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AN ASSESSMENT OF THE NEW POLITICAL ECONOMY: ITS POTENTIAL FOR THE SOCIAL SCIENCES AND FOR SOCIOLOGY IN PARTICULAR

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1. INTRODUCTION

Let me begin with three statements that put the remainder of this paper into perspective:

First: The best thing that ever happened to social science is its beginning (at the time of Adam Smith);

Second: The second best thing that ever happened to social science is the new political economy;

Third: A lot remains to be done.

For a sociologist (and that's what I am), all three statements are somewhat out of the ordinary. So let me briefly expand on all three statements before I properly address the topic of this paper. In doing so, I have no qualm about exaggerating my points to drive them home more clearly.

Adam Smith had one particular scientific idol: Isaac Newton. And he tried to create a social science that had some of the explanatory power and elegance of Newton's axiomatic models. In doing so, he came up with three essential elements: *choice*, *institutions*, and *models* that interrelate choice and institutions. At this point, I will say no more about these models (see later), but let's see what happened next: out of Adam Smith's legacy developed (micro-) economics as a science of choice and models pretty much without the consideration of institutions; and *against* that science, sociology developed as a science of institutions without consideration of choice and models.

The new political economy brings all three elements back together again: choice, institutions, and models. In my estimation, the new political economy, born in the 1950s, is the single most important factor of bringing Adam Smith's program back to social science.¹ Yet, more than one hundred and fifty years of neglect cannot be erased over night. A lot remains to be done before we have truly moved from a new political *economics* to a general *explanatory social science* with division of labor.

2. THE POTENTIAL

The new political economy is first and foremost an "economics of politics," that is the study of political processes by means of economic theories and models. Examples of topics studied are: voting, political party competition and coalition formation, constitutions, the supply of public goods and interest groups, bureaucracy, property rights, public policy

and finance.² As Buchanan (1975) observed, *all* of these topics owe their theoretical relevance to methodological individualism, and, we might add, their practical relevance to the soundness of methodological individualism. This is to say, that the theoretical starting point is the *individual* who, having preferences and being confronted with constraints, has to make *choices*. Given the fact that preference-constraints combinations³ are different for different individuals, leading to different choices, and given the fact that individuals have to cooperate on some things and know they do, one crucial question becomes: how are different individual choices reconciled by (political) *institutions*? Once this question is asked, others follow quite naturally. For example: Which (political) institutions fail to reconcile individual choices and why? These and related questions are answered by means of verbal or mathematical explanatory *models*. In this way, the three basic elements of Adam Smith's program have been brought together again by the new political economy.

The question is what general import (potential) this development has for social science in general and for sociology in particular. In my view, the application of economic theories and models to the explanation of political processes and the ensuing collaboration between economists and political scientists has certainly been worthwhile, resulting in an impressive array of new models and research that show by and large cumulative progress within the program and considerable improvement compared to the state of political science without the aid of economics. Yet, this progress only promises a similar worthwhile collaboration to other social sciences, including sociology, on the basis of inductive hope: if it has worked here why should it not work for others? This basis is too weak to persuade many sociologists, anthropologists, historians and social-psychologists who have invested a great deal of their time to work out their own identity *vis-a-vis* and *against* economics. The potential needs to be made as explicit as possible in order to persuade these scholars to cut their losses and invest into something new.

This potential consists, I think, of four interrelated points:

- a. the model of man used in (micro-) economics, properly understood;
- b. the deductive style of reasoning in economics
- c. the emphasis on explanatory models in economics;

- d. the potential of a, b, and c for the integration of the micro and macro levels.

Let me discuss these points in order.

2.1 MODELS OF MAN

2.1.1 MODELS OF MAN IN ECONOMICS

Sociologists in particular have made it part of their tradition to argue against what they take to be the *homo oeconomicus*. This homunculus is seen as an atomistic unit endowed with materialistic cravings, sly, fully informed and utterly indifferent to the happiness of others (unless, of course, he can gain from this happiness). The cutthroat robber baron of the 19th century would come closest to this construction were it not for the fact that he too was actually an endearing man playing the role assigned to him by the economic system of a budding capitalism. It cannot be denied that a great number of "laissez-faire ultras" (as Schumpeter called them) have helped to create this impression, especially in France in the 19th century when Durkheim established his science of sociology against what he called "the utilitarians"; with their model of man, so argued Durkheim, they had no shadow of a chance to explain moral rules and altruism. As I will argue later, Durkheim had a point, as did Talcott Parsons who made the same allegation some forty years later and even more forcefully. But both sociological theorists, who are largely responsible for the sociologist's bias against *homo oeconomicus*, thoroughly misunderstood this model of man and what it can or cannot do. Let me present a short reconstruction of this model (the heart of micro-economics) the way I think it has been conceived, explicitly or implicitly, since the day of Adam Smith.

The model can be expressed by an acronym (following and expanding a suggestion by Meckling, 1976): RREEMM. These letters stand for:

- Resourceful: man can search for and find possibilities; he can learn and be inventive;
- Restricted: man is confronted with scarcity and must substitute (choose);
- Expecting: man attaches subjective probabilities to (future) events;
- Evaluating: man has ordered preferences and evaluates (future) events;
- Maximizing: man maximizes (expected) utility when choosing a course of action;
- Man.

Utility theories are certain systematizations of this model of man. Certain assumptions may be added in the process, such as an assumption about marginal utility (or marginal rate of substitution) and about the relation between utility and expectation.

The beauty of this model of man is that by and large we can say something interesting, worthwhile and reasonably accurate about an aggregate of individuals even if we know only relatively *little* about

them. The magic wand is a reasonably accurate assumption about universal and stable preferences. Adam Smith produced this magic wand: man's generosity is very limited and he cares most of all for an improvement of his condition, consisting of physical wellbeing and social approval. There may be better ones but this one has proved to be well corroborated. Even if we know nothing more, we have already an extended framework for social science on this basis. For example, since we want social approval, we are willing to substitute some physical wellbeing for social approval, thus giving consideration to others even if it costs us some effort. Where possible, we will exchange, where necessary we will cooperate. Social differentiation depends on the differentiation of restrictions which in turn depends on the nature of the economy: in a society of hunters and gatherers, not much can be accumulated and little can be inherited, while in a trading society accumulation and inheritance is likely. Thus, the former does not allow much differentiation of restrictions and hence not much social inequality, while the latter allows much social inequality. Innovation is unlikely to occur when the effort put into resourceful behavior is unlikely to produce an improvement in one's condition. In each society, man learns which goods improve his condition. Thus, in a monetarized society with a market, man will attempt to improve his income in order to buy the goods that improve his condition. If his income is independent of his effort, he will not invest much effort into the activity he is paid for unless the activity also produces social approval, etc. Books could be written to continue the layout of this framework. RREEMM and the substantive assumption about universal, stable preferences is in my view the most powerful tool of social science ever invented. It contains three elements that are systematically related: *environment* (restriction, expectation, evaluation), *choice* (maximization of utility), and *continuity or change* (resourcefulness). The framework is thus inherently dynamic.

The only true way in which this model is "economic" is that by using it, we economize on information since it is simple and has great explanatory power. Through various historic circumstances, it was mostly economists who took advantage of it. 'Homo oeconomicus' is a complete misnomer for this universal model of man. This will become even clearer if we compare it to the two most common models of man used in sociology.

2.1.2 MODELS OF MAN IN SOCIOLOGY

Sociologists have founded their science in its modern form on the premise that behavior is *socially* determined. Marx had prepared the way for this premise to crystallize and do so *in opposition* to economics. For example: "The economists express it like this: each person has his private interests in mind, and

nothing else. . . . The point is rather that private interest is itself already a socially determined interest. . . ." (Marx, 1971:65) Durkheim devoted his entire work to the elaboration of this premise. He did this in two ways. On the one hand, social determination of behavior takes the form of morality, on the other hand it takes the form of "social currents." *Morality* operates as a system of rules that have become part of the personality of the individual (i.e. they are "internalized") in such a way that these rules include the duty to sanction other individuals when and if they transgress the rules. In the process of becoming a social being through socialization in the family, in schools, or in an occupation, the individual thus learns to *want* to do what he or she *should* do. The desired fuses with the desirable. In case this fusion is not perfect, the sanction of others (part of *their* moral behavior) will bring behavior back into line with the desirable. Where does the desirable come from? "Moral goals, then, are those the object of which is *society*. To act morally is to act in terms of the collective interest." (Durkheim, 1961:59) Yet, "the collective interest, if it is only the sum of self-interests, is itself amoral. If society is to be considered as the normal goal of moral conduct, then it must be possible to see in it something other than a sum of individuals; it must constitute a being *sui generis*, which has its own special character distinct from that of its members. . . . Thus, the conception of society as being distinct from the individuals who compose it, a conception demonstrated by sociology at a theoretical level, is here confirmed at the practical level. For the fundamental proposition of the moral conscience is not otherwise explicable." (*ibid.*:60)⁴

Social currents are less institutionalized forms of social influence. They are neither systems of rules nor part of people's personality. Rather, they are temporary social influence on opinions and emotions such as "great movements of enthusiasm, indignation, and pity in a crowd. . . ." (Durkheim, 1950:4) Often, "we are the victims of the illusion of having ourselves created that which actually forced itself from without." (*ibid.*:5) Social currents are temporary only in comparison to moral rules. They may be momentary, as a movement in a crowd, or more stable as "opinions on religious, political, literary, or artistic matters which are constantly being formed around us, whether in society as a whole or in more limited circles." (*ibid.*:5) As does morality, social currents eliminate choice: "We can no more choose the style of our houses than of our clothing. . . both are equally obligatory." (*ibid.*:11) "Currents of opinion, with an intensity varying according to the time and place, impel certain groups either to more marriages, for example, or to more suicides, or to a higher or lower birthrate, etc." (*ibid.*:8)

Between about 1920 and 1960, the face of modern sociology was developed in the United States. The context of this development was decidedly American. While the classical European sociologists had

been preoccupied with causes and consequences of the industrial and the political revolutions, neo-classical American sociologists were drawn into the peculiarities of American nation-building: the stream of immigrants from various cultures and the financially effective quest for useful policy sciences. Studying ethnic groups and trying to understand their cultures, increased the influence of anthropology on the development of sociology and encouraged the study of classical authors who had dealt with questions of culturally formed personalities and the system of rules (morality) that set them apart as a group from other ethnic groups. Durkheim had had a strong influence on the development of anthropology and he was discovered as *the* relevant classical sociologist for the task at hand. In this way, his view of morality entered neo-classical sociology from two sides: via anthropology and directly. The theoretical development of sociology in this time, viz. "structural-functionalism," thus drew heavily from Durkheim's work and its anthropological codifications, especially under the influence of Talcott Parsons. Durkheim's "enrichment of the theoretical resources of the field of social science. . . is incalculable. Only a very select few among the figures in intellectual history have contributed so crucially—at such a significant juncture—to the development of scientific culture." (Parsons, 1974:lxiv) The *homo sociologicus* of structural functionalism, to be described shortly, is thus essentially a legacy of Durkheim.

Sociology as a useful policy science was not exhausted by the study of ethnic groups. Political processes of the nation as a whole or of political parties in a democratic context also stimulated the study of more changeable social states, especially opinions, on a large scale. Sociology as an empirical social science derived a considerable impetus from this demand. Again, Durkheim emerged as the most relevant classical sociologist for this task. Not only was he firmly committed to sociology as an empirical science, he also furnished a theoretical background for the usefulness of sociology as a policy science, especially with regard to "social currents," i.e. social influences on opinion. Voting for instance could be seen as the politically relevant expression of opinions, and in order to understand and/or manipulate voting one needed to understand the influences on voting. Thus emerged yet another *homo sociologicus*, that of sociological empiricism, essentially based on Durkheim's work. The fact that both models of man (that of structural functionalism and that of empiricism) derived from the same author seems to have discouraged explicit confrontation between the two, who have ruled sociology in more or less peaceful coexistence for almost fifty years now.

In terms of an acronym, the *homo sociologicus* of structural functionalism can be presented as: SRSM. These letters stand for:

Socialized: man internalizes role expectations (norms and values);

Role-playing: man acts according to situational role expectations;

Sanctioned: man is guarded against deviancy by the sanctions of others in case socialization is not perfect;

Man

The *homo sociologicus of sociological empiricism* can be presented as: OSAM. These letters stand for:

Opinionated: man forms an opinion about everything;

Sensitive: man's opinion is easily influenced by others;

Acting: man acts directly on the basis of his opinions;
Man.

2.1.3 THE MODELS OF MAN COMPARED

If we compare SRSM with RREEMM, we see that SRSM is not resourceful and is subject only to the following restrictions: sanctions and role expectations. Scarcity has only an indirect effect on SRSM, namely in so far as role expectations have been molded by the premise "ought implies can." SRSM expects only role behavior of others and evaluates events only in terms of conformity and deviance. Choice (substitution) is ruled out and thus nothing is being maximized.

Compared to RREEMM, OSAM lacks resourcefulness and restrictions. His attitudes, sensitive to social influence, contain expectations and evaluations; but it is important to notice the expectations and evaluations do not relate to a process of choice and maximization. Rather, they create behavioral *dispositions*, i.e. situationally specific activations of behavioral patterns.

It is a defensible proposition, that in very stable societies most, if not all, relevant restrictions will have found their way into role expectations and sanctions. In these situations, SRSM is a handy shorthand expression of institutions, structural constraints and social behavior all in one. This boils down to situations in which the expected behavior always belongs to the set of viable alternatives and in which the expected rewards minus the expected costs are always highest for the behavioral alternative that is prescribed by the role expectations. Imagine an organization in which everybody behaves the way he or she is told. Then all we need to do in order to predict the behavior of each individual in a certain position is to recreate the blueprint of the organization (that is, the description of interlocking role expectations). It is not surprising that anthropologists have frequently applied this blueprint approach to small, stable societies, using SRSM as their model of man. Yet it is important to notice that RREEMM behaves under these restrictive conditions *as if* he were a SRSM. The moment these restrictive conditions do not apply anymore, SRSM gives us no clue as to how behavior might change. For example, take a primitive society of hunters and gatherers in

which males are socialized into the role of the hunter and females into the role of the gatherer. Now let the population of this and surrounding bands increase so that the (biologically determined) stock of resources (edible wild animals, berries etc.) is slowly depleted. What will happen? Men will continue to hunt and women will continue to gather but the marginal productivity of their (role-conforming) efforts will decline. The number of individuals for whom role-conforming behavior is the most rewarding behavior will also decline, creating a situation in which RREEMM will act less and less as a SRSM. Although RREEMM does not allow us to say what alternatives become more rewarding than the ones prescribed by role expectations, we know that RREEMM is resourceful, i.e. he is able to learn and is inventive. Let one band begin to check its population growth (say by infanticide); let another band create taboos about hunting and gathering on certain days, thus restricting exploitation of the resources; let a third band use a number of their hunters to guard a territory against other bands. Each band is resourceful in a different way, but the third band created a totally new situation: within its guarded territory, any further resourcefulness of its members will benefit only its members, greatly increasing the marginal value of this kind of behavior to the band. Attempts to keep edible animals within the guarded territory is likely to lead to the domestication of some of them. Attempts to increase yields from plants within the territory is likely to lead to the search for new edible plants and to some knowledge of seeds and cultivation.⁵ Slowly a sedentary agricultural society will develop with very different roles than the hunting and gathering type of society. Because the scarce resources in an agricultural society (live-stock, arable land) can be accumulated, resourceful and maximizing behavior will lead to much more social inequality than existed in a society in which scarce resources (wild animals, berries, etc.) could not be accumulated. Since accumulated wealth can be transferred, children can inherit the wealth of their family, rendering conditions of social inequality quite stable (Smith, *Wealth of Nations*, 1776, Book V, Ch I, Part II). Slowly RREEMM will act again *as if* he were a SRSM, while relative rewards of activities slowly begin to shift again away from role-prescribed behavior.

It is ironical that, as this illustration indicates, RREEMM is a much more general model of man which allow us to predict that under certain circumstances man will act as if he were a SRSM. It is ironical because SRSM was *meant* to be the more general model of the two. "The theory of institutional behavior, which is essentially socio-logical theory," observes Parsons, "is precisely of the highest significance in social science because by setting the problems of social dynamics in a context of institutional structure . . . this theory is enabled to exploit and extend the knowledge of modern psychology about the non- and irrational aspects of motivation

in order to analyze social processes. It follows also that any conceptual scheme which utilizes only the motivational elements of rational instrumental goal-orientation can be an adequate theory only of certain relatively specialized processes *within* the framework of an institutionally structured social system." (Parsons, 1951:43) The non- and irrational aspects of motivation referred to by Parsons, are exactly those of SRSM: the fusion of institutionalized values and norms with the need-dispositions of the personality through the process of socialization. Somehow, SRSM is thought as leaving room for RREEMM-like behavior wherever this is compatible with institutionalized values and norms; "there is room for much fulfillment of private interests." (*ibid.*, p. 41) And because RREEMM is only applicable to this circumscribed space within SRSM, economic theory "cannot provide an adequate model for the dynamic analysis of the social system in general terms." (*ibid.*, p. 42)

A BIG MISUNDERSTANDING

An institutional set of common value patterns is quite essential to the reconstruction of a functioning society. This insight also emerges within the contractarian paradigm which, in turn, is one important part of the new political economy. Individual behavior is embedded in this "constitutional" framework and socialization is surely a relevant, and probably *the* relevant, mechanism by which this embeddedness is brought about. In addition, roles are useful constructions that combine situationally integrated restrictions, standards of evaluation, and co-orientation. The sociological criticism of "RREEMM" cannot be dismissed as so much nonsense. At the core of the misunderstanding lies not a substantive but a *methodological* issue: a fundamental misunderstanding concerning explanation and model building.

Scientists always have to simplify, there is no difference of opinion between economists and sociologists. But where do you simplify most? My central thesis concerning the sociological misunderstanding of RREEMM is this: following Durkheim, sociologists simplified inversely to the order of analytical importance. Analytically, the most important thing for a sociologist is the institutional embeddedness of social behavior; analytically least important for a sociologist is the individual as a focus of initiative. Therefore, following the sociological principle of simplification, sociologists should simplify least those aspects of man that relate to his or her institutional embeddedness and simplify most those aspects that relate to the individual as a focus of initiative. SRSM is the product of this rule of simplification: central in this conception is socialization and role-playing, stabilized by sanctions. All three emphasize the institutional embeddedness of social behavior.⁶ An extreme simplifying assumption is that man would only be sensitive to cost/benefit aspects related to role deviance and conformity, respectively; or, conversely, that costs and benefits are always distributed

in such a way that conformity is the most rewarding behavioral alternative. Every sociologist knows that these two assumptions are not realistic, but then every scientist has to introduce simplifications . . .⁷

SRSM, then, is a model of man that, contrary to some form of RREEMM, stresses what's really important to a sociologist. Yet, the sociological principle of simplification on which SRSM is built, has a most serious drawback: it does not allow replacement of unrealistic assumptions by less unrealistic assumptions (method of decreasing abstraction). The role playing aspect of individual behavior is so much built in to SRSM that it is virtually impossible to relax the assumptions above which jointly imply that the individual can safely be assumed not to be a focus of initiative. This is true even if there are circumstances that by definition contradict the simplifying assumptions. For example, for Parsons, the stable, integrated process of interaction in terms of established role relationships "is the fundamental point of reference for all dynamic motivational analysis of social process." (Parsons, 1951:205) Yet, he also observes that "changes in the fundamental reward system of the society" are continual. "Organizations are continually restructured, old ones die out or decline while new ones rise, and the role structure within those which continue is altered." (*ibid.*, p. 513) The "fundamental point of reference" is so much built into SRSM that the observation about continual change in the "reward system of the society" cannot possibly lead to an adjustment in the simplifying assumptions. The individual as a focus of initiative is thereby permanently placed *outside* the framework of analysis. Witness a telling example: "To have more money rather than less is simply, with only a few exceptions, to be in a more advantageous position to realize *whatever* goals the actor may have in mind. In this sense the 'profit motive' is nothing but (sic) a primary aspect of what may be called 'practical rationality'." (Parsons, 1951:244) "Practical rationality" is, of course, no part of SRSM, but the qualification "nothing but" indicates again that for the sociologist, this aspect can be safely ignored, *although* the reward system changes continually.

The misunderstanding thus lies in the sociologist's assumption that the model of man should indicate what is important to a particular social science rather than serving as an explanatory device. The consequences for sociology are far-reaching: the sociologist is ultimately forced to conceive of institutional and cultural change as autonomous processes.⁸ "There is literally no way of making the transition in gratification-deprivation terms from the individual actor to the social system." (Parsons, 1951:497) And, in case we missed the point, Parsons states it even more clearly: "That personalities are above all oriented to the optimization of gratification as their fundamental directional principle (sic), while social systems are oriented to cultural change, is an inference from, and a way of stating, the mutual *independence* of the two classes of system." (*ibid.*, p. 502,

emphasis mine) Durkheim, the "inventor" of SRSM had been forced to a similar conclusion: "individual natures are merely the indeterminate material that the social factor molds and transforms." (Durkheim, 1950:106) "When the individual has been eliminated, society alone remains. We must, then, seek the explanation of social life in the nature of society itself." (*ibid.*, p.102) Yet, explanation is exactly the one achievement not possible without a model of man that can serve as an explanatory device.

Another way of expressing the misunderstanding is the following: In all social sciences, society (institutions, social structure, socialization, co-orientation, etc.) should have *analytical* primacy and the individual (in terms of a model of man that can explain social behavior) should have *theoretical* (or explanatory) primacy. The sociological misunderstanding lies in the failure to distinguish these two kinds of primacies and to assume that the analytical primacy of society should dictate the model of man (Lindenberg, 1976, 1977, 1981). It is entirely possible that many economists escaped this mistake by the sheer fact that for them the individual was more important than society. Since RREEMM expressed what they considered to be important for an economist, they actually followed the same principle of simplification as sociologists, and by accident they thus opted for a model of man that happens to be a powerful explanatory device. In the process, they neglected society (enter sociology). However, the new political economy shows that consideration of institutions is not only compatible with RREEMM but needs RREEMM if one wants to explain social phenomena.

The model of man of sociological empiricism (OSAM) lies somewhere between SRSM and RREEMM. If one interprets social influence as socialization of role expectations then OSAM is identical to SRSM (without explicit reference to sanctions, however). "Opinions" are then the same as "role expectations" (See Gadourek, 1982 as example). If "opinions" are taken to be ordered preferences, then OSAM turns into RREEMM in a special situation, viz. RREEMM unfettered by restrictions of any kind and thus freed from the necessity to substitute. When attitudes are formed by behavior itself, as is likely with routine behavior, then restrictions are already pushed into the background and no substitution *seems* to be necessary. In both cases, RREEMM acts as if he or she were an OSAM. But since these situations are either not so interesting (e.g. routine behavior) or not frequently approximated (e.g. no restrictions), OSAM is not a very useful tool to predict and explain behavior (Fishbein and Ajzen, 1975; Bentler and Speckart, 1979; Stapf, 1981). Consequently, sociological studies using OSAM concentrate mostly on "opinions," "attitudes" and "orientations" rather than on behavior. In this "withdrawal to cognitions," they meet with many sociological studies based on SRSM, in which scholars concentrate on role *expectations* rather than role

behavior for very much the same reason, as we saw above. While it is conceivable that sociology restricts itself to be the study of "social orientations," as OSAM and SRSM would suggest, claims are otherwise. For example, opinion polls are widely offered and taken to be indications of imminent actions. Another example is the study of "work orientations," a rather popular subject, and a typical interpretation of the possibilities offered by these kinds of studies is the following: "All values and attitudes capable of being expressed in the work situation, in fact, are potential influences upon orientations to work and upon *the social patterns that result.*" (Fox, 1971:15, emphasis mine). Somehow, social patterns are expected to result from orientations, but we do not know which, when and how.

In order to prevent a possible misunderstanding, I would like to stress again that the study of "orientations" is by itself interesting and legitimate. What I do claim is that, contrary to what we are led to expect, we learn precious little about behavior or "resulting social patterns" from these studies *because* they are based on OSAM and thus lack an explanatory device for behavior.

2.1.4 CONCLUSIONS ABOUT THE MODEL OF MAN

What the new political economy has done is to show that *homo æconomicus can be cleansed from its "enrichment" that pushed the model of man of Adam Smith into the carnival's costume of an all-knowing, money-loving preference machine that encountered similar beings in spontaneous exchanges. The result is the rediscovery of RREEMM, and it came about by applying homo æconomicus to contexts in which institutions and uncertainty played such an obvious role that they had to be dealt with: the realm of politics. Ironically, homo æconomicus turned out to need very little "cleansing" in order to turn back into RREEMM because it was known all along that perfect information, evaluations in terms of money, and institution-free contexts of exchange with perfect competition and spontaneous agreements, are assumptions that are unrealistic but allow very neat model-building. Once these assumptions did not even have that advantage, as happened in the field of politics, they were quite easily dropped and no basic, different model of man had to be adopted.*

More important than the "cleansing" of *homo æconomicus* was the fact that the new political economy showed that RREEMM is indeed a powerful explanatory device. In order to do this, it was necessary to use a particular style of deductive reasoning: model-building. To this I will turn shortly.

Seeing that it did not take much to cleanse the neo-classical model of man, the question arises why this has not happened in sociology. The answer I tried to give above is the following: sociologists had many useful and important things to say about the social context of behavior (socialization and social influence on attitudes, norms, values and role expect-

turbing factors left. While one cannot prove that general hypotheses must be incomplete, there are good arguments for the view that completion is impossible (see Gadenne, 1979). When boundary conditions are fairly stable, possibly disturbing influences are fairly stable and the incompleteness of our general hypotheses is not a big problem (as in many branches of the natural sciences). If, however, boundary conditions are not stable, as in the social sciences, incompleteness practically renders every general hypothesis "false" because in replications, boundary conditions have changed in ways we cannot be sure of. (For a more detailed account, see Lindenberg, 1982.)

What can be done in order to get some handle on possible disturbing factors? Of course, the problem can be handled *ad hoc* every time we test a general hypothesis. Our background knowledge may give us some clues which factors we should incorporate in our testing, although the theory has not specified these factors. Or we may feel justified in believing that the error term is uncorrelated with the independent variables. Yet, to the serious empiricist, this *ad hoc* solution remains problematical. Not entirely surprisingly, a call for more theory for the solution of this problem can be heard.¹¹ If we have explanations of how the hypothesized relationship comes about, then we know more about the context of this relationship and this may give us a systematic handle on possible disturbing factors. In this way, theorists would become important to the task of empiricists, and Zetterberg's suggestions become more relevant again. There are recent attempts in this direction (See Collins, 1975; Turner, 1982). While these attempts are certainly an improvement over the *ad hoc* solution, they run into a serious difficulty: the additional general hypotheses are again incomplete and we don't know what changes in the boundary conditions render them "false." In order to get a systematic handle on the additional hypotheses we must thus find still other general hypotheses etc. We are in an infinite regress. An example that is both known to economists and sociologists is useful at this point to illustrate both the problem and the particular flavor of the deductive reasoning that is characteristic of "more advanced" sociological theorizing. Here comes the example:

Olson (1965) wrote a book on the logic of collective action that has become very influential both in economics and in sociology. Imagine now that we don't know much about economics and look through Olson's book in search for a general hypothesis we can put to empirical test. On page 36 we may have found a possible candidate: "The larger the group is, the farther it will fall short of obtaining an optimal supply of any collective good and the less likely that it will act to obtain even a minimal amount of such a good." We repress the impulse to start immediately with preparations for testing because we first want to find general hypotheses that, in turn, explain the relationship just quoted. With the aid of some

further reading and some reconstruction, we come up with the following:

- H₁: The greater the group the smaller the degree of organization.
- H₂: The smaller the degree of organization the less likely that this group is supplied with any public good.

From these two general hypotheses, T₁ (our original hypothesis) follows:

- T₁: The greater the group the less likely that this group is supplied with any public good.

The problem is that H₁ and H₂ are again incomplete hypotheses. What disturbing factors can there be? Clearly, before we can use H₁ and H₂ to get a handle on disturbing factors for T₁, we have to first get a handle on H₁ and H₂.

After some further reading and reconstruction, we find the following series of hypotheses for H₁:

- S₁: The greater the group the greater the organization costs of this group.
- S₂: The greater the group the smaller the relative amount of resources for organization within that group.
- S₃: The greater the organization costs for a group and the smaller the relative amount of resources for organization, the smaller the degree of organization.

From these three hypotheses follows H₁. But each of the new hypotheses must again be considered incomplete. Where do we stop?

The exercise has provided us no doubt with some useful hypotheses and maybe some insights. Degree of organization, organization costs, amount of resources, all these variables we may not have thought of when we began with T₁. Including them in our test procedure will be more promising than the *ad hoc* approach, and yet we still have no way of systematical reduction of incompleteness. It is also unlikely that Olson would have come up with all these hypotheses had he followed this kind of deductive reasoning. It does not generate hypotheses, it only lends itself to the deductive reconstruction of arguments in terms of layers of chains of hypotheses. If some links in the chain "break," i.e. don't turn out the way we had thought they would, what do we do then with T₁? What conclusions do we draw? *Logically*, each breaking link would be a disturbing factor but we don't know how to interpret them *theoretically* and *empirically*.

When such chains of hypotheses contain links to the individual level, as they would if we had continued the regress, the chain actually becomes weaker, i.e. the incompleteness increases. Why is this so? In order to go to the individual level, there must be a hypothesis that links the two levels, such as: "The larger the group the less each group member will contribute towards the costs of the production of public goods." Since T₁ was assumed to be a general

tations, sanctions and institutionalization). They assumed that a model of man should reflect what is important to a particular social science and so they invented SRSM and OSAM to fit their insights. *Homo oeconomicus* seemed to stress all the human aspects that were rather unimportant to sociologists, and thus it was the major target against which SRSM and OSAM were developed. Because RREEMM behaves under certain circumstances as if he or she were a SRSM or an OSAM (e.g. stable, primitive societies, routine behavior, voting provided it is not raining) and because economists had traditionally stayed away from these circumstances, sociologists even had exemplars⁹ to show that SRSM and OSAM had some explanatory value. Yet, in order to have the models of man reflect the sociologically important aspects, sociologists had to exclude all aspects that stress the individual as a focus of initiative. As a result, sociologists were pushed into a position they did not anticipate: they had to conceive of institutional and cultural change as autonomous processes which may have a "direction" but cannot be explained, and they had to concentrate on "orientations" (values, norms, expectations, attitudes, opinions, ideologies, world-views, ethos, ethics, life-worlds and zeitgeists) without being able to explain how these orientations relate to behavior when RREEMM does not behave like SRSM or OSAM (i.e. in most cases in our societies).

Maybe this account is correct and yet beside the point. Is it not so that sociologists are mainly concerned about macro phenomena, like the relation between integration and suicide rates or between class-structure and mobility rates or between the degree of the division of labor and forms of solidarity? If so, what does it matter that behavior of individuals cannot be explained? Could SRSM and OSAM not better be interpreted to be *heuristic* devices (as opposed to explanatory devices) that help establish hypotheses about macro phenomena? In the next section on deduction and model-building, I will attempt to show that this defense is untenable.

2.2 DEDUCTIVE STYLE OF REASONING

In the modern philosophy of science, one view seems to have crystallized concerning theory: a theory of X is the (deductive) explanation of X. In conjunction with the view that it is the ultimate office of science to explain,¹⁰ one can thus say: it is the ultimate office of science to produce (deductive) theories. Two advantages, at least, follow from this view: rigorous argumentation and the possibility for cumulative growth of knowledge. Rigorous argumentation has the added advantage that we can trace the implications of our hypotheses and assumptions (Lindenberg, 1971; Hummell, 1972). Deductive theories also allow the most parsimonious summary of actual or anticipated research findings.

In sociology, this kind of theorizing has been an implicit goal for a long time (see de Vos, 1981), but

it never really took off as an explicit practice. In 1951, Zetterberg has argued forcefully for a deductive approach to theorizing in sociology. For him, a theory in an interrelated set of propositions, and each proposition is a *general* hypothesis. A list of propositions should be analyzed and ordered in such a way that most of the propositions follow from a small set of propositions from this list. This small set, then, contains the general hypotheses, and the other propositions are theorems. An example of such a set of general hypotheses, given by Zetterberg (1965:160f), is:

- I. The greater the division of labor, the greater the solidarity.
- II. The greater the solidarity, the greater the consensus.
- III. The greater the number of associates per member, the greater the division of labor.
- IV. The greater the solidarity, the smaller the number of rejections of deviants.

Zetterberg found some echo among sociological theorists but by and large, it was a modest response. Somehow, his suggestions did not speak to the theoretical imagination of most theorists. For the greater part, his "method" was restricted to the ordering of propositions in sociological textbooks. Empirical sociologists found Zetterberg much more stimulating in the sense that they found their (Durkheimian) goal confirmed: to find *general* hypotheses. Zetterberg had said himself: "the outcome of the theoretical enterprise should be . . . laws." (*ibid.*: 103) Durkheim had maintained that "sociological explanation consists exclusively in establishing relations of causality, . . . a matter of connecting a phenomenon to its cause" (Durkheim, 1950:125) To find general causal connections (laws) ("the ultimate task of empirical research") is thus also a theoretical (explanatory) effort. Zetterberg has (maybe unwittingly) reconfirmed this point of view. Theoreticians merely order and file these general hypotheses.

Hang on. In order to put the contribution of the new political economy into its proper perspective, I have to dwell (boringly if need be) on the issue of deductive reasoning and why it went wrong in sociology (as well as political science before economists entered that field).

Sociological empiricists had one nagging problem concerning the execution of their main task (i.e. to find general hypotheses that are well confirmed): when you test a general hypothesis, you have to make some assumptions about the behavior of uncontrolled variables. Not only is there measurement error, but uncontrolled variables may effectively disturb the hypothesized relationship. In economics, possible disturbing factors are often verbally removed by adding "ceteris paribus." Another way of stating this problem is to say that general hypotheses that relate social phenomena to each other are generally *incomplete*. If all relevant factors could be stated in the hypothesis, there would be no possible dis-

were soon too frustrated to continue and turned furiously against the "individualistic approach" altogether.¹⁶ Others who had not even tried felt their reluctance all the more justified. Homans, insisting on general psychological propositions, had greatly underestimated the advantages of model-building.¹⁷

2.2.2 DEDUCTIVE REASONING WITH DEPTH AND MODEL-BUILDING

In order to be generally useful in the social sciences for the achievement of depth, a theory on the individual level must meet at least the following requirements:

- a. it must not require much information about each individual to which it is applied;
- b. it must make it easy to formulate bridge-assumptions that link social conditions to the individual level;
- c. it must allow bridge-assumptions that link psychological (including physiological) theories to its parameters;
- d. it must allow us to express our degree of ignorance explicitly;
- e. it must be well corroborated as a theory that explains behavior of human beings, inclusive of resourceful behavior.

At first sight, this set of requirements may seem odd. Let me briefly elaborate on each point. First, a behavioral theory may be very good but it needs so much information about each individual that it is practically useless for social sciences who have to explain behavior *on the aggregate*. For example, theories of clinical psychology strike a particular bargain:¹⁸ accurate assessment and prediction for each individual at the price of high information costs per individual. The "watering down" of such theories in order to apply them to aggregates, such as a psychiatric theory of crime (rates), neglects the bargain and pretends that a few selected parameters of the theory will actually suffice and that they can be easily estimated for an aggregate while they are difficult to establish for a single individual. Second, a behavioral theory may be very good but make it very difficult to relate to social conditions of all kinds. For example, various forms of (social) learning theory are well corroborated¹⁹ and yet they make it very difficult to consider even simple social and economic constraints. Let a family value privacy but it can afford only one bedroom for all. What happens when the family income suddenly increases considerably? We cannot say that buying a larger house all of a sudden becomes a more rewarding activity, it simply becomes possible. After some long procedure of translation, it probably will be possible to express all sorts of restrictions, constraints, benefits, costs, expectations, standards etc. in terms of a given learning theory. But this proves nothing. The point is that the focus and language of learning theories were not meant to facilitate the establishment of bridge-

assumptions but to describe certain psychological processes and to relate the theory to certain experimental results.²⁰ Again, the bargain struck for learning theories is a different one than the bargain necessary for a behavioral theory that allows us to go back and forth with some ease from the individual to the collective level. For the latter purpose we need a theory for which indeed the analytical primacy lies with society rather than the individual, *although* it is on the individual level.

Thirdly, *because* psychological theories direct analytical primacy to the individual, their results must be able to bear on any behavioral theory that attempts to link the individual and the collective level. For example, cognitive dissonance theory can be used to establish that in certain circumstances information on particular goods will be preferred to information on some other goods. Or think of the following example: income tax increases; will this increase, reduce or leave unaffected the choice between leisure and work among the highest income brackets? There is no obvious answer to this question. For the establishment of a bridge-assumption about the relation between work and kind of reward, it would be useful to know, that Bandura (1969) found that as involvement and skills in the activities increase, social, symbolic, and self-evaluative rewards assume a strong incentive function. Thus, for the jobs in the highest income brackets that require considerable skills and produce strong involvement, our best guess would be that work effort is income tax inelastic.²¹ In conjunction with the previous point and the following one, it should be clear that this plea for the use of psychological (including physiological) theories should not be misunderstood to mean that one should use them instead of a theory that was meant to add depth to social-scientific hypotheses. Because psychological theories have a different analytical primacy, they should remain auxiliary to these efforts.

Fourth, often situations become too complex to be handled theoretically or we just don't know enough. The kind of behavioral theory we are looking for in the search for depth must allow us any degree of simplification without refusing to grind out predictions. This implies, conversely, that we must be able to replace simplifying assumptions by more realistic ones if and when we think we can handle the added complexity or if and when our knowledge has grown. In short: the theory must allow us to use the method of decreasing abstraction. Bridge-assumptions must thus be able to be made on any chosen level of concreteness. For this very reason, the theory we are looking for cannot be "naturalistic," purporting to describe what goes on in the heads of individuals; it must be an "as if" theory, and in that sense it must be "instrumentalistic."

The fifth requirement speaks for itself but "corroborated" has here a slightly different meaning: the theory must be compatible with well corroborated psychological theories and it must have "worked" time and again in conjunction with adequate bridge-

hypothesis, each part of the chain must also be a general hypothesis. As such, a statement linking the collective level¹² with the individual level implies a behavioral theory in which one set of conditions (say group size) *always* leads to the same kind of behavior (say apathy with regard to contributions). Such a general hypothesis is extremely incomplete, that is, there are many factors that render it frequently false. This circumstance has not remained undetected by perceptive sociologists and it has been used to argue against inclusion of the individual level in sociological theorizing. Many sociologists fiercely defend this argument. It might seem quite surprising, therefore, to observe so many sociologists, among them the most ardent "collectivists," to jump to the individual level whenever they really try to explain social phenomena (even Durkheim did this; see Lindenberg, 1975). As Homans (1964:818) observed: "They keep psychological explanations under the table and bring them out furtively like a bottle of whiskey, for use when they really need help." Why would this be so? As I will argue in the next section, it is exactly the individual level that helps us to specify under what social conditions what kind of behavior is likely to occur. This level adds "depth" to our explanations, even to informal explanations.

2.2.1 DEDUCTIVE REASONING WITH DEPTH

In order to get some systematic handle on disturbing factors, we need theories with more "depth"¹³ than the hypothesis we want to test. This means that we need a theory that tells us under what conditions the (supposedly general but incomplete) hypothesis is (likely) to hold and under what conditions this is not (likely to be) the case. How can we get such a theory?

Sociologists were right to claim that the inclusion of the individual level in chains of hypotheses about social phenomena increases the incompleteness, as we have seen above. Yet, it is precisely the individual level that, if taken by itself, promises more completeness than the "collective" level. The reason for this is fairly straight forward: human nature can reasonably be assumed to be much more stable than social conditions.¹⁴ General hypotheses about human nature can thus be more complete than general hypotheses about social phenomena. In order to increase the completeness of the latter, we would have to make use of the former, whether we are interested in "individuals" or not. Using chains of hypotheses, as we have seen, does not allow us to profit from the higher completeness of the individual level. What other possibility is there?

The crucial step is to avoid general hypotheses in which the independent variable is on the "collective" level and the dependent variable is on the "individual" level. Let's take a general proposition on the "individual" level: "Men are more likely to perform an activity, the more valuable they perceive the reward of that activity to be" (Homans, 1961:55).

The perceived reward of an activity depends on various and changing social conditions, while a proposition that "mixes" the levels would imply that a particular social conditions *always* has the same reward value. For example: "The larger the group the less each group member will contribute towards the costs of the production of public goods"; this proposition implies that it is always the case that contribution towards the costs of public goods becomes less rewarding the larger the group. Changes in social conditions that affect the reward value of contributions thus appear as "disturbing factors" for this (very incomplete) proposition. We can avoid such incomplete propositions by linking social condition and reward value with a "bridge-assumption" (Lindenberg, 1981) that is intentionally *not* general. For example, the following qualified (and rather clumsy) claim is such a bridge-assumption: "In Western Europe and the United States in the twentieth century up to the 1980's the following regularity is said to hold for groups in which a) contributions towards production costs of public goods are voluntary and b) all advantages from group membership do not vary with the size of such contributions: the larger the group, the less valuable contributing towards the production costs of public goods for a member is likely to be." One can do better than that, but as an example this assumption will do. It is qualified as to space (i.e. Western Europe and the United States), time (twentieth century up to the 1980's), and determinism (i.e. "is likely to be").

What we have done by introducing a bridge-assumption is that we have made a distinction between the *initial conditions* of the general proposition on the individual level and the *independent variable* on the collective level, linking the two by a proposition that does not confront us with the problems of incompleteness because it is not a general hypothesis. The value of this distinction between initial condition and independent variable for the explanatory possibilities of the social sciences can hardly be overestimated. In sociology, it was first systematically introduced by Homans (1961), then worked out by Malewski (1967) and expanded by Opp and Hummell (1973).

Initially, the response to Homans's work among sociologists was very positive but soon the enthusiasm ebbed away and even turned into its opposite: Homans's work was frequently used to demonstrate that the "individualistic" approach does not work in sociology. Why has this happened? Homans had worked out this approach for small groups with face-to-face interaction. In this context, it was fairly easy to work out bridge-assumptions and it was even possible to dodge the issue about the generality of bridge-assumptions because historical, institutional constraints played no significant role in small, spontaneous groups.¹⁵ The impression was given that all sociologists had to do, is to apply this approach to other contexts than small groups. Some sociologists tried very hard to do just that but most of them

action costs, social costs, information costs, etc.) and *system-specific utility arguments* (like the maximization of votes or of budgets or of staff), they all indicate topics or aspects of topics with which the new political economy is concerned. Each of these examples (and the list could be longer) reveals the analytical primacy of society in a field that staunchly defends the explanatory primacy of the individual level. How intimately these two kinds of primacies are connected can be seen from the fact that a considerable advance in the theoretical analysis of social and institutional constraints could be made by applying utility theory to its own elements (creating a two-stage model of decision making): "Decision making is like a process of production. It feeds upon a flow of inputs to produce decisions as its output, and this entails using up scarce resources owned by the decision maker: time, energy, and general purchasing power . . . This means that it will scarcely ever be rational to seek to exhaust all relevant information before making a decision. Short cuts will be called for; 'rules of thumb' will be used." (Rothenberg, 1966:231, summarizing work in this area by Shubik, Downs, Stigler and others). Constraints, such as high information costs, strongly influence the input. In this way, the structure of constraints can be related to aggregate behavior via the utility theory.

2.3 THE MICRO-MACRO LINKAGE

As I have argued above, utility theory was made to link the collective level with the individual level for the purpose of achieving depth. In this way, institutions or group size or the cost of information help us explain categories of individual behavior. Because utility theory makes it particularly easy to take the collective level into account on the individual level (for instance through system-specific utility arguments) it is much easier to connect the explained behavior back to the collective level. For example, let's assume that on the basis of a variety of inter-related bridge assumptions about the relationship between government and bureaucracies we conclude (by means of our utility theory) that top civil servants in a bureaucracy tend to maximize budgets. Then this explained behavior for a category of individuals directs our attention back to the collective level: overproduction, rise of state budgets over time, eroding tax bases, etc. The theoretical primacy of the individual level did not get in the way of our analytical primacy of society after the former had done its work. Take by contrast a psychological theory, in which the collective level cannot be directly "brought into" the individual level. Then the kind of behavior we explain reveals the analytical primacy of the individual: defensive, adaptive, aggressive, self-rewarding, coping, problem-solving, avoiding, etc. behavior. These things may be very useful (even for the social scientist, as I indicated above) but they make it very difficult for us to relate the explained

behavior back to the collective level. Model-building for the achievement of depth thus has this dual advantage: it allows us to explain behavior as dependent on social phenomena *and* it allows us to conceive of social phenomena as dependent on behavior much more easily than with naturalistic theories of individual behavior.²⁶

Problems relating to the micro-macro link are, of course, much more involved than the aspects mentioned so far. The relation between micro- and macroeconomics is a vexed problem, and to my knowledge (see for instance Weintraub, 1979) the dust has not settled. Nonetheless, as far as I can see, the distinction between general equilibrium theory and Keynes's General Theory may be called "micro versus macro" but it has nothing to do with what I called the individual versus the collective level.²⁷ Aggregation takes place both in the equilibrium theory and in the General Theory and both make use of theories on the individual level. But by and large, Keynes's abandoned model-building *for the achievement of depth*, and he relied quite heavily on psychological assumptions outside the utility framework in order to render choice as irrelevant as possible.²⁸ If this assessment is approximately right, then it will ultimately be impossible for Keynesians to specify under what conditions the General Theory is supposed to hold other than by reverting to arguments that involve model-building for the achievement of depth, i.e. models that approximate the requirements listed in section 2.2.2 (including resourcefulness). Weintraub (1979) may have it by the right end when he suggested that the great advantages of the General Theory were due to a set of particular bridge-assumptions in neo-classical economics, namely those that implied well-coordinated outcomes (such as perfect information, zero transaction costs, etc.), while questions of unemployment, inflation, and control of economic aggregates imply coordination failures. It has been the merit of the new political economy to take information costs, transaction costs and political constraints very seriously *within* a framework of model-building for the achievement of depth. As far as that is concerned one may expect important contributions from this kind of theorizing for the very topics of coordination failure to which Keynes addressed himself. Much remains to be done.

3. CONCLUSION

To my view, the new political economy has been the most important *recent* development in the social sciences, reviving a program for the social sciences initiated by Adam Smith and some of his contemporaries. This program had gotten lost through the particular development of economics, sociology and historiography. But essential features of this program had stayed alive in economics (and *not* in sociology or historiography), so that its revival only could come from economics. On the other hand, it would be a misnomer to call this program "the economic

assumptions. The addendum about resourceful behavior requires that the theory must be able to convey an individual as a focus of initiative, i.e. it must be a "purposive" theory.²²

The only theory I know that approximates all these requirements is utility theory in various forms, based on RREEMM. It is the heart of model-building for the achievement of depth. This theory has a long history (see Stigler, 1950) but here we are less concerned about its *enrichment* with standard bridge-assumptions (such as an assumption about decreasing marginal utility) as about its virtues in terms of the requirements listed above. From its beginnings with Adam Smith it was a theory on the individual level for the purpose of linking individual and collective levels. In using this theory, he chose utility arguments in such a way that they made this connection particularly easy. For example, he distinguished three "orders" or classes of men in "civilized society" according to the source of revenue: rent (landowners), wages (laborer), and profit (proprietors) (*Wealth of Nations*, Book I, Chapters VIII–XI). Within each class, utility is a function of the amount of revenue in addition to the two general utility arguments: physical wellbeing and social approval. With one stroke he combined the societal and the individual levels in this "mixed" utility function and he could proceed to infer restrictions from the nature of the activity needed to maximize the particular kind of revenue. Given the lack of time and education, the worker has very high information costs which makes it difficult for him to judge which policy decisions are in his interest and which are not in his interest. Consequence: the laborers will be no important political factor unless their information costs are lowered. The other two classes were similarly analyzed. In addition, it was now easy for Smith to make conjectures about the conditions under which, say, laborers and proprietors would come into class conflict, etc. Psychological bridge-assumptions were freely used by Smith in conjunction with (not instead of) the utility theory. For example, he observed that the probability of gain is generally over-estimated while the probability of loss is generally under-estimated, preventing objective risk from having a strong influence on income (*Wealth of Nations*, Book I, Chapters X, Part I). Whether or not this statement is correct, it shows how Smith used such bridge-assumptions.

As the various social sciences developed into separate disciplines, economics was the only one of them to keep utility theory as an explanatory device in the sense just outlined. Historiography did not follow the example given by the Scottish school, *although* many historians kept RREEMM in their repertoire. The use of RREEMM for the understanding of a historical person's behavior, i.e. as a "situational logic," is decidedly not the same as using utility theory with the analytical primacy of society (as Humen, Smith, and Millar had done).²³ Political science, similarly, kept RREEMM in its repertoire

without model-building for the achievement of depth. Sociology did not even keep RREEMM in its mainline repertoire, as I have argued above. If anything changed within economics, it was a further refinement of model-building in this sense. But the crucial elements stayed the same: the importance of non-descriptive (i.e. abstract) bridge-assumptions, the emphasis on the explanation of aggregate behavior, the "instrumentalist" use of utility theory and of bridge-assumptions, and the work on bridge-assumptions in order to reduce their abstractness while keeping their usefulness. Stigler (1965:266f) has summarized this position neatly in his discussion of the bridge-assumption called 'perfect competition': "we should notice the most common and the most important criticism of the concept of perfect competition—that it is unrealistic . . . One could reply to this criticism that all concepts sufficiently general and sufficiently precise to be useful in scientific analysis must be abstract: that, if a science is to deal with a large class of phenomena, clearly it cannot work with concepts that are faithfully descriptive of even one phenomenon, for then they will be grotesquely un-descriptive of others . . . (The) second defense is that the concept of perfect competition has defeated its newer rivals in the decisive area: the day-to-day work of the economic theorist . . . The vitality of the concept is strongly spoken for by this triumph . . . Of course, this is not counsel of complacency . . . much work must be done before important aspects of the definition of competition can be clarified." To the methodological purist (whose clan has contributed very little to the advancement of the social sciences), this statement by Stigler must be quite nauseating. But then it was exactly the strength of economics to stick to model-building in this sense and not to be tempted or pressured by philosophers of science to forgo its advantages for the thrills of realism and naturalism.²⁴

As economics developed it naturally became more and more involved with its own problems, concentrating more and more on bridge-assumptions tailored to these problems. From the outside (say from sociology) the method of model-building for the achievement of depth and the specific bridge-assumptions, such as perfect competition and perfect knowledge, seemed to form one package against which an alternative social science had to be developed. It was precisely the considerable contribution of the new political economy to *show* (not just to claim) that the package could be decomposed and that new bridge-assumptions could be formulated for basically the same theory.²⁵ True to the whole approach, the newer development emphasized the analytical priority of society. This becomes obvious from the kinds of topics. For example, the effects of *kinds of goods* (private, public, sharing, positional, etc.) on behavior and institutions constitute one such topic. Conversely, the effect of *kinds of institutions* on behavior and the production of kinds of goods, is another such topic. *Public choice*, *kinds of costs* (trans-

- individual will find social approval rewarding). This made it seemingly unnecessary to introduce additional (nongeneral) assumptions about the historically variable "symbols" that express a reward (like social approval). Also, the goods exchanged in small groups (viz. sentiments) cannot be stolen and thus such institutions as property rights play no role in such exchanges. Nor would a small-group-study inspire anyone to think of the relevance of such institutions.
16. The most prominent example is Peter Blau who first tried to explain collective phenomena on this basis (Blau, 1964) and then, frustrated by his efforts, turned vehemently against "the individualistic" approach (see Blau, 1974 and 1977, and a critique of Blau's "new" approach: Lindenberg, 1977a).
 17. Initially, the same has happened among those who developed Popper's methodology for the social sciences, most prominently among them Hans Albert (1963) who criticized economics exactly for its model-building and non-naturalistic approach. Later, Albert softened his view (Albert, 1973).
 18. The general necessity to strike "bargains" (or trade-offs) even in science is shown in Lindenberg and Oppenheim, 1978.
 19. The claim that learning theories are well corroborated should not be exaggerated, however. For example, Homans's (1961) behavioral theory, based on Skinner's learning theory, relates value to quantity (frequency). This is very useful because 'normal' utility theory is not stochastic and thus cannot relate utility to quantity of behavior. As Homans (1974:31) explicates: the probability P_i that alternative_i will be chosen is proportional to the reward value of this alternative relative to the sum of rewards values of all other alternatives ($p_i = R_i / \sum R_i$). It can be shown that this theory performs rather badly when choice proportions have to be explained (see Lindenberg, 1981a).
 20. This is probably one of the main reasons why even explanatory (but 'naturalistic') approaches in sociology have great difficulty considering such an important variable as *income* in their behavioral theories. On the basis of utility theory, income and other such variables can be explicitly related to *sociologically* relevant behavior (see Lindenberg, 1982a).
 21. A study by Break (1957) among British lawyers and accountants corroborates this conclusion.
 22. As such, this requirement is not incompatible with operant conditioning if the operant is allowed to be the result of individual resourcefulness.
 23. This is not to say that the use of 'situational logic' (Popper, 1957:147ff and Popper, 1973:178ff) is not useful; it just asserts that situational logic is something other than model-building for the achievement of depth (the former may be a useful ingredient when models are constructed). Situational logic lacks the *dual* focus that is characteristic for models for the achievement of depth: the analytical primacy of society and the theoretical primacy of the individual.
 24. The practical effect of the pressure by the philosophers of science was to make economists sometimes uncertain whether they were busy with a positive or a normative science. Of course, bridge-assumptions can be chosen and/or interpreted to be either positive or normative. Yet, positive assumption are subject to the method of decreasing abstraction while normative assumptions are not.
 25. For example, the work of institutionalists such as Commons (1934) was very important in its own right but it did not show that the package could be decomposed. A similar assessment can be made regarding various historical schools. The impact of both is thus negligible compared to the new political economy regarding the social sciences in general.
 26. A concrete example of the importance of this dual advantage is the explanation of revolutions, a task in which the disadvantage of naturalistic explanations becomes particularly obvious (see Lindenberg, 1982b).
 27. See note 12.
 28. This is similar to what sociologists have done by introducing SRSM, with the great difference that Keynes really achieved a macro-economic theory while sociologists did not succeed in creating a macro-sociological theory (yet).

BIBLIOGRAPHY

- Albert, H., 'Modell-Platonismus,' in: F. Karrenberg and H. Albert (eds.), *Sozialwissenschaft und Gesellschaftsgestaltung. Festschrift für Gerhard Weisser*. Berlin: Duncker and Humblot, 1963, pp.45-76.
- . 'Der Gesetzesbegriff im ökonomischen Denken,' in: *Macht und ökonomisches Gesetz*. Schriften des Vereins für Sozialpolitik, Neue Folge Band 74/1. Berlin, 1973.
- Bandura, A., *Principles of Behavior Modification*. New York: Holt, Rinehart & Winston, 1969.
- Bentler, P. and Speckart, G., 'Models of attitude-behavior-relations.' *Psychological Review*, 86, 1979, pp.452-464.
- Blalock, H., 'The measurement problem: a gap between the languages of theory and research,' in: H. Blalock and Ann B. Blalock (eds.), *Methodology in Social Research*. New York: McGraw-Hill, 1968, pp.5-27.
- . 'Beyond ordinal measurement: weak tests of stronger theories,' in: H. Blalock (ed.), *Measurement In The Social Sciences*. London: Macmillan, 1974, pp.424-456.
- . and Wilkin P., *Intergroup Processes, A Micro-Macro Perspective*. New York: Free Press, 1979.
- Blau, P., *Exchange and Power In Social Life*. New York: Wiley, 1964.
- . 'Parameters of social structure,' *American Sociological Review*, 39, 1974, pp.615-635.
- . *Inequality and Heterogeneity. A Primitive Theory of Social Structure*. New York: Free Press, 1977.
- Break, G., 'Income taxes and incentives to work: an empirical study,' *American Economic Review*, Vol.XLVII, 5, 1957, pp.529-549.
- Buchanan, J., *The Limits of Liberty*. Chicago: University of Chicago Press, 1975.
- Collins, R., *Conflict Sociology: Toward an Explanatory Science*. New York: Academic Press, 1975.
- Commons, J., *Institutional Economics*. Madison: University of Wisconsin Press, 1934.
- Demsetz, H., 'Toward a theory of property rights,' *American Economic Review*, 57, 1967, pp.347-359.
- Doel, J. van den, *Demokratie en Welvaartstheorie*. Alphen aan den Rijn: Samson, 1975.
- Durkheim, E., *The Rules of Sociological Method*. Glencoe: Free Press, 1950 (originally 1895 in French).
- . *Suicide. A Study in Sociology*. Free Press (no city mentioned), 1951 (originally 1897 in French).
- . *Moral Education. A Study in the Theory and Application of the Sociology of Education*. New York: Free Press, 1961 (originally published posthumously in 1925 in French).
- . *The Division of Labor in Society*. Glencoe: Free Press, 1964 (originally 1893 in French).

program" simply because the revival came from economics. The essential features of this program are, as we have seen, the combination of a particular model of man (viz. Resourceful, Restricted, Expecting, Evaluating, Maximizing Man, or RREEMM for short) with model-building for the achievement of depth. In this kind of model-building the analytical primacy is focused on society while the theoretical primacy is focused on the individual. This dual focus, together with RREEMM, allows qualifying explanations of social regularities. This is to say that it generates depth, i.e. qualifying explanations. Under certain circumstances, RREEMM acts *as if* he or she were a SRSM or an OSAM, namely in small, stable and relatively undifferentiated societies and in unrestricted situations, respectively. These situations have falsely been used to corroborate the explanatory power of SRSM and of OSAM.

The ideas going into the making of SRSM contain relevant criticisms of *homo oeconomicus* (that is RREEMM "enriched" with very unrealistic model assumptions), especially regarding the importance of moral obligations and regarding the importance of institutions in general. While the new political economy has successfully dealt with institutions in general, it has not yet sought an answer to the questions concerning moral obligations. As shown elsewhere (Lindenberg, 1983), moral obligations can be adequately handled *within* the RREEMM framework (although adjustments in the formal utility theory are necessary). This does not mean that the consequences of this possibility have been worked out. Here, too, a lot remains to be done. Ricketts (1978: 178) put it in a nutshell: "Thus taking up once more the study of political economy, we are at last beginning to catch up with Adam Smith."

NOTES

1. Game theory, strong on choice, structure (i.e. interdependent decisions) and models acquired its relevance for social sciences in general only on the basis of this renaissance of political economy. Of course, it may have helped inspire this renaissance (as did a number of individual authors, such as Beard, Pigou, Schumpeter, Hayek, Arrow) without having been part of it.
2. It should be stressed that this paper is not meant to serve also as a review article on relevant work done within the new political economy. For that purpose, useful orientations are van den Doel (1975), Frey (1977), Buchanan et al. (1978), McKenzie and Tullock (1978), *Jahrbuch für Neue Politische Ökonomie* vol. 1 (1982).
3. This careful expression leaves open whether only constraints differ among individuals or whether also preferences differ. The more usual assumption is that both differ, but Adam Smith (and recently and much more explicitly, Stigler and Becker (1977)) tended towards the assumption that only constraints differ, so that 'new tastes' could be derived from general human preferences and changing constraints. Even here, we may come back to Smith's position.
4. See also Durkheim, 1974:35ff. This and all other references to Durkheim in this paper stress Durkheim's *program*. One can show that in his substantive work, he used a much more sophisticated model of man (see Lindenberg, 1975). But it was his program (and his work interpreted in the light of his program) that was so influential on the development of sociology.
5. Such an explanation sketch has been worked out by North and Thomas (1977). It is interesting to note that although SRSM is meant to 'catch' the institutional aspects of behavior, such a momentous change as the invention of communal property rights (through the guarding of territory) completely escapes any SRSM account. (See also Demsetz (1967), Lindenberg (1979) and Voss (1982).)
6. The assumption of perfect information does crowd out attention to expectations. It is somewhat ironical that while (role-) expectations play a central role in SRSM, they have a similar effect: they crowd out attention to risk and uncertainty; that is to say: the fiction of perfect information has also slipped into SRSM.
7. That these really are simplifying assumption, i.e. assumption that are strictly speaking false, can be seen from Parsons, 1951:244, 321, 513.
8. In the same way, Marx was forced to make economic change an autonomous process because he had eliminated RREEMM as a historical constant (see Lindenberg, 1980).
9. According to Kuhn (1970), 'exemplars' are one major element of a successful paradigm.
10. This has been forcefully argued by Popper (1973:191-205). Adam Smith argued actually in a similar vein, except that he stressed that for the social sciences, the explanatory principles are likely to be well known ones (see Skinner, 1979:9f).
11. Blalock, 1968 and 1974, has given much attention to this problem, and it may not be so surprising that he, as one of the most eminent advocates of the "general hypotheses" approach in sociology, has turned towards utility theory and explanatory models with depth in recent times (see Blalock and Wilken, 1979).
12. The difference between 'individual' and 'collective' level, as used in this paper, is not quite the same as 'micro' and 'macro' in economics. The 'individual' level pertains to characteristics that can be attributed to an individual, to individual cognitions, motivations, and behavior, and to relations between these; the 'collective' level pertains to collective phenomena (such as collective action, structures, rates for aggregates, distributions, institutions, group size etc.) and to relations among these.
13. The concept of "depth" was introduced by Popper (1973:191-205). See also de Vos, 1981:45ff.
14. This does by no means imply that everything on the individual level is stable; just human *nature* can reasonably be supposed to be stable. Edmund Burke had seen very clearly the importance of Adam Smith's introduction of this stable level for the achievement of depth: "A theory like yours founded on the Nature of man, which is always the same, will last, when those that are founded on his opinions, which are always changing, will and must be forgotten." (Letter 38 from Burke to Smith, Westminster, September 10th, 1759).
15. Some bridge-assumptions were general and entirely on the individual level (as the assumption that every

- _____. *Sociology and Philosophy. With an Article on Durkheim's Life and Work* by Talcott Parsons. New York: Free Press, 1974 (originally published posthumously in 1924 in French).
- Fishbein, M. Ajzen, I., *Belief, Attitude, Intention and Behavior: An Introduction to Theory and Research*. Reading, Mass.: Addison-Wesley, 1975.
- Fox, A., *A Sociology of Work in Industry*. London: Macmillan, 1971.
- Frey, B., *Moderne Politische Ökonomie*. München: Piper, 1977.
- Gadourek, I., *Social Change As Redefinition of Roles*. Assen: Van Gorcum, 1982.
- Gadenne, V., 'Die Unvollständigkeit sozialwissenschaftlicher Hypothesen,' in: H. Albert and K. Stapf (eds.), *Theorie und Erfahrung*. Stuttgart: Klett-Cotta, 1979.
- Homans, G., *Social Behavior. Its Elementary Forms*. New York: Harcourt, Brace & World, 1961.
- _____. *Social Behavior; Its Elementary Forms*. Revised edition. New York: Harcourt, Brace & World, 1974.
- Hummell, H., 'Zur Problematik der Ableitung in sozialwissenschaftlichen Aussagensystemen. Ein Plädoyer für Formalisierung,' *Zeitschrift für Soziologie*, 1, 1972, pp.118-138 *Jahrbuch für Neue Politische Ökonomie*. Band 1, 1982.
- Knight, F., *Risk, Uncertainty and Profit*. Chicago: University of Chicago Press, 1971 (originally 1921).
- Kuhn, T., *The Structure of Scientific Revolutions*. Rev. ed. Chicago: University of Chicago Press, 1970.
- Lindenberg, S., 'Simulation und Theoriebildung,' in: H. Albert (ed.), *Sozialtheorie und soziale Praxis*. Meisenheim; Anton Hain, 1971, pp.78-113.
- _____. 'Three psychological theories of a classical sociologist,' *Mens en Maatschappij*, 50, 1975, pp.133-153.
- _____. 'De structuur van theorieën van collectieve verschijnselen,' in: W. Arts, S. Lindenberg, and R. Wippler, *Gedrag en Structuur*. Rotterdam: University Press, 1976, pp.1-20.
- _____. 'Individuelle Effekte, kollektive Phänomene und das Problem der Transformation,' in: K. Eichner and W. Habermehl (eds.), *Probleme der Erklärung sozialen Verhaltens*. Meisenheim: Anton Hain, 1977, pp.46-84.
- _____. 'Differentiation among people: Blau's revised view of sociology,' *Mens en Maatschappij*, 52, 1977, pp.301-323 (1977a).
- _____. 'Solidaritätsnormen und soziale Struktur.' Paper presented at the annual meeting of the Section for Theoretical Sociology of the *Deutsche Gesellschaft für Soziologie*, 1979, mimeo.
- _____. 'Instigation of and participation in revolts and revolutions: An analysis of Marx' possible contribution to this problem and analysis of an alternative.' Paper presented at the annual meeting of the *Werkgemeenschap Verklarende Sociologie*, Utrecht, 1980, mimeo.
- _____. 'Erklärung als Modellbau: Zur soziologischen Nutzung von Nutzentheorien,' in: W. Schulte (ed.), *Soziologie in der Gesellschaft*. Bremen: University Press, 1981, pp.20-35.
- _____. 'Rational, repetitive choice: The discrimination model versus the Camilleri-Berger model,' *Social Psychology Quarterly*, 44, 1981, pp.312-330 (1981a).
- _____. 'De onvolledigheid van algemene hypothesen: een pleidooi voor verklarende modellen,' *Mens en Maatschappij*, 57, 1982, pp.373-391.
- _____. 'Sharing groups: theory and suggested applications,' *Journal of Mathematical Sociology*, 9, 1982 (1982a).
- _____. 'Theories of revolution and empirical evidence,' in: W. Raub (ed.), *Theoretical Models and Empirical Analysis*. Utrecht: Explanatory Sociology Publications, 1982, pp.63-75 (1982b).
- _____. 'Utility and morality,' *Kyklos*, 36,4, 1983.
- _____. and Oppenheim, P., 'The Bargain Principle,' *Synthese*, 37, 1978, pp.387-412.
- Malewski, A., *Verhalten und Interaktion*. Tübingen: Siebeck-Mohr, 1967.
- Marx, K., *The Grundrisse*. Edited and translated by D. McLellan. New York: Harper and Row, 1971.
- McKenzie, R. and Tullock, G., *Modern Political Economy*. New York: McGraw-Hill, 1978.
- Meckling, W. 'Values and the choice of the model of the individual in the social sciences,' *Schweizerische Zeitschrift für Volkswirtschaft und Statistik*, 112, pp.,545-559.
- North, D. and Thomas, P., 'The first economic revolution,' *Economic History Review, Second Series*, vol.XXX,2, 1977, pp.229-241.
- Opp, K.D. and H. Hummell, *Soziales Verhalten und soziale Systeme*. Frankfurt/Main:Athenäum, 1973.
- Olson, M., *The Logic of Collective Action*. Cambridge, Mass.: Harvard University Press, 1965.
- Parsons, T., *The Social System*. Glencoe: Free Press, 1951.
- _____. 'The life and work of Emile Durkheim' in: Durkheim, 1974, pp.xliii-lxx.
- Popper, K., *The Poverty of Historicism*. London: Routledge & Kegan Paul, 1957.
- _____. *Objective Knowledge. An Evolutionary Approach*. Oxford: The Clarendon Press, 1973.
- Raub, W., (ed.), *Theoretical Models and Empirical Analysis*. Utrecht: Explanatory Sociology Publications, 1982.
- _____. and Voss, T., *Individuelles Verhalten und gesellschaftliche Folgen*. Darmstadt: Luchterhand, 1981.
- Ricketts, M., 'Adam Smith on politics and bureaucracy,' in: Buchanan et al., 1978, pp.173-182.
- Rothenberg, J., 'Values and value theory in economics,' in: S.R. Krupp (ed.), *The structure of Economic Science*. Englewood Cliffs, N.J.: Prentice-Hall, 1966, pp.221-242.
- Schütte, H.G., 'Het dilemma van de macrosociologie,' in: W. Arts, S. Lindenberg, and R. Wippler, *Gedrag en Structuur*. Rotterdam: University Press, pp.51-74.
- Skinner, A., *A System of Social Science. Papers Relating to Adam Smith*. Oxford: Clarendon Press, 1979.
- Smith, A., *The Theory of Moral Sentiments*. (originally 1759).
- _____. *The Wealth of Nations*. (originally 1776).
- Stapf, K., 'Einstellungsmessung und Verhaltensprognose,' in: H. Stachowiak et al. (eds.), *Bedürfnisse, Werte und Normen im Wandel*. Band II. München: Fink, 1981.
- Stigler, G., 'The development of utility theory,' *The Journal of Political Economy*, vol.LVII, 1950 (also reprinted in Stigler, 1965).
- _____. *Essays in the History of Economics*. Chicago: University of Chicago Press, 1965.
- _____. and Becker, G.S., 'De gustibus non est disputandum,' *The American Economic Review*, 67, 1977, pp.76-90.
- Turner, J.H., *The structure of Sociological Theory*. Homewood, Ill.: Dorsey, 1982, 3rd ed.
- Vos, H.de, *Verklaring en Interpretatie in de Sociologie*. Deventer: Van Loghum Slaterus, 1981.
- Voss, T., 'Rational actors and social institutions: the case of the organic emergence of norms,' in: W. Raub (ed.), *Theoretical Models and Empirical Analysis*. Utrecht: Explanatory Sociology Publications, 1982, pp.76-100.
- Weintraub, E.R., *Microfoundations. The Compatibility of Microeconomics and Macroeconomics*. Cambridge: University Press, 1979.
- Wippler, R., 'The structural-individualistic approach in

Dutch sociology,' *The Netherlands Journal of Sociology*, 14, 1978, pp.135-155.

Zetterberg, H., *Theory and Verification in Sociology*. Totowa: Bedminster press, 1965, 3rd edition.