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Postface: critically examining complex networks science

Although we have reviewed or mentioned more than 600 scientific works on equilibrium and non-equilibrium processes in complex networks, the present book is by no means intended as an exhaustive account of all research activities in network science. It would have been extremely easy to list more than twice as many scientific papers and, as we have stressed in several passages of this book, we have decided to focus our attention on that subset of network research that deals with dynamical processes in large-scale networks characterized by complex features such as large-scale fluctuations and heterogeneities. This implies the selection of studies and approaches tailored to tackle the large-scale properties and asymptotic behavior of phenomena and, as a consequence, models that usually trade details and realism for generality and a high level of abstraction. This corresponds to a methodological approach that has its roots in statistical physics and has greatly contributed to generate an entire research area labeled in recent years as the "new science of networks" (Watts, 2004). As network science is an interdisciplinary endeavor, however, we do not want to overlook the criticisms raised by several authors to this "new science of networks" and to the statistical physics and complex systems approaches. These criticisms are articulated around several key points that are echoed in several commentaries and essays (see for instance Urry [2004], Keller [2005] and Mitzenmacher [2006]), and at the end of 12 long chapters we believe we are obliged to provide a discussion of some of these key points.

One of the criticisms often raised is that the "new science of networks" is not new at all; that is, most of the papers considering complex networks are just reinventing the wheel. This criticism is rooted in the fact that the results produced by complex network science borrow heavily from previous works and sometimes recover results implicitly contained in those works. On the other hand the new generation research is based on a truly interdisciplinary approach that benefits from unprecedented computing power and data and allows the use of computational and analytical techniques which were not available in the past. For instance, the

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intrusion of physics techniques and methodology, as with all cross-fertilization of research areas, is producing truly novel approaches to old problems. In some cases this corresponds to conceptually new results and understanding. In many other cases, the innovation lies in the approach itself. Statistical physics has long dealt with the mathematical modeling of very complicated systems, acquiring vast experience in isolating the main ingredients and performing approximations to allow the understanding of complex problems without resorting to rigorous mathematical proofs. Such proofs may be very elegant but are often out of reach of all but a few mathematically gifted minds. Physics is in many cases the art of approximation and often uses the quick and dirty approach which has the advantage of lying within reach of a much larger audience. In recent years the use of numerical simulations and agent-based modeling, in which physicists are at the forefront of research and implementation, is also helping to elucidate concepts and phenomena which sometimes hide behind the veil of formal mathematics. The immediacy of concepts and the possibility of easy manipulation of models are extremely important.

The critics of complex networks science also address a number of technical points that should be carefully scrutinized. A recurrent criticism found in the literature focuses on the poor evidence for power-law behavior and the fact that most of the so-called “power laws” would fail more rigorous statistical tests. As we discussed in Chapter 2, this is technically true in many cases, since most of the real world networks exhibit upper and lower cut-offs, offsets, different functional regimes and all the fluctuations due to the dirt and dust of real-world phenomena. In strict mathematical and statistical terms it is therefore correct to state that most of the claimed power-laws are actually just an approximation of the actual data. In addition, careful statistical tests may be relevant in order to identify and discriminate other possible functional forms. On the other hand, an over-zealous approach to “fitting” loses the focus on the main conceptual issue. Most real-world networks differ from the Poissonian graphs we are used to considering as zeroth order approximations. They are skewed, heterogeneous, heavy-tailed networks with impressive levels of statistical fluctuations. Their modeling with power laws is a better approximation than a Poissonian random graph but nonetheless an approximation. Lengthy debates on how accurate an exponent is, if at all measurable, are therefore a kind of academic exercise that misses the crucial point: the real world is in many cases better approximated by heavy-tailed graphs. Furthermore, many of the small and large-scale deviations from the power-law behavior are in fact predicted in the more detailed models, thus becoming a confirmation of the overall heavy-tailed nature of the system. The power law should then be viewed simply as a conceptual modeling framework that allows the analytical estimation of the effect of fluctuations on dynamical processes. We should always keep in mind that this is an approximation, but also that, if physicists had started to criticize the theory of electronic bands in crystals by saying that actually most real materials are much more complicated, we would still have computers working with valves instead of microchips.

Another major criticism is that the scale-free ideas and the quest for universal laws are a physicist's obsession that cannot apply to network science which deals with a variety of systems governed by very different dynamical processes. Well, that is true, and all knowledgeable physicists would agree on that. This criticism indeed comes from a general misconception, and also often from a superficial study of the field of phase transition by scientists with non-physics training. Physicists know extremely well that the universality of some statistical laws is not to be confused with the "equivalence of systems", and we have already warned against this confusion in Chapters 3 and 5. Systems sharing the same universal critical properties have at the same time enormous macroscopic differences. The scaling exponents of a ferromagnetic system and a lattice gas at the critical point may be the same, but any other quantities such as density or temperature at the transition are completely different and their calculation requires the inclusion of the system details. Even the evaluation of scaling and critical exponents generally needs to take into account some specific features of the system. A non-physicist will find it hard to believe, but the Ising model so widely used as the prototype for phase transitions does not work for real-world ferromagnets. The quantum nature of spins and other properties that alter the basic symmetries of real systems have demanded the development of more refined models such as the Heisenberg model, the XY model and many others, all of them having different critical behavior. What, then, is the origin of the success of the Ising model and universal phase transitions? It lies in its conceptual power. To calculate the critical behavior and the scaling of a system we need to focus on basic symmetries and interactions, which are responsible for the large-scale statistical behavior. The concept of universality allows the discrimination of what is relevant and what is superfluous in the determination of a specific set of statistical properties, defining general classes of the system's large-scale behavior. This does not imply that these are the sole relevant properties of the system. The system diversity indeed may be contained in features that are not relevant for the large-scale scaling properties but are crucial for the other properties. In many cases, outside the physics community, the ideas of scaling behavior and the mechanisms ruling its emergence in complex networks are equivocated with the claim that "all networks are equal in the eyes of the physicist." This is far from reality, as testified by the fact that while other scientific communities are writing dozens of papers to discredit the BA model as a realistic model for a specific system, unknown to them physicists have already written hundreds of papers in which different ingredients and dynamics more specifically devised for different systems are developed. The value of the BA model, like that of the Ising model, is purely conceptual. No one considers the Ising model as a realistic candidate for most physical systems, and so it should be with the BA and the Watts–Strogatz models. For instance, the various extensions of the BA model developed for specific systems are surely more appropriate than the original BA model itself. At the same time, the study of conceptual models is well worth while, since they represent the guinea pigs on which the theoretician is supposed to test ideas and achieve understanding before moving to more realistic situations.

The issue of engineering versus self-organization represents still another point of discussion. As we discussed in some of the previous chapters, engineering and function play a crucial role in the shaping of networks and dynamical processes. Notwithstanding engineering, even networks which are human artifacts do not have a blueprint above a certain scale. The Internet, for instance, is clearly blueprinted at the level of Local Area Networks and large Autonomous Systems. It is also constrained by technical, economic, and geographical features which are very specific and to some extent time-dependent. The large-scale structure, however, is not planned and is surely the outcome of a self-organization process. The large-scale statistical approach is therefore not in conflict with the detailed engineering approach. On the contrary, the two approaches deal with different scales and features of the problem, and a final understanding of the internet structure will result only from the combination of the two. This combination is crucial but unlikely to happen before the two approaches have been extensively exploited at their respective scales and all their facets mastered.

In this book we have focused mostly on approaches dealing with the large-scale and general properties of systems by using stylized models. There is no doubt that a complete understanding of networked structure requires diving into the specifics of each system by adopting a domain specific perspective. We believe, however, that domain specialists have a lot to gain from the general approach and the "new science of complex networks." The models presented are amenable to many refinements and the analytical techniques used can be easily exported. New observables and measurements can be suggested. The interplay of the micro-level with the macro-level is a major question in most of the systems we are facing nowadays and the approach to complex networks presented here can provide basic insights into this issue. We hope that this book will contribute to a bidirectional exchange of ideas, eventually leading to a truly interdisciplinary approach to network science.