Relational Methodologies and Epistemology in Economics and Management Sciences

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Chapter 1
Network Analysis for Economics and Management Studies

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ABSTRACT

Sociology and other social sciences have employed network analysis earlier than management and organization sciences, and much earlier than economics, which has been the last one to systematically adopt it. Nevertheless, the development of network economics during last 15 years has been massive, alongside three main research streams: strategic formation network modeling, (mostly descriptive) analysis of real economic networks, and optimization methods of economic networks. The main reason why this enthusiastic and rapidly diffused interest of economists came so late is that the most essential network properties, like externalities, endogenous change processes, and nonlinear propagation processes, definitely prevent the possibility to build a general – and indeed even partial – competitive equilibrium theory. For this paradigm has dominated economics in the last century, this incompatibility operated as a hard brake, and presented network analysis as an inappropriate epistemology. Further, being intrinsically (and often, until recent times, also radically) structuralist, social network analysis was also antithetic to radical methodological individualism, which was – and still is – economics dominant methodology. Though culturally and scientifically influenced by economists in some fields, like finance, banking and industry studies, scholars in management and organization sciences were free from "neoclassical economics chains", and therefore more ready and open to adopt the methodology and epistemology of social network analysis. The main and early field through which its methods were channeled was the sociology of organizations, and in particular group structure and communication, because this is a research area largely overlapped between sociology and management studies. Currently, network analysis is becoming more and more diffused within management and organization sciences. Mostly descriptive until 15 years ago, all the fields of social network analysis have a great opportunity of enriching and developing its methods of investigation through statistical network modeling, which offers the possibility to develop, respectively, network formation and network dynamics models. They are a good compromise between the much more powerful agent-based simulation models and the usually descriptive (or poorly analytical) methods.

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INTRODUCTION: DIFFERENT DISCIPLINARY PERSPECTIVES

Network analysis research has become so vast that it is almost impossible to give a synthesis of its developments and scope of applications. This impossibility holds also if restricting the review to Social Network Analysis (SNA), under which we can include network-based approaches in anthropology, psychology, sociology (SOC), and economic sciences in a broad sense, which can be further distinguished into economics (ECON), and management and organization sciences (MOS). The first three have been matter of many studies, even (to a less extent) from a historical perspective, while the latter two, which are the specific subject of this volume, have been not yet well reviewed, especially from a historical perspective. Moreover, the few existing contributions are very recent, in particular those concerning network economics, and while some contrasts SOC and ECON (Jackson, 2010a, 2010b) - with the partial exception of Knoke (2012), who restricts the view of network economics only to few themes - no one contrasts ECON and MOS. As it often happens, even though two scientific areas are largely overlapped[1], they remain encapsulated into the respective disciplinary boundaries, so hindering a fruitful cross-fertilization. Most researchers in one area do not know the works in the other area. Therefore, the main aim of this chapter is to fill in this gap by directly contrasting ECON and MOS perspectives on network analysis.

There are various accounts of the history of network analysis in general, and SNA in particular. Among the former there is Barabasi's (2002) fortunate and popular book, and the classic Biggs *et al.* (1976). Among the latter there are many contributions (Berkowitz, 1982; Leinhardt, 1977; Marsden & Lin, 1982; Scott, 1992), some of which quite recent (Freeman, 2004; Prell, 2011). Leaving aside books dedicated to specific methodological or real topics, there is a certain number of handbooks (Borgatti *et al.*, 2013; Hanneman & Riddle, 2005; Knoke & Kuklinski, 1982; Scott, 1992; Wasserman & Faust, 1994; and the more recent Borgatti *et al.*, 2013; Knoke & Yang, 2008; Robins, 2015; Scott & Carrington, 2011). And of course a lot of readings (Carrington *et al.*, 2005; Wasserman & Galaskiewicz, 1994; Wellman & Berkowitz, 1988; among the many). There are also many books and papers dealing with both fields of social and natural sciences (Barabasi, 2002; Biggiero, 2011; Bornholdt & Schuster, 2003; Caldarelli, 2007; Dehmer & Emmert-Streib, 2009; Lewis, 2009; Newman, 2010; Newman *et al.*, 2006), but with few exceptions they are written by mathematicians or physicists.

Although the mathematical base of network analysis is graph theory, and so even developments outside mathematics and physics (MAPH) must be based on this ground, substantially different stories took place in the development of SNA and graph theory[2] during the 80 years between the thirties of last century and these last years. An appropriate treatment of this subject would deserve an accurate and extensive research far beyond this section and also beyond a full dedicated paper, but in extreme synthesis we can say that the story told by mathematicians and physicists, like Barabasi (2002), Watts (2003) and Newman 2010), describe a network analysis which, besides the pioneering work by Euler and some early studies in the thirties, was systematized by Erdős and Rényi during the fifties. Their work – and that of their followers - focused mostly on random or regular graphs, until in the nineties Barabasi (2002), Barabasi & Albert (1999) on one side, and Watts (2003, 2004a, 2004b) and colleagues (Watts & Strogatz, 1998; Newman, 2001, 2005) on the other side discover the peculiarity and wide diffusion in social and natural networks of, respectively, the scale-free and the small-world topology. Indeed, scale-free topologies had been discovered already in the fifties by the economist Nobel Prize Simon (1955; Simon & Bonini, 1958), who indeed exploited previous studies from economics (Gibrat, 1931; Pareto, 1896) and linguistics (Zipf, 1932)[3]. And the small-world structure was baptized as such by the sociologist Milgram (1967; Travers & Milgram, 1969) in his studies in the sixties, which were further somehow anticipated by others few years before[4]. These "discoveries", and more generally the crucial relevance of "irregular" topologies to understand real networks, had a remarkable impact on network analysis applied to natural sciences. Moreover, MAPH authors underline the remarkable shift, occurring during the nineties, of the focus from a pure static analysis of mathematical and statistical properties of random networks to the processes with which irregular topologies *can be* generated: the famous preferential attachment mechanism for the scale-free structure and the rewiring mechanism for the small-world structure are such examples.

The story told by some SNA scholars (Freeman, 2004; Wasserman & Faust, 1994; Prell, 2011; Scott, 1992) is quite different: besides some precursors at the turn of the 20th century, a substantial impulse came already in the thirties with Moreno's work (1937), and right after in the forties and fifties mostly in US and UK with various research groups. Some of them were more familiar with (and oriented to use) graph theory and some other less, but all were sharing the common convincement that the explanations for – or at least the understanding of – social phenomena had to be discovered primarily – albeit not exclusively - into social structures. As in most structuralist perspectives, the role of the subject were not neglected, especially in the research area working on ego-networks, but it was argued that an actor's behaviour is constrained and also induced and influenced by his relational pattern and the position covered into the whole structure. This shared structuralist view is apparent in some SNA book (Berkowitz, 1982; Marsden & Lin, 1982) and papers titles, and exemplary underlined by Wellman & Berkowitz's (1988b) introduction to their edited influential book on social structures. They argue that structural analysis "is less and more than network analysis": less because network analysis provide powerful tools and theories, and more because structural analysis includes also rather different fields, like general systems theory, cybernetics, etc., which at that time could appear far from SNA[5], and includes also methods far from those typical of network analysis. Wellman (1988) remarks that network analysis is not only a method(ology) or a metaphor, but rather "a comprehensive paradigmatic way of taking social structure seriously by studying directly how patterns of ties allocate resources in a social system". A position that seems due more to the need to claim attention into the scientific landscape than to a real substance.

Though some mathematician and physician, like Harary and White, has been fully enrolled into SNA, others were just collaborating to one or more groups. They were supporting a theoretical, methodological and epistemological framework rather different from that that was being developed in graph theory. Two differences appear particularly interesting. The first one is that the major interest was less on random graphs, and more on networks that could realistically approximate social structures. The second one is the acknowledgment of the relevance also of nodes' attributes, and hence the interplay between the strength of positions with the peculiar characteristics of the node covering those positions. Things are moving towards a major interaction between SOC and MAPH even though maintaining some methodological and epistemological differences, which are well mirrored in its respective approaches to modelling network analysis. Recently, SNA needs to go more deeply into understanding the causes and antecedents of network formation a modelling methodology[6].

As addressed by Freeman (2004), Wasserman & Faust (1994), Prell (2011) and Scott (1992), methodological knowledge acquisition of SOC from MAPH started since the fifties with many works made by Harary, systematized into two important books (Harary & Norman, 1953, and Harary *et al.*, 1965). Recent good examples of such a MAPH direct involvement as researchers and not just as tool-givers are into the field of network analysis applied to international trade (see below next section). In particular, scale-free and small-world properties have been re-discovered and then intensively investigated by physicists and mathematicians, and then empirically applied with growing diffusion in SNA. More generally, these contributions come with a relatively recent renewed interest of physicists, statisticians, and mathematicians for a direct involvement into the theoretical development of social sciences. This phenomenon is all but new, if we think that, especially in economic theory, mathematicians have been directly (and often, primarily) involved in building theoretical pillars of social sciences. However, now there is an effort so extensive and numerous that two new branches have been nurtured: sociophysics and econophysics (Aoyama *et al.*, 2011; Chakrabarti *et al.*, 2006; Chatterjee & Chakrabarti, 2008; Cockshott *et al.*, 2009). Nevertheless, it is not yet clear whether this is an episodic intensification of an endemic phenomenon or a more systematic change in the dynamics of contemporary science.

For SNA history and concepts have been widely investigated, the main aim of this chapter is giving a picture of the way in which network analysis has been applied in ECON and MOS. Then I will proceed with discussing the "modelling revolution", which is in full development during last 15 years. The focus will be mostly on ERGM (Exponential Random Graph Models) and SAOM (Stochastic Actor-Oriented Models). Next, it will be underlined that statistical models of network analysis can combine and reconcile methodological individualism and structuralism. Finally, it is argued that the embrace of network analysis will force economics – and especially the general equilibrium research program - to depart from its neoclassical foundations.

NETWORK ANALYSIS IN ECONOMICS

This section has two objectives: providing suggestions to explain the very late engagement of economics into network analysis, and depicting the main developments that such an involvement has taken from the beginning of this century. Both purposes are necessary because, with few exceptions (Knoke, 2012), SNA historical profiles have substantially neglected network economics, and because during last 15 years network economics literature exploded and applied to a number of different fields of applications (Jackson, 2014a, 2014b). Indeed, it is quite singular that the apparently network-like structure of economic phenomena has been neglected so long and rather systematically by mainstream economics. The explanation offered by Jackson (2014a), who adduces only "practical" reasons, like the lack of relational data or computing power, is not fully convincing, because other disciplines, like sociology, psychology, etc. had to face with the same problem, but they found that obstacle not so decisive. Moreover, other branches of economic theory, since Pigou's neoclassical economics (Screpanti & Zamagni, 2005), adopted network analysis to some extent, especially into the field of transportation problems. Was not there a lack of relational data? The main reason could have a rather different nature: a theoretical nature, lying on two main forms of incompatibility with mainstream economics. The first one occurs between the structuralist epistemology beneath SNA, which implies also a methodological structuralism, incompatible with the methodological individualism of standard economics[7]. The second one is standard economists' suspect – or perhaps reasonable doubt - that the acknowledgment of the network nature of economic phenomena makes its treatment incompatible with the obtainment of a general competitive equilibrium.

The second objective of this section is to outline the landscape of current network economics. There is only one contribution (Goyal, 2007) that covers (almost all) the main sectors in which current network economics is developing, but it is not very recent, and network economics is extremely rapidly growing. Knoke (2012) discusses SNA and ECON, but this latter field is rather under-treated, being mostly shifted on the MOS side, which is quite far from what is currently meant as network economics. In fact, his book titles "Economic networks", which concerns only one of the three main branches of network economics[8]. Conversely, Jackson (2008, 2010) presents SOC and network economics rather extensively, but he substantially neglects MOS. Therefore, though there is here space for only a brief discussion, this is a good occasion to fill in this gap and give a short outline of network economics and MOS network analysis, so to favor their reciprocal acknowledgment and comparison. To these aims, this section is divided into a first part dedicated to the first objective – explaining network economics delay - and a second part dedicated to outline the main sectors of network economics.

Explaining network economics delay. Excepted for applications mostly focused on transportation networks among which the pioneering contribution by Pigou in 1920 and some others published right after König's book on graph theory in 1936[9], the attention dedicated by economists to network analysis has been episodic until the end of nineties, and almost always limited to spatial and transportation problems. To see a substantial focus enlargement it is necessary to wait until last twelve years. Therefore, it could be argued that within social sciences economics has been the last one to adopt network analysis[10], and this is apparently paradoxical if one thinks that, intuitively, individuals and firms and banks and even nations are strongly interconnected. And it's even more surprising, if one considers that early economists' approaches (Screpanti & Zamagni, 2005; Landreth & Colander, 2001), like the *Tableau economique* of the French School in the second half of eighteen century or the Marx's schemes of simple and extended production are precisely matrices, and thus, economic networks. Further, recent Arrow's (2009) lapidary judgment that "network theory … may not be all that useful or needed" weighs as a rock on network economics. And this is even more relevant not only because of his prestige, but also because he has been among the founders of the Santa Fè Institute (Arrow *et al.*, 1988; Arthur, 2010; Blume & Durlauf, 2006; Waldrop, 1992), where complexity economics has been nurtured and network theory largely employed.

Nevertheless, during the last 15 years network economics is rapidly growing and extending to many areas of interest: two dedicated journals – the *Review of Network Economics*, and the *International Journal of Management and Network Economics* - and many special issues of important journals, like the *International Journal of Industrial Organization*, the *Review of Economic Design*, etc., mark the great attention paid to this research area. Not to say innumerable contributions, many of which appeared on top journals, like *American Economic Review*, *Journal of Economic Theory*, *Econometrica*, etc.

Hence, it remains to explain why such an enthusiasm came so late respect to other SNA areas. I speculate that the incompatibilities with neoclassical economics in general, and standard economics in particular, rose by structuralist methodology and network-related mathematical implications are the main reasons. An acknowledgment of complex networks properties prevent the acceptance of the theoretical assumptions required by general economic equilibrium, either in neo-Walrasias or in non-Walrasian versions (Bridel, 2012; Hahn & Petri, 2002; Ingrao & Israel, 1990; Kirman, 1976; Loasby, 1991; Screpanti & Zamagni, 2005; Zamagni, 1993). Consequently, this dominant economic paradigm hindered or overlooked a methodology like network analysis – and in particular its SNA formulations - that is incompatible with its fundamental methodological and conceptual/epistemological requisites. Indeed, this fact is typical of normal sciences, which try to protect its hard kernel (Kuhn, 1962)[11]. Consistently, observations, theories, facts, approaches and views that endanger the achievement of its goals, have been discharged or neglected by standard economics.

Indeed, economic theory is a large family[12] with a major group constituted by various types of neoclassical economics, and other minority approaches, like the neo-Marxian and, (relatively) more recently, evolutionary and cognitive economics (Dopfer, 2005; England, 1994; Foster, 1997; Foster & Metcalf, 2001; Gigerenzer, 2008; Gigerenzer & Selten, 2001; Hodgson, 1993, 2000; Kirman, 2011; Loasby, 1991; Witt, 1993). Neoclassical economics approaches share three common views (Landreth

& Colander, 2001; Screpanti & Zamagni, 2005; Zamagni, 1993): one is methodological individualism (Udehn, 2002), according to which the basic unit of analysis is the *independent* individual; another one is the theory of utility-value, according to which the source of value is a good's utility (and scarcity) and capital is a productive factor perfectly and fully substitutable with labour; and the third is individuals' selfishness, according to which individuals pursue their own goals and interests, regardless of others' needs, goals and desires[13]. Within this majority group, which includes also Austrian economics and game theories, the mainstream is represented by neo-Walrasian approaches to general economic equilibrium (Bridel, 2012; Hahn & Petri, 2002; Ingrao & Israel, 1990; Kirman, 1976). This theoretical framework is so diffused that likely in almost all departments or business schools or masters in economics, that one is the main point of reference for teaching. So much, that it might be labelled "standard" economics, which can be found in all basic courses of microeconomics everywhere, at least in its version of partial equilibrium analysis.

Standard economics, especially in its version of *general competitive equilibrium*, adds to the three shared views of neoclassical "family" approaches some further fundamental requisites (Hahn & Petri, 2002; Ingrao & Israel, 1990; Kirman, 1976; Loasby, 1991; Screpanti & Zamagni, 2005; Zamagni, 1993): i) economic agents are not simply *independent* individuals pursuing their interests, but also so *cognitively strong* to be able to find optimizing solutions; ii) information must be completely (or almost completely) free and available[14]; iii) given the two previous conditions and a set of strong constraints[15], there should be a set of prices of all goods and services that optimize resource allocation and individual utility preferences; iv) such a set of prices should be a fixed point with the characteristics of stability (and hopefully, also of unicity) calculated respect to the system of differential equations describing the economic system. In short, economic theory is a price theory that defines the set of prices that keep the economic system in a stable (and hopefully unique) equilibrium state, such that all resources are efficiently (optimally) allocated and individuals' preferences efficiently (optimally) satisfied.

In a static approach, the entire economy can be assimilated to an immense auction where – with or without the support of an auctioneer, depending on the specific theoretical approach – with a series of declarations an adjustment – bargaining, in some theoretical approaches - process leads to an equilibrium point. There, price calibration – possibly negotiation in some theoretical approaches - stops and exchanges take place. However, for preferences and technologies do change – and as well states of nature changes, due to the discovery of new mines, to accidental events, etc. – the auction should be "convened" again, and restarted. As a consequence, *market exchanges are spot events that, moving from a given equilibrium point, occur any time that a change of the set of preferences, resources and technologies happens*. In fact, in a dynamic perspective, in order to find optimal solutions, economic agents continually change their counterparts, be them suppliers or buyers. Indeed, due to its substantial mathematical difficulty and to its endangering effects on the accomplishment of standard economics goals, dynamic approaches have been quite unexplored, and left more visited by other neoclassical approaches, like Austrian economics, or by sectoral and "softened" applications[16].

As can be seen, respect to the basic neoclassical "family", standard economics requires four more essential constraints, and it focuses on demonstrating the existence, uniqueness and stability of a general economic equilibrium. These demonstrations are so fundamental because, if they lacked, it could not be argued that self-interest, competition, and market mechanisms guarantee the perfect (efficient) allocation of resources and satisfaction of preferences. In short, it could not be argued any more that capitalism is an efficient governance mechanism, especially if left on its own dynamics. The effectiveness and efficiency of the "invisible hand" would vanish.

There is a plethora of strong criticisms to the whole family of neoclassical approaches, and in particular to standard economics. Leaving aside those more related to technical aspects concerning the mathematical requisites and problems affecting the uniqueness and the stability of a fixed point, most criticisms focus on the absolute unrealism of its empirical requisites. Just to mention a few, let remind that information is not free, and often neither available, especially concerning future states of nature. On the other hand, the fulfilment of general economic equilibrium requires to know supply and demand (and production technology) of all goods under each state of nature. For instance, recalling a Hahn's famous example, in order to calculate umbrella prices today, one would need to know the demand and supply of any kind of umbrella in all future rainy days in Cambridge. Even if one would be less ambitious, who could "estimate" the demand of oil and gas in 2050? Further, due to their cognitive bias (Gigerenzer, 2008; Gigerenzer & Selten, 2001; Kahneman *et al.*, 1982), individuals are not able to correctly estimate neither well known information nor facts. Moreover, most problems cannot be optimized because they have a multicriteria nature (Biggiero & Laise, 2003)[17]. Still, capital and labour cannot be substitutes one another in all circumstances, and cannot move freely over territories.

Because of these and other reasons, mainstream economics has been challenged by many sides: neo-keynesian economics as concerning macro-economics, ecological economics as concerning environment and natural resources issues, behavioural and cognitive economics as concerning decision-making and information processing, MOS as concerning firms strategy and organization, neo-Schumpeterian and evolutionary economics as concerning growth and innovation, etc. Another set of criticism, which concerns not only standard economics but also many streams of applied economics, comes from experimental economics (Kagel & Roth, 1995; Plott & Smith, 2008), either with real or virtual labs experiments. Nevertheless, as it can be expected during decline phases of normal sciences, the "protective belt" (Lakatos, 1978) around its core concepts enlarges considerably and progressively appeal to a disproportionate use of unrealistic and ad-hoc hypotheses. As a result, notwithstanding the innumerable and conclusive criticisms, standard economics continues to dominate teaching courses, and – to a far less extent – to influence applied researches.

Let's focus on the aspects that involve directly the network view, because some of the issues just mentioned could be combined with the network view, and indeed it is what is happening in most network economics. A view of the economy as composed by networks contrasts frontally with many of the fundamental requisites of neoclassical – and even more with standard – economics. The most important points of incompatibility are the following three. If economic agents are interconnected into a multiplicity of relationships, among which also social and psychological ones, then it is hard to argue that they are independent and that they limit connections only to price declarations. Moreover, spot exchanges contrast with the (relatively stable) relationships that characterize real markets, and especially inter-firm relationships in industries and territories (see below the section on MOS network analysis). Not to say that, by definition, (radical) methodological individualism is incompatible with (radical) structuralism, which was SNA methodological and epistemological ground[18]. Thirdly, social and economic networks are characterized by many reciprocal connections and cyclical patterns, and thus, in a dynamic perspective, they should be seen as recursive nonlinear networks. This means that, not only there are network externalities, but also that network dynamics follows nonlinear ways, and both these characteristics vanish any attempt to find single, and especially, stable fixed points, as pursued by the standard economics research programme.

A strong support of this argument concerning the standard economics "epistemological reluctance" to tackle with networks lies in its late and difficult development of a theory of the firm, and even later and more difficult development of a theory of organization. As well known, in general competitive equilibrium models there is no space for firms and organizations: only single consumers or producers confront to find equilibrium prices into a "fictitious universal and pervasive auction". Marshall and the Anglo-American research tradition were more aware that a theory of the firm could not be lacking, but a convincing micro-foundation of industrial economics and a sound orthodox rationale for building a theory of firm had to wait for the late fifties. Given the self-evident existence of firms – which from the beginning of the 20th century were rapidly assuming the form, size and *structure* of giant companies - this "theoretical hole" was lying as an embarrassing missing point in the whole edifice of standard economics. In his 1937 seminal paper on "The nature of firm" Coase was just trying to find a way to fill in that gap by linking the existence of firms to the presence and effects of transaction costs. An argument that remained relatively unfollowed until Williamson (1975, 1981, 1985, 1994, 1996) put it (at the beginning of the seventies) at the ground of transaction cost economics and organizational economics.

Other approaches to an economic theory of the firm and economic theory of organization, with few anticipations during the sixties (like that of Alchian, 1965), consolidated in the seventies with the theory of teams (Alchian & Demsetz, 1972; Marschak & Radner, 1972) and the theory of property rights (Furubotn & Svetozar, 1972; Hart & Moore, 1990), which have been developed into various forms of agency cost theory (Fama, 1980; Holmstrom & Milgrom, 1991; Jensen & Meckling, 1976) and mechanisms of design theory (Hurwicz, 1973; Hurwicz & Reiter, 2006)[19]. All these approaches are somehow orthodox with neoclassical economics, because they try to remain as closest as possible to its principles and possibly also to standard economics. In fact, such an orthodoxy was still keeping the economic theory of the firm and the economic theory of organization so far from real empirical problems and so late respect to other theoretical perspectives, that still in 1992 a prestigious scholar like Radner put the goal of persuading economists of the importance of analysing management and organization structures as a priority[20]. Conversely, approaches linked to innovation and evolution on one side, and to bounded rationality and behavioralism on the other side followed a more heterodox perspective respect to standard economics, converging currently into the galaxy of evolutionary and cognitive economics (Antonelli, 2011, 2014; Dopfer, 2005; Dosi *et al.*, 2000; Foster & Metcalf, 2001; Nooteboom, 2010; Saviotti, 1996, 2003; Teece, 2012)[21].

What mostly matters here within the extremely rich and complex history of the economic theories of firm and organization is that all of them have been forced to face with the fact that organizations – regardless whether profit or not-for-profit – are (more or less stable) structures connecting workers and managers with objects (machines, materials, ideas, information). Therefore, they are socio-technical and cognitive networks[22] (Carley, 1999; Monge & Contractor, 2003). This structural, deep and evident nature is incompatible with standard economics, as argued above, because it is unavoidably structuralist, and hence its theorization was always uneasy within standard economics. This is further confirmed by the efforts made by organizational economics, especially in its more orthodox versions, to dissolve the network nature of organizations into the same theoretical framework used for standard economics. Organizations and firms become a sort of special auctions where labour contracts are negotiated, and the typical network properties, which require a minimum of stability to produce its effects, are annihilated by supposing fast and spot contracts or by simply neglecting network properties. Needless to say, many of the (above listed) usual unrealistic hypotheses are required and adopted by these theories.

The late development of an economic theory of firms and organizations shortly preceded the explosive development of network economics (which is discussed here below). That delay does not depend, of course, *only* on the incompatibility of networks with standard economics, but that latent theoretical conflict plausibly contributed to it.

Last but not least. Last comers into the field of social sciences to massively adopt network analysis, once the chains of general competitive equilibrium have been broken, economists have enthusiastically engaged with this methodology. In a short survey of network economics Shy (2011) lists the following topics: consumer demand under network effects, compatibility decisions and standardization, technology advances in network industries, two-sided markets, information networks and intellectual property, and social influence. Various forms of network externalities are put at the core of network economics (Economides, 1996; Nagurney, 2009; Nagurney & Dong, 2002; Saberi, 2010; Schuett, 2010). In an even narrower definition Bobzin (2006) delimits network economics to virtual and real transportation networks, including information as things that can be transported. In recent times, Goyal (2007) and Jackson (2010) delimit the field of network economics in a much broader way. After considering all these contributions, network economics can be divided into three main research streams: network formation, economic networks, and economic networks optimization. All the three are producing a growing number of contributions, and here below some of the main will be sketched.

A first branch is network formation modelling, which aims at building models that show how some simple rules lead an actor's decision to link to other actors to maximize his utility function. According to Jackson (2003), for most models define actor's behaviour in terms of strategic behaviour, we can call this branch of network economics as Strategic Network Formation Models (SNFM). Indeed, this emphasis on network formation is all but new, because into MAPH many models of network formation have been proposed and extensively applied, from the random graph models (Bollobas, 1985; Dorogovtsev & Mendes, 2003) to preferential attachment (Albert & Barabasi, 2002; Barabasi, 2002), or network rewiring (Watts, 1999; Watts & Strogatz, 1998), to mention just the most renowned[23].

In general, in SNFM linking provides costs and payoffs, which usually vary with (topological or geographical distance). At node's level networking goes on until an agent is in balance between costs and payoffs, and at network level the process stops when all nodes are balanced and equilibrium is reached. Underlying the lack of strategic models within SNA literature, Jackson (2003) remarks the "birth of a literature that uses game theoretic reasoning to develop such models of self-organizing network relationships"[24]. Respect to non-network games, here the whole network value and allocation rules and efficiency will depend not only on agents number and (when considered) differences, but substantially also on network structure. Equilibrium is defined in terms of Nash equilibrium, and then systematic problems in these models concern the frequent case of the existence of multiple equilibria instead of a single equilibrium point, of its stability, and (even more) of its efficiency. In fact, only under very strict hypotheses on agents' attributes and behaviour it becomes possible to accomplish the three objectives all together: just one stable and efficient equilibrium. Besides the usual categorization in cooperative and noncooperative games, another important distinction is made between pairwise and non-pairwise games. The former means that incumbent links can be deleted only one-by-one, and that to be established the consent of both players is needed, and any deviations can occur pair-by-pair. These were the early models to be created (Aumann & Myerson, 1988; Myerson, 1991), but it resulted difficult to converge to stable states. Hence, in further developments these constraints were relaxed, and the formation of coalitions allowed. Notice that in these models value allocation is separated and postponed to when network stabilizes and the whole value is determined. Therefore, some models (Currarini & Morelli, 2000) allow to jointly determine link formation and value allocation between two players through a bargaining process conditioning their value shares to the possibility to establish a link.

Other models (Jackson & Watts, 2002a) introduce some sort of dynamic link formation, where - starting from a random link distribution - players add or delete links depending on a contingent evaluation whether their new "ego-network" increases or reduces their payoff. The process goes on until no player finds convenient to add or delete a link. Clearly, the possibility to reach a stable state strongly depends on players' rationality (Page & Kamat, 2003): if they are provided by perfect rationality and under many other (mostly inexplicit) hypotheses, then it becomes not too difficult to build a game converging to a stable state.

Examples of applications range widely: friendship networks explored through the (pairwise) connections model (Jackson & Wolinsky, 1996), where links benefits typically deteriorate with distance, but costs hold only for maintaining direct relationships. Another model interprets network topology as geography, so to build a spatial connections model (Johnson & Gilles, 2000). By considering agents as countries, Furusawa & Konishi (2007) build a model on the formation of free-trade agreements network, where a link establishment gives the benefit of eliminating trade barriers. Two other fields investigated by network economics - that it is not possible to discuss in this paper, but that it is important to mention - are those of social learning and public goods. Bala & Goyal (1998, 2001; Goyal, 2003, 2007) proposed a model of social learning networks, with various extensions. Bramoullé & Kranton (2007) developed a first network model on public goods that are non-excludable along social or geographic links. Besides these, there are models on labour networks, buyer-seller networks, co-authorship, etc.

An interesting and recent contribution is that of Gallo (2012) on small-world networks with segregation patterns and brokers, who innovates and enriches previous SNFM (Jackson & Rogers, 2005, 2007). It represents a substantial advancement of network economics towards two directions: one is that of behavioural economics, because – drawing from a previous model (Galeotti *et al.*, 2006)[25] - in a SNFM it introduces agents heterogeneity under the form of a distinction between those with unlimited and those with bounded rationality. In fact, the majority of network economics models implicitly assume that agents have a complete knowledge of the network in which they are embedded. The other advancement is towards SNA and MOS by considering the problems of segregation (Girvan & Newman, 2002) and structural holes (Burt, 1992). Agents' utility to build connections decays with distance, in accordance with Bloch & Jackson (2007), but being conditioned by their cognitive capacity, agents' choices and benefits do not necessarily correspond to the real topology. Further, Gallo introduces the concept of cognitively stable networks, which are an adaptation of the concept of pairwise Nash stability (Calvò-Armengol & Ilkiliç̧, 2010), because equilibrium does not depend on the real network structure, but rather on that that is perceived by agents according to their cognitive "mapping" of the network. This way "for a broad range of the parameters, all the cognitively stable networks belong to a family of network architectures that are structurally richer and more realistic than equilibrium/stable networks previously characterized in the strategic network formation literature" (Gallo, 2012: 3). Finally, Gallo demonstrates that all the cognitively stable networks have a small-world and scale-free topology, and segregation patterns with brokers connecting the segregated communities[26].

The same logic of SNFM is followed by the mechanisms design approach (Hurwicz & Reiter, 2006), which deals not only with industrial problems, but it enlarges the spectrum to any problem of collective decisions. Analogously to game theory approaches, problems are reformulated into this perspective by bringing it at the abstraction level required to be formalized within a typical neoclassical framework, and hence to accomplish its usual objectives of finding an equilibrium, which (possibly) should be also efficient and stable[27]. The low degree of correspondence with real-world characteristics is, as usual, the price to pay for that accomplishment[28].

Another branch of network economics identifies this field as economic approaches to economic networks, usually applied to industries or phenomena whose products or services are characterized by one or more of the following aspects (Shy, 2001): complementarity, compatibility and standards, consumption or production externalities, switching costs and lock-ins. In particular, network effects[29] and externalities are usually stressed as key factors[30], especially in Internet, computer software and telecommunications sectors, but also in audio-video, airlines, and banking sectors. These markets and industries can be hardly treated by standard microeconomics and industrial economics, because they cannot function as competitive markets, characterized by a high number of price-taking agents. In this sense, this branch of network economics is consistent with the other two, because it also does not allow fitting general (or even partial) competitive equilibrium. Indeed, a specific, dedicated and systematic development of a form of network economics[31] can be dated back to the eighties and the full acknowledgment of network effects (Katz & Shapiro, 1985; Liebowitz & Margolis, 1994), and the network neutrality issue (Economides & Hermalin, 2012; Schuett, 2010).

However, in a certain sense, notwithstanding its self-definition as network economics, this collocation is misplaced, because it implies a confusion of interdependence with correlation. In fact, that effect means that a single node's benefit is correlated with that of one or more other nodes, and it is measured by correlations. However, two events can be correlated even without any kind of direct or indirect interdependence (or even simple dependence). At best, correlation indicates that there are some common factors that push two events to move into the same (or the opposite) direction with significant intensity. A high correlation coefficient holding between A and B absolutely does not mean that one can directly or indirectly influence the other. On the other hand, two (inter)connected nodes (or events) are correlated because the influence of a node (or any other entity) on another one is logically and mathematically expressed through a function, and its form determines the way in which (the rule with which) the two variables are (cor)related. Therefore, excepted for very complex phenomena[32], interconnectedness implies correlation, while correlation does not necessary implies interconnectedness. In fact, a collection elements is not necessary a network, even though such elements could be correlated.

This mistake occurs not only when network economics deals with network industries, but often also when it deals with the problem of coalitions, which are depicted as networks even when members are nor directly neither indirectly connected, that is when they do not constitute a network but just a group. Likely, this confusion is fed by game theory, which applies more or less the same logic and concepts to networks and groups. Shy (2011) warns that in most parts of his survey of network economics, "the term "network" will refer to a group of users (consumers or firms) who use products and services that are based on similar technologies". Quite strangely, this definition of network violates that that can be found in any textbook of graph theory or network analysis, because it lacks the fundament of networks: the (albeit potential) connections among nodes. Connections should not necessarily be incumbent, but at least its potential occurrence should be justified according to some theory or empirics. Hence, it seems that a significant part of network economics is based on a misinterpretation of what networks are.

Besides directly claiming input-output matrixes at macroeconomic level, in his recent emphasis on the relevance of networks also on industrial production, Carvalho (2014) shapes industry structures in terms of some prototypical network forms, and he shows very clearly that there are "network effects" else than externalities and the others listed by Shy. These effects refer to the typical nonlinear impact that could be called also "nested interconnection", which occurs when a sector enters into the production of others and these in turn into others, and so on. In these cases, a shock occurring in the upstream sector amplifies its (positive or negative) effects as a cascade. Further, the characteristics of such amplifications strongly depend on the topology on the interconnected industries. We could add that this is, indeed, the well-known research area of network propagation processes, investigated in reference to many kinds of what is propagated – viruses, innovations, information, financial shock, etc. – with its own specificity. The fact that these issues – production processes, like filière or value chains, seen as networks, and the instability generated by possible shocks, as well as the dependence of instability on network structure – are considered as novelties to be published into a leading journal marks the delay with which economics is dealing with network analysis, even after 15 years of explosive research developments[33].

Still belonging to this (mostly descriptive) approach to network economics is the research stream applying network analysis to international trade networks, which from episodic contributions published in marginal journals or in non-economics journals – mostly SOC and MAPH – and often done by sociologists or physicists, is now landing to top economic journals (Chaney, 2014; Furusawa & Konishi, 2007). This way, the very long research tradition on international trade patterns has been enriched by a streamline which, starting about 25 years ago, applied network analysis to this field (Kali & Reyes, 2007; Kali *et al.*, 2007; Kick & Byron, 2001; Kim & Shin, 2002; Snyder & Kick, 1979, Maoz, 2011, among the many). Further, during the second half of the past decade, a number of paper investigated the world trade web from the MAPH network analysis and econophysics perspective[34] (Barigozzi *et al.*, 2010a, 2010b; Bhattacharia *et al.*, 2008a, 2008b; Chakrabarti *et al.*, 2006; Fagiolo *et al.*, 2009; Garlaschelli and Loffredo, 2004; Ruzzenenti *et al.*, 2010; Serrano & Boguñà, 2003; Serrano *et al.*, 2007; among the many).

The third branch of network economics is an area of operating research applied to economic networks, not differently as some parts of neoclassical economics did and (still do), and at which prestigious economists like Arrow, Koopmans, and many others actively participated. Here the purpose of network economics is to optimize economic networks: traditionally, transportation and communication networks (Bobzin, 2006; Nagurney, 1999, 2000), supplier-buyer networks (Nagurney, 2006), and more recently financial networks (Kalyagin *et al.*, 2014; Nagurney, 2003; Nagurney & Siokos, 1997). This branch of network economics is only a little bit connected with the previous one – descriptive economic networks of network industries – and much less connected with SNFM. Moreover, it is totally extraneous to the other areas of SNA.

Economists' scientific interest for network analysis grew fast opening different research streams, enlarging the scope and weakening the rigidity – mostly due to the "neoclassical chains" – of network economics. A well picture of this process of transformation that network analysis is inducing on economics can be seen by confronting the recent presentations of network economics made by Jackson (2014b) and Carvalho (2014) with Zuckerman's (2003) review of a readings edited by Rauch & Casella (2001). This latter discusses if and how the traditional view of markets as constituted by independent firms and customers could be challenged by a view of markets as networks. Besides important methodological warnings on network identification and analysis – and other specific points that here it is not possible to discuss - what apparently emerges is the reviewer's caution and reluctance to accept the rather self-evident fact that many (most?) industries – and also many exchange markets, according to White (2002) - are networks. A fact that 10 years later Jackson (2014) and Carvalho (2014) do not hesitate to argue. Though in both these papers SNA and MOS references are rather residual, it seems that the usage and familiarization with network analysis is producing a sort of "side effect" on mainstream economics weakening its rigidity on traditional traits: a focus on equilibrium and stability at the price of adopting unrealistic hypotheses, which condemn it to irrelevance in representing and interpreting real-world (Helbing & Kirman, 2013; Screpanti & Zamagni, 2005). Perhaps, *it was just this "mutation danger" that pushed Arrow to devalue network analysis for economics*.

There are other examples of economists' investigations – via network economics - into typical SOC or MOS topics: Ballester *et al.* (2006), Currarini *et al.* (2009), Goyal (2003, 2007), Goyal & Vega-Redondo (2005, 2007), among others. It is not possible to discuss all of them, but just a couple more, which are particularly significant because they are placed into two leading economic journals, traditionally representing neoclassical orthodoxy, and more particularly into its mainstream expression of general competitive equilibrium[35]. Ballester and colleagues explicitly establish a bridge with SNA by studying if Bonacich network centrality index (Bonacich, 1987; Hannemann & Riddle, 2005; Wasserman & Faust, 1994) can be an interesting indicator for players of a large set of network games. They demonstrate that, in conditions of Nash equilibrium, that index is proportional to each single player's advantage. Further, they introduce an "intercentrality" measure, which is able to individuate "the key-players, whose removal results in the maximal decrease in overall activity"[36]. Goyal & Vega-Redondo (2007) deals with the issue of structural holes in exchange networks, one of the main topics in SOC and MOS literature. Again, the main scientific added value respect to that literature is to explicitly and formally considering values coming to an actor by forming links "to create surplus, to gain intermediation rents, and to circumvent others who are trying to become intermediary". On the other side, the main scientific added value respect to network economics literature is introducing into a network formation model the advantages of filling in structural holes. Goyal & Vega-Redondo find some prototypical topologies emerging in equilibrium depending on some key parameters - links capacity to carry values, and players' heterogeneity in terms of payoffs.

The excellent and promising reformulation of microeconomics as an interaction-based microeconomics recently made by Elsner (2012), and the abovementioned contributions indicate that the "contagion" with network analysis – and especially with SNA – is producing remarkable mutations of the dominant economic paradigm. Even if a direct confrontation and hybridization between SNA and ECON does not always happen[37], it seems that in more recent works such a confrontation and mutual acknowledgment is growing, and so it could be that future developments will be more open to external contaminations. This speculation seems to be confirmed by looking at the more open and less rigid contributions coming from last years, like that of Gallo (2012) above discussed, and that of van der Leij & Goyal (2011), who examine the "celebrated "strength of weak ties" theory of Granovetter". In doing so they fruitfully and deeply entered into a typical SOC issue approaching it from a perspective and a style reasoning of network economics. Hopefully, along the road of network analysis the distances between SOC, MOS and ECON theoretical frameworks can reduce, and hence favour a cross-fertilization among them.

NETWORK ANALYSIS IN MANAGEMENT AND ORGANIZATION SCIENCES

Knoke (2012) is currently the only book that reviews economic networks taking both SOC and MOS perspectives, at the same time giving some (few) hints to network economics. Therefore, that book is now a good reference point to visit MOS network analysis, and its connections with SOC, ranging over all levels of aggregation: from interpersonal to group and organizational level, and then proceeding from the inter-organizational – business groups and industrial clusters – to the international level – global value chains, international trade, and international corporate or political organizations. Knoke's book is among the few to discuss Burt's (1980, 1988) and White's (1981, 1993, 2002) studies on economic networks placing them into an organic theoretical framework. Indeed, excepted perhaps the field of interlocking directorates, there is no comparative and *integrative* analysis that combine SOC – which in this case could be ascribed also to MOS – with specialized network economics literature. The tradition of industry studies seems quite untouched by that related to SOC network analysis.

Some other works review the articulation and evolution of network analysis applications in MOS: Borgatti & Foster (2003), Brass *et al.* (2004), Carpenter *et al.* (2012), Kilduff & Brass (2010). In this section two of them are synthesized, then shifting the focus on one of the typical field reviewed in these general frameworks: the inter-organizational level of analysis, which has been also outlined by two recent works (Provan *et al.*, 2007; Zaheer *et al.*, 2010). Then, inter-organizational knowledge networks (Phelps *et al.*, 2012) will be zoomed, because this topic – and more in general that of *knowledge networks* in SNA - is becoming particularly relevant in MOS, and covering a large space within that of inter-organizational networks. This choice is led also by the idea that offering a set of topics overlapping with industrial economics enhances a comparison between the ways in which network analysis has been developed into economics and MOS[38]. In fact, the same concept of inter-organizational (and especially, inter-firm) network clearly implies the view that – at least many of industrial – markets are networks. Hence, the delay, caution and suspicion with which economics is (or at least, was) looking at this view will seem here rather unjustified and anachronistic, given the many and solid arguments and empirical analysis provided by MOS.

Borgatti & Foster (2003) underline the exponential grow during 1970-2000 of publications containing "social network" in the abstract and title, and they review the field of the network paradigm in organizational research by evidencing six fields, and four research dimensions. The first field is that of social capital (Lin, 2001), which drew its initial inspiration from Putnam's studies on the civics tradition in Italian provinces (1993). This issue refers to the density and pattern of connections around a focal node, if related to ego-networks, or in a whole network, depending on the level of analysis. The advantages of density have been emphasized by Coleman (1988, 1990) who, much in line with Putnam, underlines that density enhances mutual understanding and coordination. Conversely, Burt (1992) argues that social capital lies into the number of structural holes covered by a given position, that is, into an actor's potential (topological) power to broker other actors, which are eventually connected with him (ego) but not among them (alters). The dispute went on 25 years, and it is recently coming to a final (and combined) view (Burt, 2001, 2005), according to which both meanings matter, but its relative salience depends on the time in network evolution (Soda *et al.*, 2004), and on other variables like the type of network, the type of relationships, and the level of analysis – ego, dyad, sub-network, and whole network. The use of social capital as an explanatory variable to understand various types of advantages and performances is still a very interesting issue in many research fields, like innovation capacity, reputation, etc. The other issue reviewed by Borgatti & Foster (2003) is that of embeddedness, which seems now rather "dated", given that Granovetter (1985) raised the point addressing standard economists, who were quite reluctant to admit that economic relations are embedded into a dense texture of various types of social relationships, and that they influence one another. As seen already in the previous section, excepted undefeated supporters of the autonomy of economic sphere, even neoclassical economists acknowledge the embeddedness of economic relations within a net of social relations, and its mutual influence.

The third issue reviewed was that of "network organizations and organizational networks". The concept of network organizations is also rather dated, because it made sense when in organization theory and business strategy – and especially in organizational economics - it was traced a line of separation between hierarchies and (just) network organizations. The (wrong) idea was that hierarchies are not networks, and thus, it was the case to separate the two. However, since long it is clear that hierarchies are networks, namely out-trees[39], and thus, the interesting question is not to argue a false dichotomy between hierarchies and network organizations, but rather it concerns how to express in formal network structures the main types of organizational forms (simple/artisan, functional, matrix, multi-divisional, etc.). In this view the idea of network organization refers more precisely and interestingly to those organizations whose boundaries are unstable, either because they are frequently (re)designed due to changing environmental features, or because some functions are managed in strict collaboration with other organizations, like in the cases of co-design, co-marketing, etc.

As "organizational networks" Borgatti & Foster addressed to what are properly defined inter-organizational networks, which indeed comprises various other topics: interlocking directorates, joint ventures, strategic alliances, and various other forms of collaboration and (voluntary or involuntary, formal or informal) exchanges, and among which knowledge exchanges and innovation diffusion are particularly interesting. It's impossible here to review a literature that has grown rapidly from the beginning of nineties up to now. Here, just the basic idea is outlined, with a brief comment on some paper[40]. In extreme synthesis, the basic idea is that the famous dilemma "make-or-buy" is indeed a trilemma "make-or-buy-or-ally" (Geyskens *et al.*, 2006**;** Mudambi & Tallman, 2010). In fact, there are many circumstances under which either producing on its own or purchasing from the "market" is rather inconvenient or too risky. According to transaction cost economics (Theurl, 2005; Williamson, 1975, 1981, 1985, 1994, 1996), these are the cases that were initially defined as relational or contractual governance mechanisms, characterized by high frequency and middle specificity of exchanges. Now, a transactional situation like this can occur not only for a pair of organizations, but also for triples or for many pairs, and if some of these small structures are connected, then they form an inter-organizational network[41]. Further, if an entire industry is characterized by these traits, then relational governance becomes the predominant mechanism, and thus, most (or many) organizations will issue some of the many possible forms of agreement. It becomes a network industry, in a much broader sense than the restrictive sense given by network economics to that label, which limits it only to industries or markets where some form of network effect or externality holds. What happened is that, after an initial metaphorical or analogic use of the network concept, after the end of the last century, theoretical and empirical researchers started to use the network concept in a formal and operating way, and therefore to apply SNA to the field of inter-organizational networks.

Another topic mentioned by Borgatti & Foster is knowledge management, which they refer to those of social cognition (Carley, 1989, 1999; Carley & Krackhardt, 1996) and transactive memory systems theory (Hollingshead, 1998, 2000), which both involves social influence theories (Friedkin, 1998, 2003), and interaction patterns[42]. Finally, their list closes with the topic of group structures (Friedkin & Johnsen, 2011), which of course crosses also the three previous ones, because groups are also cognitive networks that create, manage and transfer knowledge, whose efficiency and effectiveness depends on "who knows what" and "who knows who knows what": the basic questions of transactive memory systems theory.

As concerning the research dimensions, besides the already mentioned question of the levels of analysis, Borgatti & Foster focus on the question of the direction of causality, "whether the studies are about the causes of network structures or the consequences". They evidence that the majority of research is about the latter, but that – especially into the field of proximity and homophily (McPherson *et al.*, 2001) – the studies on network antecedents and network formation were at the beginning. Indeed, as we have seen in the previous section (and will treat deeper into the next one) SNFM came only during last 15 years in both ECON and MOS.

Almost 12 years after, another review (Carpenter *et al.*, 2012) synthesizes the state of network analysis in MOS. Two of the research dimensions discussed by Borgatti & Foster – direction of causality and level of analysis - are used even in this paper to cross-classify all studies into a 2x2 table. Further, since social capital has been most frequently considered as an antecedent of network structure, they call this (half) dimension "social capital research", juxtaposed to "network development research", which would concern network consequences. On the other dimension, network levels have been compressed to two: interpersonal and inter-organizational ones. Forced by an expositive choice, here only the latter will be briefly discussed, and as well shortly addressed to one of its main research fields: inter-organizational knowledge networks.

Carpenter and colleagues enumerate many findings obtained by network research in the field of inter-organizational networks, concerning either network determinants and network structures or its effects and endogenous forces. Some years before, Provan *et al.* (2007) dedicated their paper on reviewing only inter-organizational networks literature, focusing on whole network analysis. By applying rather restrictive criteria of selection, they identified 26 publications, an outcome that motivated their evaluation of this as a relatively underdeveloped research area, respect to other fields of MOS network analysis. Later on, Zaheer and colleagues (2010) provided a further review suggesting a categorization of key findings and research issues articulated in two dimensions, one of which is again that of levels of analysis, while the other is articulated into the following four aspects: resource access, trust, power/control, and signalling. They call these aspects "theoretical mechanisms", because each of them refers to one or more theories, mostly created and developed within SOC and MOS. Strangely enough, among the list of the main aspects there is not economic convenience, and (consistently) among theories there is not transaction cost economics. Of special interest are some contributions that, though not applying algorithms and methods specifically designed for network dynamics analysis, adopt a clear dynamic perspective, either in empirical testing through longitudinal data or in theoretical modelling: Cowan & Jonard (2009), Gulati *et al.* (2012), Rivera *et al.* (2010), Schilling & Phelps (2007), Soda *et al.* (2004), Uzzi & Spiro (2005), and Zaheer & Soda (2009)[43].

This absence of an explicit and direct economic dimension – meaning that links have an economic value and purpose – characterizes, with few exceptions, most MOS literature at any of the three classical levels of analysis. This is a good reason for enhancing a substantial knowledge exchange between ECON and MOS, a cross-fertilization that does not imply sharing all of the "other side", but that could produce a great benefit on both sides. Moreover, as concerning inter-organizational networks it should be noted that most part of the specific and abundant literature – and basically all that was produced until the end of last century – use the network concept in a metaphorical or intuitive sense, but does not apply network analysis tools. With few exceptions (Nohria & Eccles, 1992), this holds also for studies explicitly dealing with inter-organizational network formation (Ebers, 1997) or structure (Grandori, 1999). Still recently, good reviews of this research field, like that of Parmigiani & Rivera-Santos (2011), consider network analysis applications in a non-crucial position.

Industrial cluster theory and research (Karlsson, 2008; Karlsson *et al.*, 2005) is starting to give a valid contribution to that type of inter-organizational networks that are geographically defined and (usually) also industrially specialized in one or few sectors. However, here too happened that until few years ago most literature was treating an industrial cluster network structure more as a metaphor or an intuitive concept, sometimes approximately visualized, than a formal methodology with its specific measures. Only recently[44], this formal approach has been taken systematically: Giuliani (2006), Arikan (2009), Staber (2001). Most industrial clusters[45] are *multi-level multiplex large socio-economic networks*, which means that they are micro-societies, because between heterogeneous organizations – artisans, small, medium and often also large firms, higher education institutions, local governmental institutions – there are various kinds of social, trade, informative, and personal relationships. Nodes are individuals, groups, firms, trade unions, trade associations, etc., and hence, there are multiple levels. Likely, it is due to this extremely high complexity that many research issues, like the supposed correlations, at the meso-level of firms, between single nodes positions and performance, or at the whole level researched correlations between some network properties – say, its small-worldliness - and some kind of industrial cluster performance, remain unanswered even after decades of inconclusive studies.

Notwithstanding this evident nature of industrial clusters as multilevel multiplex, Giuliani (2007) is one of the few studies that, with formal network analysis[46] and not just in a descriptive and intuitive way, approached them trying to grasp at least part of this complexity. She analyses the structural properties of knowledge networks in three wine clusters in Italy and Chile, and she compares knowledge topologies with trade topologies in each of them.

Strictly related to that of inter-organizational networks and industrial clusters is the topic of innovation networks (Ahrweiler, 2010; Pyka & Küppers, 2002), because large part of studies have focused those that, in both cases, have high innovating capacity. As for previous issues, most literature uses the network reference more as a metaphor or descriptive/intuitive phenomenon, rather than as a precise ontology whose investigation requires specific formal methods. However, as for the other topics, things are moving towards this *double acknowledgment*: ontological/epistemological on one side, and methodological on the other side. A clear example of this process is represented by the journal *Industry & Innovation,* which dedicated to the analysis of innovation networks with network analysis methods a special issue in 2011, followed by another one in the same journal during the same year, but focused on organizing inter- and intra-firm networks to improve innovation performance.

This (inevitably selective and partial) excursus on MOS network analysis ends up with a look at the literature on inter-organizational knowledge networks. Phelps *et al.* (2012) recently survey the field by building an articulated conceptual structure: their "typology organizes knowledge network research based on three important dimensions: knowledge outcomes (knowledge creation, knowledge transfer and learning, and knowledge adoption), knowledge network properties (properties of network structure, relations, nodes, and knowledge flows), and level of analysis (interpersonal, intraorganizational, and interorganizational)" (2012: 1119). Phelps and colleagues review literature that connects knowledge outcomes, like indicators of innovation performance, to firms network position, or its ego network structure or the whole network structure. As concerning correlations between network position and innovativeness, results are inconclusive, either for direct or indirect centrality. There are, in fact, a lot of contingent and moderating variables, like the type of knowledge[47], geographical proximity and the kind of performance parameter, not to say the industrial sector and the methods of data collection.

Things do not go better as concerning the role of social capital, meant à la Burt or à la Coleman, that is the effects determined by, respectively, open (as structural holes) or closed (complete) ego network structures on knowledge creation and transfer: studies are as well inconclusive. Some more agreement there is as concerning the relationship between network properties at network level and knowledge outcome, but as Phelps and colleagues underline this agreement depends to some extent on the scarcity of specific studies. They review Schilling & Phelps (2007) and Uzzi & Spiro (2005) who find that the small-world structure is particularly effective to enhance innovativeness, but with the need of a non-excessive intra-cluster density, because otherwise the side effect of strong closure reduces firms creativity (Gargiulo & Benassi, 2000). This idea has been elaborated by Nooteboom *et al.* (2007) in terms of optimal cognitive distance for knowledge absorptive capacity. Broekel & Boshma (2012) argue a similar idea with their proximity paradox, showing that while cognitive, social, organizational and geographical proximity were crucial for explaining the knowledge network of the Dutch aviation industry, too much proximity in one of these dimensions harms firms innovative performance. Considering another well-known network property, Lin & Li (2010) show that, respect to random, small-world, and regular topologies, the scale-free provides an optimal pattern for knowledge transfer.

Phelps and colleagues remark that even as concerning the role of tie strength there are conflicting results between those who find support for a positive relationships between strong ties and knowledge performance, and those who find the opposite. Finally, as concerning nodal similarity/proximity, it seems that the best conditions occur when the organizations constituting a network have an intermediate degree of similarity, because if they are too similar, then there is no interesting exchange[48], and if they are too diverse, then it becomes too difficult to understand the usefulness of one others' knowledge. A result consistent with Nooteboom and colleagues (2007) and Broekel & Boshma (2012).

Interestingly, the issue of inter-organizational knowledge networks crosses that of industrial clusters, because inter-organizational networks constitute the "structural backbone" of industrial clusters, and much of the competitiveness capacity of high-tech industrial clusters is supposed to come from the effectiveness and efficiency of knowledge creation and diffusion within industrial clusters and between an industrial cluster and its external inter-organizational network. Here much of literature on innovation networks, industrial clusters and inter-organizational knowledge networks can be placed. Examples are the abovementioned work of Giuliani (2007) on the wine clusters and those of Alberti & Pizzurno (2015), Biggiero & Sammarra (2010), Broekel & Boshma (2012), and Sammarra & Biggiero (2008) on the aerospace cluster. Remarkably, this topic of inter-organizational knowledge network, especially if combined with geographical aspects, has been matter of recent applications of ERGM (Broekel *et al.*, 2014; Hazir & Autant-Bernard, 2014; Zhang *et al.*, 2014), which are the theme of the next section.

THE MODELING REVOLUTION

Despite methodological and epistemological differences, SOC, MOS and ECON – and to a less extent, MAPH too – are progressively overlapping on some common areas of research interests, namely small-world (Baum *et al.*, 2003; Schilling & Phelps, 2007; Soda *et al.,* 2004; Uzzi & Spiro, 2005) and scale-free structures (Biggiero & Angelini, 2015; Breschi & Cusmano, 2004; Di Guilmi *et al.*, 2003; Ebel *et al.*, 2002; Gaffeo *et al.*, 2008; Jackson & Rogers, 2007; Konno, 2009; Roediger-Schluga & Barber, 2006; Tseng *et al.*, 2009), social capital (Coleman, 1988, 1990; Lin, 2001), structural holes (Burt, 1992, 2005; Gallo, 2012), positional advantages (Doreian *et al.*, 2005; McBride, 2008; Wasserman & Faust, 1994) and equivalence (Wasserman & Faust, 1994), core-periphery structures (Borgatti & Everett, 1999; Hojman & Szeidl, 2008; Wasserman & Faust, 1994), community structures (Girvan & Newman, 2002). A growing attention is being paid to understand not just the properties and the consequences of these phenomena, but also how they have been generated, in relation to endogenous dynamics. A never ending descriptive and analytic research work is being accompanied by "retrodictive" (network formation) and predictive modelling. If in SFNM and MAPH network formation and statistical modelling has been standard scientific work, things evolved differently in SOC and MOS.

Until the nineties, SOC and MOS network analysis have been done, with few exceptions (Breiger *et al.*, 2003; Doreian & Stockman, 1997), mostly with descriptive approaches and sometimes with analytic or descriptive statistical approaches: antecedents or (more frequently) consequences of a given network (or a set of similar networks) are analyzed in relation with that network structure. By the end of nineties, mostly due to Snijders and his colleagues, dynamic statistical modeling to SNA and MOS start developing[49], initially limitedly to longitudinal methods (Snijders, 1996, 2001, 2005; Snijders & van Duijn, 1997). With early studies during the nineties (Wasserman & Pattison, 1996), who built on seminal works by Holland & Leinhardt (1981) and Frank & Strauss (1986), and a substantial milestone represented by p* modeling (Wasserman & Robins, 2005)[50], during last years a statistical modeling is being developed and consolidating mostly into ERGM (Lusher *et al.*, 2013; Harris, 2014).

ERGM represent a methodological revolution because, in reference to a given (observed) network, they allow to make a sort of "reverse engineering" to understand its most probable combinatory "parent". Though the statistical methods can be quite sophisticated, the basic idea is very simple and can be summarized in the following steps:

1. Analyze your observed network to formulate what you consider its distinctive and fundamental elementary properties, which can be either first-order, like density, average degree centrality, etc. or second-order parameters, like reciprocity, transitivity, assortativity, clusterization, small-worldliness, etc.: these are your descriptors, some of which will be your explanans and some others your explanandum;
2. Take a random graph of the same size and density of your observed network, and within the sample space of all possible combinations, create a number of random graphs. Then, check if your observed network falls in the mean value respect to each of your selected statistics or if, conversely, they have a low probability. In the former case, the statistics you have selected do not identify a distinctive trait of the observed network, because it would (as well probably) occur randomly. Conversely, those statistics are a good candidate for building – alone or jointly with some others – an ERGM for your observed network;
3. Build the ERGM by "imposing" to a random graph the statistics you have selected. In other words, within the sample space of all possible combinations, create a number of random graphs with the constraint of having the selected statistics. This produces a distribution of statistics generated by the model;
4. Proceed to find the "right" weight (estimate) of each statistic, that is its parameter value, in order to fit the model respect to the observed network. It means that you should choose the best combination of all parameters "so that observed statistics are not extreme, but are central, in the distribution of graph statistics from the resulting ERGM" (Robins & Lusher, 2013: 35).

Though ERGM was originally focused on link formation constrained by other links distribution – for example, reciprocity degree or micro-patterns like in-2-stars – current developments are working to widening modeling potentialities[51]. *From a methodological point of view*, they are radically different from standard statistical approaches, which assume independence of observations, because link formation is based on some mechanism of dependence between (types of) links and local patterns. Thus, standard statistical approaches – with its typical methods such as t-tests, ANOVA, regressions - cannot be employed[52]. *From an epistemological point of view*, ERGM acknowledge many aspects of complex social systems, like mutual dependence, structural heterogeneity and specificity.

A criticism raised by Newman (2010) and Jackson (2010) to ERGM application potentialities concerns the serious limitations imposed by the computationally intractable combinatorial space implied by non-small networks. In fact, the number of configurations to be computed to find and test the fittest one scales as $2^{n(n-1)}$ for directed and $2^{n(n-1)/2}$ for undirected graphs. Obviously, for network size major than a very small one, it would become impossible to compute all configurations. More recently, Chandrasekhar

& Jackson (2014) argue that ERGM and SNFM belong to the general class of Statistical Exponential Random Graph Models, as defined by them. Further, they show that "by reformulating network formation as a distribution over the space of sufficient statistics instead of the space of networks, the size of the space of estimation can be greatly reduced, making estimation practical and easy". This way they offer a solution to the criticism abovementioned.

A generalization of that solution implies to escape from an abstract implementation, and cope with real-world networks, because it is unlikely to face with networks about which we have no other factual or theoretical information on its main characteristics else than one or two simple rules concerning link attachment. One could have diachronic information regarding more or less probable states that the network has assumed in the past, and a number of information concerning nodes attributes and link distribution, which could substantially reduce the degree of freedom of single nodes position and network topology. For example, suppose to deal with an inter-firm network corresponding to an industry. This means that there is a technology and a certain degree of heterogeneity among nodes: some of them will be direct rivals, while others only weak competitors, and others strong complementary within a same segment (parts of the production cycle) or across segments (phases of the filière). The joint action of technology and heterogeneity determines a number of forbidden combinations: for instance, firms of the type A cannot connect with those of type B because this would result in a technological nonsense. Vice versa, others would be preferred (or even forced) connections: A firms could be forcedly linked only to a C or D firm.

The key-point is that the more type and number of forbidden or forced combinations, the more dramatic is the reduction of combinatorial space. In fact, as the combinatorial space scales exponentially in the growing direction, so it does also in the opposite direction. Let's recall Simon's example in discussing the architecture of complexity (1962). Of course, the more constraints and forbidden combinations, the more tractable is calculation, but also the less generalizable is the resulting model.

Toivonen *et al.* (2009) propose the following categorization of socio-economic network modeling, slightly different from that of Chandrasekhar & Jackson (2014): network evolution models, nodal attribute models, and ERGM. The former class substantially coincides with that of SNFM, and thus, with network economics modeling, where links and node are added iteratively according to certain rules. This class can be further divided into growing and dynamic models. Growing models fix a predetermined network size (or density) at which "evolution" must stop, while dynamic models stop when "selected statistics no longer vary". This is proper of most SNFM, which end up when some sort of Nash equilibrium is reached. According to the classification advanced by Toivonen and colleagues, nodal attribute models are based on distributing links depending of nodes attributes similarities, that is on its homophily under some respect. Spatial models (Boguna *et al.*, 2004; Wong *et al.*, 2006) would belong to this class, because in this case similarity would be defined in terms of a node's "location" in a social or geographical space. Finally, ERGM "are used to test to what extent nodal attributes (exogenous factors) and local structural dependencies (endogenous factors) explain the observed global structure" (2009: 241)[53].

During the last 10 years – and especially in the last 2-3 years – a plenty of ERGM applications have been done, mostly within SOC[54] (Ellwardt *et al.*, 2012; Yap & Harrigan, 2015; Zappa, 2011), but also in MOS (Lusher *et al.*, 2012; Pahor *et al.*, 2008; Wong *et al.*, 2015; Zhang *et al.*, 2014) and other fields (Broekel *et al.*, 2014; Hazir & Autant-Bernard, 2014; Henry *et al.*, 2010). Among the many, and in line with the focus of this chapter and the previous section, a couple of them dealing with inter-organizational networks will be reviewed. Lee *et al.* (2012) study an inter-organizational collaboration network related to a regional economic policy development within the Orlando Metropolitan Area in Florida in 2006-2007. The network is made of 40 government and non-governmental organizations, which could coordinate or act independently for local policy development. The main aim of the study is to understand the logic of building connections so that the observed network results as a probable outcome. The theoretical problem is that each organization has good reasons to reciprocally exchange information and coordinate, because this would improve its policy effectiveness, but at the same time this behavior would subject to a high defection risk, because information obtained through interactions could give policy intervention advantages. In other words, in a game theorizing perspective, the setting involves either zero-sum or additive games. Lee and colleagues build an ERGM building on the following five statistics: reciprocity, clustering (transitive triads), popularity (in-k-stars)[55], brokerage (2-node paths), homophily. Notice that this latter is an attributive instead of a topological statistic, as the previous ones, which are the most used in the ERGM standard approach. Results show that organizations prefer connecting with dense clusters, but not necessarily with very popular (central) nodes. Further, it is strongly preferred to bond with non-brokers than with brokers. Coordination and information advantages drive to connect and reciprocate, but the high defection risk discourages to connect with popular and bridging alters.

Sharing the SOC and MOS view of industries as networks, Lomi & Fonti (2012) analyze the collaboration network of 72 Italian tiles producers, accounting for more than 90% of total industry sale. They hypothesize three endogenous factors explaining the observed network structure: 1) reciprocity, which makes connection more likely to be established among reciprocating firms; 2) transitivity, which makes connection more likely to occur between companies sharing the same partner; 3) bridging, which makes connection more unlikely to occur with companies playing a brokering role, that is filling in many structural holes. By building an ERGM they found confirm of all the three hypotheses, with a particular weight of the reciprocity mechanism.

Research on ERGM – and more generally on any kind of statistical network models - is under tumultuous development. Among some important advancements there has been the extension of ERGM methods to bipartite networks (Agneessens & Roose, 2008; Skvoretz & Faust, 1999; Wang *et al.*, 2009, 2013). Further developments concern that particular sub-set of the class of bipartite networks in which nodes (or the members/individuals) of one group can be affiliated only to one node of the other group. This is a way to look at multi-level networks, which have been developed by Snijders & Bosker (2012) with hierarchical linear models. Recently, Wang *et al.* (2013) advanced ERGM methods to deal with multi-level networks, and to remove the constraining requisite of hierarchical linear models according to which links between and within groups do not exist. Zappa & Lomi (2015) build a multilevel ERGM to study the effects of hierarchical relations between organizational subunits on the presence or absence of informal network relations among organizational members.

Besides ERGM, another network analysis simulation model has been developed and it is diffusing in SOC and MOS: stochastic actor-oriented modeling (SAOM), which has been introduced by Snijders and some colleague (Snijders, 1996, 2001, 2005, 2008; Snijders & van Duijn, 1997; Snijders *et al.*, 2010)[56]. As SNFM, these models are actor-based instead of link-based, and use objective functions - called evaluation functions - that drive actors' choice to (dis)connect with others, but differently from SFNM, actors do not necessarily seek to maximize some utility function[57], that is, connections could have no economic values. Further, though SAOM resemble SNFM because they aim at understanding network inner dynamics, they differ because – as ERGM – they do not start from a single or a kernel of actors and then let them grow until a fixed predetermined size or until some kind of algorithmic or ontological equilibrium is reached. On the contrary, they assume an initial (and expected) size, and let the network evolve without a predetermined final state. Jackson () reviews various types of statistical models for network analysis presenting SNFM as a different category respect to ERGM, underlining that, contrary to ERGM, SNFM are based on agent's choice about utility-driven (dis)connecting behavior and agents' dependence.

Model building is driven by actor-based probabilistic specifications that are proportional to the exponential transformation of the objective function, and parameters estimation occurs in a similar way to ERGM (Snijders *et al.*, 2010)[58]. To a larger extent than SNFM, which until now – due to its neoclassical chains - have been quite limited as respect to agent's behavior[59], SAOM allow to take into account either social influence or social selection mechanisms, and hence they allow to distinguish to what extent and how actor's attributes influence network structure and the reverse[60]. Only in recent developments ERGM are employing also actor's attributes, and thus, broadening its explanatory power and reducing its methodological distance from SAOM. And both can be used as simulation modeling and not only for identifying elementary patterns or predictive purposes: it is enough to impose instead of estimate parameters.

All the three types of modeling - ERGM, SNFM and SAOM – are progressively leaving the over-simplifying assumption of agents' behavioral homogeneity, that is their uniform way to (dis)connect and react to others' behaviors, but they are still far from incorporating actors' realistic behaviors, as depicted by cognitive and evolutionary economics (Gigerenzer, 2008; Gigerenzer & Selten, 2001; Kahneman *et al.*, 1982). The main difference between ERGM and MAPH models on one side, and SNFM on the other side is that these latter explicitly and formally interpret link formation or deletion as driven by its economic costs and payoffs, expressed through an utility function. The rationale justifying why (and also how) an agent – represented by a node – establishes or severs a connection to some other agent is because it provides an economic advantage, which should be evaluated respect to its cost. Conversely, in SNA and MAPH network formation modelling the rationale pushing a node to connect to another one does not have an explicit economic nature. It is usually related to the intuitive advantages to be directly or indirectly central or – more rarely – to be placed in specific areas of the network. In general, there are topological reasons to connect, which of course in certain contexts can be reinterpreted in an economic sense, but *per se* they are not conceived as economic reasons[61].

On the other hand, ERGM, SFNM and SAOM are particularly advanced for neoclassical economics – especially standard economics - because they allow taking account of endogenous processes. While in SOC and MOS they are all but new or "strange phenomena", in economics endogenous processes have curiously been eluded, especially from formal theorization[62]. In fact, general and partial equilibrium cannot deal with them, because equilibrium existence is not more guaranteed, nor it is guaranteed its uniqueness or stability (Ingrao & Israel, 1990; Kirman, 1976; Loasby, 1991; Screpanti & Zamagni, 2005). Game theorizing (Gintis, 2000) and network economics are starting to tackle with this issue, and in fact they are opening a sort of "Pandora box" of effects, which often, in order to obtain some sort of equilibrium, force adding further constraints to usual modeling.

ERGM are static, because they assume the observed network as a stable state and try to search for its fittest estimated model "rolling the dice" at each trial. Though the Markovian Chain Monte Carlo algorithm adjusts estimation parameters in order to increase fitness, that is, in order to guarantee convergence to the fittest models[63], trials are independent each other: there is not direct consequence from one to the nother. It is as if we had a given Lego construction – the observed network - and we hypothesize that, justified by some theory, that construction could (or perhaps, should) be obtained by assembling certain building blocks and joining rules. Further, the observation and disassembling of the real construction gives us information about the relative weight (relevance) of each block and rule. Once we know all this, we design a method to build models of the real construction, models that are built according to the specifications – blocks and rules – and its weights. In this methodology there is no dynamics, no path dependence, nor interactions among actors and links. All these aspects (and thus, dynamics and self-organization) can be – but not necessarily should be – into the theory that is behind the formulation of the hypotheses/specifications, but this does not imply that dynamics and self-organization are also (transferred) into the model.

Conversely, SAOM and (many) SNFM are dynamic models, because they are based on path dependence[64]: a network topology at time t+1 follows *entirely* from what happened at time t, and the transformation does not need exogenous events[65]. But this form of dynamics is rather poor and simplified respect to recursive processes, especially when such processes: i) influence agent's behavior; ii) links reciprocal influence[66]; iii) produce emergent effects, which eventually feedback to influence agent's behavior and links influence. This is the true complexity (Biggiero, 2001; Casti, 1994; Mainzner, 1994; Standish *et al.*, 1998), which of course is also path dependent, but besides being this, it is much more. To grasp "true dynamics" or complexity, path dependence is not enough. To the extent that analytical sociology has defined itself as mechanism-based or generative-based explanations (Manzo, 2014b), it does not seem that there has been a full understanding of what complexity means, at least in a deep epistemological sense, and not just as a synonym of "complication" (Biggiero, 2001). Provided that mechanisms and interactions are the same thing, in order to qualify *complex* systems/networks, a focus on mechanisms is not enough. It should occur another couple of features: such mechanisms should be connected in a recursive and nonlinear way. Nonlinearity, which is the main "ingredient" of (almost all) sources of complexity, requires recursiveness (of interactions) to produce complex behaviors.

ERGM are sophisticated methods to check whether a certain set of network descriptors can be "combined" to produce a given, specific observed network. It is an exercise in reverse engineering and not the generation of "true dynamics". ERGM represent an extremely powerful method for understanding the determinants of network structure, but they cannot incorporate recursive processes nor actors' reactions and changes in consequence of others actions/decisions. The potentiality of ERGM has a limit in the degree of complexity of the observed network and in the degree of the required accuracy of estimates. If both are high, they should leave the scene to agent-based simulation models (ABSM), which in principle have no limits in neither of the two aspects, and can truly be "generative explanations" (Epstein, 1999, 2007). Therefore, ERGM cannot deal with the true essence of complexity, which lies in recursive interactions among short-sighting complex agents. Conversely, ABSM (Carley, 2009, Davis *et al.*, 2007; Gilbert, 2008; Gilbert & Terna, 2000; Gilbert & Troitzsch, 2005; Harrison *et al.*, 2007; Tesfatsion & Judd, 2006) can take into account, at least in principle, as much "true" complexity as required, and thanks to this capacity they are qualitatively different from ERGM, SAOM, and the other kinds of SNFM[67]. This demarcation is controversial in current debate. For instance, Snijders & Steglich (2015) argue there are no substantial differences, so that an advanced SAOM model like the one they discuss "may be regarded as an agent-based model". Conversely, Hedstrom & Manzo (2015), in the editorial of the same special issue where Snijders & Steglich (2015) publish their paper, suggest "a preliminary distinction between ''statistical'' and ''theoretical'' models …, the former referring to models that are used for estimation purposes while the latter type of models seek to formally represent a process believed to have generated a specific type of outcome". This distinction between estimation and representation purposes is not sound, and it is full of ambiguities due to the vagueness and overlaps between the two concepts. Conversely, the distinction between models capacity to deal with nonlinear recursiveness is clear and strong, and thus, it is the best candidate to be a demarcation line between the two modelling families of ABSM on one side and many (perhaps most) other modeling methods on the other side.

It should be stressed that, if a relatively simple network-based methodology and epistemology like those implied and brought forth by static and descriptive network analysis is already rather incompatible with the methodology and epistemology standard (mainstream) economics (especially if meant as the traditional approaches to general competitive equilibrium theory), dynamic modeling as the various kinds of SNFM, SAOM, and ERGM remarkably enlarges the scope of incompatibility. This does not mean that it becomes impossible to build competitive equilibrium models, but that they cannot be general any more, and that they –even limited to partial equilibrium analysis - can hardly be grounded on stable and unique equilibria. As we have seen, most SNFM remain within neoclassical economics, and many of them are partial equilibrium models. according to the ways in which they have been built until now, SAOM assume actors' behaviors as optimization of utility (objective) functions, and in so doing they share a basic assumption of neoclassical economics. As well occurs generally (but not necessarily) with NK modeling (see next chapter of the book) and artificial neural network simulation (see chapter 3). ABSM are potentially much more disruptive for neoclassical economics, even for non-equilibrium approaches, because they allow any degree of freedom in complexity. Of course, it does not mean that it is impossible to design neoclassical models – and even equilibrium models - with agent-based methods, but only that with ABSM can be done what with neoclassical economics cannot be done. In other words, neoclassical models – and even more equilibrium models – become special restrictive cases of reality oversimplifications[68].

FROM JUXTAPOSING TO COMBINING

In previous sections, neoclassical economics methodological individualism has been juxtaposed to SNA structuralism, and further it has been posed as the main reason why economics delayed to engage in network analysis. Now it is necessary to go deeper into this issue, in order to better specify its characteristics and implications. In fact, recent network modeling of ERGM and SAOM are realizing a fruitful and necessary combination and reconciliation of the old juxtaposition between methodological individualism and structuralism. In its radical formulations, the former perspective focuses exclusively on individuals choices, and structures dissolve into environmental constraints and resources. Neoclassical economics is a paradigmatic example, not only in its general competitive equilibrium approaches, but also in Austrian approaches: independent agents pursue their utility maximization, given their information, resources and technologies. Of course, Marxian approaches took the opposite view, because individuals are seen as strongly embedded – as we would say today – into their social, economic and technological production (and re-production) structures. Marx (and Marxism) was intrinsically structuralist. Granovetter's (1985) argument of under- and over-socialized agents is just another way to present the same problem. Sociology has been not immune from methodological individualism, which oriented a large part of sociological thought, even if usually in a less extreme version than that assumed by neoclassical economics. On the other side, radical structuralism – as that that dominated early sixty years of social network analysis from Moreno's time up to the nineties, and that was prevalent in large part of sociology during the same years – was denying (or heavily reducing) agent's attributes and discretionary power, besides the expected behavior defined by the "topological opportunities" determined by her position. Agents cannot do much more or different than what is determined by their positions.

A corresponding juxtaposition characterized methodological individualism on one side and systems science on the other side, and it is clearly reflected into the history of organizational thought (Biggiero, 1990; Pfeffer, 1997; Tsoukas & Chia, 2011). Usually, scholars dealing with organization design and structural aspects – including technological aspects – were emphasizing the relevance of structure respect to single individuals, while scholars dealing with organizational behavior and personnel management were underlining the opposite. The issue of organizational decision making is highlighting in this sense: on one side the hyper-rational view of who prescribes that agents should individuate, analyze, and compare alternatives, and then choose the optimizing one; on the other side who remarks that real agents could not be farther respect to the "ideal" view. Individual's choices within organizations are heavily influenced by the organizational context in terms of socio-cognitive ad social-psychological variables[69].

As some other methodological and epistemological juxtapositions[70], even this one revealed wrong, because agents' behavior is structurally constrained and co-determined by their (at least direct and close) interconnections, but often it can happen also that such constraints and co-determinations could be modified – to some extent – by the agent, and this capacity depends on the agent's attributes. Network modeling – and to a larger extent, ABSM – helps stopping the "ideological war" between individualism and structuralism by giving the possibility to combine the two. Boudon (1998) and (much more) Giddens (1984) have built theoretical frameworks in the perspective of overcoming the juxtaposition between individualism and structuralism. Especially Giddens' structuration is consistent with a dualist view, where individualism and structuralism are reconciled in a unified social theory. However, until the questions are put in very general (and correspondingly vague) terms, it becomes very difficult to confront and persuade one another. A bad use of scientific rhetoric (McCloskey, 1985, 1994; Simons, 1989, 1990) can "digest" almost everything.

More recently, Hedström (2005) and Udehn (2002) support the "reconciling perspective", and in the same view is moving current analytical sociology (Manzo, 2014a; Squazzoni, 2012). Manzo (2014b) refers to *structural methodological individualism* to synthesize the combination of individualism with structuralism, and he underlines the (obvious) fact that micro-behaviors (and mechanisms) precede the (emergent) macro-effects. Biggiero (2011) argues that social (and natural) evolution results from co-evolutionary processes in either horizontal sense, because elements (nodes) interact and influence each other, or in vertical sense, because emergent (macro) entities – be them concrete or symbolic or other kind of structures – are generated by the bottom interactions, but in turn they can influence them in various ways - by constraining, speeding, or qualifying bottom interactions. In this multi-level view of double (bottom-up and top-down) co-evolutionary processes, what is emergent and macro respect to the underlying level of description (aggregation) becomes the bottom (lower) level respect to its emergent outcomes, which are at a corresponding higher (meta-level) of description (aggregation). Each level is emergent respect to its lower one: groups are emergent respect to its members interactions, organizations are emergent respect to its groups, etc. Further, Biggiero (1990) proposes to interpret this ontological view in terms of Bateson's logical types (1972), and (in 2009 and 2012) he applies this interpretation to the distinction between information and knowledge: this latter would be an emergent property from information network dynamics.

Indeed, much more effective than very general and broad frameworks and arguments is the capacity of a new approach to *produce concrete applications*, and it becomes even more convincing if it has the capacity to produce falsifiable predictions (Biggiero, 1997; Lakatos, 1978; Lakatos & Musgrave, 1970; Popper, 1972) through clear and controllable-replicable methods, like real and virtual experiments. And this is exactly what network (and agent-based) modeling enables.

By operationalizing agent's behavior respect to other agents and by imposing the presence of a certain share of motifs (configurations) or other kinds of structural specifications, SAOM and ERGM can dissolve the juxtaposition into concrete hypotheses about how, respectively, the network will evolve or how it was generated. The magic of operationalizing in concrete models is also demonstrating the sterility of ideological juxtapositions. Once we wonder what a specific type of agent - in a specific context respect to a specific problem – can or cannot do and how his ego-network or whole topology can constraint or enhance his behavior, ideological juxtapositions appear sterile and the debate is shifted to the plausibility of specific modeling choices and to the corresponding implications.

Clearly, being its modeling potentialities limited respect to what can be done with ABSM, network modeling cannot fully develop the scope of free combinations of individualism and structuralism. In fact ABSM, especially when are enough rich to consider the feedback from emergent effects on single agents, can fully take into account agent's potentialities and structural specificities (Squazzoni, 2012). On the other hand, network models offer the advantage to be less complex to be designed and interpreted. However, being (relatively) simpler, network models are more contendible, and in this sense they are more fruitful for scientific knowledge growth[71].

We can get explicit references and traces of this reconciling approach in Snijders', who initially argued that his "approach to network evolution is in line with the theoretical sociological principle of methodological individualism" (1996, 2001), but later moves towards the positions of Udehn (2002) and Hedström (2005): "The actors control their outgoing ties. This means not that actors can change their outgoing ties at will, but that changes in ties are made by the actors who send the tie, on the basis of their and others' attributes, their position in the network, and their perceptions about the rest of the network. This assumption is the reason for using the term 'actor-based model'. This approach to modeling is in line with the methodological approach of structural individualism (Udehn, 2002; Hedström, 2005), where actors are assumed to be purposeful and to behave subject to structural constraints. The assumption of purposeful actors is not required, however, but a question of model interpretation" (2010:).

CONCLUSION

It seems that Arrows's (2009) judgment that network analysis is superfluous has been not shared even by economists. Schweitzer and colleagues' (2009) exhortation to deal with economic networks was confirmed by the prolific development of studies on economic networks. In fact, as we have seen there is currently a dramatic growth of network-based research in almost all fields of social sciences. The idea that life and nature are made of networks seems fully acknowledged, not only from an epistemological side, but also recognizing that a methodological revolution was needed to cope with this fundamental way in which social and natural phenomena occur. Standard statistics is not enough: better said, it is not appropriate, because it deals with attributes and not relationships. It presumes - and it matches well when there is - ontological independence, while it mismatches when elements (events, cases, agents) are structurally connected. As rightly and repeatedly emphasized by social network scholars during the seventies and eighties, what unifies the various network approaches is structuralism. Conversely, what perhaps they did not stress enough is that a structuralist view requires an appropriate methodology, which cannot be standard statistics. They underlined it "only" by stressing that the core idea of network analysis lies into the properties that a single node acquires regardless of its attributes, just because of the specific position it covers into the network. Putting relations and (relative) positions at the core of analysis means presuming and acknowledging elements dependence. This view is completely different than putting at the core of analysis attributes of independent elements.

This contrast is all but trivial, because it requires an epistemological and methodological revolution in the way of approaching real or virtual phenomena and the corresponding scientific analysis. And in fact this revolution is so less trivial that standard economics employed at least fifty years more than sociology and management and organization sciences to make it. Standard economics was – and it still is in the mainstream – firmly grounded into radical methodological individualism, and thus, exactly into the view of socio-economic phenomena as constituted by independent individuals, on whom what counts is their attributes: preferences, income, capital, etc. This methodological and epistemological view is juxtaposed to the (radical) structuralism of traditional social network analysis, and incompatible with structural methodological individualism of current analytical sociology, which is adopted by network modeling. In fact, this methodological and epistemological perspective combines the two sides, and thus, it does not coincide with only one of them, especially in the standard economics radical approach. Further, since socio-economic networks are (almost all) redundant in connections – that is, they are not efficient and characterized by cycles – then three more pillars of standard economics crumble: i) the hypothesis of homogeneous agents, and hence of the representative agent; 2) the hypothesis of constant returns and proportionality between causes and effects, due to eventual shocks; 3) the hypothesis of lack of endogenous processes and changes.

The extent and depth of such theoretical changes give an idea of the relevance of the methodological and epistemological revolution that network economics is bringing into standard economics. Put in this light, it becomes not surprising and not coincidental that economics embraced network analysis so late, and the arguments that the delay depends on the scarcity of (relational) data and computational power sound more as ex-post rationalizations, functional for reducing cognitive dissonance (Festinger, 1964) rather than the main explanation. They fail to grasp the essential and crucial rationale, which lies into the substantial incompatibility of network economics with many methodological and epistemological traits of standard economics, and in a particular way with competitive equilibrium approaches, and especially general competitive equilibrium, which during last century has been mainstream economics.

If the *methodological chains of neoclassical economics* have already been significantly broken, the same cannot be said for the *theoretical chains*. In fact, network economics is still bounded into the usual theoretical framework of utility and production functions optimization (Simon, 1997), of perfect (or quasi-perfect) and substantive rationality (Gigerenzer, 2008; Gigerenzer & Selten, 2001; Kahneman *et al.*, 1982; Simon, 1962, 1997), and - among the many other aspects - the obsessive focus on equilibrium conditions (Arthur, 2006, 2010; Loasby, 1991). This theoretical framework becomes particularly strict when considering the two main streams of research in which network economics is currently engaged: that on network formation, and (even more) network optimization, which indeed is a branch of operating research. However, most recent developments (Jackson, 2014, 2015) leave hoping that, likely with the substantial help of cognitive and evolutionary economics, the "silent methodological and epistemological revolution" brought forth by network economics could further make free from neoclassical chains[72]. This is exactly the matter on which, from different perspectives, recent scientific advancements in social sciences are putting most efforts, largely based on relational methodologies[73].

Besides the analysis of network industries, the main contribution of network economics has been in its emphasis on network formation studies, which indeed was less (and almost only recently) developed by SOC and almost no developed at all by MOS applications. Into this field network economics joined MAPH. In particular, concerning some issues, like the small-world or the power-law or the structural hole or the segregation patterns, network economics produced remarkable and promising contributions, different and richer than those originally proposed by MAPH.

SOC has had the great merit to stress the crucial relevance of structuralism, with its methodological and epistemological (revolutionary) implications. Network analysis constituted this way the formal and quantitative ground and backbone of structuralism, which however was often limited to a metaphorical or just qualitative approach. The problem is that a methodology is not, *per se*, a theory. It can be a way to look at it, that is, it can be also an epistemology, and a way to gather and elaborate data on a certain problem, but this is not yet a theory about it. In this sense, economists are right to side among the ones who argue that network analysis – including SNA – is a set of tools, a methodology, and not a set of theories.

Differences between ECON and SOC or MOS network modeling remain as concerning models degree of abstraction. As claimed by Manzo (2014b), analytical sociology requires: "(1) a realistic description of the relevant micro-level entities at work…; (2) a realistic description of their properties and activities…; and (3) a realistic description of the structural interdependencies that are likely to relate these entities" (p. 18). Conversely, network economics modeling usually employs abstract hypotheses as concerning agents' behaviors and interactions. Interestingly, this high abstraction degree has been adopted also by early developments of sociological ABSM, as exemplified by the so-called KISS approach (Gilbert, 2008), which has been recently challenged by the KIDS approach (Edmonds & Moss, 2004), sided on the realistic description methodology.

In order to build network-based theories of social and economic phenomena it is necessary to work on and combine two sides: empirical research through descriptive and explicative analysis, and theory-driven and empirically-grounded models of network formation. A third approach, which is growingly employed by economists and far less by sociologists and management scholars, is that of laboratory experiments (Kagel & Roth, 1995; Plott & Smith, 2008), which have been fruitfully employed in network economics to support network formation modeling with plausible parameters. Modeling network formation and evolution appears as the natural and promising field of research in all the three SNA areas, a field that has all the requisites to facilitate cross-fertilization. Scientific knowledge growth, especially when dealing with complex phenomena, can be realized only through methodological pluralism (Mingers & Gill, 1997; Salanti & Screpanti, 1997; Samuels, 1998), which does not mean "simply"[74] that more methodologies should be admitted and promoted, but also that the corresponding results should be carefully considered and employed in a fruitful recursive interaction.

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ENDNOTES

1 Economics has developed from its very beginning a theory of industrial and consumer markets, which has been consolidated in industrial economics (and industrial organization) and in a theory of the firm after the Second World War. From the sixties, economics started also developing an organization theory, later extended to a theory of bureaucracy and public administration. On the other "side", MOS was born just as a theory of the firm and organization theory, benefiting of substantial theoretical and empirical supports from sociology and psychology. Hence, ECON and MOS are overlapped since long in the fields of the theory of the firm and organization theory. Moreover, since the beginning of eighties, MOS developed its own theories of industrial organization. Therefore, since at least 35 years, ECON and MOS are fully overlapped also into this field. For some theoretical suggestions and epistemological implications of this progressive overlapping and competition, see Biggiero (1990) and Biggiero & Laise (1998a, 1998b), and Chapter 7 and 13 in this book.

2 With this label it is meant either pure theoretical developments in mathematics and physics or its applications in natural sciences.

[3] Barabasi (2002) and Newman (2010) hint on these precursors, but for an historical deepening and the subtle distinctions between scale-free and lognormal (and other kinds of nonlinear) distributions see Caldarelli (2007), Gabaix (1999), Mitzenmacher (2011), and Newman (2005).

[4] Both Barabasi and Watts acknowledge such primacies of economists and sociologists, and their great merit is to have theoretically systematized and empirically confirmed (in an extensive and intensive ways) the two issues. Further, they offered a possible generative rule for scale-free and small-world topologies, respectively, with the preferential attachment mechanism and the link rewiring. Milgram's experiments has been replicated, extended and confirmed by Watts and his collaborators about 12 years ago (Dodds *et al.*, 2003). Historical hints and conceptual deepening on the small-world issue "from the two perspectives" of MAPH (first two) and SOC (second two) can be found in Barabasi (2002), Watts (1999, 2003), Freeman (2004), and Prell (2011).

[5] This is definitely not true if one think that during the fifties and sixties cybernetics, general systems theory, and graph theory were nurturing the development of automata studies, followed by cellular automata, and from there to artificial life and artificial societies. There are at least two key-scientists that, at that time, embodied with their own life and work the crossroads of all these research streams: one is Rapoport (1957; Solomonoff & Rapoport, 1951) and the other is von Foerster (1982, 2003). Though a deep and rich history of these two scientists and the thirty years between 1945 and 1975 should still be written, a good introduction can be found in Heims' two books (1982, 1991).

[6] See below the section on the "modelling revolution" and the Conclusions chapter of the book.

[7] Of course, other economic theories, above all neo-Marxian approaches, are completely structuralist.

[8] Further, he deals with economic networks almost exclusively from the sociology and MOS perspectives.

[9] See Nagurney (2003, 2009) for a short history.

[10] Depending on the various authors (Leinhardt, 1977; Freeman, 2004; Prell, 2011), sociology, psychology, and anthropology contend each other the early three positions, but only Prell does not neglect network economics.

[11] Biggiero (1988) applied the Lakatosian theoretical framework of "protective belt and hard kernel" (Lakatos, 1978; Lakatos & Musgrave, 1970) to neoclassical economics, and specifically to general competitive equilibrium models in the neo-walrasian version.

[12] Obviously, it is not possible to go deeply into the history of economic thought, which is so rich, long and complex. For recent contributions in this field, see previous references, and in particular Colander (2000), who reviews the history of economic thought in the light of complexity economics.

[13] Moreover, more or less explicitly, it is supposed that, being resources fixed and scarce, interests are conflicting, and thus, economy is (almost always) a zero-sum game. However, there is a florid literature also on cooperative games and non-zero-sum games.

[14] Indeed, information asymmetry is acknowledged in theorizing since long, but then, in order to prevent that is becomes too disruptive for maintaining the possibility to get – even only a partial or local – competitive equilibrium, a number of other ad hoc unrealistic hypotheses are introduced. The theoretical perspective facing with this attempt to introduce this issue within the neoclassical framework is developed into the fields of organizational economics (Milgrom & Roberts, 1992)

and industrial organization (Schmalensee & Willig, 1989; Tirole, 1988). With many implications for the issue of (network or non-network) externalities, see Shapiro & Varian (1998).

15 They are: i) perfect competition of markets and industries, that is a high number of exchangers, all of them price-taking; ii) a *given and known* distribution of preferences and resources available now and predictable for all the future states of the world; iii) a *given and known* set of technologies that are now (and will be) available for all the future states of the world; iv) constant returns to scale; v) no technological discontinuities.

16 Here I refer to the fact that, albeit claiming a strong link with the standard model, its applications to sectoral aspects, like industrial economics, international economic, etc., do not maintain all requisites of the standard model. This is due to the fact that, if they did it, they would result even more unrealistic and inapplicable of what they already are.

17 On this point see notes lvii and lix.

18 Conversely, modern views reconcile and combine the two methodological perspectives. See below Section "From juxtaposing to combining".

19 See Milrom & Roberts (1992), Schmalensee & Willig (1989) and the more recent Dietrich & Kraft (2012) for surveys on these issues.

20 This fact is still more meaningful because that journal is also very prestigious, and so ideal "to convince the reader that managing is an activity worthy of economic analysis". In the same incipit, Radner points that "this phenomenon has not escaped the attention of our colleagues in schools of business and management, where many courses are devoted to the subject". And right after he complains that "the pure science of economics, however, has been slower to focus on this phenomenon, and pure theory even slower. The picture of the firm in most economic textbooks, and in much current economic research, is still that of a unitary "entrepreneur", bent on maximizing profits". The core issue of that contribution is an investigation around the question of optimal hierarchy, whose relevance is framed by Biggiero & Mastrogiorgio in the Chapter 7 of this book.

21 Some of these arguments can be found also in Hodgson (2000) and Loasby (1999).

22 Being free from "neoclassical chains", MOS had no problem to acknowledge the network nature of firms and organizations. All organization theories, and in particular its part dealing with organization design, have been developed in the (more or less explicit) structuralist perspective. This convincement has been especially evident in the scientific literature on the structural aspects of hierarchical power. See Biggiero & Mastrogiorgio in this volume for a definition and some references to the long and rich debate.

23 For an extensive discussion see Lewis (2009), Newman (2010), and Vega-Redondo (2007).

24 An excellent way to introduce the combinations between graph theory and game theory is in Easley & Kleinberg (2010). This book drives the reader also ahead to complexity economics (Antonelli, 2011; Arrow *et al.*, 1988; Arthur *et al.*, 1997; Blume & Durlauf, 2006) and agent-based modeling (Hommes, 2013; Leitner & Wall, 2014; Teglio *et al.*, 2012; Tesfatsion & Judd, 2006). For an overview of evolving game theory, see Gintis (2000).

25 They introduced the modeling of heterogeneous players respect to values as well as the costs of forming links.

26 Here, segregation patterns mean the presence of communities, and brokers are the bridge between them. In this sense, brokers do not simply fill in structural holes within triples but rather between entire sub-networks represented by the communities.

27 Narahari *et al.* (2009) offer a good merge of the two perspectives.

[28] Not by chance, the *Review of Economic Design* hosts a lot of papers in network economics.

[29] Network neutrality is a typical topic of this research field (Economides, 1996; Economides & Hermalin, 2012; Schuett, 2010).

[30] Shy (2011) reclaims 10 years later that this definition encompasses the whole area of network economics.

[31] Overlooking some episodic contribution and the Santa Fè Institute, which has been supported by Arrow and other prestigious economists.

[32] Some nonlinear recursive function produces chaotic behaviors (Casti, 1989, 1994; Cohen & Stewart, 1995; Eve *et al.*, 1997; Kiel & Elliot, 1997; Prigogine & Stengers, 1984; Waldrop, 1992), and in this regime the relationship between variables becomes unpredictable, and thus, correlation vanishes. See also Chapter 2 of this book.

[33] For instance, into the MOS perspective of network analysis the representation of single industries or entire industrial clusters as networks dates at least 15 years before (see next section). As well, questions concerning innovation diffusion and other types of propagation processes have been addressed since long in MAPH and MOS perspectives of network analysis (Lewis, 2009; Newman, 2010; Valente, 1995, 2005; Vega-Redondo, 2007).

[34] Interestingly, a contribution on a typical theme of international economics has been recently published by two non-economists in a physics journal (Koskinen & Lomi, 2013).

[35] A crucial part of contributions to the debate on existence, uniqueness, and stability of a general competitive equilibrium – including Arrow's fundamental works (Arrow, & Hurwicz, 1958; Arrow *et al.*, 1959) – have been published in these two journals (Ingrao & Israel, 1990; Screpanti & Zamagni, 2005).

[36] This index improves Borgatti's (2003) algorithm to find key players by considering also links values and agents utilities.

[37] For instance, an author like Jackson, who is among the most open and available to draw concepts and tools from SOC, in a paper on communication networks Dutta & Jackson (2000) did not confront at all with SOC in a field like this, where it produced an intensive and extensive research.

[38] This research area is very much overlapped with that of economic geography, which in a timing parallel to MOS is progressively applying network analysis to regional (or local) innovation networks, industrial clusters, etc. And recently it is also deepening the issue of geographical (and inter-organizational) knowledge networks. However, for reason of space and (already oversize) length of this chapter, the review of this interesting literature is skipped.

[39] See Biggiero & Mastrogiorgio (Chapter 7 of this volume), who provide also a reference literature.

[40] The ponderous size of a recent handbook (Cropper *et al.*, 2008) witnesses the broad range of questions dealing with this research area.

[41] This extremely short synthesis would be enlighten by seeing Nooteboom (1999, 2004), who deepens the issue and enlarges the spectrum to the role played by many other variables and theories (else than transaction cost economics).

[42] For a smart connection of transactive memory systems theory with SNA see Monge & Contractor (2003).

[43] Interestingly, inter-organizational network dynamics has started to be investigated also with statistical network methods, namely the stochastic actor-oriented models discussed in next nection. See van de Bunt & Groenewegen (2007) and Angelini (Chapter 8 of this book).

[44] It should be said that this delay was due also to the lack of empirical data that, respect to what happens in other fields, like international trade, is not provided by public institutions. Further, into this field there are a lot of methodological problems in data collection, which are discussed in many SNA handbooks (Marsden, 2005; Frank, 2005).

[45] In particular, that sub-class of industrial clusters which is represented by industrial districts (Biggiero, 1999), because they are characterized by denser networks, more interconnected with the local social networks. On the origins of industrial districts, and the distinction between industrial clusters and industrial districts see also Belussi & Caldari (2009).

[46] With agent-based modeling there are in fact various models, among which two in this volume: see Chapter 12 (Biggiero & Basevi) and 14 (Carbonara). In both there are references to this specialized literature.

[47] As for the other variables, there are many ways to distinguish knowledge types: created, absorbed, accessed, or transferred knowledge; managerial, technological or market knowledge; knowledge depth and breadth; tacit or explicit; etc.

[48] Moreover, if they are also rivals in terms of products, then the risk to be exploited during collaboration prevent it.

[49] As noticed above, in MAPH statistical modeling this is the constant and main approach to network analysis. Besides random graph networks, later on also scale-free and small-world became fields of systematic statistical modeling (for an outline see Jackson, 2008; Lewis, 2009; Newman, 2010).

[50] For some indications about the path followed by SNA scholars to reach ERGM, see Harris, 2014; Lusher *et al.*, 2013; Prell, 2011; Wasserman & Robins, 2005. Very briefly, the main steps have been the following: i) in 1981 Holland & Leinhardt developed a model for binary directed networks, called p_1 model, to estimate the amount of reciprocation and unequal in-degree distribution; ii) in 1986 Frank & Strauss developed the first dyadic dependence model (the former were always assuming links independence), and introduced exponential distributions and Markov chains into network modeling. They added *k-star* structures as elementary configurations to the initial ones (reciprocation and unequal in-degree distribution); iii) in 1996 Wasserman & Pattison extended previous models introducing conditional probability in ties distribution, so that the probability that two or more ties do exist is usually different than that of each single existence, and they called them p^* models. In this generation of modeling, the original purposes of reciprocation, in-degree irregular distributions and *k-star* had been further extended to homophily; iv) the last generation started 10 years later (Robins *et al.*, 2007; Snijders *et al.*, 2006), extending possible model specifications and methods to avoid model degeneracy and non-convergence. From that time on, because the basic statistical distribution was maintained within the exponential random graph family, this modeling was preferably called ERGM.

[51] Indeed, after an early debate on whether it is possible to employ *any* kind of statistic, be it topological or node-attributive, now it seems that, in principle, there are no restrictions. Topological are statistics related to dyadic or local pattern or network characteristics. Dyadic statistics are those that, for instance, establish a certain probabilities of having a link if two nodes have certain features, while examples of local patterns are in- or out-k-star structures. Local patterns are called configurations, which are also called “motifs” (Ohnishi *et al.*, 2010), especially in network biology (Milo *et al.*, 2002). Network-level characteristics are reciprocity, density, degree distribution, (dis) assortativity, etc. Node-attributes can be any statistical or topological attribute. The former depend

on the ontology of the specific network: for instance, in an inter-organizational network, nodes' attributes can be its juridical form, size, etc.

52 In fact, though there are some conceptual similarities, ERGM estimating methods cannot be standard logistic regression procedures, because they assume independent observations.

53 Notice that in a truly realistic (and complex) model nodal attribute could change according to nodal position within network topology.

54 These appeared often in the journal *Social Networks*, especially those with a methodological content.

55 When *k* can be even more than 2.

56 To know more on SAOM theoretical and methodological aspects, and to follow and application, see Angelini's Chapter 8 in this volume.

57 Even though the model is designed to optimize actor's behavior, the model allows implementing also different algorithms. Seeking multiple objectives determined by more than one specification, it could happen that, if some objective-criteria are genuinely – that is, (onto)logically – independent, then standard optimization cannot be applied (Biggiero & Laise, 2003). See also Note lix.

58 Indeed, it seems that Snijders brought into ERGM what he developed for SAOM. Likely, between the two modeling approaches it could be shared much more than what was thought in the pioneering stage of twenty years ago.

59 As underlined by Angelini (chap. 8, this volume): "Actors are "memoryless" for Markov's chains assume that the next state only depends on the actual state and not on the sequence of events that preceded it. Moreover they are strategically myopic for they are not able to imagine conjectures about the countermoves of the other nodes and they cannot ally or coordinate their behaviors. On the other side they are omniscient on the relational dimension because they perfectly know the state of the networks; that is all the nodes and their connections. Obviously, such an assumption is hard to be sustained for large networks, in those cases the interpretation of the model results should be aware of it". Hence, SAOM current version is rather unrealistic as concerning actor's behavior – in this respect it is a rather neoclassical approach, because actors, albeit heterogeneous in terms of attributes, have the same objective function that consists in maximizing their utility. Moreover, agents have no computational limits and resolve possible conflicts inside their objective function through the standard multicriteria utility function. That is, they implicitly assume that among all specifications there are trade-offs, and thus, substitutability coefficients, which allow to express a given specification in terms of another one. In other words, the weight of a certain parameter is reduced in favor of another one. Specifications are like criteria into a multi-criteria objective function. (The same mechanism is employed also by ERGM.) However, if some specifications were truly independent, then a multi-criteria (or multi-specifications) objective function could not be built, and hence specifications parameters could not be varied as in a trade-off relationship. Consequently, and consistently with cognitive and evolutionary economics, agents had to be satisfiers and not maximizers, and instead of optimizing utility functions actors had to employ other heuristic algorithms, among which outranking methods (Bouyussou, 2001; Bouyussou *et al.*, 2000) are the most appropriate even though not the most practiced. To go deeper into these issues, see Biggiero & Laise (2003a, 2003b, 2007).

60 SAOM have been initially designed to study networks of individuals, but then van de Bunt & Groenewegen (2007) and Angelini (see Chapter 8 of this book) applied them to interorganizational

networks. However, the fundamental SAOM methodological requirement of assuming nodes' behavior as driven by utility functions do remain.

61 Though not into the field of economic networks, Jackson & Rogers (2007) have created a very interesting model of network formation, which is rather simple and at the same time so powerful to generate all typical features of many social networks: small-wordliness, power-lawness, assortativity, and a negative relationship between a node's degree centrality and clusterization. Remarkably, network economics became so interesting for economics journals that such a *social* network model has been published in a leading journal of economics.

62 Austrian economics has been among the branches of economic theory that has given more emphasis on endogeneity, either in the form of self-organizing mechanisms to optimize resource allocation in competitive markets or in the form of innovation processes. Von Hayek and Schumpeter are the prominent scientists that stressed and developed these two issues, respectively. However, until the reformulations of their ideas into evolutionary economics (Dopfer, 2005; England, 1994; Foster, 1997; Hodgson, 2000; Kirman, 2011; Loasby, 1991; Witt, 1993), both issues were neither formalized nor modelled.

63 And it is just on this aspect that points some of the criticisms raised by Newman (2010) and Jackson (2010).

64 Many SNFM do not take into account path dependency, because they follows the "auction logic", which means that there is a sort of "virtual adaptation" while looking for equilibrium. In other models there is still such a powerful rationality that agents can evaluate the rational choices of all others and make the right (optimal) decision. In other SFNM there is path dependency in the form of Markov chains, as in SAOM.

65 On many aspects of path dependence see Garud & Karnøe (2001).

66 Here I mean that most social or economic networks are multiplex, that is its nodes are interconnected through more than one – usually many – type of relationship. And generally, the different types of relationship interact between themselves and with actors. For instance, in an inter-firm network representing an industrial cluster, firms are connected through trade relationships, friendship relationships, knowledge-exchange relationships, union relationships, labor mobility relationships, etc. And friendship relationships can likely influence trade relationships: link-link interactions. Moreover, an eventual change of position into the trade network could – and likely will - induce a different behavior into future link formations in that or in other topologies corresponding to the other dimensions (friendship, knowledge, etc.). see more in the Conclusions chapter of the book.

67 In the Conclusions chapter of this book, a comparison between the four relational methodologies under nine evaluation criteria is provided.

68 ABSM criticisms to neoclassical economics have been addressed by Gallegati & Richiardi (2009) and Moss (2009). The impact of relational methodologies on neoclassical economics is revisited also in the Conclusions chapter of this book.

69 Among the set of hyper-rationalist we can enroll most economists and also a large group of management scholars, while on the other side many sociologists and psychologists, and a significant group of organization scholars.

70 Another case of strile juxtaposition is that between epistemological realism and relativism, which have been combined and reconciled by pragmatism (Hack & Lane, 2006; Putnam, 1995; Rorty, 1979, 1982). For an approach to MOS inspired to epistemological pragmatism, see Biggiero (2001, 2009, 2012), Tsoukas (2005).

[71] For more on this issue, see also the Conclusions chapter at the end of the book.

[72] An excellent view of what a “new microeconomics” could become once acknowledging the various forms of social complexity – evolution, interaction, cognition, and institutions – is presented by Elsner (2012).

[73] Besides the works already recalled, the following are still noteworthy to mention: Edmonds & Meyer (2013), Jörg (2011).

[74] Indeed, this acknowledgment is all but simple, because entire theoretical frameworks – and even paradigms – are mono-methodological, that is encapsulated into a single methodological approach.