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One social science or many?

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ONE SOCIAL SCIENCE OR MANY?

by

Jon Elster

Abstract

The current economic crisis is also a crisis of the social sciences, notably of economics, and offers an opportunity to reflect on the nature and the prospect of these disciplines. The demise of neoclassical macroeconomics and the emergence of behavioural economics suggest that the social sciences ought to lower their ambitions, to focus on the accumulation of small-scale mechanisms rather than on the development of grand theory. Rational-choice theory, while useful in specific domains, can no longer claim to be the unifying theory for the social sciences. In fact, there is not and probably will never be one unifying theory, only a toolbox of mechanisms. A common language for the social sciences may yet be created if all social scientists receive a thorough grounding in the classics of historical writing.

When I accepted the invitation to give this talk, in the Fall of 2007, I did not expect that the social sciences, and notably economics, was about to be forced into a deep self-examination triggered by the financial crisis. It seems as if the Hollywood slogan about the prospects of a newly released movie, “Nobody knows anything”, suddenly applied to basic issues of economics and finance. The status of macroeconomics as a science seems less compelling than before, to put it mildly. As for microeconomics, its status as a science has become increasingly fragile over the last thirty years or so. The other social sciences, notably sociology, had less to lose, as their reputation was not that high in the first place.

Before I turn to these matters, I have to spend some time discussing the key terms of the title of my talk: “social” and “science”. As you may know, a proposed 28th amendment to the Constitution of the United States has the following wording:

It is the inalienable right of each citizen and non-citizen to use any term with the meaning that he or she decides to give it, on the condition that the meaning of the term and some reasons for using it in this sense are stated.

Assuming that this clause applies to the present proceedings, I shall feel free to offer a stipulative definition of “social science”, guided by my specific aim in this talk and constrained by common usage.

In my understanding, the goal of social science is to uncover proximate causes of behaviour. On this definition, the historical sciences are part of the social sciences, since they also are concerned with the causes of behaviour. Although one might try to draw a distinction between historians as *consumers of mechanisms* and social scientists as *producers of mechanisms*, this would be quite misleading. Tocqueville’s study of the *ancien régime* and Paul Veyne’s study of civic giving - evergetism - in Classical Antiquity both contain more and more fertile mechanisms than almost any work in social science I can think of. Conversely, most economists, sociologists and political scientists are tool-users rather than tool-makers.

Whereas the social sciences are concerned with the proximate causes of behaviour, the search for ultimate causes belongs to evolutionary theory. There is some intersection between social science and the theory of evolution, as exemplified by the disciplines of evolutionary psychology and evolutionary game theory. In my opinion, these disciplines have not yet developed to the point where they can offer

testable explanations. As is often noted, many of them have the flavour of just-so stories. They are consistent with the known facts, but do not pass the crucial test of generating novel facts. Even if someone should disagree with this statement, I shall appeal to the proposed amendment and claim my right to exclude evolutionary theory from social science.

By proximate causes I have in mind mental phenomena such as beliefs, desires, perceptions, and emotions. This stipulative definition enables me to also exclude neuro-social-science, notably neuroeconomics, from the field of social science. Although brain processes are proximate causes of behaviour, they are not the kind of causes I shall be looking at. My reason for excluding them is partly my lack of competence in this area and partly my hunch that these approaches are premature. For instance, identifying neuronal correlates of subtle but important differences between beliefs, such as the difference between believing that someone is an atheist and believing that he is an agnostic, is probably a task for the very distant future. Once again, if anyone disagrees I appeal to the 28th amendment.

Let me digress briefly at this point, to observe that the title of my talk could also have been: “One social science, many, or none?” It sometimes seems as if between the two of them, evolutionary biology and neuroscience occupy more and more of the space that traditionally was the reserved domain of the social sciences. We might ask, therefore, whether social science is in danger of disappearing. I do not think there is any immediate danger, perhaps not even a remote one. I have no objection *in principle* to a naturalistic turn in the social sciences. Any such objection would be obscurantist. Yet my amateur impression is that there are so many interactions among parts of the genome and among parts of the brain that it will prove difficult or impossible to establish robust links between subsets of either on the one hand and behaviour on the other.

The claim I made a minute ago, that beliefs and other mental states are the proximate causes of behaviour, might seem to commit me to the principle of methodological individualism. I am in fact a firm believer in that principle, but it doesn't actually follow from the claim. There has been a recent interest in the idea that *groups* can have beliefs, intentions and even emotions that cannot be reduced to the corresponding mental states of their members. As far as I can see, nobody has shown that we need to adopt that idea to explain phenomena that would otherwise resist explanation. I shall not, therefore, pursue this issue.

I do need, however, to say a few words about methodological individualism – the claim that all social phenomena *should be* and in principle *can be* explained by independent variables at the level of the individual. I have always believed and continue to believe that the principle is a truism – trivially and boringly true. Durkheim said that “even if individual psychology were to give up all its secrets, it would still not be able to provide the solution” to the problems of sociology. I find this statement not as much false as unintelligible.

The principle of methodological individualism is routinely violated when firms and households are treated as agents. In the case of households, it has been known for some time that treating them as rational unitary agents does not yield correct predictions. In 1998, Browning and Chiappori showed that we get better predictions if we disaggregate households into their individual members, and assume that these are utility maximizing agents. To achieve this result, however, they also had to assume that consumption choices are Pareto efficient. This assumption is *not* consistent with methodological individualism, as it excludes, for instance, collective action problems within the household.

This remark is not intended as a criticism. In practice, individual-level explanations may be intractable and require data that do not exist. My point is only that the use of aggregates as the unit of analysis is always a second-best option, and that there is no reason to choose it for its own sake.

Before I try to answer the question of my title, I need to explain the “science” part of “social science”. As I understand it, the aim of science is to offer verified – or not-yet-falsified - causal explanations of observed phenomena. On this account, some alleged social sciences do not count as science. Large chunks of anthropology, for instance, are closer to literary interpretation than to causal analysis. Also, functional explanations of social phenomena in terms of their consequences rather than their causes do not count as science. An example is the explanation of vendettas as a “device” for

keeping population within sustainable limits. Maybe vendettas do have that effect, but it cannot be cited to explain them unless we also demonstrate the existence of some kind of homeostatic feedback loop. To my knowledge, nobody has even tried to do that. In a broad perspective, the work of Foucault and Bourdieu has been especially important in licensing claims of this sort. As I know first-hand from my exposure to current French social science, their influence is persistent and pernicious.

I shall also stipulate that science is *cumulative*, a claim that can be taken in one of three senses. First, over time scientists explain more and more facts. Better telescopes enable the exploration of deeper parts of space. Second, new scientific theories build on previous ones, generalize their results, and when necessary explain their failures. The relations between Newton and Einstein, or between Condorcet and Kenneth Arrow, illustrate this idea. Cumulativity in this sense also implies irreversibility. There are no neo-Newtonians in physics, in the way there are neo-Marxists, post-Keynesians or neo-Austrians in economics. To some extent, of course, these are marginal sects. Yet the current revival of Keynes in mainstream economics shows that even here, in the allegedly most scientific part of the social sciences, the properties of cumulativity and irreversibility are lacking.

I do not believe there is cumulative theory-building in the social sciences, since I do not think there are any successful *theories* in the social sciences. By a theory I mean a set of interconnected universal propositions from which, given initial conditions, unique predictions can be derived. Although the social sciences do contain would-be theories in this sense, none of them are successful in the sense of their predictions being routinely verified to a reasonable degree of precision. The main candidate for a social-science theory is obviously rational-choice theory, including game theory. In contemporary social science it is the dominant paradigm in economics and to a lesser degree in political science. I shall have more to say about rational-choice theory later. For now, let me only note that the field of sociology, which has a proud tradition of theory-building, seems to have lost its self-confidence. Unlike rational-choice theory, network theory and agent-based modelling do not pretend to yield strong predictions across large varieties of behaviour.

Let me now state the third sense in which the social sciences can be cumulative. It relies on the idea that the basic units of social science are *mechanisms* rather than theories. By mechanisms I mean *frequently occurring and easily recognizable causal patterns that are triggered under generally unknown conditions or with indeterminate consequences*. Since this bare statement may be close to unintelligible, let me offer three examples, the first two inspired by Tocqueville's writings.

If a King offers tax exemptions to the nobility but not to the bourgeoisie, the latter may react either by envy towards their rivals or by anger towards the King. Even if we cannot predict which of these two reactions will occur, whichever of them does occur can be explained by the King's behavior.

If a King enacts repressive measures, his action can make the subjects less likely to rebel, because the measures heighten their fear, and also more likely to rebel, because the measures increase their hatred. The net effect is in general unpredictable, but if in a given case we observe that repression causes rebellion, we can conclude that the second effect dominated the first.

As a third example, let me cite La Fontaine's dictum that "Each believes easily what he fears and what he hopes". Since the hope that a given state of affairs will obtain is equivalent to the fear that it will not obtain, what determines whether, in a given situation, the first or the second belief will be triggered? In the recent movements of the stock market there may have been a point when agents switched from unjustified optimism to unjustified pessimism, but as Alan Greenspan recently wrote, the psychology of the switch seems ill-understood.

To illustrate the ambiguity and indeterminacy of mechanisms, consider also the gambler's fallacy and its nameless converse. The purchase of earthquake insurance sometimes increases sharply after an earthquake, but then falls steadily as memory fades. Like gamblers who make the mistake of believing that red is more likely to come up again if it has come up several times in a row, the purchasers form their beliefs by using the *availability heuristic*. Their judgment about the likelihood of an event is shaped by the ease with which it can be brought to mind, recent events being more easily available than earlier ones. Conversely, people living in areas that are subject to frequent floods sometimes believe, or act as if they believe, that a flood is less likely to occur in year **n+1** if one has occurred in year **n**. Like

gamblers who make the mistake believing that red is *less* likely to come up again if it has come up several times in a row, they form their beliefs by relying on the *representativeness heuristics*. They believe, or act as if they believe, that a short sequence of events is likely to be representative of a longer sequence of which it is embedded. In casinos, players have been found to be equally vulnerable to either effect.

I shall cite many more mechanisms shortly. For now, I only want to state that the social sciences are cumulative in the sense of acquiring more and more mechanisms. Each new mechanism is added to the toolbox or to the repertoire of the social scientist. The progress is irreversible, since mechanisms identified by Aristotle, Montaigne or Tocqueville are still with us today.

I can now begin to answer the question in the title. My answer is that there is only *one social science*, but that it is *not unified*. In his massive treatise *Foundations of Social Behavior*, James Coleman argued that rational-choice theory could be a unified and unifying theory of all of social science. With one qualification, it did indeed hold out a *promise* of a unified theory, capable of generating unique predictions given initial conditions. The qualification arises from the fact that the theory fails to prescribe and predict behaviour if the situation is sufficiently complex. The current financial crisis illustrates this point perfectly. It is not so much that the actors are irrational, although they may be that too, as that *nobody knows what it would be rational to do*.

As a matter of fact, this was not news. Allow me to cite from my book *Explaining Social Behavior*, published in April 2007, four months before the subprime crisis broke:

Many models impute cognitive capacities to agents that they demonstrably do not possess. The point is so trivial that it is almost embarrassing: how can an economist assume that an agent has the ability to carry out calculations that he or she (the economist) needs many pages of highly technical appendices to spell out? The temptation is strong to say, "Come on! Get real!"

In his book *The Black Swan*, published around the same time, Nassim Nicholas Taleb made a different claim to the same effect. He argued, on the one hand, that the distribution of many important economic and financial variables is non-Gaussian and, on the other hand, that because of the nature of the distributions we will never have enough data points to estimate their parameters. In more technical work, Martin Weitzman refers to this problem as one of *structural uncertainty*.

Bracketing this issue of indeterminacy, the promise of a unifying theory that so entranced Coleman and many others was in any case never fulfilled. In many well-documented cases agents fail to live up to the prescriptions and predictions of rational-choice theory, *even when these are within their cognitive grasp*. In other words, they behave irrationally. In a general way, this, too, is not exactly news. The Allais paradox and the Ellsberg paradox, stated in 1953 and 1961 respectively, showed that most people violate a standard version of rational choice theory. However, for a long time these and other anomalies, such as the gambler's fallacy, were not taken very seriously, since nobody could propose an alternative theory to account for them. As you cannot beat something with nothing, and as rational-choice theory definitely was something, with many achievements to its credit, it remained in place as the dominant paradigm. Although irrational behaviour was recognized, it was viewed only as a residual category. There was no positive account of irrational behaviour. At the same time, rational-choice theory had – and still has – undisputed success in many policy areas. The assumption that economic agents *respond to incentives* has been shown to be valid in numerous instances.

The situation changed in the mid nineteen-seventies. In 1974, Daniel Kahneman and Amos Tversky published the first of their major papers on decision-making under uncertainty, in which they introduced the heuristics of availability and representativeness that I mentioned earlier. In 1975, George Ainslie resurrected the theory of hyperbolic time discounting proposed by R. H. Strotz in 1955, and showed that it could account for many puzzling inconsistencies in behavior. A later landmark was the 1979 paper by Kahneman and Tversky on prospect theory, one of the most influential papers in the history of economics and the one for which Kahneman, after the death of Tversky, received the Alfred Nobel Memorial Prize in economics. Incidentally, although this prize has mostly been awarded to

economists working firmly within the rational-choice framework, this was the only time it has been given for work that has generated verified predictions. By contrast, the Nobel Prize in physics is given *only* for such work, which explains why Stephen Hawking has not received it nor any string theorist.

In the 35 years that followed, the research program of behavioural economics has unearthed a vast number of positive mechanisms that generate irrational behaviour. Although it would be impossible to attempt a complete statement of the irrationality-generating mechanisms, I shall try to produce a representative shortlist. If we go by the literature, the two most important ones are probably loss aversion, an aspect of prospect theory, and hyperbolic discounting. In my view emotions are at least equally important, although for reasons I shall explain they have proved less tractable for experimental purposes. Among other mechanisms the following may be cited:

- the sunk-cost fallacy and the planning fallacy (especially deadly in conjunction)
- the tendency of unusual events to trigger stronger emotional reactions (an implication of “norm theory”)
- the cold-hot and hot-cold empathy gaps
- trade-off aversion and ambiguity aversion
- anchoring in the elicitation of beliefs and preferences
- the representativeness and availability heuristics
- the conjunction and disjunction fallacies
- the certainty effect and the pseudo-certainty effect
- choice bracketing, framing, and mental accounting
- cases when “less is more” and “more is less”
- sensitiveness to changes from a reference point rather than to absolute levels
- status quo bias and the salience of default options
- meliorizing rather than maximizing
- motivated reasoning and self-serving biases in judgment
- flaws of expert judgments and of expert predictions
- self-signaling and magical thinking
- non-consequentialist and reason-based choice
- overconfidence and the illusion of control
- spurious pattern-finding

As stated, this list is not very informative. I produce it mainly to underline the fact that unlike rational-choice economics, behavioural economics does not rest on a unified theory. Rather, it consists of a bunch of theories or mechanisms that are not deductively linked among themselves. *Yet there is only one social science, because all practitioners can use the same toolbox.* There is no reason why an economist should refrain from using a mechanism developed by an historian of Classical Antiquity. In fact, an accomplishment in which I take some pride is that in my book *Sour Grapes* I managed to communicate the idea of adaptive preferences developed by Paul Veyne to the community of economists.

In this perspective, human behaviour seems to be guided by a number of *unrelated quirks* rather than by consistent maximization of utility. In fact, there are so many quirks that one suspects that for any observed behaviour, there would be a quirk that fits it. Many mainstream economists seem to shy away from behavioural economics because they think it invites ad-hoc and ex-post explanations.

Another, similar source of resistance by the mainstream is due to the plethora of *motivations* invoked by writers within behavioural economics. As we all know, *homo economicus* is supposed to be not only rational, but also consistently self-interested. The second feature of his make-up is less central than the first. Gary Becker, a staunch defender of the rationality assumption, has done much to further the study of altruism in economics. Yet for three distinct reasons, most economists are more comfortable with the assumption that agents are self-interested.

They are so, first, because of the macho tradition of the discipline. As Robert Frank has written, “The flint-eyed researcher fears no greater humiliation than to have called some action altruistic, only to have a more sophisticated colleague later demonstrate that it was self-serving”. Economists often seem prone to what has been called “the hermeneutics of suspicion”. For instance, if economic migrants send remittances back home, the suspicion is that they are basically bribing their fellow citizens to remain in the old country so that they will not compete with them on the labour market in the new country.

Second, economists prefer the assumption of self-interest because in their role as institutional designers they want to *economize on virtue*. Following David Hume, they may start from the assumption that it “is [...] a just *political* maxim, *that every man must be supposed to be a knave*; though, at the same time, it appears somewhat strange, that a maxim should be true in *politics* which is false in *fact*.” Over time, they may end up believing that the fiction is actually true.

Finally, economists may assume self-interested motivations for reasons of theoretical simplicity and parsimony. Paraphrasing Tolstoy, every selfish person is alike, but all non-selfish persons are non-selfish in their own way. Behavioural economists have come up with an amazing range of non-selfish motivations, including altruism, envy, resentment, inequality-aversion, fairness, and many others. Once again, there is a suspicion that for any observed behaviour one can find a non-selfish motivation that would fit. And once again, the risk of ad-hoc and ex-post explanations seems very real.

I want to distinguish sharply, however, between ex-post and ad-hoc. Ad-hoc explanations are of course to be avoided. They are stipulated for the sole purpose of saving the phenomena. A genuine explanation has to do more than merely provide a hypothesis from which the explanandum can be deduced. Given any social event or any social fact, any social scientist worth his or her salt should be able to come up with half a dozen possible accounts that *could* explain it. To argue that one then *does* explain it, additional steps are needed. Plausible rival accounts have to be set up and then shot down, and additional testable implications of the favoured account have to be derived and verified. If these are “novel facts”, not previously observed, they lend even more strength to the explanation.

By contrast, there is nothing wrong with ex-post explanations provided that they follow the procedure I just stated. Let me take a trivial but typical puzzle, based on my own experience: why are there so many more standing ovations on Broadway today than twenty years ago. The playwright Arthur Miller proposed this explanation: “I guess the audience just feels that having paid \$75 to sit down, it’s their time to stand up. I don’t mean to be a cynic but it probably all changed when the price went up.” When people have to pay \$75 or more for a seat, many cannot admit to themselves that the show was poor or mediocre, and that they have wasted their money. To confirm to themselves that they had a good time, they applaud wildly. So far, this is no more than a “just-so” story, one possible account among many. It would gain in strength if it could be shown that there are fewer standing ovations when large numbers of tickets to a show are sold to firms and given by them to their employees. This would count as a novel fact. Even if these tickets are expensive, the spectators have not paid for them out of their own pocket and hence do not need to tell themselves that they are getting their money’s worth. Although this account can be buttressed by the theory of cognitive dissonance reduction, about which more later, it stands quite well on its own.

In my vision of the social sciences, micro-economics, updated as behavioral economics, and social psychology have a privileged role. They illuminate the individual choices and actions that are the building blocks of more complicated phenomena. Yet they face the challenge of how we link the behavior observed in the laboratory to spontaneous behavior outside it. Many critics deny that findings from an artificial experimental setting can be generalized to other contexts. Some of these objections are readily dismissed. In particular, the fact that most findings can be replicated with large monetary

stakes refutes the objection that the experiments are insignificant because they only involve trivial amounts of money.

Other objections may be more worrisome. In the post-Milgram era, scholars are prohibited from conducting experiments with high-stake *emotional* charges. Extrapolating from behavioural expressions of the positive affect subjects feel when given candy or when discovering that the pay phone already has a coin in it may not be justified. At the other end of the emotional spectrum, extrapolations from the behavioural expressions of negative affect generated by unfair behaviour in an Ultimatum Game or a Trust Game are not necessarily justified. Moreover, it is virtually impossible to recreate, inside the laboratory, the ongoing open-ended interactions that shape much of social behaviour. Finally, although experimenters take great pains to create a screen of anonymity to prevent irrelevant influences from distorting the findings, very few real-life actions are completely hidden from the regards of others.

To address these issues, psychologists and behavioural economists can and no doubt will refine their experiments. They ought also, in my opinion, go outside the laboratory. The great psychologist Leon Festinger can serve as an example. In the process of arriving at the theory of cognitive dissonance, he was influenced by a puzzling finding by an Indian psychologist, Prasad, who reported that the vast majority of the rumors that followed the great Indian earthquake of 1934 predicted even worse disasters to come. Here is the puzzle and Festinger's solution:

Certainly the belief that horrible disasters were about to occur is not a very pleasant belief, and we may ask why rumors that were 'anxiety provoking' arose and were so widely accepted. Finally a possible answer to this question occurred to us – an answer that held promise of having rather general application: perhaps these rumors predicting even worse disasters to come were not 'anxiety provoking' at all but were rather 'anxiety justifying'.

Although the theory of cognitive dissonance arose in response to a real-world puzzle, Festinger went on to derive and test additional implications in the laboratory. At the same time, he carried out fieldwork to confirm and develop the theory. He infiltrated a group of people who believed the world was about to end on a specific date and who had taken decisive action based on that belief, in order to observe what they would do when the prophecy failed.

If you do not know what they did, I shall not tell you. The book he wrote about it, *When Prophecy Fails*, is a wonderful read and I recommend that you find out for yourself. I mention the study here only because of the exemplary methodology it embodies, *combining theory, experiments and field work*.

Let me cite some recent research that proceeded in a similar way, the work by Linda Babcock, George Loewenstein and their co-workers on *self-serving conceptions of fairness*. Their theoretical framework is similar to, although not inspired by, that of the French moralists. Roughly, they assume that human beings have two central concerns: a concern with material self-interest and a concern with having an image of themselves as *not* concerned only with material self-interest. They want, for instance, to believe that they are capable of acting fairly and not only selfishly. Sometimes, we observe a tradeoff between these two concerns, as when people sacrifice some of their material welfare in order to be fair, but not as much as a strict conception of fairness would require. In other cases, they *bend* their conception of fairness to fit their self-interest. This is the idea of self-serving conceptions of fairness.

Babcock and Loewenstein carried out experiments in which subjects are assigned to the role either of plaintiff or of defendant in a tort case and asked to negotiate a settlement. They were also asked to predict the award of the judge and to assess what they consider a fair out-of-court settlement for the plaintiff. The study found that plaintiffs predicted higher awards and assessed higher fair-settlement amounts than defendants, and that pairs of subjects who reached more similar predictions and assessments were more likely to settle amiably than those who reached very different assessments. In a variant of the experiment, the subjects had to make their assessments of fairness before they knew whether they were going to take the role of the plaintiff or the defendant in the negotiation process. They found that there were four times as many disagreements when bargainers knew their roles initially as when they did not know their roles. Genuine fairness, it seems, is possible only behind the veil of ignorance.

These findings were confirmed by a field study of wage negotiations between teachers' unions and school boards in Pennsylvania. The researchers mailed questionnaires to representatives of these bodies in 75 school districts, asking them to list the districts they felt were comparable to their own for the purpose of contract negotiation, and also whether they tended to use teachers in other school districts or residents in their community for salary comparison purposes. Responses to both questions showed a strong self-serving bias. Moreover, the greater the differences between the answers from the two sides, the larger the likelihood that the negotiations would break down and that a strike would occur.

Once again, my concern here is not with the substance, but with the exemplary methodology, combining, as in Festinger's case, theory, experiments, and field studies. Many behavioural economists are content, however, with adding real-life *illustrations* to the experimental findings. Consider, for instance, sunk-cost fallacy, compellingly demonstrated in an experiment where one asked 61 subjects the following question:

"Assume that you have spent \$100 on a ticket for a weekend ski trip to Michigan. Several weeks later you buy a \$50 ticket for a weekend ski trip to Wisconsin. You think you will enjoy the Wisconsin ski trip more than the Michigan ski trip. As you are putting your just-purchased Wisconsin ski trip ticket in your wallet, you notice that the Michigan ski trip and the Wisconsin ski trip are for the same weekend! It's too late to sell either ticket, and you cannot return either one. You must use one ticket and not the other. Which ski trip will you go on?"

33 subjects chose the \$100 ski trip to Michigan 33, 28 the \$50 ski trip to Wisconsin. Clearly, those who chose Michigan wanted to minimize the amount of money they wasted, not to maximize their utility.

To motivate experiments on the sunk-cost fallacy or to illustrate their findings, scholars often cite examples such as the Vietnam War, the tunnel under the English Channel, or the Concorde aircraft. They rarely try, however, to demonstrate that these projects were actually caused by specific agents committing the fallacy. The fact that they *might be* does not prove that they *were*. To prove that they were, one would at the very least have to consider rival explanations and show that they are invalid.

Let me cite an example of how one can go astray in this respect. It is perhaps a little bit unfair, in the sense that the claim I shall contest is not central to the argument of the authors, yet it is a claim that they make. The example is taken from a study by Daniel Kahneman and Dale Miller on "norm theory", in which they assert a

Correlation between the perception of abnormality of an event and the intensity of the affective reaction to it, whether the affective reaction be one of regret, horror, or outrage. This correlation can have consequences that violate other rules of justice. An example that attracted international attention a few years ago was the bombing of a synagogue in Paris, in which some people who happened to be walking their dogs near the building were killed in the blast. Condemning the incident, a government official singled out the tragedy of the "innocent passers-by." The official's embarrassing comment, with its apparent (surely unintended) implication that the other victims were not innocent, merely reflects a general intuition: The death of a person who was not an intended target *is* more poignant than the death of a target.

The statement by the "government official" – it was in fact Raymond Barre, the Prime Minister at the time – is indeed *consistent with* the proposed explanation in terms of norm theory. It is also, however, consistent with an explanation in terms of an anti-Semitic prejudice. There is a considerable amount of evidence suggesting that Barre had an anti-Semitic bias, including his strong defense of Maurice Papon and a directive he signed in 1977 (later struck down by the Conseil d'État) that effectively cancelled anti-racist legislation from 1972. Moreover, Barre's actual comment was somewhat less innocuous than in the paraphrase of Kahneman and Miller. He referred to "the odious attack that intended to strike Jews on the way to the synagogue and that struck innocent French citizens crossing the street". As a matter of fact, the Jews in question were French too. In my view, this phrasing supports an explanation in terms of anti-Semitism. Although Barre may not have "intended" the implications that the Jewish victims were not innocent, and that they were not French, many studies show that prejudice often operates at an unconscious level. The claim that the official's comment

“merely reflects a general intuition” *may* be correct, but Kahneman and Miller do not show that it is more plausible than alternative explanations.

To conclude, let me refer once again to Leon Festinger. Amos Tversky once told me about a meeting he had attended with the foremost psychological scholars in the U.S., including Festinger. At one point they were all asked to identify what they saw as the most important current problem in psychology. Festinger’s answer was: “Excessive ambitions”. The social sciences more generally have been suffering from excessive ambitions. The aspiration of rational-choice theory to become the master theory of human behavior offers one example. Another is provided by the strong claims often made for statistical models. As emphasized by the late David Freedman, data analysis often aspires to do more than it can deliver. In one of his comments on the use of regression models in the social sciences, he asserted that in his view the truth of the matter is somewhere between the following: “regression sometimes works in the hands of skilful practitioners, but it isn’t suitable for routine use” and “regression might work, but it hasn’t yet”. I should add that this is an argument from authority, since I do not myself have the technical competence to criticize the statistical models that are the target of Freedman’s criticism.

If social sciences have to lower their aim, what should they do? Two proposals have been implicit in my talk: we should keep *accumulating mechanisms* and use them to carry out *fine-grained case studies*. In addition, statistical models should be kept simple and robust. In the words of Amar Bhidé, “Where the medians and means (and basic cross-tabulations) don’t persuade, the argument probably isn’t worth making”. Due diligence in data-gathering, or what Freedman called “shoe-leather” research, should replace endless tinkering with statistical packages.

Needless to say, simplicity and robustness are not enough: good ideas are also needed. To this end, I recommend that all social scientists spend a large part of their time immersing themselves in the classic writings of history, which can provide them both with the “telling detail” and the “provocative anomaly”. Thomas Schelling once told me that before writing *The Strategy of Conflict*, he read widely and randomly in military history. This is not the preparation current social science departments give their students. Within economics, economic history is almost at the bottom of the prestige hierarchy, just a notch above the history of economic thought. Within political science, students do read the history of political thought, but virtually no political history. In sociology, they may read Marx, Weber and Durkheim, but to the best of my knowledge little social history. Perhaps the best way of creating a unitary social science with a common language would be for all social scientists to have a grounding in history.

About the Author

Jon Elster holds the Chaire de Rationalité et Sciences Sociales at the Collège de France. He has published twenty-one monographs, which have been translated into seventeen languages. His most recent books are *Le désintéressement* (2009) and *Alexis de Tocqueville: The First Social Scientist* (2009). Among his main research interests are philosophical psychology and the comparative study of constitution-making.