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THE NATURE OF SCIENTIFIC CONSENSUS AND THE CASE OF THE SOCIAL SCIENCES*

The data presented here derive from a comprehensive study probing into the characteristics of 'external' and 'internal' (Leinfellner, 1974) control in social science development. The concepts used in the study are tied up with Kuhn's theory of scientific revolutions (1962, 1970), according to which the systems of rules and theses in mature sciences encounter a more or less world wide acceptance on the part of the scientists involved with them. During the pre-paradigmatic stage of a science, however, there is a series of schools that are ensnared in reciprocal controversies about their specific metaphysical basic assumptions, which ultimately constitutes their only raison d'etre. The scientific efforts are dialogues between the members of different schools rather than with nature. The transition to mature science is identical with the triumph of one of these schools and with the general acceptance of the first paradigm. Simultaneously the nature of the paradigm changes. Only after the change is normal puzzle-solving research possible.

In his postscript of 1969 Kuhn points out that evidently also the schools of 'pre-paradigmatic' science show paradigms. Hence, it is not the existence of paradigms, but their acceptance beyond the boundaries of the individual schools as well as their applicability as a tool for scientific problem solving that mark the transition to mature science. Various critics have reproached Kuhn that he drastically overstates the unanimity of scientists in their allegiance to a paradigm. This reproach is at least in part due to a misunderstanding, since no dividing line is drawn between the various levels on which a paradigm is operating. Obviously individual paradigm components (or entries in a 'disciplinary matrix') can at the same time furnish the consolidating basis for an entire series of areas of research as well as they can themselves be made the subject of controversial analyses in other speciality fields. Therefore, the concept of professional consensus must always be understood in the sense of a *preliminary* consensus. In our view such a preliminary consensus is a logical correlative of the growth of scientific knowledge, since scientific results can only be converted into

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cumulative 'knowledge' by inter-subjective consentaneous acceptance. The actual dissensus, which Kuhn and in the following others have attested to the social sciences, is therefore rightly regarded as an indicator for the lack of progress of knowledge (see Kuhn, 1962; Storer, 1967; Zuckerman and Merton, 1971; Lohdal and Gordon, 1972; Lammers, 1974).

The introduction of professional consensus as a demarcation criterion between pre-paradigmatic (social) and paradigmatic (natural) sciences evokes a series of questions some of them we would like to illustrate in the present paper by reference to empirical data. The most fundamental question is undoubtedly under which conditions a general agreement can be expected to arise in scientific communities. Furthermore there is the question of whether the commonly held judgement on the actual dissensus in the social sciences can at all be sustained. Another question which comes up is as to which effects the differing 'consensus potential' in the natural and social sciences will engender within the science-internal control systems; and finally there is the question as to whether the observed dissensus in the social sciences is in accordance with the cognitive expectation structures of the scientists themselves.

1. CONSENSUS AS A FUNCTION OF THE TRIVIALITY OF DECISIONS

The first thing we are interested to explore is the question of the conditions which enhance or hinder the development of consentaneous agreement transcending the boundaries of 'schools' in science. The answer given by those adhering to a materialist theory of knowledge may well be that consensus can only be achieved under the condition of identical interests and the hence resulting convergence of opinions (e.g. Sandkiihler, 1975). Analytical philosophy of science, on the other hand, will refer to the inherent rationality of the scientific operation, which, being backed by the scientists system of norms, implies that a scientist would be obliged (and competent enough) to decide in favour of the scientifically 'better' alternative. Conflicting career interests and differential access to resources make it appear impossible to reconstruct consensus in terms of identical interests without reference to a system of norms. However, the assumption of the inherent rationality of scientific action is also met by objections which impose themselves immediately: firstly, the possible *incom-*

mensurability of scientific theories or paradigms, in itself the object of unresolved controversies in the system of science; and secondly, the *context-dependence* of scientific action which - irrespective of incommensurability - makes the possibility of aggregating the individual adherences to norms appear unlikely. Since the individual scientific decision is taken in a cognitive field, which must be relatively restricted with respect to the consideration of factors relevant for decision making, this decision is of necessity only partially rational, even if no external non-cognitive factors are introduced. In addition, the individual rationality of the scientist is permeated by particularistic considerations, which by no means can all be rendered functional for the scientific enterprize with the help of the reward system.[1]

In the light of these difficulties we consider it useful to substitute the optimistic inference of science-internal consensus from cumulative rational individual decisions by a more pessimistic alternative, whose advantage mainly consists in having to make much weaker assumptions on the aggregating potential of individual interest-linked rationality. Accordingly, consensus would not be a matter of convergence of opinions due to identical interests and/or equivalent normative orientations, but rather a *function of the triviality of decisions*[2] involved. We call a decision trivial if one cannot avoid its outcome without calling into question one's own rationality as a competent subject. We now maintain that the degree to which decisions on acceptability of knowledge-claims are trivial depends on the structure of the chains of argumentation[3] and justification which they refer to. According to Toulrnin's (1958) structure chart[4] an argument would have to include a *conclusion* (C) or a knowledge-claim; in order to establish the merits of the conclusion we appeal to facts as the foundation for the claim - what Toulmin refers to as *data* (D). Being required to indicate the bearing on our conclusion of the data already produced we would have to propose a *warrant* which shows that the step to the original claim or conclusion is an appropriate and legitimate one. Warrants, in contrast to data, are appealed to implicitly; furthermore they are general, certifying the soundness of all arguments of the appropriate type. Standing behind our warrants there will normally be other assurances *('backings',* B) which vary from one field of argumentation to another. Finally, the conclusion is subject to certain *conditions of exception* or rebuttal (R) which have to be specified. The argument thus assumes the

following simplified form (Figures 1 and 2):

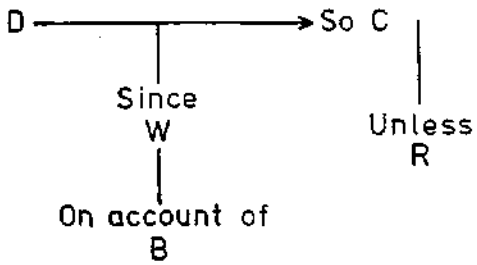

Fig. 1.

The following example Toulmin's should serve as an illustration:

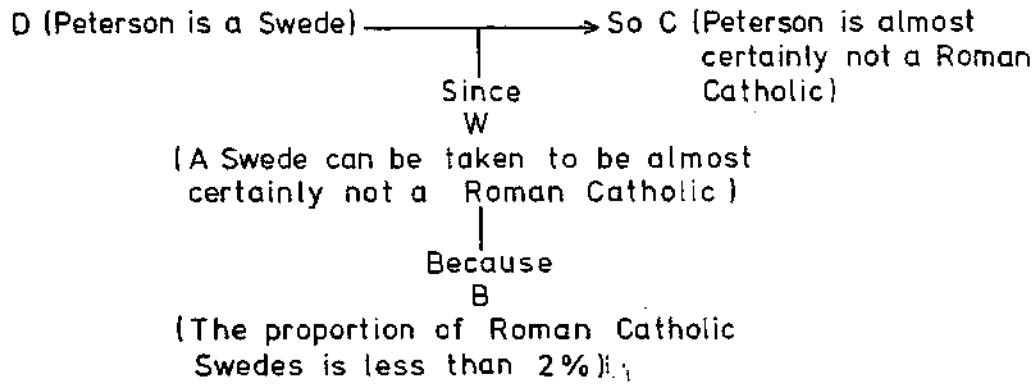

Fig. 2.

According to the type of backing put forward a tentative distinction of the following structures of argumentation can be made:

(a) *Analytical argumentations* according to Toulmin refer to those arguments whose warrants are backed not by experience, but by entailment, or, in other words, in which checking the backing of the warrant involves ipso facto checking the truth or falsity of the conclusion (Type 1).

(b) *Empirical argumentations* accordingly refer to those arguments, whose backings revert to empirical data, which can supposedly be attained through the operation of rules of correspondence (Type 2)

(c) *Dialectical argumentations* finally refer to arguments where the attempt to justify a generalization W_x involves a new argument containing a further generalization W_{2i} etc. (Type 3).

The above distinctions allow us to differentiate between structurally *open* and structurally *closed* chains of argumentation in terms of the consensus which can be reached about the 'truth' or 'falsity' of the arguments involved. With structurally open arguments it should in principle be diffi-

cult to reach consensus about the truth of their knowledge-claïms solely on cognitive grounds - a situation which might enhance the operation of factors such as power or fashion in the process of establishing the number of followers of a thesis. On the other hand, structural closure of an argumentation depends on the standards of truth and falsity presupposed or accepted and on what constitutes a 'proper' calling into question of knowledge claims. Standards of truth presupposed are those required by the process of symbolization itself such as principles of logic and those required by the process of communication such as acceptance of what we can observe as 'facts' (cf. Feigl, 1963). By standards of truth accepted we mean those methodological rules and procedures in science adherence to which guarantees that truth is (held to be) conserved over or brought out by a whole series of steps of interaction with 'reality' based on scientific instruments of some kind. In a clearly analytic argument, because of the presupposition of the rules of symbolization to any argument failure to accept the truth of the message is a sign not of potential counter-evidence, but rather of a 'defect of reason' on the part of the person raising the question. Hence, with *analytic arguments*^ closure in terms of consensus is implied by the very structure of what is put forward. On the other hand, *dialectical arguments* are potentially open arguments, since they constantly have to rely on new, unproven non-methodological warrants. With dialectical arguments as with empirical arguments the range of proper callings into question is much wider than with analytic arguments. We refer to 'proper' callings into question because of the fact that the potential of critique and objection embedded in every argument is not fully exploitet by fellow scientists. Thus, a correctly established empirical proof of a thesis will in the normal practice of natural science hardly raise objections as far as the factual result is concerned in spite of the fact that empirical proofs because of all the background knowledge and ceteris paribus clauses involved will never be ultimately conclusive. Consequently, closure of the argument by consensus is reached because actual areas *of proper* and *effective* (i.e. taken serious by fellow scientists) callings into question are much more limited than potential ones.

A legitimate area of objections in the case of empirical arguments involving *theoretical terms* (cf. Carnap, 1959) is that of correspondence between knowledge claims and the data presented. We held that in the social sciences consent on and hence closure of type 2 argumentations

based on theoretical terms meets with fundamental difficulties because of the lack of a *theory of meaning* establishing connections between those terms and measurement results.[5] Furthermore, measurement results themselves as far as they transcend clearly observable factual information have to be made sense of in the light of such a theory of meaning. If, however, structural consent in empirical chains of justification remains restricted to arguments based on *observational terms,* growth of knowledge in the social sciences is reduced to the level of empirical generalizations. This implies that the social sciences are banned from an effective reduction of complexity which goes together with cumulative theoretical development (Luhmann). One of the symptoms of such a situation is the 'gap' between theory and research observed by many social scientists, for which empirical evidence is now being accumulated. Cole (1975) has proven for a sociological specialty field that only 3% of the empirical studies which explicitly rely on a certain theory do in fact attempt to test the respective theoretical assumptions, whereas the rest only vaguely relates to them thus continuing the gap.

That a theory of meaning or a theory of symbolical communication is fundamental for the further development of the social sciences has been demonstrated in particular by Cicourel (1964,1970) in his methodological and linguistic studies. Numerous other authors have also pointed to the necessity of developing a theory of the social and cognitive constitution of meaning and its relationship to knowledge claims (e.g., Schütz, 1932; Winch, 1958; Fodor and Katz, 1964; Habermas, 1967; Luhmann, 1974). We consider it extremely important that distinctions like that between theoretical and observational terms be included into such a theory. In our view it is no coincidence that the main explanatory variables of empirical sociology once compiled by Baldamus (1973) are concepts of mainly factual nature: income, profession, education, sex, age, status, religion, race, residence (urban-rural origin), political attitude and social class, the latter being the most strongly theory-oriented concept in the enumeration[6]. Obviously this repertory of explanatory concepts has evolved pragmatically in the course of time from the fact that these variables are indeed effective in a typical sociological survey study. That they were at all considered is probably due to their every-day relevance rather than to any embedding in scientific theories. We do not want to deny that these concepts yield as a matter of fact high empirical correla-

tions. We only think that the 'paradigmatic traits' shown by many areas of empirical sociology and indicated by a potential consensus on explanatory variables (as well as on a certain methodological approach, and the like) could not be imagined, without the objective or objectifiable character of the dimensions mentioned. These paradigmatic traits are also not impaired if the empirical sociology in question has been severely criticized in the meantime. For it was not the falsity of the results that was critized, but their substantial limitation or inadequateness in dealing with the problems involved.

In the field of sociological theory, however, no approximately equivalent consensus on relevant concepts can be found, not to speak of the consensus on theoretical generalizations. Freese (1972) rightly indicates that in this context it is not only the problem of the 'gap' between theory and research that is relevant for the assessment of the situation, but above all also the mutual interplay of many theories or of the theoretical implications of many empirical studies. Yet the problem does not seem to primarily reside in the fact that we allow our theoretical knowledge to evolve haphazardly; in the natural sciences there also is no a priori integration of research results. What there is, however, is a sort of 'pre-established harmony' on the basis of the fact that concepts can be well defined (which does not necessarily mean that they are always well defined) in the framework of their theoretical and factual references. The latter guarantees that a positive or negative (i.e. falsifying) integration of the research results is in principle possible, just as well as an appropriate linking of the different theory fragments. The possibility to formalize empirical and theoretical generalizations in wide areas of the natural sciences is generally considered as an indication of the assumed well-definition of meanings. As opposed to this in sociology, for instance, as well as in most other social sciences, it is the *haphazard relation of meaning in our knowledge claims* due to unknown mechanisms of the social and cognitive constitution of symbolic content that we held to be responsible for the lack of cumulativity whenever theoretical terms are involved.[7]

We are now in the position to present a more detailed version of our initial hypothesis: accordingly, the amount of consensus in a certain field is a function of the *degree of triviality of decisions,* and this again is a function of the *structure of the chains of argumentation and justification* involved. The amount of consensus will be greatest when structures of

argumentation and justification of *Type 1* dominate. The potential closure of empirical arguments in the social sciences depends on the type of terms used: on the level of argumentation operating with observational terms the possibility to render decisions trivial and hence to promote selections leading to consensus seems to be guaranteed. In the field of what has been called 'theoretical' terms, on the other hand, there will remain difficulties in achieving consensus as long as no theory of meaning brings us closer to the solution of two main interlinked difficulties: the relation between theoretical and empirical generalizations *(the measurement problem)* and the relation between different theoretical contents *(the problem of meaning).* Since a clear-cut distinction of areas operating with observational terms from those operating with theoretical terms hardly seems possible in most social sciences and since scientists themselves show a tendency to create theory-based interpretations for their observational findings, in sum total we expect rather little consensus for the *empirical structures of argumentation* and justification. One can assume that, for the latter reasons, the extent of agreement in the area of empirical argumentation will hardly differ from the expected low degree of consensus in the area of dialectical argumentations.

2. CONSENSUS AND CUMULATIVITY OF SCIENTIFIC KNOWLEDGE

The asserted correlation between consensus and structures of argumentation and justification has an important consequence for the relation between consensus and cumulativity of scientific knowledge postulated by Kuhn: it implies that it is the predominance of the respective chains of argumentation and justification or their codified methodological versions [8] and *not* necessarily the *level of development* of a discipline that is essential for the explanation of a given amount of consensus or dissensus. Therefore, our second main assumption states that consensus is though a *necessary* correlative to cumulative progress of knowledge, yet does not represent a *sufficient* condition for achieving substantially cumulative results. Hence - depending on the methodology - the potential gap between consensus and cumulativity makes it appear possible that high rates of consensus go together with a granted lack of cumulativity, for instance when analytical structures of argumentation are linked with substantial problems in the realm of theoretical terms. This is the case, because com-

plex decision making processes (such as for instance within the research process) according to our hypothesis enhance an orientation towards those decision criteria which are easily decided and evaluated. Within the context of decision making in scientific research practice such an orientation brings about a *cumulative selection* in favour of those aspects and topics which involve *trivial* (i.e. easily decided) *decisions*, both in the framework of the decision making problems of the individual scientist as well as in the framework of the diachronic development of individual scientific fields[9]. Such a selection, however, implies a shift of perspective in a direction which potentially neglects the original substantial problem involved.

Selection processes in favor of trivial decisions not only derive fiom an orientation towards easily decided and evaluated criteria, they are also encouraged by other related factors. One is that non-controversial criteria are apt to unambiguously signalize the *professional competence* of the author; another is that rewards are allocated on the basis of those aspects of a scientific effort which can easily (trivially) be controlled by those in charge for the allocation of rewards; a third is the fact that only consentient decisions allow for 'definite' *error identification* as well as for a (cumulative) selection of 'correct' results basing on it. Obvious examples for such selections are the model platonism in economy as well as in general the much deplored methodism of social science disciplines. In both cases one can speak of a universally recognized deepening of knowledge with respect to the development of mathematical and statistical procedures and other codifiable problem aspects at the cost of the substantial problems involved. The most common explanation for this phenomenon points to the artificial imitation of natural science interests and procedures in order to account for this development (e.g., Habermas, 1968). Our hypotheses do not deny this interrelation; they rather attempt to specify those mechanisms that were responsible for converting general value orientations into the actual flourishing of certain subjects.

3. SCIENTIFIC ORTHODOXY AS FACTUAL FALSIFICATIONISM

Our last assumption is a specification of Kuhn's thesis on the relative orthodoxy of normal scientific orientations in the light of the statements so far made. It maintains that the predominance of potentially trivial

decisions within a certain research practice leads to the formation of *convergent*[10] expectation structures whereas the respective contrary practice ought to lead to divergent orientations. Here the dichotomy convergent-divergent denotes the difference between expectation structures which are oriented towards consentaneous, problem-solving results easily integrated into the original frame of reference on the one hand, as opposed to *divergent,* disagreeing results, on the other hand. The assumption becomes understandable when it is pointed out that trivial decidability also means *trivial controllability* in the dual sense of the word: firstly, if the appropriate decision criteria are present, the scientist is well adviced to control his own results before submitting them to the scientific community (primary control), because of the very ease of control on the part if his fellow scientists; and secondly, the further use of the results by other scientists implies an indirect and mostly also unintentional secondary control in that possible problems are identified as errors and can be fed back to the original results. In our view this last possibility presents a particularly decisive difference to those fields which do not dispose of easily closed chains of argumentation and justification.

In order to illustrate this we would like to begin with an ideal type of example. Let us imagine a hypothetical natural scientist, whose empirical results do not correspond with his (paradigmatic) expectations. Such a scientist will first of all repeat his experiments in order to be sure that no procedural error has crept into his results. If again the results do not coincide with his expectations he will embark upon a detailed examination of the method used (re-calibration, etc.). If no flaws can be found here either he will consider whether he had not made some general conceptual mistake which ran through all his checks and therefore prevented him from finding the 'correct' results. If his results again withstand this examination he might- maybe after another thorough control of the individual steps - do the following:

(1) The results disappear in a drawer, because our scientist or his professor do not want to run the risk of being reproached with bad working methods, or, worse, scientific incompetence;

(2) The results are published, but with the specific comment that they cannot be generalized, and with an emphasis on the special ceteris paribus clauses under which the experiments were carried out;

(3) Our scientist goes on checking his results, which implies that he

submits them to a still more scrutinized and profound investigation.

The time and money consuming severe tests of his results are not undertaken because our scientist is interested in the *falsification* of the underlying theory. Rather, scrutinity was provoked by the ease of primary and secondary control of results and by the fact that easily identified errors in the controllable sphere of his work might make people doubt the scientific competence of their author, thus bringing about serious existential consequences for the natural scientist. This is quite contrary to the situation of a typical social scientist, say a sociologist, whose empirical examination has led to results which contradict some of the results generally accepted in the literature. Due to the problems of measurement and meaning a sound control of his survey responses is in principle only possible when the survey has been restricted to concepts based on observational terms. The same applies to the possibility of validating the respective method. Since pragmatic difficulties usually prevent the two control steps from being taken, checkups of possible conceptual mistakes do not exactly impose themselves, with the effect that there are no obstacles impairing the immediate publication of the results including an emphasis on their being new and different. The best controllable and most controlled aspect of a survey is that of the proper use and correct application of statistical methods and techniques. A step, which does not tie up with the items of data selection, data quality and data meaning that are strategical for obtaining valid results.

We summarize our examples as follows: the production of research results under the condition of open structures of argumentation and justification in wide areas of the social sciences operating with theoretical terms is characterized by a *verification circle,* in which the lack of adequate control possibilities as well as of rigid expectation guidance by generally accepted knowledge (the entries in a 'disciplinary matrix') leads to a situation in which the accumulation of different and contradicting results imposes itself. The primary and secondary control possibilities in the natural sciences, on the other hand, represent a *negative control circle,* which stabilizes the system by orienting it towards the target value 'truth'. As in the social sciences the steering mechanism is not a direct one, but one which operates via the system of norms and rewards serving as a guide-line for the members of scientific communities. Their relative orthodoxy and their orientation towards the production of 'consonant'

results conforming with the generally accepted knowledge turns out to be a directed *falsificationism* that submits *those* results to a specially rigid investigation that question the ruling and effective patterns of explanation.[11]

If orthodox patterns of expectation and the assumption of severe (directed!) testing of results can be reconciled with each other within the negative control circle, the verification circle described by us should not be confused with the lack of any expectations or any constraints (e.g., Stehr, 1974). However in the case of verification, expectation structures are different in the following respects: firstly, these expectations are not oriented towards the elaboration of *convergent* results, since the lack of a generally recognized body of knowledge and the difficulties of a rigid endogeneous control do not promote such an orientation. Therefore, the norm of novelty and innovation characterizing science works in a direction where an almost unrestricted variety of isolated, diverse and contradicting results can be produced. Secondly, it is true that work within a rigid scientific tradition (a school) will yield accordingly orthodox expectations - and a critical control will primarily attack those results that contradict these expectations - yet the expectations themselves are not grounded in *generally accepted* knowledge, but on particular doctrines. And finally it is also true that the complexity and diversity of the results produced by a faulty endogeneous selection is reduced by the expectation structures, but, contrary to the natural sciences, this reduction is governed by influences *external* to science (cf. Leinfellner, forthcoming).[12] Since generally accepted theoretical interpretation calculi are practically nonexistent and since an effective internal elimination of errors and erroneous interpretations is largely lacking because of the mal-functioning methodological control circle, said reduction of complexity is carried out in the social sciences by ruling *societal patterns of interpretation.* We are not in a position here to present a detailed definition of the concept of societal patterns of interpretation. All that should be mentioned is that value judgements as well as class interest and metaphysical assumptions flow into the 'every-day knowledge' which constitutes the basis of theoretical hypotheses as well as of empirical variables in the social sciences. This knowledge is conveyed by the concepts and conceptions rooted in everyday language; it is imparted by the structures of expectation of the individual scientists who determine the selection and interpretation of the data.

The arguments presented so far lead to a qualification of our assumption of the potential gap between cumulativity of knowledge and consensus: it imposes itself to make the coincidence of substantial cumulativity as a criterion for the maturity of a science, on the one hand, and of consensus, on the other hand, depend on the *adequate functioning* of the control circle described. For only the functioning of this control circle is tantamount to a primary science-internal error reduction and selection (obtained by an effective further utilization) of 'true' results meeting the requirements of correct inference and tieing up with accepted bodies of knowledge.

In this paper only two of the above assumptions can be substantiated empirically. Firstly the assumption of the interrelation between consensus in a specific field and easily closed 'trivial' decisions in the research process; and second the hypothesis that the expectations scientists place in their results reflect their methodological orientations, the latter being linked with certain structures of argumentation and justification. For a more detailed presentation of the relevant empirical data, see Knorr (1975).

4. DESIGN AND IMPLEMENTATION OF THE STUDY

The above mentioned hypotheses were operationalized in a study that was carried out in Austria in 1973-74. It is a survey of all research units working in the social sciences, our definition of social sciences comprising the following disciplines and fields: psychology, paedagogics, sociology, political science, economy, business administration, ethnology, modern history, regional and urban research. In the respective research units the head of the unit was personally interviewed, while his research staff was asked to fill in written questionnaires. Since the survey addressed institutionally defined 'units' and not individual scientists, individual scientists without an institutional backing fitting our definition of social sciences were not included. Not considered were also those few units exclusively engaged in teaching social sciences. Members of a unit were regarded as research staff when they either had an academic degree or at least held the position of a research assistent.

In total responses from 624 scientists were obtained. The rate of response differed slightly according to the various survey regions as well as according to whether the data were collected at universities or in extra-

mural sectors. An average of 58% for questionnaire responses was achieved. (65% at universities). The rate of response for the unit heads interviewed orally amounted to an average of 86%. Since in many questions the questionnaire presupposed research experience as well as adequate training in one of the social sciences and/or experience in one specialty field, a major portion of the loss of respondents is due to the fact that the questionnaire could not be applied to those 'researchers' in a unit who had no research experience of their own or whose functions were primarily administrative. In these cases the worse rates of response do not imply a distortion with respect to the questions investigated in this study in that these persons do not belong to the target population of the communities of social scientists.

5. OPERATIONALIZATION OF THE CONCEPT OF CONSENSUS

For controlling our assumptions it was doubtlessly important to be able to operationalize the concept of consensus. The following aspects appeared to be relevant: on the one hand, the respondents had to be offered response categories which took into account the possible context dependence of the respondent's consensus perception. In other words, it was to be prevented that a sociologist claimed a consensus in a certain field, which was as 'strong' as a physicist's would be only because the implicit frame of reference 'sociology' made degrees of consensus appear high, which would receive a completely different rank if an interdisciplinary comparison were made. Secondly, the sociology of science concept of a 'paradigm' had to be disassembled into those components known to the respondent with which he would supposedly be able to associate convergence or divergence of opinions. Thirdly, if we wanted to check out on the assumption of an interplay between consensus and actual procedure in the research process independent of and irrespective of the discipline involved, the concept of consensus was not a priori to be linked with any discipline[13]. And finally a possibility had to be found to distinguish between the forefront of scientific events, which presumably is controversial in every field, and the potentially consentient decision making process on subjects which have been under discussion for a longer period of time.[14] The definite step towards operationalizing the concept comprises a series of questions on the extent of consensus as to different paradigm com-

ponents, which once refer to the most competent speciality field of the respondent (supposedly in a state of potential consensus), the second time to the concrete subject the respondent has been studying lately (supposedly characterizing the forefront of events). The selected paradigm components range from 'epistemological presuppositions' to 'generally accepted results'. In order to counteract the possible context dependence of the consensus perception mentioned above the question was not on the amount of consensus, but on the *number* of *different positions* existing on the subject within the relevant group of scientists.[15] In the following we shall primarily refer to those consensus items concerning the specialty field of the respondent.

6. CONSENSUS AND METHODOLOGY

The evidence of an interrelation between consensus and concrete methodological procedures in the research process is based on the question which sources of data collection, which methods of data analysis and what level of theoretical presentation of the results is used by the respondent, and to which extent[16]. In order to examine the role of time the question was posed for a period three years ago as well as for the last year. The individual items were combined into different indices, which all yield the same results as shown in detail in Knorr (1975). Here we shall restrict ourselves to a summary presentation of the results of three indices which classified the respondents according to the methodology used into scientists working *"dialectically\ 'empirically'* and *'analytically'*. All indices filtered out those respondents who had used the *same* procedures 'three years ago' as well as 'during the last year'. As a criterion for distinguishing the first two orientations mentioned Index 1 mainly used the question as to whether the social scientist's argumentational basis consisted of arguments stated in texts and historical sources, or of primary data collected by observation, experiment or interview.[17] Index 2 differentiated between dialectical, empirical and analytical orientations exclusively by drawing from the methods of data analysis used by the respondents during the last three years[18]. Index 3 was produced on the basis of Index 2 and was combined with the variable discipline in that the individual methodological orientations were aggregated only for those disciplines which were typical for them[19].

The indices elaborated were tabulated with the various consensus-variables while controlling for the existence or non-existence of practical research experience[20] on the part of the respondent, and the level of significance as well as the degree of association were calculated[21]. Thereby the hypothesis was confirmed that the respective methodological procedure correlated with the consensus perceived; in addition, we found interaction effects such that the significant relations mentioned were only observed, with some exceptions, when the respondent already had some practical research experience. The following figure illustrates for Index 1 the interrelation between methodological orientation and consensus perceived in the respective specialty field:

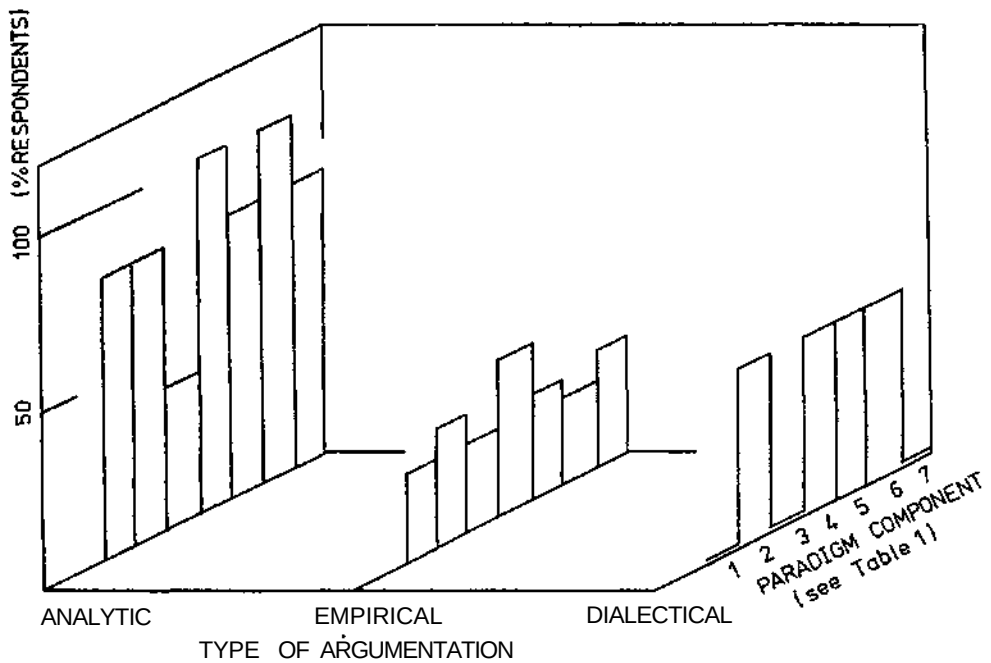

Fig. 3. Percentage of representatives of a specific methodological orientation (or type of argumentation) and consensus (Index 1).

Figure 3 shows for all items on consensus collected that the percentage of social scientists who for their specialty field claimed a high degree of consensus on specific fields of study[22] increases significantly from the adherents of 'dialectical' or 'empirical' methodologies to the scientists proceeding in a formal-mathematical ('analytical') manner. Thus not a single social scientist proceeding 'dialectically', whereas 80% of the formal

type claimed consensus on the central problem of their field of study. Or: only 33% of the supporters of an 'empirical' methodology, but 80% of the adherents of analytical procedures claimed consensus on the epistemologically 'proper' pre-suppositions of their specialty field or on the acceptance of methods and techniques respectively. Even for the theoretical approaches, the component of paradigms of social science with the evidently worst results, the percentage of social scientists proceeding in a formal - mathematical manner who perceived consensus on relevant theories is still three times higher than that of the scientists proceeding qualitatively (namely 40% as compared to 0%). The last mentioned share hardly increases for social scientists proceeding empirically and drastically confirms - in the self-evaluation of the social scientists - the well-known theoretical disorder of the disciplines in question. The percentage of respondents who claimed consensus on theoretical approaches is also the only one remaining below the 50% limit. There follows a summary of the associations between methodological orientation and consensus as yielded by all 3 indices (see Table I).

TABLE I

Degree of interrelation (Cramér's *V)* between perceived consensus and research methodology

Consensus in the specialty field with regard to:	Index 1 *N=31* Cramér's *V*		Index 2 JV=51 Cramér's *V*	Index 3 iV=17 Cramér's *V*
1. Central problem of the field	.47**	(.04)	.31*	.43
2. Epistemological presuppositions	.35*	(.30)	.29	.43
3. Theoretical approaches	.20	(.06)	.22	.39
4. Priority research problems	.41*	(.31)	.34**	.70**
5. Accepted techniques and methods	.39*	(.07)	.29	.32
6. Most promising techniques and methods	.57**	(.12)	.43**	.59*
7. Accepted results	.41*	(.08)	.32*	.65**

* significance <.10
** significance <.05

Table I presents the coefficients under the condition of practical research experience. The figures in brackets for Index 1 show the equivalent coefficients for respondents without practical research experience. A comparison of the two kinds of coefficients for index 1 shows (except for variables 2 and 4) that significant associations between perceived consensus and research methodology obviously are obtained only for those respondents who already have of practical research experience.

In other words, whether or not opinions are perceived as crystallizing into consensus or dissensus depends on whether practical decisions in the context of *comprehensive* research tasks have been *coped with.* Since practical research experience has been operationalized by means of the research 'projects' a scientist has completed, the wholeness and task character implied by the concept of 'project' stresses the importance of the role of entire series of interrelated research steps.[23]

7. THE ROLE OF THE DISCIPLINE

It was obvious that the interrelations obtained should first of all be checked with respect to the variable 'discipline'. Within the framework of this study the analysis of the role of the discipline confronts the difficulty of obtaining significant results in spite of the drastic reduction of the cell sizes linked with index construction and multivariate fragmentation. In the following nevertheless we summarize some of the results obtained:

(a) No significant relations were observed between consensus claimed and the field originally *studied* by the respondents; a partly significant, but not very strong, interrelation between perceived consensus and the discipline *practiced* at present, and a slightly stronger interrelation when these two fields *coincided*[2]*.

(b) If the responses to the consensus items are aggregated to an overall consensus index the following classification of disciplines[25] with a high degree of consensus is obtained (see Table II).

This order with slight variations is also confirmed for the other measures of consensus. It surprises on the one hand by the unexpectedly consolidated picture of history (or rather: modern history) and on the other hand by its agreement with the results of a survey carried out in 80 U.S. university institutes, a survey which, albeit being operationalized in a completely different manner, resulted in the same ranking of economy

TABLE II

Classification of the disciplines with respect to the percentage of respondents claiming consensus

Discipline	Respondents claiming consensus, in %	*N*
History	43.5	23
Economy	33.7	101
Business administration	28.8	66
Psychology	24.5	49
Sociology	17.1	70
Paedagogics	14.9	47
Political science	10.5	19
Urban and regional research	7.4	27
Significance	.01	
Cramér's *V:*	.21	

before psychology, sociology and political science and placed these social sciences all together behind physics, chemistry and biology (Lohdal and Gordon, 1972: 60)

(c) The interrelation between consensus and methodological procedure is generally stronger than between consensus and discipline[26]. Since in general the contingencies between methodology and consensus are maintained *within* the individual disciplines too, they *cannot* be explained by the effect of the discipline. However, deviations between the contingency coefficients in the various disciplines indicate an interaction effect.

(d) It is important that no significant interrelation exists between the evaluation of the cognitive capacity of their discipline by the respondents themselves and the degree of consensus claimed, not even then when the effect of practical research experience is controlled for[27] (see Table III).

If the opinions of the respondents are accepted as opinions of experts and hence as the best possible measurement of the *level of development* of the respective discipline, the low or missing contingencies imply that the fundamental thesis of Kuhn's theory on the interrelation between consensus and cognitive capacity in a discipline is not confirmed here. This would result in another, though negative, evidence in favour of our assertion of the *primary* importance of the structure of argumentation and the corresponding decisions in the research process. However, apart from

TABLE HI

Interrelation between consensus and the evaluation of the state of development of the respective discipline under the condition of practical research experience made by the respondents

	Cramér's *V:* (JV=112)	(A)
1. Central problem of the field	.05	33%
2. Epistemological presuppositions	.18	21%
3. Theoretical approaches	.11	13%
4. Priority research problems	.09	38%
5. Accepted techniques and methods	.10	31%
6. Most promising techniques and methods	.18	31%
7. Accepted results	.16	19%

(A): The percentage of those scientists who assert that their discipline has either 'not yet found a fruitful approach for coping with their specialty field' or that 'it is at present going through a fundamental crisis', but who nevertheless claimed a high degree of consensus.

the problem whether our question was an adequate operational instrument it is not at all clear whether the state of development of a discipline can adequately be measured by the opinions of the respective representatives.

Taken together, the results presented with respect to the discipline confirm our initial assumption that it is primarily the *methodological procedure* and the corresponding structures of argumentation and justification and not the discipline that we have to turn to if we want to explain the amount of consensus observed. However, it is likely that the discipline plays a not unimportant role as a context-variable influencing, through the type of its research object, the possibility of instrumentalizing methodological procedures for achieving cumulative knowledge.

8. THE ROLE OF PRACTICAL RESEARCH EXPERIENCE

As a complement to the role of the discipline we considered it necessary to carry out some further analysis on the unexpected importance of prac-

tical research experience. With regard to our original result, namely that the interrelation between methodological procedure and consensus depends on the existence of practical research experience of the respondent, we obtained the following additional results:

(1) There is possibly a *direct* influence between practical research experience and consensus in that the perceived consensus increases with the growth of experience.[28]

(2) The interrelation mentioned *cannot* be explained by the effect of the discipline. If the factor discipline is controlled, the coefficients in many cases increase, which is an indication for an original suppressor-effect of the discipline (see Table IV).

TABLE IV

Interrelation between practical research experience and consensus[29] within the individual disciplines

Discipline	Cramér's *V:*	*N*
Business administration	.32	44
Economy	.47	34
Psychology	.35	26
Sociology	.25	10
Modern history	.59	14
Political science	(.55)	7

Due to low class frequencies the results of some disciplines can only be interpreted as a tendency.

(3) Furthermore the interrelation mentioned *cannot* be explained by the *age* of the respondent. Whereas a tabulation of the respective consensus items with age did not yield any significant interrelations[30] the association between research experience and consensus was maintained within the individual age groups.[31]

Taken together, the results confirm that it is the actual involvement in chains of practical research decisions and argumentations and not the mere knowledge of certain types of procedures that is decisive for the development of an interrelation between consensus and methodology. We can throw some light on this result by pointing to the fact that structures of argumentation and justification can obviously only be effective if they are incorporated into actual practical decisions. On the other hand,

it is not perfectly clear to us why a several year's use of a certain methodological procedure corresponding to a specific type of argumentation does not result in the same interrelationship with consensus as research experience in terms of projects does.[32] Ultimate clarification of this aspect will have to wait for some more analysis.[33]

9. EXPECTATION STRUCTURE AND METHODOLOGY

Our hypothesis with regard to the structure of expectations was based on the following considerations: if the degree of consensus constitutes a function of the possibility of trivializing methodological decisions and this again ties up with the structure of argumentation processes, it is obvious that the expectations of the individual scientist underlying his results reflect these structural conditions - mediated and enhanced by the scientific control and reward system. Accordingly, representatives of a structurally 'open' methodology should be significantly more oriented towards argumentative dialectics than those scientists who apply structurally closed procedures of justification. In order to operationalize our assumption the scientists were asked which type of (positive) criticism they would welcome most; as response categories we offered the dimensions 'problem solving', exploration of 'fresh lines' of research, 'critical' discussion and 'insight' as well as methodical 'implementation' and 'argumentative' evidence.[34]

For the analysis the above mentioned indices of methodological procedures (see Table I) were applied. The results are summarized in the following table; for every dimension of the expectation structure the methodological orientation with the highest class frequency is presented. It is important that the interrelations are again only observed under the condition of practical research experience (see Table V).

As can be seen from the following table the adherents of a *'dialectical'* qualitative methodology show the expected orientation towards *critical disproof,* doubt or exposure as well as towards the dimension of *'insight* \ On the other hand, the adherents of an'*empirical'* quantitative procedure primarily go for the items encompassed by the concepts *'fresh lines'* and *'implementation*[1]*,* and the social scientists proceeding *'analytically'* consistently prefer the dimension *'problem solving'.* Inconsistencies are only observed with respect to the dimension 'argumentation', which the re-

TABLE V

Methodological procedure with highest class frequency per dimension of expectation

Dimension of expectation	Index 1	Index 2	Index 3
1. 'Criticism' and 'insight'	D	D	D
2. 'Argumentation'	D	A	D
3. 'Problem solving'	A	A	A
4. 'Fresh lines' and 'implementation'	E	E	E
Cramér's *V*	.30	.44	.49
Significance	-	.009	.12
N	(26)	(45)	(21)

'D': Representatives of a dialectical qualitative methodology account for the highest percentage of this dimension of expectation

'A': Representatives of an analytical methodology account for the highest percentage

'E': Representatives of an empirical quantitative methodology account for the highest percentage.

spondents possibly interpreted in different ways[36]. The following block diagram gives an impression of the respective percentage of respondents adhering to a certain methodological orientation who decided in favour of items of the first dimension (Figure 4):

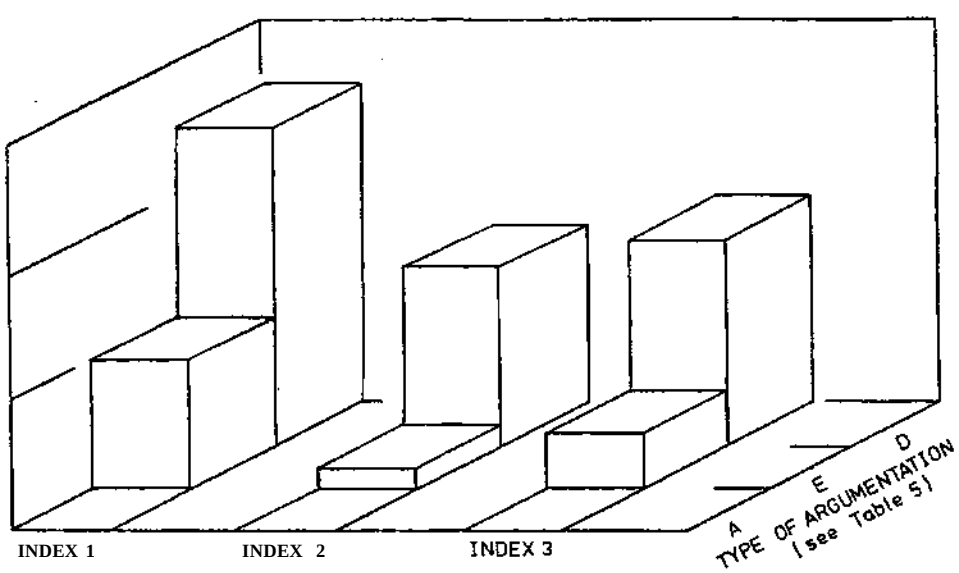

Fig. 4. Percentage of Respondents adhering to a certain methodological orientation (type of argumentation) and choosing critical discussion and 'insight' items.

Finally one more result is important for illustrating the role of the discipline with respect to structures of expectation: The degree of association between the latter and methodological procedures *within* the indivdual disciplines in general increases slightly, which clearly proves that it is again the *practical research procedures* and not the discipline that play the key role in determining expectation structures (see Table VI).

However, due to the low class frequencies the results can again only be interpreted as a tendency.

TABLE VI

Interrelation between expectation structure and research methodology within various disciplines (Index 2)

Discipline	F-Coefficient	*N*
Business administration	.58	15
Economy	.49	31
Psychology	.54	16
Sociology	.55	22
Political science	(.58)	(4)
Modern history	(.66)	(9)

10. SUMMARY AND CONCLUSION

The present paper has tried to explain the amount of consensus in a given field of social science by the *structures of argumentation and justification* entrenched in the methodological procedures used. It has been shown that the assumption of cognitive disunity among social scientists must be modified in that - depending on the methodological procedure used - extreme consolidation and extreme disagreement coexist side by side. The high degree of consensus can have two meanings: either the relatively extensive consolidation of areas proceeding formally is taken as an indication for the *'mature"* level of development of these areas in contrast to all the other fields of social science; or one accepts the thesis of the *potential gap* between consensus and substantial growth of knowledge. We asserted that the proved consolidation does though speak in favour of a cumulative selection leading to easily closed 'trivial' structures of argumentation and justification, yet does not guarantee substantial cumulativity. The latter depends on the functioning of *endogenous methodological control,* which

- in addition to the unresolved problem of measurement and meaning - does not seem to operate effectively in those areas of social science which ground their results extensively in theoretical terms. Instead of the oriented and directed falsificationism of orthodox normal science we are confronted in these areas with verification circles which pave the way for the production of a variety of divergent and dissonant results. The complexity and diversity produced is reduced by *current societal patterns of interpretation* : by means of the language of social science that is rooted in everyday language, and by means of social science theory formation that is rooted in everyday knowledge, they guide and govern the expectations of the scientists. Nevertheless, this influencing process is not unilateral. Both the possible accumulation of empirical generalizations in the realm of observational terms as well as the systematic articulation and elaboration of theoretical concepts and systems of knowledge in the area of individual disciplines guarantee feedback effects. It will be the task of further studies to examine these.

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NOTES

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[1] A system of norms integrated with the gratification system should imply that by allegiance to the norms a simultaneous maximization or at least satisfaction of one's personal interests can be achieved. However, the cooperative model of society of the functionalist approach underlying the assumption of successfully functioning norms in the above mentioned sense has been strongly criticized if not completely abandoned. Therefore, today, a normative foundation of scientific progress where the latter is based on socialization, operated by motivation and backed by prompt gratification is today less plausible than ever before.

[2] The triviality of decisions is a matter of degree. The concept is to indicate that the border-case of perfect triviality of a decision actually constitutes a reduction to a non-decision.

[3] The concept of 'argumentation' has been recently emphasized by Böhme (see his contribution to the same volume) as characteristic of scientific activity. But while Böhme analyzes the existence and function of interrelated arguments we are, by leaning heavily on Toulmin (1958), concerned with analyzing the *structure* of argumentation processes.

[4] As compared to the Hempel-Oppenheim reconstruction of the concept of explanation Toulmin's layout of arguments has the advantage of greater generality and therefore receives priority in our discussion.

[5] For a more detailed investigation into the meaning of different terms in methodological procedures see also Knorr (1974).

[6] The concept of social class belongs to the set of directly accessible concepts insofar as it is based on a conceptual combination of profession, income and education. The problems of classification and categorization, which appear regularly when measuring variables like that of 'profession' do not in principle rule out the possibility of measurement. The question is whether there is a sufficiently unambiguous everyday classification such that there are clearly identifiable links between 'facts' and 'labels'.

[7] Before returning to our initial assumption we would like to indicate that the distinction between studies based on observational terms and those based on theoretical terms is not equivalent with the distinction between 'descriptive' and 'analytical' (or 'theoretical', 'systematic') procedures. This, for instance, is shown by the 'purely descriptive' ethnographical tribe-monographs, which usually apply inadequate theoretical concepts, i.e. concepts borrowed from a different culture, such as 'economy', 'religion' or 'totemism'.

[8] We maintain that the structures of argumentation and justification are rooted in the methodological procedures of the individual fields of science insofar as these procedures circumscribe the codifiable and standardizable aspects of scientific activity to which the structures we describe doubtlessly belong.

We are using the term 'methodology' instead of 'methods' in order to stress the importance of the general approach or the structure of the procedure as opposed to the concretely elaborated versions.

[9] This assumption does, of course, not hold true in an unqualified manner: For instance, the use of analytical procedures (i.e. quantitative or formal procedures) must present a real alternative within the reach of the individual scientist if selective choice is to work. Besides, one can imagine a series of constraints which would still prevent such a selection. Here further analysis is urgently required.

[10] We borrow the terms convergent-divergent from the work of Hudson (1966, 1968).

[11] Compare Lakatos (1971) for an epistemological analysis of protective attitudes towards scientific results.

[12] Leinfellner (forthcoming) develops the argument that once a science has reached the theoretical state it becomes nearly immune to external influences while internal determinants begin to regulate the development of this science.

[13] For the reasons mentioned we could not demand from the respondent, like Lohdal and Gordon (1972) did, a rankordering of disciplines according to the degree of consensus on paradigms; nor could we use simple Likert-scales in order to determine the extent of consensus.

[14] Accordingly, the questions on consensus concerning the problems studied most recently generally ought to yield less consensus than the questions on the specialty field - which indeed turned out to be the case.

[15] The exact wording of the questions on consensus concerning the specialty field of the respondent is: "If you consider the relevant literature on this (meant is: your most competent) *specialty field*, how much *agreement* is there with respect to: - *the problem* which today ought to be considered as central for the field; - the *epistemological* presuppositions of the field; - the *theoretical* approaches which ought to be applied for exploring the field; - those *problems* which ought to be granted top priority; - those techniques and methods that can be considered as generally *accepted* - the most *promising* and useful methods for exploring the field; - the *results* which so far can be

considered as generally accepted." The respondent was asked to indicate whether the topic in question was 'practically not discussed', whether there was 'farreaching unanimity of opinion' on it, whether there were 'two very different basic positions', or 'several different positions'.

[16] The exact wording of the question was: "Please state to which extent you have used the following research approaches and methods (a) during the last year and (b) three years ago"; the response categories 'never', 'in some studies' and 'in practically all studies' were offered. The sources of data collection were: texts and historical sources, existing statistics, existing data from projects, observational procedures, questionnaires, interviews, experiments. The methods of data analysis offered ranged from the 'interpretation of texts and qualitative analyses' to 'purely mathematical procedures', the categories for the degree of theoretical presentation of the results from 'description' to 'axiomatic theories'.

[17] Or rather: Those respondents were classified as scientists proceeding 'dialectically' who had used 'texts and historical sources' in practically all their studies, possibly had also referred to existing data from projects and existing statistics, but had never themselves collected data by means of observations, experiments or interviews. On the other hand, those were considered 'empirical' scientists who in practically all their studies had made use of the latter methods of data collection.

The methods of data collection present the difficulty that they do allow for a distinction between data-oriented and literature-oriented procedures, but none between the retrieval of 'facts' and that of 'opinion'.

[18] Those respondents were classified as scientists proceeding 'qualitatively' or 'dialectically' who in 'practically all their studies' had used interpretations of texts and qualitative analyses, but never statistical or mathematical procedures. Those respondents who in practically all their studies had used contingency analyses, mean values, significance tests, scaling procedures, regression statistics or multivariate procedures were placed in the category 'empirical-quantitative' whereas those scientists were considered as proceeding on a formal-mathematical ('analytical') basis, who in practically all their studies had used purely mathematical procedures such as differential equations, matrix algebra, and the like.

[19] Or rather: The qualitative methodology of index 2 (see Table I) was attributed to the historians and political scientists, the empirical-quantitative one to the sociologists and psychologists and the formal-mathematical procedures to the economists and business managers. This classification is based on the appropriate correlations between these procedures and the disciplines mentioned.

[20] The practical research experience was operationalized by the question as to how many projects the respondent had concluded in his research unit. Thereby the concept of project did not refer to one distinct methodological procedure. The restriction to the last three years was made in order to establish a correlation between the respondent's specialty field of today and the consensus perception. Projects carried out jointly with others were not included in the analysis because of the danger of contamination by opinion formation processes in the project group.

[21] Apart from the X^1 test of significance Cramer's V was generally used as a measure of the degree of association, since compared to the Pearson's contingency coefficient and Tschuprow's Tit has the advantage of an upper limit 1. The formula for V^2 is:

$$\frac{X^2}{N\min(r - 1,\ c - 1)}$$

r meaning the number of rows, *c* the number of columns and min (r—1, *c* — 1) the smaller of the two numbers *r—l* or c—1. See Cramér (1961).

The X^2 test was carried out in spite of the fact that we surveyed the complete universe of Austrian social science units. Since our questions aim at generalizations which apply to the social sciences as a whole we must consider our respondents as a sample from a fictitious totality, which reintroduces the necessity of performing significance tests. (see Pfanzagl, 1967).

[22] The response categories of the consensus questions were dychotomized in order to prevent low class frequencies in the cells. The categories 'practically not discussed' and 'farreaching unanimity of opinion' were qualified as 'consensus', whereas 'two very different basic positions' and 'several different positions' were qualified as 'dissensus'.

[23] The interrelations between methodology and consensus with respect to the epistemological pre-suppositions as well as with respect to the problems which require top priority obviously exist irrespective of the practical research experience. Since in the case of the epistemological foundation we have to cope with a meta-problem that can be easily ignored in substantial scientific work, the irrelevance of practical experience does not surprise. The additional irrelevance with respect to the 'priority problems' might make it seem advisable to draw a dividing line between paradigm components 'external' to substantial science (meta-aspects, aspects induced by society, such as for instance the priority problems) and science-internal components, i.e. components which depend on research practice.

[24] The distinction between the discipline studied, the discipline practiced and the same discipline for both cases was made in order to take account of the different educational preconditions for social scientists, who are often working in fields that have been institutionalized and professionalized only recently, such as political science. The assumption was made that the correlation between discipline and consensus would be generally higher when the discipline studied and the discipline practiced coincided; this proved to be true. For the individual coefficients see Knorr (1975).

[25] If not specified differently the discipline practiced was used.

[26] The F-coefficients between the discipline practiced and the consensus-variables 1 to 7 in the case of practical research experience are: .30, .29, .19, .18, .22, .26 and .22; a significance of .05 or .10 respectively was achieved in the first two cases only.

[27] The 'cognitive capacity' of a discipline was operationalized by the question whether in the respondent's view his discipline 'has not yet found a fruitful approach for coping with its field', is 'at present going through a fundamental crisis', 'is progressing as a whole, but showing disruptions in some fields', is developing 'continuously' or is at present in a 'period of strong progress'.

[28] As the following contingency coefficients and significances (in brackets) presented in the order of the consensus-variables indicate, the interrelations are in general not very high: .16 (.01); .18 (.01); .16 (.01); .24 (.001); .08 (--); .14 (.05); .13 (.05).

[29] The ratings of consensus-items were aggregated to an overall measure for all mentioned consensus-variables; the table shows those respondents for whom the discipline practiced today coincided with the discipline studied.

[30] F-coefficients between the age of the respondents and the consensus-variables in the order of these variables: .13, .10, .09, .09, .07, .07.

[31] The F-coefBcients in the various age groups (=years of birth) for consensus-variables 1-7 run as follows:

Age group up to 1915: .20, .35, .18, .19, .29, .31, .32
Age group 1916-1925: .18, .54, .19, .23, .14, .31, .23

Age group 1926-1935: .45, .47, .30, .34, .23, .36, .15
Age group 1936-1945: .19, .13, .17, .25, .13, .10, .17
Age group 1946-1955: .19, .13, .25, .26, .15, .18, .08

Significances of .05 or less were mainly obtained in the strongest age groups 3 and 4. The differences between the age groups indicate interaction effects which call for further investigation but can be neglected for the present.

[32] This is due to the fact that the ^-coefficients frequently tend to increase when the diachronic indices reaching over three years are used as opposed to the synchronous indices, yet the formation of significant coefficients on the whole depends on whether the respondent has at all conducted research projects.

[33] However, the importance of finished research tasks can be linked with the fact that it is only within such tasks that the chains of argumentation and justification can be concluded with a chance of achieving consensus.

[34] The exact wording of the question was: "Assuming you receive a (rather positive) criticism in a respectable journal, which comments on your scientific work would you be most pleased about?"; the respondent was supposed to choose up to three of the following possibilities of response: "Brilliant argumentation..." (1), "Excellent counter-evidence...," (2), "Extraordinary approach..." (3), "Problem brilliantly solved..." (4), "Minute details..." (5), "Ingenious analysis..." (6), "Aggressive exposure..." (7), "Fresh line of research..." (8), "Attractive insight..." (9), "Unassailable implementation..." (10), "Pitiless doubt..." (11) and "Finally precise clarification..." (12). As operationalizations of the same aspect the items 4 and 12 were drawn together to the dimension 'problem solving', 3 and 8 to the dimension 'fresh line', 2, 7 and 11 to the dimension 'criticism', 6 and 9 to the dimension 'insight'.

[35] Possible divergent interpretations are: 'correct evidence and deduction' as well as 'good presentation'.

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