The network takeover

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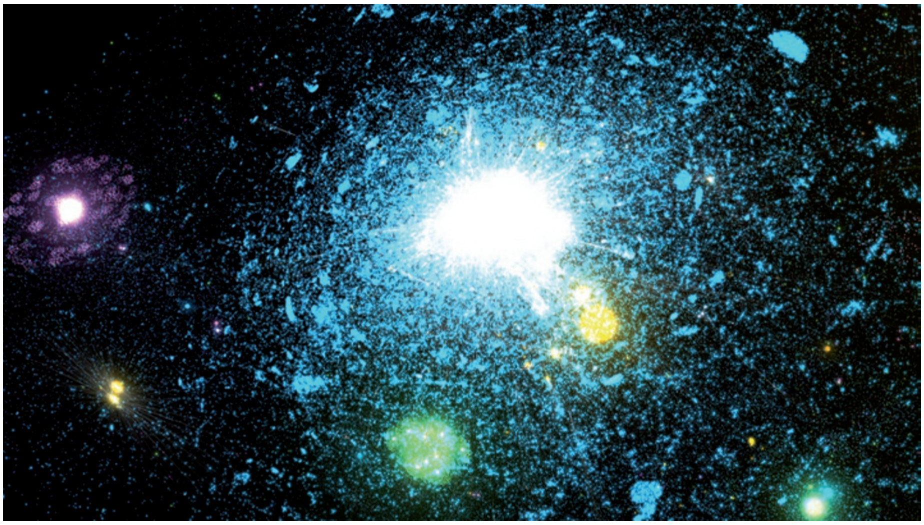
Reductionism, as a paradigm, is expired, and complexity, as a field, is tired. Data-based mathematical models of complex systems are offering a fresh perspective, rapidly developing into a new discipline: network science.

Reports of the death of reductionism are greatly exaggerated. It is so ingrained in our thinking that if one day some magical force should make us all forget it, we would promptly have to reinvent it. The real worry is not with reductionism, which, as a paradigm and tool, is rather useful. It is necessary, but no longer sufficient. But, weighing up better ideas, it became a burden.

"You never want a serious crisis to go to waste," Ralph Emmanuel, at that time Obama's chief of staff, famously proclaimed in November 2008, at the height of the financial meltdown. Indeed, forced by an imminent need to go beyond reductionism, a new network-based paradigm is emerging that is taking science by storm. It relies on datasets that are inherently incomplete and noisy. It builds on a set of sharp tools, developed during the past decade, that seem to be just as useful in search engines as in cell biology. It is making a real impact from science to industry. Along the way it points to a new way to handle a century-old problem: complexity.

A better understanding of the pieces cannot solve the difficulties that many research fields currently face, from cell biology to software design. There is no 'cancer gene'. A typical cancer patient has mutations in a few dozen of about 300 genes, an elusive combinatorial problem whose complexity is increasingly a worry to the medical community. No single regulation can legislate away the economic malady that is slowly eating at our wealth. It is the web of diverging financial and political interests that makes policy so difficult to implement. Consciousness cannot be reduced to a single neuron. It is an emergent property that engages billions of synapses. In fact, the more we know about the workings of individual genes, banks or neurons, the less we understand the system as a whole. Consequently, an increasing number of the big questions of contemporary science are rooted in the same problem: we hit the limits of reductionism. No need to mount a defence of it. Instead, we need to tackle the real question in front of us: complexity.



Network universe. A visualization of the first large-scale network explicitly mapped out to explore the large-scale structure of real networks. The map was generated in 1999 and represents a small portion of the World Wide Web[11]; this map has led to the discovery of scale-free networks. Nodes are web documents; links correspond to URLs. Visualization by Mauro Martino, Alec Pawling and Chaoming Song.

The complexity argument is by no means new. It has re-emerged repeatedly during the past decades. The fact that it is still fresh underlines the lack of progress achieved so far. It also stays with us for good reason: complexity research is a thorny undertaking. First, its goals are easily confusing to the outsider. What does it aim to address — the origins of social order, biological complexity or economic interconnectedness? Second, decades of research on complexity were driven by big, sweeping theoretical ideas, inspired by toy models and differential equations that ultimately failed to deliver. Think synergetics and its slave modes; think chaos theory, ultimately telling us more about unpredictability than how to predict nonlinear systems; think self-organized criticality, a sweeping collection of scaling ideas squeezed into a sand pile; think fractals, hailed once as the source of all answers to the problems of pattern formation. We learned a lot, but achieved little: our tools failed to keep up with the shifting challenges that complex systems pose. Third, there is a looming methodological question: what should a theory of complexity deliver? A new Maxwellian formula, condensing into a set of elegant equations every ill that science faces today? Or a new uncertainty principle, encoding what we can and what we can't do in complex systems? Finally, who owns the science of complexity? Physics? Engineering? Biology, mathematics, computer science? All of the above? Anyone?

These questions have resisted answers for decades. Yet something has changed in the past few years. The driving force behind this change can be condensed into a single word: data. Fuelled by cheap sensors and high-throughput technologies, the data explosion that we witness today, from social media to cell biology, is offering unparalleled opportunities to document the inner workings of many complex systems. Microarray and proteomic tools offer us the simultaneous activity of all human genes and proteins; mobile-phone records capture the communication and mobility patterns of whole countries[1]; import–export and stock data condense economic activity into easily accessible databases[2]. As scientists sift through these mountains of data, we are witnessing an increasing awareness that if we are to tackle complexity, the tools to do so are being born right now, in front of our eyes. The field that benefited most from this data windfall is often called network theory, and it is fundamentally reshaping our approach to complexity.

Born at the twilight of the twentieth century, network theory aims to understand the origins and characteristics of networks that hold together the components in various complex systems. By simultaneously looking at the World Wide Web and genetic networks, Internet and social systems, it led to the discovery that despite the many differences in the nature of the nodes and the interactions between them, the networks behind most complex systems are governed by a series of fundamental laws that determine and limit their behaviour.

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On the surface, network theory is prone to the failings of its predecessors. It has its own big ideas, from scale-free networks to the theory of network evolution[3], from community formation[4,5] to dynamics on networks[6]. But there is a defining difference. These ideas have not been gleaned from toy models or mathematical anomalies. They are based on data and meticulous observations. The theory of evolving networks was motivated by extensive empirical evidence documenting the scale-free nature of the degree distribution, from the cell to the World Wide Web; the formalism behind degree correlations was preceded by data documenting correlations on the Internet and on cellular maps[7,8]; the extensive theoretical work on spreading processes was preceded by decades of meticulous data collection on the spread of viruses and fads, gaining a proper theoretical footing in the network context[6]. This data-inspired methodology is an important shift compared with earlier takes on complex systems. Indeed, in a survey of the ten most influential papers in complexity, it will be difficult to find one that builds directly on experimental data. In contrast, among the ten most cited papers in network theory, you will be hard pressed to find one that does not directly rely on empirical evidence.

With its deep empirical basis and its host of analytical and algorithmic tools, today network theory is indispensible in the study of complex systems. We will never understand the workings of a cell if we ignore the intricate networks through which its proteins and metabolites interact with each other. We will never foresee economic meltdowns unless we map out the web of indebtedness that characterizes the financial system. These profound changes in complexity research echo major economic and social shifts. The economic giants of our era are no longer carmakers and oil producers, but the companies that build, manage or fuel our networks: Cisco, Google, Facebook, Apple or Twitter. Consequently, during the past decade, question by question and system by system, network science has hijacked complexity research. Reductionism deconstructed complex systems, bringing us a theory of individual nodes and links. Network theory is painstakingly reassembling them, helping us to see the whole again. One thing is increasingly clear: no theory of the cell, of social media or of the Internet can ignore the profound network effects that their interconnectedness cause. Therefore, if we are ever to have a theory of complexity, it will sit on the shoulders of network theory.

The daunting reality of complexity research is that the problems it tackles are so diverse that no single theory can satisfy all needs. The expectations of social scientists for a theory of social complexity are quite different from the questions posed by biologists as they seek to uncover the phenotypic heterogeneity of cardiovascular disease. We may, however, follow in the footsteps of Steve Jobs, who once insisted that it is not the consumer's job to know what they want. It is our job, those of us working on the mathematical theory of complex systems, to define the science of the complex. Although no theory can satisfy all needs, what we can strive for is a broad framework within which most needs can be addressed.

The twentieth century has witnessed the birth of such a sweeping, enabling framework: quantum mechanics. Many advances of the century, from electronics to astrophysics, from nuclear energy to quantum computation, were built on the theoretical foundations that it offered. In the twenty-first century, network theory is emerging as its worthy successor: it is building a theoretical and algorithmic framework that is energizing many research fields, and it is closely followed by many industries. As network theory develops its mathematical and intellectual core, it is becoming an indispensible platform for science, business and security, helping to discover new drug targets, delivering Facebook's latest algorithms and aiding the efforts to halt terrorism.

Who owns the science of complexity?

As physicists, we cannot avoid the elephant in the room: what is the role of physics in this journey? We physicists do not have an excellent track record in investing in our future. For decades, we forced astronomers into separate departments, under the slogan: it is not physics. Now we bestow on them our highest awards, such as last year's Nobel Prize. For decades we resisted biological physics, exiling our brightest colleagues to medical schools. Along the way we missed out on the bio-revolution, bypassing the financial windfall that the National Institutes of Health bestowed on biological complexity, proudly shrinking our physics departments instead. We let materials science be taken over by engineering schools just when the science had matured enough to be truly lucrative. Old reflexes never die, making many now wonder whether network science is truly physics. The answer is obvious: it is much bigger than physics. Yet physics is deeply entangled with it: the Institute for Scientific Information (ISI) highlighted two network papers[3,9] among the ten most cited physics papers of the past decade, and in about a year Chandrashekhar's 1945 tome, which has been the most cited paper in *Review of Modern Physics* for decades, will be dethroned by a decade-old paper on network theory[10]. Physics has as much to offer to this journey as it has to benefit from it.

Although physics has owned complexity research for many decades, it is not without competition any longer. Computer science, fuelled by its poster progenies, such as Google or Facebook, is mounting a successful attack on complexity, fuelled by the conviction that a sufficiently fast algorithm can tackle any problem, no matter how complex. This confidence has prompted the US Directorate for Computer and Information Science and Engineering to establish the first network-science programme within the US National Science Foundation. Bioinformatics, with its rich resources backed by the National Institutes of Health, is pushing from a different direction, aiming to quantify biological complexity. Complexity and network science need both the intellectual and financial resources that different communities can muster. But as the field enters the spotlight, physics must assert its engagement if it wants to continue to be present at the table.

As I follow the debate surrounding the faster-than-light neutrinos, I wish deep down for it to be true. Physics needs the shot in the arm that such a development could deliver. Our children no longer want to become physicists and astronauts. They want to invent the next Facebook instead. Short of that, they are happy to land a job at Google. They don't talk quanta — they dream bits. They don't see entanglement but recognize with ease nodes and links. As complexity takes a driving seat in science, engineering and business, we physicists cannot afford to sit on the sidelines. We helped to create it. We owned it for decades. We must learn to take pride in it. And this means, as our forerunners did a century ago with quantum mechanics, that we must invest in it and take it to its conclusion. ❐

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