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The study of Nordic varieties of capitalism. A plea for contextual generalization through comparative specification.

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The political economy of the Nordic countries became a topic in academic social science in the wake of the 1960s student revolt. In this note, I give a methodological evaluation of this literature. I start from an insight arrived at in my study (Mjøset 2005a, Mjøset 2006abc) of what social scientists mean by the term theory. I argue that high-level notions of theory are not well suited for accumulation of empirical knowledge.

Student revolters and shifting cultural problems

Towards the end of the turbulent 1960s, the new infrastructure of higher education in the social sciences was filled with students who knew all the answers and whose attacks on the small establishment of university teachers were based on an overpoliticianized sociology of knowledge. Whatever the problems with this leftist heroism, these groups reoriented the social sciences towards a new, macro-oriented interdisciplinarity: political economy, class analysis, critical theory linked to social movements, social and economic history, etc.

The student revolt faded as the first cohort of revolters entered the labour market, taking up jobs in the public or private service sectors. Some of those who moved into academic positions maintained the political economy focus, but soon adapted the program to disciplinary specializations and diverging methodologies. Through the next decades, the social, political and cultural problems that preoccupied them changed. One could write the social history of those shifts: every generation of young, aspiring intellectuals strives to coin its own interpretations of the present, often with more or less explicit links to secular philosophies of history. Here, I only have space to provide some quick labels (cf. Mjøset 2006a).

In the 1970s, the heroic idea of a revolutionary break with the socio-economic structure of capitalism was replaced by the problem of reform: Could social democracy master capitalism without altering the relations of production? The older social-democratic idea of a “third way” between socialism and capitalism received new support. As the world economy slid into its first real downturn after the Golden Age, Nordic scholars discussed how the egalitarian social democratic management of mixed economies with strong welfare states could smooth out the imperatives of capitalist restructuring. Modern research on the Nordic model was born.

Since the mid-1980s, the changes in monetary integration and the busting of financial bubbles all over the Western world (also in social democratic strongholds as Sweden and Norway) triggered a stronger focus on national and international financial sectors.

In the 1990s, the focus was first influenced by the surge of European unification efforts, then it turned more to the firms and their efforts to sustain high-wage/strong welfare constellations (by innovating, restructuring) in the emerging globalized, service-/knowledge-economy.

While globalization was mainly treated as a Western world phenomenon in the 1990s, the focus in the present has included the emerging new Asian economies (creating new raw materials booms for the Nordic area), as well as the challenges implied by the increasingly global flows of labour.

As the problems shifted, different dimensions of the state/economy-interface attracted attention: incomes policies related to wage negotiations in the 1970s, the destabilizing effects of financial deregulation in the 1980s, corporate governance in the 1990s.

While the political economists also dealt with international structures and processes, and studied other cases, there was still a tendency, perhaps driven by the policy making
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community’s demand for practical alternatives, to focus on the success cases. Sweden was a success case of the 1960s (Rehn/Meidner macro-economic model), as was Japan. Following the financial turmoil of the late 1980s, Denmark (early “flexicurity”) played the success case role for some time. In the most recent era of globalization, Finland and Ireland (national innovation systems) count as Western European success cases.

This brief “sociology of political economic knowledge” omits many nuances but it allows us to understand how new generations of scholars were driven by the shifting conceptions of “major challenges” or “dominant problems” (akin to Weber’s notion of cultural problems), and by a focus on certain success cases.

At the same time, scholars were in career tracks that obliged them to relate to the prevailing disciplinary methodologies. These were often out of tune with both the case- and the problem-focus! They were in one way or another committed to high level notions of theory! Because of this, the danger of excess falsificationism loomed large. To the extent that researchers accept high level theory ideals, they are tempted to falsify the “theory” pursued by earlier generations, while ignoring both shifts in cultural problems and paradigmatic success cases. Thus, one could find a 1980s student of financial deregulation criticizing the theoretical approach of a 1970s student of incomes policies, only to be criticized later by a 1990s student of corporate governance.

Any political economist would agree that incomes policies, financial deregulation and corporate governance are dimensions that should all be a part of a broad study of political economy. But ideals of high level theorizing made each new generation confront earlier generations, turning the general theory of their dimension against earlier generations’ general theories, their recent success cases against failing success cases, regardless of the fact that those earlier theories were rooted in the study of other dimensions/periods/cases. While interdisciplinary political economy research certainly has expanded since the 1970s, methodological conventions have barred synthetic works. The returns in terms of accumulated knowledge have been lower than they could have been. Furthermore, while much of this work did relate to historical research, productive interdisciplinary interchange was lacking.

Such a critical assessment – painted in a rather stark contrast in a short note as this one – should not be read as a plea for unfettered eclecticism. There have been works that had not submitted to excessive falsificationism. In order to highlight their underlying principles, let us consider the variety of theory ideals in the social sciences.

Strategies of generalization and specification

High level theory denotes general theory, one that is “not sensitive to context” and thus “applicable to societal contexts widely separated over both time and space” (Goldthorpe 2000: 62, for critical comments see Mjøset 2005a: 387-401). But there are three quite different ways to strive for such theory. We shall specify three strategies of generalization that yield three different types of high level theory aspirations. The first one is based on empirical material. The segmenting strategy of generalization relies on large-scale data sets, using statistical inference (as in a regression equation, e.g.) to arrive at theories conceived as relations between variables. Within a statistical margin of error one finds general empirical patterns, but those relate only to narrow segments of society for the time(s) data are collected. Such studies are well known in economics and sociology, and pursued, for instance, in statistical bureaus that assist the state in keeping track of social and macroeconomic trends.

The 1960s/70s political economists brought up some novel variables (profit rates, classes, and the like) and even some amount of cross-national focus (e.g. Erik Olin Wright’s comparative project on class structures). But most of these scholars remained “methodological nationalists”. While their studies passed through a phase in which several scholars believed that new sophisticated statistical models could turn correlations into causes, scholars now agree that, at the most, statistical studies represent powerful means of description. But theoretical explanation must uncover the “mechanisms”, i.e. the patterns of micro-behaviour and systems of unintended consequences constituting the behaviour of the “variables.”

This argument is related to the second notion of high level theory. Several scholars of the 1960s/70s political economy wave aligned their theorizing with the updated versions of the neoclassical economics they had earlier opposed. Modern rational choice theory has been general from the outset; its style of reasoning is the thought experiment, general without being empirical, reflecting an insulating strategy of generalization. The argument is phrased in terms of...
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a model world, which allows exact, impeccable deductions, given the assumptions. The challenge, however, is the relationship between assumptions and the real world, and the string of auxiliary hypotheses needed to bridge between the two. This “protective belt” – the Lakatosian term for such hypotheses – loosens up the relation of deduction: several theories may account equally well for the same data/empirical observations.

While none of these versions of high level theory advocate a reduction of social science to natural science, they do commit social science to styles of reasoning that are well known and widely used also in the natural sciences. But for those social scientists who are sceptical of the convergence with natural science styles, a third kind of high level theory also exists in the social sciences. Its style of reasoning is parallel to that of the humanities.

Many of the 1960s/70s political economists were historically oriented. The academic discipline of history – put very briefly – represents a latent criticism of any high level notion of theory, but this criticism mostly implies a commitment to the exceptionalist strategy of specification. Space does not allow a more detailed discussion (see Mjøset 2006b), except for a brief claim that lately historians have been tempted by either one of the two most recent social science notions of theory: reconstructionist or deconstructionist notions (Mjøset 2006a). These social-scientific arguments are close to the humanities’ style of interpreting texts and other artifacts. But like any others, these notions of theory influence the strategies of generalization pursued by empirical researchers who are inspired by them.

The reconstructionists start with an investigation of the transcendental conditions of (social scientific) knowledge. The 1960s student movement set out to reconstruct Marx. This procedure was later extended to the sociological classics in general, with the aim of recovering a non-positivist theory of action (Mjøset 2006a). From this high theoretical position, empirical trends are approached “from above,” as a study of modernity writ-large. Often, the classics are plundered for their most cultural-critical statements concerning the existential challenges of the modern world. This is the third strategy of generalization that I want to point out: social scientists working with historical material generalize by reference to modernity.

The reconstructionist notion was soon followed by a deconstructionist notion, drawing on other roots of European humanities (linguistics, rhetoric, structuralism), and remaining altogether sceptical of the idea of transcendental foundations (Mjøset 2006c). The only general view here was the view that there is no general knowledge. The main theoretical reference among social scientists was Foucault’s view of knowledge as an expression of the desire to dominate. Regardless of their impact in the humanities, in social science these philosophical ideas have led to a focus on texts, leading on to empirical work such as discourse analysis, conceptual history, and mentality history. But as methods of empirical research discourse analysis and related techniques face the challenge of generalization. Here, deconstructionists and reconstructionists converge on the third strategy of generalization: with reference to a loose periodization (e.g. some qualification – “post” or “late” – modernity), they make existential statements about our present predicament, and/or they decide explanatory contests in favour of cultural factors.

In the interface between history and social science, the philosophical influence of re-/deconstructionist notions has led to a preoccupation with national identity, a sort of social psychology writ large. This fits the academic discipline of history, which in all European states had broken with their past as the organic intellectuals of nation-state building, emphasizing social, cultural, regional and international history more strongly. Deconstructivism allowed a detached and reflected view on national identity. Modernity indicated a shift of attention from national concerns to the concerns of Western culture as such. Furthermore, new interdisciplinary ventures, such as cultural, media and science/technology studies, became as popular among 1990s students as political economy had been among 1970s students. Back then, social science had influenced the humanities, now the major direction of influence goes in the opposite way.

This cultural change led to less interest in political economy, but it also influenced those who still chose to do political economy: use of deconstructionist terms and modernity-based periodization indicates that the two humanities-oriented notions of theory caught the interest of younger scholars. As so often before, debates on social science methodology became a clash between two sets of principles originating outside of social science itself! The old polarization between researchers’ affiliations with natural sciences or the humanities reasserted itself.

High level notions of theory are divisive. They serve to sustain individualist research styles and seduce researchers to engage in overly philosophical debates. 2 This is unfor-
tunate since high level notions of theory are not the only ones in social science.

Controversies ensued among the different versions of high level theory. Debates on action theory emerged between rational choice scholars preferring idealization and the social philosophers with their reconstructionist (transcendental) notion. The segmenting strategy of generalization is an empirical one, but leads to a fragmentation of social science into fields covered in different data-sets. This strategy hardly yields an analysis which is general in terms of covering social development as the outcome of the interplay between processes in various fields or sectors.

Any branch of social science engages in philosophical discussions and many of them are interesting. But I worry about the effects of exclusively high-level debates on empirical research. Scholars who remain at this level will not get the most out of their empirical material. They are often led towards empirical specifications that make it difficult for them to provide real explanations as answers to their research problems. They risk getting trapped in a vicious circle between weak explanations and a focus on (overly) general, existential research questions.

Within the philosophy of the social sciences, we have seen debates and reflections on both variables-oriented and idealizing notions of theory. In both cases, a notion of theory as mechanisms (Hedstrøm/Swedberg 1998) has emerged, one which is not easily related to the high level. Such a notion was already launched by Merton in the late 1940s, but since at that time mainstream notions (canonizing some fusion of the segmenting and insulating strategies of generalization) were completely dominant, Merton defensively presented his notion of middle-range theory as a temporary solution, to be replaced by high level theory when social science matured. Other scholars – primarily in the tradition of Chicago school sociology – launched more offensive notions at the middle range, especially grounded theory (Glaser/Strauss 1967), converging methodologically with European critical notions (Mjøset 2005a). For our purposes, the critical notion is less important (although it is clearly relevant to our discussion about changing cultural problems over the decades), so we shall focus on what we prefer to label explanation-based theory, of which grounded theory is one variety.

The third alternative

The explanation-based and critical notions of theory are the expression of a pragmatist attitude in social science (for the notion of researcher attitudes, see Mjøset 2005a: 380-1). Such a notion refers solely to the activity of doing social science, abandoning any parallels to either natural science or humanities styles of reasoning. This attitude can be found also in classical political economy, but it led a rather marginalized life in early postwar methodological thinking. The diversification of understandings of social research since the 1960s, and particularly the 1960s student revolt, brought it back on the agenda. But as I have tried to argue, high level notions of theory continued to dominate in political economy, and heroic methodologists have been quick to discard the third alternative as descriptive. In contrast, I would like to assert its analytic importance.

I will use as an example a Nordic project in which I myself participated (Mjøset 1986, 1987). This project is about distinguishing five Nordic “models”, specified as regimes of economic policy-making, and analysed with reference to internal and external pressures (dynamic forces) and economic/historical/institutional legacies (i.e. from dominant export sectors to an institutional framework grounded in the Polanyian defence of land, labour, and capital as “factors of production”). In this project we found that the relatively stable constellation of routines in the postwar Golden Age gave way to phases of “fumbling” as new routines were tried out since the 1970s. The project investigated whether these revised routines constituted a new coherent regime.

The study was not committed to any high level notions of theory. There was no variables-oriented statistical inference, just plain descriptive statistics. There was no rational choice modelling, just a quite loose set of mechanisms in connection with economic policy routines and techno-economic linkages. Neither was there much of a social-philosophical perspective: There were no references to modernity, rather to five specific national trajectories, but specified along the same dimensions (e.g. dominant export sectors, nature of class compromise, structure of parliamentary system).

The study relied heavily on historical works, but it did not pursue an exceptionalist strategy of specification. The five models were five “exceptions”, that is varieties of a stylized definition of “the Nordic model”. Thus, the notion of a Nordic model could be left behind. If we “deconstructed”
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capitalism. Our five Nordic models went some way towards an account of Nordic varieties of capitalism. But the book that launched this research program (Hall/Soskice 2001) illustrates some of the problems with high level theory: it gives us only two varieties (liberal versus coordinated market economics, i.e. CME/LME dualism), deducing these in a way which is committed to the idealizing notion of theory, and the various chapters of the book too often engage in excess falsification.

In contrast to such an approach, our claim is that accumulation of knowledge on varieties of capitalism can best be achieved by pursuing a program of grounded theory. Rather than game-theoretical dualisms, one would map variations of capitalism in Rokkan-style typological maps (Rokkan 1999, Mjøset 2000). This would be a regrounding of what the Hall/Soskice volume tells us about U.S. and German capitalisms, placed into a larger matrix that would systematically recover the rich case material from the growing literature on political economy. Such a regrounding would bring out the full potential of the notion of institutional complementarities, potential that are restrained by the CME/LME dualism.

These complementarities can be traced as recurrent combinations of specific types along the various dimensions already studied by many political economists: financial systems, monetary arrangements, trade-patterns and institutions, welfare states, party-systems, labour relations, labour market institutions, natural resources/sectoral patterns, economic policy making, corporate governance-patterns, etc. Developing typologies based on a large matrix – which can only be produced by collective research efforts – would represent a contribution to general substantive theory (Mjøset 2006b). Such theory is general in the sense that it provides context for more specialized studies on these various dimensions of contemporary capitalist societies. Comparative typologies should be developed towards saturation in as many dimensions as possible.

Such a specification would lean on the best historical monographs but avoid “over-grounding” in historical context. Such typological maps would counteract the exclusive focus on success cases only. These maps should also be related to historical periodization, both with reference to global patterns and to relevant regional/local developments.

The conclusion is that research done with reference to high level notions of theory (be they either rational choice or social-philosophical) have proven quite weak in terms of accumulating knowledge on which we can build with future research. Works that rely (consciously or without much reflection on theory and method) on lower level notions of theory (e.g. explanation-based approaches such as grounded theory) have more successfully contributed to the local research frontiers relevant to the study of capitalism’s varieties. Further work on the Nordic countries should take this into consideration and work towards a systematic typological and comparative approach to the variety of Nordic capitalisms, as part of a broader study of the varieties of present day capitalism.

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Endnotes

1 This paper is based on my notes for an intervention at a workshop on the “Nordic model” at the University of Birmingham in the summer of 2004. I am thankful to Mikko Kuisma and J. Magnus Ryner, organizers of the workshop, for having me write it out. Given that I was addressing a workshop of political economists, I have not been explicit about the relationship between political economy and economic sociology. As for my own work, I have never really distinguished the two (nor have I distinguished them from comparative historical sociology to any large extent), but this is probably because my work has always been at the macro level of nation states and international structures. Limitations of space prevent me from discussing how the kind of macro-studies reported here can be related to more micro-oriented economic sociology, save for the general remark that the way Harrison White’s analysis of markets (1992, 2002) has been incorporated into this field of research (e.g. Aspers 2005) seems quite compatible with the pragmatist methodology briefly sketched in the last part of this paper.

2 C. W. Mills’ (1959) criticism of Parsons’ grand theory can be updated as a criticism of reconstructionist social philosophy, and
his criticism of abstract empiricism must be seen as one of the early attacks on the segmenting strategy of generalization. But we must go further than Mills. We have seen that the influence of high level ideals can also be traced among rational choice theorists, among social philosophers studying modernity, and even among deconstructionists. In the two latter cases, both individualism of the researchers and the philosophizing inclination is sustained via the influence of methodological understandings typical of the humanities.

3 The European roots are in the Marxist tradition and in the many social-movements-related standpoint-theories in its wake. Other European roots are in the German historical school as interpreted by Max Weber. The U.S. roots are in pragmatist philosophy and many traditions it inspired (institutional economics, social-psychological approaches to learning, Chicago-school case-oriented sociology, ethnography, etc.).

4 In order to economize on references, I here point to a number of my own papers (the reader should note that in these articles, one can find further references to the relevant literature): Mjøset 1992a (national systems of innovation approach), Mjøset 1992b (Senghaas/Hirschman-approach), Mjøset 1995 (regulation school), Mjøset 2000 (Rokkan-tradition), Mjøset 2001 (Korpi/Esping-Andersen typology of welfare states), Mjøset 2003 (comparative analysis of state formation). – In our search for shoulders to stand on, there was no priority to the student revolt generation. In fact, looking back at social research on Norden, we find the third alternative not only in the early postwar period, but even further back. To the extent these researchers betray political colour, they differ widely. What unites them is a particular style of reasoning, a specific approach to the combination of the specific and the general. While Rokkan (see Mjøset 2000) is clearly the most important postwar pioneer, others have been rediscovered after the 1986 project was completed. As for institutional economics, Johan Åkerman’s work from the 1930s to the 1950s is very important, both for its very sophisticated methodological reflection and for its analysis of the British, French, German and U.S. varieties of capitalism (see Mjøset 1994, 1997).

Furthermore, the retired Norwegian diplomat, Einar Maseng, in the 1960s wrote a masterful account of the transformations of the Nordic state system over five hundred years, see Mjøset 2003. Maseng’s reference points were Nordic self-reliance during World War 1, the interwar work to consolidate the “Norden association”, and the policy of “armed neutrality” which required that Norden was seen as a unity in both military and economic terms, cf. Mjøset 2005b.

5 For instance, regrounding might involve ignoring high theory generalities (e.g. extensive game theoretic considerations), but utilizing a range of concrete similarities and differences brought out through an investigation such as that of Milner’s (1994). Alternatively, regrounding might also imply explicit disregard of results in terms of “explained variation” of statistical exercises (a “net effects” approach to causal analysis) – sometimes employed in the Korpi/Esping-Andersen line of research on welfare states – but relating (by low-tech methods) to the descriptive statistics available in the various relevant data-sources, as suggested by Shalev (2006).

6 For a much more detailed exploration of such a methodology of comparative macro-studies – using an example from political sociology – see Mjøset 2006b.

7 A follow up to the 1986-project (Mjøset 1992a) added three new case countries (Ireland, Switzerland, Austria) to the five Nordic ones.

8 The programme of grounded theory is often criticized by the “critical realist” school within the philosophy of the social sciences, which insists on the reality of generative structures such as Marx’ “driving forces” of capitalism. This debate cannot be pursued here (but cf. the remarks on Burawoy in Mjøset 2005a). The claim that grounded theory is incompatible with a study of driving forces must be doubted. But it is plain that a grounded theory approach requires a more accurate contextualization than what has been common in earlier political economy. It would, for instance, be hard to talk about driving forces in Norwegian capitalism without specifying the role of the oil sector and the specific kind of welfare state. Furthermore, the driving forces one postulates will be partly dependent on the kind of research question asked. We can trace driving forces with reference to a model of national capitalism, and this does not exclude consideration of driving forces beyond the nation state. But these cannot be stated a priori: meticulous comparison of phases of world economic development, specificities of hegemonic states, great powers and regional state systems is necessary – also in these fields we must discover grounded theory.

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