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**Interdisciplinarity in the social sciences**

**Bateson's problem, analytical philosophy and anthropology**

*Abstract*

*Two formal questions are relevant with respect to the question of unifying the social sciences: First, would this synopsis really be sufficient for the satisfactory and exhaustive investigation of the study objects, amongst which* culture, society *and* mind*? Second: Would it be possible to safeguard the* consistency *of the proposed synopsis?*

*A rather underexplored area in socio-cultural anthropology and the social sciences in general is the body of questions that deal with the logical hierarchy of concepts. One of the few authors who paid attention to these questions was Gregory Bateson. The (im)possibility to distinguish levels in concept like 'culture', 'society' and 'mind' is relevant for the question whether or not the social sciences can be unified. As Bateson pointed out, logical hierarchy problems have been rarely systematically investigated, while it would have been most appropriate to do so with the help of analytical philosophy starting from Russell's paradox.*

*If concepts can be used as a reference for a* studied object *as well as the* environment *in which one studies, the question of how to deal with levels within social sciences becomes pertinent. I denote the quest for a theory dealing with the hierarchy of analytical levels in social sciences as* Bateson's problem*. Analytical philosophy, with such scholars like Russell, Gödel and Curry, can be a great help when investigating*

*Baseton's problem. In doing so, I intend to demonstrate that Bateson's problem is of such a nature that something like a common foundation for all social sciences is not very likely.*

## 1. Introduction

When we talk about the possibility and desirability of unity within the social sciences, we must occupy ourselves not only with methodological problems concerning certain academic disciplines vis-à-vis each other, but also with the question of how much space for unity the subject matters of the social sciences actually grant these sciences. Starting from a situation of diversity in the social sciences, it would perhaps be possible to unite these sciences into a synopsis or a total social science by formulating a body of methodological principles and fundamental assumptions from which the presently existing content of the concerned sciences could be derived. Two formal questions are relevant with respect to such a process: First, would this synopsis really be sufficient for the satisfactory and exhaustive investigation of the study objects, amongst which e.g. *culture*, *society* and *mind*? Said otherwise, would the unified total social science be *complete* in the sense of covering all that there is to be investigated? If not, this would leave open the possibility that apart from the incomplete generalisation we proposed, another social scientific approach would have to be formulated, which would create the problem of unification all over again. The second question is: Would it be possible to safeguard the proposed total social science's *consistency*? An inconsistent body of assumptions and principles is usually not considered a convincing scientific way of describing the world.

The question whether the currently existing social sciences are compatible at all is often formulated with an eye to the results of the natural sciences, which somehow appear to have succeeded better in achieving mutual compatibility, even when there exists a considerable degree of labour division. The state of affairs in the social sciences leads to

a sense of awkwardness. Howarth (2004, 229) describes this sense thus: 'The scandal of the human and social sciences is their interminable dispute.' But as yet there does not exist a convincing motivation why one should agree with this assertion of scandal. It could well be that the study objects of the social sciences themselves allow for less mutually compatible theories than those we usually encounter in the natural sciences.

It would not be convenient to invoke solely the social sciences themselves for providing insight in this topic, since this would simply result in begging the question. Therefore I propose to draw on some ideas taken from analytical philosophy. With special attention to the topics of *completeness* and *consistency*, I intend to show that analytical philosophy can be used to shed light on the problem of unity and diversity in the social sciences. Analytical philosophy studies formal aspects of such topics as 'deductive theories', 'sets', 'signs', 'meaning', 'concepts', and the like. Since it is not itself a social science, analytical philosophy perfectly suggests itself for studying the formal aspects of theorising in the social sciences.

Since I am mostly familiar with cultural anthropology, the focus in this article will be on applications of analytical philosophy on that science. Nevertheless I believe the investigations to have wider relevance. In many respects I follow ideas of Gregory Bateson (1904-1980), an anthropologist with a keen interest in related topics, such as human communication, learning, and the interaction of humanity with its environment. I will discuss a theory Bateson referred to in several places, namely Bertrand Russell's theory of logical types, but I will also extend the discussion by dealing with the paradox named after Haskell Curry as well as Kurt Gödel's first incompleteness theorem. These topics all deal, in different ways, with problems of completeness and consistency. Crucial for Bateson's approach as I will discuss it, is that they are connected to the investigation of *hierarchies of levels*. Amongst others, I will shortly discuss ideas of Renato Rosaldo and Christoph Brumann from this viewpoint. Even though their approaches seem quite different at first sight, they appear to have a lot in common with respect to type-theoretical questions.

My use of analytical philosophy shall be twofold: In the first place it is intended to deliver an example of interdisciplinary research in so far as the application of analytical philosophy to social sciences is concerned. In the second place I shall try, with some understandings of that application, to say something about the unity or diversity of the social sciences. Despite its high potential for interesting investigations, the use of analytical philosophy has been far from widespread among the social sciences. Dumont (1983, 228) has stated that Bateson was one of the few anthropologists who clearly saw the necessity of recognising a hierarchy of levels. Since Dumont's statement, however, not much has been done to explore further the lines of thought Bateson had been setting up. Though discussion of levels is not always absent (cf. Howarth 2004, 241), the related problems of completeness and consistency in the social sciences are far from exhaustively explored yet.

In the sequel, I will first give an exposition of the basic ideas in which Bateson was involved; second, I elaborate a bit on these ideas; and third, I discuss what such studies could tell us about the unity or diversity of the social sciences.

## 2. Bateson's problem:
## The distinction of hierarchies in the study of social phenomena

Frequently in his writings, notably *Steps to an Ecology of Mind* (1972) and *Mind and Nature* (1979) and the 'Epilogue 1958' of *Naven* (1958 [1936]), Bateson refers to Russell's theory of (logical) types (Russell & Whitehead 1910, 39-68). Russell developed this theory as a mathematical device to avoid the emergence of a paradox, which he himself had discovered in what is now known as *naive set theory*. Russell's paradox results from the possibility in naive set theory to define some class *A* as *the class of all and only those classes that do not contain themselves as an element*. If one tries to answer the question whether the class *A* does or does not contain itself as an element, one gets into trouble, because if it would, it would not, and if it would not, it would,

according to the definition of *A*. The theory of types avoids the emergence of this paradox through the prohibition of mentioning in an object's definiens an object that is of the same 'type' as the definiendum. Thus the definition of *A* above is forbidden, because it treats 'class of classes' on equal footing with 'class'. As a definiendum, *A* appears as a class of classes, but the paradox arises because it is judged in the definiens as a mere class along with other classes. Basically, the theory of types introduces a *hierarchy* between types, blocking Russell's paradox because now classes are of a different, lower, type than classes of classes, and the definition of *A* above is declared invalid.

Now Bateson was convinced of the relevance of the theory of types to the work of scientists. For a good understanding of what he meant, it is worth to quote the following *in extenso*:

> 'It is the observer who creates messages (i.e., science) about the system which he is studying, and it is these messages that are of necessity in some language or other and must therefore have *order*: they must be of some or other Logical Type or of some combination of Types. The scientist's task is only to be a good scientist, that is, to create his description of the system out of messages of such logical typology (or so interrelated in their typology) as may be appropriate to the particular system. Whether Russell's Types "exist" in the systems which the scientist studies is a philosophical question beyond the scientist's scope – perhaps even an unreal question. For the scientist, it suffices to note that logical typing is an inevitable ingredient in the relationship between any describer and any system to be described' (Bateson 1958 [1936], 294; emphasis original).

The search for a suitable - if any - application of a theory of types in social scientific theorising will be labelled here as 'Bateson's problem'. In the quotation Bateson clearly sees logical types as relevant only for the *theories* to be formulated, not for the *subject matter*. However, the suggestion that it is beyond the scientist's scope to investigate the existence of types in the systems to be studied is debatable, if it is considered that the

absence or presence of logical types in the studied systems would in itself be an important feature (i.e. of those systems) to be studied.

There are at least two reasons to think that if we occupy ourselves with Bateson's problem in proposed theories, we must also do so with respect to the subject material that these theories are about. First, there is the possibility that theories in some way adequately describe studied phenomena (this is, after all, what is intended). Theories are meant to represent the matter they are about: In the terminology of Bateson (1958 [1936], 294-95), they are aggregates of messages of description, which are *mapped* on diagrams of logical types. Consequently, one might expect that if studied phenomena are successfully mapped on some type-theoretical framework, one could look for hierarchies in the subject matter corresponding to the hierarchies in that framework. Indeed, Bateson himself gives the following judgement: 'The typological relations between the messages of a description could also be used, subject to rules of coding, to represent relations *within* the system to be described' (1958 [1936], 295; emphasis added). So even in the case logical types could not be found in the studied material by direct observation, they might be present in some theory about that material, and one could conclude from the theory (assuming it is adequate) that logical types must be present in the studied material. On the other hand, if the concerned theory is unable to accommodate logical types, one can ask whether this inability is actually due to the set-up of the studied system.

A second reason for dealing with the problem of logical types within the subject material is that the distinction between the studied systems and the theories about them may not be as absolute as Bateson suggests in the quotation above. Again an example of the contrary is given by Bateson himself, namely when he deals with the application of ecological theory: '[T]he problem of how to transmit our ecological reasoning to those whom we wish to influence in what seems to us to be an ecologically "good" direction is itself an ecological problem. We are not outside the ecology for which we plan – we are always and inevitably a part of it' (Bateson 1972, 512).

The observations made thus far have relevance with respect to the social sciences when it is considered that an inadequate understanding of hierarchies (or blurring thereof) in theories and/or studied systems might hamper valid descriptions of social reality: 'If social scientists would keep the levels straight, they would not use phrases like "society forces the individual" or "history teaches" ' (G. Bateson, in: M.C. Bateson 1994 [1984], 207). Even if theories in social sciences are formulated in more differentiated ways than Bateson indicated here, this remark reminds us that we should thoroughly investigate hierarchy questions concerning their consequences for describing and explaining the phenomena studied in the social sciences. I will take up this issue in the sequel, recapturing some of Bateson's thoughts and try to indicate how some further steps could be taken in the research he set out.

### 3. Bateson's problem with respect to social collective notions: Some examples

Bateson (1979, 229) states with respect to logical types: 'The name is not the thing named but is of different logical type, higher than that of the thing named.' If one accepts this statement, then for a consistent use of the theory of types one must remember that the thing named not only *is not* but also *does not contain* the name (since this would be contradictory to the acceptance of the name's being of higher logical type than the thing named). It is at this point, however, that difficulties emerge. Let us consider some study objects in the social sciences in which the distinction of hierarchies is particularly tough, namely such objects referred to as 'culture', 'society', 'cognition' or 'mind'.

The concept of 'culture' (or such notions like 'social setting' and 'context') has a long history within and outside anthropology. It was and is used both to *denote* collectively certain people's behaviours, thoughts and the products thereof, as well as to *explain* these phenomena. Following Bateson, one could expect problems with the explanatory power of theories using 'culture' if one could not 'keep the levels straight' (i.e. could not avoid the inappropriate blurring of different logical types) in the application of the term. But

first it must be investigated whether it is possible at all to straighten out the levels. Indeed this is rarely systematically done in anthropological research. To show that such investigations do have relevance, I now wish to discuss some examples derived from quite different currents in anthropology.[2]

For instance, the cultural-relativistic doctrine that 'a culture can best be understood in its own terms' is often invoked without any indication that Bateson's problem might crop up here. In the doctrine it is assumed that the best descriptions and explanations of a culture are provided by the terms, ideas and assertions of the concerned culture itself (Herskovits 1972, 38), and not by the terms, ideas and assertions derived from other cultures. The methodology of cultural relativism is considered a basis for an *objective* understanding of cultures (Herskovits 1972, 38, 40-41). An opposing view would have it, that it is exactly a view informed by a foreign idiom, which makes it possible to give an accurate account of a culture. For participants using only their indigenous cultural idiom, it would be difficult, if not impossible, to give an explicit analytical account of their own behaviour patterns and beliefs (Benedict 1946, 13-14; Van Baal 1974, 1-13). There is a dilemma here: In the latter view, the 'levels are kept straight', because the messages of the theory about the studied culture do not contain elements of that culture. On the other hand, a situation is created in which researchers using some cultural idiom would be able to study any culture on earth except the one consisting of that same idiom; something unwanted in the light of comparative studies. On the other hand the cultural-relativistic methodology violates the type-theoretical principle (as formulated above) that the *name* cannot be included in the *thing named*. By this violation it would introduce circularity in the methodology concerning both descriptive and explanatory aspects of cultural analysis; to explain a culture terms are used that originate from that same culture.

Bateson's problem is not only relevant for cultural relativism, but also for more recent proposals for the study of culture. Renato Rosaldo, contributor to the famous *Writing*

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[2] The examples below are really meant as illustrations of the point that Bateson's problem emerges in widely diverging anthropological research approaches, not as a complete historical overview.

*Culture* volume (Clifford & Marcus 1986), writes for example that 'indigenous usage [of language] is always correct in its own setting' (Rosaldo 1986, 83). If the possibility that indigenous practitioners of indigenous language can make this judgement themselves is not ruled out (which Rosaldo indeed does not seem to do), people can justify any usage of language with the invocation of their 'own setting'. Just as with cultural relativism, a violation of the above-mentioned type-theoretical principle occurs. This is methodologically problematic, for it leaves aside the question of what counts as 'indigenous' or 'own' setting in the first place, something which cannot be answered by invoking the 'indigenous' and the 'own setting' again, since this would constitute a complete blurring of question and answer. (The judgement *about* what counts as 'indigenous' would be at the same level - be of the same type - of what is *invoked* as indigenous.) It also has quite some moral implications, for it prohibits those denoted as 'non-indigenous' to judge, evaluate, debate or criticise statements made by the studied people denoted as 'indigenous' who speak from the certainty of their all-validating 'own setting'.

Post-*Writing Culture* definitions of 'culture', sometimes intended as a rehabilitation of the concept after the distrust caused by the 'post-modern' movement in anthropology (cf. Brumann 1999), also mostly leave Bateson's problem implicit. Pascal Boyer (1999, 206), a representative of the school of evolutionary psychology, gives: ' "ideational culture" [is] the set of mental representations entertained by members of a particular group that makes that group different from others', a definition that does not prohibit the inclusion of mental representations about 'ideational culture' in the set of mental representations. Said otherwise, Boyer's definition allows for the inclusion of the definiendum in the definiens, again an instance of not 'keeping the levels straight'. Another proposal for definition, by Christoph Brumann (1999), takes on the form of a matrix. The proposal entails that rows 'stand for individuals' and columns 'for identifiable ways of thinking, feeling, and acting' – features (1999, S6), where it can be indicated whether or not a certain individual has, believes or practices a certain feature. '[T]he term ["a culture"] refers to an abstract aggregate, namely, the prolonged copresence of a set of certain individual items' (1999, S6).

Brumann suggests that individuals can be ascribed membership with respect to a culture according to their sharing of features in the matrix with others (1999, S6-S7). But he also allows features themselves to represent statements about individuals belonging to social collectivities, because 'any observable feature can be included in [...] a matrix, including emic[3] categories […] and self-categorization' (1999, S6n8). This means that while viewers of the matrix may ascribe membership of individuals concluding *from* information in the matrix, such judgements can also be included already *in* the matrix through a feature. Here the levels are not kept straight; in fact, the judgement of the viewers who draw conclusions from the structural aspects of the matrix may be inconsistent with the information represented by some single feature. It seems very strange, however, either to forbid that the matrix could be judged from the outside, or to block the possibility for features to refer within the matrix to self-categorisations also known in outside judgements. Clearly we have here an instance of Bateson's problem (are cultural categorisations of a higher logical type than the features in the matrix or not?), but the issue is not seriously addressed in Brumann's article.[4]

Francisco Gil-White (2001), like Boyer representative for evolutionary psychology, gives a clear recent variant of the view that subscribes to a distinction between observer and observed. He argues that people associate in groups in accordance with modularly conceived brain functions: '[Such] "modules" [can be] described in a cognitive sense […] as a set of processing biases and assumptions activated by the domain-relevant inputs [e.g.] social groupings' (2001, 517). Crucial to Gil-White's approach is the distinction between 'ordinary folk', who are 'naive essentialists' (p. 516), and anthropological researchers, who are not (pp. 515-16). The theory of cognitive modularity is then supposed to explain why 'ordinary folk' are 'essentialists' who ascribe the cultural transmission of norms and behaviours to biological descent (pp. 518-19). However, it remains unclear how this theory

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[3] 'Emic' is an anthropological term that indicates that conceptualisations of the studied people are concerned, instead of conceptualisations from the outside, such as those of professional anthropologists (called 'etic').
[4] Brumann (1999, S24) mentions the possibility of 'a characteristic combination of nonunique traits [being] itself a unique meta-trait' but only as being proposed by a 'die-hard sophist' (1999, S24).

should be related to the minds of the researchers, because even if they would be essentialists, they could no longer be considered *naive* by the nature of the research as being *about* essentialism. In this sense the theory creates its own exceptions.

I conclude that Bateson's problem is a recurrent phenomenon in anthropology (even if implicit) for which solutions have been offered during the decades either by accepting the equality between observer and observed, with the danger of confusion, or by distinguishing between them, at the price of the observers' (and perhaps others') exclusion from the scope of the proposed descriptions or theories. The diversity of the approaches contrasts with the commonality of the problem they have to deal with.

## 4. Stepping across levels: Curry's paradox and Gödel's incompleteness theorems

I have tried to make clear in the previous paragraph that Bateson's problem is a relevant topic for proposals from different theoretical outlooks. Blurring of levels, often made visible through self-referential constructions, seems to be hardly avoidable, yet it leads to considerable even if unnoticed difficulties, which one may expect hampers the straightforward development of theory. Is there a way out? To investigate this question I shall look at two topics as a further elaboration of Bateson's research. The first concerns Curry's paradox, the second Gödel's first incompleteness theorem.

Apart from the blurring of levels, Bateson seemed to believe that also the logical operation of *negation* was responsible for the possible emergence of paradox, for he wrote that 'no paradox can be generated [in iconic[5] communication] because in purely [...] iconic communication there is no signal for "not" ' (Bateson 1972, 291). However, Haskell Curry discovered a paradox, an elaboration of Russell's that does not use negation. Several versions of the paradox exist (e.g. Curry, Feys & Craig 1958, 258-59;

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[5] With 'iconic' communication, Bateson means communication in which there exists some direct link between objects and their images, in contrast with what he calls digital communication, where links are arbitrary.

Visser 1989, 643), but they have in common that an object is treated as an equivalent of an implication in which the same object acts as an antecedent. Any arbitrary statement can then be derived using the implication, a paradoxical result since the arbitrariness means that for any derived statement also its opposite can be derived.[6] Curry's paradox only aggravates the difficulties encountered already with Russell's, since the self-inclusion or self-reference of certain objects cannot always be avoided in social life. For example, if the objects and people referred to by the term 'a culture' can be studied at all, it cannot be excluded that people who might be included in the reference of that term study those objects and people themselves (see the discussion of Brumann 1999 above). But then the idea of *explaining* or *deriving* facts by using 'culture' (or similar concepts) as a term in social-scientific theories is doomed to fail because by Curry's paradox such explanations are totally arbitrary. The proposal that 'culture does not exist' does not seem to be helpful because this would mean that the term 'culture' really refers to nothing at all in whatever interpretation, a rather trivial way out of an interesting question of semantics. Viewed in the light of Curry's paradox, the consequence of the existence of such collective items like culture would be that these items are actively preventing the formulation of any straightforward and consistent explanatory theory about themselves. A possibility to be investigated is that one considers certain *parts* of the collective items in question, and proposes (hopefully consistent) theories about those parts. The problem arising then is immediately relevant for our research on the possibility of unifying the social sciences; one would wish to avoid the result that the facts we encounter in social life would all be explainable separately with separate theories while at the same time collectively with an overall theory they would not.

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[6] For a set-theoretical version, see Krajewski (1981, 23):

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| $X = \{a \mid (a \in a) \rightarrow p\}$ | definition | (*i*) |
| If $X \in X$, then $(X \in X) \rightarrow p$, hence $p$ | from (*i*) | (*ii*) |
| $(X \in X) \rightarrow p$ | from (*ii*) | (*iii*) |
| $(X \in X)$ | from (*i*) and (*iii*) | (*iv*) |
| $p$ | from (*iii*) and (*iv*). | (*v*) |

Here no a priori conditions for the contents of $p$ exist, so the derivation implies that any arbitrary statement can be proved starting from the definition of $X$ alone.

We have seen that Bateson stressed the importance of the theory of types in order to avoid paradoxes in mathematics and theories in the social sciences. There does exist a formal procedure in mathematics, however, which violates the principle that 'the name should not be included in the thing named', but nevertheless does not produce paradoxes or inconsistencies. I am hinting at Kurt Gödel's method of using numbers to *code for* expressions of Peano arithmetic and then use them in that same Peano arithmetic.[7] Peano arithmetic ('arithmetic' for short) is an axiomatic system that defines the natural numbers (0, 1, 2 etc.). It had been a problem for mathematicians to find out whether this system could be shown to be *complete*, i.e. to demonstrate that from the axioms making up arithmetic, all true statements of arithmetic could be derived (and all false statements could be disproved). Gödel showed in 1930 that this is not possible. He demonstrated that there exists at least one statement of arithmetic, which is true but cannot be derived from the axioms (Gödel 1988 [1931]).[8]

A simple example will suffice here to see what Gödel's method entailed. Suppose we write down some sentence in the language of arithmetic, like:

$$2 + 2 = 5.$$

This sentence, which is false, must be considered *not provable* from the axioms of arithmetic. But the statement ' "2 + 2 = 5" is not provable' constitutes a statement *about* the sentence '2 + 2 = 5', not in the language of arithmetic itself, but in a *meta*-mathematical language. Gödel showed, however, that the notion 'provable' could itself be written as an arithmetical operation. Gödel demonstrated further that arithmetical expressions could be coded for using natural numbers (in fact, the coding numbers are now called 'Gödel numbers').[9] Expressions like *provable* and *coding number of* are themselves expressible in

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[7] More precisely, Gödel dealt with Peano arithmetic as incorporated in the system of Russell and Whitehead's *Principia Mathematica*.

[8] See e.g. Nagel and Newman (1959); Smullyan (1992); for a less technical exposition see Schultz (1980, 135-42).

[9] Nagel and Newman (1959, 76) give the example of the sentence '0 = 0', which could be rewritten by writing for '0' *6* and for '=' *5*. The Gödel number coding for '0 = 0' is then obtained by using prime numbers, yielding $2^6 \times 3^5 \times 5^6 = 243,000,000$.

the coding device Gödel established. Gödel then showed there exists a sentence in arithmetic with coding number *h*, its corresponding meta-mathematical statement running like:

The sentence with the associated coding number *h* is not provable.

The sentence cannot be proved from the axioms of arithmetic, which is what it indeed asserts itself, but meta-mathematically we know that the sentence is true. In this way arithmetic is shown to be incomplete. In fact, if the sentence would be provable, arithmetic would be inconsistent and useless as a mathematical theory.

Gödel's result provides an interesting possible elaboration of Bateson's insightful investigations. In the 'Epilogue 1958' of *Naven*, Bateson introduces the following state of affairs: 'Take [...] a system *S* of which we have a description with given complexity *C*' (1958 [1936], 299) and then writes almost at the end of the book: 'Here is the central difficulty which results from the phenomenon of logical typing. It is not, in the nature of the case, possible to predict from a description having complexity *C* what the system would look like if it had complexity *C* + 1' (1958 [1936], 302). Indeed, prediction is likely to be impossible, but with Gödel one does have a glimpse of complexity *C* + 1 when looking from *C*. Gödel avoided paradox through his ingenious numbering system in which the distinction between the language of arithmetic and the meta-language is kept intact. But by actually showing that such a numbering system is indeed possible, Gödel's method provides an alternative for Bateson's (1979, 229) use of the theory of types as a prohibition to present a name as of equal (or higher) logical type as the object named. Even though Gödel numbers should not be regarded as 'denoting' the arithmetical sentences (Shanker 1988, 216), it is still the case that they both *code for* sentences and can be *incorporated in* sentences. In fact, in Gödel's unprovable sentence one is incorporated in an object of the code for that very object, without any paradoxical consequence of the 'Russell' or 'Curry' sort. There is also the recognition that the expression 'This sentence is not provable' can itself be read as a meta-mathematical judgement about its corresponding arithmetical sentence, which we know, through the

coding system, to be stating its own unprovability. The truth of that judgement can be established with meta-mathematical considerations, but not with the devices provided by arithmetic itself. However, when the meta-mathematical language would be formalised, this would in turn contain unprovable sentences (i.e. only provable by yet another more powerful language, and so on).

This recognition has given rise to debates as to whether or not the human brain, or its functioning, can be modelled with fully formalisable theories (see e.g. Penrose 1990 [1989], 407-09). It is impossible to give here a discussion of these debates (which still seem to be undecided), but at least it shows that Gödel's result is relevant for social sciences dealing with cognition and learning. For example, evolutionary psychologists tend to adhere to a view of the human mind as a *computational* device (Samuels 2000, 15), clearly a statement for which Gödel's discovery has relevance. But Gödel is also of interest for the present article in its relevance for what it might have to say about the possibility of unifying the social sciences.

## 5. Theories and study objects

Judging from the above investigations concerning Bateson's problem, I think it not likely that a total and absolute distinction between theories and study objects in the social sciences is feasible, in so far as such theories are supposed to be unambiguously *about* the study objects. Rather, study objects with powerful enough communication devices are likely to be able to refer to the theories proposed about them. Such theories are consequently somehow incorporated in their own study objects. If this does not lead to the logical chaos of Curry's paradox, then at least it will have to be concluded that any proposed *consistent* theory falls short of providing explanations for all the observed phenomena concerned, if the theory belongs to the observational scope of itself. In fact, intended study objects (e.g. people's cognitive capabilities; human culture) might be considered in some ways to be about the theories just as the theories can be said to be

about these study objects, but this recognition leads to the realisation that no formalisable type-theoretical distinction is possible between theory and study object in the way Bateson meant. This casts doubt on the ability of social scientific theories to provide for an adequate and exhaustive explanation of their study objects.

This is not just a matter of the peculiarities of self-reference. The possibility for self-reference as encountered in Curry's paradox, or in incompleteness of the Gödel sort, are themselves possible by virtue of general features of the objects at hand. For example, theories may allow for a mixture of types where other theories do not, or contain a variety of axioms that are absent in simpler theories. Analogously, whether or not social sciences can be unified (in the sense of becoming a single consistent summary encompassing the existing social sciences), might very well be depend on certain characteristics inherent in objects like *culture* and *society* (e.g. anthropology; political science) or *mind* (e.g. psychology, pedagogy).

Perhaps this is not only confined to the social sciences. The issue of distinguishing between object and theory is taken up in physics by John Barrow (1998, 221-230). Since physical theories are mostly written in mathematical formulas, the incompleteness of mathematical theories could possibly imply that nature could not exhaustively be explained using these formulas. For the social sciences the matter seems even more pressing: If the statements *about* the study objects can be *represented* in the study objects, this could well imply that these study objects contain phenomena that are *part of* the study objects but are *not explainable* using information from these very study objects, while *adding extra information* would not ultimately solve the requirement of full explainability. For example, one could not claim that 'culture explains human behaviour' or 'psychological dispositions explain human behaviour' if we would really mean *all* behaviour, but also not when one adds these claims to an assertion 'culture and psychological dispositions explain all of human behaviour'. So Bateson was, I think, right to say 'If social scientists would keep the levels straight, they would not use phrases like "society forces the individual".' My point here is that his comment could be

extended to an analysis of what can be said about levels, or blurring thereof, in society itself.

A practical example may illuminate the above. Van Dijk (1991, 44) defines *discourse analysis* as 'a multidisciplinary approach to the study of language use and communication [which] emerged in the late 1960s and early 1970s from different but related developments in anthropology, ethnography, linguistics, poetics, psychology, micro-sociology, mass communication, history, political science, and other disciplines in the humanities and social sciences'. In discourse analysis, many already existing different disciplines are added up to form a new scientific entity, which is then itself restricted to the study of 'textual or conversational structures' of which the 'explanatory frameworks [...] derive from the analysis of the relationships between "text and context"' (Van Dijk 1991, 45). Van Dijk himself tries to understand the role discourse plays concerning (the reproduction of) racism (cf. 1991, ix).

That a fully integrated explanatory framework is not easy, however, may be clear from the comments of Condor and Antaki (1997, 320). They distinguish between two approaches in the study of discourse, one in which researchers use psychological mechanisms of individuals as explanans for the perception of themselves and others, and one in which researchers try to understand individuals' cognition, using social interaction as an explanans. Combining these two approaches, with opposite chains of explanation, may perhaps enlarge the area of investigation, but the distinction between explanatory levels has become quite uncertain. The mutual embeddedness of the explanation chains may result in a circular theoretical apparatus, thereby trivially capable of explaining any result.

Such uncertainty with respect to explanatory levels is only aggravated when it is considered that researchers may actually publicly discuss their results in newspapers or other media that they otherwise use as study objects of their research. A solution to the resulting circularity may be that one acknowledges that discourse analysis cannot deal

with all discourse as far as explanation thereof is concerned. Let us consider Van Dijk (2003), who uses aspects of his research to back up his supposition that a certain well-known Dutch author is behind a racist pamphlet pubished in 1990, the writer of which being only known by a pseudonym (cf. Shadid & Van Koningsveld 1995, 13). Van Oostendorp (2003a; 2003b) discusses some arguments of van Dijk that deal with style and word choice of authors and tends to the conclusion that Van Dijk's supposition is unlikely to be true, and this would throw into question the current state of affairs in discourse analysis as applied by Van Dijk. On a factual level, Van Oostendorp's publications could conceivably play a role in the eventual establishment of the truth or falsehood of Van Dijk's supposition. The point here, however, is to determine whether Van Oostendorp's writings can be seen as a genuine study object of discourse analysis. Can they function as confirmations or denials of Van Dijk's theories? If it is a task of discourse analysis to develop theoretical devices for analysing the interplay between texts and discrimination in society, a negative outcome (for Van Dijk's supposition) of Van Oostendorp's discussions that are *critical* about the present state of affairs in discourse analysis dealing with the reproduction of racism, should not be taken as *confirming* Van Dijk's discourse analytical theories as being an example of such reproduction (for example because those critical discussions would hinder research about racism - cf. Van Dijk 2003, 179).[10] I maintain that a critical discussion *about* methodological questions should not be rejected while using arguments of the very methodology that is reviewed, since such rejection would allow the methodology to be self-proving, analogously to the situation we have with Curry's paradox. A (hypothetical) positive outcome (for Van Dijk's supposition) within Van Oostendorp's writings should not be taken as an explanandum of Van Dijk's theory of discourse as reproducing racism either – for, *as* discourse, Van Oostendorp's writings would itself form a counterexample to the theory, since it would be discourse that does not reproduce racism.

In short, apart from mentioning the interplay between text and discrimination, Van Oostendorp's pieces also include references to discourse analytical arguments about that

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[10] No other than methodological questions are at stake in Van Oostendorp's 2003a and 2003b pieces.

interplay. Therefore the pieces cannot be considered to be type-theoretically congruent with the usual study objects of discourse analysis in which the approach itself does not play a part. Being about aspects of discourse analysis, Van Oostendorp's writings constitute discourse outside the *explanatory* range of Van Dijk's discourse analytical ideas. Results on the factual level (e.g. Van Dijk's supposition about the identity of an anonymous writer) are not necessarily consistent with results on a higher level (Van Dijk's application of theories *about* facts). We can be quite sure that comparable situations exist for discourse analysis as a whole. This is not a weakness of discourse analysis; on the contrary, when Van Oostendorp's pieces, or similar ones, would actually be explained by discourse analysis this would either be self-proving or self-contradictory. As with Gödel's unprovable sentence, leaving them out of the explananda of discourse analysis, however multidisciplinary, brings the blind spots necessary for the consistency of the approach.

## 6. Conclusions

I have been discussing aspects of Gregory Bateson's ideas about the application of Russell's theory of types to the social sciences. I intended to show that the problems for which this theory was supposed to be a solution are relevant for the social sciences, even though the solution itself does seem to hold for at least some objects studied within the social sciences. I have applied certain arguments about these matters to the problem of distinguishing study objects and theories and the relationship of this problem with the possibility for unifying the social sciences.

It is quite possible that study objects of the social sciences, like *society* and *mind,* are such that they prevent their own being fully and consistently explained (if we take explanation to mean that all events occurring in these objects should be derivable from some deductively constituted theory). To establish this would itself be a positive discovery about these items, and one might expect fruitful contributions from analytical

philosophy when investigating their logical properties. Applying analytical philosophy in social science is certainly not commonplace. For instance, as far as I know earlier applications of Curry's paradox to definitions of 'culture' are absent in anthropology, despite its rather obvious relevance. Many problems are also related to such topics as *reference* and *semantics,* as well as set-theoretical ideas.

As for the unification of the social sciences, one might try to formulate a consistent overall theory derived from them (assuming that these sciences are internally consistent). However, it is possible that this overall theory still leaves research questions uncovered, when its explanatory devices together have formal properties that prevent analytical completeness. On the other hand, it may appear that the combination of, say, anthropology and psychology is providing methods that cover all possible research questions, but turns out to be inconsistent. The requirement of consistency would then lead to a renewed division of research attention into separate parts, resulting in a situation of relativism in which research is separated according to methodological points of view. These viewpoints may then be constituted as traditional academic disciplines or crosscutting research schools.

If the latter situation would arise, it could in the first instance be taken as a 'failure' to unify the social sciences. But there can only be a justified desire for unifying if the research objects allow it. If research objects themselves imply that they can only be approached relativistically, it is not a failure to actually do so. In fact, the peculiar constitution of human *culture* and *mind* may very well be at the base of the possibility to do social scientific research at all. There could have been no theory about them if these objects would themselves 'keep the levels straight' all too drastically. There is also nothing unscientific about a non-unified social science. At least some hypotheses will still be formulatable and testable; it is only unlikely that a general body of axioms will be discovered from which such hypotheses could be mechanically derived for all social scientific research.

## References

Barrow, J.D. (1998) *Impossibility: The limits of science and the science of limits*. Oxford: Oxford University Press.

Bateson, G. (1958 [1936]) *Naven: A survey of the problems suggested by a composite Picture of the culture of a New Guinea tribe drawn from three points of view*. Stanford: Stanford University Press.

Bateson, G. (1972) *Steps to an ecology of mind: Collected essays in anthropology, psychiatry, evolution, and epistemology*. San Francisco: Chandler.

Bateson, G. (1979) *Mind and nature: A necessary unity*. New York: E.P. Dutton.

Bateson, M.C. (1994 [1984]) *With a daughter's eye: A memoir of Margaret Mead and Gregory Bateson*. New York: Harper Perennial.

Benedict, R. (1946) *The chrysanthemum and the sword: Patterns of Japanese culture*. Cambridge: Riverside Press.

Boyer, P. (1999) 'Human Cognition and Cultural Evolution.' In: H.L. Moore (ed.) *Anthropological theory today*. Cambridge: Polity Press.

Brumann, C. (1999) 'Writing for culture: Why a successful concept should not be discarded.' *Current Anthropology*, 40 (1): pp. S1-S27.

Clifford, J. & G.E. Marcus (eds.) (1986) *Writing culture: The poetics and politics of ethnography*. Berkeley: University of California Press.

Condor, S. & C. Antaki (1997) 'Social cognition and discourse.' In: T.A. van Dijk (ed.) *Discourse studies: A multidisciplinary introduction, Volume 1, Discourse as structure and process*. London: Sage.

Curry, H.B., R. Feys & W. Craig (1958) *Combinatory logic: Vol. 1*. Amsterdam: North-Holland.

Dumont, L. (1958) *Essais sur l'individualisme: Une perspective anthropologique sur l'ideologie moderne*. Paris: Seuil.

Gil-White, F.J. (2001) 'Are Ethnic Groups Biological "Species" to the Human Brain? Essentialism in our cognition of some social categories.' *Current anthropology*, 42: pp. 515-554.

Gödel, K. (1988 [1931]) 'On formally undecidable propositions of "Principia Mathematica" and related systems I.' In: S.G. Shanker (ed.) *Gödel's theorem in focus*. London: Croom Helm.

Herskovits, M.J. (1972) *Cultural relativism: Perspectives in cultural pluralism.* New York: Random House.

Howarth, D. (2004) 'Towards a Heideggerian social science: Heidegger, Kisiel and Weiner on the limits of anthropological discourse.' *Anthropological theory*, 4: pp. 229-247.

Krajewski, S. (1981) 'Antinomies.' In: W. Marciszewski (ed.) *Dictionary of logic as applied in the study of language*. The Hague: Martinus Nijhoff.

Nagel, E. & J.R. Newman, (1959) *Gödel's proof*. London: Routledge & Kegan Paul.

Penrose, R. (1990 [1989]) *De nieuwe geest van de keizer: Over computers, de menselijke geest en de natuurwetten*. Amsterdam: Prometheus. Translated by J. den Bekker.

Rosaldo, R. (1986) 'From the door of his tent: The fieldworker and the inquisitor.' In: J. Clifford & G.E. Marcus (eds.) *Writing Culture: The Poetics and Politics of Ethnography*. Berkeley: University of California Press.

Russell, B. & A.N. Whitehead (1910) *Principia Mathematica: Vol. 1*. Cambridge: Cambridge University Press.

Samuels, R. (2000) 'Massively modular minds: Evolutionary psychology and cognitive architecture.' In: P. Carruthers & A. Chamberlain (eds.) *Evolution and the human mind: Modularity, language and meta-cognition*. Cambridge: Cambridge University Press.

Schultz, R.A. (1980) 'What could self-reflexiveness be? Or Goedel's theorem goes to Hollywood and discovers that's all done with mirrors.' *Semiotica*, 30: pp. 135-152.

Shadid, W. & P.S. van Koningsveld (1995) *De mythe van het islamitische gevaar: Hindernissen bij integratie*. Kampen: Kok. Second print.

Shanker, S.G. (1988) 'Wittgenstein's remarks on the significance of Gödel's theorem.' In: S.G. Shanker (ed.) *Gödel's theorem in focus*. London: Croom Helm.

Smullyan, R.M. (1992) *Gödel's incompleteness theorems*. New York: Oxford University Press.

Van Baal, J. (1974) *De agressie der gelijken*. Assen: Van Gorcum.

Van Dijk, T.A. (1991) *Racism and the press*. London: Routledge.

Van Dijk, T.A. (2003) *De Rasoel-Komrij Affaire*. Amsterdam: Critics.

Van Oostendorp, M. (2003a) *Je bent hoe je schrijft: Verraadt de stijl de auteur?* http://www.onzetaal.nl/nieuws/auteursherkenning.html. Accessed 28 December 2005.

Van Oostendorp, M. (2003b) *Je bent hoe je schrijft: Verraadt de stijl de auteur?* http://www.vanoostendorp.nl/linguist/auteursherkenning.html. Accessed 28 December 2005.

Visser, A. (1989) 'Semantics and the Liar Paradox.' In: D. Gabbay & F. Guenthner (eds.) *Handbook of philosophical logic: Vol. IV*. Dordrecht: Reidel.