1 CONTROVERSIES AND POLITICAL DECISION MAKING

René von Schomberg
University of Twente, The Netherlands

1.1 Introduction

1.1.1 Science: a functional authority?

The relation between science and governmental policy is one of functional authority. Whenever policy makers rely on the data of scientific research they admit their belief in a scientific system which has produced that information. This belief or trust in an authority is functional since it is not founded on the veracity and blessed knowledge of individual research workers but on the competency of a scientific system (Luhmann, 1973). This trust in the transfer of truth by science is based on a functional necessity. The complexity of certain questions and problems is so enormous that we have to depend on an intermediate party for the production of information. Such information can only be utilized in a meaningful way when available in a reduced and simplified form fit for practical purposes. The scientific system can fulfil a social function since science is acknowledged as being a reliable source of supplying and production of information. The appeal to an authority depicts this function. We do appeal to an authority in those cases where we lack the ability, or do not have at our disposal the means, to verify particular statements. In such cases social actions are freed from discussions. In situations where we are forced to act without delay we can make the right decisions because of our trust in an authority. We assume the truth of the statements of an authority and we orientate our actions in accordance with the truth of such statements. Social actions are, therefore, to a large extent made possible through a trust in such an authority. The belief or trust in the assignment of truth by persons and institutions could perhaps be regarded as a condition for the development and progress of any society. With the increase in the complexity of a society the necessity also increases, in any case for particular branches of knowledge, to accept such a trust in the knowledge of other persons and institutions.

In general we may say that an appeal to the authority of science is without problems. A policy maker will not, in most cases, have doubts about the expertise of the experts in question and will, therefore, see no reason to have the available research verified. This just about always happens when the scientific information available is non controversial. If the scientific information is of a non-controversial nature, there is clarity about what should be regarded as relevant information. It is, in addition, clear that the information is potentially available. This means that the data could, more or less, be anticipated: we know what conclusions could be inferred from what sort of data. Scientific research of a non-controversial nature has a pronounced problem solving character. Since the theories and methods deployed are not in question. The problems are regarded as solvable in principle. In this type of research theories are not tested; the person responsible for the research, however, is open to test. It is the scientist who has to sort out something. The point is that he/she has to bring to light the relevant data. If he/she is successful, the mistake/fault is not supposed to lie in a wrong theory; it is the scientist who is at fault. It is the scientist who has failed in the execution of his task. The ultimate differences of opinion amongst scientists concerning the interpretation of the research data can be reconstructed in terms of a *theoretical empirical discussion*. The arguments brought forward in these types of discussions have, in principle, a consensus enforcing power. In this type of discussion we normally use explanations and predictions. In such a discussion the truth of statements are at stake. For instance, when we are discussing the predictions of a weather forecast, discussions which indeed can become controversial, the consensus enforcing power of explanations and predictions are derived from the fact that we *know* under what kind of conditions we could accept the statement 'it will probably rain tomorrow in Holland'. For example, we could accept the prediction when somebody can explain that there is a depression above the Canal heading west. The simple point I want to make about this type of discussion is that the consensus enforcing potential of predictions and explanations are derived from the possibility to explicate the truth-conditions of the controversial statements. In the case of science as a functional authority we entrust the scientific system to clarify these conditions.

The idea of science sketched above, i.e. science as a functional authority, is however not always unproblematic. In this article I will deal with one type of circumstance which is relevant in order to revise the idea

of the functional authority of science. This refers to instances of scientific controversies. An analyses with the means of argumentationtheory will show that we cannot expect an agreement among opposing scientist under circumstances I am going to specify. Actually, it can be very unreasonable to demand such an agreement. I will make a proposal how to achieve a reasonable consent in the policy making process against the background of scientific controversies.

1.1.2 Scientific controversies

The functional authority of science is threatened whenever there are signs of a controversy. The belief that transfer of truth is a function of the scientific institution cannot be founded on conflicting truths of different associations of experts. Discussions in science usually unfolds around the question of how new knowledge could be obtained. In this context the available knowledge is controversial. This means that the certainty of previously accepted knowledge is questioned or regarded as limited. In such cases discussions in science could also include disputes about the methods to be employed. It is, moreover, not always clear what scientific discipline should lay a claim as to the best solution of the problem in question. Discussions about acquisition of new knowledge could be translated in terms of an *epistemic discussion.* The opposing scientist can only draw on arguments like analogies, authoritive appeals to scientific principles and counterfactual arguments which articulate uncertain and inadequate knowledge: plausibility[1]. The arguments brought forward in such discussions have no consensus enforcing potential because the experts are not able to explicate the truth conditions of statements because of a lack of knowledge. In an epistemic discussion, the controversy is not bound to the level of statement and counter-statement. Here we have to deal with a situation where a whole domain of knowledge, and even the methods of how we gain new knowledge, becomes controversial. In that case we will have to discuss the plausibility of theories and hypotheses with which the knowability of certain knowledge domains can be stated. The typical arguments in an epistemic debate do not serve the consent-achieving process but rather cope with the adequate disclosure of new domains of knowledge by constructing coherent theories, suppositions and hypothesis. In such a discussion, it is not the truth of statements which is at stake, but rather the *plausibility of knowledge-claims*[2]. Within an epistemic debate, which is

about to be cleared, we can only expect a reasonable dissent but not a reasonable consent. The conflicting knowledge claims of the experts constitute epistemic uncertainty. Decisions within the field of policy making realised against the background of such discussions will, therefore, be subjected to these conditions of uncertainty. The uncertainties of epistemic discussions can lead to an inducement for a public debate. Epistemic discussions do not necessarily become public. However no one will be surprised that it is this type of debate which will have societal consequences whenever the quest for new knowledge depends on important social choices. It is more obvious, when one thinks of the increasing critical public awareness of developments within science. I will talk about a scientific controversy if epistemic discussions induce public debate.

I will start with a discussion of the structure of epistemic discussions. This I will do with the help of an example: the risks of the deliberate release of genetically engineered organisms. Since the middle of the eighties this induced both an academic and a public debate which are still going on[3] (Von Schomberg 1992b). I will not make a complete reconstruction of this debate. It is my first intention to clear the general problems of epistemic discussions. However, my presentation should clarify that all possible factual arguments can actually be covered by the concept of an epistemic discussion which I will explicate. The concept of an epistemic discussion can be utilized in different debates. This explication will be followed by some ethical questions and problems of legitimacy which arise in the context of scientific controversies. It can be shown that these problems can not be adequately dealt with in the usual policy procedures. Finally I will propose a solution which is inspired by the framework of the so called 'discourse ethics'(Habermas, 1983; Apel, 1988)

1.2 The epistemic debate about the ecological effects of the deliberate release of genetically engineered organisms

Epistemic discussions features a multidisciplinary aspect. In this case we have to deal with the competing claims of molecular biologists and ecologists. It discloses two perspectives on the new knowledge-domain of the ecological effects of the deliberate release of genetically engineered organisms.

1.2.1 Authoritive appeals to scientific principles versus analogies and counterfactual arguments

Both scientific disciplines use analogies in order to disclose the new scientific field. The different analogies lead to different assessments. Both analogies are mutually rejected on the grounds of an authoritive appeal to a scientific principle which disqualifies the experience brought in by a particular discipline. The scientific principle itself counts as undisputable within the boundaries of each discipline.

The analogy of the ecologist runs as follows:

> If one wants to judge the risks of spread of engineered organisms, one has to evaluate the chance in how far organisms can leave their predestined paths and effect the structure and function of a ecosystem. The introduction of exotic(problem) plants provides a basis for such an evaluation. In both cases naturally limiting factors for the spread and establishment are overcome by organisms.
>
> *knowledge-claim: so the experience with exotic plants provides a basis for the evaluation of the risks of spread and establishment of engineered organisms.*

The molecular biologist counters with an authoritive appeal to a scientific principle:

> One reason why critics urge caution over the release of genetically engineered plants is experience with problem plant species. By means of genetic engineering, in contrast, the organism, rather than the environment, is changed; the problems do not originate from changes in the genetic make-up of the plant but from introduction into a new environment.
>
> *knowledge claim: the experience with problem-plants is not relevant.*

The molecular biologist uses a different analogy:

> Predictions of the risk of deliberate release can be based on the experience of traditional practices in agriculture (like plant breeding). In traditional plant breeding the exact genetic changes are unknown. In the case of genetic experimentation, however, the specific modifications can be characterized. Plants have been crossed (traditional genetic engineering) by man for cen-

turies. New variants resulting from such breeding have not caused serious problems. Some crosses include those that would not occur without man's intervention. Breeders have never taken and do not now take special precautions in testing these plants in the field because they know from experience that these extensive mixings have not produced serious problems.

knowledge claim: there is no reason to expect that engineered organisms could cause greater problems than traditional techniques.

The ecologist, on his turn, can now bring in an authoritive appeal to a scientific principle:

The ecological consequences of the introduction of engineered organisms can not be predicted only with the knowledge of the genetic structure of an organism (= rejection of a scientific principle). Therefore one needs to know the biological properties of the organisms and their chance to reproduce and survive in the environment (relevant scientific principle)

knowledge claim: the knowledge of the genetic structure only can not be the basis for a successful prediction

The arguments used by the experts provide the means for an open ended discussion. The methodological analogy of the molecular biologist is encountered by an appeal to a scientific principle of the ecologist. Vice versa, the methodological analogy of the ecologist is also rejected by an appeal to a scientific principle of the molecular biologist. These arguments do not have any consensus enforcing power. Actually, one could as a non-participating scientist agree with *both* the plausibility claims of ecologist *and* the plausibility claims of the molecular biologist[4]. In the actual debate, this insight of the argumentation theorist that the plausibility claims do not *effect* each other, will not be articulated since the experts can only demonstrate their loyalty and adherence to their specific discipline.

It is not a coincidence that we have to deal with authoritive appeals and analogies in the context of epistemic discussions since they have a specific argumentative function. In the use of analogies one makes a case for methodology. The molecular biologists say that the risk issue should be studied in terms of the genetic characteristics of an organism. The ecologist maintain that one should study the biological properties of a organism. The different analogies refer to the different methodologies of scien-

tific disciplines. These methodologies are indisputable within the boundaries of the disciplines, but are not self-evident for disciplines which claim knowledge of the same issue. On the contrary, in a large number of practical debates one rejects the opponent's view as 'unscientific'. In a dispute about methodology, there is no discussion about the truth of statements; there are, however, different claims as to how new knowledge should be acquired. The different disciplines develop their own perspective on this problem. The use of an appeal to a scientific principle is a comparable case. The 'scientific principles' are, again within the boundaries of a discipline, acknowledged as reliable sources, but as one transgresses the field they can become controversial.

In the use of analogies we mobilize knowledge from well-known areas of research. In our case the ecologists do not have the knowledge in order to predict the ecological consequences of the introduction of engineered organisms. Therefore they try to mobilize knowledge (using an analogy) from the field of the introduction of exotic plants. Such an analogy enables ecologists to constitute a domain of possible relevant facts. In this way analogies have the function of the mobilization of knowledge. In the use of an appeal to a scientific principle we are also confronted with the possibility to claim knowledge. In this case an appeal to a principle enables us to get access to a certain problem. In complex scientific issues we are confronted with a whole domain of inconsistent and incoherent data which cannot all be assessed. An appeal to a scientific principle can reduce the complexity of the issue. In this way we can make an issue suitable for research. So analogies and authoritive appeals state the possibility to do research in new domains and to anticipate the relevance of data which are still to be gained.

We have seen that the arguments put forward do not actually effect each other but only articulate the epistemic uncertainty of the new field. A counterfactual argument is the only means at our disposal, to doubt the plausibility of a claim. This argumention form can force us, sometimes in the form of a reductio ad absurdum, to reconsider the premises of our arguments. In an epistemic discussion the molecular biologists make, for example, instance an appeal to the principle of adaptation in evolution-theory: "Pre-existing organisms compete successfully with genetic engineered organisms in the environment because the former are better adapted to the environment". The ecologist can challenge the plausibility of the claim of the molecular biologist with a counterfactual argument:

Suppose: it is correct that pre-existing organisms compete successfully
it follows: if pre-existing would compete successfully then it would be impossible for engineered organisms to persist
but: genetic engineered organisms are designed to have a function in the field and therefore they obviously can not be out competed too soon

so: it is plausible that for genetic engineered organisms to be of any use, they must at least persist to some extent.

It may, in the case of counterfactual argument, be easier to demonstrate that we have to deal with plausibility claims. It does not make sense to judge the conclusions of a counterfactual argument in terms of truth conditions. In a counterfactual we do not start with a *proposition* but with a *presupposition* which is announced by: 'let us suppose that' or: 'suppose for the sake of the argument that.' In a counterfactual argument we could even start with a premise which involves the knowledge-claim of a whole theory: let us presume this theory is true. In the case of epistemic uncertainty, we are most likely to encounter this kind of argument. The imagination of experts, oriented to (subjunctive) thinking of what might happen, is challenged by the unknowns of the issue. Plausible arguments, which are neither inductive nor deductive, do not feature qualifiers[5]. Qualifiers normally, depict the conditional character of truth claims. Yet, in the context of plausible reasoning they do not make sense. We cannot say that something is *presumably* plausible. We can neither say that something is *generally* nor that something is *obviously* plausible[6]. These remarks reinforce our intuition that 'plausible' unlike true is not a special predicate of statements but rather of knowledge-claims.

In epistemic discussions we have to make use of the weak arguments mentioned above. They are more or less indicators for a fundamental lack of knowledge. They all have a function in the acquisition of knowledge, especially in the context of controversial knowledge-claims. It is important to notice that these arguments can not establish conclusions. They are brought forward to make plausible and promising proposals, still in need of further investigation.

1.2.2 Prospective plausibility claims

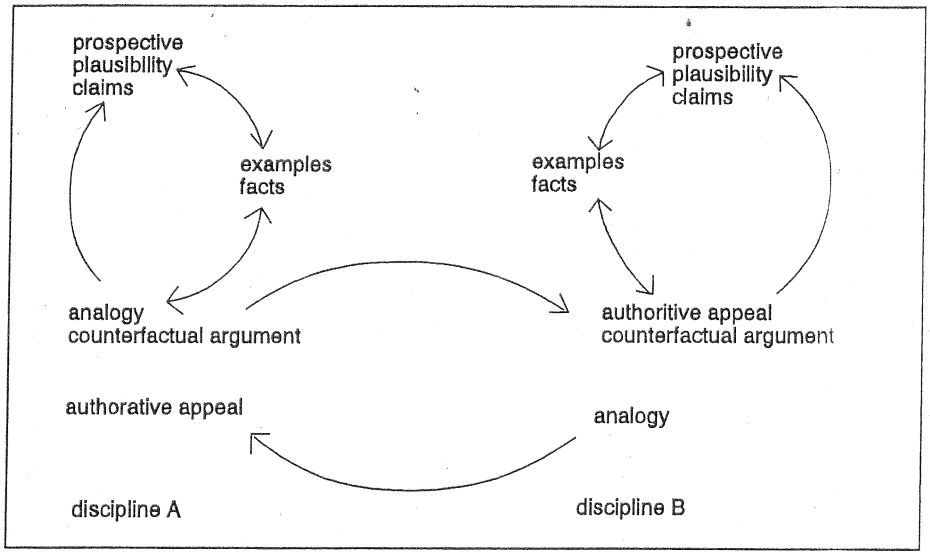
On the basis of analogies and authorative appeals to scientific principles expectations having the appearance of predictions can be made. Molecular biologists; e.g., appealing to the principle of adaptation in the theory of evolution claim, for instance, prospective plausibility for the assertion that genetically engineered organisms when released in the environment cannot overcome the pressure of selection of already existing organisms. Ecologists, however, maintain that on the basis of their experience with the introduction of new species of plants we have to consider similar unforeseeable catastrophes as had occurred with the introduction of exotic plants. The claim to predictions (in the theoretical scientific sense of the word) can only be made under conditions of presumed valid knowledge. This, however, is not the case since only plausible principles are available which still have to be found acceptable in this new field. This important epistemelogical difference has real pragmatic consequences. The corroboration of predictions can be seen as a test of the law-like statements involved, and, would then count as an element of a discourse about truth. On the contrary incomparable prospective plausibility claims can in the long run only be qualified as pragmatic selfconfirmations of their own special paradigms. The competing plausibility claims of course refers to the same subject matter but cannot be related directly to each other, and, what is more, the non- confirmation of a prospective plausibility claim can have no falsificationary value[7]. According to my opinion the examples as well as facts incidentally introduced in arguments and having an illustrative function only, should be seen in a similar way. They have no falsificationary power they rather justify the attempt to investigate the same subject matter from different authoritative points of view of the different scientific disciplines in question. Figure 1 represents a view of the structure of an epistemic discourse.

1.2.3 The definition of problems by the experts and the inconsistent recommendations to the public and the political arena

Ecologists and biotechnologists put forward different recommendations to politics in accordance with their particular scientific argumentation. Biotechnologists advise that a no particular regulation of the new technique is necessary: the existing control of the traditional methods for plantbreeding guarantees sufficient certainty given the analogy with natural and traditio-

nal processes for breeding. The advice of ecologists is found by their argument on the data that a prediction as regards the ecological effects of a release into the environment is impossible in principle: new tests should be developed to investigate the products of genetic technology with respect to the case in question. Both scientific groups demand a 'science regulated policy' but put forward unavoidable incompatible assessments of the problem at hand[8]. Ecologists define the problem as being a new problem while biotechnologists maintain their own principle everything always remains as it has been.

Figure 1 The structure of epistemic discussions



Moreover the biotechnologists propose that decisions be made in accordance with available information whereas ecologists believe that a new definite decision can only be made after new knowledge has been acquired.

1.3 Epistemic discussions within the context of contemporary political decision making

In justifications of decisions in policymaking we without any doubt mostly employ a form of argumentation which could be reconstructed in terms of the well-known model of rational decision making. The idea of scientific agreement as the foundation of a rational policy could be found in the model of rational decision making which e.g. forms the basis of cost-benefit analyses. In this model we start with given problems where the policy maker has at his disposal clearly defined aims of policy. Besides this, he also has available a list of alternative means whereby each of these aims could be realised. His choice of means is based on specified rules of preference and selection. Scientific information occupies an important position in this model since it determines the possibility of realizing the aims of a policy and the effectiveness of the means. The quality of a policy is expressed in terms of effectivity and efficiency. The ideal conception, forming the basis of that rational model of decision, rests on the assumption that an agreement (consensus) amongst scientific experts guarantees the quality of the policy.

These important assumptions cannot be made when we would have to make an appeal to information borrowed from an epistemic discussion in science. Against the background of epistemic discussions there appear the uncertainty of our present knowledge: the principal incompleteness of the information and the inconsistency of the data. The scientific information can now constitute a strategic source which, dependent on the different political options, could be interpreted differently. Within this context of the contemporary political decisionmaking and the epistemic discussions we can find the following phenomena whereby the inadequate rationality of societal actions could be catalogued.

1. An inadmissible translation of the data from an epistemic discussion to expressions having truth functional or probability characteristics (or the translation of illustrative data in proof, or the transformation of dangers in risks)[9]. This can be seen, e.g., in the case of the translation of plausible knowledge claims to predictions with probability characteristics. Such 'probabilities' must then, again and again, be adjusted to suit new events, states of affairs, and catastrophes. This insight is generally acknowledged

after the Chernobyl event. In our example of genetic engineering the data of field experiments have to prove the safety of genetic engineering.

2. In a particular policy preference is given to a discipline that can provide facts (data). The hypothetical dangers formulated by opposing scientific disciplines do not sufficiently flow into the process of decision making[10]. The emphasis on the inadequacy of our knowledge constitutes a loss of scientific authority for that particular discipline rather than a problem for political decision making. The discipline which accords with the subjective preference of the policymaker is the winner (decisionism). In Europe we come across the remarkable situation that what is dangerous in Denmark (a stop (moratorium) on the deliberate intentional freeing of genetically manipulation organisms), is regarded as inquestionable in Italy (the absence of any control; at least depending on an eventual EC control).

3. When an epistemic discussion attracts the attention of the general public the result is a quite often an unproper politicising effect on scientific debate. This can be shown on the one hand in the irrational struggle concerning the data: interest groups look for support from experts who share their political objectives. On the other hand one reacts affirmatively on the presuppositions of the model of rational decison making[11]: as long as the controversy continues, there will be no decision. Depending on the political preferences one feeds a controversy with explosive new data or one tries to escape scientific dissent. It can also be the case that interestgroups call for political decisions, whereas in the policy making process one refers to a controversy in order to delay decisions. I do not want to reinforce the wide accepted notion that in the case of controversies, interests come into play. I rather want to stress a point what should be important for a theory of society, the fact that science has become a resource for strategic action. It looses its functional authority. This means that science looses its function of unburdening societal actions from (unnecessary) discussions. In the policy making process a contradiction arises: an appeal to science seems necessary (because of the complexity of the issue), but is not possible (since there is a controversy) and what is impossible (an appeal to a source which can provide authoritive data) becomes necessary.

4. The sequence of the actions in the policy: collecting the facts before making the decisions leads to a situation where scientific unsolved or

unsolvable problems do not appear on the agenda. Science is put under pressure to produce hard facts and, that, in those fields where we cannot expect them to do that. What is wanted are 'one handed scientists'(Rip,Groenewegen 1988, p 149-172)

5. The scientific debate amongst disciplines for claiming acknowledgement and authority as regards a new field of research is threatened to deteriorate into public campaigns for recruiting declarations of sympathy. Biotechnologists promulgates the promises and blessings of the new technology while ecologists do the same for a threatened environment.

6. Even the legal system seems to be unable to cope with the problems. This is apparent from the following:

a. The principle of the causal agent (blame) cannot be applied. Eventual "actors" nor "victims" can be identified when using the new technology. It is, e.g., impossible for a victim of the Chernobyl disaster in Europe to go to a court of law and claim that the disease from which he is suffering has been caused by nuclear radiation.

b. Legal norms can no longer be controlled in practice. (The observance of standards is often the result of informal agreements (and negotiations) between public authorities and individuals. In some cases legal norms cannot even be defined. In most of the western countries the admissibility of the maximum amount of radioactive radiation in the vicinity of nuclear reactors is determined by what laws concerned with atomic affairs refer to in terms of 'the most recent state of affairs in science and technology'.

c. The legal system can no longer fulfil a normative role as regards the admissibility of technological actions. this is very well illustrated in a judgement of the "Bundesverfassungsgericht"(supreme court) in Germany concerning the controversy about the Kalkar plant: "It is not the duty of the lawgiver to determine the possible kinds of risks, factors of risk, the methods to determine such, or, fix the limits of toleration". It is obvious that the judge has shifted this problem to the realm of politics(Wolf, 1991).

d. The conflicts in society cannot be settled under the conditions of the equality of power of a judicial judgement; it is left to a social unequal power struggle where human beings depend on the responsibility of individual citizens. This is a difficult and in the future undoubtly unrealizable task due to the fact that the risks of the new technologies can no longer be observed by the individual citizen.

Against the background of these phenomena there arise the problems of legitimacy confronting the planning state who, on the one hand, can no longer agree with the definitions of problems of interestgroups, and, on the other hand, does not know how to cope with the disapproval of the overpowering processes of innovation about which the citizens cannot make decisions. The problems of legitimacy is partly compensated by the tendency of public management to negotiate with different groups in society. Examples are to found in the representation of such groups in the councils of health and environment. I do not expect spectacular results from mutual concessions reached during the negotiations since such concessions normally arise wtihin the framework of strategic actions and unequal conditions of power. It is far more important to note that on this road the question concerning the way to solve by means of a justifiable procedure the historical new problem of making decisions under conditions of scientific uncertainty is abandoned. The fact that we do need a procedural solution is apparent since there does not yet exist an accepted institution in society that could determine which actors in what way would participate in the decisionmaking within the context of scientific controversies. In the last paragraph I will try to show that the use of a procedural process applied to such problems would generate the general framework for a justifiable solution. In this way the problematic phenomena (mentioned under 1 and 6) could be eliminated.

1.4 A discursive procedure under the conditions of administrative law

The socalled "discourse ethics" could provide an understanding of the way to answer questions about just procedural solutions without getting stuck in ethical partial criticism of technology (from abortion to nuclear energy) or

the dogmatic unwillingness to make certain values subject of an argumentative test. Within discourse ethics one argues that a material ethics maintaining that one could, for once and always, prescribe norms cannot be founded on arguments. History has taught that our ethical and scientific insights have, time and again, been shown to be fallible, and, that those insights had to be revised in the light of new situations and problems. The validity of norms can no longer be derived from sources that have been regarded as infallible. The validity of norms should rather be sought for in the free mutual acknowledgement of these norms by (potential) discussion partners as this could only be found in discussions. The conditions for the acknowledgement of norms is, therefore, of the utmost importance (as a matter of fact this is true for all procedural theories of justice and democracy). In one particular sense we could put the case that the conditions for a discussion are simultaneously the conditions for a rational agreement (about norms). In the light of our question about the way to reach an agreement about policy, given the background of epistemic discussions, we can make a list of some conditions which have to be fulfilled in discursive procedures.

1. In the analysis of the structure of epistemic discussions we have to establish the idea that there should be an acknowledgement of scientific disagreement. It is not reasonable to expect that scientific experts on the basis of scientific insights will be in agreement in the near future. This means that in procedures concerned with policy one sided participation of scientific experts cannot be justified.

2. The problem of legitimacy cannot be solved if some of the possible parties concerned with the politic process are excluded. Another condition is that all parties should have equivalent roles[12]. Experts should therefore only be allowed to supply information for the discussion and should not have an advisory or determinative function as regards policy.

3. Decisions having irreversible consequences for non-identifiable groups or future generations who are not able to participate in the discussions should carry the burden of a heavy legitimacy and should, if possible, be evaded.

Discursive procedures implies that those norms will be rejected which seems to exclude the universalisable interests in anticipation. In discursive procedures the norms are not grounded, but selected negatively.

4. All relevant aspects of the problem should be dealt with within the framework of a discursive procedure. Next to the eventual scientific problems field the following are also relevant:
ethical questions such as: what options are desirable? (in general: how do we want to live?)
moral justification such as: what norms should in the interest of all be included (e.g. questions about the division of risks in the society)
questions about justice: What (social and technological) aims should be promoted or limited within the framework of the rules of law. (Example: should biotechnological research be controlled by legal means?)

During the short history of technological policy in the Western countries we find a few proposals to develop procedures of this problemfield. During the seventies there were heated discussions about a Science Court (especially in the United States). The idea being an occasional body where scientists from different fields could act as judges in order to reach agreement, however, with the promise that normative aspects should be kept out of the discussions. This idea however was never realised in an institution.

In the light of our foregoing analysis such a body would not be able to contribute towards the solution of the controversies since the procedure in question is based on the assumption of an agreement amongst scientists if the normative aspects are set aside. That this was an illusion became evident very soon. Another form of developing procedures, however, was institutionalized. This took the form of the so called Technology Assessment. Initially this implied the establishment of an instrument of planning where the expected effects and side-effects of technology could be mapped and used as input in the process of making a decision. In this case the possibility of a rational consideration of the pro's and con's of technology was the guiding light. An Office of Technology Assessment was set up as early as 1973 in the United States. This office is an advisory board for the American Congress. In some European countries there exist similar offices; in the Netherlands, e.g., since the middle of the eighties. That office can give advice to the Minister either voluntary or by request. This

development of a procedure could be seen as the first attempt at institutionalizing since it aimed at an actual and democratic guidance and control of technology. This essential element, however, hardly manifests itself in the real functioning of these offices. This is not possible due to the fact that an 'evaluation' of technology always comes to late. The public information on new technologies only starts moving when the products of the new technologies have been realized. Moreover, the information on the new products are, quite often, too limited by patent laws. The six phenomena mentioned under 1.3 do not seem to be eliminated by the presence of Asessmentoffices (certainly not in their present form). What is more, there is no acknowledgement (at least not of the nature of a procedural acknowledgement) of any scientific uncertainty or scientific dissent. Assessmentoffices seem, as far as a commissions for interdisciplinary research are concerned, to anticipate agreement amongst scientists. Epistemic discussions are, ultimately, analyzed in terms of conflicting interests. This leads to the loss of the possiblilty to select on the basis of universalisable interests. Not with standing these remarks, the Dutch office, for example, has contributed towards a social learning process which could develop in the direction of a discursive procedure. From this point of view the office has a social function. Social groups could approach the office to make known their desires and need of information. It would, however, be an essential improvement if all the discussion parties involved could be granted equal power in a discursive procedure. In order to achieve this, however, the necessary change as regards administrative law has to be introduced. The conditions for a discursive procedure should be legally settled and, above all, the rights and duties as regards the distribution of information should be installed. The policy process would then no longer be of a evaluative nature; it will become constructive in the form of a continuous interaction between, the providing of information, exchange of information and determination of policy. In such a policy process real democratically controlled learning processes with technology could be implemented, - and last but not least - mistakes could be restored. In any case it is plausible that the phenomena mentioned under 1.3 could be eliminated.

Notes

1. Epistemic plausibility is, on the one hand, related to plausibility on plausible arguments which are neither deductive nor inductive. They do not answer to the traditional formal-logical claims. On the other hand epistemic plausibility is related to the plausibility of assumptions and premises having a non-propositional structure(see also Rescher 1976). The plausibility of conclusions does not rest on the presupposed truth of the premises, but acquires its authority from the reliability of the sources of knowledge to which can be appealed. The plausible arguments referred to, by me, are up to now as far as I know the only expliticaton of Rescher's insight.

2. Many problems in the traditional philosophical theories about truth are, to my mind, founded on a confusion between reference to truth and reference to experience; or, to put it in modern terms to the confusion of discourse- theoretical truth and epistemic plausibility. Peirce who explicated the concept of plausibility , offered a starting point for a solution. Those authors who work in the tradition of Peirce, however, make either an absolute claim of epistemic plausibility (Rescher) or an absolute claim of discourse theoretical truth (Habermas,1973).

3. See especially: Brill, W.J., *Science*, vol 227, 1985, 381-384; Colwell, R.K., et al., *Science*, vol 229, 1985, page 111 and Davis, B., *Science* vol 235, 1987, page 1329.

4. This would, of course, not be the case where it concerns conflicting claims on truth. Accepting a truth-claim does require the refusal of a conflicting claim. The plausibility of knowledge-claims, however, is only touched by the paradigmatic internal coherence of particular assumptions and statements. There is a class of theoretical linguistic differences which cannot be explicated here. Intuitively, it should be clear by now that plausibility and truth do not have to converge.

5. This includes the abductive conclusion, not mentioned here.

6. This idea is missing by Toulmin (1958,1984)

7. The claim that the structure of explanations and predictions are identical has been dropped after a debate which lasted for two decennia (see H. Lenk (1986). Stegmüller (1969) already differentiated a list of 30 different types of predictions. The structure of prospective plausibility claims has not been revealed up to now.

8. Occasional attempts have been made at a solution in the form of interdisciplinary research. The experts, then, argue unendlessly about which disciplines should participate in the research.

9. For the transformation of dangers in risks see Evers, Nowotny (1987).

10. In the policy making process one cannot deal with the concept of hypothetical risk either. See Kollek (1988, p 34).

11. I do not mean that one, in the political realm, actually explicitly turns to the normative model of rational decision making. I only assert that the empirically founded arguments in the policy making process could be optimally represented in this way.

12. (Kettner has explicated a number of other conditions, see chapter 7 this volume).

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