# 5 SLOPPY DATA FLOODS OR PRECISE SOCIAL SCIENCE METHODOLOGIES? DILEMMAS IN THE TRANSITION TO DATA-INTENSIVE RESEARCH IN SOCIOLOGY AND ECONOMICS

CLEMENT LEVALLOIS, STEPHANIE STEINMETZ, AND PAUL WOUTERS

With enough data, the numbers speak for themselves.

—Chris Anderson (2008)

What are the implications of the emergence of new data sources for scientific and scholarly research? In 2008 this question was provocatively raised in an article by Chris Anderson, editor in chief of the journal *Wired*. He claims that scientists need no longer rely on hypothesis or experimentation. Increasingly, we will all be children of "The Petabyte age." According to Anderson, "at the petabyte scale, information is not a matter of simple three- and four-dimensional taxonomy and order, but of dimensionally agnostic statistics." This calls for "an entirely different approach," one that no longer aims for visualization of data in their entirety. Anderson pleads the case for approaching data streams mathematically first, and establishing a context for them later, rather than starting with trying to understand the data. In short, according to Anderson, we no longer need theory, at least not in the traditional sense of the word. "Out with every theory of human behavior, from linguistics to sociology. Forget taxonomy, ontology, and psychology. Who knows why people do what they do? The point is they do it, and we can track and measure it with unprecedented fidelity. With enough data, the numbers speak for themselves."

Anderson has accurately captured an emerging enthusiasm for data floods. Although his article (to which many have responded in print and on the Web) exaggerates the actual developments in some ways, the basic argument is highly relevant, particularly in the case of computational research.

For example, in March 2009 the journal *Science* published a similar article by computer scientists from Microsoft Research and Johns Hopkins University, titled "Computer science: Beyond the data deluge." In it, Gordon Bell, Tony Hey, and Alex Szalay (2009, 1297) argue that "today, some areas of science are facing hundred- to thousand-fold increases in data volumes from satellites, telescopes, high throughput instruments, sensor networks, accelerators, and supercomputers, compared to the volumes generated only a decade ago." This increase in the amount of data is, according to Bell et al., a serious challenge to a variety of disciplines. Not merely an incremental change, it represents a new research paradigm. According to Gray and Szalay (2007), this new "fourth paradigm" is supposed to succeed three older paradigms: the experimental, theoretical, and simulation paradigms. Within computer science it means that the term *e-science* is not primarily concerned with faster computation, but with more advanced database technologies. Astronomy was the first field to make the shift to data-intensive research by collecting all observational data in a global data infrastructure accessible through Web services.[1] High-energy physics, genomics, and oceanography are expected to follow. According to the Microsoft Research team, "data-intensive science will be integral to many future scientific endeavors, but demands specialized skills and analysis tools" (Bell et al. 2009, 1298). The examples they discuss are all based on data-producing technologies, such as digital telescopes, sensors, and particle accelerators.

The postulate of a "fourth paradigm" is further elaborated in a book titled *The Fourth Paradigm: Data-Intensive Scientific Discovery*, edited by Hey, Tansley, and Tolle (2009). The main argument of *The Fourth Paradigm* is that "if we are to achieve *dramatic* breakthroughs, new approaches will be required. We need to embrace the next, fourth paradigm of science." (Mundie 2009, 223) This collection of essays, co-authored by computer scientists and "domain scientists" in the areas of environmental and health research, spells out the implications of large datasets for scientific research and for research infrastructures. It is based on work by the late Jim Gray, a computer scientist who is celebrated as a visionary. Gray defines the fourth paradigm as follows:

> The world of science has changed, and there is no question about this. The new model is for the data to be captured by instruments or generated by simulations before being processed by software and for the resulting information or knowledge to be stored in computers. Scientists only get to look at their data fairly late in this

pipeline. The techniques and technologies for such data-intensive science are so different that it is worth distinguishing data-intensive science from computational science as a new, *fourth paradigm* for scientific exploration. (Gray, cited in Hey, Tansley, and Tolle 2009, xix)

According to Gray, we are seeing the evolution of two branches in every discipline: a computational branch and a data-processing branch. For example, in ecology there is now "both computational ecology, which is to do with simulating ecologies, and eco-informatics, which is to do with collecting and analyzing ecological information" (xix).

How will the social sciences be affected by these developments? This chapter aims to contribute to a better understanding of the implications of data-intensive and computational research methodologies for the social sciences by focusing on two social science fields: sociology and economics. We address the implications of this debate for sociology and economics by uncovering what is at stake here. Although different kinds of "new data" are collected by both disciplines (transactional versus brain data), they serve as good examples to demonstrate how disciplines are responding to the availability of new data sources. Moreover, contrasting the two fields provides insight into the subtle but relevant differences among various forms of data-intensive research. This comparative study also shows how the notion of "data-intensive research" is actually defined. It is clear that there is no consensus as to when data are massive or complex enough to count as the basis for data-intensive research. A dataset of a few terabytes would certainly count as huge in sociology, but less so in astronomy. As a preliminary definition, we propose to define *data-intensive research* as research that requires radical changes in the discipline. If new, possibly more standardized and technology-intensive ways to store, annotate, and share data are needed, we see this as a case of "data-intensive research." This means that the concept may point toward quite different research practices and computational tools.

We address the following questions for each discipline: Who is making claims on behalf of new sources of data, and what kind of evidence is presented in each case? Are these instances framed as opportunities, or as threats? What kind of resistance, if any, arises from among the challenged orthodoxies? What kinds of questions are these different approaches seeking to answer? What implications can be drawn concerning the conduct of research in, and the societal role of, the social sciences and the humanities?

In 2007, two prominent British sociologists, Mike Savage and Roger Burrows, raised the alarm in their article "the coming crisis of empirical sociology" in *Sociology*, the flagship journal of their profession, published by the British Sociological Association (Savage and Burrows 2007; see also Savage and Burrows 2009). They argue that "transactional data," the data generated by the daily use of networked communication systems by millions of people, will render the survey methodology—one of the cornerstones of the field of sociology—increasingly obsolete. In its place, research groups in private companies have been able to circumvent the survey and are currently exploiting the huge data flow of their companies' operations. This enables them to analyze exhaustive population data without the need for sampling techniques. How does this fit with the data-flood revolution announced in computing and natural science? Who are the new actors in this empirical challenge to sociology, and to what extent is academic sociology really obliged to adapt to this new type of data-driven research?

Basically the same questions arise in economics, which is challenged by yet another radically new type of data: neuroimaging data. Neuroeconomists use functional magnetic resonance imaging (fMRI) scans to provide a new kind of empirical evidence concerning economic decisions. Particular types of behavior can be related to the activation of specific brain areas, leading some to suggest that a fundamental level of explanation has been achieved. Why do we behave as we do, and how? Once measured, emotional states and drivers of decision making provide a more realistic account of behavior, which theoretical constructs in economics have not yet been able to create. Economics has long been ambivalent about the empirical relevance of its sophisticated mathematical theories. These doubts re-emerged when it became clear that the 2008 financial crisis had taken most economists completely by surprise. The emerging subfield of neuroeconomics, which brings together economists, psychologists, and neuroscientists, aims to identify the neurological foundations of human decisions in order to improve the empirical validity of the theoretical decision-making models. As is the case in the debate within sociology, it remains to be seen whether the importing of new datasets will have a significant impact on how academic science is conducted, and on its boundaries. This is the central question explored in this chapter.

We start by re-contextualizing the empirical tradition in sociology and economics. Both disciplines have routinely dealt with relatively large datasets and, indeed, created them. We take this as the background for a critical re-evaluation of the claims that data-intensive research represents a radical novelty, threatening theoretically oriented social sciences. How are the new datasets different from the existing empirical material available in sociology and economics? We then present the responses within the scientific communities. If voices oppose this predicted trend, what is their counter-proposal, and from which corners of sociology and economics do they originate? Here, clear differences are highlighted between the two cases, showing that it is indeed necessary to go beyond the simple observation of an era of data-intensive research and to assess each case in terms of its particular historical dynamic and balance of internal intellectual currents. Moving away from the (often confrontational) responses to new forms of data-intensive research, we try to achieve a more balanced view of the implications for the two disciplines. Is the "end of theory" a likely outcome? What (if any) reconfigurations of the fields can be expected in terms of boundaries, expertise, and institutions? Finally, we reflect on a possible alternative scenario which can be discerned in economics as well as in sociology. In this scenario, theoretical reasoning will not be "pushed aside" by the ascendance of data-driven research. Rather, theory work will prosper from the availability of a greater amount of raw data. In the concluding section, we reappraise the lessons that can be learned by the social sciences.

## NEW FORMS OF DATA COLLECTION AND ANALYSIS IN SOCIOLOGY

First, we examine whether technological innovation and increasing possibilities to collect vast amounts of data render obsolete theoretical and methodological foundations in empirical sociology. As was mentioned at the beginning of this chapter, this topic has been addressed by Savage and Burrows (2007), who posit a "coming crisis of empirical sociology," and by Anderson (2008), who developed the "end of theory" scenario. Both articles have led to debate in scholarly journals and Internet forums.[2] However, as sociology can hardly be considered a unified field, answering the question of whether theory is becoming obsolete is a challenging task. A look at the diverse topics and methods addressed by the 55 research committees of the International Sociological Association in 2011 throws light on how differently the question might be answered even within those groups. Against this background, a reflection on the empirical tradition in sociology is required to obtain a deeper understanding of the implications of new data forms.

## THE EMPIRICAL TRADITION IN SOCIOLOGY: BACKGROUND

At the end of the nineteenth century, sociology emerged as an academic response to the challenges presented by industrialization and rationalization. During this phase of "becoming a science" (Zald 1991), the dominant methodological approach was to treat the discipline in broadly the same manner as natural sciences. An emphasis on empiricism (positivism) served to provide the foundation for sociological claims and, more important, to distinguish sociology from less empirical fields, such as philosophy (Comte 1848; Durkheim 1895). This prospect of empirical social analysis was questioned by various intellectuals. Karl Marx rejected Comtean positivism in favor of dialectic analysis, arguing that appearances ought to be critically examined rather than simply documented. Nevertheless, Marx endeavored to produce a science of society grounded in the economic determinism of historical materialism (Marx 1847). At the turn of the twentieth century, the first wave of German sociologists, including Max Weber and Georg Simmel, introduced sociological anti-positivism, arguing that the appropriate objective for sociological endeavor should be a hermeneutic (interpretative) understanding, rather than law-like generalization. That methodological paradigm formed and shaped the discipline for several decades and led broadly to two contrasting models of sociological research and theorizing: the positivist/quantitative and the interpretative/qualitative.[3] The positivist tradition reached its peak of popularity in the "quantitative revolution" between the 1950s and the 1960s, whereas the qualitative tradition became less important in that period and began to regain recognition in the 1970s. Even though some researchers have proclaimed an end to this paradigmatic quarrel, it can hardly be denied that it still is the main divide within the discipline.[4]

Empirical sociology has an even longer history.[5] Agricultural and trade statistics, as well as censuses, were already developed in ancient China, Egypt, and the Roman Empire (Schnell et al. 1995). The first inquiries of empirical social research are primarily socio-graphic rather than sociological. Usually, their main purpose was to increase bureaucratic efficiency (Bethlehem 2009).[6] Political arithmetic, founded by John Graunt, William Perry, and Edmund Halley (Diekmann 2008), gave a strong impulse to empirical social research and statistics in Great Britain. It can be seen as the forerunner of quantitative analysis of social phenomena as the basis for causal explanation of social conditions. Political arithmetic dominated statistical thinking up to the beginning of the nineteenth century. Gradually it turned into a new field of social statistics ("social calculus"). From 1830 to 1849, in the so-called "era of enthusiasm" (Westergaard 1932), the foundations for modern social statistics were laid. Central statistical bureaus, statistical societies, conferences, and statistical journals were established soon after that period (Kern 1982; Diekmann 2008).

To summarize, the early development of empirical social research was mostly independent of the growth of academic sociology. The founding of several institutes for empirical social research, the founding of market-research and opinion-research institutes, and the founding of research institutions related to evaluation and policy research were important developments.[7] As sociological research became increasingly employed as a tool by governments and businesses, new types of quantitative and qualitative research methods were developed in the second half of the twentieth century, displacing (and often delegitimizing) the older forms of empirical research. Particularly at a time when social data could not be easily collected, stored, and manipulated (Brunt 2001; Bulmer et al. 1991; Osborne and Rose 1999, 2004), the new generation of empirical sociologists perceived themselves as methodological innovators as a result of the invention of the sample survey, the early adoption of the principles of statistical inference, and the codification of the in-depth interview. Because this still-dominant form of empirical sociology has been challenged by "data-driven research," we focus our analysis on that aspect of the discipline in particular.

## IS THERE A CRISIS, AND WHO IS AFFECTED?

Savage and Burrows (2007) make claims about a "coming crisis of empirical sociology" and a radical change in the significance of empirical research. They argue that empirical sociologists, instead of keeping pace with the latest developments in the online environment, are beginning to lose their "innovative" role in collecting and analyzing social data. As they frame it, empirical sociologists "occupy an increasingly marginal position in the huge research infrastructure that forms an integral feature of what Thrift (2005) characterizes as "knowing capitalism": "circuits of information proliferate and are embedded in numerous kinds of information technologies" (Savage and Burrows 2007, 886).

This crisis affects three questions in academic sociology: who qualifies as a legitimate knowledge producer, which methods are validated as "scientific," and what counts as proper data. The commercialization of empirical sociology via the Internet enables new and powerful social agents, including private firms and institutions, to collect huge amounts of data on complete (sub)-populations as a by-product of their daily transactions.[8] Furthermore, private entities are developing sophisticated methodological techniques for "mining" traces left by digital transactions on the Web or on mobile phone networks. Savage and Burrows (2009) claim that the preferred tools of empirical sociology—the in-depth interview and the sample survey[9]—will therefore become less relevant.[10] The third aspect of the proclaimed crisis of empirical sociology relates to the very nature of what is a sociological variable. The electronic footprints left by the activities of individuals engaged in online shopping or social networking have become a more important proxy for sociological information than the personal and social attributes of the individual "transactor," such as education, gender, age, race, and class, which have been the central variables in sociological research for almost a century.

### RESPONSES BY THE SCIENTIFIC COMMUNITY

The idea of a "coming crisis" has dawned upon the scientific community rather slowly, and it has been mainly discussed in the context of British sociology. Savage and Burrows (2007) conclude that empirical sociologists (quantitative as well as qualitative) need to rethink their methodological practices in a radically innovative way. Instead of getting caught up in internal disputes, sociologists will have to become more attentive to the deployment of new methods seeking to describe different forms of the "social." With this line of reasoning, empirical sociologists have two options: they can ignore new forms of social data by invoking their academic superiority and sophistication in social theory, or they can critically engage with research on transactional data by questioning classifications, assumptions, and procedures. In the words of Savage and Burrows, they "could seek to get their hands dirty by exploring the potentials of such new methods and the issues posed by their use" (2009, 766–767).[11]

Rosemary Crompton (2008) reacted to Savage and Burrows' recommendations by emphasizing that the "old" paradigms were still present in contemporary sociology. In her opinion, empirical sociologists should not choose one party (qualitative or quantitative). As these paradigms are interdependencies rather than binaries, sociologists have to develop the capacity to work across what appear to be conflicting approaches. This also means that they will have to develop an understanding of a range of different empirical research methods. Consequently, Crompton critiques the suggestion to embrace the "new methodological turn" with its preference for description and classification rather than causal explanation. She suggests that Savage and Burrows remain caught within the binary opposition of the qualitative versus quantitative tradition, and sees this as dangerous. In her opinion, "we need to describe, interpret and understand but we should also be concerned with causes" (ibid., 1223). In this context, Crompton acknowledges that sociologists should be sensitive and responsive to methodological innovation. Her main concerns are related to the possibility that the suggestions by Savage and Burrows could be seen as an argument by sociologists to avoid the study of quantitative methods.

Further rebuttals to the "coming crisis of empirical sociology" were raised in a special edition of the 2009 *European Journal of Social Theory* in which several authors discussed the paradoxical emergence of a crisis of empirical sociology and a return to the empirical within the field. The contributions of Adkins and Lury (2009), Clough 2009, and Savage (2009) show that there is a shift in the very nature of the empirical that requires the reconsideration of the relations between fact and value. In this debate, the crisis of the empirical is not so much understood as the decoupling of sociological expertise and the academic sociologist (a decoupling that might be remedied through greater methodological creativity and innovation). Rather it is perceived as a necessary and productive destabilization of the role of empirical research in shaping the characteristics of sociology as a discipline.

In addition to the responses to Savage and Burrows, Anderson's hypothesis also triggered a lively debate that is directly relevant to the "crisis in empirical sociology." Interestingly, this debate involved intellectuals with no particular allegiance to academic institutions, who provided an alternative angle to the "academic" viewpoint. (See also the discussion at the homepage of edge.org, an eclectic group of intellectuals). For instance, Kevin Kelly (a science writer and a co-founder of *Wired* magazine) guesses that "this emerging method (correlations) will be one additional tool in the evolution of the scientific method. It will not replace any current methods (sorry, no end of science) but will compliment established[,] theory-driven science. . .

. It is not the end of theories, but the end of theories we understand." Similarly, Daniel Hillis (chairman and chief technology officer of Applied Minds, a knowledge-based company) writes: "I do not see why large amounts of data will undermine the scientific method. We will begin, as always, by looking for simple patterns in what we have observed and use that to hypothesize what is true elsewhere. . . . We will extrapolate from the data first and then establish a context later. This is the way science has worked for hundreds of years." These arguments demonstrate that, to some, newly emerging data floods do not signify the end of "the scientific method," by which they mean the basic deductive-nomological model of explanation. On the contrary, it seems that these authors perceive the current developments in data-driven research as a kind of "natural cycle" in the scientific world, leading to methodological innovations and new theorizing without banishing the traditional approaches.

Against this background, it seems that Savage and Burrows' proclamation of a "crisis" succeeded in compelling sociologists to think about and discuss their discipline and changes occurring within it. In this respect, the arguments listed above show that new forms of (transactional) mass data have raised awareness of the necessity for change and adaptation in some theoretical and methodological aspects. However, this need not be perceived as a threat to the field. Newly emerging tools developed both inside and outside academia will not necessarily lead to the abandonment of traditional approaches. They were not born in isolation from other methods and approaches; rather, they developed in a stepwise process of confronting "the old" with "the new." Long before the articles about a crisis in sociology were published, social scientists discussed the impact of the Internet on their practices (Hine 2006; Schroeder and Fry 2007). For instance, the creation of the Association of Internet Researchers in the year 2000 and the comments in the Social Sciences Week Blog of 2005 show how quickly social scientists recognized the Internet both as a research tool offering new research methods for collecting, analyzing, and disseminating data and as a social phenomenon worthy of study in itself.[12] The possibilities offered by new technologies are generally regarded as an enrichment rather than an impoverishment of academic knowledge production.[13] Furthermore, the reactions of social scientists to the development of data collection via the Internet[14] also demonstrate that it is unjustified to chastise sociologists for their ignorance of new methodological challenges. Even though online surveys were mainly a tool for commercial marketing agencies, survey methodologists had already begun to examine their advantages and disadvantages at the end of the 1980s. This resulted in several attempts to improve the methodological tools for online data collection and analysis (Couper 2000; Fricker and Schonlau 2002; Vehovar and Manfreda 1999).[15]

## WHAT ARE THE IMPLICATIONS FOR THE FIELD?

Is there a crisis in empirical sociology or not? Should academic sociologists give up on their traditional methodologies, or should we believe the more moderate commentators who play down the significance of the change? We propose that a useful perspective in this debate is to see the recurring cycles of a feeling of crisis in sociology as a fundamental characteristic of the discipline rather than as something external (Hollands and Stanley 2009). Robert Michels (1932, 123–24) characterized sociology as "largely demoralized" and undergoing "an intense spiritual self-criticism." More than 40 years later, Robert K. Merton (1975, 22) commented that sociology "has typically been in an unstable state, alternating between planes of extravagant optimism and extravagant pessimism." One of the most influential articulations of a "crisis" in sociology was expressed by Alwin Gouldner (1970, 1979, 1985). In his view, sociology had become too monolithic, accompanied by a lack of reflexivity with respect to its theories, its methods and its relationship to research and a failure to engage with the changing world. In the subsequent discussions during the 1990s and across the millennial years, concerns were expressed repeatedly about the discipline's decline due to diversity and fragmentation (Turner and Turner 1990; Horowitz 1993; Esping-Anderson 2000; Cole 2001; Berger, 2002)[16] and its future in connection with "public sociology" (Burawoy 2004; Acker 2005; Aronowitz 2005; Ghamari-Tabrizi 2005; Urry 2005).

Against this background, Savage and Burrows' (2007) appeal seems to be a continuation of this "crisis" tradition. The claim of a coming crisis is a metaphor for a perceived need to reflect on the discipline and its internal changes brought about by social, economic, and technical innovations. In this context, the arguments regarding the implications and challenges of the emerging data floods for the field of sociology seem to be both methodological and theoretical. First, it seems worthwhile for empirical sociologists to reflect on their methodological approaches more critically. This includes a change in their own assessment as well as a fresh look at new approaches and more complex methods of data analysis. The collection of new forms of mass data on human behavior, societies, and economies will stimulate the use of advanced computing in sociology. Furthermore, the informed use of innovations in grid computing, visualization techniques, and other novel analytical methods will be necessary to achieve a complete understanding and meaning of the patterns detected. This will require a willingness to team science and novel computational methods. (See chapter 2 above.) First steps in this direction have been undertaken by social scientists in "complexity research."[17] (For more details, see Fischer et al. 2008, 533.) This interdisciplinary and multi-disciplinary field bridges most of the traditional divides that have evolved in social science, such as reductionist/non-reductionist, aggregated/disaggregated, and micro/macro. However, how empirical sociologists will take advantage of this potential will depend on their individual openness to new methods as well as on the acceptance and institutionalization of these methods within the discipline. It seems likely that many empirical sociologists will continue to "do empirical sociology" in more traditional ways.

New technologies are also creating opportunities for new substantive theory. As Blank (2008, 538) argues, these "new theoretical opportunities come in part from the new social forms and new communities created by online technologies." Moreover, online research casts new light on older social formations. New impulses may also arise from the new mass of data. Social network theory is a good example of a theoretical development that, though its roots can be dated back to Simmel and Durkheim, was recently fostered by major advances in computing and methodological tools (Moody and White 2003). These effects will continue, but it is hard to predict which other theories will benefit. After all, the relationship between data and theory is complex. As Blank (2008, 543) underlines, even though the new data promise a remarkably fine-grained, detailed picture of people in all kinds of social situations, this will not necessarily lead to a more advanced understanding of the social. As Bulmer (1984) emphasized, "the scientific method is not understood simply as a meticulous and painstaking collection and analysis of data, but involves a concern with theoretical problems and an urge to explain social phenomena through theoretical work." In this respect, doing social research necessarily involves theory as well as the strategies and techniques of empirical investigation. The collection of large amounts of data alone is not sufficient to guarantee useful research results explaining social phenomena.

## NEUROIMAGING DATA IN ECONOMICS

Neuroeconomics is an emerging subfield at the intersection of economics, biology and psychology. Concerned with the elucidation of the neural basis for decision making, it represents an effort to confront data and theories from different fields across the traditional border between natural and social science, displaying a degree of interdisciplinarity rarely witnessed before (Glimcher et al. 2008). Economics received a rather bad press after 2008 because it failed to foresee the global financial and economic crisis; it is even suspected of having played an initiating role in it. This has created a dispute about fundamentals within the field of economics itself. In a widely read essay on the collective failure of the economics profession to predict the financial crisis, Paul Krugman claimed that "economists, as a group, mistook beauty, clad in impressive-looking mathematics, for truth" (2009). Is this discussion comparable to the discussion within sociology? In other words, will new, neurologically plausible models, being considered more relevant due to their supposed proximity to "reality" or "truth," replace mathematical economics? Just as the traditional survey in empirical sociology is threatened by massive amounts of transactional data, traditional theoretical economics is challenged by a new type of empirical material. Neural data generated by neuroscientists in collaboration with psychologists and economists are increasingly accepted in publications with a bearing on economic decision making. To appreciate the meaning of this evolution, let us put this in historical perspective.

### THE EMPIRICAL TRADITION IN ECONOMICS: BACKGROUND

In the first three decades of the twentieth century, economics progressively abandoned all references to the biological sciences. This was part of a larger trend in the social sciences, partly inspired by the ambition for professional status on a par with the natural sciences. Culture was seen as independent from a biologically defined human nature. Analyses of cultural behavior were to be self-referential, without the need for physical or biological foundations. This development precluded the use of natural science data. Biometrics, psychophysiological measurements, and population censuses based on biologically inspired definitions all rapidly disappeared from economics. Henceforth, data in economics would be extracted from the body politic (Ross 1991; Degler 1991; Leonard 2005).

Indeed, in the 1930s, major economic datasets were created. Keynesianism provided the rationale for the development of national accounting, an information system collecting data on economic variables relevant for macro-policy. With the Great Depression and the transition to a war economy, it became central to the definition of an economist's job to collect, interpret, and predict figures of unemployment, inflation, or investment, all culminating in the measurement of the ultimate index of economic activity: Gross Domestic Product, measurement of which started during World War II in the United States and Great Britain (Mitra-Kahn 2011).

Although data exchange between economics and biology decreased strongly, biological concepts kept percolating through economics. Following Armen Alchian's article on economic natural selection (1950), the analogical comparison between economic competition and natural selection provided the foundations for evolutionary economics, which eventually became a major field in heterodox economics. Pursued in a different form, this analogy also inspired Chicagoan economics, analytically developed by George Stigler (1968) in a "survival principle" in industrial economics. These were mere analogies in the sense that no correlation with biological data was implied by the economic selection models. Only the logical structure of the model was explicitly transferred.

The 1970s saw the first cracks in this strict separation of social science data from biological data. In *Sociobiology: The New Synthesis*, the biologist Edward Wilson (1975) challenged evolutionary biology and social science to check their models and conclusions against the latest biological theories supported by new empirical findings. Provocatively, Wilson wrote that philosophers should revise all their conclusions on human nature, since the seat of their own mind was located in the brain—a biological organ under the influence of emotions. The controversy (the "sociobiology debate") ensuing thereafter in biology and in economics and the rest of the social sciences testified to a fundamental incommensurability between natural and social sciences: data drawn from the biological body could not legitimately inform the social or the cultural sciences. Genetic fitness, the biological variable postulated by the theories of social evolution developed in *Sociobiology*, did not feature in further investigations of economic behavior. However, the debate laid the foundations for a renewed interest in applying insights from the life sciences to understanding complex social behaviors in human societies. With neuroeconomics, biological (neural) data on human behavior eventually found a receptive audience in economics. A number of economists see neurological data as a potentially fatal threat to the fiction of *homo economicus*, the rational "utility maximizer" central to microeconomic theories. Neural data shows that individuals exhibit consistent biases in their choices. Humans are "hardwired" in a manner that seems to contradict the axioms of choice taken for granted in microeconomics and seems to suggest that advances in the science of decision making will come from experimental investigations in neurobiology rather than from mathematical refinements of the economic models based on the assumption of rational utility maximization.

## IS THERE A CRISIS, AND WHO IS AFFECTED?

The wealth of physiological data provided by functional magnetic resonance imaging (fMRI)—a brain imaging technology, developed in the late 1980s, that claims to allow visualization of how neural activity correlates with precise mental states (see Beaulieu 2000 for an analysis of these developments)—created considerable excitement among economists.[18] Human subjects placed in a scanner can perform experiments similar to those usually conducted in a behavioral lab, such as an ultimatum game or an exercise in financial decision making, and their behavioral responses can be compared against the neural states recorded while the task is being performed. Data from such observations make it possible to supplement the verbal accounts of the subjects with an understanding of how their decisions are made at a subconscious level, beyond the reach of *post hoc* justifications by the subject. This empirical connection differs from the various Darwinist or organic analogies, which previously linked economics to biology. Results produced in experimental settings by neurobiologists inform economic models by replacing the usual normative sets of economic axioms with neurologically validated behavioral assumptions. This suggests that neuroeconomics might displace or even render irrelevant much of mainstream economic theorizing. In this regard, and in response to Krugman's criticism of mathematically oriented economics, some neuroeconomists believe that they can provide new economic models based on behavioral assumptions validated by neurological data. Whereas neoclassical economics postulates a chimerical utility maximizing individual, neuroeconomics would provide a "real" model of a decision maker based on the accumulation of empirical findings in cognitive neuroscience. We illustrate this with an example.

Alan Sanfey's 2003 paper "The neural basis of economic decision making in the ultimatum game" represents an empirical challenge to theoretical economics in several respects. Investigating a paradigm of behavioral game theory, it shows that even in a two-person simple game with monetary reward some participants display altruistic behavior whereas others enforce punishments (at a cost to themselves) when confronted with an unfair partner. These behaviors question the validity of neoclassical economic models based on the assumption of "the self-interested agent." The fMRI study confirms that specific brain areas are selectively activated if a certain type of behavior is displayed. For example, the rejection of an unfair offer is correlated with an activation of the anterior insula, which in previous studies had been associated with subjects experiencing feelings of disgust. This suggests that economic decision making can be associated with emotional states, and it further suggests an empirical research program in neurology concerned with interactions between emotional states and cognitive control. This would threaten the purely axiomatic study of decision making currently practiced in economics.

## HOW DID THE SCIENTIFIC COMMUNITY RESPOND?

Behavioral economists, whose research programs provide psychologically rich accounts of economic decision making, are among the most vocal promoters of neuroeconomics.[19] They claim that, in addition to the psychological data gathered in behavioral experiments in labs, neurophysiological data challenge the ethereal theories of decision making developed by mainstream economics—theories built on axiomatic foundations. Psychological bias in decision making, such as framing or endowment, would have a demonstrable neural substrate, and the wiring of the brain does not allow for the perfect rationality postulated by neoclassical economists (Breiter et al. 2001; Knutson et al. 2008).

A manifesto by leading figures of behavioral economics argues that "feelings and thoughts *can* be measured directly now, because of recent breakthroughs in neuroscience," and that "if neural mechanisms do not always produce rational choice and judgment, the brain evidence has the *potential* to suggest better theory" (Camerer, Loewenstein, and Prelec 2004, 556). This challenge is fundamental. It would require economists to supplement their methodology (consisting of axiomatic representations of choice, optimization techniques, and statistical analysis) with very different methods: behavioral experiments (which have already found their place in many economics departments) and the use of animal models, brain imaging, patient studies, and neuropharmacological experiments. That economists will abandon their core competence and convert en masse to biodata-collecting methodologies is improbable, however.

A recent counter-manifesto criticized the boldest claims of neuroeconomists. The economists Faruk Gul and Wolfgang Pesendorfer wrote a long defense of economics as traditionally practiced, denying the relevance of experimental neurological data to explain economic choices:

> Economics and psychology address different questions, utilize different abstractions, and address different types of empirical evidence. Neuroscience evidence cannot refute economic models because the latter make no assumptions and draw no conclusions about the physiology of the brain. Conversely, brain science cannot revolutionize economics because the former has no vehicle for addressing the concerns of economics. We also argue that the methods of standard economics are much more flexible than it is assumed in the neuroeconomics critique and illustrate this with examples of how standard economics deals with inconsistent preferences, mistakes, and biases. (2008, 3)[20]

This argumentation for the separate and independent paths of academic fields rests on a combination of Samuelsonian and Friedmanian methodologies. According to the "revealed preference" theory, advocated by Paul Samuelson in *Foundations of Economic Analysis* (1947), economists have no concern for the psychological antecedents underlying an individual's choice. The task of economists consists of analyzing the market consequences of individual decisions to consume or produce, consequences interpretable in terms of changes in supply and demand schedules and price. In this economic framework, "utility maximization" is not a theoretical assumption that can be dismissed by scanning brains, since "the terms *utility maximization* and *choice* are synonymous" (Gul and Pesendorfer 2008, 7). In a way reminiscent of the behaviorist credo, Gul and Pesendorfer maintain that the only reasonable assumption is that individuals' observed choices are what they want most. This viewpoint precludes any refutation of such theories by neurocognitive data interpreted to disprove the assumption of maximizing behavior.

The second line of defense recalls Milton Friedman's (1953) famous proposal that models ("hypotheses") should be judged according to their fecundity and not according to the empirical validity of their assumptions. Following this line of reasoning, Gul and Pesendorfer argue that neuroeconomists are wrong-headed in their attempt to improve the validity of economic behavioral assumptions by experimental neuroscience data. Theories *are* abstractions, which implies that their merits cannot be judged on the basis of their fit with data which they were not designed to address. As long as economic theories are able to explain the relative welfare generated by different institutional arrangements, it is irrelevant how valid they are from a neurologic perspective.

## WHAT ARE THE IMPLICATIONS FOR THE FIELD?

Both proponents and opponents of neuroeconomics foresee fundamental implications for economics. Yet an examination of the field and the literature in neuroeconomics suggests a much more conciliatory conclusion: that neuroeconomics seems to exemplify consilience, the "interlocking of causal explanations across disciplines" (Wilson 1998, 359). One discipline would not supersede others, rather, consilience is a state of interdisciplinarity in which disciplines evolve to become more compatible. The multiplication of neuroeconomics laboratories at major American universities suggests that this is an ongoing development. Economists are learning to collaborate in teams with psychologists and neuroscientists without renouncing their own theoretical frameworks. In these interdisciplinary teams, economists are valued for their understanding of the broad frameworks of expected utility and game theory, which introduce such concepts as Bayesian probability and strategic behavior to neuroscientists who work on models of decision making. Neuroscientists and psychologists, in their turn, provide expertise in experimental design and recording neural data associated with decisions made by subjects, while relating the theoretical variables in economic models to corresponding cognitive functions and their underlying physiological substrate. In this collaboration, a division of labor between economists and neuroscientists still exists, but they strive for theoretical and empirical coherence across disciplinary boundaries. Two examples will illustrate how neuroeconomics produces new empirical data and improves on existing biological and economic theories.

Paul Glimcher of New York University is a psychologist and neuroscientist specializing in the study of sensory perception (the visual pathway) and motor control. In the late 1990s, neuroscientists disagreed about the interpretation of the neural signals recorded in monkeys engaged in a visual task before movement was initiated. Were they signs of the monkey's *attention* to the visual cue, or did those neurons fire because of the monkey's *intention* to initiate a movement? On the basis of these experiments, neuroscientists could not decide between those competing hypotheses. Glimcher's intuition, formulated together with his postdoctoral student Michael Platt in a paper now widely credited for launching neuroeconomics, was that those neurons encoded variables related to the computational goal to be performed by the monkey. Following a Darwinian logic, Platt and Glimcher (1999) reasoned that the monkey's ultimate goal in the experiment was to maximize the juice reward. To achieve this, it is necessary for the monkey to keep track of at least two basic variables: the probability of certain visual cues to yield a reward and the amount of juice associated with a reward. Combined, these variables define the reward which the monkey can expect in the next sequence of the experiment. Manipulating these variables in the experiment confirmed that neurons localized in the region of interest fired in accordance with the reward the monkey could expect.

In this case, the economic theory of expected utility gave a meaning to the data gathered through neuroscientific experiments. More precisely, framing the problem in economic terms allowed the design of new hypotheses to account for a monkey's actions by introducing new variables (intensity and probability of reward) and a new conceptual apparatus (the theory of choice). This implies that the scenario of a clash between data from natural science and social theories might be ill-conceived. Neuroscience is an experimental science producing radically new data, which are in turn molded by theoretical frameworks. This predicts that economics might actually benefit from contact with empirical data about cognition and decision making. Glimcher's lab produced further results in this direction, using game theory to analyze neural activity in strategic decision making by monkeys and humans (Dorris and Glimcher 2004). In these new studies, innovation came less from the use of neural data to study economic choice than from the high yield of the economic theoretical framework used to interpret it: the concepts of mixed strategies and Nash equilibrium showed that random patterns of neuron firing in monkeys facing uncertain choices could be explained as an ordered and optimal response of the monkey's brain attempting to track the best available choice option.[21]

This example is concerned with biologists benefiting from theoretical insights in economics. It might remain true, as Gul and Pesendorfer have argued, that economics can expect no benefit from imaging data provided by neuroscience. A recent paper, however, provides counterarguments to this proposition.

In a paper published in *Science* in 2009, four neuroeconomists at the California Institute of Technology, Ian Krajbich, Colin Camerer, John Ledyard, and Antonio Rangel, chose to attack the free-rider problem, one of the oldest problems in economics, from a new angle. The free-rider problem is the one faced by an individual who can choose to pay or not pay his part of a collective investment, given that the investment will benefit all individuals irrespective of his contributions. (In economic terminology, the investment is a "public good"; a typical example is national defense.) In this situation, models of rational economic decision making and experimental games show that individuals tend to understate their willingness to pay. Each individual prefers to "free ride." The line of reasoning behind this is that if the others pay for the investment, the individual will benefit from it anyway. The aggregation of the individual preferences leads to the suboptimal result that the funds needed to finance the investment are not raised.

This classic negative result in public economics can be much improved by recourse to neurological monitoring of individuals during the phase in which they declare their willingness to pay, Krajbich et al. explain. The individuals' brains are scanned while they choose to contribute to the investment or not, and this data is used to detect any discrepancy between their stated willingness to contribute and their actual desire to see the investment implemented. This is not merely an expensive form of lie detection—fMRI's performance in detecting free riders is just above random. It is the threat of being detected, and the associated monetary punishment, that deters individuals from lying about their real preferences. As it turns out, the social welfare achieved in the experiment corresponds to 93 percent of the optimal outcome. Nearly all of the socially desirable investments are chosen by the participants, versus the 23 percent usually obtained in classic experimental settings. This study demonstrates that economic theorizing and economic policy making are able to capitalize on biological data. In other words, not only can neurological data be used to test a model in social science; such data also can be integrated into the existing frameworks and can yield new results.

Neuroeconomics invalidates the notion that the availability of new types of data will displace theoretical investigations and eventually render them obsolete. Datasets and theorizing do not displace each other, as in a zero-sum game. Rather, what the interdisciplinary relationship between economics and biology demonstrates is that they tend to nurture each other. What are the practical implications for economics?

Traditional education in the framework of rational decision making is in high demand in cognitive neurosciences. Competing neurological hypotheses can be tested against economic theories of utility maximization, and the axiomatic formulation of a choice theory is already used with a comparable purpose: clarifying and selecting between competing neural models in systems neuroscience (Caplin and Dean 2008). The statistical skills of econometricians, where they have a certain comparative advantage, will also probably become valuable in the testing of models of choice. Should economists rejoice, and change none of their old habits? Probably not. The eruption of data of a radically new nature, generated in unfamiliar settings by practitioners outside economics, will demand from economists that they practice their skills in an interdisciplinary way—something they are notoriously poorly trained to do.

Economics, because of its relatively advanced formalism and the united methodological front displayed by its mainstream practitioners, has often demonstrated an overbearing attitude toward other social sciences. Biology has also been described by some economists as a field in which economic "imperialism" could be exerted (Hirshleifer 1985). If the economics profession accepts the possibility that an influential (and not necessarily competing) view of individuals' choices may come from neurobiology, the eruption of new data in neurobiology may discourage the use of confrontational metaphors. The blindness of the economics profession to any data not conforming to its mathematical models has been criticized recently, and calls have been made for more pluralistic methodological teaching within the field (Denis 2009). The emergence of neuroeconomics shows that openness to currents of thought and streams of data coming from outside of economics is certainly needed.

## CONCLUSION: DATA CHALLENGES TO SOCIAL THEORY?

In this chapter we have explored the implications of the emergence of radically new data sources for scholarly research. We have compared two social science disciplines in which empirical research is seen as highly relevant to theoretical innovation: sociology and economics. We have discussed who has been making claims for the importance of new data sources, we have seen how different positions in both sociology and economics have been pitted against each other, and we have recognized areas of compromise and blurring of boundaries between different research styles and paradigms. In this final section, we compare the two case studies in more detail and relate them to the more general discussion about a "fourth research paradigm," which is supposed to underpin the initiatives in building infrastructures for e-science and e-research.

Perhaps our first conclusion should be that the hypothesis of the fourth research paradigm, taken literally as proposed in much of the scientific literature, is a simplification of a much more complex reality, and that it risks losing sight of important dimensions of scientific and scholarly work. This holds not only for the hyperbolic claims of journalists but also for the claims that underpin investment schemes for e-science and cloud computing infrastructures by commercial companies such as Microsoft, Amazon, and Google. To put it bluntly: If we take into account how new data sources have been implicated in empirical sociology and in economics, a straightforward shift from previous research paradigms to a novel "fourth paradigm" is improbable.

However, this does not mean that the claim that there is a fourth research paradigm is nonsensical or lacks veracity. It does represent a valid advocacy position. Basically, it is a form of standpoint epistemology that tries to shape the world to its ideal image. Both in sociology and in economics, data-driven research has taken on new forms that have become highly relevant to important areas of research. These forms do represent radical innovations in the field, but not quite in the way envisioned by proponents of the fourth research paradigm. Detailed study of these patterns of innovation actually yields more interesting results than the more abstract discussion about the future of e-research in general.

In both sociology and economics, novel data sources are exploited by relative outsiders. Commercial counterparts to academic sociology using powerful micro-level data sources not available to academics have emerged. In economics, innovation has come from the participation of biologists and neuroscientists with their neuroimaging instruments in the field of decision theory. It is a familiar pattern in the history of science that innovation is fostered by outsiders and by the use of novel instrumentation. Less attention usually is paid to the specific forms of resistance within established scholarly communities. These strategies are quite relevant, though, because they shape the patterns of (inter)disciplinary development that inevitably ensue when new ideas invade a discipline.

In the case of sociology, we have recognized two different forms of resistance: denial of authority and assimilation. The first is evident in the claims that commercial forms of sociology, including market analysis within large companies and freely available social networking tools developed by commercial search engines, lack sophistication and rigor. Seen from the perspective of the methodological standards of the field, this is patently true. However, it remains to be seen whether this criticism will be able to counter the argument that the traditional standards are no longer valid owing to the very large scale of the new types of transactional data. We have also noted that the field of sociology has not yet taken up the challenge of processing geographical data. Since these data have been a staple of geography and some other fields, it will be interesting to see whether new forms of interdisciplinary work on the boundaries of these fields will provide new research methodologies for sociology in general.

Assimilation is a strategy employed by those social scientists who claim that the new data sources are not so new as to require special treatment. From this position, new data should, rather, be treated in the same way as other forms of data with which the field is already familiar. This is a pincer movement: if the data are included in the field's traditional data-processing routines, the methodological standards are also upheld as still highly relevant. An example of this strategy is the claim that having more transactional data does not necessarily mean that we understand more about the role of these transactions. That would require social theory, and theory does not emerge from the data. Although we would not want to claim that theory emerges from data, neither would we want to preclude the possibility that inductive theoretical innovation can happen on the basis of data exploration. Prescriptive methodological notions are usually not very good at capturing the myriad ways in which theories and data interact in daily sociological practice.

In the case of neuroeconomics, we have seen a somewhat different pattern. Though commercial sociology has not really bothered to attack academic sociology, neuroeconomists have staged a frontal assault on the central assumptions of the field of economics in their defense of brain research as a basis for understanding economic decision making in particular and economic behavior in general. In particular, the popular notion of the rationally choosing human has been undermined by the demonstration of emotional factors, supposedly indicated by the activation of particular areas of the brain. In this case, the dominant defense strategy has been one of distancing the two domains from one another. We discussed how this was achieved in the claim that economic theory is indifferent to the neurological basis of decision making. It simply does not have anything to say about the biological basis of behavior, and it cannot be refuted by biological research results. In other words, the neuro-expertise is not disputed but is declared irrelevant. Here another approach in neuroeconomics is relevant, one more subtle than the frontal assault. As we have seen, one of the dominant research streams in neuroeconomics does not limit the validity of economic theory but extends its reach by reinterpreting neurological behavior into economic terms such as the maximizing of utility. Seen from this perspective, neural substrates are actually computing economic puzzles. This is an interesting epistemic move, since it aims to change the field of economics by extending its relevance to experiments and data that formerly were not subject to economic analysis.

To sum up, our analysis shows that traditional theories in social science are not doomed by the new data floods. Economics and sociology have a long tradition of dealing with large amounts of data and are too often portrayed as incapable, as if sociology could be reduced to Talcott Parsons' systems theory and as if economics consisted only of untested mathematical models. Both sciences actually have considerable expertise in dealing with extensive quantitative and qualitative data, as studies based on census data or the field of applied econometrics amply testify.

The history of economics in the postwar period epitomizes how social scientists' exclusive sense of identity led them to erect a strict boundary between their theories of social phenomena and (generic) data generated outside those boundaries. Many professional sociologists seem to appraise "commercially" collected transactional data in a similar way. Because those data were generated and processed outside of academia, without a theoretically guided research schema, they lack the sophistication that traditional sociology deems necessary. Not all social scientists, indeed not all sociologists, adopt such a defensive stance. For instance, management departments in business schools use their organic relationships with companies to get access to transactional data, then use those data for research in finance or marketing.

Technological innovation might be a common factor behind the emergence of extremely large new datasets in different sciences, but the specific type of data produced should also be considered. (See chapter 7 below.) Transactional data are behavioral data of a particular kind: they provide information on the choices made by individuals in relation to other individuals. Whereas traditional sociology (and economics and other social sciences, for that matter) could safely treat each observation as independent from other observations, the point of networks is precisely that modification or suppression of a data point brings changes to the rest of the dataset. Analysis of datasets has already triggered the design of a new theoretical framework of interpretation within the thriving field of social network analysis. A similar conclusion can be drawn with regard to neural data in economics: they foster the development of a behavioral type of economic theorizing in a way that contradicts the picture of a passive landscape of theories overpowered by data.

Finally, the methodological toolkit of the social sciences continues to be transformed and enriched by the eruption of new forms of data. Therefore, we can expect lively theoretical work rather than the demise of social theory. This may go together with an intricate re-drawing of boundaries between approaches within fields and between fields. Moreover, it may result in the splitting of fields (e.g., academic versus commercial sociology) or in the merging of fields (e.g., primatology and economics). Rather than the emergence of a robust fourth paradigm, we are witnessing a myriad of knowledge "patchworks," partly overlapping, and partly contradictory, which are becoming more complicated rather than less so. Having more data may or may not lead to more knowledge, but it certainly leads to a need for more theoretical constructs with which to analyze those data.

## NOTES

1. The Sloan Digital Sky Survey (http://www.sdss.org/) laid the foundation for this infrastructure. Examples of publicly available results are Microsoft's World Wide Telescope (at http://www.worldwidetelescope.org) and Google Sky (at http://www.google.com).

2. For responses to Savage and Burrows, see the *European Journal of Social Theory*, *Sociology*, *Cultural Sociology.* For responses to Anderson by Kevin Kelly, Daniel Hillis, and others, see http://www.edge.org/discourse/the_end_of_theory.html.

3. Quantitative methods are concerned with attempts to quantify social phenomena and to collect and analyze numerical data. Common tools include surveys,

questionnaires, and secondary analysis of statistical data. Qualitative methods, on the other hand, are more concerned with understanding the meaning of social phenomena. Therefore, they emphasize personal experiences and interpretation over quantification. Commonly used tools include focus groups, participant observation, and the in-depth interview.

4. Particularly in the United States, the positivist tradition remains ubiquitous in sociology. The discipline's two most cited journals, the *American Journal of Sociology* and the *American Sociological Review*, primarily publish research in the positivist tradition. The *British Journal of Sociology*, in contrast, publishes primarily non-positivist articles.

5. Survey research and the collection of micro-economic and transactional data to build government and private statistical databases are the main forms of research in this tradition of empirical sociology.

6. A good example is the development of the deutsche Universitätsstatistik, which is associated with Herman Conring and Gottfried Achenwall (Schnell et al. 1995).

7. The Chicago School's studies in urban sociology emphasized two other kinds of "empirical" research: fieldwork and ethnographic work, which remained dominant methodologies in American sociology until the 1940s (Park, Burgess, and McKenzie 1925; Thomas and Znaniecki 1920; Coulon 2004).

8. The amount of digital information created, captured, and replicated worldwide in 2007 was 2.25 × 1,021 bits—about 281 exabytes, or 281 billion gigabytes (Gantz et al. 2008). The term *transactional data* covers all sorts of records collected routinely by public and private organizations, such as the duration of a communication between two mobile phone users by their respective companies, or the kind of information collected by cookies installed on the computer of visitors to a website.

9. However, Savage and Burrows don't intend that the sample survey become obsolete. They note that in some areas it will continue to be a central research tool because of the limits of transactional data (2007, 892).

10. This also holds for ethical concerns about anonymity and confidentiality.

11. With this suggestion Savage and Burrows do not mean to sell the sociological soul to the devil of market research. Rather, they emphasize that an involvement with such technical innovations entails reflecting on the methods of such powerful commercial agents and engaging with them in "public sociology."

12. The Internet has become a heavily studied subject on its own, as various sociological conference series testify.

13. See the Social Sciences Week Blog (hosted by the Social Science Information Gateway, and ended in July of 2005), which reflects on changes triggered by technical innovations in sociology.

14. With the advent of computer-assisted survey information collection (CASIC) in the 1980s, and particularly with the expansion of Web-based surveys in the late 1990s, technology revolutionized data collection.

15. In this context, it has also been noted that an engagement with commercial sociology is difficult because market-research agencies are often reluctant to provide detailed insights into their methodological applications (Danielsson 2004).

16. The subtitle of the theme of the American Sociological Association's 100th annual meeting, held in 2005, was "Accounting for the Rising and Declining Significance of Sociology."

17. The term *Complex Systems* (CS) denotes an interdisciplinary research methodology currently in favor in the social sciences and elsewhere. CS research originated from physics and nonlinear systems some decades ago. Its models soon permeated such distant fields as economics, political science, and (more recently) sociology. In social systems, the essence of CS is the characterization of the distributed dynamics of how the interaction of many actors and variables leads to predictable phenomena, which often involve hierarchy, emergence, dynamic structures, and large-scale transitions.

18. Functional magnetic-resonance imaging is the technology often singled out in presentations of neuroeconomics, but a variety of other recording techniques are also used, among them electroencephalograms and magnetoencephalograms, cell recordings, and endocrinological measurements.

19. Daniel Kahneman, the founder of behavioral economics and a co-recipient of the Nobel Prize in Economics in 2002, was recently invited to receive a doctorate *honoris causa* from Erasmus University Rotterdam. When evoking the future of his research program, Kahneman singled out neuroeconomics as a promising venture.

20. Gul and Pesendorfer 2008 circulated as a working paper for three years before appearing in print in a volume presenting a collection of the reactions it generated (Caplin and Schotter 2008).

21. A Nash equilibrium is a state of equilibrium reached when each player knows that each other player will also play his best strategy and thus no player has any incentive left to deviate from his best strategy.

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