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Abstract

The main purpose of this paper lies in the solution of a specific problem area, referred to as *modeling dilemma*. In doing so, two major and, hopefully, innovative claims can be made: First, the social sciences can be characterized by *at least* two pragmatically highly differentiated modeling approaches to the socio-economic ensembles which, in different degrees, offer *complementary* classes of information and which, moreover, *increase* the understanding of the complexities of these socio-economic universes. Second, these two major modeling approaches have become, by now, part and parcel of separate *epistemic cultures* which will, in a process of co-evolution, form major *basins of attraction* for future practices within the social sciences.

Zusammenfassung

In diesem Artikel soll ein spezielles Problemgebiet gelöst werden, das als *Modellierungs-Dilemma* bezeichnet wird und das den *prekären* Status vieler Annahmen im Bereich der Modellbildung in der Ökonomie, der Soziologie oder auch der Politikwissenschaft zum Inhalt hat. Mit dem angebotenen Lösungsansatz sollen zudem gleich zwei neuartige Behauptungen verbunden sein. So können, so die *erste* Behauptung, die Sozialwissenschaften wenigstens durch *zwei* unterschiedliche und hochgradig ausdifferenzierte Modellierungsweisen charakterisiert werden, welche zudem *komplementäre* Informationen bereitstellen und jeweils auf *ihre* Weise einen Beitrag zum Verständnis komplexer sozio-ökonomischer Ensembles leisten. *Zweitens* gehören diese beiden unterschiedlichen Modellzugänge mittlerweile zu jeweils unterschiedlichen *epistemischen Kulturen*, welche hinkünftig ko-evolutiv wichtige Ziel- und Brennpunkte für die sozialwissenschaftlichen Forschungen darstellen werden.

In the academic year 1939/1940, Joseph A. Schumpeter organized a *non*-dying Harvard Seminar[1] on the topic of rationality in the social sciences, including economics. After one year of intense lectures from different scientific fields, Talcott Parsons was to edit a collection of articles originating from this seminar. Schumpeter himself contributed a draft version where he introduced, by systematic ordering, a table of four elementary problem areas relevant for the topic of rationality. Accordingly, Schumpeter distinguished between two areas of observation (observer/observed), and, moreover, between an internal and external perspective, where the *internal* or *subjective* point of view referred to inner states, intentions or preferences of individuals, be they on the observer or on the observed side, and the *external* or *objective* side could be qualified as *ascriptive*. Thus, Schumpeter arrived at the *identification* of four main problem areas on rationality. However, Schumpeter did not succeed in providing *satisficing* accounts and solutions for each of the four rationality fields. Moreover, the planned publication underwent the process of a *dying* Harvard or Non-Harvard collection volume. [2] Consequently, the rest *had* to be silence ...[3]

This small historical episode has been chosen as an introduction because of two reasons. First, it offers a *systematic* summary of essential problem areas with respect to the utilization of rationality-assumptions in the social sciences.[4] And second, the episode makes abundantly clear that a highly problematic configuration, subsequently introduced and defined as *modeling dilemma*, has persisted for decades, not only within economics, but within the social sciences in general. Moreover, the

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1 The phrase of the *dying seminar* in Harvard refers to the following narrative by Thomas C. Schelling (1978) -

> *Somebody organizes a group of twenty-five who are eager to meet regularly to pursue a subject of common interest. It meets at some hour at which people expect to be free. The first meeting has a good turnout, three quarters or more, a few having some conflict. By the third or fourth meeting the attendance is not mich more than half and pretty soon only a handful attend. Eventually the enterprise lapses, by consent among the few at a meeting or by the organizers'giving up and arranging no more (SCHELLING 1978:91f.)* -

where an unequal distribution of critical treshhold values for attendance leads to a continuous fading away of participants.

2 In a mode of analogy, the phrase of the *dying collection volume*, Harvard and otherwise, could relate to the following complex configuration -

> *Somebody organizes a group of twenty-five who are eager to write a paper on a subject of common interest. He sets a dead-line at which people are expected to have completed their contribution. The first dead-line has a good turnout, two thirds or more, a few having some conflict. By the third or fourth dead-line the additional manuscript-turnout becomes negligible and pretty soon no one sends a manuscript any more. Moreover, a small number of authors withdraws their contributions. At this stage, the enterprise* eventually *lapses, since an interesting dynamic development is set in motion ...* -

where unequal distributions of critical treshhold values for *deadly* deadlines on the one hand and for *necessary* necessities to get articles published lead to all sorts of dynamic trajectories, *including* the fading away of manuscripts ...

3 For a brief summary of the Schumpeter seminar, see SWEDBERG 1994:175f.

4 In the following article, the term *social science* refers to *all* types of scientific disciplines which, in one way or the other, are concentrating on human actions, interactions and their respective results. Thus, economics as well as sociology, political science or psychology form *core* elements for a comprehensive set of social science disciplines. Moreover, the examples in this article are deliberatley chosen from a wide array of disciplinary fields, ranging from economic theory or econometrics to sociology and political science. Thus, any example from a disciplinary segement, which is supposed to exemplify a specific piece of information, should be taken as *pars pro toto*, since highly similar configurations could be identified within disciplinary areas, too.

utilization of rationality assumptions must be considered only as part of a *wider* methodological conundrum, since a *large* number of *heterogeneous* simplifying components, ranging from statistics to the needs and peculiarities of the algorithms used, enters into the *actual* model-building processes ...
At this stage, it seems *highly* appropriate, to introduce the central focus of the present article, namely the concept of the *modeling dilemma,* in closer detail. Put in a conventional methodological perspective by separating between *empirical* and *normative* domains of discourse, the following dilemma arises, seemingly by necessity, within the social sciences and its model-building operations[5], past and present:

> On the one side of the horn, any *empirical* interpretation of the normal model-applications in economics, sociology, political science or demography is confronted with the immediate objection that *essential* model-building operations violate even the most tolerant test-conditions. Consider the following excerpts from a fairly recent book on macroeconomics which reflects the current state of the art of macro-economic modeling (FRISCH/WÖRGÖTTER 1993) and which, due to its *very* characteristic features, has been quoted extensively.
>
> > Consider a small open economy inhabited by a large number of identical individuals. The lifetime utility of the representative individual is given by ... (CALVO/VEGH, 10)
> > In order to account for the international trade in capital goods, we assume that both domestically-produced and imported goods can be converted into an investment good according to a constant returns-to-scale technology ... (GAVIN, 31)
> > There are three regions: Germany, France (which together make up Europe) and the United States ... here representing the rest of the world ... Exchange rate developments are perfectly anticipated apart from the effects of initial shocks (HALLETT *et al.*, 49)
> > Domestic producers are assumed to maximise profit ... by optimally choosing the variable inputs L_t and N_t ... We assume that domestic residents allocate their financial wealth between domestic money, domestic bonds and foreign bonds (HOF, 73f.)
> > Production is carried out by many identical competitive firms. For notational simplicity, the number of firms is equal to the size of the population (also equal to the size of the labor force) (HOON/PHELPS, 97)

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[5] The expression *model-building* is confined to those social science frameworks only that are characterized by a comparatively high degree of formalization and, moreover, are utilized within an *explanatory* context (See, e.g. FARARO 1989, TROITZSCH 1990). Thus, the set of models under consideration ranges from classical macro- or microeconomic equilibrium models (WEINTRAUB 1977), to the growing stock rational choice-versions both in political science or in sociology (ELSTER 1983, 1986), or to the rapidly expanding class of complex models *across* the social sciences (ANDERSON/ARROW/PINES 1988, CASDAGLI/EUBANK 1992, CASTI 1992, 1994, HAAG/MUELLER/TROITZSCH 1992, JEN 1990, MÜLLER/HAAG 1994, STEIN 1989, WEIDLICH/HAAG 1983) ...

> We will assume a small open economy, and hypothesise a state of perfect capital mobility, that is, perfect substitutability between domestic and foreign interest-bearing assets (CLAASSEN, 137)
>
> Investment depends on a number of non-quantifiable factors such as political stability and the industrial relations climate. Assuming these are favourable, we may write investment as a function of expected prices and their variance ... (WORRELL, 161)
>
> The model contains a rudimentary construction sector supplying a durable good producing housing services ... For simplicity, no rental market for housing is included, that is to say, all dwellings are owner-occupied (NIELSEN/SORENSEN, 205)
>
> We assume that there are two classes of speculators. One class is called 'chartists' (noise traders), the other 'fundamentalists' ... The 'chartists' use the past of the exchange rates to detect patterns which they extrapolate into the future. The 'fundamentalists' compute the equilibrium value of the exchange rate (De GRAUWE/De WACHTER, 355)
>
> Agents are assumed to have a qualitative (intuitive) understanding of the models. This is formalised by assuming that they use their own estimates (guesses) of the parameters in place of the true values. The assumption is that these parameter estimates have the same algebraic signs as those of at least one of the RE reduced forms. (GOLDBERG/FRYDMAN, 384)
>
> It is supposed that there are two players on the market, and 'market expectations' are determined as a weighted sum of rational expectations and chartist expectations. (VIJAYRAGHAVAN, 401)

Thus, it seems quite obvious that any interpretation for these stage setting modeling assumptions as empirically *well*-founded or *valid* runs into the serious risk of having to accept *any* account, be it astrology, be it in the creationist spirit or be it, more generally speaking, of the type characterized by Martin Gardner as *bogus science* (GARDNER 1981), as genuinely *empirical*.

Turning now to the *other* side of the dilemma by charcterizing the prevalent modes of operation as *normative*, one is immediately confronted with a different and similarly devastating inconsistency. Why? Take, for example, the three region assumption in the article by Hallett *et al.* Here, one cannot find *any* normative commitment that the world *should* consist of three major nation-states only. In a similar manner, the Nielsen and Sorensen-paper does not state that it would be *rational* to exclude a rental market for housing. Consequently, any normative interpretation imposes an unjustifiable and, in most instances, *highly* implausible account of the role and function of essential model-building assumptions. Moreover, any normative assessment clearly violates the actual practices with respect to the output of economic, political or sociological model-building

which, once again, is not phrased in terms of goals, reachability and choices of appropriate means but which, in most instances, is couched in a conventional *explanatory framework* by focussing on specific interaction patterns or on a set of specially interlinked causes.

Consequently, Schumpeter's fourfold rationality domains turn out to be a comparatively small sub-set of a wider class of modeling components, rational or otherwise[6], whose crucial deficiency and difficulty lies in their unresolved status, placing them apparently in the nowhereland *between* empirical and normative social science applications.

1. Escape-Strategies

Even in the year 1995, the *puzzling* aspects with respect to the utilization of rationality assumptions and other simplifying devices in the course of social science model-building have not vanished. On the contrary! A small non-representative sample from a recent survey on the significance of testing in econometrics from the *Journal of Econometrics* (KREUZENKAMP/MAGNUS 1995a) reveals highly unfulfilled preferences -

> Many outsiders are doubtful of the value added of econometric testing ... But also many econometricians are increasingly worried about the credibility gap between econometric theory and applied economics (KREUZENKAMP/MAGNUS 1995b:5)
>
> There is a fragmentation of traditions, with some groups prosecuting empirical practices semi-decoupled from most formal economic theory, others hewing to some loose Marshallian or Nash game-theoretic (as opposed to Walrasian) dictates, some standard statistical practices mutating into theoretical traditions (as with rational expectations), other decoupled theoretical traditions mutating into statistical projects ..., and finally, numerous statistical practices lacking any solid foundation in probablility theory at all. That all can, if they so choose, claim to be part and parcel of the orthodoxy is due to some loose shared beleifs, such as privileging constrained individual optimization or perhaps treating the standard restrictions upon utility functions as inviolate. (MIROWSKI 1995:32)
>
> It is usually assumed that economists test to strengthen conviction. How this is achieved is mostly left undisclosed. In the conventional view, there is a clear line of logical implication

[6] It must be emphasized, once again, that so many of the modeling parts which have been quoted in the introductory chapter like the equalisation of the number of firms with the size of the population (HOON/PHELPS 1993:97) *cannot* be attributed to the field of *rational behavior* and the domains of *homo economicus*.

from theory to model; but as we shall see, in the process there is a narrowing and specialization of the hypothesis so that it is not clear what weight should be placed upon the result of a test on the model. (KIM/MARCHI/MORGAN 1995:81f.)

Given the unresolved status of core notions like *testing* and *empiricity* within an entire discipline and, more generally, within the social sciences as a whole, it should come as no surprise that a comprehensive set of escape strategies has been furnished so far which follow *one* of the *twelve* directions[7] presented below in a simple alphabetical ordering:

ABANDONMENT: According to the first line of argumentation, any model-exploration into the socio-economic environments has the unavoidable consequence of becoming a *necessary* failure. In the words of David F. Hendry's Golden Rule Nr. 4 -

Stick to being a theorist (HENDRY 1987:30)

any attempts to link the socio-economic universes and data therefrom with socio-economic models will suffer from incurable inadequacies, namely *unbridgable* distances between model-worlds and the intricacies of human life-worlds. While this type of argument is hardly found in contemporary social science methodology any more, it must be noted that in the evolution of economics as a science, especially during the years of the well known *Methodenstreit* in Germany, the *abandonment*-strategy has played a vital and very controversial role. Likewise, the famous *Positivismusstreit* during the 1960's has, from the viewpoint of critical theorists, brought forward interesting variations on the *thema* of model-abandonment for the social sciences in general. (ADORNO *et al.* 1972)

APPROXIMATIONS: The *second* group of arguments stresses the fact that *any* modeling effort *has* to use approximations. Thus, the modeling assumptions utilized within the social science realm, economic or otherwise, are not confined to the humanities alone, but can be found within the *whole* scientific arena. For example, physics has introduced concepts like *ideal* gas, *ideal* temperature, etc. which must be qualified as approximations, too. (HEMPEL 1966) Not only that, also typologies in the social sciences like Max Weber's heuristic devices for ideal types which remain well below the level of proto-modeling have to rely on distances and approximations. Thus, upon *very* close inspection, modeling frameworks like Rational Choice differ not substantially from life-world accounts like those elaborated by Alfred Schütz. (ESSER 1990, 1991)

---

[7] A *systematic* treatment of escape strategies will, in all probability, reveal *additional* approaches to the modeling dilemma so that the number reached here, namely twelve, should be considered as a biblically inspired *limit number* and *not* as the result of a comprehensive and *all*-encompassing taxonomy.

CONFIDENCE-INTERVALS: The *third* line of attempted escapes from the modeling dilemma has been prominently advocated by Edward E. Leamer and can be summarized in a single device -

> Experience has taught us that many inferences are quite fragile. And since we have no formal tools for separating fragile from sturdy inferences, we tend to act as if no inferences are sturdy. This unfortunate state of affairs would be much improved if we used a statistical theory that explicitly allows some inferences to be fragile and others to be sturdy. Inferences based on intervals of probabilities have this property. In some cases, after viewing the data, the intervals of probabilities will be short enough to be useful, but in other cases incredibly narrow initial probabilities will be required to produce usefully narrow posterior probabilities. (LEAMER 1987:3)

Thus, the introduction of probability intervals and, moreover, of probability distributions within such an interval may be seen, according to Edward E. Leamer, as the necessary pre-requirement which lies at the basis of subsequent models and the statistical inferences derived from them.

DUALISM: Despite David E. Hendry's proclamation of the *Fourth Golden Rule* quoted above, he himself offers an interesting advice to those *who still wish to undertake empirical research in economics*.(HENDRY 1987:30) First, Hendry separates the realm of models into two types, namely into *theory models* which are -

> free creations of the human mind, deriving implications from asserted theory relationships involving context-dependent latent constructs (IBID:30)

and *empirical models* which are -

> anything but free creations. Their form is usually dependent on a corresponding theory model, but their properties derive from the process that actually generated the data, that is, observations on the economic mechanism of the relevant time and place filtered via a measurement system whereby certain data constructs are definded and quantified. (IBID:31)

From this basic distinction, Hendry arrives at a comprehensive set of evaluation criteria for both the empirical and the theoretical model set which, under the heading of *congruency,* includes criteria like *exogeneity* or *invariance* for the empirical models and *consistency* and *identifiability* for the theory models. Finally, Hendry is able to *close* his

dualist strategy by operationalizing a criterion of *encompassing* both for rival models as well as for consecutive stages of a specific model family through which *progressive* research programs (Imre Lakatos) can be identified.

EXTERNALIZATION: According to the *fifth* view, model-building, especially the utilization of rationality assumptions, should *not* be considered, contrary to the *history* of decision and action theory, as a variation of *intentional* explanations, assuming, on the one hand, the *causal* efficacy of reasons and, on the other hand, the rationality as well as the internal consistency of beliefs and preferences of agents. Instead, an *externalization* move which is in acoordance with experimental settings for the psychology of human decision procedures (SIMON 1955, 1956) postulates that one has to distinguish clearly between an *internalist* from an *externalist* explanation:

> Describing the conventional requirement of intentional causal agency as an internalist interpretation, they (i.e. externalists, K.H.M.) contend that it is unnecessarily demanding for many of the questions social scientists study ... Accordingly, they propose that the theory should be thought of as illuminating 'structures of social interaction in markets, governments, and other institutions ...As such they do not necessarily depend on psychological foundations. (GREEN/SHAPIRO 1994:21f.)

On this account, rationality assumptions as well as other behavioral or environmental specifications should be interpreted, at worst, in a *counterfactual*, externalist manner ....

INFANCY: The *sixth* strategy lays heavy emphasis on the *preliminary* nature of the model-assumptions which are characterized as a *first*, albeit a necessary approximation of comparatively young disciplines (see e.g. SPENGLER 1961) which, in due course, should be replaced at some later stage by their appropriate and empirically *well*-founded counterparts. Consequently, equilibrium models have been or will be supplemented by their non-equilibrium counterparts, static models by dynamic ones (e.g. GOODWIN/PUNZO 1987), linear preference models by nonlinear preference and utility theory (see e.g. FISHBURN 1988), the set of models with three regions will be generalized to the case of n areas, perfect capital mobility will be replaced by an imperfect version ... In a generalized perspective of the *very* long run, the existing model stock will become, by and large, *dead* and will be substituted by a comprehensive new model-repertoire which, finally, will be able to account for the actual complexities of socio-economic life.

INSTRUMENTALISM: In the *seventh* version, made originally famous by Milton Friedman, the importance of model-building assumptions is effectively reduced to zero,

since the only viable assessment criterion is seen in the successful *predictive* performance of the model as a whole.

> The relevant question to ask about the 'assumptions' of a theory is not whether they are descriptively 'realistic', for they never are, but whether they are sufficiently good approximations for the purpose in hand. And this question can be answered only by seeing whether the theory works, which means whether it yields sufficiently accurate predictions. (FRIEDMAN 1962:15)

Thus, in Friedman's paradigmatic *Don't mind*-strategy, model assumptions, contrary to his dictum on money, do *not* matter ....

MODEL-SELECTION: An *eigth* move away from the dreary consequences of the modeling dilemma lies in the abandonment of problems of model testing altogether and in the shift to questions of model *selection* only, a move moreover, which has been prompted by the -

> difficulties with testing economic theories, particularly that the theories may be vague, may relate to a decision interval different from the observation periode, and may need a metric to convert a complicated testing situation to an easier one. We argue that it is better to use model selection procedures rather than formal hypothesis testing when deciding on model specification. (GRANGER/KING/WHITE 1995:173)

NONEXPERIMENTAL MODELING-TECHNIQUES: Already the *ninth* attempted path away from the modeling dilemma consists in a re-direction of basic modeling designs, away from a methodological spectrum suited for experimental data, and directed towards designs especially appropriate for non-experimental data.

> Although a multitude of reasons has led to this unsatisfactory state of affairs, ... the single most important contributing factor is the nonexperimental nature of the overwhelming majority of economic data whose modeling is undertaken using statistical procedures better suited for 'experimental-like' data. (SPANOS 1995:190)

Thus, for the social sciences, too, an adoption of biometric methods is advocated where concepts like chance and randomness play, contrary to the conventional theory of errors, a crucial and decisive role.

> In the biometric tradition, 'chance' is inherent in human behavior because of its complexity and the data are viewed as realizations of random variables. In this tradition, 'randomness' enters the modeling via the joint distribution of the observable random variables involved and not that of the errors. Using such joint distributions, the concepts of correlation and regression were introduced. (IBID:207)

SEGMENTATION: Following GREEN/SHAPIRO (1994:27), the segmentation strategy restricts model accounts like Rational Choice to a set of *paradigmatic* applications only. In this manner, the predominant research task, *before* utilizing modeling approaches in the social sciences, becomes the assessment of the adequacy or of the appropriateness of the proposed research field with respect to modeling operations. According to the segmentation view, thus, only those domains should be selected for modeling which fall into the set of *admissable* modeling segments.

STRUCTURALISM: An eleventh line of reasoning stresses the ill-founded character of the notion of empirical testability by furnishing a set-theoretic apparatus in which a clear differentiation must be made with respect to the theoretic *core* of a model, economic or otherwise, and the realm of its *intended applications* (BALZER *et al.* 1984, BALZER *et al.* 1987, SNEED 1979, STEGMÜLLER 1980, STEGMÜLLER *et al.* 1982). From a structuralist perspective, then, the problem of empirical testability depends crucially on the *demarcations* drawn *within* the structuralist reconstructions.

WEAKENING: Finally, according to a last group of escape strategies, a split has to be introduced between *thick (broad)* and, not surprisingly, *thin* types of frameworks. (ELSTER 1985, FEREJOHN 1991, 1993) Taking, once again, Rational Choice models of human action as reference case, *thick (broad)* accounts are assumed to asssume too much by positing specific preferences and beliefs on part of human actors, whereas a thin rationality program presupposes only the far weaker requirement that subjects *efficiently* employ the means available to pursue their ends. In its *thinnest* form, only *consistency* requirements with respect to weak preference ordering are needed in order to be able to interpret social actions as *rational*.

It would be too tedious, especially at this point, to highlight the major shortcomings and deficiencies of the twelve escape routes from the modeling dilemma. Suffice to say that despite the apparent ingenuity of the arguments it seems nearly impossible to overcome this dilemma: Any *empirical* justification strategy which *is* in accordance with the *actual* practices of the model-building processes in the social sciences, of *data* collections, of model-*testing* procedures and of model-*results*, cannot offer a *satisficing* account of the *empirical* status of essential model-building assumptions. *E converso*, any normative interpretative framework is not only forced to neglect the *empirical* character of the model-

building practices, but also the *non*-normative utilization contexts. Moreover, the modeling dilemma is not simply confined to the dismal science of economics, but reappears in political science, in sociology or psychology as well. It, the modeling dilemma has assumed, by now, pervasive proportions ...

2. Preliminary Considerations

In order to transform Schumpeter's draft version and, moreover, the twelve escape-strategies elaborated so far into a *satisficing* escape from the modeling dilemma, a requirement of *historical reachability* will be introduced. The subsequent dissolution-sketch of the modeling dilemma will utilize only those cognitive elements that have been available in Schumpeter's days, too. In other words, the dissolution will be accomplished *via* a reconfiguration and rearrangements of cognitive components well known and ready at hand fifty years ago.[8]

The starting point for the successful dissolution of the modeling-dilemma in the social sciences lies, first, in the utilization of a very well-known and, by now, classical distinction, originally made famous by Rudolf Carnap already in the 1940's, namely the differentiation between *pure* forms of semantic analyses and their *descriptive* counterparts.[9] By analogy, a similar differentiation will be introduced here with respect to *pragmatics* by differentiating between two main roads of analysis, namely between *pure* pragmatics on the one hand and *descriptive* pragmatics on the other hand.[10] Thus, the

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[8] In certain contexts within the history of science, especially in the case of dilemmata, it becomes worthwhile to stress the point that, by an *adequate* process of permutations and reconfigurations, or, to borrow a phrase from Douglas R. Hofstadter, by a suitable *variation on a thema* (HOFSTADTER 1985), a successful problem-solution would have been ready at hand already a long time ago.

[9] With respect to semantics, this separation took, according to Rudolf Carnap, the following form:

> *By descriptive semantics we mean the description and analysis of the semantical features either of some particular given language, e.g. French, or of all historically given languages in general .. On the other hand, we may set up a system of semantical rules, whether in close connection with a historically given language or freely invented; we call this a semantical system. The construction and analysis of semantical systems is called pure semantics ... Pure semantics consists of definitions ... and their consequences; therefore, in contradistinction to descriptive semantics, it is entirely analytic and without factual content.* (CARNAP 1975:11f.)

[10] Thus, the two main roads for *pragmatic* analyses can be formulated in the following variational manner:

> *By descriptive pragmatics we mean the description and analysis of the pragmatic features either of some particular given scientific language games, e.g. during the period of the French Enlightment, or of all historically given scientific language games in general .. Thus, descriptive pragmatics describes facts; it is an empirical science. On the other hand, we may set up a system of pragmatic rules, whether in close connection with a historically given scientific language games or freely invented; we call this a pragmatic system. The construction and analysis of pragmatic systems is called pure pragmatics ... Pure pragmatics consists of rule constructions ... and their consequences; therefore, in contradistinction to descriptive pragmatics, it is entirely analytic and without factual content.*

main ingredients for a *pragmatic* analysis of science lie in the area of *scientific language games* and *rule-systems* which characterize the essential moves, operations or practices of such language games.[11] One of the most interesting and heuristically fruitful tools for *both* types of pragmatic analyses of scientific language games consists, as has been indicated already by another member of the Vienna Circle, namely by Otto Neurath, in a *morphological* analysis[12] (Neurath 1981). Neurath has elaborated on the morphological method in various articles around the time Schumpeter had finished his *Vergangenheit und Zukunft der Sozialwissenschaften* (1915). Proceeding along the *analytical* branch of morphological analyses in the field of pragmatics, i.e. scientific rule systems, one could, in principle, define a *large* number of different cross-tables. Take, for example, the classical dimension, dating back to David Hume, between the empirical and the normative realms (HUME 1989, STREMINGER 1994), take, as a special instance, the subsequent differentiation between two areas of decision theory as a reference point -

> Normative decision theory ... is deductive. It postulates certain criteria of optimality or rationality or equity and derives strategies or methods of allocation or methods of aggregating preferences that are supposed to satisfy these criteria. A descriptive theory starts with observations of how actors choose in given classes of decision situations and attempts to describe their behaviour as systematically as possible. (RAPOPORT 1989:5f.) -

and take, finally, a distinction put forward by Mario Bunge (BUNGE 1977, 1979, 1983a, 1983b) who differentiated strictly between two basic types of systems approaches, namely *concrete* and *conceptual* ones -

> A system, then, is a complex object, the components of which are interrelated rather than loose. If the components are conceptual, so is the system; if they are concrete or material, then they constitute a concrete or material system. A theory is a conceptual system, a school a concrete system of the social kind. These are the only kingdoms we recognize: conceptual and concrete. (BUNGE 1979:4)

Such a two-fold separation can lead to an interesting cross-table in which important groups of scientific practices or language games can be distributed across the resulting four areas.

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[11] Since the general focus lies in a pragmatic analysis, it will become clear, in due course, that the classical controversies of the Carnap-Quine debate (QUINE 1961, CREATH 1990) simply *cannot* arise. Why? Because the pure or analytical dimension will receive, due to the *pragmatic* perspective, a thoroughly *Quinean* interpretation and justification ...

[12] For a morphological analysis, be it along the empirical or along the analytical path, the necessary research steps can be summarized in the following manner: *First*, a small number of different and heterogeneous dimensions must be identified and arranged as cross-tables, offering a m x n *array of distinct combinations. Second*, these cross-tables can then be used in different ways, ranging from the empirically observable and measurable distribution of these different combinations up to the detection of white spots which have not been realized so far ... (See also DUBACH 1977)

Table 1: A Morphological Space for Language Games in the Social Sciences

		Dimension$_2$	
		empirical	*normative*
Dimension$_1$	*concrete*	Area I	Area II
	conceptual	Area III	Area IV

More concretely, these four areas imply a division of research practices within the social sciences according to which large parts of evaluation research, planning or optimizations occupy Area II, in which most components of linguistics and language-based research are situated in Area III, in which formal areas like mathematics or statistics lie within Area IV - and in which the main propoprtion of the scientific output resides within the first area.[13]

With respect to the modeling dilemma itself, the four problem areas of the Schumpeter sketch on rationality can be generalized and arranged in the following manner:

Table 2: A Morphological Space for the Modeling Dilemma

		Dimension$_2$	
		observer	*observed*
Dimension$_1$	*internal*	Dilemma I	Dilemma II
	external	Dilemma III	Dilemma IV

The problems of treating rationality assumptions and similar simplification components within the model building-operations in the social sciences can thus be separated into four areas. The modeling dilemma itself, while situated in all four domains can be treated on two distinct levels, namely on a general and on a specific niveau. According to the *specific* levels, modeling components, like rationality assumptions and the like, are utilised *within* one of the four particular dilemmata areas of Table 2, whereas in the *general* version the peculiarities of specific utilization contexts do not play a

[13] It might be asked to which field the morphological explorations of the present article belongs, since it is neither empirical in nature nor concerned with the conceptual structures of social science research nor, for that matter, normative in character ... It will become clear, within the subsequent chapters, that *another* dimension must be added to the two-dimensional matrix of Table 1 so that a *satisficing* location of the morphological approach can be undertaken. By chance, this additional dimension will also effectively dissolve the modeling dilemma with which the present article had started.

significant role. For reasons of deductivity, the modeling dilemma will be dissolved subsequently in its *general* form since this *overall* solution will, simultaneously, pave the way for the more specific four variants, too.

3. The Dissolution-Sketch for the Modeling Dilemma

Having arrived at a general framework for the identification of language games within the scientific system and their relevant rule systems, it should become feasible, finally, to produce a new type, and, it must be added, a more *successful* type of a *general* dissolution of the modeling dilemma. Moreover, the following remarks will shed new light on the principal heuristics for different ways of socio-economic world-making and for the separate roles of model-building in the social sciences where these different approaches can and must be qualified as *equally* necessary and indispensable.

The starting point consists in the introduction of a new dimension[14], which comes originally from modal logic and which has started in its modern form with C.I. Lewis already around 1920. Accordingly, a differentiation into two reference areas will be undertaken, namely into *possible world* domains and the realm of the *actual* world.[15] (See also HUGHES/CRESSWELL 1985:75ff. or, for social science modeling, GILBERT 1981:1ff.) Thus, a very simple two-dimensional morphological space assumes the following form:

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[14] It should be stressed especially at this point that morphological analyses can be undertaken in a variety of different ways. More concretely, a new dimension, substituting Bunge's, could be introduced in which the scientific output is classified into two broad categories, which have been introduced by Paul K. Lazarsfeld (1941) already in the early 1940's, namely into *administrative* and *critical* research. Consequently the resulting space of scientific language games covers, on the one hand, administrative research both of the empirical and normative type and, on the other hand, critical normative as well as critical empirical investigations.
An alternative differentiation could be made by substituting the critical/administrative distinction by a standard/non-standard separation which should account for the wide variety of scientific practices, ranging from Niklas Luhmann's theory of(f?) *social systems* (1984), from Lyotard's *Postmodern Knowledge* and the subsequent literature on *postmodernity* (CROOK/PAKULSKI/WATERS 1992, WATERS 1994), from Carol Giligan's *In a Different Voice* (1982) to Anthony Giddens' voyages into *structuration theory* (1984, 1990, 1991) or to Jim Coleman's attempted unifaction via a set of Rational Choice-theories on social actions and interactions (1990) ... Moreover, such a partitioning should make it clear that a large number of scientific language games and, more importantly, of regional or disciplinary scientific cultures exist in which the classic or standard goal-set, consisting of prediction, explanation and control (CASTI 1989:456ff.) is replaced by *non*-standard elements where criteria like the historical scope, the wide range of comparisons, the density of analogy-formation, the degree of surprise and novelty etc. occupy the center-stage.
Thus, the morphological analysis along the analytical road offers *potentially* useful groupings or clusters of scientific practices or language games. Moreover, the morphological approach can be undertaken in a self-referential manner, too. Defining a new dimension, in which the morphological partitionings are classified according to the degree of novelty, it remains a task for the reader to determine the most appropriate area for the present considerations ...

[15] It must be stressed from the beginning that the distinction between *actual world* and *possible worlds* is *not* to be confused with a *realistic* committment. On the contrary, *actual world* and *possible worlds* are differentiated in a purely *pragmatic* manner by focussing on differences with respect to *rule systems* and *evaluation criteria* for both types of operations. To be more precise, the rule systems for *actual* world practices focus on observation rules, measurement-rules, testing-rules and the like which are largely *absent* in the case of *possible* worlds-practices.

Table 3: A Modal-based Morphological Space for Four Types of Language Games in the Social Sciences

		Dimension$_2$	
		empirical	*normative*
Dimension$_1$	*possible world*	Field I	Field II
	actual world	Field III	Field IV

Even at this point, no cognitive *cash value* (Wilfried Sellars) will be recognizable since the partitioning into Field I- and Field III-games must, in all probability, lead to a complete marginalization of the model-building efforts within the social sciences, too. By characterizing the bulk of modeling activities as a scientific language game within *possible* worlds, the modeling activities lie, apparently, within the same camp as any type of *bogus* science which, after all, can be qualified as science-*fiction*, too. But at this stage, an explicit reference to the underlying basic pragmatic concepts, namely that of rules, rule systems and evaluation criteria, becomes essential. In the subsequent paragraphs, it will be demonstrated in an *a priori* fashion that, on the one hand, rule systems differ radically for research within Field I and within Field III and that, moreover, specific sets of *non*-trivial evaluation criteria can be found which differentiate clearly between interesting, fruitful research in each of the two respective fields from their unattractive, trivial or *bogus* counterparts.

3.1. Main Differences in the Rule Systems

Consequently, it will and must become the task of the subsequent part to highlight some of the main differences with respect to the rule systems for Field I-activities on the one hand and for Field III-operations on the other hand. The easiest way to identify major differences consists in the elaboration of two paradigmatic modeling examples, one from the side of actual world-modeling, the other from the possible worlds-area. In this manner of *exemplar based learning* (CHARNIAK/McDERMOTT 1985), two social models will be presented on the domain of education, one long term model of the Austrian education system focussing on the distribution of pupils across various school-types[16], the

[16] For the first model and its subsequent revisions, see especially MÜLLER/LASSNIGG 1992, HAAG/MÜLLER 1992, MÜLLER/HAAG 1994, MÜLLER 1995b.

other one on the French education system and the transition process from the end of High School either to the universities or to *the Instituts universitaires de Technologie* (IUT).[17]

Using a master-equation framework (HAKEN 1981, 1983, WEIDLICH/HAAG 1988), the distribution of pupils across various school-types in Austria from 1970 to 1990 has been modeled by building up three factor sets determining group behavior in a large scale social system. The following explanatory schema has been used -

$$\text{Changes in school type}_i\text{: } f\{\text{attractivities, barriers, global mobility}\}$$

It would require too much space within this article to present a formally accurate account of the overall modeling framweork[18], so it must be sufficient, at this stage, to characterize it in a qualitative manner. The set of barriers, restricting the *movement* of pupils across or between school-types, consisted of -

{Legal restrictions[19], delays in the schooling career,[20]
gender separation in school types[21], dualism[22]}

whereas the attractivity set has been composed of the following elements -

{Synergy parameter (agglomeration)[23], expected duration time[24],
capacities[25], sectoral distribution of the employment system[26]}

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17 For the French model, see esp. BOUDON 1979:122ff.

18 For more details, see the references quoted in Footnote 17.

19 This factor has been specified in form of a matrix of legally allowd transition between school types, with value 1 for legally admitted transition and 0 otherwise.

20 A second matrix on so called-*Schullaufbahnverluste* has been constructed in the following manner: Given the reference years for different school forms like five years for Upper Vocational Schools and the like, the time delay associated with each of these transitions has been calculated. Consequently, the probability for a change of the school type $j \rightarrow i$ is assumed to depend also on those expected delays.

21 According to this factor, transitions are assumed to slow down significantly in the case of highly segregated schools, the one being predominantly male, the other mainly female.

22 *Dualism* refers to the split between the university linked school forms and, with the exception of the Primary School, the remaining segments and has been formalized as a 1/0 matrix, too.

23 Here, a logistically shaped diffusion process is postulated, assuming that in the case of education systems too, agglomeration effects play an essential role.

24 To put it very briefly, those schools which offer, on the average, a long potential time span within the education system are considered to be more attractive than ortther forms, where, again on the average, a comparatively short time span within the education system is to be expected.

25 This factor is related to the supply side in the eduication system and assumes that capacity problems and bottlennecks within a certain school type contribute significantly to the attractivity of the school type.

26 The operationalisation of this factor used the distribution of past and present shares of the primary, the secondary and the tertiary sector as an appropriate explanatory variable.

Finally, the outcome of the model-estimation consisted in a reference-scenario for the Austrian system of education in which, by introducing assumptions like *average* migration flows and the like, the distribution of pupils exhibited the development pattern of Table 4:

Table 4: Changes in the Stock of Pupils in the Austrian School-system 1990 - 2005[27]

REFERENCE PATTERN

SCHOOL COMPOSITION IN THE YEAR 1990 (IN %)				SCHOOL COMPOSITION IN THE YEAR 2005 (IN %)			
PS:	33.31	IV_1:	2.03	PS:	32.91 (-0.4)	IV_1:	1.65 (-0.38)
SP:	.72	IV_2:	.91	SP:	.45 (-0.27)	IV_2:	.44 (-0.47)
GS:	21.94	IV_3:	.92	GS:	16.93 (-5.01)	IV_3:	.76 (-0.16)
AS_1:	8.61	IV_4:	.40	AS_1:	13.61 *(+5.00)*	IV_4:	.25 (-0.15)
SS:	.90	UV_1:	4.29	SS:	.75 (-0.15)	UV_1:	6.81 *(+2.52)*
DV_1:	3.34	UV_2:	.29	DV_1:	2.05 (-1.29)	UV_2:	.17 (-0.12)
DV_2:	.29	UV_3:	3.26	DV_2:	.12 (-0.17)	UV_3:	7.98 *(+4.72)*
DV_3:	7.85	UV_4:	.84	DV_3:	5.84 (-2.01)	UV_4:	1.64 *(+0.80)*
DV_4:	2.22	AS_2:	5.76	DV_4:	1.38 (-0.84)	AS_2:	4.49 (-1.27)
PTS:	2.11			PTS:	1.76 (-0.35)		

SIMULATIONS

ENTRANCE-VARIATION (SMALL INCREASES IN MIGRATIONS) (%-CHANGES TO REFERENCE SCENARIO)				ENTRANCE-VARIATION (SMALL INCREASES IN BIRTH RATES) (%-CHANGES TO REFERENCE SCENARIO)			
PS:	+1.75	IV_1:	+2.81	PS:	+7.16	IV_1:	+2.37
SP:	+1.81	IV_2:	+3.11	SP:	+0.00	IV_2:	+2.36
GS:	+4.76	IV_3:	+2.37	GS:	+3.49	IV_3:	+2.94
AS_1:	+0.00	IV_4:	+2.98	AS_1:	+4.85	IV_4:	+2.95
SS:	+3.57	UV_1:	+0.58	SS:	+0.67	UV_1:	+1.53
DV_1:	+1.97	UV_2:	+1.44	DV_1:	+1.78	UV_2:	+1.75
DV_2:	+2.63	UV_3:	+1.04	DV_2:	+2.18	UV_3:	+0.87
DV_3:	+2.22	UV_4:	+0.46	DV_3:	+1.21	UV_4:	+2.72
DV_4:	+1.91	AS_2:	+0.25	DV_4:	+1.82	AS_2:	+2.73
PTS:	+4.46			PTS:	+3.23		

[27] The abbreviations stand for: PS (Primary School), SP (Special Primary School), GS (General Secondary School), AS_1 (Academic Secondary School I, SS (Special Secondary School), DV_{1-4} (Dual Vocational School), PTS (Polytechnical School), IV_{1-4} (Intermediate Vocational School), UV_{1-4}(Upper Vocational School), AS_2(Academic Secondary School II). Furthermore. the index number 1 designates *multiple* school forms whereas the indices 2 to 4 stand for *singular* schools (2: agriculturally related, 3: industrially linked, 4: service-oriented)

The Boudon-model starts, first, with actual data from France (BOUDON 1979:112) on the distribution of students in various university segements and the IUT which would indicate a Field-I approach, too. The decisive step towards a genuine Field III-enterprise is made, then, with the introduction of relevant modeling-assumptions and with the subsequent elaboration of an explanatory$_P$ framework. First, assumptions are postulated with respect to the potential benefits of universities for a cohort of twenty *identical* students in a typical possible worlds-manner: 6 students are assumed to earn a benefit of 2 money units, 8 students a benefit of one money unit and six will reap no benefits at all, whereas the benefits for the IUT are postulated to be one money unit for the entire group of 20. Due to this distribution of potential benefits, the explanatory scheme highlights a significant aggregation problem, since the decision-configuration of a single person out of the cohort of 20 takes the following form:

Table 5: Utility Matrix for a Single Student in the Cohort of 20

Strategies for the student$_i$	Number of students who, aside from student$_i$, opt for the university						
	0	1	2 ... 7	... 10	.... 15 .	... 20	
IUT	1	1	1	1	... 1	... 1	... 1
University	2	2	2	1.86	... 1,55	... 1,25	... 1[28]

It is interesting to note that Boudon makes the limitations of his explanatory scheme[29] as well as the Field-III interprtation of his results[30] *abundantly* clear. He concedes that the

[28] The values in the utility matrix are to be understood as expected utilities. Thus, the value 1,86 is the result of a configuration in which a student has the chance of 6/7 toearn 2 money units and 1/7 to obtain only one money unit. Consequently, (6/7)x2 + (1/7)x1 = 1,86 money units. The other values in the utility matrix are computed in the same manner. (See also BOUDON 1979:134)

[29] Boudon, or to be more precise: Boudon's translator, wrote the following remarks *highly* characteristic for a possible worlds-approach:

> *Dieses Modell stellt selbstverständlich eine Idealisierung dar: Es trifft selbstevrständlich nicht zu, daß jeder Student sich für genau so gut wie alle anderen Studenten hält; es trifft nicht zu, daß die Studenten vollständig über die Spielregeln informiert sind. (BOUDON 1979:135)*

[30] With respect to the interpretation of the results, Boudon remains *consistently* within the confines of a possible worlds-language game:

> *Möglicherweise ist die Ursache für das Scheitern der Kurzstudienlehrgänge eher in Paradoxien der Aggregation individueller Entscheidungen zu suchen als in kulturalistischen Erklärungen (Entwertung technischer Bildung usw.). (BOUDON 1979:135)*

modeling framework is a typical possible world-approach only and, more importantly, he confines the results to the mere *possibility* that problems of aggregation rather than socio-cultural factors lie at the heart of the low acceptance of IUT in France. The decisive point at this stage lies in the fact that Boudon's modeling$_P$ account should be and must be considered as *successful* and *important* simply because it fulfills criteria like *the reduction of complexity*, i.e. the reduction of a highly complex socio-economic transition process to one control-variable only or the *counter-intuitive* nature of the results since traditional theories of school transitions (ARROW 1973, BOWMAN 1981, PSACHAROPOULOS 1987) have focussed on many different areas *except* for an *aggregation* problem of individual choices.

With these two examples it should become easier to follow the distinctions introduced via Table 6 where the main *differences* in the scientific practices within Field I-modeling work and Field III-activities are summarized.

Table 6: Rule Systems for Two Types of Modeling Practices

FIELD I: MODELING OPERATIONS FOR ACTUAL WORLD	FIELD III: MODELING OPERATIONS FOR POSSIBLE WORLDS
$Set_{1,A}$: *Strong* Data Requirements with respect to - - Observability - Testability - Measurements - Tests	$Set_{1,P}$: No Data Requirements[31] *Freedom to choose*
$Set_{2,A}$: *Strong* Requirements for *Non*-Theoretic Modeling Components$_A$ with respect to - - Observability - Testability	$Set_{2,P}$: No Requirements *for Non*-Theoretic Modeling Components$_P$ - *Freedom to choose*

It should not be considered as a mere coincidence however, that Boudon and so many other writers have followed *consistently* along the basic distinctions which have been introduced in Table 2 and along the different rule systems and criteria which are presented in Table 6.

[31] As a point of illustration, the data work necessary for the Field I-education model was in the range of twelve months and required an intensive cooperation between statistical offices and the IAS-team. For the Boudon-model however, none of the data-collection and data-adaptation procedures was required. Here, an inspired afternoon is sufficient to find an approporiate distribution of utility values and the like.

- Measurements
- Tests

$Set_{3,A}$:	Requirements for Theoretical Modeling Components$_A$	$Set_{3,P}$:	Requirements for Theoretical Modeling Components$_P$
	Consistent Links to the Body of Non-Theoretical Elements$_A$ and Data$_A$		Consistent Links to the Body of Non-Theoretical Elements$_P$ and Data$_P$

Phrased in a *very* analogical manner, the rule systems for Field I-work and for Field III-studies differ like the *standard* recipes for conducting empirical social research (BORTZ 1984) from Paul K. Feyerabend's *Anything goes*-rule (1978, 1985).

3.2. Main Differences in the Evaluation Criteria

But the rule systems for Field I-research and Field III-practices do not constitute the sole *principium divisionis*, the evaluation criteria[32] for these two types of scientific language games differ radically

[32] *Evaluation criteria* as a pragmatic concept refers to the set of all those attributes by which the *actual* moves and practices within the context of a language game and its rule system can be evaluated. Thus, turning to Wittgenstein's famous characterization of *games* and *family resemblances* -

> *Betrachte z.B. einmal die Vorgänge, die wir 'Spiele' nennen. Ich meine Brettspiele, Kartenspiele, Ballspiel, Kampfspiele, usw. Was ist allen diesen gemeinsam? - Sag nicht: 'Es muß ihnen etwas gemeinsam sein, sonst hießen sie nicht 'Spiele'' - sondern schau, ob ihnen allen etwas gemeinsam ist. - Denn, wenn du sie anschaust, wirst du zwar nicht sehen, was allen gemeinsam wäre, aber du wirst Ähnlichkeiten, Verwandtschaften, sehen, und zwar eine ganze Reihe. Wie gesagt: denk nicht, sondern schau! - Schau z.B. die Brettspiele an, mit ihren mannigfachen Verwandtschaften. Nun geh zu den Kartenspielen über: hier findest du viele Entsprechungen mit jener ersten Klasse, aber viele gemeinsamen Züge verschwinden, andere treten auf. Wenn wir nun zu den Ballspielen übergehen, so bleibt manches Gemeinsame erhalten, aber vieles geht verloren. - Sind sie alle 'unterhaltend'? Vergleiche Schach mit dem Mühlfahren. Oder gibt es überall ein gewinnen und Verlieren, oder eine Konkurrenz der Spielenden? Denk an die Patiencen ... Und das Ergebnis dieser Betrachtungen lautet nun: Wir sehen ein kompliziertes Netz von Ähnlichkeiten, die einander übergreifen und kreuzen. Ähnlichkeiten im Großen und Kleinen. Ich kann diese Ähnlichkeiten nicht besser charakterisieren als durch das Wort 'Familienähnlichkeiten'; denn so übergreifen und kreuzen sich die verschiedenen Ähnlichkeiten, die zwischen den Gliedern einer Familie bestehen. (WITTGENSTEIN 1971:PU 66f.)*

one may, in similar fashion, introduce the term of evaluation criteria in the following manner:

too. Introducing, in the spirit of historical reachability, the famous Hempel-Oppenheim articles on explanation (HEMPEL 1942, HEMPEL/OPPENHEIM 1948), the main evaluation criterion for Field I-investigations can be stated in a straightforward manner for it lies in the successful proliferation of explanations, predictions and retrodictions where „successful" should refer to the *simultaneous* fulfillment of the four explanatory requirements put forward by Hempel and Oppenheim, especially the *truth condition*.[33] Field III-evaluations, however, focus on explanations, too, but here a significant elimination must take place since the truth condition$_A$ has to be abandoned. Moreover, an additional set of conditions has to be introduced in which criteria like surprise, simplicity, high degree of formalization, empirical reachability („Anschlußfähigkeit", to borrow a term from Niklas Luhmann) and the like occupy a *predominant* role.

Table 7: Evaluation Criteria for Two Distinct Scientific Modeling Practices

FIELD I: EVALUATION OF MODEL SOLUTIONS FOR THE *ACTUAL WORLD*	FIELD III: EVALUATION OF MODEL SOLUTIONS FOR *POSSIBLE WORLDS*
$Set_{I,A}$: EXPLANATION$_A$ (Prediction, Retrodiction) Main Requirements: - Explanans$_A$ - Explanandum$_A$ - Consistency - Truth$_A$	$Set_{I,P}$: EXPLANATION$_P$ (Forward and Retrosimulation$_P$) Main Requirements - Explanans$_P$ - Explanandum$_P$ - Consistency

Betrachte z.B. einmal die Regeln, die wir 'Bewertungen von Spielen' nennen. Ich meine Bewertungen von Brettspielen, Kartenspielen, Ballspielen, Kampfspielen, usw. Was ist allen diesen gemeinsam? - Sag nicht: 'Es muß ihnen etwas gemeinsam sein, sonst hießen sie nicht 'Bewertungen von Spielen'' - sondern schau, ob ihnen allen etwas gemeinsam ist. - Denn, wenn du sie anschaust, wirst du zwar nicht sehen, was allen gemeinsam wäre, aber du wirst Ähnlichkeiten, Verwandtschaften, sehen, und zwar eine ganze Reihe. Wie gesagt: denk nicht, sondern schau! - Schau z.B. die Bewertungsregeln für Brettspiele an, mit ihren mannigfachen Verwandtschaften. Nun geh zu den Kartenspielen über: hier findest du viele Entsprechungen mit jener ersten Klasse, aber viele gemeinsamen Züge verschwinden, andere treten auf. Wenn wir nun zu den Ballspielen übergehen, so bleibt manches Gemeinsame erhalten, aber vieles geht verloren. - Werden sie alle nach der Spielerqualität bewertet? Vergleiche reine Glückspiele mit Schach. Oder gibt es überall ein Gewinnen und Verlieren? Denk an das Spiel, sich einen Ball zuzuwerfen. Oder eine Konkurrenz der Spielenden? Denk an die Patiencen ... Und das Ergebnis dieser Betrachtungen lautet nun: Wir sehen ein kompliziertes Netz von Ähnlichkeiten, die einander übergreifen und kreuzen. Ähnlichkeiten im Großen und Kleinen. Ich kann diese Ähnlichkeiten nicht besser charakterisieren als durch das Wort 'Familienähnlichkeiten'; denn so übergreifen und kreuzen sich die verschiedenen Ähnlichkeiten, die zwischen den Gliedern einer Familie bestehen. (WITTGENSTEIN 1971:PU 66f.)

[33] It goes (almost) without saying that, due to the overall pragmatic context, the truth condition has to be interpreted in a pragmatic manner too, i.e. with reference to *rule-systems*. Consequently, truth means the satisfaction or the following of *specific* rule systems.

Set_{2A}:	ADDITIONAL VALUE (Improved Understandig of the Socio-economic Universes)	Set_{2P}:	ADDITIONAL VALUE (Improved Understanding of the Socio-economic Universes)
	$Simulations_A$[34]		$Simulations_P$[35]
	$Innovation_A$		$Innovation_P$
	$Policy\ Relevance_A$[36]		$Policy\ Relevance_P$[37]
	$Generalizability_A$[38]		$Generalizability_P$[39]
	$Surprising_A$ Results		$Counter\text{-}intuitive_P$ Results (Paradoxes, Critical Thresholds, etc.)
	Mastering of Complexity		Reduction of Complexity
	Above Average Degree of Formalization[41]		*High* Degree of Formalization[40]
			Testability in Principle („Anschluß-fähigkeit")
	$Congruency_A$[42]		$Congruency_P$[43]

Thus, model accounts, ranking high on the criteria $sets_P$, will be qualified as *successes*, *innovative*, or *imaginative irrespective* of their extremely low correspondence to *actual* data, observations or measurements. A rational choice account on the *optimal* course of a psychic depression or on an *optimal* allocation of manic-depressive phases during one's life cycle will and must become, if successful along the evaluation set_P, a potentially *interesting* focus of discussion. Once again, a

---

34 It should be emphasized that the variations, introduced in a Field I-simulation, must follow the normal Field I-criteria.

35 Likewise, $simulations_P$ are typically un-restricted and can be put forward with the fullest freedom of choice.

36 Within Field I, a *direct* connection can be established between $model_A$-work, $simulations_A$ and the corresponding $policy\ advices_A$.

37 For Field III, policy advice can be given too, which must be couched, however, in an indirect manner, i.e. as possible worlds-advice on potential problem areas, etc. For a typical advice of this type, see the discussion of robert Axelrod's book in the following chapter.

38 $Generalizability_A$ means the extension from one area of $application_A$ to other $empirical_A$ domains.

39 Contrary to the Field I-meaning, $generalizability_P$ consists mainly in the removal of specific restrictions by their non-restricted counterparts (e.g., from 2 person games to n person games, from perfect information to imperfect information, etc.)

40 For Field III, the linking of modeling work with recent *advances* in mathematics or statistics must be seen as an essential and *very* impoprtant evaluation criterion.

41 Within Field I however, it is regarded, normally, as a special success if *medium* types of formalization can be utilised for $empirical_A$ data.

42 $Congruency_A$ is a typical residual category, comprising *additional* evaluation standards for Field I. In order to present an aoperational definition, $congruency_A$ can be equated with those elements which one may find in David F. Hendry (1987), but *not* in Table 7.

43 Similarly, $congruency_P$ can be considered as the set of those evaluation criteria for Field III-investigations which are explicitly mentioned in Hendry (1987), but not in Table 7.

counter-intuitive example might be helpful to demonstrate the universal applicability of the *rational* mode of attribution for *evidently* inappropriate configurations. The example itself is devoted to the topic of *altruism* and is couched, moreover, in a *typical* Field III-manner:

> *First*, a possible world situation is built up, by postulating two persons, father and son, by focussing on intergenerational transfers and, moreover, by assuming a single consumption commodity, -
>
> > Let C denote the sole consumption good, corn, the total amount of which we fix arbitrarily. Suppose all this corn is initially under the father's control. The level of corn consumed by an individual affects his pleasure. We refer to this direct pleasure as 'felicity' and describe it by functions .... (STARK 1995:15f.)
>
> *Second*, altruism is introduced via a simple scalar β_i -
>
> > the weight that one places on the utility of the other relative to one's own felicity. (IBID:16)
>
> *Third*, the optimal consumption level both for the consumption of the father as well as that of the son is calculated, yielding a solution where -
>
> > the father's optimal allocation is such that he wishes to consume a larger proportion of corn than his son wishes him to consume (IBID:18)
>
> *Finally*, several implications are drawn from this analytic solution, by pointing out to the beneficiary role of altruism for producing mutually agreeable transfers, by showing that altruism, while reducing conflict, does not eliminate it, or by demonstrating that a rise in altruism *may* result in a worse outcome for both parties involved. (IBID:19ff.)

More generally, *any* phenomenon of the social worlds, from marriages and family life (BECKER 1981) to seemingly remote areas like health and sickness, feelings, including, *pace* ELSTER (1992), those of shame, can *legitimately* become an object of a possible worlds-investigation within the context of the *rational* stance. Likewise, everyday calculations, management behaviour or the activities of scientists can be subject to a *normative stance* (ELSTER 1990), an *emotional* stance (ELSTER 1992), or an *ethnographic stance.* (LATOUR 1987, 1991, LAVE 1989)[44]

On the other hand, complex Field I-models focussing, for example, on *group* behavior and a *habitual* stance, can be extended to areas *outside* the existing set of paradigmatic applications$_A$: from migration (WEIDLICH/HAAG 1988), sectoral employment (MÜLLER/HAAG 1994) into more remote areas like politics (ERDMANN 1986, HOFINGER/GRÜTZMANN 1994), innovation and diffusion dynamics (WEIDLICH/HAAG 1983, MENSCH/WEIDLICH/HAAG 1991, ZHANG 1991), cognitive dynamics (MÜLLER 1992, 1993). Moreover, successful Field I-models can and must be, if possible,

[44] On concepts like *intentional stance*, *rational stance*, *emotional stance*, etc. see especially chapter 5 of the present article.

combined to comparatively larger scale models, leading thus to a steady increase for predictions$_A$ and simulations$_A$.

4. Epistemic Cultures in the Social Sciences

Having reached, by now, a *successful* dissolution of the modeling dilemma by focussing on two highly differentiated sets of rules and evaluations, the next step will carry the preceding result one step further by generalizing it even further. For this purpose, the introduction of a new concept will become necessary, namely that of an *epistemic culture*, which has been proposed first by Karin Knorr-Cetina in order to describe *clusters* of essential scientific activities and practices.(KNORR 1992b)[45] An epistemic culture, then, consists of a set of basic scientific research operations which must be considered as highly typical with respect to the fabrication of a particular knowledge domain and, more general, to the orchestration and organisation of particular areas of investigation.

> The notion (of *epistemic culture*, K.H.M.) foregrounds not only the difference between the notion of a laboratory and the concept of experiment traditionally defined, it also foregrounds the disunity of the sciences in regard to the meaning of the empirical, the enactment of object relations, the construction and fashioning of the social within science.(KNORR-CETINA 1992b:3)

More specifically, epistemic cultures typify special and unique relations to their cognitive as well as to their outside environment. In this sense, Knorr-Cetina distinguishes, within the context of the *natural* sciences, two dominant epistemic cultures, one located in the area of high-energy physics and described in terms of a closed and self-contained type of knowledge production, the other situated in areas like molecular biology, where open processes of *trial and error* dominate the research scenes.

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[45] It must be noted that the introduction of the recent concept of epistemic culture does not violate the principle of historical reachability since, once again, Otto Neurath had already in the 1930's and early 1940's proposed a *Gelehrtenbehavioristik* where one could find the following remarks -

> *Sociologists deal, among other things, with tools and tales, with the language of magic, theology, jurisprudence, economics, and pedagogics; but also with the language used by the sociologists themselves, with their statements and their habits, i.e. with the behavioristics of sociologists ... We have fine questionnaires as far as preliterate tribes are concerned but hardly any when we try to ask sociologists how they themselves behave in arguing and writing. Since sociological predictions are directly connected with actions which create what has been predicted, the difficulty is very understandable. More than in other sciences, taboos and old folklore come into the picture when human actions are under consideration. (NEURATH 1971:42f.)*

Once again, the principal ways to arrive at a meaningful and empirically grounded notion of epistemic cultures can proceed along two different lines:

> The first route, undertaken for example by Knorr-Cetina, makes use of an intensive, methodologically sophisticated inspection[46] of the *actual* practices of scientific day- to day activities in the context of laboratories -
>
> > The laboratory allowed ... to consider the technical activities of science within the wider context of equipment and symbolic practices within which they are embedded ... In other words, the study of laboratories has brought to the fore the full spectrum of activities involved in the production of knowledge. (IBID.)
>
> Via a rich theoretical background, Knorr-Cetina achieves a successful *ordering* and *clustering* of the mass of empirical protocols, and arrives, in the end, at an *empirically well*-founded separation between main types or clusters of scientific research activities.
> The second way however, does proceed in a *morphological* and *a priori* manner, separating between principal components of scientific operations and arriving, then, at a variety of *possible* configurations and recombinations. These *possible* types of scientific operations possess the status of *potentially* fruitful and enlightening conjectures which need further empirical collaborations to determine their *actual* distribution or their historical development path.

Since the subsequent remarks will follow along a possible worlds-strategy, the evaluation criteria for the usefulness of the identification of epistemic cultures in the social sciences are clear. The distinctions must clearly exhibit a heuristic *surplus-value*, *i.e.* they must rank high with respect to evaluation criteria like *reduction of complexity*, *counter-intuitive insights*, *innovative content*, *„Anschlußfähigkeit"* and the like ...
Again, like in the preceding chapters on two types of modeling, an *exemplar*-based approach will be chosen in which two sets of products, highly typical for two different *epistemic* social science cultures, will be introduced and discussed.

> The first example comes from a classic on classics, namely from John Madge's book on the origins of scientific sociology (MADGE 1962). In this volume, Madge gives a detailed account of path-breaking studies from Emile Durkheim's analysis of suicide and anomy (DURKHEIM 1983) to Leon Festinger's and Harold H. Kelley's investigations on attitude changes through social contacts (FESTINGER/KELLEY 1951). Between these two poles, one finds, within Madge's volume, a large number of sociological projects,

[46] In Knorr's case, ethnomethodology, phenomenology of the Merleau-Ponty style as well as a strong reference to Michel Foucault's work form the theoretical background for the interpretative work on *science observed*.

ranging from William I. Thomas'and F. Znaniecki's books on *The Polish Peasant in Europe and America* (THOMAS/ZNANIECKI 1918-1920), from the Chicago School and, more specifically, from H.W. Zorbough's *The Gold Coast and the Slum* (1929) to the studies on the rise and the roots of fascism by the Frankfurt Institute (ACKERMANN *et al.* 1976) and to Robert F. Bales' account on *Interaction Process Analysis* (BALES 1950) In a final chapter on *The Lessons,* Madge turns to the question of similarities and characteristic traits for major works in sociology and arrives, after reviewing the methods and techniques used in a comparative manner, at the following conclusion.

> Against this background the characteristic novelty of the works introduced in this book becomes apparent. Each item of research is unremittingly empirical and, like the products of the social-survey movement, almost all the studies are immidiately concerned with the alleviation of current social problems. At the same time, almost without exception, each study makes a concurrent contribution to verifiable knowledge. (MADGE 1962:537)

The social sciences, by employing documents, interviews and observations from and within their socio-economic environments, have apparently developed, over the last hundred years, a distinctive epistemic culture which is, following Madge's observations, both *empirically*$_A$ and *policy*$_A$ oriented.[47]

The second example has more recent origins and is the widely cited and acclaimed book by Robert Axelrod on the evolution of cooperation (AXELROD 1984). The research operations necessary for this type of study differ *very* significantly from the first series of classical sociological investigations. First, the *direct* connex with social problems and their reduction is not given any more although Axelrod discusses at length an *actual world*-example, namely *the live-and let-live system in trench warfare in World War I* (AXELROD 1984:73ff.). Second, the data base is not established via questionnaires, observations or interviews from the manifold of life-worlds but within the social laboratory itself.[48] More precisely, a typical possible worlds-configuration is set up for iterated games of the PD (prisoner's dilemma) which offers, for each player, two strategies, namely cooperation (C) and defection (D) and which exhibits a payoff-

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[47] On the importance of the policy-side and on the close linkages between state apparatus and social sciences, see esp. WAGNER 1990.

[48] One might object immediately that Axelrod's computer tournament required a substantial *postal* input from outside, namely all the strategy suggestions for the Prisoner's Dilemma. But this specific detail is irrelevant to the present questions of data sources since a morphological analysis within the laboratory itself would have yielded a similar combination of strategies. One might argue, however, that Axelrod's computer tournament has to say very much on the sociology of science as well because it reveals the preferences of scientists engaged in game theory, evolutionary biology and the like ...

distribution of 5 (T for temptation to defect), 3 (R for reward for cooperation), 1 (P for punishment for mutual defection) and 0 (S for sucker's payoff). Third, the prevalent mode of investigation lies in a *computer* simulation, i.e. in simulations which take place *within* the context of the laboratory itself. In the case of Axelrod, the design of a computer tournament has been chosen in which basically all strategies admitted were allowed to play against each other. Fourth, and *very* importantly, the *results* of the tournament, viz. a rank-ordering of strategies, were subject to a second order analysis with respect to common attributes which could be identified for successful tournament strategies, a validation move which might be qualified as *typical* for possible worlds-modeling. Fifth, a set of rules has been formulated which one should follow in socio-economic configurations of the PD-type with pay-offs sufficiently *similar* to a 5,3,1,0-distribution -

> Don't be envious (110ff.), Don't be the first to defect (113ff.), Reciprocate both cooperation and defection (118ff.), Don't be too clever (120ff.)

Sixth, and finally, the fourth part of Axelrod's book is devoted, under the heading of *How to Promote Cooperation* (AXELROD 1984:124) - to *reformers* and offers advice *consistent* with the design and the result of his study. Thus, Axelrod has definitely an extremely important *political* advice to offer -

> Enlarge the shadow of the future (126ff.), Change the payoffs (133f.), Teach people to care about each other (134ff.), Teach reciprocity (136ff.), Improve recognition abilities (139ff.) -,
>
> although the results and the scientific basis for these devices have been confined, throughout the study, to the laboratory alone

From these two highly significant examples it becomes relatively easy to find an inductive generalisation to the notion of epistemic cultures. In social science areas like sociology or in political science[49], the traditional and *predominant* mode of knowledge production has been clustered within an epistemic culture which can be described by attributes like a focus on the *actual world* (empirical$_A$) and openness (data generation for processes *outside* the social science laboratory), whereas the Axelrod book is one of the most prominent examples of an entirely *different* epistemic culture with

[49] It must be pointed out that economics, since the introduction of the utility synthesis during the 1870's, may be considered as the leading discipline *away* from the confines and principles of the traditional epistemic culture. Moreover, it would be an *extremely* enlightening research objective to frame the debates, starting around the so-called *Methodenstreit* until to the present time, within the conceptual apparatus of *dominant* epistemic cultures, *assimilation* attempts to it and a *new* epistemic culture in the making ...

opposite attributes: with a focus on *possible worlds* (empirical$_P$), and closure (data generation *within* the social science laboratory itself). Via a two case-inductivism, one is led, therefore, to the following table which, once again, highlights significant differences between two epistemic cultures within the social sciences.[50]

Table 8: Two Epistemic Cultures in the Social Sciences I - Principal Components

		Dimension$_2$	
		open	*closed*
Dimension$_1$	*actual world*	Epistemic Culture I	[Intermediaries I]
	possible worlds	[Intermediaries II]	Epistemic Culture II

Seen from a history of science-perspective, it would become an extremely challenging, albeit rewarding reserach task to describe the evolution of theoretical economics or econometrics as a consecutive path from the upper left area, via the intermediary station II, to the lower right side of Table 8. Moreover, from the preceding table, a conjecture can be put forward that only over the last decades, starting with the diffusion of new information processing technologies, a separation into at least two major epistemic cultures has been established which can be summarized via Table 9:

Table 9: Two Epistemic Cultures in the Social Sciences II - Main Characteristics

EPISTEMIC CULTURE I:	Open Rule Systems for the Actual World	EPISTEMIC CULTURE II:	Closed Rule Systems for Possible Worlds
Main Focus with Respect to			
DATA	Observable , Non-Experimental Processes Data with Strong Quality restrictions		Laboratory Data, Experimental Data, Artificial Data

[50] It should be added, once again, that the distinctions between the two epistemic cultures just introduced should be considered by no means as exhaustive. On the contrary, especially within the social sciences, it should become very useful to distinguish between a variety of epistemic cultures, some of them being confined to special territories, some of them to a *specific* cluster of disciplines, etc. With respect to *modeling* operations however, the separation between just *two* epistemic cultures should be considered both as necessary *and* sufficient.

	(Reliability, Validity)	
THEORY AND MODELING	Grounded Theory; Middle-range Theories Grand Foundations[51]	Highly Formalized Theories
GENRES[52]	*Thick* Descriptions[53]: Explanation sketches$_A$, ... Model-Explanations$_A$ Model-Simulations$_A$	*Thin* Formalizations: Model-Explanations$_P$, Model-Simulations$_P$, ... Explanation skecthes$_P$

Thus, the hypothetical identification of *at least* two *epistemic* cultures *within* the social sciences must be considered as the most *general* dissolution of the modeling dilemma. Due to this *overall* separation, one is invited, therefore, to distinguish clearly between *two* types of modeling and simulation within the contemporary social sciences, one labelled modeling$_A$ and adhering to the traditional, *open*-oriented

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[51] Especially within sociology, a remarkable feature lies in an *excessive* pre-occupation of reconfiguring the *entire* discipline *anew*. From Talcott Parsons *Theory of Social Action* (1961) onward, one finds, over the last decades, a wide range of *very* comprehensive *foundation* attempts, culminating in voluminous works by HABERMAS (1981), LUHMANN (1984), MÜNCH (1988, 1993), etc.

[52] Concluding, in an analogical spirit, the horizontal tour on games, rules, evaluation criteria and genres, one could adapt the Wittgensteinian original by the following variant -

> *Betrachte z.B. einmal die Gegenstände, die wir 'Spielausstattungen' nennen. Ich meine die Ausstattungen von Brettspielen, Kartenspielen, Ballspielen, Kampfspielen, usw. Was ist allen diesen gemeinsam? - Sag nicht: 'Es muß ihnen etwas gemeinsam sein, sonst hießen sie nicht 'Spielausstattungen' - sondern schau, ob ihnen allen etwas gemeinsam ist. - Denn, wenn du sie anschaust, wirst du zwar nicht sehen, was allen gemeinsam wäre, aber du wirst Ähnlichkeiten, Verwandtschaften, sehen, und zwar eine ganze Reihe. Wie gesagt: denk nicht, sondern schau! - Schau z.B. die Ausstattungen für Brettspiele an, mit ihren mannigfachen Verwandtschaften. Nun geh zu den Kartenspielen über: hier findest du manche Entsprechungen mit jener ersten Klasse, aber sehr viele Gemeinsamkeitene verschwinden, andere treten auf. Wenn wir nun zu den Ausstattungen für Ballspiele übergehen, so bleibt manches Gemeinsame erhalten, aber vieles geht verloren. - Brauchen sie alle ein Spielmaterial? Vergleiche Schach mit dem Stein-Schere-Papier-Knobeln von Kindern. Oder gibt es überall einen besonderen Ort? Und wie steht es mit der Kleidung? ... Das Ergebnis dieser Betrachtungen lautet nun: Wir sehen ein kompliziertes Netz von Ähnlichkeiten, die einander übergreifen und kreuzen. Ähnlichkeiten im Großen und Kleinen. Ich kann diese Ähnlichkeiten nicht besser charakterisieren als durch das Wort 'Familienähnlichkeiten'; denn so übergreifen und kreuzen sich die verschiedenen Ähnlichkeiten, die zwischen den Gliedern einer Familie bestehen. (WITTGENSTEIN 1971:PU 66f.)*

[53] The concept of a *thick description* refers to the phenomenon that within the traditional epistemic culture a *very* large number of books has been produced which cover hundreds and hundreds of pages and which, by and large, qualify as *descriptive* frameworks. (See e.g. the extremely stimulating discussion on the impossibility of transforming a thick and volumnious description, namely S:P. Huntington's book on modernization (HUNTINGTON 1968), into a thin and consistent model in the article by Krause in this volume)

epistemic culture$_A$, the other one characterized as *modeling*$_P$ and being the core element of a *new* type of epistemic culture$_P$, namely that of a *closed* laboratory setting$_P$. Table 10 summarizes the separation of two modeling approaches in which many essential ingredients like explanations, simulations, predictions, retrodiction, control, etc. should be used with appropriate subindices - A or P - in order to facilitate the identification of the scientific language games pursued.

Table 10: Two Modeling Approaches in the Social Sciences

		Dimension$_2$
	open	*closed*
actual world	Epistemic Culture$_A$ {Modeling$_A$}	
Dimension$_1$ *possible world*		Epistemic Culture$_P$ {Modeling$_P$}

A final argument can be put forward which should demonstrate, once again, the *heuristic* value of the basic differentiations introduced here. Within the social sciences, the next decades will experience a tremendous surge in modeling approaches, which, to varying degrees, belong to the set of *complex* models. (See, aside from the literature quoted in Footnote 6, also CAMPBELL/ECKE/HYMAN 1992, CRILLY/EARNSHAW/JONES 1991, KAYE 1993) This modeling revolution which has been well under way for the last decade already will make it almost imperative to separate clearly between model$_P$-approaches and their model$_A$-counterparts for, within the small survey on recent model advances in the sciences of complexity presented in Table 11 (below), one finds many model families which can be utilized within a modeling$_A$-environment *as well as* in a modeling$_P$ context. Moreover, the emergence of *virtual* laboratories -

> A virtual laboratory can be divided into two components: the application programs, data files and textual descriptions that describe the experiments; and the system support that provides the framework on which these domain-dependent experiments are built (PRUSINKIEWICZ/LINDENMAYER 1990:194) -

will exert a considerable impact on the rapid development of the epistemic culture$_P$ and, consequently, on the course of the co-evolution between the new epistemic culture and its long established counterpart.[54]

Table 11: Survey of Complex Models

TRADEMARKS	CORE-DOMAINS	HEURISTICS
AUTOPOIESIS	*Biology*, Artificial Intelligence, Theories of Action, et al.	Organisation/ Structure; Closedness, Autonomy; Recursiveness; Observer-dependence, etc.
CELLULAR AUTOMATA	*Engineering*, Physics, Biology, Demography, Sociology, et al.	Von Neumann-neighborhood, Moore-neighborhood, etc.
CHAOS-THEORY	*Dimension-theory*, Meteorology, et al.	*Strange Attractors*; Mandelbrot-Set; Julia-Set; Ljapunov Coefficients, etc.
CLASSIFIER SYSTEMS AND EVOLUTIONARY PROGRAMMING	*Engineering*, Psychology, Science of Science, Artificial Intelligence, Artificial Life et al.	Bucket-Brigade Algorithms, Cross-over, Schema-Theorem etc.
COMPLEXITY THEORY	*Computer Science*, Artificial Intelligence, Linguistics, Biolgy, et al.	Deterministic Turing-Machines, NP-problems etc.
DISSIPATIVE STRUCTURES	*Nonequilibrium Thermodynamics*, *Chemistry*, et al.	Disturbance-parameters; Brüsselator, etc.
EVOLUTIONARY GAMETHEORY	*Biology*, Neurophysiology, *Sociology*, et al.	Evolutionary Stable Strategies (ESS), etc.

[54] Again, a final remark becomes appropriate to point to the fact of a *multitude* of epistemic cultures within the contemporary social sciences, especially at the *regional* or the *gender* level. With respect to *modeling* activities however, the separation between *two* dominant cultures should be, so the argument, *both* necessary - *and* suffiecient.

GROUP-THEORY	*Mathematics*, Quantum Mechanics, Chemistry, Psychology et al.	Symmetry-operations, Symmetry-groups, etc.
HYPERCYCLES	*Biochemistry*, Chemistry, et al.	Families of Nonlinear Equation Systems, etc.
NEURAL NETWORKS	*Computer Science*, Brain Reserach, Artificial Intelligence, et al.	Backpropagation; Delta rule, etc.
POPULATION DYNAMICS	*Biology*, Ethology, Demography, Sociology et al.	Selection Models, Predator-Prey-Models, etc.
SYNERGETICS	*Laser-research*, *Physics*, Pattern Recognition et al.	Masterequation; Fokker-Planck-Equation; Slaving Principle; Control- and Orderparameters, etc.
THEORY OF CATASTROPHES	*Differential-topolgy*, Biology, Sociology, et al.	Families of Generic Equations; Typology of Bifurcations, etc.

With these distinctions it should be easier to accept that modeling in the social sciences adheres to *different* epistemic cultures and that, moreover, modeling can be performed in two highly differentiated manners: either as a Field I-practice which sticks to the main principles of the traditional epistemic culture or as a Field III-operation which follows a *different* set of rules and, even more importantly, of evaluation criteria.[55] With the present article it is hoped for that the basic distinctions between

[55] It would be an extremely interesting task to apply the notion of *epistemic regimes* (Björn Wittrock) to the cognitive as well as to the socio-economic transformations of the period from 1970 to the present time in order to arrive at a comprehensive general framework in which the emergence of a *new* epistemic culture, based on *closed* laboratory research and *possible* worlds-accounts, could be related to massive changes in the technological settings of laboratories across the social sciences as well as to a growing disenchantment between the predominant discourse-coalitions of the late 1960's and early 1970's. (For the process of consecutive epistemic regimes from 1800 to 1970, see esp. WITTROCK

modeling$_P$ and modeling$_A$, between modeling rules$_A$ and modeling rules$_P$, and, finally, between evaluation criteria$_A$ and evaluation criteria$_P$ can be considered both as necessary and sufficient for a *satisficing* dissolution of the modeling dilemma and, consequently, for an *adequate* understanding and interpretation of the role and function of rationality assumptions within the social sciences.

5. Schumpeter Revisited

So far, the article has concentrated on the *overall* solution for the modeling dilemma and, more specifically, to the four Schumpeterian problem areas of rationality only. The *general* dissolution, however, has the distinctive advantage of being *applicable*, *mutatis mutandis*, to the specific four areas-set, introduced in Table 2, too.

In order to stick to the format of a single article and not to the reference frame of a booklet on social science methodology, only a *bare* methodology sketch can be presented to arrive at similar *satisficing* answers for the *specific* four problem fields of the original Schumpeter article. The most important move, which, however, would have been a highly unlikely one for the *realist* Schumpeter (SCUMPETER 1989), consists in a *radically constructivist* turn by pointing to the unavoidable and necessary role of the observer.[56] Following more recent advances initiated by Heinz von Foerster (1985), Ernst von Glasersfeld (1986), Humberto R. Maturana (1985), Jean Piaget (1973, 1983, 1985, 1992) or Francisco J. Varela (1989), the role of the observer must be transformed from an unavoidable nuisance backstage to that of a central main stage-actor. According to this turn of *Bringing the observer back in* (WATZLAWICK/KRIEG 1991), any account of the socio-economic worlds, by necessity, is bound to be observer-*dependent*. In this spirit, rational decision theory (see, e.g. BACHARACH/HURLEY 1994) becomes an *external* mode of attribution no less than the *intentional stance* (Daniel C. Dennett) which, following DENNETT (1987), can be used as an *attribution* strategy for the whole animate and, at least *partly*, for the *inanimate* world, too:

> Do people actually use this strategy? Yes, all the time. There may someday be other strategies for attributing belief and desire and for predicting behavior, but this is the only one we all know now. And when does it work? It works with people almost all the time ... The strategy also works on most other mammals most of the time. For instance, you can use it to design better

1993) Likewise, the distinctions between two different modes of knowledge production, namely mode I and mode II (GIBBONS et al. 1994, NOWOTNY 1995), might serve as an interesting *overall* reference frame, too.

[56] In Chapter 5, the condition of historical reachability is to be abandoned. This requirement has been employed to demonstrate that all *essential* cognitive ingredients would had been available in the 1940's or 1950's already to solve the modeling dilemma. The subsequent remarks in the chapter on *Schumpeter Revisited* are directed to a *contemporary* solution of the rationality problems in four specific areas of investigation.

> traps to catch those mammals, by reasoning about what the creature knows or believes about various things, what it prefers, what it wants to avoid. The strategy works on birds, and on fish, and on reptiles, and on insects and spiders, and even on such lowly and unenterprising creatures as clams ... It also works on some artifacts ... The strategy even works for plants ... It even works for such inanimate and apparently undesigned phenomena as lightning. An electrician once explained to me how he worked out how to protect my underground water pump from lightning damage: lightning, he said, always wants to find the best way to ground, but sometimes it gets tricked into taking second-best paths. (DENNETT 1987:21f.)

Thus, a *contemporary* partitioning[57] of the four areas for rationality problems in particular and modeling components in general can, then, be put forward in the subsequent fashion, where the Schumpeterian internal/external dimension is operationally defined in the following manner: Descriptions of *neural* states or *emergent* descriptions of such states must be located on the *internal* pole and modes of *behavior*-attribution - the *intentional*, *emotional*, *ethnographic* stances ... - on the *external* side. Likewise, relations between an observer and her or his environment can be qualified as *self*-referential whereas relations between a scientific observer and her or his fields *outside* one's own environment is to be categorized as *referential*.

Table 12: A contemporary morphological space for the four rationality problems

	Dimension$_2$	
	observer/ observer (self-referential)	*observer/ observed* (referential)
internal (neural states) Dimension$_1$	Area I	Area II
external (modes of attribution for actions or practices	Area III	Area IV

[57] It must be noted that the requirement of historcial reachability, after having *dissolved* the modeling dilemma in a *satisficing* manner, is dropped for the subsequent modeling sketch which, therefore, will be concerned mainly with *contemporary* advances within the social sciences and related cognitive domains.

Not surprisingly, a wide array of research topics and of different modeling approaches, depending on their Field I- or on their Field III-localisation, can be used. Starting with the problem classes for which all of the examples have been chosen[58], the following specific additions and qualifications become necessary:

Area IV: Classically, Area IV belongs to the *core*-domain of micro-sociology, micro-economics, micro-political science and the like. The preceding discussion should have pointed out the *heuristic* value and the *usefulness* for distinguishing clearly between Field I-approaches and Field III-analyses especially in this domain. Moreover, models of the *homo oeconomicus* variety are most efficiently utilized within Field III-work where they fulfil the necessary rule-requirements and evaluation criteria in a surprisingly *successful* manner.[59] Only *very* rarely however, models, focussing on rational decision procedures or on game-theoretic frameworks, should and can be employed in Field I-investigations.

Area II: For these fields, a cognitive revolution on cognitivism or, to use a book-title by Michael Gazzaniga, on the *cognitive neuro-sciences* has occured (GAZZANIGA 1995) whereby the internal neuro-states of individuals become subject to a rapidly increasing variety either of Field I- or to Field III-explorations. Taking a separation from the domain of Artificial Life (LANGTON 1989, LANGTON et al. 1992, LANGTON 1994, VARELA/BOURGINE 1992) it becomes useful to separate the research-areas, aside from the traditional micro-level, into two *additional* domains. On the one hand, a *basic* or *fento*-area can be distinguished, where

> tasks like ... wandering, avoiding obstacles, wall following, looking for a certain object, delivering some object, cleaning the floor, following someone, etc. (BROOKS 1992:436)
> [60]

become the central focus of investigation. On the other hand, a *meso* or *pico*-domain (see, e.g. AINSLIE 1992) can be identified whose main research interest lies in the problem of *task-integration*. Thus, the following three areas of investigations can be put forward for this relatively recent area of *neural*-based social sciences:

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[58] It should be added that the dissolution of the modeling dilemma is *not* confined to the examples from micro-economics or micro-sociology alone, but can be, in principle, reformulated for any level in the social science-complex: from ist macro-macro-levels well to the level of pico-economics, fento-sociology and the like ...

[59] Thus, the verve of contemporary criticism against *homo oeconomicus* modeling, like the one from Etzioni (1994) or, to a lesser extent, from Friedberg (1995), must be seen as *valid* with respect to its comparative *disadvantages* in Field I and as *highly invalid* with respect to the *successes* within Field III.

[60] It must be stressed, however, that in the articles by Rodney Brooks one finds a separation into *micro*-domains, *macro*-areas and the *ecological* level which, following the terminology introduced here, corresponds to the *fento-* *pico-* and *micro*-distinctions. The new terminology has been chosen for two reasons. *First*, Ainslie's book on *pico-economics* (1992) has become a well-known social science standard for problems of conflicts *within* persons. *Second*, the micro- macro-dualism is very much entrenched in the current social science literature and has acquired, by now, relatively clear boundaries.

First, *tasks* (Rodney A. Brooks), *drafts* (Daniel C. Dennett) or *agents* (Marvin Minsky), at the fento-, pico- and micro-level, especially for senso-motoric processes like walking, seeing, grasping, hearing, and the like ...

Second, *recursive couplings*, especially, but, *pace* Luhmann (1988, 1990), not exclusively communications, at the fento, pico- and micro-level ...

Third, *disturbances*, again on all three levels of investigation, and their corresponding neural settings. (For more details, see MÜLLER 1991)

In Area II, *rationality* will play a major role, especially in the form of principles of *maximization*, *minimization* or *optimality* which become essential for the explanatory frameworks for the interaction patterns of neural groups, *both* for Field I- and for Field III-studies. (See, esp. EDELMAN 1989, 1992, 1993)

The remaining two domains belong to the discipline of science of science since they focus on the actions and practices of scientists (Area III) or on the neuro-settings of scientists (Area I). More precisely, the following reserach topics can be identified which, once again, can be dealt within a modeling$_A$ or modeling$_P$ approach:

Area III: Once again, the same model-types which are at the disposal for Area IV, can be applied to the scientific realm, too. Moreover, extremely interesting moves toward *self*-referentiality could be accomplished since the modeling frameworks$_{A,P}$ can be used for purposes of *self*-explanations, too. Take, for example, a Field I-model which is couched in a master-equation scheme and which is specified to capture the cognitive dynamics within a scientific domain or discipline (MÜLLER 1994b), then, via a *consistent* process of self-specification and data-collection, an explanatory scheme for the most likely diffusion trajectories of this type of model can be built up. Like in the case of Area IV, *homo oeconomicus*-variations will play an essential role in Field III-explorations on the economics of research, on the detection of possible *critical* limits in diffusion processes, on *paradoxical* results with respect to innovation patterns and the like ....

Area I: Finally, establishing links between advances in the cognitive sciences with an in-depth analysis of the neural ensmebles of scientists might turn out, in the future, as an extremely valuable research road. Again, typical Field III-approaches like the utilization of genetic algorithms (HOLLAND et al. 1986, HOLLAND 1992, KOUZA 1993) or, alternatively, PET-tomography and similar experimental routes along Field I should become a frequently used research tool for neuro-based investigations *of science in action* (Bruno Latour). Once again, rationality will occupy a central stage, especially since principles of *maximization*, *minimization* or *optimality* will become essential explanatory framworks for the interaction patterns of neural groups, *both* for Field I- and for Field

III-investigations. (See, e.g. HANSON/OLSON 1990, KOCH/SEGEV 1991, or WISE 1987)

Thus, the unfinished article by Joseph A. Schumpeter could be completed along the main lines just outlined. At this stage however, a major research task is still unfinished and will become the central topic in the next chapter.

6. Rescue Operations

The preceding discussions have made it clear why the escape strategies characterized in the first part of the present article suffer from incurable shortcomings since they are centered, as will be shown in this concluding summary, on *one* side of the two epistemic modeling cultures in the social sciences only. A quick overview will reveal which escape strategies follow which type of modeling arrangement:

ABANDONMENT: The path of *sticking to be a theorist* must be considered as a typical Field III-strategy only, where the role of data, data collection, measurement errors or model-testing play a relatively insignificant role. Taking the first three advices by David F. Hendry seriously -

Think brilliantly ....
Be infinitely creative ...
Be Outstandingly lucky (HENDRY 1987:29f.)

one immidiately realizes that these three rules have a rather different meaning, depending on which field of scientific practices they are applied. For Field I-reserach, brilliance, creativity and luck is needed in order to link social science models to the existing body of data, to measurement standards, to theory testing criteria, etc. As a Field III-strategy, Hendry's Golden Rules must be seen as an attempt to increase explanatory variation, to explore innovative possible world-settings, to focus on unexpected and counter-intuitive explanatory factors, etc. It should be added, though, that over the last decades the abandonment-strategy for Field I- and the reliance on Field III-work has been *actually* pursued by economists. In an analysis of the articles in the *Journal of Econometrics* from 1973 to 1990, Hugo A. Kreuzenkamp and Jan R. Magnus could demonstrate that from a

total of 668 papers, only 21% conveyed an *empirical message that exceeds mere illustration* (KREUZENKAMP/MAGNUS 1995b:19). Upon closer insepction however -

> the papers which explicitly attempt to test a theory statistically are rare (less than a dozen); the cases where a clear conclusion (acceptance or rejection of the theory) emerges, are even rarer. (IBID:290)

APPROXIMATIONS: The rescue operation via the *necessary* nature of approximations is clearly bound to fail since approximations mean, within the context of two distinct modeling cultures, two different things. Within the Field-I, the essential requirement for introducing approximations is the empirical measurable and observable *distance* to those processes for which they are assumed to be approximations. In the context of Field III, *any* assumption may be legitimately qualified as approximation since the *actual* distances do not play *any* significant role in the model-building operations. It may be argued, however, that modeling assumptions with an *intuitive* appeal to close approximations will rank favorably with respect to evaluation criteria like *Anschlußfähigkeit*, etc.[61]

CONFIDENCE-INTERVALS: This type of escape route solves an extremely important problem for the *transformation* of Field III-models into Field I-models, since via measures of this type problems of data errors, specification errors and the like can be dealt with. However, this type of reasoning, like the strategy for abandoning Field I-work altogether, is restricted to a particular, albeit *central* aspect of the overall problems only.

DUALISM: The closest links to the dissolution of the modeling dilemma, offered within the present article, can be found between the dualisation strategy and the modal split into possible worlds- and actual world approaches. The major difference however, must be seen in the fact that dualism is still bound to the reference frame of the traditional epistemic culture whereas here a radical separation between at least two types of cultures has been put forward. Consequently, the dualist proposals can be regarded as *partial* solution which must be enriched, according to chapters three and four, not only with *additional* rule systems and evaluation criteria, but also with a more comprehensive overall framework stressing the successful differentiation and separation into different epistemic cultures.

EXTERNALIZATION: Likewise, the externalization move must be seen as a fore-runner of the presently introduced distinctions of modeling approaches as well as of different epistemic cultures. The main deficiency of the externalisation-strategy lies in the fact that this split does not go much beyond the basic distinctions already introduced by Joseph A. Schumpeter fifty years ago. Accordingly, externalization is a necessary *pre*-requirement

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61 It must be added immidiately that approximations within the context of the whole article are not to be interpreted as a relation between modeling assumptions and the real world, but as a relation between one type of *descriptions*, i.e. modeling assumptions, to *other* types of descriptions like observations, measurements, etc., i.e. to modeling assumptions with a high degree of reliability and credibility.

for a successful dissolution of the modeling dilemma, but its status is that of an *opening* move in a game, not that of a winning-strategy.

INFANCY: The infancy argument may be qualified as the comparatively weakest escape-path since the social sciences have had their great model visions and their corresponding classics like „The Wealth of Nations" or „Tableau Economique" already in the period between 1758 and 1776 which comes relatively close to Newton's „Principia Mathematica" of 1687. Thus, the infancy-strategy must be qualified as a variation of an *immunization-strategy* (H. ALBERT) with zero-content since, despite an already *extremely* prolonged period of *non*-maturtity, it can be, in principle, re-iterated at *any* point in the future ....

INSTRUMENTALISM: The fifth version, namely Freedman's „Don't worry, be happy"-strategy, can be seen as a radical *mixture* between Field I- and Field III-operations, since it presumes both the overall context of a Field I-game and the actual rules of a Field III-operation, i.e. the *freedom to choose* any type of modeling assumption, etc. Thus, Freedman's methodological device can be seen analogous to two players who *start* their game in front of a chessboard with all the 32 chessmen, who turn, in due course, to a game of checkers by eliminating the unnecessary components and by restricting the remaining elements to the diagonal moves of checkers only, and who, in the end, *interpret* the result of their game-operations as victories or defeats in a *chess*-game ...

MODEL-SELECTION: Due to the separation into two modeling approaches, the seemingly uniform criterion of model-selection is immediately split into two subsets of problems. First, within Field I, model-selection implies a search according to a set of criteria similar to the left hand side of Table 7. Second, model-selection$_P$ must be centered around the evaluation criteria of possible world approaches where heavy emphasis should and must be devoted to heuristic criteria like reduction of complexity, paradox detection or the degree of formalizability, etc. Moreover, a switch from criteria of model-testing to criteria of model selection should, in all probability, simply lead to a problem-transfer from one level to a higher one since all the interesting and puzzling problems with respect to choosing a well-*tested* model will re-appear at the level of model-selection criteria *again*.

NONEXPERIMENTAL MODELING-TECHNIQUES: Viewed within the new framework of different epistemic cultures, model-selection becomes a typical device for Field I-work since in Field III the data used may exhibit all sorts of qualities, but, in general, they will be experimental, artificial in nature. Consequently, switching to *nonexperimental* data designs will and can be of tremendous use - but only as a Field I-device for employing models which, by their very design, come closer to the data generation processes.

SEGMENTATION: The segmentation-strategy can be considered, in principle, as a useful reserach task for Field I-approaches as well as for Field III-frameworks simply because it highlights comparative strengths and weaknesses in the application of various research programs. However, taking Rational Choice (RC) as paradigmatic example, the main deficiency of the segmentation strategy lies in the fact that it remains largely unclear *where* the areas of intended applications should be located:

> For Aldrich (1993), the stakes, involved in rational decision making, must be substantial and the individual must have, moreover, a significant impact on the final outcome of actions.
> For Elster (1986), RC-accounts should be used in decision cases with a fixed number of alternatives, where no interference with the decisions of other actors can be assumed.
> For Ferejohn (1991, 1993), RC-frameworks should be implemented in the case of severely constrained actions and interactions.
> For Maoz (1990), it is the absence of extremely low and extremely high levels of stress, since low stress *can imply both low motivational drives and low practical constraints*, whereas in high-stress *circumstances the motivational drive is extremely strong, and time pressure is acute* (MAOZ 1990:318ff.)
> For Schumpeter (1975), *closeness* to the economic realm should qualify as the criterion upon which the utilization of RC-models should be based.

If *high* stake-actions, *severly* constrained, with *fixed* numbers of alternatives, well *between* critical threshold-values of too low or too high stress and, finally, close *enough* to market interactions, are to be understood as the *paradigmatic* RC-applications, *which* socio-economic configurations and ensembles, if any, can *legitimately* qualify as explanatory RC-candidates? Especially in the case of segmentation strategies, it seems worthwhile to differentiate, at the outset, whether the realm of segmented applications is to be located within Field I or Field III.

STRUCTURALISM: It would be too long for this article to highlight the consequences, if any, of the pragmatic separation into distinct fields and into different types of modeling cultures for structuralist reconstructions. But it might be wothwhile to add, at this point, that the pragmatic differentiations with respect to the realms of intended applications$_{A,P}$ or with respect to evaluation criteria$_{A,P}$ both for the theoretical and the non-theoretical model-components could offer an additional set of conditions which could be added in order to be able to account, in a more satisficing manner, for the evolution$_{A}$ of scientific disciplines, especially the social sciences.

WEAKENING: Finally, the split between *thick*-rational accounts and their *thin* counterparts may be qualified as a typical division for the two types of modeling fields. *Thin* analyses happen, under normal circumstances, within the *third* field, whereas *thick* forms of investigations are ordinarily confined to the *first* field. It should be added however, that an argument could be made to use *very* thin modeling components like the consisteny of weak preference ordering also as a testable assumption for Field I-operations, too.

In sum, the distinction, introduced here on at least *two* different epistemic cultures within the social sciences and, consequently, on two separate modeling approaches corresponding to these two cultures, should provide a suitable understanding *why* the modeling dilemma and, consequently, the escape-strategies have arisen at all. The modeling dilemma as well as the attempts for its dissolution can be seen as *futile* endeavors to reconcile exploratory modeling$_P$ operations which, in view of a highly complex environment become *absolutely* essential, with the practices, premises, and principles of a *traditional* epistemic culture. These attempts for adaptation have been and still are bound, *by necessity*, to fail. It has been demonstrated that within the conetxt of an emerging *alternative* epistemic culture, the normal practices and operations with respect to model-building can be interpreted in a comparatively straightforward and *almost* natural manner. Moreover, the escape-strategies for the modeling dilemma enlisted in the second chapter can be seen, in one way or the other, as focussing on *one* side of the two modeling fields in the social sciences only.

7. Concluding Outlooks

Seven years ago, two nobel prize winners in economics, Lawrence Klein and Maurice Allais, delivered, independently from each other, lectures on the future course of economics at the Institute for Advanced Studies. At first sight, the upshots of their lectures seemed utterly contradictory: Klein advocated more interdisciplinary cooperation between economists, psychologists, sociologists, historians and the like, more powerful computers, more data ... Allais, on the other hand, favored more powerful models with few, but highly significant components, more axiomatizations, more thinking ... The present article offers, among many other features, a convenient way to reconcile *both* perspectives by attributing Klein's vision as a rational *widening* strategy for scientific endeavors within Field I, whereas Allais' plea for *deepening* should be considered as a highly relevant agenda for Field III-operations.
In the end, *both* roads to the socio-economic universes around us, be they on the micro- or on the macro-scale, are in almost desparate need for rapid reconfigurations, adaptations, and modernizations.

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