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Social Science as a Vocation
- Are Max Weber's Warnings Still Valid?

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Fritz W. Scharpf

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Social Science as a Vocation
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European University Institute

Introduction

At the opening of the Max Weber Postdoctoral Program, I should like to congratulate the European University Institute as well as the European Commission for having created a unique and most valuable opportunity for the next generation of European leaders in the social sciences. And you, the fellows of the program, must of course be congratulated for having been chosen, on the strength of your work, from among hundreds of applicants from all over Europe and beyond. What you can expect is time to look back and to look forward — time to work out the implications, and to communicate the findings of your doctoral research, time to explore new interests for future research, and time to widen your transnational network of friends and colleagues — and last but not least, time to explore and enjoy the incomparable environment of the Villa La Fonte, of Florence, of Tuscany and of Italy in which you will be privileged to live. At the same time, the EUI will do its best to help you in preparing for an academic career in the European Union and beyond.

But this could also be an opportunity to reflect on your chosen course. A last stop in a roadside café, as it were, before entering the toll road of a career with infrequent and increasingly costly exit opportunities.

Of course you know the destinations that you cannot expect to reach on this highway: If your goal in life had been to get rich, you would have gone into investment banking. If you had wanted to become powerful, you would be struggling toward the top in big government or big business. I also think it most unlikely that you should have chosen an academic career in the social sciences as a way to get famous. But if it is neither wealth, nor power, nor fame — what could be the valid motive that drives you to choose an academic career in the social sciences?

More than 87 years ago, Max Weber, the patron saint of your program, was addressing the same question in a famous lecture under the title “*Wissenschaft als Beruf*” — which one could variously translate as science or scholarship as a vocation or as a calling — (Weber 1919/1975). His answer was similar to the counsel which I used to give to students at the crossroads between academe and career options outside: “Unless it is the only thing you can see yourself doing — don’t go into academic social science.” This was serious advice, and it was taken seriously. Some of my most brilliant doctoral students have risen to the top in government, banking or international NGOs. They could have had first-rate academic careers; their dissertations are still frequently cited, but when they look back on their years on the threshold of academe, it is with a certain nostalgia combined with a strong sense of relief: “Thank God I had the good sense and the courage to escape into a life of action.” The point is: For young women and men of your intellectual calibre, there are indeed attractive options outside of and beyond academe.

But then, there were others who were in fact wooed into well-paid and highly promising jobs in multinational firms or international organizations. Yet after doing well in these jobs, they decided to return to a post-doc stipend or to the offer of a junior professorship — which is one of the least well-paid and most insecure positions in German academe. In the light of very attractive alternatives, that is, they had come to realize that research and teaching in the social sciences was truly the only thing they really wanted to do.

But that only pushes us to a further question: What is it that could make you feel that a career in the social sciences is what you most want to do?

You are of course familiar with the downside: low salaries and insecure jobs, at least initially, probably combined with high teaching loads and the burdens of increasingly bureaucratized evaluations. You think you can take that, and you are also not deterred by the glimpses you have seen of the pleasures and frustrations of faculty self-government. Moreover, you have already experienced the joys and the pain of designing your own research project and of organizing what you found into a coherent structure of chapters and verses and, most painful of all, into consecutive and lucid sentences.

If this experience has been mostly joyful for you, you can almost be sure that you didn’t dig deep enough and didn’t work hard enough in your doctoral research, and that you didn’t pay sufficient attention to the importance of lucid communication. By contrast, if the process of producing your dissertation has been an almost unbearable pain, you may be on the right track. But then you should be assured that things will not get any easier with experience. The pain and the insecurity will remain as acute with your fifth book and your fiftieth journal article as it was with the first.

So why should you want to do that to yourself? What makes it worth your while to forego the gratifications of wealth, power and fame, and to accept the punishments associated with teaching, research and writing?

In his lecture of 1919, Max Weber gave one emphatic answer to this question: You shouldn't do it, he said, unless the fate of your soul depends on the definitive resolution of scientific puzzles, unless you are driven by the passion to achieve something that will endure for the ages (Weber 1919/1975, 11-12).

“Without this strange intoxication, ridiculed by every outsider; without this passion, this ‘thousands of years had to pass before you entered into life, and thousands more expect in silence’ — whether or not you will succeed in resolving this puzzle; without this, you have no calling for science and you should do something else” (ibid).¹

But since he was speaking to students of all disciplines, Weber also acknowledged that his answer would mean different things in different contexts. It seems fully sufficient in the natural sciences. There, objects of research are generally stable, causal relations are assumed to be invariant in time and space, and the analysis of even very complex constellations can often be simplified through the experimental isolation of individual factors. Even though, as Weber noted, work on the frontiers of modern science requires ever increasing specialization, the causal relationships so discovered can be generalized and will endure. Thus even if they may appear minute to the non-specialist, they will be building blocks for the cathedral of cumulative and secure scientific knowledge. In other words, and quite apart from any considerations of practical usefulness, all scientific discoveries will be “worth knowing”, and doing science for science's sake appears as a socially respectable and psychologically plausible motive for a lifelong commitment (ibid, 22).

In the humanities and the social sciences, by contrast, for Weber the crucial question is indeed what should be “worth knowing”. The reason lies in the ontology of our subject matter which he had spelled out fifteen years before in his essay on “Objectivity in Social Science and Social Policy” (Weber 1904/1949):

“Now, as soon as we attempt to reflect about the way in which life confronts us in immediate concrete situations, it presents an infinite multiplicity of successively and coexistently emerging and disappearing events, both “within” and “outside” ourselves. The absolute infinitude of this multiplicity is seen to remain undiminished even when our attention is focused on a single object, for instance, a concrete act of exchange, as soon as we seriously attempt an exhaustive description of all the individual components of this individual phenomena, to say nothing of explaining it causally” (ibid. 72).

Now of course, the natural world around us is complex as well. So the difference that matters in Weber's view is the absence, or in any case the practical irrelevance, of general laws in the study of social phenomena — general laws that is, representing simple but stable causal relationships which could then be combined in modular fashion to explain even very complex phenomena. In the absence of such laws, we must conclude, the social sciences are compelled to take a more holistic approach — which then forces them to confront directly the potential “infinitude” of causal factors and the “multiplicity” of interaction effects among them which, in Weber's view, are characteristic of social interactions.

¹ Translation by Geerth and Mills (1946), slightly modified.

For Weber, the need to cope directly with the overwhelming complexity of social phenomena had two fundamental methodological implications: The first is that the social sciences must necessarily be selective. Since only a finite portion of infinite reality could ever become the object of any scientific investigation, the question of what is “important in the sense of being worthy of being known” (ibid.) must gain crucial importance for social scientists who care about how they spend their lives. In this regard, Weber’s response is emphatic: Researchers’ choices ought to reflect the culturally shared value orientations of human action and social interactions:

“Only a small portion of existing concrete reality is coloured by our value-conditioned interest and it alone is significant to us. It is significant because it reveals relationships which are important to us due to their connection with our values. Only because and to the extent that this is the case is it worthwhile for us to know it in its individual features” (ibid. 76).

The second implication concerns the conceptual tools social scientists could employ. Weber was convinced that the search for universal causal relationships would be futile. Nevertheless, the social sciences should seek insights of greater generality than could be provided by the historiographic description of unique conditions and interactions. His proposed solution was a holistic approach that would capture the logic of complex social constellations through the construction of “ideal types” (ibid. 90-104) — a solution to which I will return at the end of this lecture. But before I do so, I will first take up the question raised in the title of this lecture: Should we still have reason to worry about the constraints of social science research that Weber had emphasized a century ago?

The Ontology of Social Science Research

Weber rejected the belief that the social sciences should emulate the natural sciences in their search for law-like generalizations that might ultimately lead to “a system of propositions from which reality can be deduced” (ibid. 73). In his view, “laws are important and valuable in the exact natural sciences, in the measure that those sciences are universally valid.” But universal validity was not a meaningful goal for disciplines studying culturally shaped and historically unique objects: “For the knowledge of historical phenomena in their concreteness, the most general laws, because they are most devoid of content are also the least valuable.” And, even more emphatically: “In the cultural sciences, the knowledge of the universal or the general is never valuable in itself” (ibid. 80).

But, of course, these warnings were written at the dawn of modern social sciences — long before their practice was transformed from a “calling” into a “vocation” and then into a professional career; and long before the establishment of social-science curricula in which thousands of undergraduate and graduate students are trained each year in research methods and the craft of statistical analyses. They were also written before micro-economic and game-theoretic modelling gained influence on social-science theories, and before empirical work was facilitated by the wide availability of aggregate statistical data, or of survey and panel data based on individual responses. Moreover, while Weber did make extensive use of descriptive statistics in his empirical work, he could not foresee the progress in inferential statistical methods, including multivariate regressions and pooled time series analyses that have come to dominate

quantitative comparative research. And, most importantly, Max Weber did not live to see how the philosophy of science would become dominated by the covering-law dogma of the Vienna School (e.g., Popper 1959, ch. III), and how more and more branches of the social sciences, in seeking the status of a “normal science”, did indeed come to define the search for generalized causal relations as their primary aspiration.

Peter Hall (2003) has recently described the continuous spread of this aspiration in comparative research, which became dogmatized, even for qualitative studies based on small numbers of cases, in the standard work of King, Keohane and Verba (1994). However, Hall also pointed to the very limited success of this search for generalized causal relationships. In his own field of comparative political economy, the findings produced by even the most sophisticated multivariate statistical analyses appear to be remarkably unstable and highly sensitive to changes in the cases and time periods covered, and the variables included or ignored.² In his view, this state of affairs reflects a fundamental discrepancy between the correlational approach and the ontology of the field: Multivariate correlations seek to identify the general causal effectiveness of single variables, whereas the field consists of historically contingent social constellations that are characterized by complex interdependence, multiple causation, equifinality, and path dependence.

The solution which Hall proposes would replace correlational studies with a program of “systematic process analysis in multiple cases” (ibid.). That seems good advice as far as it goes. But in my view, Hall’s arguments are not radical enough, or do not go far enough, in the ontological dimension — and for that reason, perhaps paradoxically, I also find him to be too sceptical in the methodological dimension. As will be seen, moreover, my somewhat more optimistic arguments will rely heavily on Hall’s own work on the varieties of institutionalist approaches in the social sciences (Hall and Taylor 1998).

On the ontological level, the most fundamental problem of the social sciences is that we are studying phenomena that involve intentional actions of human beings. Undoubtedly, these can and must be considered causal factors which, among others, will bring about a certain effect. But from the researcher’s perspective, their place in an analysis of chains of causation differs fundamentally from that of natural factors. In the natural case, the behavior of a factor can be assumed to be completely determined by antecedent conditions and its own invariant reaction function, both of which are in principle accessible to the researcher. Intentional human action, by contrast, is shaped not only by objective antecedent conditions, but also by the actor’s perception of these conditions; and responses are shaped not only by quasi-natural reaction functions but also by preferences reflecting the actor’s purposes, goals, values or norms.

As social scientists, we need not take sides in recent controversies between brain researchers and philosophers on determinism versus free will (Pauen and Roth 2001). What matters for us is that actors’ perceptions and preferences are “subjective” — in the sense that they cannot be directly observed by others. For all we know, they may be only loosely coupled to the “objective” world, and they may vary from one actor to another. It need not concern us whether some of this variance may be an expression of free choice or whether it is entirely determined by differences in neural networks that are shaped through genetic codes and individual life histories. For us, it is important that we have no way of directly observing actors’ subjective perceptions and preferences.

² The same conclusion is reached by one of the foremost practitioners of quantitative comparative analyses (Kittel and Winner 2005; Kittel 2006).

But the same lack of transparency must affect the actors themselves, who also cannot directly observe each others' perceptions and preferences. Moreover, almost every human being is physically capable of harming and killing any other human being. Thus, if nothing at all could be known about actor intentions, mutual intransparency would create conditions of mutual distrust and fear, justifying cognitive orientations resembling worst-case scenarios and strategic preferences of generalized precaution. Thus, even if social interactions would not amount to a Hobbesian war among all, it can be shown analytically that generalized distrust would still prevent all productive social interactions (Scharpf 1990).

We also know, however, that the *bellum omnium contra omnes* is not waged everywhere, and that not all players in social interactions are invariably committed to playing *maxi-min* strategies. Hence the basic ontological puzzle of the social sciences must be: "What makes society possible?" And the solution of this puzzle is directly linked to the basic methodological question that I wish to discuss here: "What makes social science possible?"

In principle, there are three responses: Neoclassical economics and hard-core rational-choice approaches resolve the ontological problem by defining it away; and they resolve the methodological problem by the use of universal and determinate assumptions about actor preferences and perceptions which do not depend on empirical identification or confirmation.

At the other extreme, historiography and the descriptive social sciences are agnostic about the ontological problem and see no point in resolving it in theoretical terms. Since they are interested in the accurate representation of social reality, rather than in generalizable explanations or predictions, their methodological emphasis is on the quality of the data and the reliability of the methods used in their analysis. It is thanks to these efforts of social historians, social statisticians, demographers, survey researchers or social geographers that modern societies have the chance of obtaining accurate information about themselves.

In between these extremes, we find those branches of economics, sociology and political science which hope to go beyond description by seeking generalizable explanations and predictions but do not want to resort to the super-heroic assumptions of neoclassical theory. At the theoretical and at the methodological level, therefore, they — or should I say, we? — cannot avoid dealing directly with the ontological indeterminacy and intransparency of human intentions. As a consequence, we are continuously engaged in meta-theoretical debates about "approaches" or "frameworks" that might nevertheless allow us to expect patterns or regularities that could be generalized beyond the instances immediately observed. To the extent that our various approaches are at all concerned about the micro-level of social interactions, they must refer to mechanisms that might plausibly affect actor perceptions and preferences in such a way that they may become mutually predictable. In current meta-theoretical debates in the social sciences, such mechanisms are primarily discussed under the common label of "institutions". Hence most of the competing "frameworks", "approaches" or "paradigms" are appropriately considered varieties of "institutionalisms".

Varieties of Institutionalisms

In discussing the differences among approaches that introduce institutions as an explanatory variable,³ it seems most useful to start from the neoclassical extreme and to ask which of its assumptions are relaxed, and which institutional mechanisms are introduced for coping with the resultant behavioral indeterminacy.

In institution-free neoclassical theory, actor perceptions are assumed to be accurate representations of objective reality, cognitive capabilities are unlimited; and preferences over outcomes are utility-maximizing and strictly self-interested — rather than solidaristic, altruistic, competitive or hostile (Scharpf 1997, 84-89). In economic interactions, moreover, actors seek to maximize narrowly defined economic utility, and they prefer to do so only through engaging in voluntary exchanges.

If these assumptions are granted, intentional action will become a completely transparent and predictable reaction function — for other actors as well as for the researcher. Moreover, neoclassical economic theory is then able to show that social interactions will constitute ideal markets which will ensure socially optimal outcomes (Arrow and Debreu 1954). Conversely, if one or more of these assumptions are relaxed, transparency, predictability and social optimality are no longer ensured in the theoretical model — unless they can be replaced by the introduction of plausible and functionally equivalent institutional mechanisms.

Thus, if the assumption of a natural preference for voluntary exchanges is relaxed, the options considered by utility-maximizing actors could also include cheating, bribery, embezzlement, theft, robbery or murder. To maintain the socially benign implications of the model, therefore, the ideal market must at least be complemented by institutions that will effectively discourage economic actors from resorting to these strategies. In contemporary versions of neoclassical theory, this is achieved by adding the protection of “property rights” as another assumption to the model. The empirical weightiness of this assumption, however, and the enormous practical difficulties of approximating it in practice, have become apparent in the “good-governance” literature (Weiss 2000; Joerges and Dehousse 2002). In the same vein, if the assumption of complete information and unlimited cognitive capacity is relaxed, the acceptance of “bounded rationality” (Simon 1957; 1972) and of information asymmetries would introduce the possibility of “opportunistic” behaviour which would again undermine the efficiency of market transactions — and which could only be countered by the introduction of very sophisticated and demanding non-market institutions of economic governance (Williamson 1985). In short, even in its own field of explaining and predicting the behavior of economic subjects in the market, present-day economic theory is thoroughly afflicted with a need for institutional amendments. What remains unchanged, however, in all variants of economic institutionalism, is the assumption that actors will maximize their own economic utility. In other words, institutions are understood as sanctioned rules creating incentives and disincentives that

³ In the present context, I will treat institutional mechanisms as “independent variables” used to explain and predict the outcomes of social interactions. It is clear that this leaves out a large body of equally significant literature that treats institutions as the dependent variable. Loosely united under the label of “historical institutionalism”, this work focuses on the question of how institutions come about, what ensures their stability, and how do institutions change (Schotter 1981; North 1981; 1991; Axelrod 1984; Steinmo et al. 1991; Hall and Taylor 1998; Immergut 1998; Pierson 2000; 2002; Thelen 2002; Streeck and Thelen 2005; Schmidt 2006). Since institutions are also man-made, however, the explanations proposed here will again refer to the mechanisms suggested by the rational-choice and sociological variants of institutionalism discussed in the text.

will only affect actors' "preferences over strategy" but not their underlying "preference over outcomes". For generic *rational-choice institutionalism*, however, which tries to apply the economic model to social and political interactions beyond the narrowly defined economic domain, the identification of actor preferences must be a major conceptual challenge.

At the one extreme, "hard" versions of the economic theory of politics continue to assume rational and self-interested actors maximizing their economic utility — which will then support theories of predatory government (Levi 1988), rent-seeking politicians (McCormick and Tollison 1981), budget-maximizing bureaucracy (Niskanen 1971), and a plethora of other models of "government failure" (Brunsson 1989; LeGrand 1991). By the same token, unfortunately, it is also hard to see which type of actors could implement the institutional remedies which normative public-choice theory proposes (Buchanan 1986; 1993).

At the other extreme, "soft" versions of the rational-choice approach maintain the rationality postulate but make no a-priori assumptions about the type of utility actors are expected to maximize. As in historiography, therefore, preferences must be determined empirically in the individual case in order to construct rational-choice explanations. Given the difficulties of obtaining valid information about true actor preferences, however, the temptation seems great to take actual behavior (which expresses preferences over strategies under given circumstances) as an expression of preferences over outcomes. At the conceptual level, this is of course wrong. At the methodological level, nevertheless, "revealed preference theory" has attained respectability in the economics of consumer behavior (Houthakker 1950). If generally applied, however, it would allow rational-choice explanations to degenerate into mere tautologies (Hausman 1992): Actors do what they do because they have a preference for doing it.

Theoretically more ambitious, and empirically more helpful, are sociologically sophisticated versions of rational-choice institutionalism which advocate the use of role-theoretic definitions of preferences. Following Adam Smith, they assume a universal self-interest in physical well-being and social recognition. In order to become empirically useful, however, this abstract self-interest needs to be translated into role-specific preferences by a "social production function" (Lindenberg 1989; 2001): If you are a doctor in private practice, your best way to achieve well-being and social recognition is to take good care of your patients. As a first approximation, similar role-specific preferences may be plausibly defined for union leaders, lobbyists, central bankers, bureaucrats or partisan politicians — even if we do not (yet) have empirical information at the level of individual actors. By the same logic, employees may be assumed to pursue organizational goals if their personal preferences are neutralized through the employment contract (Simon 1951).

At the same time, sophisticated rational-choice institutionalism has also modified the cognitive assumptions of neoclassical economics. It adopted not only Herbert Simon's propositions on "bounded rationality" and the search for "satisficing" solutions, but also his concept of "selective perception" — which suggests role-specific differences in how limited capacities for attention are in fact allocated (Dearborn and Simon 1958): Central bankers and union leaders will pay attention to different types of information. Moreover, the recent rational-choice literature has also incorporated the social-psychological concept of "framing" according to which actors may interpret

given data and events in radically different ways in the context of specific roles and institutional settings (Goffman 1974; Esser 1990; Lindenberg 1993).

From the perspective of the empirical researcher, sophisticated rational-choice institutionalism has considerable comparative advantages: It is less unrealistic than the hard-core rational-choice approach, but it still avoids the need to obtain information on individual preferences and perceptions in every case. The information it uses to specify role-specific influences on perceptions and self-interested preferences should be common knowledge among the actors themselves and, for that reason, available to the researcher as well. In other words, this approach permits the formulation of plausible explanatory hypotheses and predictions on the basis of limited and relatively accessible empirical information.

At the theoretical level, however, all variants of rational-choice institutionalism protect the core concept of self-interested actors who are trying (within the bounds of their rationality) to do as best as they can under the prevailing constraints. Among these are not only the available resources and external conditions, but also the institutional framework within which interactions take place. As a consequence, the concept of institutions must also remain a “lean” one, staying close to the core notion of formal or informal rules that are associated with positive or negative sanctions. They provide a “structure of incentives” which will — together with external constraints and opportunities — determine the translation of fixed “outcome preferences” into “strategic preferences” for particular courses of action. If these are known, game-theoretic analyses will then allow the explanation and prediction of the outcomes of social interaction (Scharpf 1997).

In Max Weber’s terms, therefore, even the sophisticated versions of rational-choice institutionalism will only theorize the mode of “purpose-rational action”, but must ignore his equally plausible conceptualizations of “value rational”, “traditional” or “affective” action orientations (Weber 1921/1972, 12-13). In the current institutionalist literature, this gap is at least partly⁴ filled by a variety of approaches which are loosely summarized under the label of “sociological institutionalism” (Meyer and Scott 1983; March and Olsen 1989; Powell and DiMaggio 1991). From this perspective, institutions appear not merely as external incentives and constraints influencing the choices of self-interested actors, but they may directly define what actors seek to attain and how they see the world. In other words, they provide a more radical, and non-individualistic answer to the ontological puzzle of the social sciences: Societies are possible because the basic indeterminacy and intransparency of human intentions is overcome through the institutional determination of actor preferences and perceptions.

There is no question that factors included in this “thick” understanding of institutions may indeed shape human actions and social interactions. On the one hand, Weber’s “traditional” action has by no means disappeared in modern societies where much interaction reflects unquestioned “routines” and “standard operating procedures”, rather than the calculated choices among alternative options presupposed by rational models. On the other hand, we need not remind ourselves of the daily news about suicide bombers to acknowledge that narrow self-interest is not the only value that may motivate human action.⁵ On the cognitive side, similarly, sociological institutionalism

⁴ The last category, affective action orientation, is under-explored in the social science literature. But see, Flam 1998; 2002; Elster 1999.

⁵ But whereas Max Weber’s “value-rational action” had emphasized personal, and perhaps idiosyncratic value commitments, sociological institutionalism refers to socially defined “norms of appropriate

postulates a “social construction of reality” (Berger and Luckmann 1966): Through socialization and communication, the subjective, mutually intransparent and potentially incompatible perceptions of individual actors are harmonized into shared and mutually taken-for-granted world views that facilitate mutual predictability, trust and social coordination.

At the pragmatic level, the potential contribution of sociological institutionalism seems to gain in importance in contemporary social science research. Comparative studies that extend beyond the confines of modern, secularized Western societies obviously need to identify the effects of culturally specific normative and cognitive orientations; and the same is true of research focusing on multi-cultural interactions inside national societies. At the same time, however, the inclusiveness of the definitions used, and the fuzziness of the concepts employed, makes it difficult to determine the conceptual cores, and the theoretical implications, of propositions advanced in norm-oriented, ideas-oriented, discourse-oriented, or broadly social-constructivist versions of sociological institutionalism.⁶

Moreover, even if the mechanisms were clearly specified, their causal effectiveness is not easily assessed (Schmidt 2000). On the one hand, the presumed causal arrow may be reversed. Actors know how to justify what they are doing. Moreover they may avoid the psychological discomforts of “cognitive dissonance” (Festinger 1957) by adjusting their (subjectively honest) beliefs to fit their actions. On the other hand, the postulated link between actions and professed beliefs may not exist at all. Communicated values may be no more than what game theorists call “cheap talk” or manifestations of culturally or politically required hypocrisy (Krasner 1999; Iankova and Katzenstein 2003).

This should not be used as a theoretical argument against hypotheses derived from sociological institutionalism. But it suggests that their convincing use in empirical research is likely to depend on sophisticated and demanding research designs. More tentatively, it might also imply a pragmatic preference for beginning major research by exploring the explanatory power of less information-intensive hypotheses derived from reasonably sophisticated versions of rational-choice institutionalism. If these explanations seem to fit, we could leave it at that and go to other interesting research problems. If they do not fit observations, we could take a step or two downward on Lindenberg’s (1991) ladder of “declining abstractions” and seek more narrowly focused pointers to role-specific preferences and perceptions. If these fail as well, we will at least know that we have an *explanandum* that may need to be explained by more research-intensive, and empirically less robust, hypotheses derived from sociological institutionalism.

Institutionalism and the Possibility of Embedded Generalizations

So how does the variety of institutional approaches allow us to deal with the ontological and methodological problems we have to face? They all suggest institutional mechanisms that could reduce the basic indeterminacy and intransparency of human intentions by constraining or shaping actor options, preferences and perceptions. If, where and when these mechanisms are effective, they can be expected to increase

behavior” (March and Olsen 1989) that may override the self-interested orientations as well as the personal beliefs of individual actors.

⁶ For an ambitious and convincing effort at classification, see Schmidt (2006).

mutual predictability among the actors involved, and thus provide solutions to the ontological puzzle of “what makes society possible?” By the same token, these mechanisms can also be expected to create behavioral regularities that can be observed in empirical research. Moreover, since the mechanisms themselves — e.g., sanctioned rules or culturally defined norms and beliefs — are in principle accessible to the researcher as well, they can be used as theoretically-based arguments in social-science explanations of the observed regularities. In other words, institutional mechanisms may support law-like generalizations and thus may also provide the answer to the methodological question of “what makes social science possible?”.

But these were conditional statements, introduced by “if, where and when these mechanisms are effective.” Of these, the if-condition appears to me least problematical. I see no reason to doubt that all of the mechanisms specified by the variants of rational choice institutionalism, sociological institutionalism, social constructivism or discourse theory may in fact be able to influence actor perceptions and preferences in predictable ways — just as we have no reason to doubt that Max Weber’s conceptualizations of purpose-rational, value-rational, traditional and affective action represent modalities that may occur in the real world. At the level of meta-theoretical discussions, therefore, I see no reason to take sides, to dogmatize one of them, or to play off one against the other. They are all valuable elements in our tool kit of potential explanatory factors. Instead, it would be very useful to engage in discussions about the real-world conditions in which one or the other framework would be most usefully applied.

More important, in light of the questions from which I started, are the “where and when conditions” of institutionalist explanations. Obviously, all of the institutional mechanisms discussed are man-made — a product either of evolutionary processes or of purposeful design, and most likely of both. In any case, their content and their effectiveness will differ in time and place. As a consequence, the explanatory power of institutionalist hypotheses will also be limited by the empirical domains of the specific mechanisms identified. For example, even if we confine ourselves to parliamentary institutions and to the rational-choice definition of formal, sanctioned rules as structures of incentives, these will differ so much between the American House of Representatives, the British House of Commons, the German Bundestag or the European Parliament, that we should not hope for a universal theory of parliamentary behavior. And the domains of expected regularities will be still further reduced if we need to add “sociological-institutionalist” hypotheses that depend on the norms of appropriate behavior in Swedish bourgeois parliamentary parties in the 1960s or the German Green party in the mid 1980s — as distinguished from norms that were effective in the late 1990s in both cases.

In other words, institutional mechanisms will indeed allow us to expect behavioral regularities, and to formulate and test explanatory hypotheses that claim validity beyond the individual case. In that sense, they will allow us to go beyond the reconstruction of individual interactions through Max Weber’s method of “emphatic understanding” (Abel 1948), or the methods of “process tracing” (George 1979) or “systematic process analysis” (Hall 2003). But our generalizations will always be institutionally embedded. Hence they will at best amount to what the late James Coleman called “sometimes-true theories” (1964, 516-519) whose scope is limited in time and space to the empirical domain of the underlying institutional mechanism. Moreover, in using such theories, we need to be always alert to the possibilities of institutional change. Institutions affect human choices, but they cannot fully determine

them. Being the product of human interactions, they continue to be shaped by further interactions which may either reinforce or erode their causal effectiveness (Streeck and Thelen 2005).

In any case, being sometimes true, our institutionally-based generalizations will also be wrong most of the time. In order to be considered valid theories, therefore, generalized statements would have to specify not only the external antecedent conditions, but also the domains of the specific institutional mechanisms that are supposed to explain the observed regularities. In practice, however, precise domains in which explanations are in fact valid are difficult to identify and cumbersome to specify. As a consequence, research findings are frequently formulated in over-generalized language, and they are almost always read without proper attention to their antecedent conditions and institutional domains. Other researchers, working in different fields, will then consider these findings disconfirmed, and proceed to report their own findings in, again, excessively generalized fashion. It is this unending cycle of over-generalized theories and equally over-generalized falsification that prevents the social sciences from realizing even their limited chances of cumulative progress.

Yet even if these mistakes are avoided, the embedded generalizations that we may identify are likely to be in the nature of single-factor theories predicting the effect of a particular institutional mechanism under *ceteris-paribus* conditions. We know, however, that these effects will be modified by variations in the external conditions and by the interaction with different institutional mechanisms operating at the same time. If we are interested in full explanations and valid predictions, therefore, we need to take these interaction effects into account.

We could do so case-by-case by using our partial theories to explain longer and more complex chains of social interactions in the spirit of “analytic narratives” (Bates et al. 1998) or of Peter Hall’s (2003) “systematic process analysis”. A more holistic and structural approach would require the stylized reconstruction of more complex institutional configurations. To take an example: A structural model of interactions in Swedish economic and labor market policy in the 1960s would at least have to include the following elements: A high-growth international environment; the institutional preponderance of the “power resources of the labor movement” (Korpi 1983; Esping-Andersen 1985); normative actor preferences shaped by a strong commitment to full employment and equality; and cognitive orientations defined by the “Rehn-Meidner model” (Meidner and Hedborg 1984) that combined Keynesian macro-economics, solidaristic wage bargaining and active-labor-market policies. As long as all these elements were present, a great variety of interactions could be perfectly explained and predicted (Scharpf 1991, ch. 6).

In similar fashion, authors in comparative political systems and comparative political economy have proposed a variety of complex models to characterize configurations of interacting institutional mechanisms. Among the well-known examples are the Lijphart’s (1999) “Westminster” and “consensus” models of parliamentary democracy; Schmitter’s (1979) comparison of “pluralist” and “neo-corporatist” modes of interest intermediation; Esping-Andersen’s (1990) “Three Worlds of Welfare Capitalism”; Hall and Soskice’s (2001) varieties of “liberal” and “coordinated” capitalism, and perhaps even the modes of “intergovernmental”, “joint-decision” and “supranational-hierarchical” policy making in the EU (Scharpf 2001). All of these models have their origin in the reconstruction of interacting influences in a particular historical and institutional setting. Here, their explanatory power is at a

maximum. But since they are generally formulated at a more abstract level as simplified and internally coherent “ideal types” of the original constellation, they may be applied beyond the original domain. In that case, however, we should remind ourselves of Max Weber’s own critical assessment of the ideal-type concept which he introduced (Weber 1904/1949, 101-102): These stylized conceptualizations are misused when employed as a general theory, but extremely useful as heuristic tools to identify characteristic features as well as the specific factors that distinguish the case at hand from the model.

Conclusion

To summarize: The institutionalist approaches that dominate current meta-theoretical discussions in the social sciences are based on an ontological understanding of social reality that allows us to expect empirical regularities in social interactions that could justify inductive generalizations (Willer and Willer 1973). Moreover, they also provide us with conceptual tools that facilitate theoretical explanations of such regularities that can claim validity beyond the specific case at hand. By the same token, however, the potential scope of such generalizations is circumscribed by the empirical domains of the institutional mechanisms invoked. Thus institutionalist explanations will not support the hope that the social sciences would eventually be able to find and explain universal causal regularities.

If this methodological limitation will not deter you, and if you can live with the knowledge that your best efforts will never produce more than “sometimes-true theories”, however, you must still face Weber’s second question: What in the myriad of complex social phenomena “is worthy of being known”? It is a serious question. Having to spend innumerable hours of hard work on research that neither you nor anybody else cares much about is a prospect that ought to scare you long before you reach your midlife crisis.

Worse yet, I should also warn you of placing false hopes on the one criterion that is likely to tempt many of you. “Problem-oriented” or “policy-oriented” research may indeed engage the social sciences in the analysis of society’s most pressing problems and the search for feasible solutions. It is in this context that our search for empirically based but theory oriented generalizations, even though their domains are limited in time and space, appears more socially useful than either the purely analytical propositions of neoclassical economics or the purely empirical information of the descriptive social sciences or of historiography. If we do our job well, we have valid insights to offer that could not have been obtained by other scientific disciplines. From a personal perspective, nevertheless, it would be a mistake to choose an academic career in the social sciences if your foremost motive were to gain political influence and to help shape the future of your society. For even if your analyses of social problems are timely and scientifically valid, and if your policy proposals are supported by solid empirical evidence and sound theoretical arguments — the most likely outcome is still that nobody outside of academe will pay much attention, and that nothing much will happen in practice.

Nor should this surprise you. If, as Aaron Wildavsky (1987) once put it, “speaking truth to power” is the aspiration of policy-oriented research, its ideal client would be a benevolent dictator who also happens to be somewhat ignorant. But this dictator is hard to find in modern democratic polities. Those who seem to be in power are struggling to hang on to it against competitors in their own parties and in the

opposition. And if they succeed, they must still share power with multiple domestic and foreign veto players that have their own interests and agendas. Under these conditions, the practical effectiveness of even the most pertinent social analyses and policy recommendations depends on windows of opportunity where they happen to suit the strategic and tactical needs of the actors in charge. Such opportunities are hard to anticipate, and if we try to chase them with our research, we are likely to end in deep frustration. With some modification, the same can be said about the attention of the media.

Even in policy-oriented research, therefore, the best we can do is to have well founded analyses and recommendations available and clearly communicated, so that political actors and the media may remember them if and when the opportunity should arise. This much is within our power, and this should be our pride. If instead influence on public policy or public opinion were your main aspiration, you would be better off going directly into politics, the bureaucracy or the media, rather than expecting that they will eventually come to you for advice or orientation.

So we have come back to where we started. If a career in the academic social sciences will bring us neither wealth nor power nor fame, and if we cannot even count on being able to influence public policy or public opinion, we might as well forget all extrinsic motives based on considerations of instrumental rationality. If we decide to commit our lives to doing social science, we must rely on intrinsic motives — and the best one I can imagine is a desire to add to our collective understanding of the social world in ways that would not be possible in any other line of work. True, the knowledge we will be able to gain will not be valid everywhere, and it will not endure for the ages. But for our own time and place, it may be the best knowledge that can be obtained. If you accept that, you should also feel free to follow Max Weber's first advice: Let your own deep value commitments guide you in selecting your research topics, even if others might seem more promising from a career perspective. Good social science is terribly hard to do, and you will not be at your best working on questions that you don't care about. But with all these caveats, let me also assure you: The life of the social scientist has its own and unique rewards, and if you care about these you should be congratulated for having chosen well.

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