ACTION SCIENCE AND THE REUNIFICATION
OF THE SOCIAL SCIENCES AND EPISTEMOLOGY.[*]

Diderik Batens

1. *Aim of this paper*

In recent years, we have witnessed a number of important methodological changes in some of the social sciences. These originated mainly in the a number of groups or tendencies that widely differ among themselves but most of which identify or sympathize with the label "action science."[1] I shall argue that these changes are basically on the right track. The 'traditional' social sciences are indeed affected by serious shortcomings, and the new methods are promising in this respect. At the same time I shall argue that an equally drastic change is required in epistemology, and that the combination of both changes will lead to the (very desirable) integration of epistemology and the social sciences. Moreover, I shall try to spell out the way in which this integration should be conducted and I shall try to do away with the objections against its possibility. By the 'traditional' view in the methodology of the social sciences and in epistemology, I mean the set of theses and presuppositions that were almost universally accepted in the sixties and that are still prevailing in our days.

The reasons for the necessity of both changes are not external. No 'ideological' arguments or general philosophical standpoints are needed to see that the intrinsic aims of the social sciences and of epistemology are not reached and cannot be reached as long as the traditional prejudices remain in effect.

There is quite some resistance against the methodological tenets of action science. It obviously derives from the fact that the formulation of the new methods is many times extremely vague, and that their justification many times is purely ideological or goes back on the 'Verstehende Methode'[2] or on similar obscure tendencies in Western thought. From an argumentative point of view, part of the resistance is quite justified, but it obviously does not follow that no need for a change would be present or that the methods proposed by action scientists are on

the bad track.

The origin of the obscure formulations and the ideological or obscure justifications is not difficult to trace. First of all it should be kept in mind that analytic philosophers have not particularly shown interest in the social sciences. The new proposals stem from social scientists who did not receive any cooperation from competent philosophers. Next, a majority of analytic philosophers was clearly on the side of traditional methodology. Those action scientists who do feel the need for a philosophical frame for their research are bound to end up with schools outside of analytic philosophy, which did indeed reflect upon a number of important features that analytic philosophy neglected. Moreover, philosophy of science displayed a rather confusing picture of itself during the Kuhnian period.[3] The views of Kuhn, Lakatos, and others, as popularized among social scientists, gave many the impression that, first, no specific competence exists in the field,[4] and, next, that almost any bunch of nonsense is rightly called a paradigm. Finally, many social scientists have startlingly poor insights in the epistemology behind the traditional methodology. Ever since I started looking at the relevant literature I was struck by the absolutely incredible misunderstandings. Many books on the methodology of the social sciences are mixtures of Popper, logical–empiricism, seventeenth century inductivism, nineteenth century positivism, Weber, perhaps some Kuhn, etc. They are neatly inconsistent, and epistemologically pre–Baconian. No wonder then that some action scientists have attacked 'objectivity', 'causality', or 'determinism'.

Apart from all this, a number of action theorists do indeed have respectable ideological reasons to propagate the new methods. Some of these reasons concern the ontology underlying the social sciences, others the role and status of the 'subjects' of the research. There is of course no reason why they should hide ideological motivations, provided these are not mistaken for epistemological justifications.

To avoid misunderstanding, I add at once that I shall not claim that the traditional methods are incorrect or useless. Methods are instruments. Whether they are suitable or not depends on the aim and the circumstances. I am convinced that, in some circumstances, some forms of knowledge about human beings may only be obtained by traditional methods. So, I am pleading for a broader range of methods and for a more careful study of their domains of application.

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2. *The traditional view in the social sciences*

Human behavior, or at least the parts of human behavior that the social sciences should or are able to capture, is seen as restricted to its observable aspects and as a function of laws that may be described without reference to internal states (beliefs, desires, etc.) This characterization is a simplification, but, I claim, is basically correct.

The traditional view does neither deny the existence of internal states nor the fact that people are conscious of them and see their behavior as determined by them. Except for some extreme formulations, it does not even deny that internal states play a causal role in observable behavior. However, the traditional view holds that the forms of human behavior, or the parts of human behavior, that may be captured in theories, at least at this historical point, are deformed by the conscious reflection of the observed. In other words, according to the traditional view (i) people may display 'normal' or 'natural' observable behavior, and may even have 'normal' internal states and processes, and (ii) this behavior and these states and processes are in general deformed if those people are conscious or them, and even more so if they consciously reflect upon them. On the one hand, consciousness about one's own behavior or internal states is taken to result in an inadequate description of these. On the other hand, the consciousness to be observed leads to changes in the behavior of the observed persons; these changes may be intended by the latter but are unwanted by the observer because they deform the 'natural' behavior that the social scientist is trying to capture in a theory. In order to escape the deforming effects of consciousness, a great number of techniques for misleading people about the intentions of the researcher have been developed.

The traditional view is trivially correct in holding that only observable behavior may be observed. The objections against it concern other points, among them the aim and continuous efforts (i) to consider behavior without any reference to internal states during 'passive' observation, and (ii) to provoke behavior, during 'active' observation, in which conscious decisions of the observed are maximally suppressed. In *A Theory of Human Action* Alvin Goldman has pointed out years ago that the avoidance of terms that refer to internal states rests on a methodological mistake. One of his central criticisms to behaviorism was that such terms should be considered as theoretical, and that the discussion about the use of theoretical terms in the so-called exact sciences had been settled for decades. Apart from this,

there are a number of cases in which it not only is desirable to refrain from suppressing conscious behavior on the part of the observed, but in which the aim of the research requires that conscious decisions and the mechanisms behind them are explicitly discussed with the observed. This type of research is sometimes labelled "communicative" and is then opposed to 'objectivating' research. Needless to say, both terms are somewhat confusing.

For the sake of clarity,. let me consider three different statements concerning internal states. The first is that there are no internal states. As remarked before, most proponents of the traditional view do not subscribe to this statement. In earlier (more positivist) days, it was sometimes propagated, in view of a strict identification of reality with sensations. The second statement is that internal states cannot or should not be referred to in theories. This is the claim that Goldman, among others, refuted; not all adherents of the traditional view subscribe to it and some of the traditional theories do not conform to it. However, the statement is ambiguous and deserves special attention.

Even if traditional theories contain terms that refer to internal states, these are usually accompanied by operational definitions that refer to observable behavior only. One of the reasons for this is that the proponents of such theories do not consider internal states as something the person to whom they belong is particularly competent about. Most of these theories see internal states either as unconscious entities, or as entities that the person under consideration may be conscious about, but that nevertheless will become deformed if this person would reflect upon them. In other words, even if internal states are recognized as real, and even if they are recognized as belonging to a person's consciousness, the act of conscious reflection, which most people consider as characteristic for humans, is still taken to disturb 'normal' behavior and hence also as a hindrance for theory formation and explanation. In this sense, the following third statement, which is methodological in nature, is a natural continuation of the two former ones: internal states and mechanisms need not and should not be discussed with the observed in a cooperative way, because such discussion would deform the object of study, rather then provoking correct knowledge. It is especially against this third statement that I want and need to argue. It will appear, however, that even the two former statements do still present problems.

The present discussion is related to the problem of so-called free decisions. The typical features of free decisions are obviously not given in observations. According to the traditional

view, such decisions cannot be the object of causal or nomolo-
gical theories. This resulted in a 'deterministic' theory of man,
either ontologically – the subjective impression of freedom is
then seen as a rationalization – or at least methodologically. The
latter position comes to this: irrespective of ontological matters,
a deterministic view on man is a necessary condition for arriving
at nomological knowledge about men; hence it is the best possible
sible methodological choice because it alone will enable us to
discover the laws, if any, to which men are subject. I shall
argue that this view also is mistaken.

3. *The traditional view in epistemology*

Epistemology essentially concerns decisions: to accept or not
accept some statement, to perform some observation or set up
some experiment, and to evaluate theories, hypotheses and meth-
ods. For this reason, traditional epistemology is constantly con-
cerned, explicitly or implicitly, with the so-called epistemic
subject, the entity performing those decisions.

According to the most extreme and most traditional view, no
laws apply to epistemic subjects. Their behavior, or at least
their relevant behavior, is merely a function of internal states
and dispositions: knowledge items and all kinds of methodological
and logical principles and procedures, that are traditionally
distinguished from knowledge items. In this sense, humans are
merely 'subjects'. The knowledge items and procedures are
accepted by decisions of the subject, and ultimately the latter
has to find a justification itself and in itself. Humans are seen as
capable of communication and even cooperation with respect to
epistemic processes; different people are able to perform collec-
tive research about the world and even about epistemology,
which to some extent is judged to be outside of the world. Mind
that humans that participate in this communication and coopera-
tion are seen as epistemic subjects; whether the object is
something external to each of them or is their very epistemic
states, processes or criteria is unimportant in this respect. I
return in section 6 to the specific problems that arise if other
humans are the objects of research.

According to less extreme epistemic views, several laws do
apply to humans as epistemic subjects, but this fact is irrele-
vant from an epistemological point of view in that such laws
cannot possibly interfere with the decision processes as such.
The matter is worth careful attention. In order to escape the
objection of attacking a straw man, I shall now consider the

facts that supposedly form the central arguments for the irrele-
vance of laws, external factors, and unconscious factors for
epistemic decisions.

Suppose that, during their epistemic cooperation, the subjects
would consider their own and each other's behavior in an exter-
nal, 'objective' way. In such case, it is claimed, they would be
unable to consider the justification problems - pertaining to
knowledge items as well as to methods and values - that are
essential to the process, and they would a fortiori be unable to
take justified decisions in this respect. There is a general and a
more specific argument for this claim. The general one derives
from a conceptual opposition: a choice that is determined by
something external cannot at the same time be the result of a
free deliberation process.

The second argument is related but more complex. Suppose
first that I realize that some external factor partly determines
my decision, and that I know this factor. This means that I view
my decision for p as at least in part determined by some fact q.
Given this, I cannot escape asking myself whether q is a good
ground, or part of a good ground, to decide for p. In doing so, I
will evaluate the weight of q with respect to other arguments. Or
at least so it seems at first sight. Indeed, although this formula-
tion sounds attractive, it is actually inaccurate. The fact (pro-
cess, etc.) that q cannot possibly be a ground or an argument; q
has to be replaced by a *reason*. So, starting off with some fact q
that had an external influence on the decision process, we end
up with an item which is internal with respect to this process,
and which is not the *fact* q but the *belief* that q is the case (or
something similar). As a consequence, I cannot, at least not
justifiably, take a decision that is influenced by an external
factor *known* to me. The very belief in the determining role of
the factor would cause its elimination.

Let us now consider external determining factors not known
to the epistemic subject. Obviously one may believe that some
such factors do exist, but one cannot possibly take them into
account *within* the decision process, the simple reason being
that one does not know them. Again, there may be such factors,
and one may believe them to exist, but they cannot possibly add
anything to the decision process, they cannot in any sense help
one to make a decision. For this reason, the claim of traditional
epistemology is not that such factors do not exist, but that they
are *irrelevant*. Their existence, and even their possible exist-
ence, causes problems for the authenticity of the decision pro-
cess. To the extent that they do play a role, the conscious
decision process is a side-effect of the actual causal process.

For this reason, traditional epistemology has tried to develop a set of solutions for the authenticity problem. Descartes' discussion of the possibility of a *malin génie* is a well-known example; and Reichenbach's work on the justification of the straight rule is in a sense another example. However important authenticity problems may be, and however hard the difficulties in solving them, they all concern the *possibility* of epistemic decision processes, but never affected the basic tenet that external factors do not and cannot possibly play any role in these.

4. *The paradoxical discrepancy*

The traditional view on the social sciences and epistemology leads to a relation between the two that is extremely remarkable, to say the least. The same culture, even subculture, contains two kinds of theories about the same entities. Each of the two is taken extremely serious. And yet the resulting images of these entities are not only distinct but even flatly inconsistent. Once the discrepancy is seen, one is baffled by hearing the same persons, sometimes within the same sentence, defend a purely 'external' view of humans and argue in strictly 'internal' terms in favor of some epistemic decision.

The discrepancy is clearly the strongest for the extreme formulation of the traditional view. It describes humans in two so incompatible ways, that it only seems defensible if some single epistemic subject exists, viz. the 'I', which is then seen as completely different from other humans, to which the laws of the social sciences apply. Typically, the epistemic subject *acts*, whereas all others *behave*. This will result in ontological solipsism or something very near to it. Any identification of the 'I' with the objects of the social sciences, or any form of cooperation with others in epistemological processes, will result in an inextricable knot.

However, there is a fundamental paradox even for the less extreme formulations, according to which internal states and the like are merely beyond the reach of the social sciences, whereas external determining factors are epistemologically irrelevant. Even in this case there is an incompatibility between the ontologies of the two *disciplines*; they describe and explain the (at least in part) same human behavior in terms of two sets of mechanisms that cannot possibly act together. Of course, we all know that there also are discrepancies between different explanations of the same phenomenon produced by different social sciences and we do not consider this extremely problematic. In

such cases, however, there is not a real paradox because (i) the disciplines are considered to be in evolution and will ultimately be unified in some or other way which does not *require* methodological or fundamental conceptual changes, and (ii) it is not always clear that the distinct explanations are indeed incompatible. The relation between the social sciences and epistemology is truly paradoxical because the mechanisms are clearly incompatible and are not only so because of the accidental historical state of the disciplines, but because the discrepancy derives from the very fundamental concepts and methods that underlie the social sciences on the one hand and epistemology on the other.

There is worse. Apart from the discrepancy between the ontologies of the two disciplines, the less extreme traditional view has a supplementary drawback. By the fact that the overall ontology leaves room for both types ('external' and 'internal') of entities and mechanisms, the social sciences as well as epistemology are bound to be necessarily inadequate: both neglect actual mechanisms in describing and explaining human behavior. Again, this drawback is not a consequence of the historical weaknesses of the disciplines, but of the basic view on their nature. For this reason the problem is so fundamental. Improving upon the object-level of the theories will not help, we need a redirection of their principles.

There have been several attempts to resolve the paradox. An old and well-known one is Descartes', about which Spinoza cynically remarks "I would be reluctant to believe that it stems from so famous a man, if it where not that keen."[5] The other best known attempts actually tried to reduce epistemology to the social sciences or vice versa. The adherents of the 'Verstehende Methode' and of several related approaches did the latter. This resulted in a number of methodological weaknesses, and the justified reproach of subjectivism. Adherents of the old psychologism and, more recently, of the 'strong program'[6] in the sociology of science, tried the first reduction. The fatal difficulties connected with this approach concern the justification of methods and the evaluation of theories.

It is extremely implausible that a reduction in either direction might lead to a sensible solution. The basic principles of both (groups of) disciplines, according to the traditional view, are such that one cannot possibly even phrase, let alone solve, the problems of the other. The only possible way out lies in a more basic unification. Social sciences should describe humans as beings that perform decision processes based on their worldview, and the behavior of which is to some extent determined by

such processes. Epistemology should describe humans as concrete beings, affected by concrete limitations that are induced by the environment, and the epistemological behavior of which is a special case of the behavior described by the social sciences. I stress once more that it will not be sufficient that researchers adhere to an ontology that leaves room for both disciplines. It will be necessary to reformulate both (sets of) disciplines with the same ontology.

Before moving to another topic, I want to say a word about the rather poor objection that my description of the traditional views, both with respect to the social sciences and with respect to epistemology is not accurate. It obviously is not. In both disciplines we find theories and theses that depart from the traditional view and are more or less in accord with the position I am defending. Without these, I probably would not have arrived at my present position anyway. Still, it cannot be denied that the present-day state of the principles of both disciplines are a mess, and the humble aim of this paper is to clear up the mess a little bit. Apart from that, I honestly think that the traditional view, as described above, is still prevailing. The majority of methodological arguments developed by social scientists (and philosophers) and the majority of epistemological arguments developed by philosophers (and, sometimes, social scientists) show that it is.

5. The status of normative systems

Most of the statements made above about epistemology apply to the broader domain of normative systems in general. According to the traditional view, these systems are the result of internal deliberations and decisions and belong wholly to the sphere of the epistemic subject. Both their genesis and their justification are considered as independent of and even unrelated to results from the social sciences. All of this holds true for normative systems in the broadest sense, even if the adherents of the traditional view have mainly commented on methodological systems, moral systems, and logic, specific logics such as probability theory, decision theory, or the theory of games included.

Of course, these normative systems correspond to specific sets of actual behavior, viz. those for which the epistemic subject relies on, or should rely on, the normative systems. If we study such behavior of other people or groups of people, within the frame of the social sciences, we often find some 'system' in or 'behind' it. Such systems are sometimes called

descriptive or factual normative systems. According to the traditional viewpoint, descriptive normative systems do not and need not correspond to the normative systems that the actors believe to apply. The descriptive systems are a systematization of observable behavior, a characterization of a black-box system.

The traditional viewpoint has lead to a peculiar conception of the relation between, on the one hand, the normative systems devised and employed by the epistemic subject and, on the other hand, the descriptive normative systems arrived at within the context of the social sciences. The main relevant point for my discussion is the belief in the existence of 'natural' behavior and 'natural' normative systems. I will expand a bit on this point because some people hope to get around the discrepancy between epistemology and the social sciences by relying on natural normative systems, whereas I take the belief in such systems to be both completely mistaken and a hindrance to resolve the discrepancy in a sensible way.

Unlike one might expect from a theoretical perspective, the belief in the existence of natural normative systems and in their intrinsic value is quite popular. Some philosophers and many others have explicitly defended this belief, but even more subscribe to it implicitly, as appears from the arguments they produce. Psychologists have been trying to describe 'natural' ways of thinking and reasoning, e.g., with respect to inference, probabilistic beliefs, rational decision making, moral behavior, etc. and with respect to the actions based upon these. Several groups of scholars have defended the view that the resulting theories may help to resolve the problem of the apparently arbitrary and conventional nature of devised normative systems. And much more numerous are those who refer to natural, uncorrupted intuitions, and the corresponding behavior, to defend logical, ethical, and other normative systems.

The distinction between, on the one hand, 'natural' normative systems and the corresponding behavior, and, on the other hand, (re)constructed such systems and the corresponding behavior, can neither be reduced to the distinction between the known and the unknown nor to the distinction between the conscious and the unconscious. Indeed, among the adherents of the traditional view that keep up with the distinction, we find three (not mutually excluding) attitudes with respect to it. The first and most traditional (not most frequent) attitude is that natural normative systems are incorrect and should be replaced by constructed ones. The second is that the function of natural normative systems is to provide information required for understanding the behavior of others and hence also for acting

adequately in view of this behavior. The third attitude consists in taking natural normative systems as superior to constructed such systems, superior in a specific respect and for reasons that will become apparent immediately. It is easily seen that, according to all three attitudes, it is possible or even desirable that natural normative systems and the corresponding behavior are made conscious and are known.

The only way to make sense of the distinction is by considering natural normative systems as the results of the interaction between innate properties of humans combined with relevant laws to which they are subject. Adherents of the third attitude consider the constructed normative systems as too restricted or even as misguided, precisely because they are only the result of our imagination combined with our limited understanding of our natural behavior. For them, constructed normative systems should be *derived* from results of the social sciences – an attitude which I already criticized for epistemology in particular. One should grant them that our imagination is usually a lot poorer than reality itself, and that, in order to obtain knowledge about some domain, it appears preferable to rely on some experience, rather than on imagination alone. The history of epistemology may be invoked as an illustration here.

I return to normative systems in section 8. There I will show that the traditional view is mistaken for pretty much the same reasons as it is mistaken in connection with epistemology, and that natural normative systems do not exist. Before doing so, however, I should spell out and clarify my objections to the traditional view with respect to epistemology and with respect to the social sciences.

6. *Further basic shortcomings of the traditional viewpoint*

I already pointed out that the traditional view results in two disconnected and incoherent types of theories about human action, and that the old attempts towards unification were directed at reducing one type of theory to the other. In the present section I want to argue that the traditional viewpoint is moreover inadequate with respect to both the social sciences and epistemology, even if considered separately.

A social scientist cannot deny that the persons he is studying are in principle similar to himself in all respects except for their specific position in the epistemic relation. More specifically, he has no reason whatsoever to deny them the internal states and processes that are essential to his understanding of his own

research. Some tried to avoid this by explicitly claiming that their own consciousness is a fictitious side-effect of actual causal processes. To these applies the criticism I mention below. Meanwhile, however, there have been attempts to describe and explain decisions taken by scientists, and in this connection it is patently impossible to avoid the epistemological dimensions. In doing so, how could one sensibly deny such dimensions to non-scientists and to scientists at times they perform other actions?

The reader might object that these arguments still rely on the discrepancy discussed in section 4, and that they depend on methodological considerations that are rather remote from the actual research and its results. For this reason, it is important to point out that several types of research situations are demonstrably burdened by the traditional viewpoint, in that its principles prevent this research from proceeding in an (internally) efficient way or even make it impossible. In general this applies to those situations in which the research presupposes a communicative collaboration between the researcher and the persons the behavior of which is studied.

Counseling to individuals, factories, or other social systems is a traditional example. In general, neither the researcher nor the client is able by him- or herself (i) to arrive at an adequate analysis of the situation, (ii) to develop sensible alternatives (for action strategies, values, etc.), (iii) to provide a correct evaluation of these alternatives, and (iv) to make a choice justified in the light of all this. The researcher has to depend on the client for the elements of a detailed analysis of the situation, possibly including the way in which the involved people experience this situation. He also depends on the client in order to obtain reliable information on the (not invariant) norms and values that the client wants to apply to the actual situation and the alternatives, and that are required for previewing the way in which the client will behave after some modification will have been introduced. Moreover, it is the client that will have to experiment purposefully with certain alternatives. On the other hand, the client has to rely on the researcher because of the latter's competence in discovering unnoticed aspects of the situation, in outlining alternatives, and in evaluating these.

Typically, the specific competences of both parties are needed to arrive at a reliable and efficient result. As a consequence the parties should be able to interact as epistemic subjects – to phrase it in traditional terminology. On the other hand at least one of the parties interacting in the research process, viz. the client, is him- or herself an essential component of the domain

under study. In some cases the researcher is that much involved and is influencing the situation to such an extent, that also his or her behavior should be explicitly studied in order to understand the changed situation and the desirable further changes.

I mentioned that counseling is a traditional example. However, it should by no means be taken as a unique application for the new methods. The fact that it is well-studied and frequently practiced is mainly a consequence of the economic and social circumstances. There is no reason why communicative interaction should be restricted to cases where the 'client' is paying for the research or is in direct need of help. If our understanding of the situation and of possible alternatives requires specific methods in those cases, they cannot possibly be useless or inapt in others.

As I announced, I also claim that the traditional view is inadequate with respect to epistemology. Let me first consider an objection that I will not attach too much importance to. Many will agree that there are obvious exceptions on the statement that there is no room for external determining factors in decision making. A well-known example concerns the case in which the subject feels a compulsion to act counter to the available reasons, e.g., a compulsion to smoke, to kill someone, or to believe something. In each of these, there are external factors that drastically influence the decision and the deciding subject knows so and knows the factors. The adherents of the traditional view might argue that these are not examples of real decisions, but this answer is unacceptable because it might save the traditional view in epistemology but nevertheless make the set of its applications empty. Next, those adherents might argue that the influence is not exerted by the external factors but by corresponding or connected reasons. The example of smoking is not very epistemic but handy to make the point. A person knowing the disadvantages of smoking might at the same time consider that the avoidance of the physical tension arising from non-smoking is decisive in the decision to smoke. I shall not discuss this any further because, in the absence of much relevant empirical data, the matter is complex and better arguments are available.

Next, the social sciences offer a large number of experiments, some several decades old, in which the decision of the observed is obviously influenced by external determining factors although he or she is convinced to be making a conscious decision, sometimes an epistemic one. The least epistemology might do here is try to find criteria that guarantee the absence of such factors. But in traditional epistemology it is even impossible to

phrase the problem.

Even irrespective of these specific cases, there are *many* reasons to reject the traditional view in epistemology. Consider first a theoretical objection that I have already argued for: one cannot possibly hope to understand (or explain, devise, ground, and so on) the justified behavior of humans in some domain if one neglects and even in principle cannot take into account the results of the social sciences. This holds for epistemology and for other normative systems as well. According to the traditional view, all of these *have* to neglect empirical data on man. Apart from this theoretical objection, there is overwhelming *factual* evidence against traditional epistemologies. The latter describe *at best* the justified epistemic processes that would obtain in a rather different world and for knowing subjects that are rather different from men and are rather differently related to (have other, e.g., much more direct ways of access to) reality. In "Meaning, acceptance and dialectics" and elsewhere I have argued that traditional epistemologies are completely inadequate with respect to the recent insights from philosophy of science (as supported by history or science); other people, e.g., Larry Laudan have made analogous points with respect to specific epistemological systems. Conceptual analysis and conceptual creativity are obviously of utmost importance. Yet, it is typical that the developments in epistemology during the last twenty years – and most of them concern phrasing problems differently and giving up old solutions rather than devising new ones – are ultimately caused by empirical, mainly historical work. Moreover, as we see from present-day history and philosophy of science, the role of these empirical data is not merely heuristic or inspiring; they constantly interact with epistemological arguments both in the historical and in the methodological analysis. As soon as one grants this importance to empirical data from the history of the sciences, one cannot invoke any reasons of principle to deny it to the results of the social sciences.

7. *A comment on two possible arguments*

I am defending the thesis that we need to develop a view on humans which is suitable for the social sciences and for epistemology, and for other normative systems as well, and that these disciplines should be based on that view. Trying to clear the way for the positive contribution, I first have to refute two central arguments in favor of the traditional view.

Some will argue as follows. We grant that there are good

reasons to develop a view on humans in which there is room for internal states and processes as well as for the laws of the social sciences. Nevertheless, it is impossible to arrive at systematic and reliable knowledge about such states and processes in other people, either because they are beyond the reach of observation or because they are too complex.[7] As I mentioned in section 2, Alvin Goldman showed long ago that the first reason is not a valid one. The argument from complexity, on the other hand, is non-conclusive either. First of all, even if a set of phenomena displays a high degree of complexity, this need not entail that the same holds true for the mechanisms that govern these phenomena and their dynamics. Moreover, complexity is relative to conceptual systems and hence to the historical period. The history of the sciences contains numerous cases in which the conceptual development made it possible to describe phenomena that previously appeared awfully complex. Consequently, complexity need not prevent us from devising theories. Moreover, precisely because of the complexity of internal states and processes it is at least sensible to recur to communicative interaction, based on theories in as far as they exist and according as they develop, in order to gather information about the internal states and processes that are relevant in a concrete situation.

A second argument from traditional quarters is that, if internal states and processes exist, they should be studied by means of the traditional methods. This position relies on the aforementioned deforming effects that act within communicative methods, and within conscious processes in general. And indeed, these deformations must not be denied, whence traditional methods are most suited in a number of circumstances. *Part* of the research involved in constructing a theory about internal states and processes should clearly rely on traditional methods. Yet, as I now shall try to explain, this does not establish the argument.

First, let me give in even more to the traditional view. In the presence of some deforming mechanisms, e.g., those in which the 'client' deliberately tries to hide some information or in which he or she is under a 'fatal' influence of unconscious mechanisms, no communicative interaction worth that name may occur. So, if such mechanisms are present, they should be very restricted or it should be possible to surmount them during the interaction, in order for communicative interaction to be possible. In the opposite case, however, it is hard to see any reasons for refraining from communicative interaction; it by all means provides more direct information in a faster way. The upshot is that communicative interaction should be attempted if, and as long as, one

has good reasons to believe that deforming mechanisms are absent. However, whether this is the case or not is an *empirical* matter that has to be decided on the basis of available observations and theories. And, as with other such decisions, mistakes are not excluded, but, on the other hand, one may revise one's judgement. Moreover, attempting to apply communicative methods may be an excellent means to discover deforming mechanisms if they are present. By all means even traditional social scientists could not possibly show the occurrence of deforming mechanisms unless by referring to the statements made by the observed. Worse, the occurrence of deforming mechanisms that are beyond the conscious control of the observed may only be established and even described by referring to internal states of the observed.[8]

In view of all this, it is hard to see why traditional social scientists do not want to consider the possibility of situations in which deforming mechanisms are justifiably believed to be sufficiently absent or to become so during the interaction. They start from an a priori which we may term *universal mistrust*. If the social sciences would be extremely successful, the a priori might seem pragmatically justified. But unfortunately they are not. So, the only sensible explanation for the a priori seems to be the need for conforming to the natural sciences. There, they believe, object and subject are neatly kept apart. Is it possible to turn this explanation into a good reason for this position? Clearly not. First, at least part of the traditional claim on objectivity relies on the neglect to address such problems as theory–ladenness. More importantly, it is quite obvious that the objects studied in the natural sciences are precisely different from humans in that they lack the properties required for making communicative interaction possible. Hence, the successes of the natural sciences cannot constitute a reason for conforming to their methods on precisely this point.

8. *Normative systems revisited*

A careful treatment of normative systems (in the widest sense of the term) is essential for articulating the alternative view I am advocating. Such systems indeed govern human decision making. At the same time some such systems are clearly the result of conscious creative processes. As the alternative view holds that epistemology and the social sciences should share the same frame, all kinds of normative systems have to be integrated in both.

Both traditional epistemology and the traditional social sciences have paid attention to a very limited number of normative systems, mainly general methodological and moral systems, and a number of systems called logics. Apart from these there are many normative systems that are more domain-specific: more or less explicit action-rules related to general or specialized skills. Examples range from the heuristic strategies typical for logical and mathematical proofs to the instructions for planing a plank. Some of these rule-systems concern the handling of materials (from soil to humans); usually such rules cannot be fully formulated in verbal terms and the corresponding skills cannot be mastered without actually handling these materials. Both epistemology and the social sciences have neglected these rule-systems; there is neither decent knowledge of such systems nor of their overall methods, structure, and other properties.

I now return to the distinction between 'natural' and devised normative systems. As I explained before, the distinction originates from the discrepancy that characterizes the traditional viewpoint. This need not imply that it cannot be sensible or that it cannot be integrated into the alternative view. Quite to the contrary, the existence of natural normative systems might provide us with an easy way to resolve the discrepancy. The social sciences would provide information about such systems; starting from this information, the 'philosophical' disciplines would articulate theories that may serve again as hypotheses for the social sciences. However, I shall now argue that this easy way to surmount the discrepancy will not do because the distinction is not sensible and hence should not be integrated.

As soon as someone comes to know a devised normative system, and judges it correct or valid for some domain of application, one tries to conform to the system. Let us consider an example. Most present-day mathematicians have been trained in first-order predicate logic with identity. Or at least they know that this system exists, that many authors of papers and textbooks claim to employ it, that, except for some specific schools, it became the general standard for mathematical reasoning. As a consequence, they try to make the proofs in their papers conform to it. In view of this, one obviously should not look for the 'natural' logic in present-day mathematical papers. Very orthodox adherents of the traditional view will claim that modern mathematicians are corrupted by classical logic, and hence that their thinking cannot form an argument with respect to correct natural thinking. So, let us consider the thinking of mathematicians that lived before the formulation of predicate logic. The example is historically interesting, because during the Renais-

sance and long thereafter, the general attitude was to consider logic as completely obvious and hence as not worth any systematic attention. However, even in those days mathematicians had been formed among other things by studying examples of proofs and they were living in a community that cherished quite precise opinions about logical correctness. Although they may not have been familiar with an explicit theory about logically correct reasoning, it is quite obvious that their thinking is influenced by cultural elements. I doubt whether the adherents of the traditional view would like to go back in time even further, but suppose we do. We may even consider the case of a more or less isolated human who is making some deductive step. Even in such case, the actual reasoning of this human will be heavily influenced by (i) previously made reasonings (and experience related to the adequacy of the subsequent decision) and (ii) the possible reflection of this human on the specific instances of reasoning or on forms of reasoning in general.

The upshot is that the actual thinking of some individual is determined by explicit or implicit theories about logical thinking and other normative domains. (And, incidentally, it is difficult to see any advantages in the implicit theories, for these by all means are constituted by prejudices.) It follows from this that there simply are no descriptive normative systems that may be termed 'natural' in the intended sense.

It follows also that the discrepancy should be surmounted in a different, more complex way than by referring to 'natural' normative systems. We shall have to leave room for internal processes, and in this respect we shall have to make sure that it remains possible to consider all traditional epistemological and other normative questions in their full original meaning and to give them their full weight with respect to justification. At the same time, we shall have to leave room for laws and other determining factors. We should give these their full weight as well, and in no way weaken such notions as determinism, causality, and nomological mechanism.

I now may point more convincingly to the central role of normative systems for the alternative view. Humans try to arrive at justified such systems. In the absence of 'natural' systems, the justification has to refer to considerations of adequacy and efficiency with respect to the intended domains of applications, and in this sense empirical considerations are fundamental. On the other hand, humans try to conform to accepted normative systems during conscious behavior. As a consequence, conscious behavior cannot be understood without referring to accepted normative systems. Even unconscious behavior may be strongly

influenced by devised normative systems, as I shall show in the next section. I hope this shows that all the problems I pointed to in this paper are directly related to normative systems. They are devised or accepted in a conscious way, *and* human behavior cannot be understood without taking them into account. At the same time, in order to devise or accept them, humans have to understand the role these systems play in their behavior and in human behavior in general. Humans have to be seen as to some extent consciously interacting with reality, including themselves, on the basis of normative systems they have devised themselves, *and* they have to understand this relation between themselves and reality in order to justifiably devise or accept normative systems.

9. *Surmounting the discrepancy*

The main obstacle for a unified approach in epistemology and the social sciences seems to be the fear that some of the problems, as presently phrased by one of these, would be eliminated or unfaithfully reinterpreted. The opposition between conscious deliberation on the one hand and lawlike behavior on the other clearly appears as insurmountable to many scholars. For this reason I shall present four steps that seem unobjectionable and that bring us where we need to be. The very first step will confront us with this opposition.

  This first step consists in accepting an ontology according to which human behavior is determined at the same time by conscious decisions and by the laws that govern humans. The problem we have to face is whether this is consistently possible. Conscious decision involves internal states and processes, deliberation, reliance on norms and values, the evaluation and acceptance of such norms and values, etc. Lawlike behavior involves mechanisms that are external to consciousness. The arguments I offered in sections 2 and 3 suggest that it is not possible for these to cooperate without one of them dominating the other in a decisive way. So, I have to show that these arguments are misguided.

  The second step consists in devising a conceptual system for epistemology, including the application of normative systems, in which there is room for external (including unconscious) determining factors and mechanisms. As such factors do play a role, this step will have a liberating effect on epistemology. To the extent that we are able to describe external determinants, we will be able to take them into account at the conscious level. In

other words, we shall increase the control on decision processes, whence the latter will become more justified (even by the present standards). We shall increase control in a second way as well. This step requires a different view on epistemology, viz. a view according to which it is a theory about concrete subsystems of the world that are among other things interested in understanding and modifying this world. In doing so, we get rid of the speculative elements of epistemology that originated from the fact that people have been theorizing about the ideal knowledge of idealized beings in an idealized world, instead of about the actual knowledge conditions of humans in this world.

This second step does not consist in adding data from the social sciences to epistemology. The present social sciences do not provide the relevant data because, as I explained before, they study the determinants of human behavior in direct relation to external observations, not in relation to the decision processes with respect to which justification problems arise. Yet, it is not too difficult to see in which way one should proceed. Fortunately, the role of empirical data with respect to normative systems has been more and more recognized in recent years and has actually been studied in connection to justification processes. This is true for mathematics, but also, e.g., for decision theory.[9] Moreover, there is a lot of experience with this type of reasoning in history and philosophy of science; although disagreements about the relation between factual knowledge on the one hand and normative and evaluative knowledge on the other are far from neglected in these disciplines, there is an impressive agreement on the methodological implications of historical data. Indeed, there is no general agreement about the precise role of empirical data in any of these disciplines. But the reason for this is precisely that such an agreement presupposes an adequate epistemology, which we are missing at present, but which is gradually taking shape in different quarters.[10] As I argued elsewhere, this epistemology should neither be hierarchical nor require absolute certainties. It should view the solution of problems (all sorts of decision making included) as a contextual matter. This implies that all knowledge elements may in principle be reconsidered, and that the judgement about their correctness (in comparison with alternatives) relies in principle on all other relevant knowledge items, irrespective of the question whether these are 'high-level' (like cognitive values) or 'low-level' (like empirical data). From this point of view, the justification of epistemological statements, and normative statements in general, proceeds in a way that is almost identical to the justification of empirical statements. The basic mechanisms

are (i) making our world-view coherent (part of it will not be directly relevant), (ii) considering derived (preliminary, etc.) problems, (iii) gathering relevant empirical data, and (iv) devising new alternatives.

I need to say a word on the latter mechanism. Recently we learned a lot about discovery and creativity, especially through the work of Thom Nickles and of the authors contributing to and referred to in the two (meanwhile famous) volumes he edited. The most important point in connection with the subject of the present paper is the interplay of factual data with theoretical issues as well as with conceptual issues. I would like to add, however, a specific point concerning normative systems. In these, the impact of empirical data is less decisive than in other theories. Yet, such data play a central role in at least two respects: (i) determining the implications and consequences of such systems and (ii) suggesting alternatives, more specifically alternatives for particular domains of application. A careful study of actual behavior in comparison to existing normative systems will reveal the weaknesses and restrictions of the latter. (A case at hand are logical systems.) This will enable us to find systematic sets of applications of specific rules, and these will provide us with the constraints needed for starting the process that should lead to an internally justified normative system.

As a third step we should integrate within the social sciences the elements of decision processes and the mechanisms governing them, and we should develop a conceptual system that enables us to do so. Again it will not be possible to simply add elements from epistemology and normative systems to the social sciences. Most epistemologies indeed are characterized by a simplistic conceptual system that does not leave room for actual mechanisms but relies on a simplistic view on man. Let me give an example. Almost any epistemology contains requirements concerning consistency. Since the development of paraconsistent logics, we know that it is quite possible to reason from inconsistent premises in a sensible way. Obviously some weakened form of consistency or coherence should hold. Only, the exact formulation this requirement should take is far from clear at the moment. The traditional requirement occurring in epistemological theories is mistaken and no alternative has been worked out. One of the consequences of this is that the limits within which sensible decision processes are possible are not known at all. In my view, it follows that the integration I am advocating has little to expect from present-day epistemology, and apparently much more from the application of communicative and cooperative methods.

At the outset of the present section I mentioned the opposition between conscious deliberation on the one hand and lawlike behavior on the other. We have seen that this opposition was not difficult to surmount from the viewpoint of epistemology. For the social sciences the basic problem is whether 'free deliberation' is compatible with their lawlike character. It seems to me that the problem may be solved in a way that is so easy that I hardly understand why the problem was ever taken to be a difficult one. The simple solution consists in introducing a law to the effect that the behavior of humans is determined by decision processes, except when external or unconscious factors interfere and to the extent that these do interfere. As a starting point, and in agreement with the common situation in young physical theories, this statement may take the form of an idealized law. Of course, it will have to be specified, tested, and supplemented with hypotheses that reduce its idealized character. However, it seems extremely plausible as a starting point; it seems not too difficult to supplement it with data, empirical generalizations and even bits of theory from present-day social sciences; and it seems not difficult to gather information and to set up experiments in order to test these extensions.

The idea of the preceding paragraph would enable us to realize the integration required by the third step. The way in which this will work out and the extent to which external and unconscious elements will play a role is obviously an 'empirical' matter − it cannot be found out unless by trying to develop the social sciences along these lines. In doing so, one will have to take into account that even unconscious mechanisms may to a large extent be shaped, and hence justified, by conscious decisions. Many times, unconscious mechanisms act as defaults: they operate as long as their function is not taken over by a conscious decision. Moreover, large parts of our behavior are guided by unconscious mechanisms that originated from conscious learning processes and that may be changed by such processes (e.g., we learn to drive a car and gradually leave most of it to unconscious habits). Given the idea of the preceding paragraph, all such mechanisms may be studied. This shows, it seems to me, that it is not at all difficult in principle to attribute a large role to decision processes within the social sciences. Moreover, in this way we shall be able to see epistemic behavior as a special case of behavior in general, as I required in section 4.

I now finally arrive at the fourth step which will consist in the integration of epistemology and the social sciences. This integration involves turning epistemology into a normative social

science (of the type arrived at by the third step) that provides information to and receives information from the other social sciences (and gradually is unified with them). This step will not be difficult in view of the two preceding ones, and, if it is successful, it will lead to theories that are obviously better than present-day epistemology and the present-day social sciences, even according to their own standards.

Communicative and cooperative research will play an important role within the resulting disciplines. They will enable us to know and understand the internal determinations of decision processes and of behavior in general. Introspection is in many respects a much less suitable method, because of its well-known unreliability and because the knowledge of external or unconscious determining factors would modify the decision processes by the mechanisms described in section 3. Communicative and cooperative research, to the contrary, may be alternated with traditional research; their combination will result in more diversified and more reliable tests of the hypotheses. Incidentally, in many cases it should not be avoided that the persons observed know that traditional research beyond their control is being conducted parallel with the cooperative research in which they participate. After hypotheses have been formed and have been tested by means of traditional research, one may engage in cooperative research again. If the hypotheses concern external or unconscious determinants, this research will provide information about the way in which the knowledge of such determinants influences the decision process; e.g., in view of this knowledge one may wish to change the decision, or future similar decisions may appear to be different. Needless to say, concrete methods for such forms of research have to be articulated. The point I want to make is that such research is possible, requires cooperative methods, and leads to the unification and integration I am arguing for.

10. *A final remark on the social sciences*

The methods defended here seem quite plausible if beings as complex as humans try to articulate theories about the members of their own species. It is hard to see any reasons for not trying this out in the most serious way possible. Obviously, this form of research will in many cases profit from the fact that the observed is competent about the research methods, exactly as the adherents of cooperative methods have claimed all the time. Having said all this, I have to introduce a restriction.

The traditional methods of the social sciences are largely the result of analyzing the principles behind the methods of the natural sciences, accepting these principles as universally valid, and developing domain-specific methods based on these principles. Pleading for alternative principles and methods, I do not consider my own or others' arguments to be conclusive. All I may offer are reasons to apply the alternative methods in a number of situations and to try developing these methods as well as the underlying principles. If we do so, we may well find out that the alternative methods are no good after all because they keep leading to unreliable results or because their results may be arrived at easier, or in a more reliable way, by means of traditional methods. And we will ultimately decide about this by relying on the theories we were able to establish – always provisionally, of course. It is essential to realize that disagreements about methods should in principle be settled by studying their results and in view of accepted theories; the latter may interfere in the evaluation of results and will serve as background-knowledge. This fact prevents us from offering final arguments for communicative methods, but at the same time it shows the inconclusive character of the objections from traditional quarters, and provides us with a further argument for urgently intensifying the application and elaboration of the new methods.

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NOTES

1. For incomplete surveys on these schools, their history, and the advocated methods, I refer to the books by Ulrike Schneider, Peter Reason and John Rowan, and Chris Argyris, Robert Putnam and Diana McLain Smith; all are listed in the reference section.
2. Of course, there have been some attempts to cover the underlying problems within an analytic framework, e.g., by Georg Henrik von Wright in his *Explanation and Understanding*, a book that has lead to interesting discussions, some of which are reported in *Essays on Explanation and Understanding*, edited by Manninen and Tuomela. All this tells more on earlier neglects of analytic philosophy, than that it

would rescue the old 'Verstehen' stuff.
3. I take this period to be over by now; see the preface to *Theory and Experiment*.
4. In "What social scientists don't understand", Paul Meehl points to the dangerous consequences of this conviction.
5. "... quam ego vix credidissem a tanto Viro prolatam esse, si minus acuta fuisset." *Ethices Pars Quinta*, Praefatio.
6. An interesting discussion of the 'strong program' is contained in *Scientific Rationality: The Sociological Turn*, edited by James Brown.
7. Some people that favor action theory or even work in it share with the traditional view the conviction that the internal states are too complex to be captured in a theory. They erroneously see this as a supplementary reason to reject the methods of the traditional social sciences.
8. The force of this argument should not be exaggerated. A smart adherent of the traditional position should refrain from talking about deforming mechanisms and simply try to show that it is possible to arrive at certain regularities by relating elements of external behavior, whereas these are lost if some of these elements are replaced by (other such elements, viz.) statements made by the observed. How this might be shown or even made plausible in general is hard to see.
9. See, e.g., Niels-Eric Sahlin, "The significance of empirical evidence for developments in the foundation of decision theory".
10. This argument is not ad hoc. As appears from my epistemological papers listed in the references, I have been defending precisely the inadequacy of traditional epistemology in this respect before I even heard of action science.

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