

# Reflections on Self-Reflection: In Favour of a Forward-oriented Self-reflection in Political Science

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## Abstract

*The present essay deals with a meta-meta-level in relation to political science, in that it reflects on how to reflect on the study of politics. 5 types of reflection on the products of political science are singled out: (1) descriptive mapping, (2) immanent prescription, (3) school-oriented transcendence, (4) problem-oriented transcendence, and (5) causal, "backward-oriented" transcendence. – They are discussed one by one, as regards their respective ability to offer guide-lines or recommendations for future research. They turn out to "score" widely different in this respect; it is concluded that we qua political scientists should stake on (1), (2) and (4). (3) and in particular (5) turn out to have a much more limited value, than it is commonly held. In this way, the essay should tell us how to invest our scarce resources of time and energy, when we reflect on the state of the discipline or the fruitfulness of a single theoretical perspective within it. Thereby, it should also have consequences for everyday research activity. In the final section, our conclusions are illustrated by way of a concrete research product in political science.*

Appeals for more – and better – self-reflection within the disciplines of political science and sociology have been frequent in European and Scandinavian journals in recent years; see for instance Lécuyer (1978), Boudon (1970), Berndtson (1975), or Sjöblom (1977). – It is stated by Berndtson that

"Although self-reflection is important in many ways, there has not been much of it in political science." (p. 178)

I think, we have had too much reflection of the wrong kind. Stated less frankly, I think that different *types* of self-reflection can offer different types of contributions to the discipline, and that we ought to favour forward-oriented types of contributions. What is meant by "forward-oriented"? Simply contributions which end up in answers to "then what"-questions for the discipline, the author or the tradition at hand, i.e. recommendations or guide-lines for future research, however vague they may be. We may distinguish between two types: firstly, recommendations as to what types of questions are likely to be most fruitful, and secondly, as to where answers to these ques-

tions are most likely to be found.

Intuitively judged, the last decade has witnessed a rapidly increasing interest in self-reflection in all the social sciences. This activity has been carried out under a lot of different labels, such as "epistemology" or "philosophy of the social sciences", "meta-sociology" or "Wissenschaftstheorie".<sup>1</sup> – Correspondingly, there has been an increased interest among philosophers in the epistemology of the social sciences.

It should be pointed out at the outset that we are dealing with reflections on intellectual ambitions and achievements, not on procedures or techniques involved in the accumulation and processing of data. Even so delimited, however, "self-reflection" may be said to cover a lot of different sub-species, whose respective ability to answer the indicated "then what"-question can be argued to be widely different. Of course, they may be evaluated differently in other respects.

## Descriptive Mapping

It seems natural to start out with a descriptive or "historical" mapping of the actual achieve-

ments (and perhaps ambitions) of the discipline (or tradition or author), we are dealing with.<sup>2</sup> This can of course be done in a variety of ways, depending on our specific purpose at hand ("pure" description is hardly more realistic here than in any other field of inquiry). At least four distinctions ought to be made: firstly, do we plan to follow up our descriptive enterprise with a prescriptive one (indicating what *should* be done), or are we merely interested in the description for its own sake (for instance as an impartial "history of ideas" for the discipline or a certain tradition within it). In the first case, a precisely defined set of descriptive categories will be required as basis for a sound prescription. In the second case, such a systematic meta-language may make the style of writing too stiff and tiresome. – Secondly, we may be interested in different *aspects* of the product at hand: its conceptual apparatus, its mode of explanation, its predictions, or its attempts at theory-building. – Thirdly, we should distinguish between the actual achievements and the ambitions of an author or a tradition, and fourthly, as regards actual achievements, we should distinguish between empirical substance and formal, logical attributes, the object of "une épistémologie positive"<sup>3</sup> (Boudon 1970, Sjöblom 1977), described by Boudon as follows:

"Bref, il s'agit de se livrer à une sorte d'analyse *grammaticale* de ces discours particuliers que sont les produits des sciences humaines, de manière à en dégager les règles et les structures implicites. . ." (pp. 249–50)

The distinctions drawn should tell us not to advocate in favour of a single language for descriptive mappings. Are we dealing with formal, "grammatical" attributes of explanation as basis of attempts at prescription, I think that the following set of categories constitutes one among other possibilities as a sort of mapping instrument: degree of abstraction, level of aggregation, recurrence (a recurrent relationship or something singular), extension (in time and or space), and changeability. These concepts can be applied both to our explanandum (what we want to explain), and what we employ as explanation for it (our explanans) (Mouritzen 1980 b).<sup>4</sup>

Whereas our descriptive mapping – performed one way or the other – is evidently a necessary condition for subsequent prescription (recommendations for future research), it is – by its very nature – not sufficient in itself: it requires an addition of some kind of normative ammunition.

There are different types of ammunition, and correspondingly different types of prescription. They will be treated one by one in what follows.

### Immanent Prescription

Immanent prescription accepts (at least tentatively) the epistemological and theoretical assumptions of the tradition or the author, we are dealing with.<sup>5</sup> From where do we get our ammunition of criticism, then? One source is logic, another is, evidently, our conception of the part of reality under investigation. (This includes, of course, substantive insights achieved in the past within our discipline).

Let us return to the set of categories concerning formal attributes of explanation stated in the previous section. The condition for its use in the present context is that we possess prescriptive propositions connecting its constituent categories. Such can only be established relative to a concrete field of study. As regards international politics, I have tried elsewhere (Mouritzen 1980 b) to establish some propositions. On the one hand, they are based on certain apparently enduring, empirical features of international politics: its anarchic character, the spatial fixity of nation-states in the international strategic system,<sup>6</sup> the relatively greater significance of the spatial factor in the strategic sphere than in the international economy, and the relatively fast rate of change in factors concerning decision-making in comparison with national "forces profondes" or structural attributes of the whole international system. On the other hand, there are certain formal, non-empirical assumptions (e.g., the more extension and the more-changeability in our explanandum, the higher degree of abstraction is required in explanans). – The resulting propositions based on these two sets of assumptions indicate – for each of 16 stipulated types of explananda – which kinds of explanans-factors are being virtually or probably excluded; they are not strong enough to give a positive indication. They can provide some vague guide-lines for future research, in that they can indicate where we should *not* waste our time in search of explanans-factors (at least at the outset), given our explanandum at hand. – There may certainly be constructed more fruitful instruments of immanent prescription; the point to stress in this context is only that the immanent type of activity under discussion in this section is indeed able to give guide-lines for future research, i.e. answer our "then what"-question. However, it is only

one part of this question, which it can answer: we are told what can explain what (or rather: what can probably not explain what); we are given no advice as to what is really a fruitful focus of explanation (explanandum). How can such advice be provided?

Within the framework of immanent prescription, we may be able to argue that the mode of asking questions is unfruitful, *given* the epistemological and theoretical premises, we have chosen for one reason or the other. In other words, "pure" logic in combination with our "credos" of different sorts provides us guidelines as to how we shall formulate our explananda, our crucial questions. This is, evidently, a forward-oriented type of contribution. But there are other sources of advice, which can complement this immanent type of activity by questioning our fundamental assumptions of different sorts.

#### School-oriented Transcendence

Transcendence of our fundamental assumptions may be organized in a variety of ways. One way is a school-oriented type of discussion. Disputes between different meta-sociological schools are to a large extent of an epistemological or ontological nature; i.e. they concern the study of human and social phenomena as a whole or perhaps even all human inquiry. Ideally, one could imagine school-oriented discussions as a kind of natural continuation of the above-mentioned descriptive mapping of a research product; we could criticize a given product in political science for instance from the standpoint of a given school, or we could tentatively adopt different schools as platforms of our criticism. Such criticism would by itself entail certain guide-lines for future inquiry. – However, school-oriented discussions such as logical positivism vs. critical rationalism, "Kapitallogik" vs. "Frankfurter-schule", or Marxist vs. non-Marxist epistemology (to take the most all-embracing and amorphous) have as a matter of fact been of little relevance for tackling "then what"-questions in political science.

There are several explanations for this. Firstly, due to different criteria of selecting "interesting" theoretical and empirical questions, representatives of the different schools will seldom pose the same explananda, which means that it will be unfruitful to compare the suggested explanantia. Secondly, arguments prescribing guide-lines for future development of political science are ultimately based on the epistemological or perhaps even

ontological assumptions of the school in question. These can – by their very nature – only be meaningfully challenged in philosophical discussions. This means that if there really is a court of last resort, it will be rather "far away", seen from the standpoint of "then what"-questions in a concrete discipline like political science (concerning the practical problems involved in this, see next section). – A third explanation is the more or less strong social ties within each school, sometimes of a clerical nature. These imply a concentration of efforts at cultivating one's own school of thought and its peculiarity vis-à-vis other schools, rather than at demonstrating its consequences for concrete "then what"-questions in a single field of inquiry. Boudon (1970) seems to have this point in mind when criticizing current reflection within the discipline for being too much bound up with "l'un ou l'autre des dogmatismes possibles" (p. 249). – A fourth explanation is the tendency in school-discussions to merge separate problems in the philosophy of social science (see next section), which makes them far too crude and clumsy.

I think, too many courses training political science -students in "Wissenschaftstheorie" are organized in a school-oriented fashion. Providing answers to "then what"-questions should be even more crucial in a pedagogical than in other contexts.

However, as the difficulties mentioned above are "only" practical (i.e. they are not logically bound up with school-oriented discussions), we should not apriori rule out that these discussions in some cases – pedagogical or other – may be successfully linked to "then what"-questions in a single discipline. But such cases are likely to be few.

#### Problem-oriented Transcendence

Instead of organizing our discussion in terms of schools of thought, this kind of discussion centers around one or the other separate problem-field in the philosophy of social science or epistemology, such as concept-formation, explanation, prediction, theory-building, the concept of truth, or perhaps even ontological-metaphysical questions such as the nature of social wholes, human action, freedom of the will, relation between language and reality, etc. In this way, we avoid the often disastrous tendency in school-oriented reflection to merge separate problems into too crude categories. We are enabled to put into perspective single, theoretical problems in a concrete disci-

pline, one by one (without losing sight of their inter-relatedness).

However, there is one problem which this enterprise shares with the school-oriented, namely how (and whether) to cut off the regress of justification, ultimately leading into "pure" epistemology or metaphysics. As regards this problem, Sjöblom (1977) has stated:

"Political scientists should use the philosophy of science in an instrumental way but we should use it as political scientists and avoid getting caught up in philosophical controversies . . . which may make us "overconscious" thinkers, unable to do research." (p. 12).

Although this statement seems to overlook the possibility of a division of labour within a research community between the "conscious" and the "overconscious", it obviously points out a genuine problem. – I would claim, however, that the mapping of actual political science products by for instance the set of categories delineated in a previous section and the subsequent immanent prescription is a way of committing methodology without necessarily committing philosophy. Those who are interested can then put into perspective or transcend the mapped products by studying the disputes among philosophers concerning one or the other of the "issues" mentioned at the beginning of this section, which is relevant for the mapped product.

Debates such as holism vs. reductionism or the nature of social wholes can hardly answer "then what"-questions for single research-products (immanent prescription being sufficient), but possible convergences in these debates should be kept in the corner of the eye by the discipline, so they can function as contributory guidelines for our long-term direction of theoretical interest.<sup>7</sup> – In any case, problem-oriented transcendence is a far more fruitful type of reflection than the school-oriented one, due to the practical advantages indicated.

There is a further type of reflection, which belongs to the present section. When saying that this or that research product, or this or that suggestion for future development of a research tradition, is "unfruitful", we often argue by referring to the "nature" of the particular subject under investigation, for instance the nature of the present international system. A research project may be criticized, because it has "overlooked" that nation-states can no longer be treated as black-boxes, or that it has tacitly equated the concepts of "fo-

reign policy" and "security policy". – Such criticism is not immanent (no logical flaws or faults in actual empirical findings are pointed out); it transcends the substantive assumptions of the project at hand (be they im- or explicit). On the other hand, it has a greater portion of empirical substance, than is the case with the more or less metaphysical debates mentioned at the beginning of this section (such as the nature of social wholes in general). – Obviously, this kind of "middle-range"-reflection is essential as a guide-line for the direction of future theoretical interest.

### Backward-oriented Transcendence

The two most frequently discussed meta-issues among political scientists seem to be the problems of the role of quantification in research (which shall not be discussed in the present context (see introduction)), and that of values and other "biases" in social science. Thus, the stressing of the importance of

" . . . mak(ing) people realize how their research is dependent on many different factors" (p. 179)

by Berndtson (1975) is often heard in scholarly discussions. This interest seems heavily inspired by the sociology of knowledge, the branch of sociology studying social conditions of knowledge (consult Stark (1967) or Coser (1968) for different subspecies of this discipline, or -as regards in particular sociology of *science*-Lecuyer (1978), the one most up-to-date).

Myrdal's well-known appeal for a normative social science – or rather several – based on alternative sets of explicitly stated value-premises is partly (and explicitly) inspired by the sociology of knowledge; if the values are not made into conscious and explicit premises for research, they will influence it in all its stages in an unconscious and biasing way (be they political, social, personal, or what not) (Myrdal 1969 pp. 3–5)

Let us first assume that Myrdal's appeal is epistemologically sound. It has been followed in a highly appreciated study within international politics by Kjell Goldmann (1971). In a review by Sjöblom (1971), it is pointed out that the value-premises stated are either self-evident (of the form "I have selected this subject for study, because I am interested in it"), or their consequences for the research product are not demonstrated (thereby remaining a sort of isolated confessions). A

third possibility is that their consequences are indeed demonstrated, and that these consequences in no way can be said to be self-evident. However, even in this case it can be argued (and has been argued) that the type of self-reflection suggested by Myrdal and others is unfruitful or impossible. Let us therefore return to the logic of the whole enterprise.<sup>8</sup>

There are some difficulties of interpretation both as regards Myrdal and the sociologists of knowledge, it should be stated at the outset. – If Myrdal does really claim to know that all asserted knowledge is value-biased (p. 9, 19), this must also be the case with this very claim itself. Accordingly, it is self-refuting (concerning this argument of reflexivity, see Grue-Sørensen (1950), and the rejection of criticism against it in Nordenbo (1977); it can be leveled against any all-embracing, causal theory of knowledge, be it sociological (Cosser 1968, p. 430), psychological, biological, or what not). – Now, let us be generous and assume that Myrdal's claim has no validity for itself (although no reason is given why this should be the case). We are still left with the problem of justifying some kind of unbiased, unprejudiced platform, from which we can unveil the hidden prejudices governing other scientific traditions or ideologies than our own. The following argument is leveled against efforts at solving this problem, in particular Mannheim's conception of a largely unbiased, "freischwebende Intelligenz".<sup>9</sup>

"... the main trouble about prejudices is that there is no such direct way of getting rid of them. For how shall we ever know that we have made any progress in our attempt to rid ourselves from prejudice? Is it not a common experience that those most convinced of having got rid of their prejudices are most prejudiced? The idea that a sociological or psychological or anthropological or any other study of prejudices may help us to rid ourselves of them is quite mistaken; for many who pursue these studies are full of prejudice; and not only does self-analysis not help us to overcome the unconscious determination of our views, it often leads to even more subtle self-deception."  
(Popper 1969b, pp. 222–3; first published 1945)<sup>10, 11</sup>

In fact, many of those wishing to perform a causal transcendence of the products of the social sciences display the most naive 17th-century empiricist positivism as in Bacon's "Novum Organum": if only we liberate ourselves and others from biases, prejudices of various kinds (*idolae mentis*), truth will become manifest.<sup>12</sup> The listings of sources

of "biases" in Myrdal (1969, pp. 3–5) and Bacon (1605, see fig. 1) exhibit a striking similarity, although somewhat different conclusions are drawn from the common starting-assumption: Bacon believes in one and only one science of manifest truth, when *idolae mentis* have been eliminated; Myrdal argues – as mentioned – in favour of several normative sciences based on alternative sets of explicitly stated value-premises.

Now, does not the logical and practical difficulties with causal transcendence (be it self-transcendence or directed against others) mentioned above lead us into sheer skepticism? Is it not necessary for any useful self-reflection to look backwards at the causal genesis of our research product, to investigate how biases of different kinds have influenced our research undertaking? No, definitely not. This can be argued by pointing to the well-known distinction between on the one hand questions of validity (and interest) of a research product, and on the other hand questions concerning its origin, its causal genesis, the confusion of which is often termed "the genetic fallacy". When discussing the plausibility of a given point of view or a whole research product, we should give *reasons* pertaining to its very content, not refer to *causes* (or motives) as to how this content may have come about, be they personal or social. This Popperian ideal of a "rational discussion" is attractive due to its simplicity, its depersonalization of research, and in the present context especially due to the fact that it makes a deed out of the epistemological (and practical) necessity to ignore "prejudices", which are not a part of the research product. We are in fact saying, firstly, that the search for (and attempts at elimination or making explicit) "hidden prejudices" is an impossible enterprise; and, secondly, that this does not matter, as only reasons pertaining to the content of the research product at hand has any bearing on its validity and interest. And evaluating the validity and interest of past research-products is a sufficient basis for setting up guide-lines for future research, in other words answering our "then what"-question.

Now, several comments and modifications ought to be made when trying to apply the simple ideal of a "rational discussion" in the present context. When saying that only the "content" of the research product has any bearing on its validity, this formulation should be understood in the widest possible sense. It includes assertions being *logically* (not causally) presupposed by the product,



Fig 1. Laying bare one's hidden prejudices is likely to be a long-lasting enterprise; there may be: "impressions of nature, which are imposed upon the mind by the sex, by the age, by the region, by health and sickness, by beauty and deformity, and the like, which are inherent and not extern; and again, those which are caused by extern fortune; as sovereignty, nobility, obscure birth, riches, want, magistracy, privateness, prosperity, adversity, constant fortune, variable fortune, rising per saltum, per gradus, and the like". Bacon (1605), cited from Coser (1968). Illustration from "Svenska Folksagor", VII, Stockholm 1945.

whether they are made explicit or not by the author or tradition at hand. If not, it is our duty as critics to point them out (by performing a logical, not a psychological or sociological analysis), and perhaps also discuss their plausibility. This involves precisely, what has been termed "descriptive mapping", "immanent prescription" and "problem-oriented transcendence" in previous sections. – In a recent study in the present journal of the dispute between "elitists" and "pluralists" in community power studies, it has been pointed out that not only factual, but also evaluative statements are being logically presupposed in the dispute (Falkemark 1978).<sup>13</sup> It is an open question whether this result can be generalized to other disputes within the discipline; the point to stress, however, is that by concentrating our efforts on "content" (in its widest sense), we have not necessarily excluded a discussion of values or perhaps even in some cases ontological questions, which may have a logical bearing on the outcome of the dispute at hand, and thereby the forward-oriented question of how to "go on" within the field concerned.

Another point to stress is that not all activities performed under the label "sociology of science" are hit by the above-mentioned argument of reflexivity (self-refutation) or the problems of justifying an independent "platform" that we have referred to. This is the case with the Mertonian tradition in the sociology of science, which – so to speak – keeps hands off the very *content* of science, instead investigating science as a social institution, including science policy, research organizations, relations to other social institutions, etc. (consult Berndtson (1975) p. 179 or the essay by Sjöblom (1977) pp. 9–10 for suggestions as to this kind of activity in relation to political science). It may teach us what kind of research organization is likely to offer the best conditions for creativity or efficiency, in other words it may be possible to answer "external" "then what"-questions for a discipline. However, it falls outside the conception of self-reflection employed in this essay, i.e. reflection on "internal" (intellectual) development of the discipline.

Lecuyer (1978) is a history of the sociology of knowledge, but also to some extent a "sociologie de la sociologie de la science" (one may really wonder, where this regress will stop). The Mertonian tradition is criticized, because it

"... s'arrête au seuil du sanctuaire inviol(able) de la science" (p. 324)

To Lecuyer's own relativism (inspired among others by Kuhn), the content of science is in no way "sacred" vis-à-vis causal transcendence. He advocates in favour of a sociology of knowledge aiming at

"... comprendre les mécanismes de sa genèse et de son développement (i.e. de la science), et l'on pourrait ajouter: de son fonctionnement effectif actuel" (p. 267)

without measuring it with any kind of general yardstick what so ever (which would be "profondement anti-historique", p. 267 (stated without further argument)). Lecuyer's position (representative of most sociology of knowledge) with no platform for his own attempts at causally transcending scientific content is hit by the argument of reflexivity previously mentioned.

Now, the reader may already have asked himself the following question: could we not avoid the arguments above by confining ourselves to a more cautious type of transcendence of content which does not seek to establish *general* causal relationships? Could we not just try to establish *singular* explanations pertaining to this or that particular content, pointing to influences from the surrounding scientific and social milieu or supposed motives of the researcher at hand, being responsible for deviations from purely intra-scientific standards? Support from a research foundation with certain interests pertaining to the subject studied may make it tempting to formulate problems which are unfruitful in relation to intra-scientific considerations, or to "overlook" certain aspects of reality; furthermore, motives as to the researcher's own scientific career may play a role. By not basing ourselves on any general and all-encompassing motive- or influence-theory, our own platform of motive- or influence-ascription may be exempted from its own type of activity; or rather; as long as nobody attacks it, it may be regarded as unproblematic.

Evidently, sneaking in our colleagues' motives or environmental background-conditions (or in the genesis of a whole tradition within the discipline) may have a heuristic function, as it may give us certain hints as to where we can set in with our criticism with possible success (and it may be funny and "interesting"). However, we should be careful not to commit the genetic fallacy; the suspected deviations from intra-scientific standards should be pointed out in the very content of the product at hand.

Can this kind of activity learn us anything as to future investigations within the field at hand, i.e. answer our "then what"-question? Hardly so, as this would presuppose that we had established certain probable *general* relationships between types of motives or social background on the one hand and substantive content on the other hand, so we could say, for instance: in the future, we should be on our guard against this or that genesis for research, because it is likely to lead research in an unfruitful direction. – But in fact, we do not have such a psychology or sociology of substantive knowledge (a science of "prejudices"), and we shall never get one, due to the practical and logical arguments, we have referred to (the "platform"-argument and the argument of reflexivity). The ambitions for such an enterprise are founded in a positivistic conception of "prejudices" as something evil, truth-destroying, which it is possible to lay bare by a special scientific activity.

It is a truism to state that fashions in the scientific community (or society at large) have influenced the content of political science in many ways. As regards the transition from "behavioralism" to "post-behavioralism" within political science, it is rather a question, if there have been *any* incentives what so ever pertaining to content. Historic studies laying bare how the content of political science has been moulded by influences of various kinds may be amusing, at the same time as they make us humble and pessimistic as to the discipline's possibilities of carrying out its ambitions (feelings which are certainly justified). But they are amorphous feelings, which can in no way contribute to answering "then what"-questions for the discipline. This would have required that they were based on recurrent relationships pertaining to content.

Let us sum up this section. The backward-oriented type of reflection preoccupied with the genesis of content may:

- 1) be relativistic, thereby falling prey to the argument of reflexivity
- 2) escape this, but then run into the problem of justifying an independent platform for itself
- 3) be epistemologically harmless, if it keeps hands off content, and concentrates on science as a social system, its external forms of organization

- 4) confine itself to singular explanations concerning this or that particular content, thereby perhaps offering heuristic advice for criticism of the product at hand (which, however, should be justified in the product itself). Cannot answer "then what"-questions, as it is dealing with singular explanations only (and would otherwise be hit by the arguments against 1) or 2))

The Yearbook of Scandinavian Political Studies, 1977, is devoted to political science in the Nordic countries 1960–75. Part I ("The Growth of the Profession") is an "external" history and sociology of the discipline (thus illustrating point 3) above); part II ("Trends in Political Research") illustrates our "descriptive mapping", but provides also singular environmental and motivational explanations of content (point 4) above).

#### **Self-reflection in Political Science: a Shrug of Shoulders or Telling us What to do?**

The object of the present essay has been to distinguish between different types of reflection on the state of the discipline, and to investigate their respective ability to offer guide-lines for future research, i.e. answer our "then what"-question. – The results tell us to stake on descriptive mappings of the discipline, immanent prescription, and as a natural complement to this the problem-oriented type of transcendence. The proposed set of categories is intended for mappings of formal attributes of explanation only, and even for this purpose, one can think of alternative mapping-instruments. The point to stress, however, is that it should make it possible to commit methodology without necessarily committing philosophy (i.e. the mapping procedure and the immanent prescription); those who are interested can then in turn provide the problem-oriented transcending reflection with relevance for the mapped products. At least as regards long-term development, it is advisable that political science keeps this kind of reflection in the corner of the eye. Equally important is "middle-range"-reflection, transcending the mapped products on a more substantive-empirical level.

The school-oriented transcendence, on the other hand, is likely to be an unfruitful enterprise in comparison with the problem-oriented, due to the practical disadvantages mentioned. – The backward-oriented, causal type of self-reflection is bound up with severe practical and epistemo-



logical problems (the problem of self-refutation, and the "platform-problem"). However, we need not feel sorry for this, due to the distinction between problems of the genesis of a scientific product, and problems concerning its validity and interest. – Two kinds of "backward"-activity are exempted from the above-mentioned criticism: the sociology of knowledge keeping hands off substantial content (dealing with outward forms of organization of science), and singular motivational or genetic explanations of content. Such may be of heuristic value for subsequent criticism of content of the very product at hand, but can hardly provide guide-lines for future research.

Two reservations ought to be made: we have been dealing with *relative* evaluations; i.e. none of the activities have been pointed out as positively harmful. I have tried to indicate in which type of self-reflection we should invest our scarce resources of time and energy. – Secondly, our evaluation is not necessarily valid from other criteria than the one here employed: for instance, school-oriented discussions may be useful for the philosopher, in that they can contribute to a *comprehensive* picture of a single school, elucidating the interrelatedness of different aspects of its philosophy.

Given these precautions, however, it is astounding, how much self-reflection in political science (in scholarly discussions, in journals and in opening chapters of textbooks) is backward-oriented, emphasizing procedure over product, circumstances of production over content, and how relatively little is forward-oriented, constructivist, in other words trying to answer "then what"-questions rather than ending up in a shrug of shoulders.

#### **An Illustration**

Brodin (1977) is a discussion of a theoretical perspective for studying official foreign policy doctrines, and two empirical applications. Furthermore, it is an attempt to sum up the substantive results of the whole series of doctrine-studies performed at the Swedish Institute of International Affairs in the late 60-ties and early 70-ties. This series of studies – as well as the other activities of the Institute – is devoted to subjects, which are likely to have a bearing on problems in contemporary Swedish foreign- and defense-policy (according to the prescriptions for the Institute, being partly financed by the Swedish ministry of foreign affairs). This strong "external" applica-

tion-orientation in combination with the fact that Brodin's study has a genuine and significant theoretical perspective (a rare combination, indeed) makes it possible to consider, whether and how the theoretical perspective has been "externally" influenced. In this way, we can illustrate some of the conclusions in this essay, and the distinctions between different types of reflection (in fact, Brodin herself has been engaged in most of these types; it is, I think, a good book (reviewed in Mouritzen (1980 a))).

The theoretical perspective employed treats official doctrines (statements in public by those officially responsible for policy) as a restriction on future foreign policy decision-making of the state in question. On the basis of this, it is hoped that it should be possible to provide – however vague – long-term predictions of state-action. – It is assumed that doctrines exert an autonomous restriction on future policy, in other words that they have a certain life of their own, due to the fact that decision-makers expect to be more or less "punished", if their policy deviates from doctrine-content. It can be argued (partly on the basis of Brodin's own reflections (Mouritzen 1981)) that this perspective is likely to be most fruitful when applied to pluralistic small-states in strategically vulnerable positions, where avoidance of threatening troubles in the past – such as involvement in world wars – is at least partly ascribed to the existing foreign policy doctrine. It may be lack of imagination, which makes one think primarily of Sweden in this context. However, it is not unlikely that the author has been unconsciously influenced by the special characteristics of the Swedish foreign policy doctrine of neutrality (the political consensus surrounding it, its almost religious status, its historical persistence), when designing her theoretical perspective. – The heuristic "a-ha"-experience after an observation of this type may make us alert of possible short-comings of the perspective vis-à-vis for instance an American context (the efforts of a new presidential administration to dissociate itself from important elements of a previous doctrine (for instance Carter's stress on human rights as distinct from Kissinger's 'realist' approach)). – Thus, by considering if there is anything "special" about the Swedish origin of the research product, we may become aware of boundary-conditions, which it would otherwise take longer time to detect. However, we should be careful not to commit the genetic fallacy, as previously pointed out: the advice offered is

"only" heuristic. – It may well be as Popper has pointed out:

"... contrary to first impression, we can learn more about production behavior by studying the products themselves, than we can learn about the products (i.e. the theoretical perspective) by studying production behavior (i.e. the causal genesis)" (1972, p. 113)

Can we learn any general lesson for future concern with doctrine-studies from preoccupation with the product's unconscious genesis? The only possible lesson is that Sweden-produced research may be Sweden-inspired, and this we should certainly know in advance. We may learn that we shall "be on our guard" vis-à-vis possible biasing influences, but not *how* to be on our guard. Our lesson is too amorphous to be of any constructive assistance as regards future research-activity. We can never predict, what kind of substantive content and fallacies are likely to arise from a certain genesis.

Another point of possible external influence is the way of applying the theoretical perspective. As a matter of fact, it is only used for predictive purposes, not post-dictive, although only the last-mentioned can provide an empirical test of it. As pointed out by Brodin herself, only "historic" studies with access to confidential government-considerations are adequate as test. – But how argue that a study of a decision in for instance 1914 is relevant for contemporary Swedish foreign- and defence-policy? – This is an example of a "negative" influence on the efforts at theory-building from the environment. But on the positive side, it can be said that the very existence of the interesting diachronic restriction-perspective is due to the environment's need for predictions. And diachronic theoretical perspectives are indeed rare within the discipline.

What can we learn for the future from this type of reflection? Next to nothing. We can learn that there may be "positive" as well as "negative" influences from the environment, but again, this is something we should know in advance. The lesson is too amorphous to give us any constructive assistance.

A third "external" influence is on delimitation of empirical study. Strictly speaking, the subject for study is not the U.S. doctrine as a whole, but those aspects of it pertaining directly to the European arena and the other superpowers (that is,

not the Vietnamese War, the Middle East, etc.) Furthermore, we are dealing exclusively with "high politics". – This demarcation could be "explained" by pointing to the prescriptions for research activity at the Institute (mentioned above), and this is in fact done by the author: the geographic areas mentioned are assumed to have the strongest immediate relevance for Swedish security policy.

A "rational discussion" of demarcation might tell us to cut reality in its "proper parts", i.e. try to get an impression of the U.S.-doctrine in its entirety, and the relative weighting of different geographic areas and different aspects of policy (a more restrictive sampling might compensate for the greater effort needed). Singular aspects (such as security policy) and regions could then be put into perspective vis-à-vis the doctrine as a whole (this argument may serve as an illustration of "middle-range"-transcendence).

How much of this criticism is heuristically inspired by a knowledge of origin? Not much, I think. We know in advance that demarcation of subject is an important matter, and this is so for all kinds of products, irrespective of their specific origin. We should be no less "on our guard", when there are no discernible extra-scientific interests at stake.

Knowledge of origin may be an "excuse" as regards the particular case, we are dealing with (if there really is something to excuse), but again, our lesson for the future is too amorphous to be constructive: we may learn (if we do not know in advance) that research organizations and foundations are likely to favour demarcations of subject which are in their own interest. We should know this in advance.

Whether to go on in the future with the indicated theoretical perspective, and how it may be modified, can only be adequately decided on the basis of a descriptive mapping of its content and a subsequent criticism. This may be heuristically inspired by a knowledge of its genesis, or it may not. – Has Brodin managed to carry out the long-term predictions (or post-dictions) across regime-shifts, which are inherent in her ambitions? If not, what can be the reasons for this? Can we become aware of new boundary-conditions for the perspective? – Are there theoretical or epistemological assumptions, which are *logically* presupposed by the perspective, which have not been stated? Does the ambition really – as Brodin herself suggests – require support from a

theory of doctrine-change? If so, we may consider, whether such a theory is a realistic ambition, empirically and epistemologically. The theoretical perspective may be said to be a combination of a "rational-actor"-assumption (decision-makers are supposed to make considerations of utility in the light of the existing doctrine (among other things)), and a role-assumption (the doctrine is treated as belonging to the state in question, not one or the other person, government or regime). What can we learn from the "great debates" concerning these assumptions, which have been carried out, partly in relation to the field of international politics in particular, partly on a more general level, relevant for all the social sciences? This kind of problem-oriented transcendence may be a complement to the immanent discussion delineated above.

The object of this final section has been to illustrate the conclusions in the previous sections, not to discuss Brodin's theoretical perspective. I hope, the short space available has not done too much injustice to it.

#### Notes

- 1 The corresponding English term "theory of science" has never won acceptance (apart from the "Department of Theory of Science" at the University of Göteborg).
- 2 Evidently, this mapping requires a meta-language, i.e. a language in which we can describe the language of the discipline, we are dealing with. – There is no less terminological confusion in the meta-language of social science than in the language itself; for instance, think of the several meanings of terms like "paradigm", "function" or "law".
- 3 I.e. "... une analyse systématique des produits de(s) ... disciplines (sociales)", a mapping of the "positively" given products. (Boudon 1970, p.249) A disadvantage of the term is its unpleasant flavour of positivism.
- 4 In the article mentioned, I have argued in favour of this set of categories as an instrument for description and prescription concerning explanations in international politics. – As regards the prescriptive aspect, see next section.
- 5 Evidently, we cannot expect that every author has made explicit his theoretical or epistemological assumptions. If not, we can get them from the "credo" of the tradition, to which he claims adherence (be it behavioralism, post-behavioralism, "Verstehende Soziologie" or a version of Marxism). There can also be lower-level assumptions, which are not stated, for instance belonging to one or the other "approach" within behavioralism.
- 6 The importance of this factor is relative to the state of weapontechnology, it should be added.
- 7 The argument in Eckstein (1975) against the comparativist "ideology" among certain political scientists may serve as an example of an attempt at direction of future research on the basis – among other things – of a convergence among philosophers of science concerning the "inductive fallacy". It is not at stake here, whether the argument is entirely fair in all its aspects.
- 8 I have not until recently been aware of the attack by Lane (1977) on Myrdal's theory of value-premises. Lane is dealing with Myrdal's production in its entirety, and the argument is more rigorously presented than is the case here. However, his central point corresponds in its essence to what is being said in what follows.
- 9 It should be pointed out that in some interpretations, Mannheim's position is said to imply a universal relativism (thereby undermining itself (the argument of reflexivity above)); in other interpretations, it is seen as "platform"-based (the "freischwebende Intelligenz" being the platform) (see Coser 1968). Adorno (1969) criticized the "Vulgärelativismus" of Mannheim and other sociologists of knowledge in the following manner:  
  
"Während die Wissenssoziologie, welche den Unterschied von richtigem und falschem Bewusstsein aufweicht, sich gebärdet, als wäre sie Fortschritt im Sinn von wissenschaftlicher Objektivität, ist sie durch jene Aufweichung hinter den bei Marx durchaus verstandenen Begriff von Wissenschaft zurückgefallen." (p. 137)  
  
As should appear, Adorno's own platform is sought justified in Marx-inspired sociology. It is the basis of the so-called "critique of ideology".
- 10 The term "prejudices" is in fact an inconvenient label in a Popperian context, as they are not seen as something evil which we should try to eliminate by a conscious effort (neither could we, if we tried):  
  
"Wir können dem Wissenschaftler nicht seine Parteilichkeit rauben, ohne ihm auch seine Menschlichkeit zu rauben. Ganz, ähnlich können wir nicht seine Wertungen verbieten oder zerstören, ohne ihn als Menschen und als Wissenschaftler zu zerstören. Unsere Motive und unsere rein wissenschaftlichen Ideale, wie das Ideal der reinen Wahrheitssuche, sind zutiefst in ausserwissenschaftlichen und zum Teil religiösen Wertungen verankert. Der objektive und der wertfreie Wissenschaftler ist nicht der ideale Wissenschaftler. Ohne Leidenschaften geht es nicht, und schon gar nicht in der reinen Wissenschaft." (Popper, 1969a, p. 114)
- 11 A more recent Popperian critique of the idea of causal transcendence can be found in "Die Logik der Sozialwissenschaften" (1969a), in Adorno (1969); in particular pp. 109–12.

- <sup>12</sup> Adorno (1969) is – in fact – referring to Bacon as regards the idea of "die objektive . . . (in der Analyse der Gesellschaftsstruktur ausweisbare) Determination falschen Bewusstseins" (p. 137). In Albert (1969), Adorno's justification of his own platform is criticized:

"Es is interessant, dass der Dialektiker hier zum eigentlichen "Positivisten" wird, indem er meint, er könne Probleme der Forschungslogik dadurch beseitigen, dass er auf faktische soziale Gegebenheiten verweist." (p. 216).

- <sup>13</sup> See also the subsequent discussion between Falkemark and the present author (Falkemark 1979).

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