How Can Alfred Schutz's Phenomenology Increase the Fruitfulness of Popper's Methodological Individualism?

CHRISTIAN ETZRODT*

Abstract: The application of Popper's falsification approach in the social sciences is extremely problematic as a result of his rationality assumption. In order to make Popper's critical rationalism applicable to the social sciences, the author proposes a combination of Popper's and Alfred Schutz's methodologies. Schutz provided two important solutions to Popper's problems. He made first a distinction between first- and second-order scientific models, which allows for rational theories of irrational behavior. And secondly, he advocated the postulate of adequacy as a solution for interpretation problems. On the other hand, Popper's concept of falsification can clarify Schutz's postulate of adequacy.

Key words: Alfred Schutz / Karl R. Popper / David Hume / falsification / verification / discourse

Karl R. Popper's critical rationalism provided an original solution to the problem of induction in the philosophy of science. He replaced the aim of showing that *one* theory is true with the aim of showing that *some* theories are wrong. Although this solution is neither perfect nor conclusive,[1)] it is in my opinion still the best methodology, because it demands of us a self-critical attitude. However, Popper's rationality assumption as a heuristic principle produces severe problems for the application of his approach in the social sciences. He effectively treated the social sciences as natural sciences, without realizing that the social sciences have additional problems, which require additional solutions. As Wettersten (1999, 523) stated, the problem is improving the Popperian program by accounting for irrational actions in social-scientific explanations. This paper tries to give an answer to this problem by adding the methodological insights of Alfred Schutz's phenomenological sociology to Popper's critical rationalism. I shall start with a discussion of Karl Popper's rejection of David Hume's psychological solution to the induction problem, in order to show that Alfred Schutz's position can be regarded as a synthesis of these two opposing standpoints. This is followed by a description of Schutz's methodology and his postulate of adequacy as the solution for interpretation problems in the social sciences. Finally, I will turn to the question of whether Popper's principle of falsification can again improve Alfred Schutz's methodology. This is, as far as I know, the first discussion of an application of the falsification principle to singular statements or phenomena.

* Part-time Lecturer, Faculty of Social Sciences, Ritsumeikan University

1. Popper's critique of Hume's psychological solution to the induction problem

David Hume was one of the most significant philosophers of the Scottish Enlightenment. His aim was an analysis of the foundation of empiricism, which led him to the problem of induction. Hume showed in his critique that the method of induction does not justify objective knowledge. His problem of induction can be formulated in the sense of Popper (1972, 93) as follows: "Is induction at all justifiable? And is there any reason why we should think it justifiable?" Since an inductive conclusion is defined as a conclusion from singular statements, for example, observations or experiments, to general statements or hypotheses or theories (Popper 1934, 3), and since general statements are necessarily abstract and impossible to experience, the question arises, how can we be sure that our unobservable general statements are true? In his attempt to answer this question Hume made a distinction between two aspects of the problem. He separated a logical problem H_L from a psychological problem H_P.

> H_L Are we justified in reasoning from [repeated] instances of which we have experience to other instances [conclusions] of which we have no experience? (Popper 1972, 4)
>
> H_P Why, nevertheless, do all reasonable people expect, and believe, that instances of which they have no experience will conform to those of which they have experience? That is, Why do we have expectations in which we have great confidence? (Popper 1972, 4)

Hume's answer to the logical problem H_L is that no reasoning can be justified to establish "that those instances of which we have had no experience, resemble those of which we have had experience" (Hume 1739/40, 89). Therefore, we are not allowed "to infer unobserved cases [general statements] from observed cases [singular statements], however many" (Popper 1972, 89).

> Let men be once fully perswaded of these two principles, *That there is nothing in any object, consider'd in itself, which can afford us a reason for drawing a conclusion beyond it*; and, *That even after the observation of the frequent or constant conjunction of objects, we have no reason to draw any inference concerning any object beyond those of which we have had experience* ... (Hume 1739/40, 139; emphasis in the original)

The reason why Hume so clearly rejected the method of induction as a foundation for logically valid inferences from singular statements to general statements is the impossibility of verifications. The problem of verifying general statements lies in the fact that they refer not only to the past but also to the future, which we can never know (Hume 1739/40, 91–2). It would only be reasonable to allow such inferences if we are sure "that the course of nature continues always uniformly the same" (Hume 1739/40, 89). But this is an assumption which

again cannot be verified. Furthermore, we "can at least conceive a change in the course of nature; which sufficiently proves, that such a change is not absolutely impossible" (Hume 1739/40, 89). Therefore, the method of inductive verification failed to provide a logically valid basis for the construction of general statements.

Hume was now troubled by the problem that human beings seem to use inductive procedures all the time, although such a procedure cannot be justified logically. This observation led him to the psychological problem H_P of induction. On what grounds do reasonable people believe in the validity of an inductive procedure, if it violates logic? Hume's answer to this question is habit or custom. For him the ideas[2] of different objects are united in the imagination, but this procedure is not based on reason (Hume 1739/40, 92). It is the result of conditioned associations based on repeated experiences (Popper 1972, 4).

> ... I conclude, that the belief, which attends the present impression, and is produc'd by a number of past impressions and conjunctions; that this belief, I say, arises immediately, without any new operation of the reason or imagination. Of this I can be certain, because I never am conscious of any such operation, and find nothing in the subject, on which it can be founded. Now as we call every thing CUSTOM, which proceeds from a past repetition, without any new reasoning or conclusion, we may establish it as a certain truth, that all the belief which follows upon any present impression, is deriv'd solely from that origin. (Hume 1739/40, 102)

Of course, such an inductive procedure cannot be called anymore a method of verification, because if it is applied automatically, it lacks the conscious aim of verifying the truth. It would probably be better to define it as a *method of inductive typifying*, which would more clearly describe the pragmatic character of this procedure. The idea or type of an object is for Hume equivalent to *one* experienced typical instance. Since he believed that 1) all knowledge is based on experiences and 2) generalizations are logically not valid, he solved the paradox "that some ideas are particular in their nature, but general in their representation" (Hume 1739/40, 22) by choosing *one* experienced case and annexing it to a general term to represent all other similar cases. And if a similar case is observed, then the chosen typical case is habitually recalled in imagination (Hume 1739/40, 22, 24, 87).

Instead of a logically valid but impossible method of inductive verification, Hume's solution was a logically invalid but possible method of inductive typifying in the sense of a habitualization. Hume (1739/40, 118) concluded that human knowledge is basically irrational, because human beliefs rely on repetitions in the past, which cannot provide any validity for general statements. Human knowledge becomes finally an irrational faith for Hume (Popper 1972, 4–5). But for Popper this was an undesirable result for his search for truth and objective knowledge, because if he had accepted Hume's conclusion, his search would have become useless, since it would be impossible to come close to the truth (Popper 1972, 90). Therefore he tried to break Hume's chain of argumentation described as follows:

Hume argues:

(1) Induction (that is, induction by repetition) is rationally totally invalid.
(2) As a matter of fact, we do rely in our actions (and thus in our belief) on the existence of some reality which is not completely chaotic.
(3) This reliance of ours is, in view of (1), irreparably irrational.
(4) Thus human nature is essentially irrational. (Popper 1972, 100)

Popper accepted the first and second of Hume's theses, but he rejected the third thesis—the conclusion drawn out of the first two—because he stated correctly that the method of inductive verification is not the only method for producing valid general statements (Popper 1972, 100). Therefore, if human reasoning were based on a method other than induction, which would guarantee the validity of general statements, Hume's conclusion that human knowledge is irrational would not be necessary. Popper (1934) showed that such an alternative method exists. It is the method of deductive falsification. General statements or theories or laws are regarded as hypotheses, because they cannot be verified (Popper 1972, 30; 1963, 72, 139). These general statements are not derived by induction, but are formulated as bold guesses, as conjectures (Popper 1972, 30). From these hypothetical general statements, prognoses are deduced which are tested against the experience. If the conjectures do not stand the tests, then they are falsified or refuted.

Popper's argument is, therefore, that although Hume showed the impossibility of inferring a theory from observations, it must not be impossible to refute a theory by observations (Popper 1963, 72). The latter is possible, because it requires much less than the method of inductive verification. Inductive verifications refer necessarily to infinite cases in the past and in the future (Popper 1972, 305), but for falsifications only a few negative examples are enough. The method of inductive verification has the aim of formulating absolute true general statements, but the method of deductive falsification wants only to show that *some* conjectures are refuted (cf. Kageyama 2003, 104). And by eliminating wrong theories, some of the surviving theories might come close to the truth (cf. Popper 1972, 30).

However, Popper did not make the distinction between the method of inductive verification and inductive typifying. He believed that if induction is not logical, then it cannot exist at all. If induction is impossible in logic, then it must also be impossible in psychology.

> One of my main results is that, since Hume is right that there is no such thing as induction by repetition in *logic*, by the principle of transference there cannot be any such thing in *psychology* (or in scientific method, or in the history of science): the idea of induction by repetition must be due to an error—a kind of illusion. In brief: *there is no such thing as induction by repetition*. (Popper 1972, 6f; emphases in the original)

Because Popper did not make a distinction between inductive verification and inductive typifying, he was forced to claim the method of deductive falsification as the natural or genetically determined method of human reasoning, to avoid Hume's conclusion of the

irrationality of human knowledge. He was forced to contest Hume's view of the common sense way of thinking. Consequently, he declared the experience of applying inductive procedures as an illusion (Popper 1974, 1015; 1972, 6–7) based on the belief in a primitive psychology (Popper 1972, 96).

> I disagree with Hume's opinion (the opinion incidentally of almost all philosophers) that induction is a fact and in any case needed. I hold that neither animals nor men use any procedure like induction, or any argument based on the repetition of instances. The belief that we use induction is simply a mistake. (Popper 1974, 1015)

My objection is that Popper's hypothesis about the nature of human reasoning completely contradicts our experience and intuition. Why it is so difficult for other philosophers to accept Popper's solution to Hume's induction problem if his solution is only describing what everybody is practicing? I would guess the reason for these difficulties is that human beings do not use a method of deductive falsification in their natural attitude. Only if the natural attitude can be made problematic, or if a "Gestalt switch" can be made, then the method of deductive falsification can be understood as Popper stated (cf. Popper 1974, 1043–4). Furthermore Popper's hypothesis is not supported by experiments. Human beings do not falsify their theories if counter-examples appear. Most often counter-examples are ignored or not even perceived at all (Camerer 1995, 595). Wason (1968) directly tested the falsification hypothesis against the verification hypothesis. Cards with an "E" or "K" on one side and a "4" or "7" on the other side were given to the subjects in this experiment and they were confronted with the following problem: "Every card with a vowel on one side has an even number on the other side. Which cards must you turn over to test whether the rule is true or false?" The most often answers were "E" and "E & 4", but this implies a verification strategy. The correct answer for the method of falsification would have been "E & 7". Therefore, Popper's hypothesis that human beings apply a method of deductive falsification in their natural attitude is clearly falsified.

I can agree with Popper's solution to the logical induction problem. It is correct as Hume and Popper said that a method of inductive verification is impossible, and Popper is right in showing that a deductive falsification is possible and that it can produce logically valid results. However, in my opinion Popper is wrong in stating that this falsificationist approach is the common-sense way of thinking. Human actors practice in their natural attitude a method of inductive typifying. But they are also capable of learning other methods. Therefore, Popper is right in demanding a more sophisticated method for scientific research, one which is logically justified, but his hypothesis, that human actors think like scientists should think, is wrong. The problem is that Popper does not make a distinction between scientific first-order constructions of the world and scientific second-order constructions of the actors' first-order constructions of the world. As a result, logically valid scientific procedures and logically invalid common-sense procedures cannot coexist for Popper. But I think that two completely different problems are involved in this problem. One is a normative and the other a

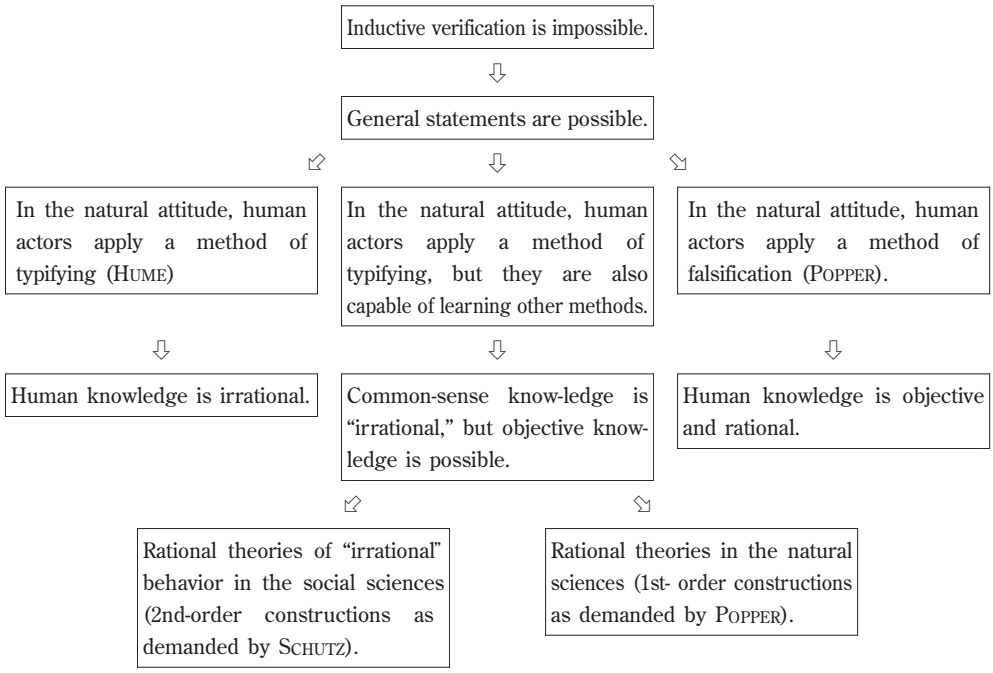

Table 1: A synthesis of Hume's and Popper's hypothesis about human reasoning

descriptive problem. The normative problem is the question: How can we guarantee objective knowledge in science? And the descriptive problem is: How are human actors constructing general statements about the social world? To the first question, if we accept objective knowledge as the aim of science, then we need scientific methods, which produce logically valid statements. But the aim of objectivity forces us also to describe something as "irrational," if it is "irrational." Therefore, we have to apply a method of deductive falsifications in the social sciences, but this should not be confused with the content of our scientific theories, which might also be irrational. For example, a hypothesis that no patriarchal religion accepts the idea of infinite reincarnations could be formulated, and the attempt to falsify this statement would probably fail. Then this hypothesis could be regarded as corroborated by this scientific method, but this hypothesis would still tell us only something about what religious people in patriarchal societies think about reincarnations, independent of the question of whether this belief itself is irrational or rational or logically justified.

In this sense a third solution to Hume's induction problem can be formulated, one which regards a method of inductive typifying as the naturally applied way to formulate general statements but which also requires—as Popper demanded—a logically valid method of deductive falsifications for scientific theory constructions (see Table 1). The advantage of this approach is that it can avoid Popper's wrong assumption about the common-sense reasoning and Hume's undesirable conclusion that all human knowledge is irrational. This standpoint leads to two types of theories: rational theories as first-order constructions in the natural

sciences (as demanded by Popper for all sciences) and rational theories of "irrational" behavior or reasoning as second-order constructions in the social sciences.

The latter was demanded by Alfred Schutz. Schutz stated without any doubt that theories have to be constructed in a rational way, but this should not lead to a construction of rational models of rational behavior. Methodology and the content of theories should not be mixed up. For him the formulation of rational models of whatever kind of behavior was the appropriate approach.

> The rationality of the construction of the model is one thing and in this sense all properly constructed models of the sciences—not merely of the social sciences—are rational; the construction of models of rational behavior is quite another thing. (Schutz 1953, 35)
>
> We have already noted that the concept of rationality has its native place not at the level of every-day conception of the social world, but at the theoretical level of the scientific observation of it, and it is here that it finds its field of methodological application. (Schutz 1943, 143)

However, the question remains how Schutz wanted to guarantee the rationality of methods in the social sciences.

2. Schutz's postulate of adequacy

Alfred Schutz's methodological problem was the problem of intersubjectivity or the Cartesian Dilemma, as it was called by Helmut Grathoff. This problem arose for him out of the "Methodenstreit" in economics between the Austrian School of Economics of Carl Menger and the German Historical School of Economics of Gustav Schmoller. The "Methodenstreit" was the result of the marginal revolution independently initiated by Menger, Leon Walras, and William Jevons. It was a shift inside economics from a historically oriented holistic approach based on the method of inductive verification towards a theory-driven approach based on methodological individualism and the method of inductive intuition.[3] Schutz as an Austrian lawyer, economist, and sociologist was a member of the Austrian School of Economics. He accepted methodological individualism as his starting point (Schutz/Luckmann 1979, 13), but he saw a problem in the method of inductive intuition applied by Menger and Mises. Menger and Mises introduced the economic laws as true *a priori* (Menger 1883, 42; Mises 1962, 5f., 44; Smith 1986, 3–4). They assumed that they were able to grasp the essence of the basic concepts by intuition following Aristotle's philosophy (Menger 1883, 6–7, footnote 4; Kauder 1958, 414; Hansen 1968, 161; Smith 1986, 3). Alfred Schutz—in his attempt to defend the Austrian School of Economics (Prendergast 1986, 4)—tried to give his school a better philosophical foundation. In doing so, he turned to Max Weber's methodology. Weber had a mediating position in the "Methodenstreit." He was interested in historical problems, but he agreed with Menger and Mises that the social

sciences have to be based on methodological individualism. However, Weber rejected the method of inductive verification as well as the method of inductive intuition (Turner/Factor 1984, 38; cf. Weber 1922, 208–9; Burger 1994, 78). He proposed instead starting with arbitrary definitions of ideal-types (Weber 1922, 193, 213, 287) and guaranteeing the objectivity of science by the criterion of internal consistency (cf. Weber 1922, 184). In other words, every researcher is allowed to choose his own subjective perspective and problem, but he has to define his key concepts precisely and has to deduce his interpretations and conclusions in a consistent way out of his definitions. Weber's approach is indeed very close to the *deductive application of theoretical concepts* in the Austrian School, without accepting that the validity of these concepts in the process of *inductive concept formation* (or definition) can be guaranteed by intuition (Turner/Factor 1984). Therefore, we would be forced to accept their arbitrariness. This is the reason why Weber called his general terms *ideal-types* and not *a priori true concepts*.

Schutz agreed with Weber's solution, but he was not convinced that the arbitrariness of types needed to be accepted. In Schutz's view, Weber failed to realize all the implications of his method of "understanding" and of the process of typifying. Weber's ideal-types and his method of "understanding" were an inspiration for him, but he replaced them with his own concepts. Finally he tried—influenced by Edmund Husserl—to solve the problems of intersubjectivity and of the constitution of the meaning structure for the common-sense way of thinking, before he transferred the results to an adequate scientific method. This made sense to Schutz (1932, 276; Srubar 1994, 260–1), because for him the position of a researcher was principally equivalent to the position of an interpreting person in the world of contemporaries (*Mitwelt*). Both contemporary and researcher interpret an observed behavior based on a scheme with the assumption that the actor has guided his behavior on a similar scheme. But if the interpreter as well as the actor refers to a similar scheme, then an arbitrarily selected scheme of interpretation as Weber supposed seems to be inappropriate. Consequently, Schutz concerned his study with the problem of intersubjective understanding as a mundane problem within the natural attitude (Schutz 1932, 107; cf. Schwinn 1993, 226).

Schutz solved the problem of intersubjectivity in several steps. He started with the problem of how a phenomenologically reduced ego can become aware of other actors, their existence (*Dasein*), concreteness (*Sosein*), and of their concrete motives of their actions (Vaitkus 1991, 77). Schutz (1932, 30–1, 42) regarded the existence of the alter ego in his mundane reduction as unproblematic. And he approached the problem of the concreteness of the alter ego from a primary experience in a close "*we-relationship*" (Schutz 1932, 184), a relationship in which ego and alter ego are mutually aware of each other (Schutz/Luckmann 1979, 91). In such situations the attention is directed to the alter ego, which makes it possible for the actors to empathize with the alter ego's feeling and thinking. In these moments the stream of consciousness of the actors runs simultaneously—they grow older together (Schutz/Luckmann 1979, 91; cf. Zaner 1961, 82; Natanson 1962, xxxiif.; Grinnell 1983, 185). This characteristic nearness of the experiences in time and space (*Erlebnisnähe*) creates a subjective experience in general (*Erfahrung*), which can be shared with others (Schutz 1932,

183f., 196). These shared experiences are then used to form types of actors, actions, and situations (cf. Schutz 1932, 205; Schutz/Luckmann 1979, 98–9). Dependent on the level of anonymity of the relevant actors, actions or situations, more abstract or more concrete types are applied as a scheme of interpretation (Schutz 1932, 222; Grathoff 1977, 73) based on the assumption that the observed behavior as well was guided by a similar scheme dependent on similar experiences.

This result led Schutz to the next problem: how can we be sure that an *unknown* alter ego had similar experiences and therefore applies a scheme similar to our scheme of interpretation? Schutz's answer was that in fact most subjective experiences were undergone in social situations. And if these social situations follow a specific pattern, then we can expect that the actors had the same typical experiences in similar situations. The existence of intersubjective types is therefore secured because of the *social origin of knowledge* in typical social situations, the *knowledge about the social distribution of knowledge* (I know that a physician typically knows something about medicine, even if I know nothing about it), and the *reciprocity of perspectives*, which means that the actors deal pragmatically with biographical differences and their standpoints are exchangeable, because of an assumed congruence of the relevance system (Schutz 1953, 7; Zaner 1961, 84ff.; Lachowska 1980, 47). Actors can understand each other, because they refer to their typical experiences by guiding their actions or by interpreting an observed action. Further, by referring to the typical experiences in terms of their interpretations and their actions, the actors will reconstruct the social situation, which again can be experienced by new members of a society as a typical social situation. *It is therefore the existence of a constantly reconstructed pattern in the social situations* which leads to the possibility that different actors can make similar and intersubjectively shared experiences. Finally, intersubjectivity is guaranteed by an experience of the existence and concreteness of other actors in a close "we-relationship" on a prephenomenal level and by a typical experience of specific patterns in social situations on a phenomenal level.

Schutz's concept of ideal-types is therefore a synthesis of Weber's concept and Mises's position (Prendergast 1986, 14ff.; Srubar 1994, 263). He rejected with Weber that general statements or ideal-types are *a priori* true, and he agreed with Mises that general statements or ideal-types are invariant, which contradicts Weber's concept of arbitrary ideal-types. Ideal-types are in this sense—for normal actors in the everyday world as well as for scientists—"heuristic principles of great generality" based on patterns in social situations "placed at the head of a deductive chain" (Prendergast 1986, 12; cf. Kaufmann 1925, 648–9). Schutz's ideal-types are a heuristic principle, because there is only a chance that they were constructed adequately. But this chance is not a probabilistic matter, if the construction of the ideal-types is guided by the shared experiences of the actors. In this case, the scheme of interpretation of the observer matches adequately the scheme of orientation of the actor (Srubar 1994, 262ff.).

However, ideal-types are not fixed, but adjust to the new experiences the actor has (Schutz 1944, 507; Schutz/Luckmann 1979, 30, 34). The construction or adjustment of ideal-types basically follows Hume's method of inductive typifying in the sense of a habitualization.

Nevertheless, there are a few differences. Hume described the process of typifying as a selection of *one typical experienced* instance to represent all other similar instances. For Schutz the process of typifying was an act of the mind to formulate an *abstract and never experienced* type out of similar experienced instances. Knowledge is based on sensual experiences, but pure general experience (*Erfahrung* in the sense of experienced) does not exist. General experience itself is a product of the mind. It is an abstraction of several concrete experiences in time and space (*Erlebnis* in the sense of experiencing).

The distinction to Popper on the other side is the question of what guarantees the rationality or validity of theories. For Popper the origin of theories—for example, guesses or intuitions—was completely unimportant, because only through tests can a theory be falsified. Schutz on the other hand believed that the origin of theories or their method of construction plays a vital role in the social sciences. He argued that social scientists can only make statements close to the truth about the ideas and beliefs of the observed actors if they were applying the same "irrational" procedures used by the observed actors, *and* if the social scientists and the observed actors share the same cultural background, which allows the application of similar abstract types (cf. Schutz 1932, 276; Williame 1973, 104). This standpoint was clearly rejected by Popper. For him a researcher's familiarity with the observed actors' ideas and beliefs was unnecessary (Popper 1945, 97; 1994, 169). He stated that all actors behave rationally to avoid the cultural dimension. Of course, Popper also realized that this hypothesis is wrong (Popper 1994, 172). Therefore, he declared it to be an empirically empty heuristic principle to immunize it against critique (Popper 1994, 169, 177–8). It is obvious that this strategy violates the basic principles of Popper's method of deductive falsification (Vanberg 1975, 109–33; Schmid 1979, 17, 26; Wettersten 1999, 520ff.; Albert 2003, 81–2). It therefore remains a problem to deal with the ideas and beliefs of human beings, if we assume that their ideas and beliefs have an influence on their behavior.

In my opinion, Schutz offered a plausible solution to this dilemma. His method of inductive typifying allows adequate type constructions, because real actors apply the same method in everyday life. Of course, the method of typifying is unacceptable from a logical point of view. But the aim here is not to obtain true statements about the physical world (first-order constructions), but to receive true statements about the world of ideas and beliefs of the actors (second-order constructions). And if human beings use an "irrational" method to form their beliefs, then the best strategy to find the truth is to use the same "irrational" method. However, it is important not to confuse what these second order constructions are valid for. They are only statements about the beliefs and ideas of the human actors, and they say nothing about the real world. If people experience a thunderstorm, and if they explain this thunderstorm causally by the anger of their gods, then a true statement would be that these people believe in the determination of nature by the will of the gods. But this does not mean that gods in the physical world really exist.

However, it remains an important question whether Schutz thought that a test of the constructed types would be necessary, or if the common-sense method of constructing them alone could guarantee the validity of the types. Three points might indicate that Schutz

regarded a test as necessary. First, he showed by describing in his paper 'The Stranger' his experiences after his immigration to America that the method of typifying can lead to systematically wrong interpretations if the actors do not share a common cultural background. It would be therefore important to confirm the adequacy of the applied scheme of interpretation. Second, Schutz spoke in his action theory of rational models of whatever kind of behavior. This seems to imply a clear distinction between the content of types—which could be irrational—and the rational confirmation and application of them. And a rational confirmation would require some kind of test. Third, Schutz (1953, 34) demanded three principles for scientific-type constructions: the postulate of logical consistency, the postulate of subjective interpretation, and the postulate of adequacy. McLain interprets the distinction between the postulate of subjective interpretation and postulate of adequacy as a sign that Schutz regarded tests or verifications as necessary.

> The postulate of logical consistency refers to the similarity between the procedures of the natural sciences and those of the social sciences, while the postulate of subjective interpretation refers to the specific difference between the two. The distinction between the postulate of adequacy and the postulate of subjective interpretation, Schutz seems to imply, rests on the fact that the latter warrants the *possibility* that sociological constructs will be consistent with the subjective meanings of actors, but that the former is necessary *in addition* in order to verify that consistency has *in fact* been established. (McLain 1981, 106–7; emphases in the original)

However, Schutz himself is not clear on this point (McLain 1981, 107; Carroll 1982, 392). His statements about the postulate of adequacy are at least ambiguous.

> Each term used in a scientific system referring to human action must be so constructed that a human act performed within the life-world by an individual actor in the way indicated by the typical construction would be reasonable and understandable for the actor himself, as well as for his fellow-men. (Schutz 1943, 147; cf. 1953, 34)
>
> The postulate of adequacy requires that the typical construction be compatible with the totality of both our daily life and our scientific experience. (Schutz 1943, 148; cf. 1932, 270)

The first statement can in my opinion be interpreted as a demand for some kind of tests, because it is only possible to know that a non-scientist understands the scientific types, if he is asked (Coenen 1985, 92). On the other hand, the second statement seems to indicate that such a test is unnecessary and that the method of inductive typifying alone can guarantee the adequacy of types, because the types only have to be compatible with *our* daily life and scientific experience.

3. Towards an adequate formulation of the postulate of adequacy

So far I have concerned myself with Alfred Schutz's methodology and his definition of the postulate of adequacy. But the problem of adequacy can also be approached independently of Schutz's ambiguous position. I will discuss here the question of how to guarantee the validity of types in the social sciences and no longer the question of how Schutz wanted to guarantee it. Three answers can be given to this problem:

1) Verification of a singular statement;
2) Falsification of a singular statement;
3) Discursive dialogue.

The difference between verification and falsification of singular statements can be made obvious in a conversation between two actors. If one actor does not understand the other one, he can ask him: "What do you mean by this statement?" He/she is asking him/her in this case to *verify* his/her former statement with probably alternative words. But if he/she asks "Do you mean this and that with your statement?", then he/she is asking him/her to *falsify* the interpretation with "no" or to confirm it with "yes." The problem with the verification of singular statements is that misinterpretations can occur (McLain 1981, 121). Actors and researchers can never be sure that their interpretation was correct, although they might have had the impression that they completely understood. Therefore, verifications seem not to be the best method to test the validity of applied scientific types. In my opinion the approach of falsifying singular statements is superior to verifications. Although we cannot be sure about the correctness of our interpretations, we can be sure that something must be wrong if our interpretation is rejected by the actor whose statement we interpreted. However, we cannot be sure about the source of the misunderstanding. It could be that my interpretation was incorrect or that the actor's interpretation of my interpretation was wrong. The method of falsification offers not much more than the method of verification for checking singular statements, but the fact that we can be sure of misunderstandings is reason enough to prefer it.

An alternative to the method of falsifying singular statements was proposed by McLain (1981, 120ff.) and Carroll (1982, 397) with the method of a discursive dialogue of critical theory. McLain and Carroll accept Richard Bernstein's critique of the postulate of adequacy in parts and try to adjust Schutz's postulate of adequacy to this critique. For Bernstein the postulate of adequacy is unsuitable and dangerous. It is unsuitable because it cannot deal with the actor's stubborn refusal to see the truth or with the actor's alienation, which prevents him from seeing the truth. Therefore, the postulate of adequacy is dangerous, because it can only lead to ideological but not scientific types. Schutz is depriving the social sciences of its critical character, because he is not aware of the problem that actors may only be able to understand those constructs which are in accordance with the dominant ideology of the culture or group under study (Bernstein 1976, 164; McLain 1981, 110).[4] In McLain's and Carroll's opinion a discursive dialogue could solve this problem. McLain describes the procedure as follows:

> The typical case may be conceived of in which the actor rejects the second-order constructs of the scientist's model as either unintelligible or unrepresentative of the actor's motivation. The rejection may result from the inadequacy of the scientific model or from the actor's present incomplete understanding of its import or from the effects of repression or some reigning ideology. In any event, the actor's rejection is sufficient reason for continuation of the dialogue, and compliance with the criterion of adequacy remains undemonstrated. As the dialogue proceeds, the sociologist may revise or reject certain features of the model on the basis of an increased understanding of the actor's meanings, may attempt to clarify and deepen the actor's motivational self-understanding and understanding of the scientific model, or may employ strategies to reduce the resistance arising from repression or ideology. (McLain 1981, 123)

A mix of the researcher's powers of persuasion and of his acceptance of the actor's statements is the solution. Unfortunately this is not very clear advice. When does a researcher get to the point where he "too hastily [arrives] at the judgment that the viewpoint of the actor is irrelevant or misguided" (McLain 1981, 125)? McLain argues against Bernstein that the postulate of adequacy is necessary to constrain the risk "that critical activity will be arbitrary and irrelevant to actually existing social and political realities, thereby undermining the critical potential of sociology" (McLain 1981, 126), but he is unwilling to accept "the subjugation of science to common-sense" (McLain 1981, 125). The basic reason to reject a method of verification or falsification of singular statements is that the actor's statements cannot be trusted (McLain 1981, 123; Carroll 1982, 401). But why should we listen to the actor's statements if they are not trustworthy? In my opinion, the actor's statements are the only basis for the application of types to individual cases. Of course, we do not need to believe everything they say. However, it is not for the researcher to decide what he wants to believe and what not. If the actor's statements are in contradiction to themselves or to reality (for example, a contradiction between a statement about the actor's past behavior and his real past behavior), then we have good reason not to trust them. But as long as no contradictions occur, the actor has the final word. There is no need for a discourse.

Finally, the *extended solution* of Alfred Schutz to the problem of *Verstehen* in the social sciences can be characterized as a method of inductive typifying for the construction of general types (as proposed by Hume) and a method of deductive falsification for the application of these general types to singular cases (inspired by Popper[5]). The method of inductive typifying is adequate—although logically invalid—because the actors apply the same method in their natural attitude. The aim is to formulate accurate second-order constructions of the actors' first-order constructions, independent of the question of how irrational the first-order constructions might be. Adequate constructed general types can be expected, because of the social origin of our shared social knowledge, as long as the researcher is familiar with the cultural background of the actors under study. The adequacy will increase with the researcher's familiarity with the actors' everyday life. The method of deductive falsification of applied general types to singular cases additionally supports, in my opinion, the postulate of

adequacy. An actor's behavior, his motives and ideas are interpreted with the scheme of interpretation provided by the method of inductive typifying. But this interpretation is not simply accepted. Instead the actor is asked to deny or to confirm the interpretation. If he confirms the interpretation, the interpretation can still be wrong (because of misinterpretations based on discrepancies in the cultural background), but if he rejects the interpretation, then the interpretation is falsified. But only the application of the general scheme of interpretation to this singular case is falsified, not the general scheme itself. Of course, if a scheme of interpretation constantly fails, then the researcher is probably not very familiar with the culture under study.

I can of course not be sure that I have interpreted Schutz's postulate of adequacy adequately, because I can no longer ask him to confirm or to falsify my conjecture that the method of falsification of singular statements should be regarded as an aspect of his postulate of adequacy, but it seems to me a logical extension of his work. However, this case also shows the limits of the proposed additional solution. The method of falsification of singular statements can only be applied if the actors can be asked and if they are willing to answer (but a correct self-understanding is not required). If this is not the case, then the method of inductive typifying and the familiarity with the actors' culture alone have to guide adequate type constructions without any further confirmation or falsification. Either way Alfred Schutz's methodology provides an important solution to Popper's program, because it is no longer necessary to assume actors to behave rationally. The researcher's cultural knowledge can be used to formulate more adequate social-scientific explanations of any kind of behavior whatever. While this combination of Popper and Schutz still does not permit conclusive falsifications, it allows us (in some cases) to test our hypotheses about the actors' motives.

Notes

1) I agree with Kageyama's (2003) interpretation of Popper's approach as an inconclusive falsificationist one. "To abandon a theory simply by a falsification entails blind faith in the truth of test results" (p. 115), and therefore "falsifications are not absolutely certain," because "we cannot exclude the possibility that they may turn out to be false" (p. 113).
2) For Hume, ideas are closely connected to experiences and not an abstract phenomenon of the mind: "Ideas always represent their objects or impressions; and *vice versa*, there are some objects necessary to give rise to every idea." (Hume 1739/40, 157) "*... all ideas are copy'd from impressions*." (Hume 1739/40, 163; emphasis in the original)
3) Also the method of intuition can be described as inductive, because it is based on experience, but in contrast to the method of inductive verification it refers not to infinite cases, but only to past experiences. The gap between known cases and all cases including unknown cases is bridged by intuition.
4) The problem with Bernstein's argument is that he confused first- and second-order constructions. The actors might be alienated as a result form inadequate first-order constructions. But this is not a scientific but rather a political problem. The scientific problem is to construct adequate second-order concepts from the perhaps inadequate first-order

concepts of the actors. I can therefore not see that Bernstein's critique suits Schutz's postulate of adequacy at all, because Schutz did not propose the postulate of adequacy as a solution for the problem of first-order constructions. It is furthermore clearly not more dangerous than to say that the researcher knows it better than the observed actor (McLain 1981, 111). This latter standpoint can drift very easily—as Popper showed—to all forms of fanaticism and intolerance.

5) It is only inspired by Popper, because he rejected the application of the method of falsification to singular statements.

References

Albert, H. 2003. Weltauffassung, Wissenschaft und Praxis: Bemerkungen zur Wissenschafts- und Wertlehre Max Webers. In *Das Weber-Paradigma: Studien zur Weiterentwicklung von Max Webers Forschungsprogramm*, edited by G. Albert, A. Bienfait, S. Sigmund and C. Wendt, 77–96. Tübingen: Mohr.

Bernstein, R.J. 1976. *The Restructuring of Social and Political Theory*. Oxford: Blackwell.

Burger, T. 1994. Deutsche Geschichtstheorie und Webersche Soziologie. In *Max Webers Wissenschaftslehre*, edited by G. Wagner and H. Zipprian, 29–104. Frankfurt: Suhrkamp.

Camerer, C. 1995. Individual decision making. In *The Handbook of Experimental Economics*, edited by J.H. Kagel and A.E. Roth, 587–703. Princeton: Princeton University Press.

Carroll, R. 1982. Adequacy in Interpretative Sociology: A Discussion of Some of the Issues and Implications of Schutz's Postulate of Adequacy. *Sociological Review* 30: 392–406.

Coenen, H. 1985. *Diesseits von subjektivem Sinn und kollektivem Zwang*. München: Fink.

Grathoff, R.H. 1977. Ansätze einer Theorie sozialen Handelns bei Alfred Schütz. In *Handlungstheorien interdisziplinär IV*, edited by H. Lenk, 59–79. München: Fink.

Grinnell, F. 1983. The Problem of Intersubjectivity: A Comparison of Martin Buber and Alfred Schutz. *Human Studies* 6: 185–95.

Hansen, R. 1968. Der Methodenstreit in den Sozialwissenschaften zwischen Gustav Schmoller und Karl Menger: Seine wissenschaftshistorische und wissenschaftstheoretische Bedeutung. In *Beiträge zur Entwicklung der Wissenschaftstheorie im 19. Jahrhundert*, edited by A. Diemer. Meisenheim: Hain.

Hume, D. 1739/40 (1978). *A Treatise of Human Nature*. Oxford: Clarendon.

Kageyama, Y. 2003. Openness to the Unknown: The Role of Falsifiability in Search of Better Knowledge. *Philosophy of the Social Sciences* 33: 100–121.

Kauder, E. 1958. Intellectual and Political Roots of the Older Austrian School. *Zeitschrift für Nationalökonomie* 17: 411–25.

Kaufmann, F. 1925. Logik und Wirtschaftswissenschaft: Eine Untersuchung über die Grundlagen der ökonomischen Theorie. *Archiv für Sozialwissenschaft und Sozialpolitik* 54: 614–56.

Lachowska, D. 1980. Alfred Schütz's Theory of Intersubjectivity. *Polish Sociological Bulletin* 51: 41–50.

McLain, R. 1981. The Postulate of Adequacy: Phenomenological Sociology and the Paradox of Science and Sociality. *Human Studies* 4: 105–30.

Menger, C. 1883. *Untersuchungen über die Methode der Socialwissenschaften, und der Politischen Oekonomie insbesondere*. Leipzig: Duncker & Humblot.

Mises, L.v. 1962. *The Ultimate Foundation of Economic Science*. Princeton: Van Nostrand.

Natanson, M. 1962. Introduction. In *Collected Papers*, vol. 1, by A. Schutz, xxv-xlvii. The Hague: Nijhoff.

Popper, K.R. 1934 (1989). *Logik der Forschung*. Tübingen: Mohr.

_____. 1945 (1971). *The Open Society and Its Enemies*, vol. 2. Princeton: Princeton University Press.

_____. 1963 (2002): *Conjectures and Refutations: The Growth of Scientific Knowledge*. London/New York: Routledge.

_____. 1972 (1979). *Objective Knowledge: An Evolutionary Approach*. Oxford: Clarendon.

_____. 1974. Replies to My Critics. In *The Philosophy of Karl Popper*, vol. 2, edited by P.A. Schilpp, 959–1197. La Salle: Open Court.

_____. 1994. Models, Instruments, and Truth: The Status of the Rationality Principle in the Social Sciences. In *The Myth of the Framework: In Defence of Science and Rationality*, by K.R. Popper, 154–184. London/New York: Routledge.

Prendergast, C. 1986. Alfred Schutz and the Austrian School of Economics. *American Journal of Sociology* 92: 1–26.

Schmid, M. 1979. *Handlungsrationalität: Kritik einer dogmatischen Handlungswissenschaft*. München: Fink.

Schutz, A. 1932. *Der sinnhafte Aufbau der sozialen Welt*. Wien: Springer.

_____. 1943. The Problem of Rationality in the Social World. *Economica* 10: 130–49.

_____. 1944. The Stranger: An Essay in Social Psychology. *American Journal of Sociology* 49: 499–507.

_____. 1953. Common-Sense and Scientific Interpretation of Human Action. *Philosophy and Phenomenological Research* 14: 1–38.

_____. 1954. Concept and Theory Formation in the Social Sciences. *Journal of Philosophy* 51: 257–273.

_____. 1967. *The Phenomenology of the Social World*. Evanston: Northwestern University Press.

Schutz, A., T. Luckmann. 1979: *Strukturen der Lebenswelt*, vol. 1. Frankfurt: Suhrkamp.

Schwinn, T. 1993. *Jenseits von Subjektivismus und Objektivismus: Max Weber, Alfred Schütz und Talcott Parsons*. Berlin: Duncker & Humblot.

Smith, B. 1986. Austrian Economics and Austrian Philosophy. In *Austrian Economics: Historical and Philosophical Background*, edited by W. Grassl and B. Smith, 1–36. London/Sydney: Croom Helm.

Srubar, I. 1994. Wertbeziehung und Relevanz: Zu Alfred Schütz' Weber-Rezeption. In *Max Webers Wissenschaftslehre*, edited by G. Wagner and H. Zipprian, 259–277. Frankfurt: Suhrkamp.

Turner, S.P., R.A. Factor. 1984. *Max Weber and the Dispute over Reason and Value: A Study in Philosophy, Ethics, and Politics*. London/Boston/Melbourne/Henley: Routledge & Kegan Paul.

Vaitkus, S. 1991. *How is Society Possible?* Dordrecht/Boston/London: Kluwer Academic Publishers.

Vanberg, V. 1975. *Die zwei Soziologien*. Tübingen: Mohr.

Wason, P.C. 1968. Reasoning about a rule. *Quarterly Journal of Experimental Psychology* 20: 273–81.

Weber, M. 1922 (1985). *Gesammelte Aufsätze zur Wissenschaftslehre*. Tübingen: Mohr.

Wettersten, J. 1999. How Can We Increase the Fruitfulness of Popper's Methodological Individualism? *Philosophy of the Social Sciences* 29: 517–526.

Williame, R. 1973. *Les fondements phénoménologiques de la sociologie compréhensive: Alfred Schutz et Max Weber*. The Hague: Nijhoff.

Zaner, R.M. 1961. Theory of Intersubjectivity: Alfred Schutz. *Social Research* 28: 71–93.