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Choice-Centred versus Subject-Centred Theories in the Social Sciences: The Influence of Simplification on *Explananda*

Siegwart Lindenberg

The idea, originating in economics and forcefully brought back by Goldthorpe, that rational choice theory and large-scale data analysis are symbiotic, is very attractive. Rational choice is in dire need of *explananda* which can be provided by large-scale data analysis, while large-scale data analysis is in dire need of an explanatory device; at the moment, rational choice can provide this better than any alternative approach. However, this idea is limited. It can only be applied to situations in which the specific simplifications made in rational choice model-building do not seriously affect the *explananda*. Ignoring this complication has created a very unsatisfactory use of rational choice theories in economics and sociology alike: a choice-centred approach in which the main task is to show that choice theory can be applied to the phenomena at hand, rather than to advancing knowledge in the field that specializes in the study of the phenomenon. What is needed is a subject-centred approach in which the quality criteria are related to advancement in the field, with rational choice theory taking a back seat. For a subject-centred approach, we need to have so-called ‘feeder theories’ which inform us about the impact of model specifications on *explananda*. If the simplification disregards important tendencies of human behaviour (such as short-sightedness), it cannot even acknowledge *explananda* of the ways people try to deal with these tendencies (say, by norms that shorten the time-horizon). There can thus be a direct relationship between the kind of simplification and the reality under consideration. Feeder theories thus open up the possibility of studying the interactive relationship between rational choice theory and *explananda* (be they from large-scale data analysis, from experiments, or from historical descriptions) and avoiding a choice-centred approach in favour of a subject-centred approach.

Introduction

Protagonists of a rational choice approach in sociology have long argued that theory formation in the social sciences should concentrate on the specification of mechanisms that relate macro-variables that are of interest to the researcher. This strategy is preferable to theory formation in the form of hypotheses which causally connect macro-variables directly. My own argument for this has been around for a long time (see Lindenberg, 1983a) but because much of what follows here builds on it, it warrants a brief refresher at the beginning of this paper.¹ Causal

factor models in the social sciences are hopelessly incomplete, much more so than in other sciences. Replication research in the social sciences is rather disappointing. Results are not stable. We don’t know the conditions under which the stipulated causal relationships hold, we don’t know what conditions can change these relationships, and we don’t know what other factors are relevant for the maintenance of the relationship over time. In order to get even a modest grip on this incompleteness, one needs to specify the causal mechanism that links the macro-factors. Each macro-factor ‘freezes’ particular action situations for the individuals

involved, although these situations are variable, and it is the micro-level specification of this variability that sheds light on the conditions of stability of the relationship. Given that we only go to the micro-level in order to increase the depth of understanding of relationships on the macro-level, we have to choose a micro-level theory of action that allows us to go 'back up' from micro to macro as easily as possible. Such a theory, a so-called individual₂ theory, has to satisfy a number of requirements, the two most important of which are information efficiency (it requires little information per individual) and modelling efficiency (it allows explicit simplifications and stepwise replacement of less simplified assumptions²). At the moment at least, rational choice theories of action approximate these requirements better than psychological or interpretative theories of action.³ Thus, we need rational choice theories for a macro-sociology.

Goldthorpe (1996) endorses this view of rational choice sociology, but he goes an important step further. He maintains that the argument for rational choice sociology is seriously incomplete itself because it neglects the question of where the *explananda* come from. While it is correct to say that factor models are not a good way to go about building theories, these very factor models, when they are the results of the quantitative analysis of large-scale data-sets (QAD), are the main source of *explananda* for rational choice explanatory models (RAT). The important point here is that, according to Goldthorpe, something has gone wrong. RAT was seen as an alternative to QAD, whereas the two need each other.

Goldthorpe's point could be paraphrased by the motto: 'Without explaining something real, you don't really explain.' And there is a tendency for rational choice theory not to explain anything real without QAD. Goldthorpe quotes Hechter as saying that there are two possible grounds for advocating new theory: to explain a particular set of findings or to resolve a theoretical problem *per se*. Goldthorpe regrets that Hechter opted for the latter and he believes that things are going from bad to worse, beyond Hechter, by theory becoming increasingly detached from central substantive concerns. Thus, RAT and QAD need each other desperately.

There is much to commend Goldthorpe's view of this symbiotic relationship. However, as stated it will

not work. The failed symbiotic relationship between microeconomics and econometrics could act as a warning. It is useful to remember Schumpeter's optimistic assessment in 1948 of the role econometrics plays for economic theory:

Perhaps the most important progress that has occurred in scientific economics is the vast increase in our command over facts. All types of information about facts have increased beyond the boldest dreams of past generations but our epoch has been particularly characterized by an increase of *statistical* information which was so great as to open up quite new possibilities for scientific research. . . . Precisely because economic theory is only an instrument of research, it cannot produce concrete results without the facts that are supplied by statistics or non-statistical description.

He then adds, the alliance between statistics and economics was not complete until the emergence of modern Econometrics.⁴

Schumpeter's and Goldthorpe's arguments are quite similar. Yet, almost fifty years later, Goldthorpe does not seem to have any reason to praise economics as an example for sociology. In his view, economics is a prime example of the withdrawal from real substantive concerns. It is like 'playing tennis with the net down'.⁵ Seemingly, the joining of rational choice models with QAD does not *by itself* deliver the goods. What is missing? The remainder of this paper is devoted to providing at least a preliminary answer to this question.

Choice-Centred Social Science

As just mentioned, rational choice models are meant to provide a better grip on the mechanisms that relate causal factors at the macro-level. In that, they are superior to pure factor models. I would like to argue that the circumstances that made rational choice models popular also contributed to a limited usefulness of these models. Let me quickly sketch this argument.

There has always been a diversity of approaches within the social sciences, but, by and large, it is fair to say that until quite recently the various social sciences (excluding economics) were dominated by the idea that social facts, be it institutions (like language and school systems) or structures (like social

classes) impose themselves on the individual. Social facts can be represented as causal factors in path diagrams and regression models. The vehicles of imposition are role expectations and processes of socialization in which the individual learns to want to conform to these expectations (see Gadourek, 1982).

This approach worked well under two separate conditions.⁶ First, when the social world is quite stable, role expectations seemingly capture the most important features of action situations. Incompleteness of macro-relationships is quite limited and there is no particular incentive to explicate the mechanisms connecting the factors other than the general one of the learning of role expectations. Not surprisingly, the prime source of evidence for the success of this approach is the small, stable kind of society, often caught by anthropologists in the nick of time before it became unstable under the pressure of Western societies.

Second, as long as the most prominent paradigm of human behaviour is biological, with social inequalities believed to arise from inequalities of race, or intelligence, or other innate tendencies, the role-theoretic social fact approach will do very well as the purest and clearest kind of counter-position.

In the first half of the twentieth century, the social fact approach was boosted by powerful racist regimes and widespread anti-semitism. The boost was so strong that biological explanations of social inequalities had become quite unacceptable (until very recent times). As a consequence, the social fact approach was left without a target. In this situation, its survival rested on its applicability, which, in turn, depended on stability. Given rapidly changing conditions in many Western countries, it is not surprising that another approach, one better equipped to handle the influence of changing conditions on behaviour, began to become prominent in the social sciences. Rational choice theories are able to map the influence of changing constraints on behaviour and are therefore superior to role-theoretic formulations when constraints are not stable. For example, family and labour-market situations are quite changeable these days and what a woman will do is better represented by her choice under constraints than by what society considers right.

Rational choice sociologists as well as economists rushed in to show that the rational choice approach was indeed better able to handle the

explanation of behaviour under changing conditions. For example, in the first excitement about 'the economic approach', Opp had argued that a hypothesis of the kind 'when the expected utility of activity *A* is higher than the expected utility of activity *B*, then we will observe *A*' has a high empirical content because with it 'we can explain the emergence (and change) of *all sorts* of activities.' (Opp, 1979; 231). Given such a hypothesis, all we seemingly have to do is to specify what the expected utilities are. This specification was thus a secondary activity, added to the primary activity of demonstrating choice.

In economics itself, a different variant of the same point was used by economists who were eager to show that the tools of economics could be used profitably outside the proper field of economics. The endeavour had been called 'economic imperialism' and much was expected of it. For example, in 1987 Radnitzky and Bernholz edited a book with the title *Economic Imperialism. The Economic Method Applied outside the Field of Economics*. On the back of the book, a quote by the prominent economist Armen Alchian illustrates the point: 'That it is the underlying theory of behaviour, rather than economics itself, that is imperialistic and powerfully productive is elegantly and instructively demonstrated in this . . . collection of essays.' The editors added some promotional text of their own. For example: '[T]he internationally known contributors maintain, the fundamental tenets of economics can be applied to practically any field of knowledge no matter how seemingly unrelated to monetary concerns: to biology, the social sciences, law, politics, psychology, and even methodology in scientific research'. It is quite evident that the spirit of economic imperialism is the conviction of the superiority of the economic approach. The first order of business is thus the demonstration that one can make a seemingly novel argument using the economic approach, even where people did not expect this approach to be applicable at all. As the editors advise the reader: 'Prepare to be surprised, delighted, challenged, occasionally disturbed, and most of all deeply informed by the rare and exceptional ideas in this ground-breaking book.'

The excitement generated by economic imperialism lies in its ability to show that things can be seen in a different light, that things do make sense when

looked at in terms of cost, benefit, and exchange. For example, marriage used to be thought to have something to do with romantic love, but one can look at it as a transaction in a market in which potential partners offer certain rights. This economic kind of analysis can be applied to all the traditionally sociological topics: religion, education, crime, suicide, quality of life, divorce, the family, organizations, institutions, art, norms, solidarity, etc. Tullock and McKenzie summarize this expanding effort by saying of economics that

[t]he change in direction and scope of the discipline has been so dramatic that the economists who have been involved in bringing about the change are no longer inclined to debate the issue of what is or is not economic in nature. They merely ask 'What can economics contribute to our understanding of this or that problem?' (Tullock and McKenzie, 1985; 3)

This is a success of sorts, but in this phase of development the criterion for success is the demonstration that the calculus of alternatives and choice can be applied to all these topics. The success criterion is not related to the overall development of the field or to the results of critical testing. Let me call this kind of endeavour *choice-centred* theories of social phenomena. In political science, a very similar critique of rational choice models with regard to empirical import has recently been voiced by Green and Shapiro (1994). According to these authors, rational choice models in this area 'do little more than restate existing knowledge in rational choice terminology.' (p. 6) The point made in the present paper is that the very existence of this kind of theorizing is due to a phase in the development of the social sciences. By itself it says little about the potentials or limitations of a rational choice approach as such.

Why did the economists let such an approach develop? Seemingly, the choice-centred approach fits in well with what economists were doing on their own terrain. There were two mutually reinforcing traditions in economics that favoured choice-centred theories. For one, information on prices, supply and demand of consumer goods, quantities of money, etc. has long been routinely gathered by the state authorities. Economists could use these data-sets and were used to limiting their theories to

what was available in the data. This situation was not experienced as a restriction because the strong modelling tradition in economics had long favoured a strategy of Simple Constraint Modelling (see Lindenberg, 1995a). For this reason, there is very little countervailing force within economics against choice-centred theories outside the field of economics. Within the field of economics itself, modelling requirements drive all bridge assumptions towards extreme simplicity. Assuming full information on goods and prices is very convenient for modelling purposes, as is the sole consideration of money as a constraint and the sole consideration of consumer goods with regard to preferences. Progress has been made within economics by relaxing these extreme assumptions to some extent. The introduction of information costs, of time as a constraint, and of shadow prices for goods other than consumer goods have opened up many new lines of investigation. But even here, modelling requirements encourage us to keep the context-dependency of actors to a minimum, even where substantive considerations would argue for adding more complexity. Simplifying assumptions exert an influence towards still more simplified assumptions for the sake of consistency. Take, for example, the issue of the relationship of utility and morality (see Lindenberg, 1983b). Conformity to market rules is generally assumed in microeconomics (unless deviant behaviour is specifically the subject of investigation). Individuals are seen 'as playing a game with fixed rules which they obey. They do not buy more than they can pay for, they do not embezzle funds, they do not rob banks.' (Diamond, 1971). This assumption is convenient for modelling purposes because it allows a very straightforward interpretation of prices and income constraints. The way this convenient assumption is squared with the other simplified assumptions is to add yet another very simplified and restrictive assumption: namely, that the price of deviance from the market rules is infinite. There is no question that economists know that this assumption is a very strong distortion of reality. My point here is that by and large, the tradition of Simple Constraint Modelling gives legitimacy to making such strong simplifications for the purpose of modelling with the implied hint that later on, these assumptions can be relaxed again. Simplification is seen as a way *how* phenomena are

studied: you see how far you get with simple and therefore tractable assumptions. There is certainly some virtue in this view, and I have myself defended the tractability of models, albeit in a modified way as a worked-out method of decreasing abstraction (see Lindenberg, 1992) and I would still defend it today. However, what is missing from this view is the critical evaluation of the influence of simplification on *what* is being studied. As I will argue in the remainder of this paper, simplification has a powerful influence on the description of reality that is to be explained rather than just an influence on the explanation of this reality. It is the disregard of this powerful influence of simplification that strongly restricts the usefulness of rational choice models in general and of choice-centred theories outside the field of economics in particular. No quantitative analysis of large-scale data-sets will by itself dislodge this influence.

The Influence of Simplification on *Explananda*

There are various kinds of simplifications and each may have a different influence on *explananda*. For the purpose of this paper, I would like to concentrate on two important classes of simplifications:

1. simplification in the number of constraints;
2. simplification in the kind of constraints considered.

I would like to discuss them in that order.

Simplification in the Number of Constraints

Let me begin with an example. Probably due to the weakening of sociology in the 1960s and a major surge towards economics as a policy science (as described above), by the late 1970s the prominent view of crime in the USA had shifted from a therapeutic approach (as favoured by many sociologists) to one of deterrence (as favoured by many economists). For economists, it was one of those fields that lay outside the traditional disciplinary boundary and that invited choice-driven theories that showed a negative influence of severity of punishment on crime. The balance of the evidence gathered in the USA on the influence of the severity of state-administered punishment on the crime-rate

favoured the expected negative relationship, and it seemed that we had learned something. The policy consequences also seemed obvious: harsher punishments. Tullock and McKenzie, pushing choice-centred theories in general, were quick to conclude: 'If punishment deters crime, then the sociological approach falls to the ground, and much of the advice given to governments by sociologists over the past 50 years is clearly wrong. Indeed, such advice might well be one of the major reasons for the rising 'crime rate.' (Tullock and McKenzie, 1985: 135).

It is entirely possible that the therapeutic approach (favoured by many sociologists) is wrong, but the question is whether the economic analyses have demonstrated that and whether they warrant the policy recommendations of harsher state-administered punishment. Doubts on this score were voiced by the very people who summarized the evidence (Blumstein *et al.*, 1978). It seems that it is clear enough what we are talking about if we use the terms 'crime rate' and 'punishment'. But the reality described by these terms is far from being clear-cut. The crime investigated by economists is almost exclusively crime that can be related to maximum years of imprisonment and that leads to convictions. If there is any reluctance on the part of judges or juries to find somebody guilty or to convict somebody that is related to the severity of the sentence then that alone would produce a spurious deterrent effect of punishment on crime, although it has little to do with the incidence of the 'criminal' act. If we imagine a situation where prisons are crowded and many potential candidates for imprisonment are released, might it not be that the releases are determined according to the severity of the crime, which in turn would be measured by the severity of the maximum punishment?

If we turn to the person who has committed the act, we also find a far greater complexity of constraints than the maximum prison sentence. An economic analysis cannot tell us how much of the presumed cost of committing a crime comes from anticipated punishment and how much from other sources, such as the loss of social approval when caught, the power differential between criminal and victim (say, whether people move about alone or in groups), observability (say, the presence of street lights), whether potential victims carry a purse, and so on (see Gottfredson and Hirschi,

1990: 28ff). When the maximum punishment for a certain criminal activity is increased, what are the individual's alternatives to committing this activity? Some might shift to other kinds of gainful crime which is more difficult to detect⁷ or in which it is difficult to be personally identified. Others might commit even larger crimes because punishment for these acts now becomes *relatively* less harsh.⁸ In the process, they will now have a higher incentive to silence their victims. In addition, those who have suffered the harsher punishment might be more likely to commit a serious and risky crime again.⁹ What would be the *net* result of a harsher punishment of the activity in question? Which increase in harshness would have what kind of net effect?¹⁰

I use these examples to make the point that it is extremely doubtful whether there is something like an *explanandum* called 'crime statistics' produced by the tradition of quantitative analysis of large-scale data-sets, to be explained by rational choice theorists. Such statistics may be gathered routinely in the political process and may be influenced by the implicit or explicit guidance of policy advisers. It is the Simple Constraint Modelling of economics that renders a particular 'crime statistic' an *explanandum*, and a very limited one at that.

Simplification in the Kind of Constraint

Here, too, the easiest way to introduce the main issue is to give an example. Above, it was mentioned that economists often assume rule conformity by implicitly or explicitly assuming the price of deviance to be infinite. Williamson (1985) has shown that this simplifying assumption, together with the assumption of unbounded rationality (complete information), masks one of the most central activities in economic transactions that are extended over time (and thus are subject to uncertainty constraints): the efforts made to deal with the opportunistic tendencies of the other. These efforts can be summarized as 'creating credible commitments' and the costs of achieving them, so-called transaction costs, are costs above and beyond the production costs of the goods exchanged. Opportunism here refers to 'the incomplete or distorted disclosure of information, especially to calculated efforts to mislead, distort, disguise, obfuscate, or otherwise confuse.' (Williamson, 1985: 47). Information

asymmetries are thus created and block otherwise profitable long-term transactions unless they can be overcome by devices that make the commitments credible. From this starting-point, Williamson unfolds a theory of the institutions of capitalism, most of which have to do with the creation of credible commitments for transactions between and within organizations.

It is very unlikely that large data-sets, produced independently of the theory of transaction costs, would have yielded the *explanandum* of governance structures for contracts under uncertainty, to be explained by rational choice theory. But one can push the example even further. Williamson was willing to go against some simplifications of the orthodox neoclassical theory. Yet, he kept more simplifications than he rejected. Most importantly, he kept the simplifying assumption that people are far-sighted in their transaction. There is considerable evidence to counter this simplification (see, for example, Loewenstein and Elster, 1992); people seem to be quite myopic. But Williamson had the same reasons for holding on to it as other economists have for holding on to simplifications: 'But for farsightedness, transaction cost economics would be denied access to one of the most important "tricks" in the economist's bag, namely the assumption that economic actors have the ability to look ahead, discern problems and prospects, and factor these back into the organizational/contractual design.' (Williamson, 1993: 129). Unless one assumes far-sightedness, one cannot use the standard instruments of neoclassical theory, and the price for that may be high in terms of the tractability of assumptions, especially for an efficiency framework. Thus Williamson sticks his neck out against neoclassical theory, but at the same time he would like to make use of that theory.

As a consequence of holding on to far-sightedness, Williamson's 'opportunism' has virtually no bite. People try to lie and distort for their own advantage, but if they truly have a profitable contracting opportunity with a partner, they don't gain very much by lying and the far-sightedness of the other reduces the possible advantage of lying even more. Had Williamson been willing to consider myopic tendencies, he would have had a much more damaging kind of opportunism: the tendency to give in to short-term temptations against one's

own long-term interest. Again there are very clear consequences for the kind of reality that is being studied. For example, in a 'far-sighted' transaction-cost framework, the instruments governing employment contracts are all related to long-term consequences, such as job security, promotion schemes, and seniority pay. In a myopic framework, the more important instruments for such contracts are short-term rewards (especially social approval and disapproval), shortening of time-horizons (through deadlines), and informal control networks (see Lindenberg, 1995*b* for more detail). *Notice that there is a relationship between people's tendencies to act and the problems people have to solve when they interact.* Thus, if I simplify by disregarding important tendencies, I also disregard the problems people will deal with in their transactions. There is a direct relationship between this kind of simplification and the reality we have to deal with. The view that one party (say the QAD people) supplies the *explananda* and the other party (say the RAT people) supplies the explanation is likely to hold only in some (lucky) cases. For the sociology of organizations, it is clear that there would be no such lucky coincidence. In neoclassical theory, descriptions of governance structures were not asked for because there was nothing to govern. In transaction-cost economics, the description of governance structures only contains long-term instruments, and in a myopic framework, the description of a governance structure would have to include a variety of short-term instruments, especially relational signals.

Subject-Centred Theories and Feeder Theories

If our intention were to find an alternative way of studying crime, we would need a theory that guides the collection of data and that makes sure that the data contain a variety of constraints on the same individual. In contrast to the tradition of economics, one would not rely on data routinely gathered by state authorities or gathered as the by-product of political processes. The very phenomenon 'crime' would have to be constructed in the light of the theory, and different data-sets might have to be produced for different subquestions. However, data not suggested by the theory in question but by other theories have to be known and dealt with as well. For example, data that don't make any economic sense would have to be acknowledged by economists. It

is known that the modal age for burglars in the USA is 17, with the rate of burglary declining rapidly with age. 'The most likely "pecuniary" outcome for a burglar is *no* gain, and his next offense is likely to be something other than burglary', an offence equally unlikely to make economic sense (Gottfredson and Hirschi, 1990: 74). These findings do not fit the assumption of gainful criminal activity made by many economists.

The person developing a choice-centred theory must be an expert in choice theory and in the translation of some aspects of the field into the parameters of this theory. The standard of success is how well the applicability of rational choice theory has been demonstrated in the field. However, in order to develop a theory with complex constraints, and especially in order to know the relevant findings that are not related to the theory, a rich background knowledge of the field is necessary in addition to the theoretical skills. It is clear, then, that the criterion of success is not whether rational choice can be applied, but whether the frontiers of the field have been expanded. I would call theories developed in this context, 'subject-centred', because the subject-matter embedded in an ongoing field of inquiry is the central focus and the source of success criteria, quite contrary to choice-centred theories.

The most likely solution for meeting this requirement of subject-centred theorizing would seem to be teamwork between field experts and experts in rational choice theory. For Complex Constraint Modelling, it is probably useful if the rational choice expert were not just an expert in choice theory but in the theory of choice in the context of interdependencies (such as game theory). However, this will work best only where the rational choice theorist is unwilling to consider a variety of constraints simultaneously and does not have the background knowledge to be confronted with 'difficult' data. In order to come to grips with the influence of simplifications of the kind of constraints an additional effort is needed. We need theories which are explicitly directed towards the question of what bridge assumptions in rational choice theory are simplifications, how serious these simplifications are, and what would have to be assumed if one were to replace them with less simplified assumptions.¹¹ For a subject-centred project, such theories can be considered to be 'feeder theories' with an important

impact on the kind of reality that needs to be described by data.

Let us take an example related to what we have said above. A theory of opportunistic behaviour would radically change what we look at when we study the workings of organizations. Although opportunistic behaviour can be observed every day and belongs to the stock of common-sense knowledge, it would never do to introduce it as a common-sense bridge assumption because of the very fact that such an assumption would have so deep an impact on research. Common-sense also informs us that people are not opportunistic all the time and there are prestigious voices pulling the solution in that direction. For example, Buchanan threw his prestige as a Nobel laureate behind a very different solution to the simplifying assumption of conformity to market rules. He argues that the attempted separation between economics and morals was 'at best, an illusion that simply cannot be sustained.' (1994: 128). And he suggests that we include moral tastes in the utility function. A good acquired by deviating from the market rules (say, by cheating) would then be worth less because it was acquired by cheating, which explains conformity to market rules. To me, this solution is not very satisfactory, although it does at least attempt to bring morality back in to economic reasoning. It negates opportunistic behaviour altogether. As long as the debate rests on the basis of the opinions of prestigious economists, everybody feels free to go ahead with business as usual. Only the weight of research findings and of theory on the conditions under which we can expect opportunistic behaviour to occur or not occur would be strong enough to pull off such an important swing in the research agenda. Serious work on feeder theories is essential for this purpose. At the present time, it looks as if the most relevant foundation is research on short-term vs. long-term tendencies in decision-making.

Another example of a feeder theory concerns the role of information. The most simplified assumption is that individuals are completely informed about alternatives prices, the first limitation on that assumption being that information itself is a scarce good. Here, feeder theories have been developed on how individuals deal with lack of information, and we have seen that, for example in Williamson's work, information asymmetries could be tied to a theory of

institution-building. There is, however, another aspect related to information that may have an even stronger impact on the research agenda: the possibility that individuals do not make use of information they have, that they attend selectively to situationally presented information. In the literature, this phenomenon is called 'framing.' The consequences of framing could be enormous for rational choice theory because it basically says that certain cost-benefit aspects are more or less screened out of the decision-making context. Prediction of what will happen now depends very much on which aspects are screened out. The data will have to include information on influences on framing. For the study of organizations (to stay with our example) this would mean that governance is also possible by the strategic effort to screen out costs of conforming and benefits of non-conforming behaviour, a complicated process that has been described in terms of relational signals (see Lindenberg, 1993).

Yet another issue with feeder theories is related to the fact that predictions of rational choice theory necessarily involve assumptions on preferences. The simplifying assumption that people care only about material consumer goods clearly has crippling consequences for the research agenda outside the most narrow field of economics. But what should replace it? There is no room in this article to go into this question. Suffice it to say that there are serious attempts to find an answer in the direction of radically changing the borderline between preferences and constraints by considering the individual to be much more a producer than a consumer (see, for example, Stigler and Becker, 1977 and Lindenberg and Frey, 1993). Such a change in the location of the borderline between preferences and constraints (in favour of constraints) also radically affects what information we would have to have in order to apply rational choice theory (especially for the prediction of processes of substitution, the heart of rational choice theory).

All these examples point to an area of theory formation that stands between rational choice and the analysis of reality. Its function is decidedly not just to make rational choice theories better able to deal with the *explananda* that are offered by people who have made it their business to describe reality by large data-sets or in any other fashion. Rather, feeder theories have a profound impact on the research agenda

that details what needs to be studied. There has to be an interactive relationship between the development of rational choice theory and the production of *explananda*.

Summary and Conclusion

Goldthorpe has agreed with a growing number of scholars who have argued that quantitative large-scale data analysis needs a theoretical approach to be able to deal with multiple constraints, and that rational choice sociology is likely to be the most appropriate approach for this purpose. However, Goldthorpe has gone an important step further in saying that, by and large, rational choice theorists are not much concerned with the explanation of real puzzles and, therefore, that they need quantitative analysis of large-scale data-sets to provide the real-life *explananda*. For some areas this may be immediately obvious, and Goldthorpe has put his finger on a very important problem in the development of both rational choice and large-scale data-set analysis. Attractive as this symmetry and symbiotic argument about the two ways of doing research is, it lacks a very important ingredient. We have seen that in economics, a similar argument about rational choice and the quantitative analysis of large-scale data-sets was made almost fifty years ago. It did not have the promised result and it is very useful to speculate why that may be so. The major arguments are three-fold. First, that most rational choice models are choice-centred rather than subject-centred; second, that there is too little awareness of the impact of simplification on the definition and description of what is to be explained; third, that we need feeder theories that will tell us what the impact of simplifications on *explananda* is.

With regard to the first point, it was argued that there were a number of reasons for the decline in traditional sociological theory. On the one hand, its major target, biologism, has been conquered. On the other hand, conditions have become too complex and changeable to be tractable by the stable effects of socialization on behaviour. There was a rising demand for theories that were able to handle the influence of changing constraints on behaviour. Rational choice sociologists and economists moved

in to show that rational choice models can actually handle changing constraints rather well. In this phase, the emphasis was on the demonstration that even outside economics, phenomena (virtually all social science phenomena) could be plausibly analysed in terms of alternatives (constraints and cost-benefit balances) and choice. The criterion for the success of this endeavour was how well the 'old' phenomena could be restructured in terms of rational choice theory. The success criterion was decidedly not related to the frontiers of the field of inquiry or to results of empirical tests. In a word, during this phase, rational choice theories of non-market phenomena were choice-centred, not subject-centred.

Choice-centred theories fitted well with two mutually reinforcing traditions in economics. First, bridge assumptions are pushed towards extreme simplicity for the purposes of model-building (so-called Simple Constraint Modelling). Second, data-sets are gathered mainly in the administrative and political context. They do contain price, demand, and supply information but little else, which is fine for Simple Constraint Modelling. Thus there is little push from economics itself to change from choice-centred to subject-centred theories.

With regard to the second point, it was argued that simplifications have a profound effect on the question of what needs to be explained. There are two major effects of this kind. Simple Constraint Modelling steers data collection or selection towards the exclusive consideration of a few simple constraints and thus only yields *explananda* that are couched in terms of these constraints. In this paper, this was illustrated by economic studies of crime. In many cases this will simply not yield *explananda* that are interesting in the field itself. Interactions between constraints are missing and confrontation with data that do not make sense in the light of the theory is also lacking in choice-centred theories. Even more important is the impact of simplifications on the kind of constraint. These are mainly simplifications in the action theory itself. In this paper, this was illustrated with the example of opportunistic behaviour. From this perspective, we need very different descriptions of what goes on in and between organizations in terms of governance of contracts. Institutional analysis will be especially strongly affected by the impact of simplifications

because there is a relationship of people's tendencies to act and the problems people have to solve when they interact. If simplifications take the problems that exist in interaction away (as would be the case if we abstract from opportunistic behaviour), their solutions will also not appear on the research agenda.

The third point made in this paper was that because simplifications have such a profound impact on the research agenda (and thus on *explananda*), it will not do to leave them in the realm of the opinions of prominent economists. As long as the problem is left in this realm, people will feel free to go on as usual with the traditional simplifications. Only theories on simplifications backed up by considerable research will have the weight necessary to swing research agendas. Such theories (for example on opportunistic behaviour, on short-term vs. long-term rationality, on framing, or on preferences) can be seen as feeder theories for subject-centred projects. They must have a legitimate place in the empirically oriented rational choice approach and they should not be relegated to psychology departments because psychologists are not concerned with the consequences of simplifications for social science *explananda*. Feeder theories are a legitimate field that overlaps sociology, economics, political science, anthropology, and psychology.

All in all, it can be said in response to Goldthorpe's suggestion that it is true that rational choice needs to change its ways and to deal with real problems. But for this to be realized, we need to accept the fact that the way we formulate and apply rational choice theories profoundly influences the *explananda* in the social sciences, except maybe in some lucky cases. The view that one group (the QAD group) produces the *explananda* and the other (the RAT group) produces the theories – a view already held by economists fifty years ago – will not lead to much advance for either qualitative analysis of large-scale data-sets or rational choice theories. What is needed is thus first, a separate effort to develop feeder theories, and second, an interactive development of *explananda* in which rational choice theorists (equipped with feeder theories), field specialists, and experts in data gathering and statistical analysis join forces in an iterative process of theory formation and *explananda* creation.

Notes

1. In this volume, Esser follows the same argument.
2. This is the so-called method of decreasing abstraction: see Lindenberg, 1992.
3. That does not mean that rational choice theories are satisfactory the way they are. As I will argue below, they need elaboration for the very purpose of explaining relationships at the macro-level.
4. This is quoted from a lecture course in January 1948 entitled 'The progress of theoretical economics during the last twenty-five years' and reprinted in Schumpeter, 1954: 1141.
5. Goldthorpe quotes Blaug's wonderful metaphor here.
6. These arguments are made in greater detail in Lindenberg, 1985.
7. Notice that this would contribute to a spurious deterrent effect of punishment.
8. 'Now I know if I's gonna rob somethin' it ought to be big, because I'm gonna get the same time.' Tullock and McKenzie (1985: 132) quote this statement of a young criminal without drawing any conclusion from it.
9. 'A person who may be depressed and care very little about living can hardly be expected to divert many resources to avoid being killed' say Tullock and McKenzie (1985: 129), but they do not apply this insight to their own analysis of the side-effects of punishment.
10. Even more sophisticated economic analyses of crime (such as Becker, 1968: ch. 4 and Ehrlich, 1973) generally do not ask these questions.
11. This was the main point of my suggestion of working out the method of decreasing abstraction (Lindenberg, 1992). You need to know what simplification to replace and by what; therefore additional theories must be in place. However, in elaborating this method I did not touch on the relationship of simplification to *explananda* and data.

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