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Notions of theory in the study of innovation systems

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Prelude — Four theses on knowledge in the social sciences

I. Social science does not predict!
There are so many people that want us to predict! This is the ideal of experimental natural science, which supplied the dominant ideal in postwar philosophy of science! You have your laws combined into a theory which predicts the outcome of an experiment. Then you conduct the experiment and your theory is tested. It would have been nice (although scary) if we could have proceeded this way, but few social scientists do, and this should not be our model of how we discover and accumulate knowledge.

II. Social science only reconstructs!
We always deal with things that have happened. There are all kinds of border cases to this generalization, and we may discuss them, but I submit that these border cases are marginal (although, cf. Thesis I, they are often suggested to us as ideals!) Certainly, social scientists can predict the future movement of certain aggregate rates, but not exactly enough for speculators to dispell with all their networking to be ahead of the crowd of stock market loosers! Social scientists are careful to point out that such projections can not be applied to single cases, which very well may turn out to be outliers. Correspondingly, it is not in our power to predict accurately grand singular historical breaks, such as the end of the Cold War (a famous cases that has been discussed in this light). Why is it that we do not predict singular cases? Most likely because social science has no solid universal laws (no arsenal of laws that have so far resisted falsification!), and — more profoundly — that we would not like to live in a society in which one (or rather: a small elite) would use general laws to predict how each single one of us will actually behave!

III. Social science does more than just to deconstruct!
If we only reconstruct events and processes that have happened, are we then nothing but historians? That depends. The days of nationalistic writing of history have long passed, at least in those parts of the Western world in which such history emerged. Historians know that any reconstruction is a construction. It is based on certain research questions, and these derive from certain standpoints. Certain positions in the philosophy of science and in the sociology of scientific knowledge tend to conclude from this that we do nothing but to tell stories, and that there is no way to judge some

stories to be more scientific than others. The problem with this position is that it turns all scholars into philosophers and imprisons them for enternity in a discourse about eternal — and thus unsolvable — problems concerning the nature of reality, what truth is, and so on. Here we need to be more pragmatic, that is, we need to work with a pragmatist philosophy. We need to see what we actually have got. What we have got is: a series of local research frontiers! Anybody willl now accept the general postmodernist critique of the ideas of European enlightenment, at least when these ideas are interpreted to imply that science can unveil the total structure of the universe. But the deconstructionists fail to see that there are many specific research areas where social science has in fact been cumulative (such areas are the study of welfare states, womens studies, the study of innovation systems, etc. etc.)

IV. Social science produces grounded theories based on constant comparison!
Social science must find its identity outside of the natural science ideals, but also outside of the philosophical fundamentalism of the humanities faculties. If social science is to find its own identity, it should accumulate knowledge in specific fields of research, working with reconstructions of processes that we find interesting, given our research problems. Doing this, we apply comparative reasoning: we start with some concrete case, then we proceed to map similarities and differences, we respecify our research problems and finally — after many such turns — arrive at specific explanations for phenomena of great significance in our present life situation.

Notions of theory in the study of innovation systems

Over the last decade several strands within social science has made contributions to the understanding of the diversity of present-day capitalism: business systems, innovation systems, systems of industrial relations, models of economic policy making, financial systems, welfare states, education- and training-systems, foreign policies, and so on.

These are all local research frontiers contributing to the broader research frontier that strives to understand the dynamics of present-day capitalism. We can think about interrelations between research frontiers in many ways: if we focus on innovation, we can trace various levels within that frontier, as in the conventional distinction between regional, national and global patterns.

This paper contains some notes on methodological questions of relevance to researchers who contribute to one or more of these research frontiers. I will expand on arguments and distinctions introduced in an earlier study of comparative historical social science.[1] I there concluded that we can distinguish three syntheses in contemporary social science: the rational choice synthesis, the social theory synthesis and the interactionist synthesis. (By “synthesis” I mean a specification of how theory and empirical observation may be connected, that is: specific ideas as to how

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[1] Lars Mjøset, “An Essay on the Foundations of Comparative Historical Social Science”, *ARENA Working Paper,* No. 22, August 2002.

knowledge can be accumulated.) There is also a non-synthesis, that of deconstructionism.

Some readers may complain that what I present here is yet another attempt at abstract methodological discussion. For certain, parts of my discussion draws on the philosophy of science, but I emphasize that my ambition is to discuss methodological questions in direct relationship to applied, substantive research.

1. A philosophy of science background

If we ask what social scientists mean by *theory* we can always classify the answer into one or more out of these three categories: middle range (explanation-based) theories, interpretations of the present, and fundamental (basic) theory.

First, social science gives us substantive, explanation oriented knowledge, often called "middle range theories" or "grounded theories" (the difference between the two will be specified later): such theories (mostly talked about in plural) are related to specific fields of knowledge-accumulation, so we have decentralized research frontiers depending on what areas funding agencies (mainly the state, but it could also be private foundations, associations, firms, social movements, etc.) want to fund. There is also an impact of the cultural problems influencing at least on the research universities. In all the related fields of relevance to the study of capitalism's varieties, such as national systems of innovation, welfare states, comparative industrial relations, comparative educational systems, etc. we find numerous "low-key"-theories that are explanation based.

Second, the social sciences provide "interpretations of the present": this holds both for the most narrow pieces of contract research and for the grand sweeping statements about organized capitalism, late capitalism, risk society, late modernity and the like. There are also many attempts to dfine the global context for capitalism's varieties: the post-Cold War era, the fifth (ICT-based) great surge, and so on. Since social science knowledge participates in society, it always interprets the present situation.

Third, it is also held that social science provides and/or is in need of general or basic or fundamental theories. The most famous one in postwar social sience is clearly the rational action theory of neoclassical economics, but since there are also quite differenttheories of action (notably in sociology and anthropology), we here need to introduce further distinctions. There are two very different notions of *high-powered,* fundamental theory around.

First: In the 1940s and 1950s the vision of fundamental theory was dominated by the ideal of experimental natural science. This ideal still exists, but more muted and modified, coming mainly in the form of rational choice theory (matehematized action theory on the neoclassical model which was again modelled on equations of classical mechanics) or in the form of "causalism": that we can gain knowledge of causal processes by utilizing sophisticated statistical methodologies.

Second: Quite another ideal is the philosophical ideal of general transcendental theory, laying down the basic categories of a theory of action, knowledge and social structure:

here a stream of philosophical worldviews is being promoted, but it is hard to see any convergence.

We obviously need some distinction to separate different conceptions of fundamental theory. This differentiation is well known from methodological debates that have flourished ever since social science was born.

Over the last few decades, several social science scholars have critisized certain deeply enthrenched dualisms in our field. I share such a skepticism, and in this note I shall critisize how the methodological dualism has been interpreted as a dualism between explanation and understanding, between nomological and ideographic approaches, and so on. Such dichotomies pit natural science against the humanities. A main problem is that this opposition reflects the academic division of labour more than a hundred years ago. By today, the social sciences have expanded and form a more distinct third group. We should not restrict our methodological reflection to a choice between ideals drawn from either the human or natural sciences. We should above all reflect on how (and the extent to whic) it accumulates knowledge _ thatis: establishes theory — with reference towhat social scientists do when they do good research (good as judged by the community of researchers). Social science should think about what it does on its own terms. Conventionally, the many dualisms are summed up as a dualism between positivism and the critique of positivism. This is not very helpful, since again social science gets no place of its own and the term "critique" can be interpreted to mean that social science is only understood as a negative contrast to the natural sciences. Even the philosophy of the natural sciences — by the way — has moved a long way since the days of logical positivism: Via Popper's critique it has moved into questions about scientific realism. On the part of the social sciences, yet another dualism has been developed in response to that development: (critical) realism versus constructionism. But for reasons to be stated later, I am even in doubt as to the usefulness of that dualism.

I suggest that we should look for various practical philosophies of science, understood as different attitudes on the part of the researchers, attitudes that are best visible when researchers — refering to the actual research process — legitimate their work as scientific. Let us approach this dualism of researchers' attitudes through the following definition of theory,[2] a term which is in fact not often defined:

> *Theory is accumulated knowledge, organized by the human mind for purposes of explanation*

Specifying this general definition, I find six notions of theory (Row 3 in Table 1) in present discussions of the philosophy of the social sciences. Table 1 shows how they can be classified according to different attitudes on the part of the researchers (row 1).

This yields a threefold typology of practical philosophies of science, one is *optimist,* rooted in the experience of natural science, particularly its experimental branches. Another is *skepticist,* and it is linked to inspiration both from philosophy and the humanities (especially reflecting the attitude of the humanities that no longer draw their legitimacy from the celebration of their respective national cultural heritages). But

[2] Mjøset, "An Essay on the Foundations of Comparative Historical Social Science", p. 2.

there is also one that can be linked specifically to the experience of doing social science, one which I shall label pragmatist!

It is of course possible to discuss these as philosophical positions: variations of positivism, pragmatism and skepticism. But this is not the point here — the focus is on these three types as practical philosophies of science, as three types of *habitus* characteristic of these three branches of the academic division of labour — or to be more specific: the focus is on three attitudes *within social science,* reproducing these three different kinds of *habitus.* This "overlay" produces different notions of theory within social science. The six conceptions of theory, then, are produced by social scientists positioning their own field relative to other branches of science: optimist notions of theory emerge as social scientists think about their work in the light of ideals drawn from natural science (and the philosophy of the natural sciences), skepticist notions emerge when the reflection is based on the humanities/philosophy, and pragmatist notions emerge when social scientists think of their own work as sui generis in comparative contrast to both the natural sciences and the humanities/philosophy.[3]

A more fine-grained overview over ways of doing social science, may be derived by relating the six types of theory to a list of the main empirical procedures employed in social science (e.g. variables oriented, comparative, case-oriented, etc.) Such a matrix will not be specified here.

This framework allows us to give the briefest history of postwar notions of social science theory. The natural science ideal was dominant (and U.S.-based) in the 1945-1960s-period. It was then challenged by notions of theory drawing more closely on the experience of doing social science (both a U.S. (Chicago school) and a European tradition (critical theory) with roots in the pre-1940-period) from the 1960s, and somewhat later also by notions that drew their inspiration from either philosophy or the humanities. These latter approaches mostly had European roots, mainly German and French.

We see that the distinction between "positivism versus critique of positivism" is much too crude, both sides of that dichotomy has distinct varieties. Rather than stating that social science has a choice between positivism and its denial, we should conceive of social science as a field in which efforts to reflect independently on its own activity are constantly challenged by attitudes rooted in the national sciences and the humanities/philosophy respectively.

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[3] Although this was not my starting point at all, I see here some convergence with Habermas' early discussion of the difference between three groups of sciences: empirical-analytic (with a technical knowledge interes), historical-hermeneutic (with a practical knowledge interest), and human action-sciences (with a liberating knowledge interest). Jürgen Habermas, *Technik und Wissenschaft als 'Ideologie',* Frankfurt a. M. 1968, p. 157-158. There is a difference in the understanding of the "practical knowledge"-interest, which Habermas conceives as understanding of meaning (from interpretation of texts) oriented towards a possible consensus between actors within an "inherited self-understanding", whereas I discuss this position in its relationship to the the position of philosophy/the humanities in the present academic division of labour. — In his later writings, Habermas has moved critical theory into the sphere of trasncendental theory of action/structure, while Adorno was close to the deconstructive postion (though retaining abstract avant-garde art as a surviving "critical subject"). The original Frankfurt school — contemporary with the Chicago school — was close to a standpoint/social movements notion. That element, however, is still retained in Habermas (that is: his communicative ethics), leading an uneasy coexistence with his attempt to develop a transcendental theory of life world and system.

Table 1. Six notions of theory in the social sciences

Practical philosophy of science	Optimist		Pragmatist		Skepticist	
Emphasis in the definition of theory	“Accumulated knowledge...”		“...for purposes of explanation.”		“...organized by the human mind...”	
Rooted in the ideals of	Natural science (deductive-nomological)		Social science	Humanities/philosophy		
Notion of	Law-oriented (nomological aspect)	Idealizing (deductive aspect)	Grounded theory	Critical theory	Trans-cendental	Deconst-ructionism
Examples	Merton: middle range theories. Elster: mechanisms.	Neoclassi-cal econo-mics, game theoretic models	Chicago school sociology, Blumer, Glaser & Strauss	Frankfurt school tradition, standpoint theories	Social phi-losophers, such as Giddens, Habermas, Luhmann	Post-modernists, post-struc-turalists,

Researchers with an optimistic attitude strives to establish theory at the highest possible level, and they approach empirical research “top down”. Such an attitude reflects the hope that all research frontiers can and should converge on an interconnected set of high-powered theories.

Researchers with a skepticist attitude either explicitly doubt that there will ever be such a convergence, *or*they strive to specify a basic transcendental theory which is supposed to supply a necessary foundation for all social science (the sciences of human action).

There are marked differences between these two varieties, and it may seem unfair to label the latter “skepticist”. Still, it can be defended, since the priority given to elaboration of fundamental, transcendental theory deflects attention from empirical research, and since the various transcendental frameworks proposed seem to be personal, in the sense that there is little convergence, while all the frameworks are hampered by a gap between transcendental teory and empirical investigations. At their best, scholars such as Giddens or Luhmann rearrange material drawn from a wide range of substantial studies, but being led only by a highly personal fundamental theory of e.g. “structuration” or “autopoiesis”, they are not able to escape the traps of older philosophy or history: they provide personal interpretations of the present, too far above the relevant local research frontiers.

The deconstructionist notion implies that any theory is a result of power (in this respect, the subsystem of science has no autonomy whatsoever vis-a-vis the restof society), so no knowledge can be trusted. At its roots, deconstructionism emerged to unmask the “transcendental subject” of Enlightenment Europe, and it has continued to hit at any transcendental constructions. The problem, however, is that skepticism — in a Nietzschean style — mostly is developed into nihilism.

In a sense, there is within this group, a permanent back and forth movement between suggested frameworks, deconstructive attacks, new frameworks by younger social philosophers, new deconstructive responses — and so on. Thus, these notions of theory are border cases when we relate them to the definition above, which has "accumulated knowledge" as a crucial ingredient.

Alternatively, to the extent that scholars who subscribe to deconstructionism try to move into substantial studies from a deconstructionist position, they tend to pursue "discourse analysis". To the extent that they do good craftwork, they may contribute reasonable explanation-based theory (which I would classify as grounded theory). But they should not believe that this is new, nor that it necessarily requires reliance on the structural linguistic analogy: constructionist studies of social problems have existed for about 100 years in American social science (e.g. within criminology), and although such studies often lack the philosophical ornaments that one can find in Foucault's studies, as substantial research, they are often quite parallell to Foucault when he studies specific professions, practices, risk-perceptions, etc.

An optimistic philosophy of science implies concepts of theory that were originally worked out in the natural sciences (deductive and/or nomological). Both the skepticist and pragmatist positions doubt that this ideal is meaningful when the research community consists of the same *kinds* as the kinds studied! The skepticist position, however, understands *theory* to be reflection on the fundamental conditions (of society, of action and structure). Within deconstructionism, this reflection leads to the position that theory is impossible: theory is seen as always partial, constructions to be understood as expressions of social power, while it would be inconsistent to try to establish the fundamental theory of the power structure.

Both varieties of skepticism are different from the pragmatist position, which intends to contribute to accumulation of knowledge, but which — at the same time — is able to reflect on the situated nature of this knowledge (thus applying a sociology of knowledge perspective, where they may well converge with elements of the skepticist position). With such an attitude it seems relevant to think about partial accumulation of knowledge in local research frontiers.

Since the transcendental and deconstructive notions have been of little influence in studies of innovation,[4] I restrict my further discussion to the four other notions of theory. In the following, then, we shall look more closely at optimist and pragmatist notions of theory.

2. Optimist notions of theory

Such optimism was an integral part of the 17th and 18th century scientific revolution, where theory was understood to mirror the mechanical world as created by God. The definition of theory above can be modified for this case: "Theory is accumulated knowledge, as originally organized by God." Theory implied the rediscovery of the mechanisms of both the natural, social and human world as God had created it.

In the early 20th century a secularized version of this view emerged, defining the dominant research attitude well into the 1960s. The objectivist scientist took the role of God! Engineers already applied the accumulated knowledge of natural science to solve

[4] Although that influence can be traced e.g. in critical human geography.

complex technical problems. In parallell, the social engineer would apply the accumulated knowledge of social science to solve social problems. Social science knowledge was seen as an autonomous subsystem, with social scientists managing the stock of accumulated knowledge, clearly demarcated from other kinds of knowledge (non-scientific knowledge, e.g. ideology). The contribution of (social) science to the solution of such problems was basically an application of the "so far best" theory. It was not deemed necessary to be concerned about whether social scientific knowledge was in any sense dependent on or influenced by the specific social problems that the scientists encountered. Both technical and social engineers were conceived as experts: although the engineer is a body in flesh and blood, his or her knowledge was still seen as objective, independent of any national or social context that the researcher would operate within. The social problems expert possesses objective knowledge independent of his or her "participation" in society.

This ideal inspired the dominant notion of explanation in early postwar philosophy of science: The covering law model, the nomological-deductive ideal, is a syllogism (*modus tollens*), combining three features: a notion of causality, at least one general law, and a notion of explanation which says that the law(s) make(s) initial conditions into causal factors which explain the outcome. Theory, then, is the collection of such laws, and these laws organize the necessary initial conditions to provide explanations. The paradigm case is a natural science experiment, where the researchers control the initial conditions, testing the theory by generating the predicted outcome.

Contemporary optimist attitudes in social science are modified versions of this view. The basic problem, soon realized, was that attempts to accumulate knowledge in the concentrated form of laws failed in the social sciences. It only resulted in tautological statements ("the less social cohesion, the more anomie"), and these provided no explanation until a multitude of contextual factors had been specified. The law(s) themselves were of no help in leading the researchers to these contextual factors.

Thus, the nomological-deductive ideal was split in two: The *nomological* branch is various programmes that strive to discover of limited regularities that may be "law-like" within certain contexts (contexts that are much more narrowly defined than the "scopes" of natural science theories). The *deductive* branch, on the other hand, decontextualize by establishing model worlds within which one can make all kinds of deductions of e.g. various games that may fit empirical cases.

Natural science still provides the normative ideal, but social science is seen as immature and the ideal is to be realised sometime in the future. This is where both pragmatists and skepticists depart, reflecting the attitude that this ideal will never be realised.

Let us first deal with the deductive branch: The basic version of the idealizing notion of theory (see Table 1) we find in neoclassical economics, who understand fundamental theory as a general equilibrium formalized by a set of simultaneous equations. One of the sources of inspiration behind this notion was inspired by the physical theories of electrical fields, which is particularly seen in Pareto's direct transfer of "the equations of rational mechanics" into economics.[5] Later, more sophisticated notions draw on the

[5] Philip Mirowski, *More Heat than Light,* Cambridge 1989, p. 221 f, 271 f, also quoting (p. 357) Norbert Wiener who wrote in *God & Golem, Inc.* (1964) that "[t]he mathematics that the social scientists employ and the mathematical physics that they use as their model are the mathematics and the mathematical

mathematical area of game theory.[6] Is this compatible with a notion of causal explanation? It seems that unlike the physicists, the economists has had great problems of specifying empirically the parameters. Instead, they turn the models into *thought experiments.* Such experiments differ from real experiments: they do not predict an outcome which is to be produced. They rather claim to explain certain given facts, such as the functionning of markets (the existence of market clearing prices).[7] Rather than manipulating causes to produce an effect, they make claims about the causes of an existing effect.[8]

Heterodox economists have claimed that the pure "calculation models" of neoclassical economics are actually a remnance of the "natural order" invoked by 17th and 18th century natural science, that is, the mechanical world as created by God (cf. the definition of theory as conceived by 17th and 18th century natural science).[9] Equilibrium models thus contrast with the reconstructions that are conducted in order to understand the causal sequences which have led to the present state of an economy/sector/firm.[10]

Turning now to the nomological branch, the broadest version of the law-oriented notion of theory (Table 1) is middle range theories of the Mertonian kind. This can be seen as an outwatered covering law model: a model of explanation with initial conditions only, devoid of any organizing laws, but established by the ambition to explain via as general patterns (patterns that connect initial conditions) as possible, with the promise that some time in the future, such explanatory efforts may converge into theory of the law-based kind.[11] This quest, always to establish theory at the highest possible level is

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physics of 1850 ... Their quantitative theories are treated with the unquestioning respect with which the physicists of a less sophisticated age treated the concepts of Newtonian physics. Very few econometricians, are aware that if they are to imitate the procedure of modern physics and not its mere appearances, a mathematical economics must begin with a critical account of these quantitative notions and the means adopted for collecting and measuring them."

[6] The recent move towards experimental, "cognitive economics" (with Kahnemann receiving the 2002 Nobel prize in economics) is an interesting sign that could lead economic theorizing away from its style of pure thought experiments. A recent overview is Daniel Kahneman & Amos Tversky, eds., *Choices, values, and frames*, Cambridge 2000.

[7] Neoclassical economists used to talk about laws of supply and demand, laws of diminishing returns, etc., but as has bee pointed out, these are not universal laws, it is not hard to find exceptions to them. They are more like normative statements, if one wants to maximise efficiency or utility one should organize the economy in accordance with these laws (so they are more akin to legal norms). The problem, however, is that in this argument, "the economy" is th thought experiment of the researcher, so that in the contextual world of real lfie it is not clear what the effects will be. Another question is whether game theory models can be seen as laws: in social science they are mainly applied to reconstruct interaction on which the researchers already has information. A full discussion of this would relate to the complicated philosophical discussion on reasons as causes. But if reasons are causes, they can at least not be seen as causes in the sense of the covering law model.

[8] John Goldthorpe, *On Sociology,* Oxford 2000, Ch. 7 has a very good discussion of this difference.

[9] Thorsten Veblen, *The Place of Science in Modern Civilization,* New York 1918 is the *locus classicus.*

[10] One of the most original followers of Veblen and Weber, Johan Åkerman, explored this dualism between calculation models (equilibrium models) and causal reconstructions. He emphasized that there was no one overarching equilibrium model for the economy, only models for specific collective actors. These models were related to causal reconstructions worked out by the main economic actors, such as the state, sectoral associations, particular firms and so on. See e.g. J. Åkerman, "Economic Plans and Causal Analysis", *International Economic Papers,* Vol. 4, 1954, pp. 181-194, and J. Åkerman, "Is it possible to complete economic theories?" *Economia Internazional,* 1957, p. 413-424.

[11] Mjøset, "An Essay on the Foundations of Comparative Historical Social Science", p. 5-7, p. 13. The standard reference is Merton's essay "On theories of the middle range", added in the third, 1968-edition

perhaps the most typical effect of the cognitive optimist attitude. The ideal theory consists of *as accumulated knowledge as possible,* implying a disregard for the other dimensions of the definition above, that this knowledge is organized by human beings and that it relates to practical questions of explanation in specific contexts.

Depending on the kind of data analysed and the research methods employed, we can specify several versions of law-oriented middle range theory in the social sciences. One notion is particularly worth mentioning here. Most of applied economics share with other social sciences a reliance on statistics. In the interface between social science and statistics, theory is often presented in the form of a regression equation or some other statistical model which is established from the correlations between variables...

$$Y^*=b_o + b_1X_1 + b_2X_2 +...+b_KX_K$$

Such models have substance, as the coefficients are derived from data. Still, these data are not produced by experiments, they are the results of "Nature's experiments".[12] There was, in the 1950s and 1960s, intense efforts to get from correlations (covariation) to causes. This work within econometrics and statistics inspired the "causalism"[13] in other social sciences (sociology, political science) in the 1970s. In this approach, causality was defined by properties of statistical models. This notion has recently been in decline, replaced by an emphasis on "lean econometrics" and the use of statistical models for descriptive purposes. In those cases, then, a notion of middle range theories is clearly implied. Research results are presented at the "highest level" possible, and the findings are supposed to contribute to the accumulation of fundamental, e.g. economic theory.

A specific problem is that most of the statistical models employed contain strong homogenizing assumptions.[14] Unless some kind of clustering procedure is employed as a first stage of the analysis, it is assumed that general patterns will exist across the whole population. Organizing the data by some kind of statistical technique produces accumulated knowledge (e.g. a regression equation) which explains a large share of the cases, while leaving a certain percentage of them as outliers about which nothing can be said at all. Thus, even the techniques used inspire the researchers to think in terms of the broadest, most parsimonious patterns possible.[15]

The dilemma of all middle range theories is that they are provisional. The term "middle range" itself cries out for progress towards higher levels! The more optimist the researcher, the more this researcher will emphasize that untill theory has been established at higher levels, it is really hard to draw any conclusions at all. Researchers with such an optimist attitude are never really happy researchers! This comes out also if we consider social scientists who strive to legitimate their activities with reference to the mainstream philosophy of the natural sciences. They tend to borrow the very

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of his *Social Theory and Social Structure,* New York 1968. Earlier editions only mentioned the notion briefly in the preface.

[12] Trygve Haavelmo, *The Probability Approach in Econometrics,* supplement to *Econometrica,* Vol. 12, 1944, pp. 1-11.

[13] Andrew Abbott, "The Causal Devolution", in Abbott, *Time Matters,* Chicago 2001, Christopher Bernert, "The career of causal analysis in American sociology", *British Journal of Sociology,* 34:2, 1983, Goldthorpe, *On Sociology,* Ch. 7.

[14] Charles C. Ragin, *Fuzzy-set Social Science,* Chicago 2000.

[15] The wish to see statistical modelling as quasi-experiments is clear, e.g. in the classical formulations of Haavelmo's *Probability Approach,* quoted above.

normative principles of Popperian falsificationism. Strikingly, they end up admitting that with reference to these norms, social science has *not* produced much accumulated knowledge at all — yet![16]

Pragmatist notions of theory

Our discussion so far has led us to the optimist "unhappy consciousness". Of course my further message is that one can be much happier as a researcher by reflecting on our predicament from a pragmatist point of view.

The pragmatist notions are grounded theory and critical theory. It may be surprising to some that critical theory is dubbed "pragmatist", but I shall show why I think this is a relevant claim.

The tradition of grounded theory roots in American pragmatist philosophy, which in the early part of the 20th century was known mainly through the writings of John Dewey and William James, but is now also with us in more sophisticated versions with reference to the complex (mostly posthumously published) work of Charles Sanders Peirce. These impulses were brought into social science by Veblen (thus constituting the tradtion of evolutionary economics), and by the Chicago school of sociology (inspired by the work of philosopher George Herbert Mead in social psychology). The latter tradition suvived the hegemony of the natural science ideal, above all in the work of Herbert Blumer.[17] Above all, his discussion of concept formation in the social sciences was a major contribution, one converging with the emphasis on the "indexicality" of concepts among social scientists working with the "strong programme" in the sociology of science, picking up on Schütz' phenomenology, Garfinkel's ethnomethodology and the late Wittgenstein's ordinary language philosophy. The notion of grounded theory emerged from Glaser and Strauss' elaboration and upgrading of the Chicago school tradition, focusing on the practical conduct of qualitative social research.[18] The most recent contributions to this tradition — notably the work of Abbott and Ragin — has added a detailed internal critique of the conventional methods of variables-oriented social science.[19]

The pragmatist position emphasizes the indexical nature of all social science concepts (like concepts, rules, norms in general social life), they can never be dealt with in isolation from the interaction orders through which they are constituted and reconstituted, this being an ongoing process. The items that social science studies are not natural kinds, but interactive kinds.[20] The researcher is of the same (interactive) kind as the kinds studied, interaction between the two is in principle always possible (which is *the* core insight in any branch of the critique of positivism: positivism was rejected because it wanted to regard interactive kinds as (non-interactive) natural kinds).

[16] Goldthorpe, *On Sociology,* p. 158, Lee Freese, "The Problem of Cumulative Knowledge", in Lee Freese, ed., *Theoretical Methods in Sociology,* Pittsburg 1980.

[17] His main statements from the 1930-1960-period were collected in Herbert Blumer, *Symbolic Interactionism,* Englewood Cliffs 1969.

[18] Barney G. Glaser & Anselm L. Strauss, *The Disovery of Grounded Theory,* New York 1967.

[19] Ragin, *Fuzzy-set Social Science;* Andrew Abbott, *Time Matters*.

[20] Ian Hacking, *The Social Construction of What?* Cambridge, Mass. 1999.

Grounded theory is established through a bottom-up process. It can also be termed explanation-based theory. In that sense, it differs from optimist notions of theory, where explanations follow from theory, and skepticist notions of theory, where theory consists of "transcendental conditions" of explanation. In the pragmatist notions of theory, theory is created through explanation, that is through a bottom-up process.

The researcher, being a participant in society, has a research problem, an intention to gather knowledge on social processes on behalf of somebody (the community of researchers, some funding agency or firm, some social movement, etc.). The starting point may also be some *stylized facts* (a term that fits well as the notion of one possible starting point for grounded theory.) If the study is a qualitative one, detailed information on cases relative to the research problem is collected. There is a back and forth process between framing based on earlier (grounded) theory (relevant to the research question), and images generated as data are analysed.[21] To the extent images lead to new frames, new grounded theory is established. Generalization is relative to the research question asked. The analysis of one or more cases leads to results that may be tried out further through a process of constant comparison. The choice of new cases is often called theoretical sampling. Pragmatic concerns relating to the research question determines when this process is (temporarily) stopped. Whether more knowledge will be developed depends on the focus of the community of researchers and of the interest of those who fund research (and in many cases, on a tense, even conflictual relation between the two). If focus and interest remain, a local research frontier is established.[22] Quality control follows within the community of researchers, rather than through external — i.e. with reference e.g. a natural science ideal — norms or criteria.[23]

Various ways of framing is possible in the light of different, but connected research problems, and a certain family of research problems will evolve as a local research frontier is established. Thus, Glaser and Strauss, firmly opposing the top-down "verificationist" approach to theory, emphasize that if we just *test* already existing theories, we learn nothing new, at least not until we have an even more general theory! In a pragmatist, bottom up perspective, they emphasize that the more theories we establish, the better. Theory is not related to prediction, but to reconstruction of processes that have occurred (not excluding processes that the researcher was herself involved in, cf. participant observation). Theory is related to explanation, and explanations are a basic input into learning, and the more theories, the more aspects we learn about.

Glaser and Strauss distinguish substantive and formal grounded theory.[24] Substantive grounded theory is related to one specific research field. Based on exploration of the

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[21] Ragin, *Constructing Social Research,* Thousand Oaks 1994.

[22] Postmodernists within social science may argue that the emphasis on local research frontiers is simply "Enlightenment" illusions. But grounded theory, as we show, differs from theenlightenment optimism by relying on a pragmatic philosophy of science. This implies a pragmatic notion of learning, not an enlightenment one (illusion of commanding all knowledge). This notion is more about knowing how to learn.

[23] In "Some Conseqeunces of Four Incapacities" (1868), C. S. Peirce agued that the notion of reality essentially involved the notion of an unlimited *community* — what information and reasoning would eventually lead to — and is thus also linked to the possibility of "definitive growth of knowledge". The essay is available in *The Essential Perice. Selected Philosophical Writings,* Vol. 1., Bloomington & Indianapolis 1992, pp. 28-55.

[24] See Glaser & Strauss, *The Discovery of Grounded Theory,* p. 32 ff.

field in the light of specified research questions, explanations are provided. The scope of these explanations are then investigated through a specification of the population studied. One may go on to explore other populations by comparison, which implies that a typology is created.

How specific a typology we establish, again depends on the research question asked. If the research question requires an answer at a level which allows intervention through decision making, and if the cases studied constitute a homogenous population at this level, one may well talk about "realism" in the sense that "driving forces" are investigated.[25] These may be reconstructed as a conjunction of causal factors (but understood as "initial conditions", not as laws). This is intended as a "determinist" explanation, although many causal chains leading to the conjuncture are cut off. It is not determinism that rests on deduction from universal laws. It is a "relative" determinism in the sense of being the so far most comprehensive and accepted reconstruction of the events/processes that are brought into focus by the research problem. This indicates the importance of the research community, the network which controls the "state of accumulated knowledge" in specific fields.[26] Not being focused on laws, this is not a determinism that can lead to prediction. There is no claim to be predictive into the future.[27] As we have noted, social science must be founded on reconstructions, based on explanations of what has happened earlier. When new outcomes are adressed, or when one tries to judge about a future outcome, it must always be checked whether the conjuncture of factors has shifted, or new factors must be added, while others are irrelevant. Exact assessment of the relative force of factors can seldom be established, but clearly a conjuncture may be changed both if one or more factors become more influential, or some factor disappears or new ones emerge.

Furthermore, if we are arguing at a level which allows intervention through decision making (for instance: organizational analysts who study a specific firm, researchers who feed their knowledge back to social movements (e.g. the womens' movement), social scientists who are area experts for non-governmental aid organizations, economists who act as experts for governments or international organizations, etc.), it is immediately evident that grounded theory at this level is *critical theory!* That it is becomes clear as soon as we reflect on the fact that the "expert" holders of knowledge (grounded theory) are themselves human beings.

Let us assume that researchers find regularities that are seen as unfavourable in the light of some objective. Decision makers learn from this knoweldge and set out to alter the regularities. We may find cases of extensive consensus, so that the changes made are close to "social engineering". More commonly, there will be struggles, conflicts or controversies surrounding these regularities. (Conflict can be due to conflicting civil society forces, but could also involve differences of opinion between experts, "reflexive modernization"). The researcher or the research community will be forced to take a stance. The more contracted research there is, the more dilemmas will emerge, although the nature of the dilemmas will differ according to the broader context. Researchers who study the welfare states of social democratic Norden may have an easier time than

[25] My hunch here is that the debate on realism/constuctionism is uninteresting (or merely philosophical) if it is not related to the level of explanation, which again is related to the research question asked.
[26] The norms of pure science, including its "communism", was a dear topic in Merton's sociology of science, cf. Merton, *Social Theory and Social Structure,* Chs. XVII-XVIII.
[27] These arguments can be read as a rejection of Goldthorpe's criticism of Ragin's explanation by multiple, conjunctural causation, cf. Goldthorpe, *On Sociology,* Ch. 3.

those who study squatter movements in Brazil, and those who study national innovation systems may have an easier time than those who study labour relations.[28] The main formal point is that the contextual regularities (by optimists called middle range theories, by pragmatists called grounded theory) found by social science are fundamentally different from natural laws, simply because they can be changed.

Formal grounded theory is the discovery of formal patterns (mechanisms, structures) across various substantive research fields. An example used by Glaser and Strauss refers to their study of "awareness of dying" where they found that nurses treated dying patients according to the "social loss" represented by the patient's eventual death and notes that the theory could be formalized into a formal theory "of how professional people give service to clients according to their respective social value".[29]

With formal theory, Glaser & Strauss claim that they fulfill two important requirements of theory: parsimony of variables and formulation, as well as scope, the applicability to a wide range of situations. But it may be asked whether such a theory explains, since for an explanation to be given, the context of the respective fields must be specified: it would seem, then, that only substantial grounded theory can be seen as explanation-based theory. If all explanations relate to specific research questions, formal grounded theory would be explanatory only if we could ask entirely decontextualized research questions. This, it seems, does exist in natural science, where at least some basic research istotally driven by the imperatives of "theory development" along quite fundamental research frontiers (one thinks of elementary particles). In social science, it seems that decontextualized research problems, that could so to say absorb many local research frontiers, are irrelevant. At least, they do not lead to systematic theory which could yield relevant knowledge through deductive sequences from fundamental theory (which, if possible, would have relieved us of the need to specify context).

But formal grounded theory may still be useful in the following sense: it can yield "modules" that recur in explanations. Formal grounded theory can thus be regarded as an inductive, bottom up "theory of action" (typical patterns of action),[30] as in Simmel, or later, in Elster's explanation by mechanisms,[31] and in Ragin's "search for similarities" in qualitative methods. But as formal grounded theory, these patterns do not explain. These high level formal patterns may only be helpful as analogies to be extended, that is: the extension of patterns found in several other research fields may

[28] Cf. Hacking's discussion of "grades of constructionist commitment", Hacking, *The Social Construction of What?* Ch. 2.

[29] Glaser & Strauss, *The Discovery of Grounded Theory,* p. 110f, p. 115.

[30] An interesting line of inquiry would be to link such formal theory to the "continualist" perspective on agency proposed by Barry Barnes, *On Agency,* London 2001. Barnes' account is strongly inspired by Wittgenstein's anaysis of following rules by extending analogies, a topic brought into sociology by Harold Garfinkel's ethnomethodology, cf. John Heritage, *Garfinkel & Ethnomethodology,* Cambridge 1984. The relationship between the interactionist school and the ethnomethodologists has — strangely enough — been marked by polarization, but can be clarified as follows: The idea of grounded theory has been developed as a reflection on the fieldwork methods practiced in many areas of social reseach. Ethnomethodology, on the other hand, deals with the "ethnic" methods of how everyday life is organized through our improvisational use of knowledge once we enter into new situations. Ethnomethodology provides a "bottom up" perspective on the "transcendental" features of daily life situations (and their famous "breeching experiments" show the chaos that ensue once we violate "common" assumptions of daily life interaction). But these two perspectives are clearly compatible (however much the ethnomethodologists deny this), since they are just different aspects of the continuity of concepts between the daily, everyday life and social science — the indexicality of concepts/classifications!

[31] Jon Elster, *Alchemies of the mind,* Cambridge 2000.

assist the discovery of formal grounded theory in other research fields. But explanaltions of processes/events in this field cannot be deduced from the formal grounded theory. Let us take a closer look at Ragin's discussion of the qualitative search for communalities.

In *Constructing Social Research,* Ragin suggested a threefold typology of social science procedures: covariation via quantitative methods, similarities via qualitative methods and diversity via comparative methods. In his other two books, however, he contrasts variables (quantitative) and case-oriented approaches, where the latter includes both qualitative and comparative.[32] My discussion above can clear up this dilemma, since the variables/case-distinction refers to two different explanatory approaches. Whether we search for communalities or diversity, we use a case-oriented appraoch, since as long as we establish substantial grounded theory, any claim about communalities must be assessed by means of comparison to different populations.[33] Qualitative, microsociological studies based on fieldwork are case-studies, as much as comparative typologies are typologies of cases. Micro-oriented case studies yield explanations, although at a very local level. But if one searches for similarities across fields, leaving what we called the local research frontier, one moves away from context, into non-explanatory formal grounded theory, searching for patterns across several research frontiers. As noted, these may aid explanation in specific research fields, but only if context is added.

Also in Glaser & Strauss' original formulation of grounded theory, the qualitative and the comparative method go together: in case-oriented methods, one explores diversity and at the same time finds homogeneity, discovering patterns within populations whose contexts are specified via comparison.[34] We can relate Ragin's two sets of distinctions to Glaser & Strauss' distinctions as follows:

Method	Explanatory approach	...investigates	...yields (in Glaser & Strauss' terms)
Qualitative	Variables-oriented	Covariation	Substantive grounded theory
Comparative	Case-oriented	Diversity	Substantive grounded theory
Qualitative	Case-oriented	Communalities	Formal grounded theory

When we look at it this way, the question arises of whether variables-oriented, quantitative and case-oriented comparative studies are equivalent as sources of substantive general theory. Answering this question, we must reach back to our discussion of practical philosophies of science. As we noted above, variables-oriented studies have mostly been discussed as a sort of social science approximation to the experimental natural science ideal. Implying a quest for the highest possible level of explanation, this philosophy of science led variables-oriented research to see the decontextualized nature of their empirical material as a big advantage. The parallell criticism in both Ragin and Abbott — a line of criticism started by H. Blumer — does

[32] Ragin, *Constructing Social Research,* compare Ragin, *Fuzzy-set Social Science,* and Ragin, *The Comparative Method,* Berkeley 1986.

[33] It must thus be doubted whether the saturation procedure often suggested for generalization in qualitative studies is enough.

[34] Thus, while Ragin, *Constructing Social Research,* Ch. 4, tries to present analytical induction (the search for formal similarities) and theoretical sampling (choice of new cases in order to establish the scope of the grounded theory) as two varieties of qualitative analysis, Glaser & Strauss contrasts the two, emphasizing the merits of theoretical sampling when used in conjunction with the method of constant comparison.

not imply that all kinds of variables-oriented studies should be avoided, they rather suggest that variables-oriented methods should be based on careful contextualization, which may be achieved through methods of clustering in conjunction with qualitative mapping of diversity — in the light of the specific research questions asked.[35] In this way, a qualitative assessment seems to be a condition if variables-oriented studies are to be successful as substantial grounded theory (and as noted, the reference to "lean econometrics" indicates such a trend among econometrians).

As I have described in detail, comparative macro-oriented social science clearly proceeds according to the principles of grounded theory.[36] Also at that level, it is impossible to distinguish qualitative from comparative analysis. Such studies often go far in the direction of establishing interpretations of the present, but they do it in a way different from grand theories of modernity. The latter namely, aim to establish such interpretations by a "top-down" process starting from transcendental theories of structure and action.[37] The pragmatist approach is bottom up, so it creates partial analyses of the present by combining a limited number of local research frontiers. (This is an alternative to non-comparative search for similarities.) For instance, Esping-Andersen's diagnosis of the present European welfare-states relies on a combination of at least three local research frontiers, focusing on family sociology, social policy studies and labour market research.[38]

Let me end this section by emphasizing two qualifications. First, the emphasis on grounded theory based on explanatory reconstructions should not be read as an expression of an eclecticism that allow us to proclaim any kind of knowledge-compilation as theory! Quite the opposite: qualitative research should be conducted in as systematic a fashion as possible.[39] As long as we impose ideals from other academic spheres — either natural science or the humanities — we run the danger of missing out on the particular experiences and methodological reflections on the craftwork that we actually do ourselves in our own small (compared to the natural sciences) division within the scientific community. There are many improvements to be made! The message here is that we can be more specific about these improvements when we reject the natural science ideal. It should also be noted that since pragmatist notions of theory reject any simple demarcation of social scientific knowledge, it relies all the more on the ethics that will always be present within the collective of researchers: clearly, we do not accept everything, but we certainly accept more than what we would if we had subscribed consistently to the natural science ideal.

A second qualification relates to what is often called "evolutionary" approaches within social science theory. Many scholars who study innovation are inspired by the programme of evolutionary economics. I see a certain tension in that programme. Its

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[35] Abbott, in *Time Matters,* often mentions that commercial research mainly uses clustering methods. Ragin's QCA (Qualitative Comparative Analysis) is a non-probablistic variables-oriented method aimed to establish maps of causal conjunctions — in the first book he usedthe term "multiple, conjunctural causation" — leading e.g. to one sismilar outcome: a revolution may occur in many ways, and the mapping yields a typology of revolutions. This typology may be of great help in studying contemporary situations of tense economic-political conflicts, but it does not allow us to predict a revolution in any determinist way.

[36] Mjøset,"An Essay on the Foundations of Comparative Historical Social Science", see also Lars Mjøset, "Stein Rokkan's thick comparisons", *Acta Sociologica,* 43:4, 2000.

[37] A. Giddens, *The Consequences of Modernity,* Cambridge 1990, is a good example.

[38] Gøsta Esping-Andersen, *The Social Foundations of Post-Industrial Economies,* Oxford 1999.

[39] This was a main motivation behind Glaser & Strauss, *The Discovery of Grounded Theory*.

more recent popularity in economics started with attempts to import modelling techniques developed within evolutionary biology (population ecology, etc) into economics. Later, the reference to path-dependency and related modelling techniques has spread also in political science and sociology (often dubbed "neo-institutionalism"). Should this approach be considered grounded theory? That depends. In another paper,[40] I discuss the interactionist synthesis (my term for grounded theory approaches), emphasising that one characteristic of this approach (as contrasted with both social philosophy and rational choice), is that it relies on no analogies that are "external" to social science. To illustrate, I see the notion of the "organism" invoked by classic social science functionalism, the notion of equilibria in economics and the notion of the system of arbitrary signs (signs with no reference) in structuralism as analogies imported from biology, mathematics and linguistics respectively. Interactionist social science relies on grounded theories only, and these theories are developed by a method of constant comparison, which can be seen as the constant extension of analogies. But these analogies are drawn only from other substantive, explanatory studies, from earlier discoveries of grounded theory (cf. the discussion of the specificitie sof formal grounded theory above). New cases are not subsumed under these results as general rules (we do not predict!), but they are related by means of contrasts and similarities (we compare earlier and recent reconstructions of events that have occurred!).

If evolutionary economics gives full priority to a family of analogies that are extended directly from evolutionary biology, one should question whether this is grounded theory. I can certainly not give a more detailed assessment of the evolutionary-economic approach to innovation studies here. I would guess that some of these works actually discover grounded theory, that is, although often chosing formal techniques and mechanisms from evolutionary biology, their discussion is in fact sufficiently grounded in either specific case-studies or in limited regularities drawn from variables oriented studies, that we would describe them as grounded theory. However, there are surely also cases in which the evolutionary analogies live their own life, whereby the researcher runs the danger of discarding important elements of his or her empirical cases in order to apply a more rigid biological analogy.

4. The study of innovation

The notion of national systems of innovation emerged from the joint work of C. Freeman and the Aalborg group starting in the 1970s. The latter group originally did work on alternative economic policies, industrial policies in particular. Early on, they analysed OECD-data on the export-specialization of Western European countries. From this descriptive, variables-oriented statistical exercise they — among other findings — were stuck by the surprisingly high export-specialization of Denmark in dairy machinery. Further qualitative, historical studies led them to see this as resulting from user/producer action in villages that were hubs in the railway network that crossed the Danish farmland. This led on to several case-studies of innovation as a result of user/producer interaction. This could also — in more recent terms be called social capital — networks as resources: the continuous incorporation of technological improvements on farming machinery due to feed back from users).

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[40] Mjøset, "An Essay on the Foundations of Comparative Historical Social Science".

Accumulation of knowledge within the innovation-research frontier would lead to substantive grounded theory: findings from øimited sets of Danish case-studies might be related to findings from other Danish cases, or from cases in other countriess. But it also led to a focus on the sources of the knowledge embedded in user/producer-networks. The notion of "systems of innovation" can be seen in this light: in some regions specific regional networks or institutions could be discerned but also a number of national institutions played a role: the cooperative movement, the educational system, and — as industrialization proceeded — industrial relations regulations, institutions of r&d-support. From such studies, there was a further development towards an overall "interpretation of the present", with "the learning economy" as the main term.[41] In Lundvall's work this has been consolidated by confronting the theory of the learning economy — definitely an interactionist synthesis — with e.g. transaction cost economics approaches — a rational choice synthesis — to the study of innovation.

Although there are now several edited volumes reporting research in the wake of — and parallell to — the Aalborg group, it seems to me that we still miss a systematic summary of typologies mapping many systems of innovation along various dimensions.

5. The Study of National Systems of Innovation

This section is preliminary and contains some notes on the study of the national setting for innovation.

It was suggested in a footnote above that we could relate to Hacking's six grades of constructionist commitment. Starting broadly with this research on varieties of capitalism followed from a "revolutionary" commitment (Hacking's sixth grade, or more moderately: his fourth and fifth grade) of the late 1960s student movement. But most of those student revolters who continued in social research certainly soon committed themselves in other ways. The simplest thing to do was certainly to emphasize the relative autonomy of knowledge accumulation in the subsystem of science. The focus would then be on the historical nature of our present state of affairs (Hacking's first grade), sometimes with muted engagement in the form of the ironical twist (Hacking's second grade) so typical of cultural criticism from Weber and onwards.[42] When the first politisized surge of poliical economy faded, the focus on varieties was strengthened, quite often in conjunction with a *reformist* commitment (Hacking's third grade). Comparative studies of national systems of innovation, of welfare states or of economic policy adjustments was drawn towards studied of success-cases and — less frequently — failure-cases. The focus was directed towards sub-frontiers of research and towards the variety of cases: in the reformist perspective, the various "national" (or "regional") systems were evaluated and in some cases resulted in learning or experiments in reforms inspired by cases judged to give better performance (consider the role of such valued terms as "competitiveness", success, etc.)

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[41] Lundvall on paradigmatic cases.

[42] The focus on diversity here took the form of an insistence on the exceptiionalism of the author's native country: any national commentator would find his or her own country exceptional in some important respect.

The rationale here was not just academic, it was to enable policy makers and other collective actors to learn from comparable cases and from middle-range generalizations. Success can be defined in many respects, e.g. in political terms or in more management-oriented terms: in the late 1970s, comparative projects involving Sweden were strongly influenced by conceptions within the anglo-american left that Sweden was the incarnation of, an equality-oriented third way between Western capitalism and Eastern communism.[43] For a long time, research into national systems of innovation used Japan as a master case.[44]

The study of success cases, whether regions or national systems, is a tempting business. The procedure in such studies often goes in three steps: (1) case reconstruction, (2) stylize the case as the expression of a strategy ("Swedish model", "Japanese model", and most recently the "Irish model"), (3) promote this strategy as policy advice.

Based on the reconstruction of specific cases, the claim is made that under present conditions of globalization, it is best to follow the lead of (a) particular success case(s), that is, one moves from case-reconstruction to interpretations of the present, i.e. give policy advice based on a generalization of success stories

Too often, remnances of an experimental and thus predictive methodology haunt such studies. Had this been a fruitful way for social science, we could have done at the scientific level what the business journalists do: i.e. conclude that other countries should really learn from success cases. If predictions had been possible, policy advice could be based on general principles and would not need to worry too much about context.

The problem is that through the three steps noted above, a lot of context is lost. Thus, such exercises yield "predictions" in the sense of making claims as to the effects of policies: if you want to catch up, do liek Ireland! "Prediction" here is area specialists telling decision makers what the effects of their policies way be: emulate Ireland, i.e. erect the same pattern of intermediary institutions, then you would get a flourishing software-sector, several spin-offs, and join the group of semi-peripheral catching up countries.

To the extent that policy advice is taken seriously by other countries, the effects are hard to predict, since the context is bound to differ. We should resist the fascination of having picked the case that for the moment plays the role as the paradigm of success. We should not submit to the temptation of providing master examples at the nation state level, parallell to all the shifting master paradigms we know from the field of business administration.

The problem is that the conventional idea of theory as the collection of the "so far" most general principles found ("lawlike regularities") is that it supports the above mentioned misguided idea of policy advice based on a predictive notion of theory. The idea of "generalizing success stories" leads to decontextualiztaion.

If too many researchers move directly on to policy advice, the research community looses the potential of systematically accumulating knowledge. Success cases all fade!

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[43] Cf. e.g. the early work of Esping-Andersen, John Stephens and Andrew Martin.

[44] One of the earliest volumes which promoted that notion was Christopher Freeman, *Technology policy and economic performance: lessons from Japan*, London 1987.

The 1990s is now regarded as Japan's lost decade! It is important to study more than just success-cases and to study the transformations in and out of success status. Such diversity is best studied in macro-oriented social science via typological maps, based on comparisons of cases along specified dimensions.

We need case-classifications with due regard to the world economic period through which the various models were successes and failures. Applying the principles of grounded theory, one would here only generalize with reference to thoroughly analysed cases, aided by the principle of "constant comparisons" between an increasing number of cases reconstructed in monographs and discussed in more synthetic works.

We must also differentiate between "model" as promoted by collective actors and "models" as stylized by researchers! Certainly these are interlinked: some actors borrow ideas from researchers, but certainly researchers also start to investigate a term that has already been promoted by activists. This is a nice example of the problem of demarcating a clear border between scientific and other knowledge in the social sciences. Examples are the discussion on the Scandinavian model, on the German model, and so on.

It is unfortunate if research on innovation systems — or the broader study of varities of capitalism — remain just a string of relatively unconnected studies of success or failure cases. It would be even worse if these studies linked up with a certain genre within middle range theorizing that I will call "bringing a factor back in"-approach. The most famous recent case is the research litterature on "bringing the state back in", which is of some relevance for the study of innovation systems. In this approach, case-studies are conducted and the impact of one set of factors (those related to the state) are given priority. Thus, groups of case-studies (some even comparative, involving several states or sectors) claim to make general theoretical points by critisizing earlier approaches — e.g. such that emphasized class conflict and other civil society factors — disregarding their chosen factors. This confrontation allows these researchers to retain their commitment to a high-powered notion of theory on the natural science ideal. But if our research problem relates to some (set of) indicator(s) of national performance, then we should really search for causal conjunctures that combine factors from several dimensions (the notion of a "national model" indicates this). A fight between various groups of factors becomes "excess falsificationism" and leads to a circle in which ever new cohorts of researchers try to generalize from a set of factors that the earlier cohort disregarded. Such a vicious circle would never allow us to work towards typological maps, and such maps are really lacking in this kind of research.[45] But typological maps are the only way in which we can really accumulate knowledge from the shifting foci on success and failure cases. We need to analyse cases even when they loose status as successes or failures.

Weber's typologies live longer than grand theories, in twenty years nobody will care about Giddens theory of structuration, but they will still read Webers typology of action, power, etc. with nodding acceptance. If they — as researchers — do not agree

[45] In the varieties of capitalism literature, there is a related approach, cf. Peter Hall & David Soskice, «An Introduction to Varieties of Capitalism», in Peter A. Hall & David Soskice, eds, *Varieties of Capitalism,* Oxford 2001, which gives us no map of capitalism's varieties, but instead strives to generalize from two cases (Germany and the U.S.) by means of game theory. The approach also includes another turn on the "bringing factors back in"-wheel, as they claim to bring firms back in. I will comment on this approach in another paper, yet to be completed.

with the typological relations, then they will try to improve them. Typologies are revised, not falsified, they are relative to reserach questions, they are not absolute, they too are theory, in the sense of being "accumulated knowledge, organized by the human mind for purposes of explanation".

Let me briefly touch upon one concrete example. A promising departure from the "bringing the state back in"-tradition is Peter Evans' studies. He starts from a dualism between rich, industrialized states and developmental states,[46] then he distinguishes between developmental states and predatory states, implying — potentially — a typology across the continuum between these two types.[47]

However, in a recent study, S. O'Riain claims that Evans has a theory of the bureaucratic development state (derived above all from South Korea as a success case), and that this theory, apart from having problems with Evans' own intermediate cases, India and Brazil, certainly cannot account for the development of Ireland and Israel.[48] the latter are, possibly with Taiwan, rather cases of "flexible development states". Rather than considering what factors (dimensions) that might be considered in order to develop a typology of "developmental states", case-study is pitted against case-study, pesumably to win out in a contest for the most general theory.

But certainly, O'Riain derives no more general theory than Evans, and at the same time he discourages cumulative efforts towards a typology. In fact, what O'Riain's very interesting study does, is to explain the particularities of the Irish case through what in grounded theory is known as theoretical sampling, the use of selected acses that iluminate his primary case. Comparing Ireland to Israel and Taiwan, he concludes: "Each country has developed a net of what I have called here 'flexible' institutions — a network of state agencies and associations which have supported, guided and promoted local networks of innovation. In contrast to the Irish case, however, they have buildt a system of innovation which is dominated by local firms — not foreign TNCs"[49] We here see the contours of Ireland as a specific conjuncture: with a Weberian, accountable bureuacracy, able to catch up even with a strong dominance of TNCs. Much more than a vague explanatory competition between notions of flexible and bureaucratic development states, the specific nature of the Irish case comes out as important.

Evans' "theory" of the bureaucratic development state is not "wrong" or "falsified" in the conventional philosophy of science understanding of the word. This may only be so if we generalize over distinct periods. As so many other contributions in this field of comparative macro-sociology, Evans has mainly reconstructed a number of cases through certain periods. Labelling him a "BDS theorist" is not appropriate.

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[46] Peter B. Evans, *Embedded Autonomy,* Princeton 1995, Peter B. Evans & J. Rauch, «Bureaucracy and growth: a cross-national analysis of the effects of 'Weberian' state states structures on economic growth», *American Sociological Review,* 64, 1999, pp. 748-65.

[47] Evans' contribution, however, is slightly US-centered, since he does not note that twenty years ago, there was a German contribution towards such a typology, cf. Dieter Senghaas, *The European Experience,* Leamington Spa 1985. This is the English report of a reserach progrect that led to a whole pile of reports in German, for an overview see Lars Mjøset, «Comparative Typologies of Development Patterns: the Menzel/Senghaas Framework», in Lars Mjøset, editor, *Contributions to the Comparative Study of Development,* Oslo: Institute for Social Research, Report 92: 2, 1992, pp. 96-162.

[48] Sean O'Riain, *Flexible development,* (forthcoming)

[49] Sean O'Riain, *Flexible development,* ms, p. 285.

It is not possible to transcend a "theory" based on one set of cases by counterposing a "theory" based on another set of cases. What we can challenge is Evans' explanation of e.g. the South Korean case. Evans could defend his explanation, but even if he failed to do it, he could claim that an explanation developed as an analogy with South Korea will not work with reference to the Irish case. A discussion here could lead towards convergence in the form of revised and extended case maps.

The relevant summary of such studies is not a celebration of the "winning" theoretical abstraction, but rather in the form of a mapping of cases, including considerations of important mechanisms that recur in groups of cases. These mappings, furthermore, must always be considered in the light of a broader periodization of the world economic period during which they prevailed.

The global context of national systems of innovation

We shall here focus on C. Perez' sequencing of the development of industial capitalism with reference to big bangs in techological change and shifting techno-economic paradigms.[50] On to this neo-schumpeterian foundation, Perez adds a discussion of financial capital, comparing four stylized phases of the roughly 50 year time span between two such big bangs (e.g. between cotton textiles and railways, or between the T-Ford and the semiconductor).[51]

In the first phase the revolutionary technology is promising and its development is aided by a lot of venture capital searching for new high returns in a phase when the old technology has matured. As the potentials of the new technology become visible, financial capital dissociates itself from productive capital in a frenzy in which the paper value of the new activities explodes (the dot-com-mania is the latest case). When this bubble bursts, new regulations are introduced and for some time financial and productive capital are again interacting in a coherent way, producing a "golden age" which really generalizes the new technology, something which in turn leads into the saturated situation where financial capital no longer has much to gain from the old technology but is eager to finance any promising small venture (searching everywhere for the new Bill Gates) with promises for the future (today: nano-technology?)

Working in the neochumpeterian tradition, Perez' work above all provides the macrocontext to the study of national innovation systems,[52] but there are more general lessons to draw from her study. The book clearly represents grounded theory at the macro level. Her empirical material is drawn from economic history, business history and the history of technology. Her cases are not nations, but the periods defined by the four phase sequence just summarized. The typology mainly takes the form of a periodization, although certain national characteristics and institutional patterns are

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[50] The study of sequences was always very important in the interactionist tradition, from the national histories (of youth criminals, of revolutions, and so on) to A. Abbott's recent work.

[51] Carlota Perez, *Technological Revolutions and Financial Capital,* Cheltenham 2002, cf. also Christopher Freeman & Fransisco Louçà, *As time goes by,* Oxford 2001.

[52] In her earlier work, Perez tried to discuss directly the relationship between this macro-context and third world evelopment, without considering national systems of innovation as an intermediate link. Carlota Perez, "Microelectronics, Long Waves and World Structural Change: New Perspectives for Developing Countries", *World Development,* 13:3, 1985; Carlota Perez, "Structural change and assimilation of new technologies in the economic and social systems", *Futures,* 15:5, 1983, 357-375

included in the analysis, since one of the hegemonic state's assets is to be ahead in technological development, while its challengers tries to catch up and forge ahead (as the U.S. and Germany did vis-a-vis Britain in the late 19th century). Perez also discuss how elements of the national innovation system of a hegemonic state are diffused outside its borders.

Thus, methodologically, Perez' work combines typology with mechanisms: each period defines the context for the mechanisms (unity or split between financial and productive capital). Such kind of grounded theory ends in a forceful interpretation of the present, not based on predictions or generalizations, but on what we can learn form a sequential reconstruction of the long-term development pattern of industrial capitalism.[53] No external analogies are applied, all comparisons are conducted the "interactionist" way, one historical situation against the other.

Perez' perspective is also — however wide — a onesided one. This points exactly to the difference between a social philosophy synthesis and an interactionist one. An interpretation of the present on interactionist grounds is always relative to the kind of questions asked. Perez asks how the interplay between productive and financial capital interacts with the process of innovation and diffucsion of basic technologies to create great surges in the history of industrial capitalism. One might take the sequence as such an try to formalize it as a "formal grounded theory" (in the restricted sense of being similar across several historical periods, but it is not presented as a sequence that are recognized across various research fields). Perez does actually use it to hint at possible future developments.[54]

But Perez is also careful to emphasize that contextual factors may influence the form of the cycle. For decision makers relating to the great surges from the point of view of nation states, it is even more important to reflect on context, since national economies have related to the surges in different ways, as we know, there are both failure and success cases.

It will also be possible to use Perez' seqeuncing (or parts of it) in conjunction with other stylized patterns in the international relations literature. From Perez' own discussion, we can conclude that in various respects, the following dimensions could be more closely related to her analysis: certain secular trends as emphasized by world systems theory, war cycles, the various (partly technological) revolutions in military affairs,[55] environmental effects,[56] impact of international organizations vis a vis state systems,[57] evolution of nation state,[58] deepening of capitalism in terms of impact on peopless life,[59] increasing impact of intangibles (knowledge),[60] incorporation of the

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53 Perez' sequence scheme allows us to classify important contributions in international political economy to various phases and aspects, for instance the work of Susan Strange on casino capitalism would related specifically to the bubble phase, Chandler's famous studies relates mainly to the fourth big surge, Castells' contribution mainly to the fifth surge. Freeman mainly focuses on technological revolutions, Veblen (p. 50) on the rhythms of economic life, but in an institutionalist perspective. Marx portrays the unrest of the transition from the second to the third surge (p. 51).

54 Perez, *Technological Revolutions and Financial Capital,* p. 11 (a new big bang?), p. 13 (the technologies of the sixth great surge), p. 5, p. 43 (the new upturn).

55 See Perez, *Technological Revolutions and Financial Capital,* p. 28.

56 See Perez, *Technological Revolutions and Financial Capital,* p. 55, p. 44 (overdevelopment).

57 See Perez, *Technological Revolutions and Financial Capital,* p. 24.

58 See Perez, *Technological Revolutions and Financial Capital,* p. 25.

59 See Perez, *Technological Revolutions and Financial Capital,* p. 20.

lower classes,[61] the transition from an epoch of family firms to organized capitalism based on corporations,[62] social mobility patterns.[63] (p. 55)

Conclusions

Our overview has shown that the pragmatist notion of theory is the one that fits closely with what social scientists actually do. In this notion of theory, knowledge is accumuated in a variety of specific ways:

— typological maps allow us to specify conditions and are related to the definition of populations (scope conditions)
— sequencing gives mechanisms. analytic induction from sequences and patterns provides mechanisms of interaction
— case-reconstructions, even just one, may serve as analogy towards new cases
— case-experiences may serve as sources of learning
— case-experiences may give rise to early warning systems[64]

There is a "third way" inbetween the natural sciences and teh humanities. Its philosophy of science basis can be worked out in a coherent way with due regard to the *specificity* of knowledge-accumulation in the social sciences, free of "external ideals". This would be useful also beacuse it would help us avoid unneccesary infighting — between researchers at the various subfrontiers — and it would help us to work together, trying to work upwards from the various subfrontiers towards a more comprehensive mapping of the varieties of capitalism.

In that respect, it strikes me that we should no longer be entirely satisfied with the many edited volumes that we are putting out. This is of course the typical output of research projects funded by various research councils or by other sources. As such it is better than the non-public reports produced by consultancy firms, and also better than xeroxed or web-based working papers that fill up the archives or servers of the research councils that financed them. But there are even better options: considering the various local research frontiers that are of relevance to the broader research frontier on varieties of capitalism, it is only within one of them, comparative political systems, that we find really extensive typological maps, namely those of Rokkan. We need more ambitious book projects which aim to synthesize the knowledge we have on many cases. If this cannot be done by one person (Rokkan's own work was unfinished, some would say he died from his hard work to complete it) or one research group, it can possibly be done

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[60] See Perez, *Technological Revolutions and Financial Capital,* p. 25.
[61] See Perez, *Technological Revolutions and Financial Capital,* p. 46.
[62] See Perez, *Technological Revolutions and Financial Capital,* p. 4.
[63] See Perez, *Technological Revolutions and Financial Capital,* p. 55.
[64] It is tempting to mention the forthcoming study — although it has nothing to do with the study of national innovation systems — by Michael Mann on genocides and ethnic cleansing in the 20th century. The general lesson he draws is a cluster of early warning signals: this knowledge is not predictive, and this knowledge should be (and is) acted upon by NGOs and other international actors. Again this is an example of the critical nature of social science knowledge accumulatin (theory): one should resist by all means that this knowledge becomes predictive of new cases, one should support interventions to make it not happen again!

as some kind of an encyclopedia.[65] So maybe ambitious researchers at the various subfronters should start to think about *The Encyclopädia of capitalism's varities.* As for innovation studies, I know that there is a handbook forthcoming, but we also need an *Encyclopädia of innovation systems!*

[65] In historical sociology, one of the research frontiers deals with revolutions, and here an encyclopedia has been published: Jack Goldstone, *Encyclopedia of revolutions,* Washington DC 1998.