TERMINOLOGY

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PLEASE NOTE:
As from the year 2000, only one issue of this journal
will appear per year. This means that the correct title of
the former volume, which was named Vol. 11 (2000),
no. 1, is Vol. 11 (2000), no. 1-2.

TERMINOLOGY SCIENCE
AT THE CROSSROADS ?

Are the different views on the theoretical foundations of terminology drifting apart ?

Since the publication of the book <u>Ausgewählte Texte zur Terminologie</u> *(1993) (Selected Texts on Terminology), in which Ch. Laurén and H. Picht took up the challenge of comparing the different schools of terminology (pp.493-536), some considerable time has lapsed. Things have continued to develop, new approaches have appeared, differences or perceived differences have been resolved. It therefore seems timely to take stock now, in order to establish in which respects fundamental theoretical positions have changed, whether they are contradictory in any way or only differ in certain aspects.*

The aim of the terminology section at the 13th European Symposium on Languages for Special Purposes held in Vasa, Finland in August 2001 was to clarify as far as possible the theoretical bases of terminology theory. It seemed that the need for clarification was an urgent one for several reasons. First, it can only be beneficial to theoretical developments in terminology science; secondly, it is to be hoped that such a discussion will help to prevent basic theoretical positions drifting fur-

ther apart; thirdly, it can provide a sounder theoretical base to our subject; and fourth, it is an essential foundation for the principles of standardisation in terminology.

In the section, the following subjects were dealt with by the speakers listed below, and in the subsequent colloquium each contribution was commented on by one or more opponents:

1. *Gerhard Budin: A critical evaluation of the state-of-the-art of terminology theory*
    Opponents: Merja Koskela, Bertha Toft

2. *Christer Laurén; Heribert Picht: Terminologie aus linguistischer Sicht*
    Opponent: Heinz Leonhard Kretzenbacher
    Johan Myking: Socioterminology, terminology planning and standardisation
    Opponents: Bassey Antia, Outi Järvi

3. *Nina Pilke: The concept and the object in terminology science*
    Opponents: Bassey Antia, María Pozzi

4. *Heribert Picht, Christer Laurén: Repräsentationsformen in der Terminologie*
    Opponents: Margaret Rogers, Sue Ellen Wright

5. *Bertha Toft: Systems of concepts and the organisation of knowledge*
    Opponent: Øivin Andersen

This issue of the IITF Journal contains the contributions of the authors indicated under 1 – 3 above plus the contributions of their respective opponents. The next issue of the journal, IITF Vol. 13 (2002) no. 1-2, which will appear early in 2002, will contain the remaining contributions. It is our hope that these two issues will provide an image – however sketchy – of the dynamic development which has taken place within terminology science over the past 10 years.

Bertha Toft   *Heribert Picht*

CONTENTS

Gerhard Budin
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A Critical Evaluation of the State-of-the-art of Terminology Theory

Contents

1 Introduction and overview – purpose and goals of this analysis

The overall purpose of this workshop is to take a critical and comparative view to terminology theory and components of such theories as far as certain topics are concerned. The task I have here in this workshop is to present the results of a critical evaluation of terminology theory from a meta perspective, i.e. from the perspective of philosophy of science and closely related fields such as sociology of science and epistemology.

Part I of this paper is located on the meta level, i.e. it is to elaborate the methodology for performing this study. First of all we have to look closer at the concept of theory as it is actually used in contemporary philosophy of science and most sciences of today. Unavoidably we also have to clarify the concepts of science and of scientific discipline. Chapter 3 elaborates the methodology of evaluating and comparing scientific theories by describing a set of parameters and criteria for this purpose. Only now, equipped with these meta theoretical and methodological instruments, we can move to part II and dare to embark on actually performing the analysis on the object level, i.e. on analysing the object of investigation – terminology theory. This part is divided into 2 chapters, one focusing on a brief historical account of the development of terminology as a field of activity, but also, by applying the research methodology developed in part I, on coming to conclusions on whether terminology is actually a scientific discipline and what terminology really is. The fundamental divergences on these fundamental questions that we are confronted with in the relevant research literature is a particular item of discussion. Chapter 5 will identify various 'theories' and compare them according to the set of criteria set out in part I. Part III of this analysis discusses the conclusions and provides a summary of the results and an outlook into future research in the field.

PART I THE META LEVEL: PREPARING THE RESEARCH METHODOLOGY

2 What is a Theory ?

For the purpose of this analysis we will concentrate on the contemporary meanings of this term 'theory', disregarding the historical

meanings and the conceptual changes from classical Greek until today. In philosophy of science we usually distinguish 4 major meaning clusters for the term 'theory' (Mittelstraß):

- *The extra-scientific meaning*: in the general language we use the term 'theory' either to denote a vague conjecture about a fact or about how to act, or to use it in a pejorative way to distinguish a rather useless theory from common sense action and practice;

- *The philosophical meanings*: the Greek θεωρία (observation, contemplation) played a crucial role in Platon's theory of ideas in order to separate the world of ideas (theoretical and philosophical knowledge) from the world of practice and from practical knowledge. Aristotle developed this distinction further into a systematic theory of knowledge where the role of logic was much more pronounced. In post-classical, Medieval, and early Modern philosophies this distinction was further deepened. Kant achieved a separation of the notion of theory from directly observable entities and focused on the constructive element in theories as a human achievement. In the context of idealism as primarily developed in Germano-Austrian philosophy in the 19th century, the term 'Lehre' was increasingly used denoting both a field of knowledge and a theory claiming to describe and explain a certain part of reality. In the humanities, therefore, it was common until the middle of the 20th century to use the word 'Lehre' synonymously for the word 'Theorie'. Since the 1960s, though, the term 'Lehre' is hardly used in scientific contexts in this sense, limiting it to the meaning of 'teaching'. Competing philosophies and epistemologies have developed competing theories of meaning and theories of truth, for example. Similarly, humanities and social sciences have been characterised by a theory pluralism that mirrors this epistemological pluralism and that is unknown in natural sciences. Out of dozens of philosophical definitions of 'theory', Husserl's definition from 1929 was much wider than his own that he had formulated around 1900 (where he focused on deductive and nomological theories only) and that included all closed systems of propositions in a scientific discipline. The idea of a theory pluralism was combined with a typology of theories that became popular in social sciences and humanities, while natural sciences have ever since focused on the unification of sci-

ence in more axiomised and formalised theories that have a uniform logical structure grounded in an empirical basis of observation protocols. In the 20th century the division was aggravated by ideological confrontations between Neo-positivistic and objectivistic conceptions of the concept of 'theory' and more subjectivistic and anti-positivistic approaches.

- *The different usages in individual scientific disciplines*: there have been many attempts at classifying different kinds of theories in various scientific disciplines. There is a wide range of theories on a spectrum of theoreticity, i.e. theories that are inherently practical and that describe a certain practice in a systematic way and inherently abstract and theoretical theories that describe abstract entities and constructions of human thought. The purpose of a theory might also vary considerably, which is a crucial factor in comparing different theories to each other. The degree of formalisation is another parameter for distinguishing different kinds of theories, as well as the nature of knowledge that is contained: hypothetico-deductive theories (either axiomatic or analytical) and constructive theories, formal and empirical theories, explanatory theories and hermeneutic theories, etc. The 20th century was characterised by a strong preference for axiomatic and deductive theories, but only at the end of that century social sciences and humanities managed to emancipate from that dogma and formulated their own criteria of theoreticity. The common denominator for evaluating all kinds of theories are the criteria of the absence of contradiction and of productivity. All other criteria are limited to a specific kind of theory. Another perspective is the object range of a theory, with quantum physics going far beyond observable entities into computed entities whose existence is postulated in a theory and is proven to exist much later (or maybe not proven to exist).
- *The usages in contemporary philosophy of science*: explanation and prognosis are the major purposes of a theory, but the means to reach this goal, to comply with this purpose, are quite different and basically equal to each other (methodological pluralism). In the paradigms of Logical Positivism, structuralistic Analytical Philosophy of Science, and Critical Rationalism that were predominant from the 1930s until the

1980s (Hempel, Carnap, Stegmüller, Popper, Sneed, Lakatos, etc.) the logical structure of theories was of prime importance. Neo-Pragmatist approaches (Stachowiak, Putnam, Suppes, Rescher, Rorty, et al.) that have gained importance over the last 30 years also look at the contexts of research and introduced pragmatic criteria of theory evaluation. Theories are neither constructed only on the basis of induction nor of deduction, but in an interactive spiral between these two complementary processes. As far as the basic nature of theories is concerned, structuralist and semantic conceptions of 'theory' have been become widely accepted. Theories are predicates or second order concepts that are represented in axiomatic systems of variables. When applied to objects, these concepts become propositions. The reconstruction of the dynamic development of theories and the criticism of theories have been focused on by constructivistic and evolutionary conceptions of philosophy of science. The pragmatic contexts of empirical experience are also considered to be of crucial importance for understanding the nature and role of theories and scientific research. The linguistic dimension in the semantic conceptions of theories has also led to the focus on and the need for terminological precision in a theory by defining all terms used by recurring either to axiomatically introduced terms or to terms defined elsewhere in the same theory.

A problem that is shared by all these different conceptions of 'theory' is the inconsistent or incorrect way of using the term 'theory'and confuse it with 'hypothesis', 'axiom', 'assumption', etc., especially because axioms and hypotheses are needed to build theories. Mostly implicit assumptions should be made explicit in order to facilitate the comprehension of theories by the science community. The focus on the historical perspective of theory dynamics and theory reconstruction (Toulmin, Callebaut, Kuhn, Thagard, Oeser, etc.) is governed by the discussion on evolutionary vs. revolutionary models of theory change and by the question whether the Kuhnian theorem of the incommensurability of subsequent theories and their concepts is preferable to the evolutionary model of incremental growth of knowledge (Popper 1972, Oeser 1976, et al.).

3   Methodologies of the evaluation and comparison of scientific theories

In comparison to industrial and service economy related quality management methodologies, there is no established methodology for evaluating and comparing scientific theories. There are several reasons for this: a) As we have seen above, there are various multidimensional schemes for distinguishing different kinds of theories in the sciences; b) Different epistemologies and philosophies of sciences have different concepts of science and of theory (pluralism in theories and methods). c) This implies that there are different methods of evaluating theories and of comparing them.

As mentioned above, the minimum quality requirements that are accepted across different epistemological paradigms that are accepted for evaluating theories are that a theory must be consistent, i.e. it must not have any internal logical contradictions, that it identifies an explanandum or at least indicate the phenomena that are to be described, and that it describes the methods of the explanation or of description in such a way that other researchers can verify such experiments, explanations or descriptions. A theory can contain or consist of one or more hypotheses that are to be tested. From the semantic and terminological points of view, a theory should also define the terms (and the underlying concepts) and describe the axiomatic propositions and their concepts that the theory is founded on.

For the social sciences, cultural sciences, and humanities, where the pluralism of theories and methods is one of the few basic assumptions that are shared by all researchers active in these fields, there has been a long discussion going on about whether at all and if so, how we can actually compare theories to each other. In sociology, for instance, this discussion has been led for at least 40 years, but positions are as diverse as ever before: while one group is convinced that the comparison of sociological theories that are directly competing, i.e. that are claiming to describe and explain the same explananda, is actually necessary in order to eliminate all but one theory that is most suited for this purpose, there is another group of researchers that is fundamentally opposed to the notion of comparing any theory to another one and even refuse to discuss this topic. Some members of this group refer to Kuhn and his concept of incommensurability between two theories in the diachronic perspective when one theory is overriding another one, in

which case the earlier theory is incompatible with the newer one and can therefore not really be compared to each other. Even worse, among those who basically agree that comparing theories to each other is at least useful, even when its purpose is not to eliminate inadequate theories but just to learn from each other (mainstream opinion), there is no agreement on how to carry out this comparison. Schneider distinguishes three major types of theory comparisons (Schneider 1999):

- *A hermeneutic and individualistic comparison of theories*: the goal is to understand the theories to be compared independently of each other and to compare how each theory claims to solve the problems it has first described. Concepts used in both theories are compared in such a way that this helps understand these theories. Different solutions are not necessarily contradictions. The evaluator of the theories may construct a common problem context and describe the different solutions that theories give in this context.

- *A coordinative comparison of theories*: the goal is to compare the theories in relation to and interaction with each other and to find out the common denominator of theories compared, and, in addition, to identify those areas where only one theory offers a solution or an explanation where the others do not, so that in the end a cluster of theories emerge that support each other and add to each other and that can be used in a combined way.

- *A confrontative comparison of theories*: the goal of the comparison is to find the best theory among those compared and to eliminate all others. Contradictions and differences are more interesting to the evaluator than commonalities and convergences. Conceptual and logical inconsistencies are more interesting than conceptual harmony among theories.

Part II The object level: An evaluative study of terminology theory

For the purpose of the analysis in this paper, I choose the coordinative type of theory comparison. The quality criteria described above are applied and the comparison follows the criteria defined above. Any comparison and evaluation of theories should first look at the historical contexts of how these theories have emerged and have developed. Secondly, it is essential to identify differing explananda these theories have

and to make explicit underlying assumptions.

4   Historical outline of the development of terminology as a field of activity

This outline is very very brief, since a full historical account of this development is beyond the scope of this paper. On the other hand, this brief historical outline is essential as a foundation and starting point for all subsequent considerations and argumentations. For more detailed and recent descriptions of historical developments see Laurén/Myking/ Picht 1998, Oeser/Picht 1998, Picht 1996, Rey 1992. While it is a truism that the history of science is at the same time a history of terminology in the sense of constantly coining new terms, creating new concepts, changing the meaning of existing terms, re-arranging the conceptual structures in theories (see Thagard for convincing case studies from the history of science), terminology has become an explicit, established and reflected field of activity and a scientific research field only recently. As a practical field of activities, terminology has very different manifestations that have co-existed and co-developed more or less independently of each other. This fragmentation is clearly mirrored in the lack of knowledge in most of these communities that are interested in terminology about the fact that there are other professions and communities that are *also* interested in terminology and work in this field. While terminology has traditionally been of great interest to language professionals of different kinds, i.e. linguists, lexicographers, translators, technical writers, etc., it has been at the same time an established activity of information professionals, e.g. librarians, archivists, etc. and also to historians of science that are interested in the terminological development of scientific disciplines. Engineers who are working in standardization efforts, scientists who are serving on terminology committees for maintaining and further developing nomenclatures and taxonomies, are usually not interested in what language or information professionals are doing in terminology, and vice versa. This situation has severe consequences for the state-of-the-art of terminology as a field and for theories of terminology, as we will see later on.

In addition to the fragmentation along the lines of division between professions, fields of activity and scientific research areas, there is another fragmentation along the lines of barriers among different cultures and different linguistic communities. Although practical termi-

nology work is a global undertaking, because it is being done all over the world, in dozens of different countries and in many different languages, terminologists in these language communities are not aware of what is done in other language areas and countries.

All attempts that have been made so far to establish theories in this field of terminology can be characterised by a strong orientation towards practical problem solving and to establish methods with scientific justification in order to make these problem solving efforts more efficient.

4.1 What is terminology ? Is terminology a science, a scientific discipline ?

In this paper I have not yet even defined what terminology is. This was done on purpose, since this definition is a major part of the problem that we are investigating here. The disagreement and divergence among terminology practicioners and terminology researchers can hardly be more blatant. All the fragmented groups mentioned above have different notions of what terminology is. But also within these groups there are very different ideas. The term "terminology" causes confusion and disagreement: it has been asserted many times in recent decades that this term has several meanings (e.g. Wüster 1974), i.e. a) the collection of terms and associated concepts in a subject field in a particular language, b) the presentation of such a collection in the form of a dictionary, a glossary, etc., and c) the theory or the scientific subject field that studies the phenomenon, either understood as the "science of terms" (based on the word elements "termino" and "logy"), or on the level of theory as the "theory of terms" or more simply and modestly as the "study of terms" (e.g. Sager 1998) or as terminology studies (or terminology science, Terminologie, Terminologieforschung oder Terminologiewissenschaft im Deutschen). Budin (1993) has criticized this threefold meaning of the term "terminology" as a contradictio in adjecto and called for using different compounds to denote the lexicographical collection and the scientific field of study in order to keep the term "terminology"only for the collection of terms and concepts in a subject field. This disambiguation of a polysem is also supported by valid ISO standards, in particular ISO 1087, the vocabulary of terminology, in its version from 1990 and also in its revised version as ISO 1087 part 1 from 2001. On the other hand we can argue that the context of use is usually disambiguating a polysem so that in practical usage of the term "terminology" we would not have any problems. This discus-

sion is led in different ways in various communities of researchers and in various language communities.

But even then the confusion is not overcome yet. Still there is another major disagreement concerning the (right ?) way of how to interpret Saussure for the field of terminology, as far as the (disputed) inseparable unity of the sign and its content (meaning) in the sense of the "term" (Terminus in German vs. the Benennung is the linguistic sign only) is concerned. These differences can only be stated here, but the problem obviously cannot be solved here.

The question whether terminology is a science and a discipline in the sciences is also answered in many different ways. While Wüster, Felber, et al. have stressed for decades that terminology is a science (nota bene "Lehre"in German in the traditional meaning, as noted above in Part I, as a practical field of knowledge AND as a scientific field of study), others have repeatedly denied this (e.g. Sager 1990), or have criticized the need for scientific disciplines and have called for a trans-disciplinary integration without borders among disciplines so that the question is rendered futile (Cabré 1998).

From a purely descriptive point of view in science research (social study of science), it is simply a fact that terminology is a scientific discipline, because it fulfils all the criteria that have been established for this question, but not because anybody has proclaimed this or has postulated this in a publication or at a conference. There is no institution in the world that would want to or would be accepted by scientists to judge whether a certain field of study is a science or not. But the following criteria definitely apply to terminology: there are journals and publications of its own dedicated to terminology. There are congresses, symposia and workshops independent of any other discipline held as scientific events. Research projects carried out in this field in many countries in the world. There are a number of institutions and associations (at national, regional and international levels) in terminology that are autonomous from such structures in other fields. There are university professors for this field of study and it is a field of study at university level in many countries (Oeser/Budin 1998). At the same time it has always been certainly true what Cabré (1998) has stressed concerning the transdisciplinarity of terminology as a field of study. This is also true for many other disciplines (cognitive science, information science, ecology, etc.).

4.2 Conclusions I

For the time being we can state that terminology in its practical manifestations is an established field of activity in the sciences, in language and information work, and as a transdisciplinary scientific field, as a transdisciplinary discipline.

5 Comparison of explicit and implicit terminology theories

A coordinative type of comparison of terminology theories was carried out by C. Laurén and H. Picht (1993). They compared several theories from the perspective of schools of thought and according to a set of criteria, i.e. the individual theoretical positions of each theory and of each school of thought on issues such as concept, term, relation concept-term, position on terminology planning and standardization, position of terminology in the system of sciences, etc., and came to the conclusion that the theories and schools compared (the so-called Vienna school with Wüster and Felber as its main representatives, the so-called Soviet school and the so-called Prague school, and several other research traditions such as Canada, Germany, Scandinavia, and some more recent efforts in terminology and knowledge engineering) have much more in common than commonly assumed, that these "terminology schools" never really existed as sharply separated and isolated traditions but rather as closely connected and interactive research traditions that share a major set of theoretical assumptions, and that the differences lie in different priorities and research interests. The conclusion was that we should rather talk about a single terminology theory that all researchers are sharing and contributing to in different ways (and in different languages). The commonalities were stressed, still the differences were identified and analyzed.

Meanwhile the field of terminology has rapidly developed and diversified into new research activities all over the world (all over Europe, in the Americas, in East Asia, South East Asia, Western Asia, Africa, etc.) that have added many interesting issues. Socioterminology is an interesting example of a strong new paradigm that (unfortunately) independently of each other has been a topic of many research activities in Scandinavian countries (E.g. numerous studies and publications by Sæbøe, Myking, Toft, Laurén, Nordman, Rangnes, Jónsson, etc.), but also in Francophone contexts (France and Quebec in Canada), and other areas such as Catalonia, Africa, etc., e.g. Gambier 1991, Gaudin 1993,

Boulanger 1995, Temmerman 2000, Cabré, Antia 2000, etc.

Computational terminology can be added as another theory cluster (Budin forthcoming in 2002) integrating corpus based terminology extraction and analysis approaches with theoretical models, but also data modeling and meta-data modeling efforts in terminology management, theoretical aspects of terminology engineering as a computer application effort, etc. Corpus-based approaches have reached a high theoretical level and have been developed in several universities and research units, also with practical implementations and tangible results concerning term recognition and term extraction (Pearson 1998, Heid 1999, all contributions in Bourigault D. Jacquemin C. L'Homme (eds.) 1998, many articles by Ahmad (e.g.1998), and many others.

An established example of an independent paradigm explicitly limited to the special epistemic, communicative, and conceptual conditions in the social sciences was founded by Fred Riggs (1984 et passim).

Many of the authors mentioned above have sharply criticized the theory of terminology elaborated by Wüster or have even denied that it is a theory at all. Contrary to ethical standards in science, as practiced in all disciplines including the humanities and social sciences, some critics simply ignored more recently published articles or monographs that are accessible in English and in other languages that do contribute to a more scientific and up-to-date account of terminology. Instead they claimed that nothing new happened after Wüster's death in 1977. Although many of the criticisms about Wüster's theory are correct, a more constructive and, as we have seen above, a coordinative approach to evaluating and comparing theories and individual assumptions and hyptheses would be much more productive in order to further develop terminology theory from a more holistic and integrative point of view. In addition, any a posteriori description and criticism of a *historical* theory must be looked at and evaluated from contextual point of view, i.e. to take into consideration what Wüster actually intended. But when the critics ask themselves for a theory of terminology (or a theory of terms, as it where), only a few initial assumptions are made. Fortunately, several doctoral dissertations have been prepared in recent years that take critical positions on established principles, while at the same time present their own theoretical model that indeed contribute to a higher level of terminology theory.

Sometimes microtheories are proposed (e.g. in a single very short article) with a few assumptions and a very brief description of the goals and other "ingredients"of a theory, sometimes detailed analyses and argumentations are presented.

5.1 Are they scientific theories ?

Applying the criteria from philosophy of science as they are usually accepted in scientific communities, all terminology theories we know of are on a relatively low level of theoreticity, usually introducing several hardly corroborated axiomatic assumptions and some descriptions of terminological phenomena. Theories have been created with practice in mind in eclectic processes of selecting some theoretical elements from different fields with a low level of integration and hardly any emergent properties of the theory of its own. Still they qualify as theories, given the minimum quality requirements described above, as they are basically consistent internally and identify the explananda and give methods of their description, but hardly ever of real explanations of certain phenomena. On the other hand, when we follow the argumentation of a more strict philosophy of science and the opinion of a number of terminology researchers (e.g. Cabré), we would have to come the conclusion that we only start now to collectively build a real terminology theory. In this case we are still far away from this goal, as the most basic underlying assumptions and axioms will have to be discussed in much more detail and on a much broader basis.

5.2 Conclusions II

Following the coordinative methodology of theory comparison and given the results of the very brief account of more recent and very numerous publications in new fields of terminology (socioterminology, computational terminology, terminology management, terminology and knowledge engineering, terminology and knowledge organization and knowledge management, social science terminology, etc.), we have good reasons for being optimistic that a more integrative and cooperative approach will prevail over time in this field. We also state that terminology science has become much more globalised in the sense that paradigms do not develop any more just along national lines or language community boundaries, but rather according to shared research interests and application areas.

6 Discussion of conclusions I and II, summary and outlook for future research

Like any other emerging scientific field of study, terminology is going through a process of systematization, community building and the development of discourse patterns, of research traditions that increaseingly interact and fertilize each other, where a culture of sound competition in theoretical and R&D activities has developed, and where a large number of events are taking place every year and all over the world, and where the transdisciplinary and transcultural character of the subject field is strengthening. From a macro-perspective, i.e. from outside terminology, and in comparison to other, more established disciplines such as linguistics, we can expect that a single, but collective, yet very multifaceted and multidimensional theory of terminology is currently emerging, on the basis of the pioneering achievements of the first generation of terminology researchers, and now with a whole new generation of young researchers bringing many new aspects into the discussion and providing interesting and promising results in their research activities. Cooperation with other disciplines (including those where terminology is considered a part of their own discipline) and with various branches of industry is vital for the further development, I expect that terminology theory will still remain to be very practice oriented, but certainly on a higher level of theoreticity, quality, relevance, and integration. It is also necessary to demystify Wüster and his œvre and to develop a postive culture of mutual respect and constructive criticism. The research facets and dimensions that individual researchers or groups of researchers are focusing on include term formation, neology, socioterminology, cognitive aspects, terminology planning, knowledge organization and terminology, terminology and knowledge engineering and knowledge management, ontologies and epistemologies, corpus linguistics and language resource management, data modeling and metadata modeling, technical communication, transcultural content management, translation technology and terminology engineering, multimedia applied semiotics, etc.

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Merja Koskela and Bertha Toft were asked to prepare comments for Gerhard Budin's contribution. As there had unfortunately been very little time to prepare a proper theoretical contribution, the two opponents chose to put a number of questions to Gerhard Budin, based on Bertha Toft's original questions (see below) as well as on Merja Koskela's longer contribution. The latter was thus not presented in full at the colloquium, but we find it important to include it in this issue.

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TEEN AGE REBELLION OR MIDLIFE CRISIS ? – A COMMENT ON GERHARD BUDIN'S PAPER "A CRITICAL EVALUATION OF THE STATE-OF-THE-ART OF TERMINOLOGY THEORY"

A critical evaluation of the state-of-the-art of any modern scientific discipline is a difficult undertaking. As we all know, it is not easy to put into perspective phenomena that one is very much part of, both when it comes to the contents of the discipline and its historical development. Nevertheless, Gerhard Budin has now given us his critical evaluation, and it is our responsibility to consider to what degree he has been able to free himself from the constraints of his own involvement. However, Gerhard Budin has made a wise choice of theoretical framework for his critique: philosophy and philosophy of science offer an excellent way of putting modern phenomena into perspective since they address general questions common to all disciplines.

In order to give a critical evaluation of the state-of-the-art of a discipline it is necessary to recognize what the state-of-the-art actually is: what is it that we are viewing critically. According to the title of the presentation it is <u>terminology theory</u> that is being viewed. On the basis of Gerhard Budin's paper we can derive the following:

1. there is a considerable fragmentation in the field of terminology because the communities interested in terminology do not know each other's work well enough since they represent different fields of activity, different scientific disciplines and different cultures. In Budin's words: "…terminologists in these language communities are not aware of what is done in other language areas and countries" (p. 6);

2. the attempts to establish theories are practically oriented efforts for finding methods that would have a scientific justifycation;
3. the term *terminology* does not have one established meaning;
4. there is a fundamental disagreement on the unity of sign and content among terminologists;
5. it is unsure if terminology is a science even if it has the characteristics of an established discipline;
6. a single terminology theory is strived at, but the field is in a state of rapid development and is diversifying in a growing speed;
7. Wüster's seminal theory is severely criticized, and some critics choose to ignore recent articles and claim that nothing has happened since Wüster; and
8. all terminological theories are of a low level of theoreticity, but they still are scientific theories (basically consistent and identify their explananda, give methods).

As the above list shows, Gerhard Budin has carried out his task with precision, i.e. taken a critical look at the state-of-the-art of the discipline, in part even theory. Naturally, an interesting question is **why** the situation is what it is. One explanation mentioned in the paper is that terminology theory is an emerging scientific field, and like in other emerging fields, a consensus has not been reached. However, it can be asked if terminology theory actually is an emerging discipline. Historically, the roots of terminology can be traced back into the beginning of the 20th century, and Wüster's seminal work was published as early as in 1930's. Therefore, it seems that this, at least 30 to 50 years old, discipline is going through neither a negative stage of childhood nor a teenage rebellion, but rather a midlife crisis. Like in midlife crisis in general, old values and truths are questioned and new directions searched, but surely this is only one part of the explanation.

A second possible explanation stems from two aspects, related to each other: firstly, the multidisciplinary nature of terminology, and secondly, the close contact with practice. In the academic world dominated by "pure sciences", many multidisciplinary and applied fields are going through excactly the same kind of discussions and developments as terminology science. This could be called an identity crisis that is caused by comparisons and constant competition with other fields. For example, there have been similar discussions on translation theory and

on applied linguistics. What is typical of these fields is that they, because of their multidisciplinary nature, have differing needs for a theory. They also need a "multifaceted and multidimensional theory" (p. 10). In this respect, the idea of one theory that would cover the whole field becomes problematic, because in order to be relevant for the whole field, the theory needs to be very general, too general to explain anything. Similarly, in terminology theory there is an inherent diffusion caused by the object of study: different fields of science have different epistemic conditions for their concepts, terms and their study. Therefore, it seems that a general theory can cover the basic starting points for the field, but that specific subtheories are needed for the different fields. Integrating them all is the major challenge. The other aspect is that theory and practice are separated from each other. Reasons for this can be many, from reluctance of theory on the part of the practitioners to the view that practice is always irrelevant for a theory on the part of theoreticians. If practical work and contextualized research are regarded as theoretical activities, the gap between theory and practice is easier to close, and the interactive spiral between induction and deduction can be set in action.

A third explanation can be seen in Kuhn's term of a scientific revolution. Maybe the present terminology theory has met too many anomalies, and it can no longer explain what it is supposed to explain. However, at least on the basis of Gerhard Budin's paper, this does not seem apply for terminology theory because there does not seem to be any total agreement on the theoretical assumptions in the field anyway (cf. p. 8).

A fourth thought would be that the present situation is a healthy reaction to the fact that in today's world there is an ever growing need for terminology. Therefore, terminology science has to meet new challenges in many respects: there are new phenomena to be described and new ways of conducting research. Therefore, there is a clear need for a broad theory that would explain as much of the new phenomena as possible, or alternatively, this is the reason for the need for "a more integrative and cooperative" theory formation, which Budin calls for.

In this brief comment I have now collected the points that Gerhard Budin has made on the state-of-the-art of terminology (and its theory). Each point in the list can be discussed in length and new aspects can be found. I have also tried to find some explanations for why things are

like they are, and of course, the reasons can be discussed further. Finally, however, I would like to take up one relevant detail, and that is the definition of 'terminology'. For, as long as we do not agree on what we are talking about, we obviously cannot expect to find any theories to explain the phenomenon. Gerhard Budin presents the most common meanings of the term and finally states that his present view is that context of use disambiguates the use of terms. Here we come to the core of terminology theory. Traditionally, the difference between special language terms and everyday words has been that terms are names on defined concepts whereas words get their meaning in the context. Therefore, it seems that the word *terminology* in this sense is being shifted from special language to general language. For the needs of the special language of terminology science, I still feel that concepts should be defined and given terms, however polysemic, in a way that makes a scientific discussion possible. Additionally, defining *terms* (p. 4) used in theories does not seem to rhyme very well with the wüsterian view of *concept* as the starting point for terminological analysis. In conclusion I can only state that there indeed seems to be a need for clarification of the basic assumptions of terminology theory, and based on Gerhard Budin's paper it seems that the work has begun and is in good progress.

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QUESTIONS TO GERHARD BUDIN

1.   The headline of section 4.1 is a question: 'Is terminology a science/a scientific discipline ?'

In the section it is then claimed that it is simply a fact that terminology is a scientific discipline, seeing that it fulfills the criteria established. Not because anybody has proclaimed it, but simply because there are journals, publications, congresses, workshops etc. independent of any other discipline.

Can we conclude, then, that the question asked in the headline of section 4.1
   a)   is unanswerable because no fixed criteria exist, or
   b)   cannot be answered without ending up in a circle ?

2.   In section 5.1 it is claimed that all the terminology theories mentioned qualify as theories since they fulfill the universally agreed minimum requirements for a theory which you have set up i sections 2 and 3.

But on the other hand it is claimed, also in 5.1, that if we follow the argumentation of a more strict philosophy of science and the opinion of a number of researchers (e.g. Cabré) we come to the conclusion that we only start now to collectively build a real terminology theory, and that in this case we are really still far away from this goal.

What is your conclusion ?

3.   According to section 3, the coordinative type of theory comparison has been chosen for your presentation. As defined in that section, its goal is to compare theories in relation to and interaction with each other, ending up with a cluster of theories mutually complementing and supporting each other.

In section 6, in which your conclusions are discussed, it is claimed that from a macro-perspective we can expect a single, but collective, yet very multifaceted and multidimensional theory of terminology to emerge from ongoing discussions.

Is this to be understood strictly as defined under the coordinative type of theory comparison, i.e. that the theories support and add to each other and can be used in a combined way ?

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TERMINOLOGIE AUS LINGUISTISCHER SICHT

Als Ausgangspunkt für unsere Darlegungen haben wir unsere Analyse zum Vergleich der 'terminologischen Schulen' von 1993 gewählt. Wir konnten aufgrund der damaligen Untersuchungen folgende graphische Darstellung erstellen und zu folgender Konklusion gelangen:
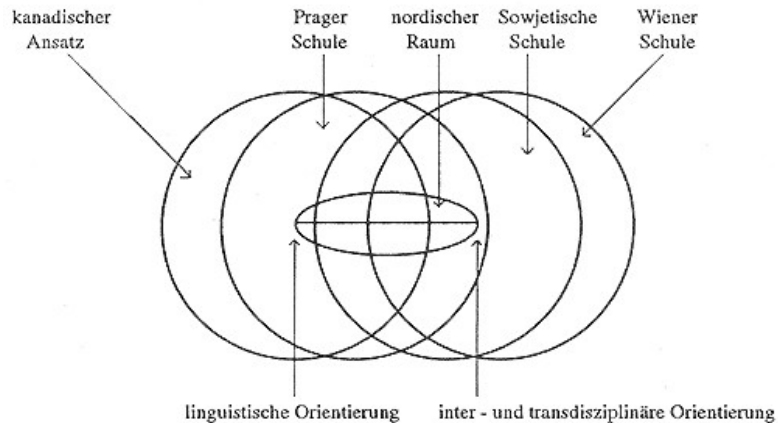
"In Fig. 1 werden die Pole durch 'linguistische Orientierung' und 'inter- und transdisziplinäre Orientierung' bezeichnet und stellen das Verhältnis der theoretischen Positionen zueinander dar.... Auf der theoretischen Ebene, die wie in jeder sich dynamisch entwickelnden Wissenschaft von der Suche nach adäquaten Lösungen, d.h. Theorienbildungen und ihrer Erprobung in und Anwendbarkeit auf die Praxis geprägt ist, gibt es naturgemäß abweichende Auffassungen oder genauer gesagt unterschiedliche Schwerpunkte - was keineswegs das gleiche sein muß. Diese Tatsache kann nur positiv bewertet werden, da jede Weiterentwicklung aus dem Spannungsfeld zweier oder mehrerer Auffassungen genährt wird."

Die Feststellung der Tatsache, daß die Sprachwissenschaft schon immer in der einen oder anderen Form und mehr oder weniger deutlich sichtbar mit der Terminologie verknüpft war und ist, ist weder neu noch

kontrovers. Diese Behauptung hat ihr nachprüfbares Fundament u.a. in folgenden Sachverhalte:

1. In Rußland hatten von Anfang an namhafte Linguisten an der Entwicklung der theoretischen und angewandten Terminologie wesentlichen Anteil, ihre Beiträge waren und sind gewichtig und konstituierend; eine Analyse der uns zugänglichen russischen terminologischen Literatur untermauert diese Aussage.

2. Wüster besaß solides linguistisches Wissen, das dem Kenntnisstand seiner Schaffensperiode entsprach; aus seinem Archiv und der zugänglichen Literatur läßt sich ablesen, mit wem er im linguistischen Bereich in Gedankenaustausch stand und welche Beiträge er in linguistischen Fachorganen veröffentlicht hat. Leo Weisgerber sei hier stellvertretend für eine Reihe von führenden Linguisten seiner Zeit genannt. Ferner sollte nicht vergessen werden, daß es gerade Wüster war, der immer wieder versuchte, die AILA für terminologische Fragestellungen zu interessieren und die Terminologie der angewandten Sprachwissenschaft nahezubringen, z.B. auf dem AILA-Kongreß 1972 in Kopenhagen. Aber auch schon vorher nahm er an den großen Linguistenkongressen teil, auf denen er seine Ideen vortrug und in Arbeitsgruppen mit namhaften Linguisten zusammenarbeitete; als Beispiel sei der 8. Internationale Linguistenkongreß in Oslo Mitte der 50er Jahre genannt.

3. Der kanadische terminologische Ansatz ist klar linguistisch orientiert, doch sind bei den einzelnen Autoren dieser Richtung graduelle Unterschiede feststellbar.

4. Der nordische Ansatz hat immer auch eine unverkennbare linguistische Ausrichtung gehabt, was mit aller Deutlichkeit aus der skandinavischsprachigen terminologischen Literatur hervorgeht.

Es kann also festgestellt werden, daß die linguistische Anbindung mehr oder weniger sichtbar und je nach Schule und Ansatz immer und ausnahmslos gegeben war.

Ferner muß festgestellt werden, daß die Linguistik als wissenschaftliche Disziplin, von einigen Ausnahmen abgesehen, sich lange Zeit wenig für Fachsprachen und Terminologie interessiert hat, wodurch diese Gebiete gezwungen waren, eigene Wege bei der Lösung ihrer akuten fachkommunikativen Probleme zu gehen. Die Main

Stream-Linguistik jener Zeit konnte zu den Problemstellungen der Terminologie und der Fachkommunikation kaum nennenswerte Lösungen anbieten. Im Mittelpunkt ihrer Bemühungen stand nicht selten der Versuch, logische Gründe zu finden und Theorien zu bilden, die das Fachgebiet Linguistik abgrenzen und das Arbeitsfeld des Linguisten abstecken sollten. Zeitweise versuchte man sogar dieses Ziel dadurch zu erreichen, daß man den Begriff 'Bedeutung' ausklammerte, da er sich nicht ohne weiteres in die damaligen Modelle einfügen ließ. Man kann so weit gehen und behaupten, daß die Linguistik sich sehr weit von gesellschaftsrelevanten Problemstellungen entfernte, was u. a. Labov (1972:187), einer der führenden Soziolinguisten, kritisierte. Er sagt: *"Linguistics has thus been defined in such a way as to exclude the study of social behavior of the study of speech ... it is difficult to avoid the common-sense conclusion that the object of linguistics must ultimately be the instrument of communication used by the speech community; and if we are not talking about that language, there is something trivial in our proceedings."*

Man übersah auch, daß die wissenschaftliche Einteilung der Welt, d.h. die Einteilung in wissenschaftliche Fachgebiete, nie logischen Grundsätzen folgte.

Dieses Bild hat sich in den letzten drei Jahrzehnten grundlegend geändert. Die Linguistik hat ihren Forschungsgegenstand erheblich erweitert und gerade durch diese Erweiterung ergab es sich fast von selbst, daß eine ganze Reihe von terminologischen Problemstellungen nun ganz natürlich in den Bereich der Linguistik fallen. Man könnte auch sagen, die Linguistik hat sich der Terminologie angenähert - ohne es zu wissen oder explizit zu wollen. Damit hat auch die Forderung, die Terminologie als selbständige Disziplin zu betrachten, einiges an Aktualität verloren. Das Verhältnis Terminologie - Linguistik kann heute mit dem anderer Bindestrichdisziplinen im Bereich der Linguistik verglichen werden.

Zwar gibt es heute noch Ansätze in der Terminologie, die ihren Ausgangspunkt in einer scharfen, ja zeitweise polemischen Kritik älterer terminologicher Ansätze nehmen, allerdings ohne in Betracht zu ziehen, daß eine ganze Reihe der von diesen Ansätzen erhobenen Forderungen längst bearbeitet oder gar erfüllt worden sind. Außerdem ist es akademisch wenig fruchtbar - um einen milden Ausdruck zu wählen - ältere Ansätze aus ihrem Bedingungsgefüge zu reißen und sie mit chronologisch inadäquaten Maßstäben zu messen. Das hieße

Aristoteles anzulasten, daß er z.B. Gadamers Gedankengut nicht in seinen Arbeiten berücksichtigt hat - ein absurder Gedanke.

Auch wird erschreckend deutlich, daß Sprachgrenzen ganz offensichtlich den terminologischen Wissenstransfer behindert haben und noch behindern. Eine Analyse von Bibliographien läßt deutlich erkennen, daß z.B. eine Reihe von Wüsters Kritikern nur einen Bruchteil seiner Arbeiten kennen und aus dieser Halbkenntnis Schlüsse gezogen und Urteile abgeleitet haben, die einer eingehenderen Überprüfung nicht standhalten. Gleiches gilt auch für die russische und die skandinavischsprachige terminologische Literatur. Adamzik (2001) weist darauf hin, daß große Sprachgemeinschaften wenig geneigt sind, auf die Fachliteratur anderer Sprachgemeinschaften zu verweisen, dies trifft besonders auf die angelsächsische Welt zu, wo oft nicht einmal auf die englischsprachige Literatur anderer Sprachgemeinschaften hingewiesen wird. Im Gegensatz hierzu ist die Tendenz, in der Fachliteratur kleinerer Sprachgemeinschaften auf einen breiten Fächer von Fachliteratur verschiedener anderer Sprachgemeinschaften zu verweisen, nachweisbar.

Mit der Erweiterung des linguistischen Forschungsgegenstandes um Bereiche wie Pragmatik, Text und Kommunikation und eine weit stärkere Einbeziehung einer semiotischen Sicht finden terminologische Forschungsgegenstände ihren natürlichen Platz im Gesamtgefüge der heutigen Linguistik.

Wir werden versuchen, dies anhand von einigen Beispielen zu verdeutlichen.

1.   Die Erweiterung des linguistischen Interessenbereichs läßt heute zu, terminologisch relevante Repräsentationsformen für begriffliches Wissen einzugliedern. Es sei hier nur an die sprachlichen und nichtsprachlichen Repräsentationsformen erinnert, auf die wir in unserem zweiten Beitrag noch eingehen werden. Schon früh hat von Hahn (1983) gefordert, daß auch die nicht-natürlichen sprachlichen Elemente der Fachkommunikation Teil der Fachsprachenforschung sein sollten.

2.   Die Auflösung des an sich wenig fruchtbaren Gegensatzes 'Fachsprache - Gemeinsprache' hat wesentlich dazu beigetragen, terminologisch relevante Repräsentationsformen auch aus linguistisch-fachkommunikativer Sicht nuancierter zu behandeln, obwohl funktionale

und damit auch fachkommunikative Ansätze in der Terminologie weit zurückverfolgt werden können - also schon lange bestanden und in der terminologischen Praxis ihren Niederschlag fanden. Schon Grimm weist im Vorwort zu seinem deutschen Wörterbuch darauf hin, daß er sich bestrebt hat, den Wortschatz von Berufen und anderen fachlichen Tätigkeiten aufzunehmen. Es sei hier ferner an die funktionale Wirtschaftslinguistik der 30er Jahre und die fachsprachlichen Arbeiten der Prager Schule erinnert, die in Drozd (1973) ihren bisher letzten Repräsentanten haben. Die pragmatischen und kommunikativen Aspekte der Modelle von Havranek und v. Hahn sind nicht zu übersehen. Das Schlagwort von 'Sprache in Funktion' ist also keineswegs neu.

3. Soziolinguistische Elemente, deren Berücksichtigung in der Terminologie heute immer lauter gefordert werden, sind ebenfalls nicht neu. Der eben angesprochene funktionale Aspekt kann ohne sozio- und pragmalinguistische Elemente nicht gedacht werden. Diese Verknüpfung ist schon in Wüster Einteilung in 'SOLL-' und 'IST-Norm' mitgedacht, Havraneks Modell beinhaltet sie geradezu als Voraussetzung und die Modelle von Hoffmann und v. Hahn beruhen ebenfalls auf dieser Verknüpfung.

4. Daß Normung, die einige Kritiker - wohl aus Unkenntnis des grundlegenden Normungskonzeptes - als das Ziel der 'klassischen' Terminologie bezeichnen, ein Teil der Sprachplanung und der in ihrem Rahmen arbeitenden Terminologieplanung ist, ist ebenfalls nicht neu und hinreichend in der terminologischen Literatur beschrieben. Auch wird wohl kaum jemand bestreiten können, daß Sprach- und Terminologieplanung linguistische Tätigkeiten sind und da sie ohne Berücksichtigung der sozio- und pragmalinguistische Elemente nicht durchgeführt werden können - die einschlägige Literatur läßt keinen Zweifel daran.

Ferner ist es eine Tatsache, daß gerade die Normung dazu beiträgt, Begriffe schärfer zu fassen und damit die dynamische Begriffsentwicklung wesentlich fördert. Der Lebenszyklus genormter Begriffe scheint kürzer zu sein, da sie öfter hinterfragt werden und so der Begriffsdynamik zugänglicher sind.

5. Was die kognitiv-semantischen und semiotischen Aspekte betrifft, so sind sie - zwar nicht mit der heute gängigen Terminologie - ebenfalls in der terminologischen Literatur nachweisbar. Wüsters Wortmodell und seine Weiterentwicklung reichte im Grunde genommen

weit über den seinerzeit dominierenden strukturalistischen Ansatz hinaus. Es wird hier von Zeichen gesprochen, die nicht auf das sprachliche Zeichen beschränkt sind. Wüsters Arbeiten zum Zeichen setzen auf einem semiotischen Ansatz auf und sind u.a. von Schröder (1993) aufgegriffen worden.

Auch der Einwand, daß Terminologie statisch sei, ist unbegründet. Damit wäre das dynamisch-kognitive Element ausgeschlossen. Da aber der Begriff sowohl als semantische als auch als kognitive Größe aufgefaßt werden muß, impliziert dies zwingend eine Begriffsdynamik, die auf das engste mit Erkenntnis und neuem Wissen verbunden ist - also auch mit der Terminologie. Selbst in der Normung, die oft als statisch dargestellt wird, ist der dynamische Aspekt, wie eben betont, nicht zu übersehen. Durch die periodische Revision von Normen ist die Einbeziehung von dynamisch-kognitiven Elementen unumgänglich und im Normungskonzept geradezu durch Regeln verankert.

Hier erscheint uns der Hinweis auf Thomas Kuhn und sein klassisches Werk über die Struktur wissenschaftlicher Revolutionen angebracht. 'Normalwissenschaft' und wissenschaftliche Revolutionen folgen aufeinander – das gilt auch für die Terminologie. Allerdings weist Kuhn auch darauf hin, daß nicht alle Revolutionen sich als Fortschritt erweisen, mit anderen Worten, daß es sich auch um Regressionen handeln kann. Diesem Gedanken sollte auch in der Terminologieforschung kritisch nachgegangen werden.

Weniger klar lassen sich einige zentrale Forschungs- und Arbeitsbereiche der Terminologie wie Wissensordnung und Wissensrepräsentation, Dokumentationssprachen (z.B. Thesauren), Nomenklaturen etc. in den Rahmen der 'neuen und geräumigen' Definition von Linguistik einordnen. Wissensordnung und ihre Repräsentation ist zwar in der Linguistik ansatzweise als Forschungsgegenstand vorhanden, z.B. in semantischen Feldern. Doch sind diese Systematisierungen für die Terminologie und ihre Anwendung in der Fachkommunikation zu rudimentär und können den fachlichen Anforderungen nur selten genügen. Hier hätte die Terminologie durch ihren Forschungsvorsprung der Linguistik einiges zu bieten. Gleiches gilt auch für Dokumentationssprachen und Nomenklaturen, die von einigen stark linguistisch orientierten Kritikern der Terminologie als nicht zur Linguistik gehörend mit der Begründung ausgeschlossen werden, daß sie nicht die natürlichen Sprachen betreffen - eine Einschränkung, die man weder

von einem linguistischen noch von einem terminologischen Standpunkt gutheißen kann. Außerdem steht eine solche Einschränkung im Gegensatz zur tatsächlich stattfindenden Fachkommunikation und ihrem pragmatischen Bedingungsgefüge. Auch in diesen Bereichen hat die Terminologie der Linguistik mehr zu bieten, als sie von ihr im Augenblick erwarten kann.

Da hier nur ein eng bemessener Zeitraum zu Verfügung steht, kann diese wichtige Diskussion nicht weiterentwickelt werden, doch meinen wir, daß die gemachten Aussagen eine ausreichende Grundlage dafür bieten, um dafür zu argumentieren, daß die Terminologie einen Teilbereich der Linguistik ausmacht und hinsichtlich ihrer Autonomie auf gleicher Ebene liegt wie andere linguistische Bindestrichdisziplinen. Dies gilt jedoch immer nur unter der Voraussetzung, daß eine Linguistikkonzeption zugrundegelegt wird, die so weit gefaßt ist, daß den Forschungsgegenständen der Terminologie und den fachkommunikativen Aufgaben der angewandten Terminologie Rechnung getragen werden kann.

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TERMINOLOGY – ON WHOSE TERMS ?

The friendly invitation by professors Picht and Laurén to give a short answer to their paper has thrilled and highly honoured me. I am a linguist by profession – not a hard core theoretical linguist though, that is because I am easily bored and therefore incapable to subscribe to ideologies. Still, I am a linguist and thus I have understood my role as an "opponent" to pose as some sort of an *advocatus diaboli.*

So here we go. Certainly, I am not going to lower us all to the level of "You hit my Bloomfield - I hit your Wüster!" I think over the last few decades the communication – or often rather miscommunication - between linguists and theorists of terminology was boringly full of such mutual attacks. And, as I have already remarked: I am easily bored.

My general attitude towards the idea of a synergetic co-operation is a very sympathetic one. I am, however, not quite as optimistic about terminology re-inventing itself as just another hyphenated linguistics ("Bindestrich-Linguistik"). I seriously doubt that our colleagues in the field of terminology would be happy to be described as representatives of just another linguistic sub-discipline. On the other hand, I can equally relate to the reluctance of a number of fellow linguists to see linguistics reduced to an ancillary function in terminology.

If we really want to work together – and I am convinced that we are meant to do so – we have to leave behind us all sorts of discipline-centered parochialism. They should have outlived their use-by date by now, having lead to so much mutual misunderstanding and even hostility between terminologists and linguists. There is, after all, as Laurén and Picht remind us, a tradition of co-operation between terminology and linguistics that was initiated by Wüster himself. Having said that, I must, however, state that with the benefit of hindsight Leo Weisgerber appears to be a strange, if not outright unsavoury, bedfellow for any academic indeed (cf. Hutton s.d., Schobert 2001).

Although I am Austrian by birth, the Austrian national hobby of sweeping differences and conflicts under the rug does not appeal to me as a good starting point for a co-operation. We should be able to work towards the same goal from our different home bases, even if I am afraid that a general "Friede – Freude – Eierkuchen" approach is too utopian to work in reality.

What I should like to propose is to jointly work out a set of ground rules clarifying
  1.   which points and axioms of our two basic disciplines can easily be agreed upon,
  2.   which ones have to be negotiated (and are negotiable), and
  3.   which ones are considered not to be negotiable by either side.

ad 1.: To the contrary of the statement made in Laurén's and Picht's paper, I do not think that linguistics is – or that structural linguistics ever has been – restricted to natural languages. Our very own linguistic patron saint, Ferdinand de Saussure, starts his series of lectures *Cours de Linguistique Générale* with a section on *La sémiologie*, in which he clearly endeavours to place his semiotic approach to linguistics within a wider, general semiotic research (cf. de Saussure 1967: 18 ff., de Saussure 1983: 15 ff.). I do not know of any subsequent linguists of any importance who would have disputed that point. Thus any arguing about a point such as this can be dismissed as gratuitous and beside the point.

ad 2.: From the viewpoint of contemporary linguistics I consider the antagonism between descriptive and prescriptive approaches to be quite overemphasized by many structural linguists after de Saussure. Going back to him again, one finds only fleeting remarks to that antagonism in the *Cours* (cf. de Saussure 1967: 1 and 97; 1983: 1 and 82). Normative grammar had already been attacked by the diachronically oriented linguists of the 19th century, if out of all the wrong reasons in de Saussure's opinion. Therefore the antagonism between synchronic and diachronic approaches appears infinitely more important to him. The pair of Saussurean terms that come closest to a distinction between normative and descriptive approaches is *langue* vs. *parole* (cf. de Saussure 1967: 11, 16 ff.; 1983: 9 f., 13 ff.).

Much of the praise of descriptive and the loathing of prescriptive approaches by subsequent linguists, I suspect, derives from the youthful

polemic antagonism that each new discipline or each new paradigm needs in order to find and keep its place in the circle of academic disciplines. The adolescence of ideas, like the adolescence of individuals, is marked by the struggle to find an appropriate place in the social group of peer ideas or peer individuals. The historiography of academic feuds is one of the more paparazzesque aspects of the history of ideas (cf. e.g. Hellman 1998). Growing up, both for individuals and for ideas, means shedding some of one's sharp edges and/or the willingness to antagonize peers in exchange for profiting from better social co-operation.

It might be all well for theoretical linguistics to insist on the pure dogma of description. Applied linguistics cannot exist without a certain normative character. The recent revision of the German spelling system for example, which paradoxically meant prescribing a less stringent norm for many individual spellings, might be considered a victory of description over prescription by declaring varying usage as the norm. As far as the teaching of German as a Foreign Language is concerned, however, it turns out to be most counterproductive.

I don't think that many colleagues in applied linguistics will strongly oppose a more relaxed attitude towards a normative-prescriptive approach as far as this is justified by the subject matter itself – as is the case with terminology.

In return, I am happy to acknowledge that the concepts of polysemy and synonymy seem to have equally been accepted widely in terminology (cf. Smith 1998) in contrast to the original demands for "Eindeutigkeit" or even "Eineindeutigkeit" widespread in terminology and some areas of LSP research (cf. Roelcke 1991, Kretzenbacher 1992: 6). Also, some lexicological models developed in linguistics have been or can be applied to the advantage of terminological description (cf. e.g. Fraas 1989 and 1990).

ad 3.: One of the things I consider not negotiable from the linguistic viewpoint is the basic semiotic theory de Saussure developed for the sign (and definitely not only for the linguistic sign). The very foundation of Saussurean semiotic linguistics challenges a notion that was prevalent in language philosophy at least up to the late 18th century – and which I suspect might unconsciously linger on within some terminological theory - the notion that language is a kind of naming game, a nomenclature (de Saussure 1967: 76 ff., 132 ff.; 1983: 65 ff., 110 ff., cf.

also Eco 1994[1993]).

The binary sign model of de Saussure, consisting of the indivisible but arbitrary bond between *signifié* and *signifiant*, has been complemented by later linguists such as Karl Bühler and Roman Jakobson. It has, however, never been left behind by linguistics and it is not likely that linguistics is going to leave it in order to accommodate terminology. Wüster's rejection of de Saussure's sign model (Wüster 1993 [1959/60]: 320 f.) shows such an amazing lack of understanding that it appears almost polemically flippant. The historiography of terminology might be able to find out why an intelligent man such as Wüster is so incredibly dismissive of the fundamental semiotic model of linguistics - I for one am at a loss to explain that.

Formulated *ex negativo*, the positive expression of this is nothing else but the binary sign model again, embedded in the model of language as a system of such signs, paradoxically a positive system that only consists of interdependent negative values. De Saussure himself goes to great lengths to state the fundamental and axiomatic nature of this assumption which he does not hesitate to call a truth:

"Always and everywhere one finds this same complex equilibrium of terms holding one another in mutual juxtaposition. In other words, *the language itself is a form, not a substance* (…). The importance of this truth cannot be overemphasised. For all our mistakes of terminology, all our incorrect ways of designating things belonging to the language originate in our unwittingly supposing that we are dealing with a substance when we deal with linguistic phenomena." (de Saussure 1983: 120, italics of the original; cf. de Saussure 1967: 146).

Given what I mentioned under point 1., we could easily exchange "linguistic phenomena" with "semiotic phenomena" in the last sentence.

The central point of disagreement between Saussurean and Wüsterian semiotics appears to be the fundamental terminological – or should I say Wüsterian ? - concept of "concept". A linguistic model of the sign cannot accept the notion of a pre-semiotic "concept" (cf. de Saussure 1967: 78 and 143 ff.; 1983: 67 and 118 ff.). Unfortunately, the use of the French term *concept* in the original and its translations such as English *concept* or German *Vorstellung/Begriff* do nothing to simplify the matter. It seems quite clear to me, however, that the Saus-

surean notion of *concept* is fully consistent with his semiotic model. According to him, concepts "are purely differential. That is to say they are […] defined not positively, in terms of their content, but negatively by contrast with other items in the same system. What characterises each most exactly is being whatever the others are not." (de Saussure 1983: 115, cf. de Saussure 1967: 139 f.).

As I understand the Wüsterian notion of *concept/Begriff* by much of its – far from consistent - usage in terminological writing, it is completely incompatible with de Saussure's notion, since it is positioned between the levels of an assumed extralinguistic reality and of an assumed linguistic reaction to this reality. Sometimes *concept* in this usage seems opposed to *term* and therefore obviously akin to the Saussurean *signifié*, in other instances it seems to refer to the sign as a whole thereby encompassing both *signifié* and *signifiant*. The inconsistency of the usage of *Begriff* and its frequent replacement by *Bezeichnung* adds to the general complexity of the problem (Kretzenbacher 1991: 195 f.).

My impression that this problem lies at the very core of misunderstandings between linguistics and terminology appears to be shared by other researchers in the field itself (cf. Myking 1998; Pozzi 2001). I seriously doubt that the problem can be solved within the theoretical framework of terminology on its own, as Sergey Grinev (1998) seems to suggest. Also, I hope that Gerhard Budin's recommendation of 1997 "that terminology theory should adopt a critical attitude towards sign models and not overestimate their importance" (Myking 1998: 1004) is not to be understood as an attempt to ignore this problem.

Unless we arrive at a clear agreement on the definition and usage of a term such as *concept*, I am afraid I cannot see linguists and terminologists working in other than a parallel way. Parallels only meet in infinity, as we know. I doubt, however, that we have so much time.

Our problem here, which on a discipline level can be addressed as the ground rules for a mutually beneficial co-operation between terminology and linguistics, on a more fundamental level concerns the very semiotic foundations of terminology.

Any dialogue has to start with shedding mutual prejudices and misunderstandings in order to get anywhere at all. It has to start with laying down one's own cards on the table in order to set up a basis for

co-operation. That might possibly hurt a few egos on both sides. I think, however, the common interest of terminology and linguistics should outweigh any personal and professional sensitivities by far.

A collaborative research project on the semiotic foundations of terminology as I can envisage it, should not aim too short. It cannot be done either by linguistics or by terminology on their respective own. I even doubt that it can be done just including those two disciplines or be content with debating and possibly integrating a Saussurean and a Wüsterian model of the sign. It would have to include semiotics and philosophy as well as the history and philosophy of science. In digging down to and laying bare the roots of our understanding of what can constitute a viable semiotic model it would have to be radical in the original sense of the adjective. It appears, however, that the problems that we, both linguists and terminologists, encounter at the surface cannot be solved at that same level.

The outcomes that we can expect of such a project of combined research efforts, on the other hand, could prove beneficial not only to the respective understanding of the theoretical foundations of our two disciplines, but beyond that fruitful for international and intercultural technical and academic communication.

If academic discourse is a civilized form of arguing, there is no better starting point to it than a good, thought-provoking challenge. By the lively discussions in this year's workshop, Johan Myking (1998: 1004) was proven right in hindsight to state that the 1997 workshop was far from signalling an end to the discussion. I think it has only just begun. Let us take up the challenge – together.

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AGAINST PRESCRIPTIVISM ? THE 'SOCIO-CRITICAL' CHALLENGE TO TERMINOLOGY

1   Introduction

To most practitioners as well as researchers of terminology, the close relationship between terminology and its social context is probably self-evident. Nevertheless, the most vigorous challenge towards traditional terminology today is heavily inspired by linguistic directions dealing with the relationship between language and society. Traditional terminology is accused of neglecting this relationship, leaving an impression that terminology is no more than a set of practical tools with a very restricted capacity – that of solving certain problems of standardisation within technological subject fields.

In this paper I will discuss two important trends of this criticism , which is inspired by context-oriented linguistics such as sociolinguistics and cognitive linguistics. I will treat them as two instances of one and the same tendency and refer to this criticism by means of the convenient and not very formal label of "socio-criticism". I intend to present some main points of their criticism before outlining some background features that may partly explain their positions. At the end, I will touch upon some general questions that deserve further discussion within terminology.

No discussion or presentation is value-free and unbiased. The reader will notice that I have outlined here and there why I find some points of criticism towards Wüster exaggerated or even wrong. Since the purpose of this paper is not deliberately apologetic, I have not, however, put much effort in documenting these points.

2   'Socio' - pertaining to society

The prefix socio- as used by recent research is pointing to three different terminological directions:

- a subfield of terminology: 'socioterminology' as proposed by Jacques Maurais (1984, 1993),
- new approaches or even 'paradigms', such as the French 'socio-terminologie' promoted by the Rouen group (e.g. Gaudin 1993, Gambier 1994), and
- the 'socio-cognitive terminology' as proposed by Rita Temmerman (Temmerman 2000)

This 'socio-criticism' has three central and evident sources of inspiration:
- Sociolinguistics, especially the Labovian direction with its focus on linguistic (speech) variation, is present in the work of Maurais (notably 1984).
- The importance of discourse analysis (in a broad sense), and also sociolinguistics, is emphasised in the works by the Rouen group
- Recent cognitive linguistics, especially as introduced by Lakoff and Johnson in their works on metaphorical concept formation is the explicit theoretical framework in Temmerman's contribution.

The position of Maurais seems to deviate from the two other positions in a significant way. To Maurais, 'socioterminology' is conceived of as a field of research dedicated to studies of terminology planning and implementation:

> Socioterminology is a convenient term that can be used to describe the relationship between society and terminology and especially the actual social use, whether by specialists or by ordinary people, of the terms coined by terminologists (Maurais 1993:121).

Within this scope 'socioterminology' might be conceived of as a branch of sociolinguistics, studying the effects of terminological efforts that, in their turn, affect and create variation in language. Where traditional sociolinguistics is concerned with speech variation, mainly on the phonological and morphological levels, the object of study of socioterminology might be formulated as that kind of lexical variation which results from deliberate intervention by terminological bodies. It does not seem justified, consequently, to subsume Maurais under the heading of 'socio-criticism', as it would not make any sense to assume incompatibility between implementation studies and traditional terminology.

More interesting with respect to the critical directions is the signal conveyed by the prefix socio-, 'pertaining to society'. An underlying

assumption is made, inferring what traditional terminology fails to be. One of terminology's basic tenets, that it is invented to solve genuine problems of communication in society, is attacked and requested at the same time. This is explicitly signalled by the wording "reintroduce terminology in the social practice constituted by discourse" in this quotation by the French socioterminologist Yves Gambier, as it is clear that it is logically impossible to "reintroduce" something that is already present:

la socioterminologie cherche à réintroduire la terminologie dans la pratique sociale qu'est tout discours, y compris le discours métaterminologique, ceci afin de l'interroger comme activité productrice/ sociale et comme activité cognitive. (Gambier 1994:102)

The suggestion is made that the contribution offered by socioterminology might fill the gap, due to recent theoretical and methodological achievements. This is explicitly stated in the writings of the Rouen socioterminology school. Louis Guespin has suggested that the prefix socio- should be ideally redundant and 'socioterminology' the unmarked and normal conception of terminology:

Il est quelque peu regrettable d'avoir à affubler notre pratique de ce formant [socio-, JM] qui l'alourdit, car après tout, toute terminologie devrait être soucieuse de la societé, du tissu même où naissent et s'échangent concepts et termes. En fait, la [socioterminologie, JM] mériterait de s'appeler tout simplement terminologie; c'est la branche wüsterienne qui, constituant une pratique restreinte, devrait être pourvue d'une détermination; il s'agit en effet d'une terminologie normalisatrice. (Guespin, "Avant-Propos" to Gaudin 1993, p. 9)

The target of this criticism is traditional terminology in the Wüsterian sense, which is said to be a terminology oriented towards standardisation ("normalisatrice") reductionist and eclectic in nature:

[La terminologie wüsterienne] reste marquée à la fois par des hypostase (le terme est x et pas y) et l'eclectisme (le terme est a et b). Son approche est essentiellement logiciste, centrée sur l'étude des notions et des systèmes de notions ainsi que celle des dénominations. (Gambier, 1994:101)

Following this line of criticism, reductionism is displayed in the very narrow scope of subject fields to be studied:

Cette terminologie de type wüsterienne [...] s'intéresse à un très faible pourcentage des communications spécialisées: elle ignore entre autres les interactions au travail, dans les laboratoires, dans l'enseignement [...] (loc.cit.)

In particular, the need for developing alternative methodologies is motivated by the need to analyse and describe the 'soft' subject fields and avoid the traditional concentration on material and technological fields.

3 'Real language studies' against 'Prescriptivism'

To sum up, terminology is accused of 'prescriptivism', the attitude of "confusing practical objectives of standardisation with facts of science" (Temmerman, e.g. p. 15). If justified, this type of criticism obviously leaves terminology in trouble. The strong connection between traditional terminology and standardisation cannot of course be denied, we are dealing with historical as well as sogiological facts. In socio-cognitive and socioterminological writings this connection is, however, interpreted as a total identification as regards theory, methods, aims and objectives. The aim of socioterminology, supported by socio-cognitivism, is said to promote a descriptive approach to terminology, in other words, to:

> get the study of terminology back to the study of real language usage. A descriptive approach to terminology is promoted, to replace the prescriptive objective of the traditional Terminology schools' approach (Temmerman 2000:31)

Again, one cannot "get something back" that is already there. As we can observe, Temmerman's and Rouen's objectives and criticism largely converge, and the suggested aim is a legitimate feature of linguistic research. This is also the reason why I do not find it justified to subsume Maurais' approach under this category, as all implementation studies must take a descriptive approach (cf. above).

The common, main critical points against traditional terminology have most explicitly been articulated by Temmerman 2000, notably ch. 1, e.g. p. 15 and 21, featuring a thorough discussion on conceptology. The socioterminological criticism has been thoroughly elaborated by Gaudin (1993, especially in part 2 of his work). The main, alleged defi-

ciencies of traditional terminology are said to concern its basic philosophical tenets as well as its practical aims and methods, and I list them according to my interpretation somewhat freely as follows:

- The philosophical platform of objectivism (Neo-positivism, Naïve realism etc.)
- a naïve belief that concepts are clear-cut instead of prototype-structured;
- over-emphasising conceptual ordering and hierarchical relations;
- a static approach to conceptology, ignoring concept formation and change;
- the objective existence of concepts in isolation from terms;
- a biased emphasis on the onomasiological methodology;
- a reductionist view of language;
- an eclectic and interdisciplinary theoretical basis;
- 'prescriptivism', i.e. confusing normative objectives, especially standardisation, with facts and science;
- context-freedom, monosemy and the 'univocity ideal', related to an
- over-emphasising of the semiotic principle of arbitrariness

The positions of Neo-Positivism and Arbitrariness have been italicized because they seem to constitute an appropriate frame of the criticism. Important keywords such as 'static', 'ordering', 'reductionism' and 'eclecticism' may be significantly framed by these two positions, as there appears to emerge a generalized consequence of this view: the paradigm of traditional terminology treats terminology, allegedly, as something existing objectively and in isolation, detached from society and its social context.

Moving on to the practical consequences of this criticism, we can observe the following:
- On the level of theoretical assumptions, both trends adopt a constructivist and cognitive approach (Temmerman, pp. 224, 228, Gaudin, as I interpret him, 96ff., 121f., 205ff.) suggesting that the point of departure for terminological analysis should be terms and not concepts ('terms' viewed as designations of units of understanding). This is in accordance with the cognitivist view that the linguistic sign is motivated, that concepts and terms are mutually

linked, and that terms are discourse (i.e. parole) items.

–   On the level of terminography, the consequence is that onomasiology and semasiology should be balanced or even that a semasiological approach should be preferred, because starting from pure concepts is not possible. (Temmerman, e.g. 227, 231, as regards Gaudin 1993, see especially the preface by Pierre Lerat).

–   Instead of introspection and context-independent conceptology, the socioterminologists (including Temmerman, p. 232) advocate the use of text corpora and they encourage the use of punctual investigations (”l'enquête ponctuelle”, Gaudin, e.g. p. 213).

–   Finally, in opposition to the 'univocity ideal' it is strongly maintained that synonymy, polysemy and variation deserve empirical studying as functional features of natural language. Any prescriptive measure, such as the introduction of neologisms, should be based on careful needs analysis and empirical studies of actual usage (Gaudin 1993, e.g. 181, Temmerman, e.g. pp. 223, 228,).

4   A broader tendency: 'correcting Eugen Wüster'

Revising the basic tenets of Wüster's terminology is no task unique to socioterminology. On the one hand, the relationship between terminology and linguistics has been thoroughly discussed for a long time with explicit reference to the works of Wüster. On the other hand, a broad and complex tendency of rethinking or even of 'correctionism' is easily observed during the last decade. A significant description of this situation is made by Alain Rey:

> […] terminology can be practised effectively only if we abandon the refining logical-semantic viewpoint which for some people epitomises terminology. […] To bring life back into the study of terminology, special languages and the translation of these languages, it will be ever more necessary to rely on the most ”general” lessons to be learned from comparative linguistics, multilingual lexicology and translation of the most culturally-specific texts, including literary works. […] we stand at the threshold of a period of of synthesis capable of correcting the dominant and overly exclusive analytical trends of the last decade(s): in short, correcting Eugen Wüster. (Rey 1996:106, italics by JM)

Within this flow of varying criticisms a number of positions might be explicitly mentioned, ranging from 'internal' revisions within the

Vienna school, e.g. Laurén, Myking, Picht 1998 (1998), Gerhard Budin (various) to contributions from other traditions. Bertha Toft has proposed an integration of traditional approaches and influences from functional and cognitive linguistics (e.g. Toft 1998). A recent contribution is Bassey Edem Antia (2000), who aims at integrating terminological principles in an operational model of multilingual language planning (see below).

All points considered and put together, the list of critical points made above is considerable. I would like to emphasise, however, that the 'correctionist' tendency is by no means uniform, on the contrary it is broad and complex. The various criticisms do not necessarily coincide in strength, content and approach. With the risk of oversimplification I suggest the following sketch of critical positions with respect to wüsterian terminology (positions a and c exemplified by Nordic works only):

(a) Moderate and "loyal": e.g. Laurén, Myking, Picht 1998
(b) Radical and "subversive": socioterminology, socio-cognitive terminology
(c) Radical and "loyal": Bertha Toft (e.g. 1998, 2001)

The position (a) is aiming at approaching terminology and linguistics, even integration, without abandoning the established methodological and theoretical tenets – such as, in particular, the onomasiological approach to conceptology. This attempt at rapprochement requires a conception of 'linguistics' that is broad, 'liberal', open to semantic description and not synonymous with 'structuralism' or 'generativism' or the like. The polarity of Terminology and Linguistics, a significant feature of earlier writings in terminology, should, consequently, be diminished (cf. Laurén, Myking, Picht 1998, ch. 12).

The position (b), on the other hand, seems to reject traditional terminology completely, as documented above. The informal label "subversive" is intended to express this intention. It is an important consequence of this conception that the polarity between traditional terminology and current linguistics is maintained and even sharpened. The socio-critical currents are the most prominent instances of this position, although similar points of criticism occur in other writings as well.

In this simple typology the main feature of position (c) seems to be a linguistic inspiration quite similar to (b) but combined with an explicit intention of analysing Wüster on the background of his historical context – hence the label "loyal". According to Bertha Toft, the wüsterian tradition needs completion and adjustment by cognitive and functional approaches instead of complete overthrowing (Toft 1998, 2001).

Instances of the approach (a) also display tendencies of the same inspiration as (c), but less clear-cut and without drawing the same radical conclusions (Laurén, Myking, Picht 1998). The reason probably is that this approach is not conceived of within an explicitly cognitive framework.

A number of writings from within the position (b), however not all, do not display many explicit and direct references to the writings of Wüster and the Vienna tradition. The empirical material as well as the scientific setting appear basically unilingual, English and French. A significant feature of the positions (a) and (c) is, in contrast, that they relate to a plurilingual setting as well as to plurilingual problems. To some extent, this corresponds to a difference of practical motivations, which is (unilingual) language planning in the case of (b) in contrast to a stronger interest in aspects of translation and even problems of standardisation in cases (a) and (c). These features deserve some investigation, because they offer partial explanations to the apparent hostility towards traditional terminology. The paradigmatic shift in linguistics towards cognitivism is directly relevant to this discussion, cf. the italicised positions in the list of alleged deficiencies in section 3 above.

5   Arbitrariness vs. motivation – a paradigmatic shift ?

The main contribution of socio-cognitive terminology is the attempt at incorporating cognitive linguistics in terminology, in particular the lakoffian emphasis on metaphorical concept formation. The basic tenet of cognitive linguistics is that of motivation: concept formation is rooted in experience, there is, consequently, a direct link between concept and symbol, the meaning of a word is directly shaped and affected by its social context mediated by experience. Within this theoretical framework, the shift from onomasiology to semasiology is easily understood.

In the past, terminologists have tended to reject bloomfieldian structuralism as well as chomskyan generativism, because of the neglect of semantic description and their tendency to separate meaning and expression as independent modules. Temmerman's claims must be interpreted against the background of cognitivism as a rejection from within linguistics of these 'system oriented' directions: not only saussurean structuralism, but also chomskyan generativism, may be subsumed under the 'Arbitrariness paradigm' by the cognitivists. I also tend to agree that Wüster's basic tenet of 'separate realms', the analytical principle that concepts and terms should be treated independently, is part of this paradigm (cf. Laurén, Myking, Picht 1998:79ff.)

There is, however, no unique and clear-cut interpretation of this paradigm: Although he insisted on the arbitrary relation between concept and expression, Saussure also claimed inseparability between meaning and expression. This latter point distinguishes him from the position taken by Wüster, for whom the notion of 'separate realms' had essential consequences for terminological methodology. As regards 'motivation', this concept has indeed been considered important by traditional terminology, but mainly as a principle of term formation (based on Saussure's notion of 'relative motivation').

There seems to be, then, an interesting paradox in the fact that the "loyal" position (a) (above) often maintains that Linguistics should open itself to directions other than the "system oriented" ones, if it is to be suitable for integration with Terminology (Laurén , Myking, Picht 1998, ch. 12), whereas the "subversive" position (b) maintains that, on the contrary, Terminology has to liberate itself completely from the system-oriented dominance if it is to be suitable for integration with linguistics.

At present the practical implications of this controversy may not be easy to foresee. The problem seems to be, however, very important for the mutual exchange of ideas between terminology and linguistics (cf. further, section 9).

6 Different orientations – unilingual vs. multilingual

The philosophical problem of arbitrariness vs. motivation has some practical implications, as it is related to the question of unilingual

or plurilingual orientation. The translation and knowledge transfer processes imply that the one-to-one assignment of concept and symbol is broken and manipulated, mainly because deliberate human efforts are involved. These practical aims themselves imply a stronger inclination towards viewing the sign relation as arbitrary.

The scope of socioterminology as well as socio-cognitive terminology appears to be restricted by unilingual orientations in both cases respectively. It is very understandable that socio-cognitive terminology must concentrate on one language and a unilingual speaker's competence at the time, given the cognitivist concentration on metaphorical concept formation. Temmerman (2000:235) explicitly recognizes English as the language of conceptualisation, and hence of 'primary term formation', in many sciences, which is of course a legitimate motivation of an individual research project. The language planning orientation of the French socioterminology also requires a basically unilingual approach. Most language planning orientations in European communities are by necessity unilingually directed, i.e. towards development of national languages within (relatively) unilingual communities, and most often they are directed towards the English dominance. In both cases, the unilingual orientation may be rooted in a particular kind of social motivation and needs analysis.

The multilingual orientation of traditional terminology, on the other hand, is also a result of a socially motivated, although different, needs analysis. This orientation is no result of a paradigmatic or epistemological choice per se, isolated from its context: Historically, the paradigmatic and philosophical choices of terminology were motivated by the type of problems to be solved, and those problems are all subsumed under the heading of 'enhancing international specialist communication'. The 'socio-critical' directions are of course right in pointing to this fact when providing explanations to obvious shortcomings in traditional theory. On the other hand, such arguments are reversible.

The concept and term analyses offered by the socioterminologists and socio-cognitivists still have not, overcome this unilingual limitation (as Temmerman admits, p. 235). It is at present not clear how the cognitive approach could be adapted to multilingual purposes, except for studying multilingual term formation as an instance of 'secondary term formation', to put it in Sager's metalanguage. Temmerman calls for research into multilingual and comparative categorization, i.e. from a

descriptive point of view (loc.cit.). If, on the other hand, socio-cognitive terminology intends to offer a long-lasting working paradigm, it must also extend its scope to practical and normative terminology work on a multilingual and supra-national level.

7   'Political' vs. 'scientific' terminology

Language prescription takes place in a number of communities and under different headings. Accusations of 'prescriptivism' are not restricted to terminology and LSP, but occur whenever normative measures are felt to be too strictly applied or too insufficiently under-pinned by scientific considerations. In some communities it is accepted that language planning should be sensitive to basic sociolinguistic tenets such as respecting variability, but this is certainly no universal tendency. (The case of Norway in contrast to Denmark is an excellent illustration that such differences exist even within a region considered as completely homogeneous when observed from the outside.)

According to Alain Rey, the French socioterminology can be considered as a scientifically motivated protest against the unbalanced political motivation of terminology in France and other Francophone communities:

> francophone terminological science has been marked by the so-cially and politically motivated official action taken in this field, even though the mission of the "socio-terminology" recently ex-pounded in France is also (or is intended as) a countermeasure to the political action that has been taken. (Rey 1996:101)

With respect to the vigorous criticism of Wüster pertaining to this school, this citation entails a suspicion that Wüster serves the purpose of secondary target, whereas the primary target is an official language policy featured by strong purist orientations and the use of legislation in the defence of language status.

The consequence of this observation would have to be that the aim, scope and efforts of French socioterminology should not be inter-preted and assessed out of context and could claim no more universality than traditional terminology. It is of course, on the other hand, of vital importance that sociolinguistic insights into language variability and variation is incorporated into a sound theory of any linguistic problem-

solving activity, and this also applies to terminology.

8   In defence of Wüster ?

By reading e.g. Wüster's Einführung it is not particularly difficult to certify that his view of synonymy is not without nuances, that a number of his works display an interest in semiotic problems, that there is a significant degree of 'semasiology' in his analysis of the 'inner forms' of terms, and above all: that his distinction of 'Ist-Norm' and 'Soll-Norm' clearly shows no neglect of the empirical and sociolinguistic underpinning of language planning. If we take into consideration the vast amount of research on terminology and corpus linguistics, e.g. as demonstrated in the TKE series, I also doubt whether the use of text corpora instead of introspection is a clear-cut parametrical difference vis-à-vis the traditional approach. To put it short: there are certain points in the 'socio-criticism' that could be ascribed to an incomplete reading of Wüster and his followers, and those points fail to hit their target.

On the other hand, the apparent cleavage of terminological directions that we witness today is perhaps also partly a consequence of the long-term insistence from terminology that linguistics and terminology are distinct and should be kept apart. As indicated above, the ambition of bridging the gap leads to an obvious question: "which linguistics", and not only: "which terminology"? The shift from structuralism to cognitivism in linguistics is a background factor that cannot be controlled or even affected by terminologists, and terminologists have to take it into account in order to communicate with the linguistic society. Approaching terminology to linguistics will obviously not make any strong appeal if terminology is not able to keep up with recent developments of linguistic theory.

Ascribing all criticism to an inadequate knowledge of Wüster's writings would, consequently, imply the risk of immunisation. The existence of the moderate position (a) has demonstrated that the need for re-thinking and clarification of the wüsterian approach is not restricted to the socioterminologists. Even by the moderate approach exemplified by Laurén, Myking and Picht (1998) it is maintained that theory-making is valuable in its own right , and that terminology is something broader than just practical problem-solving.

9   Pluralism or determinism ?

It has been customary within traditional terminology to define itself by certain contrasts or within certain dichotomies that are used to describe approaches to terminology. Within each of the following three, terminology normally identifies with the second member:
- *(a)*   Language-planning orientation vs. subject-field orientation
- *(b)*   Punctual vs. systematic investigations
- *(c)*   Semasiology vs. onomasiology

Traditional terminology has developed, according to the criticism:
−   a theoretical platform characterised by ECLECTICISM
−   a set of epistemological tenets: INDEPENDENT CONCEPTS
−   an operational method: ONOMASIOLOGY
−   a defined set of problems: STANDARDISATION.

By implication:
- *(a)*   ECLECTICISM > INDEPENDENT CONCEPTS > ONO-MASIOLOGY > STANDARDISATION

Or, inversely,
- *(b)*   STANDARDISATION > ONOMASIOLOGY > INDE-PENDENT CONCEPTS > ECLECTICISM

The socio-critical directions of terminology, on the other hand, are questioning the theoretical and epistemological platform, thereby questioning the method. Consequently, we may ask whether they are either offering an improved method for solving the same type of problems, or, maybe, whether they are aiming at solving another set of problems. Shifting emphasis to the left member of the above dichotomies might lead to a different chain of implications, however less reversible:
- *(c)*   SOCIOLINGUISTICS/COGNITIVISM > MOTIVATED SIGNS > SEMASIOLOGY/PUNCTUAL STUDIES > WHICH PROBLEMS ?

To establish such direct links from semiotic to methodological choices and from there to the selection of problems to be studied and/or solved, might apparently lead into some kind of determinism. If one member of the chain is changed, the rest, it appears, also has to be changed – implying, for example, that an onomasiological approach might not be used for purposes of language planning. Such inferences do not correspond to normal intuition. The works discussed in this paper display,

nevertheless, a strong clustering of parametrical positions, and then there are a number of vital problems to be discussed in future research:

Do ''correctionist'' directions aim at solving the same problems as does traditional terminology ? Is ''describing terminology'' a legitimate aim in itself, and is 'describing' really incompatible with 'improving' ?

Do unilingual and language planning-oriented problems in, for instance, the Francophone, Catalan and Icelandic language areas require a (more or less) different theory than traditional terminology could offer ?

What, then, about multilingual terminology at the supranational level, such as EU ? How could a cognitivist view on motivation be combined with plurilingual problem-solving, be it standardisation or the production of special dictionaries ?

My own answer is close to the traditional: 'Society' is there, and the aim of linguistic problem-solving is legitimate, justified and ever-present, just as in the case of general language lexicography or general language planning, for that matter. These facts need not imply that such objectives should be the only legitimate. A sound theory-making is essential in any case, and it is not always possible to do both at the same time. Quite clearly, for the moment there is a language barrier biasing this theory discussion, and the theoretical discussion might benefit from a less Eurocentric perspective.

I interpret Bassey Antia's approach (Antia 2000) as one such attempt. His overall aim is to provide a framework to integrate terminology in problems of multilingual language planning; he claims a pluralist approach in the fields of conceptology and ontology (pp. 81, 89); his attitude towards the use of electronic corpora is positive (157f.), and so is his answer with respect to the usefulness of an onomasiology-based conceptology (p. 84), an answer based on positions taken by Gerhard Budin. This particular discussion is on object theory, but the position taken applies to terminology as a whole:

> [...] an epistemological position for a terminological object theory must transcend the naïve realism inherent in Neo-Positivism and the solipsism epitomised by Radical Constructivism [...]. A number of intermediary positions are deemed to be more appropriate for terminology [italics by JM] [...] To give salience to these intermediary positions, or to adopt a broad epistemological outlook, is to subscribe to ontological pluralism, rather than to ontological unity (Antia 2000:89)

Such positions contradict the existence of strong determinist links between a given type of epistemological preferences and a given set of problems to be solved.

10   Concluding remarks

Several broad questions emerge from this discussion, as well as some answers.

–   Identifying traditional terminology with 'prescriptivism': is it justified ? On "loyal" reading the answer is no, but there have admittedly been some legitimate reasons for this impression.

–   Do prescriptive objectives constitute an obstacle to a sound terminology ? The main answer is no. Nevertheless, there is a little "yes", in the same way as general language planning in most communities also constantly runs the risk of neglecting sociolinguistic evidence.

–   Has traditional terminology been "cut off" from society ? The answer is of course that this question is largely based on misconceptions. The social dimension and the descriptive tasks of terminology have not always, however, been explicitly integrated and thematised in the theory of terminology, leaving terminology with the image of a purely "technical tool".

If socioterminology, for instance, is no more than 'implementation studies' (cf. Maurais), it might perhaps just as well be subsumed under another heading, be it 'language planning' or simply 'sociolinguistics'. Even if we agree that sociolinguistic and contextual perspectives need to be integrated in terminology, there is a risk that distinctive labels affect their own meaning, pushing separatism a step further than desirable. Do we, consequently, need 'socio/cognitive/terminology' as labels or concepts at all, in short: do we need new morpho-semantic designations to indicate new directions ? Or do we need nothing else but 'terminology', 'sociolinguistics' and a pluralist oriententation ?

One potential danger that has to be prevented, apparently, is that of different epistemologies and paradigmatic positions blocking practical cooperation, leading to separated networks and lack of contact between different terminological discourse communities. To prevent such problems, we have, at least, to read each others' contributions.

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METADISCOURSE IN TERMINOLOGY: THESIS, ANTITHESIS, SYNTHESIS

Introduction

This article addresses the issue of how discourse actors in terminology describe the discourses they produce and publish as research. It examines positions which, in the assessment of some actors, are diametrically opposed, then explores bases for resolution. Myking (in this volume) does an excellent job of presenting and critiquing two major directions (sharing the prefix *socio*) that are critical of the so-called traditional terminology. There is the socioterminology group associated notably with the French university city of Rouen, and the sociocognitive group represented by Temmerman. One might also mention Cabré whose own work combines the critical thrusts of the two *socio* currents reviewed by Myking.

In 1990, in an apparently less widely cited publication, Gaudin, an exponent of a strand of socioterminology, describes the goal of socioterminology as being to put the study of terminology 'back into the sciences of language, humane sciences and the social sciences.' The underlying commitment of both *socio* currents is to linguistics, with the Rouen group emphasizing the relationship between terminology and sociolinguistics through the modality of discourse analysis/corpus linguistics, and the sociocognitive current adding to the foregoing the role of language, notably figurative language, in the growth and understanding of areas of specialized subject matter.

Myking, quite appropriately, discusses the first strand from the standpoint of the dichotomy, *prescription-description*, and the second from the angle of another dichotomy, *motivation-arbitrariness of linguistic signs*. Based on linguistics and an experientialist epistemology, these two *socio* currents present themselves as alternatives to Wüsterian, traditional terminology.

The question of extent or degree of modification/replacement of the latter strand leads Myking to a 3-group typology of critical lines. On the one hand, there are the moderate and radical, yet loyal criticisms. On the other hand, we find the radical and subversive group, to which the two *socio* currents under review belong. On my reading, the pertinent questions are as follows:

1) What should dynamism in knowledge in terminology be called ? Paradigm articulation/ mopping up, or paradigm shift ?
2) What room/space for self-elaboration/expansion is granted a paradigm ?
3) What role does time play in claims of paradigm shift ? In other words, can self-elaborations of a paradigm be ignored, in order to premise the necessity for change on the paradigm's earliest formulations ?

At first glance, these are fundamental questions of the philosophy and politics of science, to the extent that description of the necessity and nature of dynamism in knowledge may be motivated by a concern with knowledge alone, or by an additional concern with intellectual dominance and supremacy.

To determine whether we are at an intellectual crossroads, and to explore what lines a synthesis might take, it is proper to analyze our two *socio* currents as an **antithesis** to a **thesis**. I will in the process repeatedly engage with Myking's argumentation. To set the stage, here are some ground-rules from the philosophy of science.

Thomas Kuhn on dynamism in knowledge

The choice of a 'revolutionary' account of dynamism in knowledge, such as proposed by Kuhn (1962/ 1996), is explained by the self-labels of, or descriptions used by, the *socio* critics: *revisionism* (Gaudin 1993)*, alternative paradigm* (Cabré 1999)*, correction of Wüster* (Rey 1996)*, substitution* (Gambier 1991), etc.

Kuhn's use of the term 'paradigm' has elicited criticism, but it will suffice here to view a paradigm as a theory. Normal science, according to Kuhn, is characterized by an articulation/elaboration of a paradigm – which would normally be 'very limited in both scope and precision at the time of its appearance' (Kuhn 1962/1996:23). What makes a paradigm successful at the time it appears is not that it is able to integrally

deal with a single problem, nor its adequacy for a large number of problems. Rather, it is the demonstrable promise (typically fulfilled later) of being able to account for a problem or range of phenomena. Most careers are devoted precisely to actualizing this promise, articulating this paradigm through confronting it with empirical data, resolving ambiguities, developing lines in which the paradigm at inception was only perfunctorily interested. (Kuhn *op. cit.*: 27). This is so because 'often a paradigm developed for one set of phenomena is ambiguous in its application to other closely related phenomena' (Kuhn *op. cit.*: 29). The foregoing is what normal science is about; it sees knowledge change as paradigm articulation.

Paradigm change or a revolution is often the outcome of abnormal or crisis-ridden science. The change occurs when an 'older paradigm is replaced in whole or in part by an incompatible new one' (Kuhn *op. cit.*: 92). The keyword here is **incompatibility**, because from the socio-political context where the word revolution comes 'revolutions aim to change institutions in ways that these institutions themselves prohibit' (Kuhn *op. cit.*: 93). Thus, the choice between two paradigms is a 'choice between incompatible modes of community life' (Kuhn *op. cit.*: 95).

Whatever disagreements he may have with Kuhn's historical account of scientific revolutions, Thagard (1992:6) would seem to concur with this incompatibility thesis when he defines conceptual changes as revolutionary only if 'they involve the replacement of a whole system of concepts and rules by a new system.'

Against the background of the following, I shall be interested in examining the bases for the revolutionary spin which the *socio* currents put on their work.

Cart before the horse for good reasons: antithesis, then thesis

The embrace of linguistics (synchrony-diachrony, sociolinguistics/corpus linguistics/discourse analysis, syntax, description as opposed to prescription, lexical semantics in the form of synonymy, etc.) and of an experientialist epistemology (as opposed to an objectivist one) would seem to give to the specific manifestations of the *socio* currents reviewed by Myking a platform upon which they distinguish themselves from Wüsterian or traditional terminology. In support of the

foregoing, I cite the following opinions to complement those cited by Myking, which I assume the reader has already seen. I begin with views related to linguistics.

Gaudin (1990, on description): 'the point is to put forward a descriptive attitude, which is the characteristic of the science of language, in order to break with an attitude of what could be compared to pre-scientific empiricism [...]. Concretely, it means working on [...] texts.'

Guespin (1990 on domains): 'Classical terminology uses an approach by 'domaines' (fields). I think this strategy is irrelevant if we take into account the actual relationships between sciences, technology and production. To establish the fact that the concept of 'domaine' (field) is not always relevant, I will take here the example of bio-technology. Indeed biotechnology is a form of encounter between 'science' and 'technology'. Perhaps a classical terminologist would grant me that the term of biotechnology must be defined as an intersection of two fields.'

Gambier (1991 on the need to study terms in discourse, the place of metaphor, etc.): 'The use of metaphor, [...] the passage of a term from one domain to another – these are processes that are frequently observed in the formulation of scientific and technical ideas. It is therefore indispensable to substitute the notion of a term as fixed and circumscribed in a definition by an approach that observes how a term functions, how it serves as a tool in the dynamic process of collective conceptualization [...].' (My translation from the French).

Cabré (1999 on reductionism associated with classical terminology): 'As a consequence of reducing terms to their denominative function, syntactic aspects of terminological units have been obviated. This obviation has prevented research on the grammatical working of terminology. The consequence of this has been the scarcity of grammatical information included in terminological databases.'

- 'Ignoring the communicative and discourse aspects of terminological units, which is also a consequence of the exclusivity that their denominative function has been granted, has prevented research in the recently developed idea that terminology can contribute to textual characterization, as far as the different types and the level of abstraction of this characterization is concerned.'

- 'Obviating formal and conceptual variation of units in special

subjects – keeping in mind that such variation is inherent to language and to both general and special communication – has generated a working method with a prescriptive basis that is assumed to be valid for all types of research, whatever the research is on and what its goals, contexts of occurrence and linguistic typology are.'

On epistemological concerns, here are some views.

Zawada & Swanepoel (1994 explaining the experiential realism hypothesis, the presumed antithesis of the epistemological position in classical terminology):
'Our concepts are not the result of our passively receiving objectively structured impressions from the outside world or of us structuring masses of raw data in terms of innate concepts.'
Temmerman (2000:16):
'Traditional terminology applies the objectivist model which is at the basis of Wüsterian thinking [...]. For objectivists reality has a rational structure which is independent of human understanding.'

In effect, we have here an antithesis supported by specific linguistic and epistemological premises. From these standpoints, the thesis is somewhat difficult to identify

In search of the thesis

A personal experience introduces this section. I began my doctoral research in terminology late 1994 in Bielefeld, Germany. My problem, soon thereafter and right up to the end of my research in 1998, was: what is the thesis to which certain *socio* currents are an antithesis, and that justifies overt and covert pleas for, and claims of, paradigm shift in the Kuhnian sense. Writings in the antithesis mindset refer to the Vienna School, *la terminologie allemande*, classical terminology, traditional terminology and, of course, Wüsterian terminology. My puzzle as a beginning student stemmed from two sources. Firstly, the observation (also made by Myking) that references to work by scholars one would associate with Vienna are typically scanty in discourse of the antithesis currents. Secondly, much of what the antithesis claimed was neglected by traditional terminology was actually being addressed by researchers who are linked to Vienna, or at least do not see their work as subver-

sive, that is, overthrowing and replacing Vienna. I begin my illustration with the latter point.

By 1994 when my doctoral research began, Heribert Picht, Gerhard Budin and others had published articles on LSP or terminological phraseology/collocations in vol.1 (1990), no. 1-2 of *Terminology Science and Research*, having organized a workshop in 1989 on the functioning of terms (grammatically or syntactically, conceptually, etc.) in discourse.

By 1994, Budin's 1992 essay on an object theory in terminology had been published. Here, Budin claims for terminology epistemological positions (Critical realism and Hypothetical realism) that are situated in between the extremes of Neo-positivism and Radical constructivism. Critical realism posits the existence of a world which sometimes differs from the way it appears to us, while Hypothetical realism postulates the existence of a real world with structures that are discoverable only to an extent.

On a related issue, by 1985 and 1989, Picht & Draskau and Arntz & Picht respectively had discussed concept characteristics, including those that are functional-relational; in other words, not innate or betraying some neopositivistic view. In publications of even date (1996) Sandrini and Mayer had been looking at contextual approaches for handling inconsistent concept relations in multilingual work on terminology in the area of comparative law.

In 1994, Wright & Budin's 1993 empirical study of data elements in terminological entries was published in *Terminology*. The survey showed, somewhat paradoxically, that it was not the documentation of concept hierarchies and the like associated with traditional terminology that was widespread in the management systems and databases examined. Rather it was the documentation of terms and term-related information, contexts, notes and administrative information of various kinds. Wright & Budin specifically say that 'very few systems make provisions for documenting concept systems as a part of the standard terminology entry or file.'

By 1993, Khurshid Ahmad and Margaret Rogers had been pioneering corpus related work, examining terms in real texts with tools developed by them (Besides the Computing Sciences Reports of their

university in Surrey, see Ahmad 1993, Ahmad & Rogers 1993). One of Rogers' interests in this respect has been with synonymic variation in LSP texts (Rogers 1995, 1997, 1999, etc.). As for Ahmad (1996, 1999), one major interest has been in concept and term evolution, in other words, diachrony – an interest shared by Budin (1996). Earlier than both Ahmad and Budin, Oeser had in 1988 already been talking about evolution of scientific concepts, and the suitability of Wüster's four-part word model, when read as a flow chart of iterative processes, for representing and managing the nature and consequences of this evolution. On the related issue of concept formation, Picht (1997) shows that there are models of formation other than the commonly known abstraction model.

By 1993 Schröder had discovered that Wüster was not just a cold-blooded onomasiologist, but that he also had a warm-hearted commitment to semasiology as evidenced by his mysteriously detailed account of sign typologies. At the latest in 1996, Christian Galinski and Heribert Picht had been articulating and elaborating this aspect of the Wüster paradigm in their writings on non-linguistic forms of knowledge representation in terminology – an interest developed in subsequent publications (e.g. Galinski & Picht 1997) and shared by Christer Laurén and Johan Myking (in Laurén, Myking & Picht 1998).

Challenges of terminology in domains that are not associated with well defined subject fields (physics, chemistry, law, etc.) were by 1993 addressed in work on interdisciplines/ paradisciplines by Ahmad & Fulford; then in 1996 by Ahmad & Salway (on safety). In 1998, Pierre Lewalle examined the related paradiscipline of chemical hazard/risk assessment. With respect to the allegation of emphasizing material/ technological fields, in other words, disinterest in the so-called soft sciences, it is worth noting that between 1979 – 1996, Riggs, Mälkiä, Budin (individually or collaboratively) studied the terminology of the social sciences. For the specific area of the law, see entries in the 300-item bibliography on legal terminology research, etc. compiled by Sandrini (1999).

Quite early in the 1990s, Khurshid Ahmad was already well on the weirdness track, using the terminological particularity of texts as basis for text classification and term extraction.

One can go on. My dilemma as a doctoral student in 1994, and

right up to 1998, was how to process claims of the antithetical *socio* currents that postdate some of the published research I have just cited from the 'thesis' current. This continues to be my dilemma to date. This is why the question was posed concerning the time factor, that is, whether self-elaborations of a paradigm should be ignored in order to premise the necessity for change on the paradigm's earliest formulations.

I do not know if the above review of thesis currents has seen me box into the Vienna corner scholars who would rather not be labeled. The earlier point about perception of knowledge dynamism or change is obviously important here. Although thesis current scholars refer to what obtains in traditional terminology, quite consistently with fine traditions of academic writing requiring a premise that borders on occupying a slighted or neglected territory, their attitude is generally not one of revisionism, revolution, *correctionism*, paradigm shift, but of paradigm articulation in a period of normal science as described by Kuhn. These scholars are the loyalists in Myking's typology.

The foregoing being my first puzzle as a beginning student, my second puzzle had to do with the source of information of *socio* currents on thesis positions. Was I reading the same research as *socio* current scholars, or did we have different readings of the same research ? Myking in his lead presentation speaks of the need to read each others' contributions. Taking Ahmad, Budin, Felber, Laurén, Picht, Rogers and Wright from the much wider pool of loyalists, I did a citation analysis to find out if they were cited by scholars who put a revisionist/ revolutionary spin on their own research. Table 1 presents my findings. Wüster is placed on the same axis as the articulators of 'his' paradigm.

	Ahmad + Rogers + Fulford	Budin	Picht + Arntz + Laurén + Draskau + Engel + Galinski	Felber + Budin + Lang/ Wersig + Rondeau	Laurén + Picht	Rogers	Wright	Wüster
Gaudin (1990) article 'Socioterminology & expert discourses'	-	-	-	-	-	-	-	1
Guespin (1990) article 'Socioterminology facing problems of standardization'	-	-	-	-	-	-	-	-
Gambier (1991) article 'Travail et vocab. Spécialisés: prolégomènes à une socioterminologie'	-	-	-	-	-	-	-	-
Gaudin (1993) article 'Socioterminologie: propos et propositions épistémologiques'	-	-	-	-	-	-	-	-
Gaudin (1993). Pour une socioterminologie. Des problèmes sémantiques aux pratiques institutionnelles.	-	-	-	1	-	-	-	1
Zawada/Swanepoel (1994) article 'On the empirical inadequacy of terminological concept theories: the case for prototype theory'	-	-	1	1	-	-	-	1
Rey (1996) book chapter. 'Beyond Terminology'	-	-	-	-	-	-	-	-
Cabré (1998) book/updated bibliography *Terminology: theory, methods & applications*	-	-	4	3	-	-	-	5
Cabré (1999) manuscript 'Elements for a theory of terminology: towards an alternative paradigm'	-	-	2	-	-	-	-	2
Temmerman (2000) book *Towards new ways of terminology description*	2	-	-	5	-	-	1	1

Table 1: Citation analysis

There are clear shortcomings of this kind of analysis. For one, people are only cited when they have a relevance for the discussion at hand. Secondly, some of the thesis scholars may not have been quite active in terminology at the time the antithesis currents were writing. Even with these admissions, Table 1 is quite instructive, not the least because criticisms of the early 1990s are a decade later still being made, with scant regard for self-elaborations of the paradigm being criticized. Of the paradigm articulators, Felber is the most cited, and is second only to Wüster. An oft-cited work of Felber's, *Terminology Manual*, contains quite a bit that makes it a punching bag. On my reading, it seems that the attitude in the *Manual* and in a few other pieces by Fel

ber differs from the one in Wüster's *Einführung*.

At any rate, given that Wüster is the most cited, I decided to establish whether his most important theoretical work, the *Einführung*, invites upon itself and its author criticisms such as are often made. Table 2 presents, in its left column, some passages from Wüster's *Einführung*, and in the right column, my interpretation. The passages have been translated from the German. All references are to the 1991 edition. The passages have been selected against the background of the antithesis criticisms (epistemology and linguistics).

P.7 Concepts as such, e.g. dog or pain, do not exist in the real world. Rather, Man lives in a world surrounded by objects that he consciously organizes, even though these objects may be more or less dependent on him.	**Wüster was not a naïve realist.**
P. 40 (see also p. 58): Some specialist expressions/terms are designations transferred from another field or another language variety. This is an application of metaphorical extension.	That sciences can live by metaphor is not a thought that was strange to Wüster.
P. 87: In terminology as in everyday language or literature, any requirement for absolute monosemy (with mononymy) is a most pious wish. This is evident from the fact that the number of concepts in a specialized area is perhaps a thousand times more than the number of available words. P. 92: Even in terminology, synonyms with differences in connotation cannot entirely be ruled out. Nonetheless, it is important to bear in mind that synonyms may be distinguished from the standpoint of the registers to which they belong, ranging from the specialized language of norms to that of the workshop.	**Synonymy is not a concept Wüster thought could be wished away from LSP.**
P. 90 A monosemous term is strictly speaking one which, in a given context, has only one, activated sense although out of this context the term may be polysemous. By context is meant either the linguistic environment or the situation of utterance. [...]. The distinction between absolute monosemy and contextual monosemy provides the basis for limiting the requirement in terminology as follows: the designation should be contextually monosemous, but need not be absolutely monosemous.	**Context–dependency of the acceptation of term tokens was not alien to Wüster.**
P. 97: Work on terminology within a given discourse community should not stop at observing how discourse works. Rather, it should aim to resolve contradictions and to improve language use. This, however, must not be at any cost. Improvement should reflect as far as possible the richness/genius of the corresponding language. Improvement is a two-step activity: research and consensus. The former is linguistic, the latter sociological.	**Description was not an alien concept to Wüster; neither were the social auspices of terminological activity.**

Table 2: Wüster quotes.

Now, even if the so-called traditional or classical school were re-ducible to one man, Wüster, the attitude underlying the views above is not one that invites to itself the kind of criticism upon which a paradigm shift can be based. In any case, it seems quite unfair for a critic to reach back in time, into the *oeuvre* of a man born in 1898 for research prem-ises, skipping in the process several important articulations/elaborations of that *oeuvre*. The critic is, in other words, refusing to engage with his or her own contemporaries (Oeser, Budin, Picht, Laurén, Rogers, Ahmad, Wright, etc.). In the current state of things, Felber and a mis-read Wüster constitute grist for criticism. With respect to the Franco-phone socioterminologists, one wonders along with Myking whether their views on the thesis derive from indirect linguistic access to pri-mary sources in German, or from a reading of the thesis through the work of the much maligned language/terminology commissions in France. This is what Myking means when he speaks of Wüster being perhaps a secondary target. Clearly, the extracts from the *Einführung* do not seem like views for which a correction of Wüster is needed, as Rey (1996) proposes to do.

Elsewhere (Antia 1999b), the suggestion has been made that there are contradictions of substance and an imbalance in emphasis in Wüster's work. (See also Gerzymisch-Arbogast 1996). My reading of the work of the *socio* currents, particularly of those representative scholars writ-ing in the early 1990s, is that theirs was an attempt to redress some of this imbalance by developing lines suggested by Wüster, but which were slighted subsequently. Either at the prompting of the *socio* cur-rents or through some internal motivation, so-called traditional termi-nology has devoted attention to these slighted dimensions, as the review of some pieces of literature shows. This development therefore under-mines any rehashing in the late 1990s (and beyond) of criticisms of the early 1990s. At any rate, whether the time frame is now or a decade ago, the incompatibility criterion for positing different paradigms ap-pears to have hardly been met. Khunian characterization therefore takes away the revolutionary/revisionist shine from *socio* currents, and puts a paradigm articulation label on their very important contributions. If any further synthesis is required, I think the foregoing sets the stage.

Synthesis

I concur with Myking when he suggests that the way forward lies

in pluralism, one that makes nonsense of any pattern of commitments (e.g. being Germanophone – onomasiology – stereotype concept theory - philosophy – prescription; or being Francophone – semasiology – prototype concept theory – linguistics – description). Myking suggests that we do not have to box ourselves into a corner where, if we do one thing, we cannot do another. Cabré (1996), even while taking a swipe at Vienna, is most conciliatory and makes the point about pluralism most eloquently. She is here quoted *in extenso*:

> Thus I am far from the rigid and monolithic conceptualization of terminology that has been extended in the central European countries. Rather I feel close to those who advocate a terminology which is diverse, adapted to a given medium and appropriate to specific goals. Further I believe that what underlies terminology is nothing else but plurality, diversity and multifunctionality. However paradoxical this may seem at first sight, behind this diversity there is unity of bases, unity of scientific object and unity of field of research: in other words, unity of discipline (Cabré 1996: 15-16).

The cause of pluralism is one which, by 1993, had already been championed by Laurén & Picht. On the basis of a parametric comparison of terminology as practiced and described in a number of axes (Canada, Prague, the then Soviet Union, the Nordic countries, and Vienna), Laurén & Picht (1993) identify different epistemological priorities/poles as far as theory is concerned, and note, with respect to the import of practice, that some of these axes are more national/ monolingual, whereas others address international/multilingual concerns. With an appropriate measure of terseness, they warn against speaking of terminology schools (or paradigms in my analysis) because such a position would imply oppositions (in my analysis, incompatibilities) not supported by reality, and that do no justice to the common goal of all terminological activities: enhancing the quality of specialized communication. At least for now, the terminology community would do well, in its rhetoric, to be mindful of Cabré's well thought-through point, and to heed Laurén & Picht's warning, both of which regrettably pre-date research that question these very positions.

The extreme compatibility of epistemologies/research agendas and the difficulty, at least for now, of successfully making revolutionary claims may be gleaned from an analysis of titles of recent theoretical discussions. In Antia (2000) the word 'alternative' in the title is used to

suggest that Language Planning oriented terminology needs to engage with the epistemic/LSP knowledge perspective, an angle which, on account of the overemphasis on social and linguistic issues, has been slighted (cf. also Antia 1999a). In a 1999 manuscript that bids fair to be published, Cabré uses 'alternative paradigm' to, among others, emphasize the social and the linguistic. In her book, Temmerman (2000) uses 'new ways' to, among other things, (re)value the linguistic (notably in its relation to cognition) and the social in terminology. These are a few of the no doubt many 'alternatives' on offer. The beauty in the proliferation of alternatives is that it allows one to question the universal validity of any single alternative, except of course a meta-alternative that accommodates alternatives of all shades and hues! This is why it amounts to holding an extremely naïve view of science to push a position as though it were some solve-all-problem formula. Such stance is unhelpful particularly because, in some cases, constructs (e.g. prototypes) taken from other fields (e.g. cognitive science) and presented as the ideal corner stones of terminological analysis do not even enjoy consensus in their 'ancestral' fields. Scholars who hold a 'prototypes-save' attitude, as Anna Wierzbicka (1996) describes such scholars, would do well to follow the discussion in the following areas:
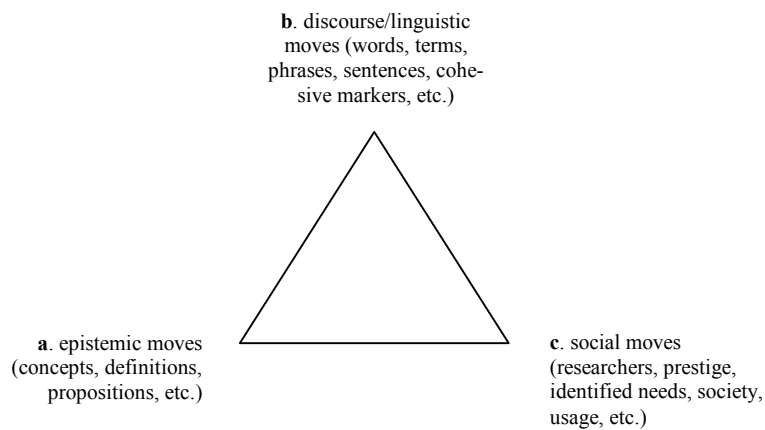
    a)   **cognitive science** [see, for example, the 80-page discussion by Laurence & Margolis (1999];

    b)   **semantics** [see, for example, Wierzbicka (1996)];

    c)   **artificial intelligence/knowledge engineering** (Sowa 1984).

In Antia (2000) is to be found a review of some of the foregoing and of others (e.g. Georges Kleiber on Lakoff), as well a presentation of directions for an all-encompassing framework.

Clearly, the question posed at the Vaasa LSP workshop as to which linguistics terminology should identify with can also be posed in respect of cognitive theories. Ultimately, there is a risk of being carried away by intellectual fads. With the recognition of a pluralist framework, a singular terminology community or network – united by the concern to enhance specialized communication – will be able to escape comparison with the personages in the anecdotal account of visually impaired persons, each of whom believed that their tactile impression of the elephant was the only and correct one.

Following in the tradition begun by Laurén & Picht (1993) and continued by Cabré (1996), I venture to give my own (alternative!)

interpretation of this complementary nature of socio-epistemological priorities in terminology. To do this, I use the ubiquitous triangle.

b. discourse/linguistic
moves (words, terms,
phrases, sentences, cohe-
sive markers, etc.)

a. epistemic moves
(concepts, definitions,
propositions, etc.)

c. social moves
(researchers, prestige,
identified needs, society,
usage, etc.)

Taking **a** as point of departure, one can explore how **a** shapes **b**,[1] and how **a** is validated or shaped by **c**.[2] One can start from **b**, and explore how **b** shapes **a**[3] and is conditioned by **c**.[4] One can start from **c**, and be interested in how communal production of knowledge (at **a**) reflects competition among scholars, or in the evolution of both **a** and **b**, etc.

1. *E.g. what features of a concept are used in term formation, or how systems of terms reflect systems of concepts).*
2. *E.g. circumstances leading to formation of a concept, acceptance or rejection of that concept).*
3. *E.g. in a process of reverse motivation in which a concept at angle* ***a*** *comes into existence through inspiration from existing terms; or a situation where concerns for collocation, etc. determine what a concept from* ***a*** *is called); or in a case where one is interested in using terms for the historical reconstruction of relationship between disciplines .*
4. *Who uses which of several synonymous terms and why ? Etc.*

In support of a synthesis that is defined for now not as *coincidentia oppositorium* but as paradigm articulation, it is important as Myking also suggests to read each others' contributions. A citation analysis of

research by thesis *aficionados* just might reveal comparable conditions of *in*-citing and insularity than those of the antithesis framework.

The terminology community may consider setting up a literature database in which entries (i.e. pieces of research) are annotated. In Antia (1999b) a plea was made for an annotated version of Wüster's *Einführung*. This bears repeating. Meanwhile, it is gratifying that English and French translations of Wüster's *Einführung* will soon go to press. Finally, the cause of synthesis may be further served, particularly in consideration of beginning students of the subject or of outsiders, by a compendium of terminology that is in the fashion of the encyclopedias of translation studies (Routledge [English], Stauffenberg [German]) or the Walter de Gruyter International Handbooks. Contributors to such a volume, prior to its publication, should be invited to read and comment on each others' contributions. In that way, in-house differences of perception can be thrashed out, so that substantive differences stand out.

Acknowledgments

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FROM PRECISE TERMS TO FUZZY WORDS - FROM BAD TO WORSE IN TERMINOLOGY SCIENCE ?

The purpose of this paper is to discuss some theoretical factors in connection with the criticism that has been presented against the traditional wüsterian terminology. (For the criticism, see Myking's article in this volume.) Temmerman, among others, accuses the traditional terminological schools for converting wishes to reality by raising principles of practical terminological work to the level of scientific facts. In other words, traditional terminologists, according to Temmerman's argumentation, only pretend to be scientists since they see the theory of terminology through the glasses of standardization work. (Temmerman 2000: 15.)

Temmerman offers an alternative approach to terminology, socio-cognitive terminology that rejects the concept-oriented approach and, instead, sees terms as the basic elements of understanding. Temmerman's argument for this is that terms can only be studied in discourse and for this reason terms should be chosen as fundamental elements for all terminological work including scientific analyzes. Terms, according to Temmerman, construct knowledge since they link new understanding to previous understanding. Terms, or words, are connected to cognitive models, such as category structures and prototypes. All these structures are more or less fuzzy. They evolve and change during times. Additionally, individuals have different cognitive models. All this results in synonymy and polysemy on the terminological level. (Temmerman 2000: 222-228)

Terminology science is accused for being normative and prescriptive. The practical terminological work, carried out by the terminological authorities in the context of standardization, naturally has prescriptive goals. Admittedly, the need for this kind of activity exists in society. Temmerman (2000: 15) sees, however, standardization as the obstacle for the creation of theoretical scientific framework of terminol-

ogy because the ideal of univocity is, according to her, taken for granted instead of focusing on problems of polysemy and synonymy that represent the prevailing practice.

The most important idea that cognitive terminology offers us is, probably, the wide-open attitude towards the fuzziness of meanings of words. 'Word' is the word that should be used here instead of 'term', because 'terms' no longer exist in cognitive terminology in the same sense as in traditional terminology. (For the traditional definition of the concept of term, see e.g. Arntz & Picht 1989: 37-41.) In cognitive terminology, terms are words and words, in turn, are always used in discourse where they represent the units of understanding instead of concepts. Since the contexts in which words are used differ, the meanings of the words are, accordingly, more or less fuzzy. (Temmerman 2000: 223-225.)

Undoubtedly, the theory of terminology is deeply rooted in the structuralistic paradigm and through its history and practice it is also connected with positivism and behaviorism. But the question is, why are 'structuralism' or 'behaviorism' or 'positivism' so outdated and dirty words and why, at the same time, everything that starts with the word 'cognitive' seems to refer to accepted scientific theories and methods ?

At the moment, it seems that we are witnessing a struggle between behavioristic and cognitive paradigms, in terminology. Likewise, we could say that there is a struggle between positivism and hermeneutics. However, the same struggle has existed for many years before this in other disciplines, especially in psychology and social sciences. Lately, the waves have settled down and also the positivistic and behavioristic paradigms have regained some understanding and acceptance even though the cognitive paradigm is the prevailing paradigm at the moment. (E.g. Selander 1992; Töttö 2000.) One of the basic ideas of behaviorism is the idea of control and the acceptance of the ability to control. Control, a negative word perhaps for most of us, was, however, used in a positive sense. The main idea was to make positive changes in people's behavior. (Skinner 1974.) The word 'cognitive' refers to mental processes that are connected with understanding, formulation of beliefs and thoughts, and acquisition of knowledge. The cognitive paradigm has its roots in the development of computers and in the analogies between human thinking and processes in the computer memory. In order to make the functions in computers more similar to the human

brain, the human cognitive processes had to be studied in detail. (See e.g. Searle 1994.) The basic idea behind this, and the same applies to positivism, was the seeking for effectiveness and profits in the society and business. If computers could "think" in the same way as humans think, the expensive manpower could be replaced by machines. However, there was a problem, and there still is. That is the fuzziness of human thinking. So far, the fact is that the fuzziness can only be reduced with the help of more or less behavioristic actions.

In terminology science, we have accepted – and probably still accept - the use of control in a positive sense. The use of terms should, at least in some contexts, be controlled in order to get positive results. Should we also accept the fuzziness of words and thinking, or do we accept it already ? In case there is a need to control, there must also be fuzziness on the other side of the coin. Fuzziness of words and thinking in one language is difficult enough, and fuzziness in a multilingual context is much worse. Undoubtedly, there is a need for terminology but for what kind of terminology ? Is it only normative terminology that we need ?

The theory of terminology is a scientific field, which, at the moment, seems to be in a state of rapid diversification. Are sociocognitive terminology and cognitive linguistics in general the magic frameworks that would solve all problems and would gather all terminologists back under one umbrella of scientific thinking ? The answer is probably negative. Sociocognitive terminology is according to Temmerman (2000: 236) an experimental model that combines different theories and methods of which cognitive linguistics seems to be the most important. Cognitive linguistics, in turn, is by no means a single and coherent theory. It is rather a paradigm within linguistics with a large number of different theories and methods. At the same time, it is also one part of cognitive sciences that include such disciplines as cognitive psychology, computational intelligence and neurosciences[1]. Entering the cognitive paradigm would probably mean that the theoretical foundations of terminology science should be totally revised.

Perhaps terminology science now needs a terminological theory in the classical meaning to put some order in the fuzzy words and concepts of its own theory. We need some more profound discussions on

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1 (<http://cognet.mit.edu/MITECS/Entry/vanhoek> 16.08.2001)

what the real aims and research objects of terminology science are. If they are the same as e.g. in discourse analysis in unilingual contexts, we can ask ourselves, why and for what purpose we need the theory of terminology. In any case, the overlapping areas of the different traditions and paradigms should be considered more carefully. On one hand, discussions like this prove the necessity of terminology. On the other hand, they prove that we seem to share the positive ideal of making things better.

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