

Homo Socio-oeconomicus: The Emergence of a General Model of Man in the Social Sciences¹

by

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“Nothing is more fundamental in setting our research agenda and informing our research methods than our view of the nature of the human being whose behavior we are studying.” H. SIMON [1985, 303]

1. Introduction

Bounded rationality is by now a term so widely accepted that it is probably here to stay. Yet this linguistic success is deceptive. In fact, Herbert Simon’s suggestions for a “turn” in economic theorizing has been widely discussed but have not really changed the bulk of microeconomic theorizing. EINHORN and HOGARTH [1982, 34] suggested that there is hope because there is finally a *comprehensive* alternative to price theory, namely Kahneman and Tversky’s prospect theory. By now we know that the prospect theory has not been able to dislodge price theory and it does not look as if this would happen in the future.² Why don’t the economists want to listen? Why has the “behavioral uprising in economics” (see GILAD et al. [1984]) not produced more change in theorizing? Are microeconomists stubbornly holding on to the neoclassical *homo oeconomicus* until cognitive psychology made sufficient advances to make a swap profitable? If so, there should be a clear indication in this direction because according to Simon’s assessment five years ago, cognitive psychology has grown up and “is no longer limited to dealing with ‘toy’ tasks ... in the laboratory, but can give rather impressive accounts of adult performance in professional-level tasks” (SIMON [1985, 295]). Yet, there are no indications that only the diehards hold on to *homo oeconomicus* rather than switch to psychological theories of decision-making.

¹ Earlier versions of this paper were presented at the workshop “Emerging Issues in Demographic Research”, September 15, 1989 in Utrecht and at the session VI-4 “Conceptions of Human Nature”, XIIth World Congress of Sociology (ISA), Madrid, 9–13 July, 1990.

² See the discussion of prospect theory in HOGARTH and REDER [1987], and the recent change in emphasis from prospect theory to scale compatibility in TVERSKY et al. [1990].

The answer given in this paper to these questions is complex but rather radical. It consists of a number of interrelated points. First, while developments in psychology are certainly important also for economics, the really important paradigmatic adjustments do not occur in the relation of economics to psychology but in the relation between economics and sociology. Second, the model of man used is indeed as important as Simon indicates in the motto to this article, but the discussion of the merits of a model of man must not just be placed in the context of the "realism" of the theories involved but also in a methodological context. What has to be achieved by using the model of man? In economics, the model of man must first of all be able to provide theoretical guidance for analyses in very different contexts. If this is considered then it becomes obvious that Simon's or Kahneman and Tversky's models will not have much chance of displacing *homo oeconomicus*. Rather, the new model of man for the socio-economic sciences must be able to combine flexibly the function of theory guidance and the possibility of approximating reality in successive steps. Third, the new model of man should be able to incorporate the specification (or definition) of the situation when this is necessary for the analysis.

In the last ten to fifteen years, important steps have been set in the direction of *homo socio-oeconomicus*, a model of man that is informed by the key issues of convergence between sociology and economics, viz. the importance of model-building and the definition of the situation.

In the following, I would first like to take up the question why the old division of labor between sociology and economics has broken down, and then talk about the *homo socio-oeconomicus* and its methodological embedding.³

2. The Relation Between Economics and Sociology

For about a century two models of man coexisted side by side: the *homo oeconomicus* and the *homo sociologicus*. The former is an all-informed, strongly consumption-oriented maximizer, the latter is a socialized, norm-oriented conformer. Although these two models are very different, they strongly depended on each other due to the fact that economics and sociology were locked into a curious division of labor in which economics would take the "rational" or "logical" side of man and sociology would take the (complementary) "non-logical" side. Due to various developments described in this section, the division of labor broke down and with it the neat duality of models of man.

The convergence between economics and sociology has advanced by leaps and bounds in the recent past, reversing a secular trend of divergence and erasing a longstanding tacit division of labor between them. New forms of cooperation and competition emerge.

³ Many of the following points have been dealt with in some more detail in LINDENBERG [1983 a] (which is partially reproduced in LINDENBERG [1985 b]) and LINDENBERG [1985 a].

This change did not occur over night but, although it started about thirty years ago, it only picked up momentum in the last ten to fifteen years, which is quite a short time given the long history of divergence and the tenacity of the conventional division of labor: tidy models and rational behavior for economics, non-rational behavior and messy social reality for sociology. This division worked well for both sides. It exempted economists from gathering and analyzing data that their models could not have dealt with (such as data on religion, tradition, ideology, self-sacrifice, mass appeal, issues of fairness, norms, complex interactions, networks, power relations, self-conceptions and reference groups) and it exempted the sociologists for obvious reasons from formulating rigorous models. This arrangement also had a conservative influence on the development in each field because it forced economists and sociologists alike to stay well within the boundaries of their spheres of research (market versus non-market). Even if they would have liked to stray into the area of the other, it would have been incompatible with the division of labor, because at least by implication, scholars in both disciplines had to assume that man is dual-natured and that the rational side comes to the fore in the market place and the non-rational side in all non-market relations.

By scientific standards, this arrangement was quite precarious because it was wrought with internal contradictions. The behavioral theories in both disciplines were elaborations of either rational or non-rational man and they could not cope with dual-natured man. No theory within neoclassical economics could specify the conditions under which man the rational maximizer would turn into non-rational man. For traditional sociology, the only way to cope with dual-natured man was to make rational behavior itself a norm that governs roles in the market place. This however left the problem unresolved how somebody who is able to behave like a rational man in the market place can be restricted to non-rational behavior in all other social relations.

2.1 Protective Belts

The scientific problems inherent in the assumption of dual-natured man are so volatile that in all likelihood they would have exploded long ago had it not been for some extra protective belts around the cores. In economics, the main protection consisted of the high costs of giving up what was becoming increasingly *more* important for the professional self-image of the well trained economist: rigorous model building. True, there were cries for more consideration of sociological insights, but they were no threat to the protective belt. More likely, these cries were themselves part and parcel of the protective strategy by having all the markings of a periodic ritual to be performed mainly in obligatory articles for state-of-the-art-conferences.⁴ Why only ritual cries? Because –

⁴ see for example KRUPP [1966]; HUPPES [1976]; BELL and KRISTOL [1981] and THUROW [1984].

due to the existing division of labor – there really was no methodological instrument to make any kind of systematic use of sociological insights in microeconomics.

In sociology, the situation was quite different. Here, the major stabilization of the arrangement with economics came from a second frontier which turned out to be the major battle ground for sociologists. An understanding of this frontier, which was established long ago, is the key to the understanding of the new situation today. The discipline that has emerged as “sociology” (and which I call “traditional sociology” for ease of identification) is the result of a considerable competition of approaches for the explanation of social behavior and other social phenomena in the late 19th and early 20th century. There were approaches that modeled social mechanisms as analogies to physics or meteorology or biology.⁵ Psychology was also used as the theoretical foundation of sociology.⁶ There is even an instinctivist variant of the psychological school.⁷

In this fierce competition, slowly but surely, yet another approach seemed to have won out against all others: the radical view, not found in any other discipline, that social behavior and all other social phenomena were themselves caused by social phenomena and only by social phenomena. The success of this approach was in no small part due to the apparent weaknesses of the other approaches. The strategy used by sociologists to expose these weaknesses was four-fold. First, they fashioned a model of man that would maximally support the radical view, viz. role-playing man⁸ which renders social behavior completely dependent on social influences. Second, in contrast to the other approaches, they presented their view as an “autonomous” discipline, not grafted

⁵ This proliferation and competition of approaches can be traced in Sorokin’s account of social theories of that period (SOROKIN [1964] (originally 1928)). Winiarsky’s theories on “le mécanique sociale” and on “l’énergie sociale” are prime examples of “theorizing by analogy” in the late 19th century. As a prominent example of the meteorology approach one can single out E. Huntington’s theories on “the character of races” and on “civilization and climate”. The biological approach is exemplified by P. LILIENFELD’S [1873] “Die menschliche Gesellschaft als realer Organismus”, and by some blatantly racial versions of this school for which G. V. DE LAPOUGE [1896] (with “Les sélections sociales”) is an outstanding example.

⁶ with G. TARDE’S [1895] “La logique sociale” as a famous example.

⁷ McDOUGALL’S [1923] instinctivist “Introduction to Social Psychology” acquired some notariety.

⁸ To be more precise, the model of man comes in two versions which I have named SRSM (Socialized, Role-Playing, socially Sanctioned Man) and its watered-down version OSAM (Opinionated, socially Sensitive, Acting Man). The first sort of man is playing roles into which he or she has been socialized, and inner (conscience) and outer sanctions see to it that behavior does not deviate from the role expectations. The second sort of man had been invented to make role-playing man fit for large survey research: he or she has an opinion (or attitude) about everything and acts accordingly, but due to the social sensitivity of man, his/her opinions are the product of social influences. (see LINDENBERG [1985 b]).

on to some other field of research, and therefore truly new and worthy of being taught in universities. Third, they maintained that every explanation of social phenomena that ignored the full causal impact of other social phenomena was seriously flawed and needed to be redressed by a discipline that was not already committed to the explanation of non-social phenomena of one kind or another. Fourth, they developed a tradition of empirical research inspired both by anthropology and by the handling of census data, thus ranging from participatory observation and secondary analysis to survey methods and multivariate analysis.

“In this way, sociologists attacked views about the determinative influence of biological characteristics (e.g. race and ethnic origin) on criminal behavior, on intelligence, on ways of thinking, on achievement motivation, on social organization and inequality. They attacked each and every explanation of social phenomena by instinct, by biological given sentiment or by biologically determined state of mind (such as the explanation of the incest taboo by instinct, the explanation of the institution of the family by the ubiquitous sentiment of jealousy, the explanation of suicide by mental illness, or the explanation of religion by “primitive mind”). In a similar vein, they took on all explanations that failed to acknowledge social determination explicitly (such as historical explanations based on “great men”, all explanations based on human preferences that failed to acknowledge the social influences on human goals and values, all explanations of social inequality based on “natural” individual characteristics and motivation, as for instance talent and laziness). Finally, they locked horns with generally held views that failed to take the complexity of social determination into account. Thus, they would show that conflict is not at all bad since it also produces solidarity, that crime is not at all bad because it reinforces common values, that informal networks would form around formal organizational structures, that many social phenomena are unintended consequences of human action and thus much more complex than most people thought they are.” (LINDENBERG [1985 a, 246])

Whereas economics was too powerful a science to be attacked in earnest, virtually every confrontation with other (and weaker) sociologically “naïve” views would see sociology come away the winner. The success was attributed to the underlying model of man, so that by implication every view less radical than that (say, a theory mixing biological and social factors as causes of social phenomena) was hereby deemed “sociologically naïve”, a battle cry that could be heard for many decades.

2.2. *The End of Sociology as Debunking*

This account also carries the key to the contemporary development. First of all, traditional sociology was an empirical *debunking* science which experienced its greatest triumphs in its battle against “sociologically naïve” competitors. Secondly, the battle ax, sharpened in successful combat, was the particular sociological model of man: role-playing man. Non-rational man was thus central to all the successes and was not easily being abandoned. Unable to subdue economics (and, for the reasons given above, safe from its competition) sociologists thus also had to live with dual-natured man and with a mutually reinforcing but

scientifically precarious division of labor. How precarious this arrangement was became apparent when traditional sociology had virtually soaked Western culture with its message.⁹ Debunking had run out of targets. Shortly thereafter the triumphant traditional sociology had itself run out of steam. Its sharp ax of success, role-playing man, turned out to be a blunt instrument indeed when the task had shifted from demonstrating *that* social phenomena were determined by other social phenomena to showing *how and why* that was supposed to be the case. Dual-natured man had finally become a liability, at least to sociologists.

In the 1960s a growing number of sociologists were beginning to look for a model of man that could revitalize theory formation. At first, many hit upon what looked like a compromise between the rational and the non-rational sides of man: behavioral propositions based on learning theory.¹⁰ But, for reasons to be discussed below, it turned out that this compromise does not lend itself to theory formation beyond the small group.

In response to that, many continued the search and found in the 1970s *homo oeconomicus*, greatly aided by the Adam Smith revival due to the 200th anniversary of *The Wealth of Nations* (see ALBERT [1977]). Soon one spoke of “the economic program” in sociology (see OPP [1985]). “Rational man” thus entered for the first time a discipline that was rich with traditions of empirical research and rich with insights that had not been theoretically integrated due to the paucity of role-theory as a theory of action. It thus also opened a door for the systematic use of results from economics in sociology. Both these points are illustrated by what happened to social network research. A sophisticated methodology and a huge amount of empirical material on social networks had been accumulating over the years in sociology, but due to the severe theoretical limits of role-playing man, not much interesting theoretical work had been done with social networks. After rational man entered the sociological scene, theoretically interesting network studies began to mushroom. Theoretical ideas from economics on the use of scarce resources and on investment behavior could now be easily connected to sociological aspects. For instance, social networks could now be seen as “social capital” for the procurement of information, social support and other services (see COLEMAN [1988]); and people are thus likely to invest in the building and maintenance of social networks (see FLAP [1988]). The accumulated insights about different social ties could now be systematically related to aspects of social capital and investment behavior.

⁹ This point was dramatically marked in many Western societies in the 1960s by the passing into adulthood of the first generations that had grown up in a thoroughly sociologized culture. In an unequal fight, the remnants of the 19th century were brushed aside by an unholy alliance of sociologist gurus with youth movements. After that there was simply no “sociologically naïve” competitor left.

¹⁰ George HOMANS [1961] may have been the most influential author in this respect (see also OPP [1985]).

In short, due to the paradigmatic shift, existing insights can be theoretically unlocked and used in the context of a strong research tradition. This situation had never existed before. But the use of *homo oeconomicus* within sociology was not tied to the economic tradition of clean models and thus there was much more willingness to use *homo oeconomicus* in ways that best suited the purpose. At times, this meant neoclassical price theory, at times this means Subjectively Expected Utility (SEU) theory, at times it meant game theory, and at times it meant an informal extension of one of these three with assumptions that are difficult to model, such as interdependencies. As we will see below, this eclectic use actually helped rather than hindered the development of a model of man common to both sociology and economics.

The convergence did not come from sociology alone. Since dealing with institutions was incompatible with the aim of rigorous model building, it had ended up on the sociological side of the division of labor. Some outstanding theoreticians, like Schumpeter and Hayek, objected to the old arrangement long before the days of convergence, but most economists did not. But slowly things changed. There was no sign that politics did not affect markets. To the contrary, it seemed that politics became ever more interwoven with markets in the budding welfare states, and so dealing with institutions became increasingly more important to a broader segment of economics itself, leading to various forms of a new political economy. In turn, this development greatly facilitated the new developments in sociology in the 1970s (see LINDENBERG [1985 b]). Thus prepared, economists also moved rapidly into traditional sociological areas, such as the family, health care, crime, discrimination, higher learning and arts, when it became clear that the sociological success story had ended. The so-called "economic imperialism" is nothing but the flamboyant end of the old division of labor (see FREY [1990] and SIEGERS [1990] as examples). In its wake there came an increased awareness that the insights sociologists had accumulated on these non-market terrains would be extremely useful if they only could be "unlocked" for economic theorizing. We have seen that this unlocking is taking place within sociology, but there are considerable methodological problems connected with the dovetailing of model building and empirical content and they will be discussed in the following section.

3. Bridge Assumptions, Behavioral Economics, "Individual₂" and RREEMM

With the waning of the old division of labor and with rational man beginning to become a unifying model of man in the socio-economic sciences, it is not surprising that much attention shifted to the discussion of rational man him- or herself. One often hears from sociologists (and some economists too) that sociology should stay away from rational choice because *homo oeconomicus* has clearly failed in economics. But is this really sound advice?

The most famous voice in this discussion may be Herbert Simon (see SIMON [1957]). In Europe, Popper's philosophy of science, with its emphasis on depth in explanation, had been brought to bear on the social sciences by the pioneering works of Hans Albert, and his criticism of the neoclassical model of man as a mere "logic of choice" was widely discussed (see ALBERT [1967] and [1984]).

By now criticism of the neoclassical model of man has become more or less institutionalized, with its own journals and societies. The most outspoken of these efforts has become known under the name of "behavioral economics", although divergent groups are associated with this term.¹¹ The controversy centers around the following point: if we know that the various versions of rational choice theory are strictly speaking empirically false or empty, should we or shouldn't we replace them by more realistic psychological theories? The proponents of behavioral economics say we should replace rational choice theory because time and again we find that certain predictions from this theory are contradicted or at least not confirmed in psychological experiments. Proponents of neoclassical economics say we should not replace rational choice theory because it allows us to use the construction of clear and understandable models with clear and testable implications, and even if some of these implications are contradicted by experimental evidence, holding on to rational choice theory is still better than losing the strong guide of general deductive reasoning in exchange for a plethora of more realistic but also more complex and less general theories of specific phenomena. For the possibility of theoretical integration in the socio-economic sciences, this controversy is of utmost importance because there is no use trying to apply economic models in various non-market areas if they cannot become more realistic. But as, by trying to make them more realistic, the advantages of model building are lost, then there is little reason to turn to economics to begin with. In order to make the controversy more intelligible, it is helpful to look at the conditions for theory-guided research in some more detail.

3.1. Theory-guided Research

The capacity for theory-guided research may be pinpointed by the right juxtaposition of two kinds of issue dimensions: major/minor and common/unique (see Figure 1). *Major* issues of a problem are those issues that, if ignored, will prevent an adequate problem solution; *minor* issues of a problem are those that are not major and that, if dealt with, may improve the quality of the problem solution. *Common* issues are those that are common to the great variety of problems dealt with in a field, to be distinguished from those *unique* to a

¹¹ There are mainly three groups: the Carnegie group around Simon, the Michigan group around ideas of Katona, and the Oxford group around Marshak and Shackle (see EARL [1988]).

Figure 1
Relevant issue dimensions for theory-guided research

		issues	
		common	unique
issues	major	type 1	type 2
	minor	type 3	type 4

subclass of problems in that field.¹² What deductive model building in microeconomics does is focus on type 1 issues, i.e. to address the common issues in a great variety of problems (say, shifts in relative prices given human resourcefulness), under the assumption that thereby it also addresses the major issues of all these problems.¹³ Type 2 issues (if any) are dealt with by creating subclasses of type 1 issues (like inventing a new sort of costs, such as transaction or information costs). Type 3 and type 4 issues are incidental. This strategy is made possible by an action theory that is clearly geared to the common issues and, judging by the success of microeconomics in the past, to major issues as well. The strong tradition of theory-guided research in this discipline results from this strategy. But two problems can arise. First, by moving into new subfields, the frequency of type 1 issues may decline and genuine type 2 issues increase, thereby reducing the theoretician's ability to guide research. In that case the theory may have to be adapted and here the exchange with the new kind of sociology has proved to be a fruitful path to take. Second, because of the strong focus on type 1 issues for the sake of theoretical guidance of research, the other kinds of issues may be too much neglected, rendering the results (i.e. the explanations) too far removed from reality. Behavioral economists have singled out this problem and they suggest to improve the quality of explanations by doing away with the distinctions between major and minor issues, because it is the neglect of these minor issues that keeps the explanations so removed from reality. Thus the vehicle used to make the distinction between major and minor, viz. utility theory, has to go and it has to be replaced by realistic psychological

¹² These are clearly not operational analytical distinctions. Often only hindsight will be able to tell whether we are dealing with one or another kind of issue. But for our purposes this is not really damaging because we are dealing with a heuristic process (that of theory-guided research) in which the distinctions of issues will be revised in an ongoing process of conjectures and critique.

¹³ This has been clearly stated by two outstanding codifiers of the neoclassical approach: F. KNIGHT [1921, 4] stated that the value of the economic method lay in the fact that (i) large groups of problem situations have elements in common, that (ii) these elements are few in number and that (iii) they are important enough to dominate the situation. A similar point has been made by FRIEDMAN [1971, 26] in his famous methodological essay.

theories. Once this is done, the psychological theories will also offer more room for unique issues because certain psychological variables (such as self-confidence) may only become relevant in a subclass of problems (such as decisions under great uncertainty). Why will this suggestion not work and what can be done about the dilemma apparent in the controversy?

3.2 *Individual₁ Versus Individual₂*

Although the problems are far from being solved, there has recently emerged a clear methodological lead as to how the dovetailing of model building and empirical content can be achieved. This lead has profited greatly from the bad experience in traditional sociology of working without strong deductive heuristics. The main issue is the neglect of the difference between the primary focus of a discipline (its *analytical* primacy) and the tools to deal with the primary focus (its *theoretical* primacy). Let us look at this issue in some more detail.¹⁴

In economics and sociology, the main task is to analyze social systems. In other words, the analytical primacy is focussed on social systems. In order to explain social systems and related social phenomena, both disciplines have to make use of theories of action; i.e. the theoretical (or explanatory) primacy is focussed on the individual. Thus the two primacies refer to two different levels. There is analytical interest in the individual but only as an instrument for coming up with explanations on the social systems level. In psychology, the situation is different. Here both analytical and instrumental (theoretical) interests focus on the individual. Given that the analytical primacy in both cases is different, different requirements are placed on the "individual" theories in psychology on the one hand and in economics and sociology on the other. Let us distinguish between two kinds of theories dealing with individuals, indicated by individual₁ and individual₂, respectively, according to the scheme in Figure 2:

Making this distinction shows that many suggestions by behavioral economists and other critics of neoclassical economics boil down to the (implicit) claim that individual₁ theories are not only more realistic than individu-

Figure 2
Different primacies for psychology versus economics and sociology

	psychology	economics and sociology
analytical primacy	individual	social system
theoretical primacy	individual ₁	individual ₂

¹⁴ For more detail, see LINDENBERG [1981], [1983a] and [1985a], and WIPPLER and LINDENBERG [1987].

al₂ theories (this is not controversial) but that they will work better than individual₂ theories no matter what the analytical primacy is. And it is exactly this claim that is unfounded.

3.3 Requirements for a Structural-individualistic Theory of Action

Let us look at a list of reasonable requirements for an individual₂ theory:

(1) It must not require much information about each individual to whom it is applied; (2) It must allow us to model institutional and social structural conditions as defining intermediate goals and constraints of action; (3) It must allow psychological (including physiological) theories to influence its assumptions. For example, the information processing capacities of individuals must not be fixed by axiom; (4) It must allow us to express our degree of ignorance explicitly. Thus it must allow us to introduce simplifying assumptions in such a way that they can be replaced with more complex assumptions as our knowledge increases (method of decreasing abstraction). Via this capability, requirement (3) has an explicit means of being satisfied; (5) It must be well corroborated as a theory that explains behavior of human beings in the aggregate, including resourceful behavior.

Let me briefly elaborate each point. First, many individual₁ theories require much information per individual (for example the learning history or the personality traits). The more information per individual a theory requires, the less likely it can be used for large aggregates for the simple reason that the necessary information becomes harder to come by. The “watering down” of such theories in order to apply them to aggregates, such as in a psychiatric theory of crime (rates), covertly reintroduces the distinction between major and minor issues but this time not for the sake of theory-guided research (i.e. to increase type 1 issues) but as a way to solve a technical problem of measurement. More importantly (point (2)), because the primacies lie on different levels in economics and sociology, a theory of action in these fields must allow the *simultaneous* attention to these two levels. This requires an approach considerably different from psychology. For example, profit maximization can be seen as a human motive or drive but then it is part of an individual₁ theory, directing attention to psychological dynamics of the individual rather than connecting the individual and the system’s level. An example of an individual₂ approach would be to focus on the structural aspect of “profit” (as based on an institution that assigns the right to the residual) and then link this to the individual level by assuming that due to the institutional arrangement, profit has become an instrumental goal to individuals with the right to the residual and with rewards positively correlated to the size of the residual. Under additional structural assumptions (say concerning the dependence of other valued outcomes on the amount of profit) the instrumental goal “profit” becomes so overriding that it can be assumed to be the main maximand. Thus, in an individual₂ context, we must be able to link the system level and the individual

level not only after dealing with the individual level (say, in a micro-to-macro step) but *while* dealing with this level. This is very difficult, if at all possible, with individual₁ theories. The assumption we use to make this link are called *bridge assumptions*. In effect, these bridge assumptions allow type 2 (i.e. major and unique) issues to be connected to type 1 (i.e. major and common) issues. Some methodological problems connected with this point will be elaborated in somewhat more detail in the section on social production functions below, but first we have to deal with the other three requirements.

The third and fourth requirements are related to the problem of bridge assumptions and have to be dealt with simultaneously. The third requirement refers to the claim that advances in individual₁ theories must be able to influence theorizing in economics and sociology. But how can that be done without replacing individual₂ theories by individual₁ theories? If the answer to this question is: by bridge assumptions, then the next problem is how the deductive advantage of model building (with its requirements of simplification) can be preserved if ever more bridge assumptions render models increasingly complex.

The essential feature of bridge assumptions is that they enter as *auxiliary* assumptions rather than as proxies for the main theory. For example, every utility theory must assume something about the individuals' information concerning relevant events. We can assume that individuals are completely informed and if we see that the results are too unrealistic, we can replace this bridge assumption by a more realistic one, say that the individual infers probabilities of event occurrences from objective frequencies; if the results are still not satisfactory, this, in turn, can be replaced by still more realistic assumptions about particular biases in the process of inference, etc. There is also a price to be paid for the more realistic bridge assumptions: the more complex they are, the more they will violate the first requirement because they will require increasingly more information. In addition, we simply may not know enough to formulate a more realistic bridge assumption and yet, we would be able to go on with the limited knowledge we have. For these reasons, bridge assumptions are to be introduced according to the *method of decreasing abstraction*: as simple as possible and as complex as necessary. Behavioral economists would be pleased to see inference biases brought into the theory, and indeed it may improve the quality of the explanation at hand. But they would also suggest that the unrealistic utility theory be *replaced* by a more realistic psychological theory and this is exactly where they neglect the conditions for theory-guided research and the possibilities offered by the method of decreasing abstraction. No matter how complicated a bridge assumption gets, it remains an auxiliary hypothesis for a *simple* utility theory. For the ability of theory to guide research, this distinction between a simple theory and (possibly) complex auxiliary assumptions is essential because a complex theory would draw research into a preoccupation with the same complexities (like inference biases) no matter what problem on the system level needs to be solved, i.e. it would neglect the distinctions between major versus minor and common versus unique issues. With the

help of the method of decreasing abstraction, we can hold on to the crucial research-guiding function of theory with its strong emphasis on type 1 issues, while at the same time we also bow to the requirement of increased empirical content by additional bridge assumptions, stepwise approximating reality (thus moving from type 1 to type 2 issues and if there are problems of connecting type 2 issues to type 1 issues then we will have to add bridge assumptions concerning type 3 issues which will allow us to connect them with the remaining type 2 issues and, possibly, to add some type 4 issues as well). How far we have to go with this decreasing abstraction depends on the problem and not on some *a priori* nomological standard.

The fifth requirement, that the individual₂ theory must be well corroborated as a theory that explains behavior of human beings in the aggregate, does decidedly not mean that the action theory must withstand all sorts of tests designed for individual₁ theories. Rather, it requires that the individual₂ theory must indeed have proven time and again that the issues common to a great variety of problems are also the major issues. And this is where price theory has done much better in traditional "economic" applications than in many non-market applications. On this point, improvement is clearly necessary (see section 4 below). But will it come from behavioral economics? Because behavioral economists ignore the distinctions between major versus minor and common versus unique, they don't address this problem. Clearly, the big contribution of behavioral economics to model building can be made in another area: providing well researched bridge assumptions, ordered by decreasing abstraction and flagged by indices of robustness. Bridge assumptions can also help to solve the problems generated by the old division of labor. The old package that bundled "collective, non-rational, moral, values and sanctions" and the one that bundled "individual, rational, instrumental, preferences and constraints" had each fallen apart. Yet few, if anybody, had dealt with the problems that fell between the packages, such as the relation between morality and utility or between value and preference. According to what has been said above, at least a good deal of the solutions to these problems should come in the form of bridge assumptions (for example, on framing, see paragraph 5 below).

The model that satisfies all five requirements can function as the new *homo socio-oeconomicus*. It has been dubbed RREEMM¹⁵ (an acronym for resourceful, restricted, expecting, evaluating, maximizing man). For each element, we can introduce very simplifying or more complex auxiliary assumptions. For example, for "evaluating" we can assume simply that goods are evaluated in market value terms. In some contexts, this simple assumption may suffice. In other contexts, we can develop the model further by introducing a more complicated, say Cobb-Douglas kind of utility function. At a later stage of development of the model, we may assume an S-shaped utility function if we have reasons to believe that this added complexity will increase the depth of the

¹⁵ see MECKLING [1976] and LINDENBERG [1983a].

theory. In no case would we insist that all models should use the most complex assumptions about utility functions. One reason is that there is no reason to assume that the more complex is *always* also more realistic, and secondly, even if the more complex assumption were more realistic, this gain would not always warrant the extra loss in theoretical guidance and the extra problems involved in the measurement.

RREEMM clearly maps sociology and economics onto one category and psychology onto another. We can now see that the discussion of bounded rationality as an issue between economics and psychology has always been hampered by the failure to distinguish individual₁ and individual₂ theories. Between economics and sociology, only individual₂ theories are relevant as a model of man, and individual₁ theories are only important as auxiliary assumptions that can be introduced at a certain stage of a model's development.

It can now also be seen, how theory-guided research and the nomological claims of behavioral economics can be reconciled. The theory of action must be simple and address the common issues but there must be a systematic possibility to deal with the minor and unique issues in order to improve the quality of the explanation, as needed. This can be done by using bridge assumptions and the method of decreasing abstraction. But in order to have knowledge about the robustness and concreteness of bridge assumptions, there should be a branch of research geared to the generation of this knowledge. Behavioral economists may eventually focus more on this task (as is done already by some economic psychologists (see LEA et al. [1987]) and by some sociologists (see for example TAZELAAR and WIPPLER [1985])).

Research on "framing", i.e. on the effects of the definition of the situation of choice, for the use in individual₂ theories is also well under way (see for example LINDENBERG [1989 a]). On the other hand, model builders should be familiar with this research in order to try to incorporate it into the "standard" theory so that this theory can remain simple *and* veridical enough to ensure a high frequency of type 1 issues.¹⁶

4. Social Production Functions

"An explanation of preferences ... is necessary if social processes are to be explained. All social sciences aim to do this." (OPP [1985, 236]). One can only agree with him, because without an explanation of preferences, choice theories remain mute on a large area of social reality or they open the doors to ad hoc theorizing about changes that are exogenous to the theory. Opp's assessment

¹⁶ RACHLIN [1980] and DE ALESSI [1983] for this view of the standard theory of action in economics.

refers already to the new times of convergence. Indeed, when sociology and economics began to converge, it seemed to be an ideal marriage for some people because at least in one point, the two were said to be complementary. Whereas economists studied constraints and took preferences as given, sociologists studied preferences (values) and dealt with constraints (i.e. alternatives and their outcomes) mostly implicitly (i.e. as part of role expectations). But due to the old division of labor, the euphoria was premature, since the complementarity was more an illusion than reality.

Due to role-playing man, sociologists had approached preferences not from the standpoint of choice under constraints but from the standpoint of social control. How is behavior socially determined? By *socializing* an individual in such a way that he will want to do what he is socially expected to do. Thus "wanting" was removed from choosing and was (if socialization was successful) itself a sign of social constraint or control. The desired (preference) was socially engineered to coincide with the desirable (value). This did not fit utility theory. Yet, without a solution to the explanation of preferences, the usefulness of rational choice theory in non-market areas would be quite limited.

Let me begin with preferences. Recent developments do offer a likely solution. They center around a change in economics that had come about as a response to non-market applications, and can be summarized by a shift from man, the consumer, to man, the producer. This shift has been propagated by Gary Becker who, based on the shift from consumer to producer, had also instigated the new home economics framework that he and others apply to the family and labor market participation. What is so important about this shift is that it allows preferences to appear entirely in an instrumental context whereby they would have to be explained as part of the social structure and thus as part of the given constraints.

This feat is accomplished by the assumption of two kinds of preferences (see STIGLER and BECKER [1977]): *universal* preferences (goals) that are identical to all human beings and therefore need no explanation, and *instrumental* preferences for the means that lead to the ultimate goals which are in fact constraints and can thus be explained in a constraint driven approach. Technically speaking, there is only one utility function for all mankind but there are systematically different production functions for different kinds of people. Buying a particular good is now not an act of consumption but the purchase of a means of production, such as a record for the production of music pleasure.

Needless to say, this approach fits nicely into the bridge-assumption methodology outlined above, because the specification of production functions can be seen as providing bridge assumptions about instrumental preferences. So far so good. But without a specification of what the ultimate goals are, the old danger of ad hoc theorizing looms large and little has been gained. For this reason, Becker's approach was further developed into what may be called the "social production function approach" (see LINDENBERG [1984 a], [1984 b], [1986 b], [1990]). Taking the lead from Adam Smith, it was assumed that there are at least

two ultimate goals: physical well-being and social approval.¹⁷ They are aspired by everybody, and therefore the means people have to reach these goals are of utmost importance to them, so important that a systematic threat to these means may cause a revolution (see LINDENBERG [1989 b]). These means vary with social position and are called “social production functions”. They work like standard operating procedures and the clearer role expectations are formulated and sanctioned the clearer the social production functions.¹⁸ When the positive and/or negative sanctions connected to the expected behavior become less and less, then the individual will look for alternative means of getting physical well-being or social approval. And since the individual is assumed to be resourceful he or she will actively look for alternatives rather than remain within the role expectations until somebody tries to resocialize him or her.

Let us take an example (see LINDENBERG [1990]). In a traditional industrial social structure with segregated gender roles, the man has his job and his life style as sources of social approval, the woman has making a home and the raising of the children as sources of social approval. When making a home and raising children yield less and less social approval, women will try to change their social production functions, for example by entering the labor market if they have not done so already for the sake of money (physical well-being). Here, the difference between the old and the new situation is quite apparent. In sociology, it has been known for a long time that social approval is an important reward connected to holding a job (see for example MORSE and WEISS [1955]), but due to “role-playing man”, this insight was not theoretically worked into a theory of labor market participation until the model of man in sociology changed to RREEMM.

In this perspective, *socialization* is not something alien to rational choice but an integral part of linking culture and rational choice through learning effects. Many social production functions will be purposefully taught. Social etiquette is one example.

5. Framing Effects

One of the core insights of economics is that relative prices (and scarcity) strongly influence behavior. One of the core insights of sociology is that the definition of the situation influences behavior. These two insights are related to the materialist and idealist traditions, respectively. How can they be integrated,

¹⁷ Loss-avoidance may be a strong candidate for a third universal goal; see LINDENBERG [1989 a].

¹⁸ Note that role expectation are here taken literally to be normative expectations concerning behavior of a person in a particular position. They do not imply that individuals follow these expectations because they have learned to do so in the process of socialization.

as they must if the behavioral bases of economics and sociology converge into one model of man?

This question of integration has been intriguing social scientists for a long time, and there are many attempts at a satisfactory solution. However, until recently, even the most sophisticated solutions were not acceptable. It seems that “framing” offers the right key to unlock this puzzle.¹⁹

Briefly stated, a situation is framed by a goal (and the relevant goal criterion) in the sense that that goal will select the relevant alternatives and thereby “define” the situation. Other utility arguments play at that time only an indirect role by influencing the firmness of the grip (the “salience”) the frame has on the definition of the situation. When utility arguments in the background become stronger, they will reduce the salience of the present frame and may cause a “frame switch”. Rather than presenting the technical details of the framing model, let me give an example.

It is well known that friendships can suddenly turn “sour”. How is this possible? Interaction between two friends may be steered by the frame “to be a good friend” with goal criteria that include the willingness “to help a friend in need”. An increase in the cost of helping will be a negative influence on the situational salience of “being a good friend”, and the probability of helping, though higher than 0.5, will decrease. If the friend keeps asking for help, the added costs keep reducing the situational salience of the frame. The probability for helping will thus approach 0.5, the frame “to be a good friend” ceases to provide a guide for choosing among the alternatives and a new frame will appear, representing the most prominent of the background aspects, which, in this case, is likely to be “minimization of any further costs connected to this person”. Frame switches allow seemingly irrational (i.e. inconsistent) behavior across situations. They also explain apparent preference changes without the assumption of instable preference structures.

Framing should change the way in which the discussion about egoism versus altruism is conducted (see for example MANSBRIDGE [1990]) because frames explain how certain utility arguments (such as “making a profit”) can be situationally submerged into the background. In this way, the difficulty with explaining the functioning of norms can also be elucidated. *Norms* are situationally prescribed goals and thus they operate via framing (cf. LINDENBERG [1983 b]). The question why somebody conforms to norms is really only so difficult because norms are in effect prescribed framings. In this way problems can arise for both rational choice and for value theories. Consider what I call *the by-product paradox of social goods: many norms have the effect that you only get what you really want by pursuing something else instead.*

¹⁹ This confrontation of framing solutions has been described in some detail in LINDENBERG [1989 a].

For any theory of rational choice without framing, this is indeed paradoxical. For example, a judge who purposefully tries to maximize what she really wants, say social approval, will in all likelihood be seen as a judge who does just that, *thereby* reducing the likelihood of getting professional recognition (i.e. social approval) for what she does. Instead, a judge is expected to maximize justice, thereby defining each situation in which she acts as a judge *in terms of this situational goal*, applying standards for the approximation of justice and ordering the alternatives accordingly. Due to the complexity of the professional standards and the difference of opinion surrounding them, this ordering of alternatives would be different if the judge tried to maximize approval from colleagues at every time. And this difference would be detected.

In economics, framing effects are clearly also relevant. For example, up until now, the theory of agency assumes that rewards should be aligned in such a way that people want to do what they are expected to do. Yet, the agency problem is sometimes much easier to solve than the theory assumes (when the frame is right) and sometimes much more difficult to solve (when it is not right) because strong frames make people act singlemindedly in given situations. Downward stickiness of wages is very likely a result of framing effects.

Due to the method of decreasing abstraction, framing effects may be ignored for a while in the development of the model because a similar effect can be achieved by assuming (ad hoc) that a particular actor has only one major goal. But eventually, the assumptions on relevant utility arguments of the actors must be based on theory and research in the area of discourse.

6. Summary and Conclusion

The discussion of bounded rationality has been dominated by the relation between economics and psychology. This paper attempted to explain why this front would not yield a useful solution to the question how economic theories could be made more realistic. Such a solution had to place the question of models of man into a methodological context. If this is done, the relevant development is the convergence of sociology and economics, and the relevant adaptations in the model of man necessary for *this* convergence will dominate the further development. Results from (cognitive) psychology will certainly play an important role in directing these adaptations, but it is very unlikely that a *homo socio-oeconomicus* will consist of a psychological behavioral theory. In order to understand this development one must thus look at the changes in the relation between sociology and economics.

Two models of men coexisted side by side: the all-informed, consumption-oriented *homo oeconomicus* and the norm-oriented, conforming *homo sociologicus*. Due to processes described in the paper, the old trend of divergence between sociology and economics reversed in recent years, pressing for integration. In order to achieve this integration, it was first of all necessary to give a

prominent place to the method of decreasing abstraction by which an explanation (in the form of a model) can stepwise be made more realistic (at the cost of also getting more complex). This allows a flexible degree of deviation from the model of man that was ideal for model-building: the old *homo oeconomicus*. "Bounded rationality", understood as more realistic assumptions about individual behavior and decision making, can be brought in by degrees as seems necessary for the explanatory task at hand. But this cannot be done by just bringing in results from cognitive psychology. Core insights of economics and sociology (relative prices and the definition of the situation) must be integrated into one model in such a way that the method of decreasing abstraction remains applicable.

This can be done by modeling the quintessential sociological insight via framing in terms of rational choice. In order to do this, a distinction between a situationally salient goal (i.e. the frame, in terms of which the alternatives are ordered) and background goals is made.

As an explanation develops, this problem of framing can at first be negated by assuming that the utility arguments chosen by the researcher are in fact the ones that define the situation for the subjects in question. In later more complex phases of the explanation this assumption may be relaxed and replaced by more realistic assumptions about the situational definitions of the subjects involved (using the method of decreasing abstraction).

This task is greatly aided by a third ingredient: the reorientation from man, the consumer, to man, the producer. Methodologically this comes down to nothing less than structural explanations of preferences in a constraints-driven heuristics. If one assumes that all individuals, no matter where and when they live, have some general goals in common because they are human beings living in society, then people differ not so much in their subjective wants as in their objective means to produce a particular level of the high level good (such as social approval). In other words, the means become the instrumental goals and man becomes a producer of his or her general goods. Socialization plays an important part in conveying and coordinating these instrumental goals, but their instrumentality for reaching the common human goals can be objectively assessed. Because of this constraint-driven heuristics, the incorporation of definitions of the situation does not lead to a "subjectivation" of the model of man and it solves the unsatisfactory treatment of preferences in the *homo oeconomicus*.

Homo socio-oeconomicus is thus flexibly strung out between a simple all-knowing maximizer and a cognitively bounded framer of complex situations. This flexibility is made possible by the method of decreasing abstraction, by modeling situational selections of utility arguments (framing) and by a constraint-driven heuristics. Together these three form a strong methodological package which makes it likely that *homo socio-oeconomicus* is indeed going to be the general model of man in the socio-oeconomic sciences.

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