitions of the latter's proposal, while Colander finds today's "social science pluralism" a "dysfunctional pluralism), is considered as necessary condition for the advent of a new "order". Be it one dominated by the former heterodoxy of institutional/evolutionary (Hodgson) or complexity economics (Colander), or one wherein the new mainstream arises out, as in Davis's (2008) "transformational" pathway to a new orthodoxy – making significant departure from neoclassicism towards evolutionary theory –, from a powerful synthesis of today's heterogeneous mainstream research programmes (Gintis).

4. Conclusions

The "C-D gap" and the "burden of knowledge" appear as two powerful reason motivating increasing specialization in economics, and possibly explaining not only the emergence of today's mainstream pluralism, but also its eventual persistence over time. The "cumulative evolutionary changes" affecting the discipline since the birth of new non-neoclassical research programmes in the Eighties might therefore lead to a "revolutionary change", to continue with using Colander, Holt and Rosser's words, that consists exactly in an unprecedented tenacity of pluralism. For sure, radical specialization would allow "the mainstream to expand, and to evolve to include a wider range of approaches and understandings" (*ibid.*). One might easily argue that a generalized and sufficiently prolonged strategy of narrowing expertise should help contain the C-D gap within reasonable bounds, while considerably reducing the "burden of knowledge" for future scholars. In truth, things are not so simple. The desirable fall in the burden of knowledge requires something of the nature of a Kuhnian "revolutionary" paradigm shift, which can "simplify the knowledge space" (Jones 2009, 310). But Heiner's framework tends to suggest that "specialization-cum-interdisciplinarity" can be used as reliable, however second-best and superficial, device available to scholars to be able to use several competing theories and decide on when to switch between them, as well as to import concepts and theories from other disciplines. In other words, compelling economists to face the challenge of interdisciplinarity (2011), specialization attenuates the need for Kuhnian shifts. Specialization is thus, as Kuhn came to believe, the true alternative to Kuhnian shifts.

Moreover, Jones (2009) provides general evidence of increasing educational attainment in times of narrowing expertise: meaning that the "distance to the frontier" is

increasing for future generations of innovators in academic disciplines. In any case, increasing specialization comes at significant costs for the discipline. Economics may become a highly heterogeneous collection of divergent approaches, with significant blurring of boundaries between orthodox and heterodoxy, and little chance of survival, in times of “reverse imperialisms”, as autonomous discipline among social sciences. This result would signal the conclusion of a process of “social scienciation”, exemplified, as first evidence, in Gintis’s proposal for the reunification of behavioral disciplines. Placing the rational actor model on a par with some heterodox research programmes of recent economics as pillars of the new unifying framework, Gintis appears to accelerate the end of economics as special (social) science, its specificity owing, in the past, to the separation between an orthodox core and heterodox alternative approaches. In truth, the proposal itself can be regarded as symptomatic not only of the consolidated pluralism of social sciences in general, but also, specifically, of the fragmented state of economics.

The use we have made of a “late Kuhnian” framework for analysing the current pluralism of mainstream economics invites reflections on the impact of the possible persistence of such pluralistic environment on the capacity to innovate in economics. As hinted at above, the literature on the “burden of knowledge” is quite explicit in pointing out that both strategies to reduce the impact of an increased stock of knowledge, that is learning more and narrowing expertise, produce negative effects on innovation. Jones’s (2009; see also Jones 2005) study on technological progress suggests that to learn more, future scholars will necessarily have less time available in the life cycle for innovation. On the other hand, specialization “can reduce individual capabilities and force innovators to work in teams”. This might pose serious problems when, as in the case of economics, (raising) training can “truncate” early life-cycle innovative capacity. Thus, “the nature of achievements would shift from conceptual to experimental reasoning” (Jones, Reedy and Weinberg 2014, 30), which would significantly and negatively affect the chances of conceptual novelty in economics. By the way, this tendency would be but strengthened by the applied/empirical revolution occurred in economics since the Seventies.

Only apparently a paradox, the “late” Kuhn tends on the contrary to argue in favour of specialization. The late Kuhn invites in fact to reconsider “the dynamics of scientific change and the nature of scientific knowledge ... as a process leading to increasing specialization”, while stressing the need to consider the “epistemic dimension of scientific specialization” neglected by sociological studies (Wray 2011, 97, 122). The

current mainstream pluralism is interpreted as a temporary phenomenon, one that will give rise to a new orthodoxy; and this latter will likely take the shape of a unifying theory such as the one illustrated by Gintis. It is to be noted that the hope for a unified science is the main reason why philosophers have traditionally neglected the relevance of specialization, generally considered as an impediment to reunification (*ibid.*, 117).

Once, however, this general pessimistic view of specialization is left aside, the potential benefits can appear, as dealt with in the preceding sections. Remarkably, the proliferation of niches, each with its own lexicon, models, and tools, produces a general incommensurability of topics, theories and results between neighbouring disciplines (see also Marqués and Weisman 2010). But in Kuhn's vision, this form of incommensurability may give positive results. In fact, it limits the spread of errors, and, notwithstanding interdisciplinary efforts, allows – somehow paradoxically – “selection biases” of the kind of those discussed in this paper (see Davis 2013). Such biases facilitate the development of “concepts that are suited to modeling the phenomena [research communities] study without too much interference from scientists in neighboring disciplines” (Wray 2011, 135). Conceptual innovations may therefore follow from inter-specialty communication breakdowns, as well as from the limited influence that dominant approach can exert on developing niches.

This means that the late Kuhn would not support Gintis's (and others') argument about the “scandalous” pluralism of social sciences. Rather, it is the variety and proliferation of niches, the resulting incommensurability, and the (necessarily) “local” knowledge produced, that allows scientific progress. A “late Kuhnian” framework would therefore postulate the existence of a specialization trade-off: while growing in size and diversity, economics fragments under the “burden of knowledge” (economists narrow their expertise), with resulting damaged capacity to innovate. Still, at an epoch of reverse imperialisms and (somehow necessary) diffusion of interdisciplinary research, specialization may in truth enhance that same capacity to innovate, by creating niches wherein, paradoxically enough, specialists are free and able to reach the frontier of knowledge.

To investigate the issues raised in these conclusions is evidently beyond the scope of the paper, which has simply tried to suggest a possible interpretation of current mainstream pluralism centred on specialization. A very vivid picture of the transition from the (presumed) monolith of the neoclassical paradigm to the various “minarets” of

today's mainstream pluralism comes from Hahn's (1991) prophecy of the decline of pure theory, once the main provider of consistency and coherence to the discipline. Hahn suggested that economics would have had less and less to do with deducing implications from axiom. The "uncertain embrace" (50) of history, sociology and biology will shape a new phase in the history of the discipline, "our successors" being "far less concerned with the general ... than we have been", he observed. "They will have to bring to the particular problems they will study particular histories and methods capable of dealing with the complexity of the particular, such as computer simulation" (*ibid.*).

Still, even Hahn considered this "specialization cum interdisciplinarity" phase as temporary: successors will "patiently wait for a new dawn such as shone on those of us who came to economic theory after the last war" (*ibid.*). Conversely, our contribution *raises* doubts about the alleged imminent dismissal of mainstream pluralism and its replacement with a new, unitary and unifying orthodoxy. The main lesson we can draw from post-Popper narratives of scientific knowledge is the historical perspective offered and defended by Kuhn and Lakatos. A possible implication of the use of this framework for the analysis of the origins and fate of current mainstream pluralism is that we could have entered a non-transitory phase of *mainstreaming*. One, that is, in which mainstream economics assumes the uncertain, fragmented and changing shape specialized scholars will give it in the course of time by exploring and *de facto* creating their niches in collaboration with specialists from other disciplines. This may result in an inherently pluralistic mainstream, continuously renewed, structurally resisting the temptation of returning to the monism of an orthodoxy. And although it will not generate the pluralist conception of economics heterodox economists are campaigning for, a pluralistic mainstream environment might nevertheless be functional to the project; exactly as "second-wave pluralism", today's anti-paradigmatic pluralism has evidently benefited, and perhaps taken a *raison d'être*, from the fragmented state of the discipline.

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