Christian M. I. M. Matthiessen
Bo Wang
Yuanyi Ma
Isaac N. Mwinlaaru

Systemic Functional Insights on Language and Linguistics

Springer

The M.A.K. Halliday Library Functional Linguistics Series

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This series focuses on studies concerning the theory and application of Systemic Functional Linguistics. It bears the name of Professor M.A.K. Halliday, as he is generally regarded as the founder of this school of linguistic thought. The series covers studies on language and context, functional grammar, semantic variation, discourse analysis, multimodality, register and genre analysis, educational linguistics and other areas. Systemic Functional Linguistics is a functional model of language inspired by the work of linguists such as Saussure, Hjelmslev, Whorf, and Firth. The theory was initially developed by Professor M.A.K. Halliday and his colleagues in London during the 1960s, and since 1974 it has held an international congress every year at various continents around the world. It is well-known for its application in a variety of fields, including education, translation, computational linguistics, multimodal studies, and healthcare, and scholars are always exploring new areas of application.

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Yuanyi Ma · Isaac N. Mwinlaaru

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Christian M. I. M. Matthiessen
School of International Studies
University of International Business
and Economics
Beijing, China

Yuanyi Ma
School of International Cooperation
Guangdong Polytechnic of Science
and Technology
Zhuhai, China

Bo Wang
School of Translation Studies
Jinan University
Zhuhai, China

Isaac N. Mwinlaaru
Department of English
University of Cape Coast
Cape Coast, Ghana

Foreword

As one of Halliday's closest collaborators, Christian Matthiessen has made significant contributions to the development of Systemic Functional Linguistics (SFL). Since he first became acquainted with Halliday's work as a university student, he has devoted himself to the study and development of SFL theory, extended the scope of its applications and mentored students and junior scholars in many parts of the world. This collection of interviews with Christian Matthiessen brings us closer to his life and work, as well as his reflections and insights on a range of theoretical and appliable issues.

This volume, which consists of ten interviews, takes the reader through Matthiessen's life and work in different parts of the world to key conceptual issues in SFL and its application in computational linguistics, cognitive linguistics, language typology and translation studies. In the first part, Personal Histories, the interviewers focus on questions relating to Matthiessen's early interest in SFL, the connections and interactions between SFL and other schools of linguistics in the European and American traditions, most notably the Prague School and West Coast Functionalism. Through recounts of his experiences studying and working in Europe, the US, Australia and Hong Kong SAR, China, Matthiessen highlights the distinctive features of SFL and its contributions to our understanding of language. For me, it is particularly interesting to read his account of how he first became fascinated by Halliday's publications and decided to work on his mini-thesis *Hallidayan Linguistics* at Lund University, his very first meeting with Halliday at Stanford University, and then travelling every day for several hours from Los Angeles to attend Halliday's lectures at UC Irvine, and so on. I believe this excitement of "things finally clicked" on first encountering Halliday's work resonates with many of us working with SFL.

In the second part, Systemic Functional Linguistics and Its Applications, Matthiessen discusses some key conceptual issues in SFL by explaining the meanings of "systemic" and "functional", the different phases of development of SFL, the contributions of SFL to computational linguistics and text generation, the meaning-based approach to cognitive linguistics, language typology and description informed by SFL and translation studies. Apart from being an outstanding grammarian, who collaborated with Halliday in revising the *Introduction to Functional Grammar*,

Matthiessen has always been trying to enrich and develop SFL theory. In exploring the connections with other areas of research, Matthiessen emphasizes all along the importance of focussing on language and using SFL as a key resource. He argues, for example, that the only way to explain cognition is "by reference to language and other semiotic systems". He also urges people working on translation studies to engage with language or language in context, which is "the most central phenomenon in translation".

As Matthiessen once commented elsewhere, Halliday's assumptions about language were "wide-ranging and proactive in nature", and the significance of many of these remain to be further explored. I very much agree with Matthiessen on the importance of encouraging team-based research and enabling teamwork for the healthy and sustainable development of SFL.

The interviews are well structured with carefully prepared questions to bring out Matthiessen's thinking on a broad range of issues. The engagingly dialogic style of this volume makes it much more accessible than Matthiessen's other publications. For researchers, postgraduates and undergraduate students working in SFL, computation linguistics, cognitive linguistics, language typology and translation studies, these interviews will make essential reading. I feel privileged to be among the first readers of this volume, and I congratulate Christian Matthiessen and the three interviewers, Bo Wang, Yuanyi Ma and Isaac N. Mwinlaaru, on successfully putting this collection of interviews together.

As series editor of the M.A.K. Halliday Library Functional Linguistics Series, I am very pleased to have this welcome addition included, and I believe readers will enjoy reading it.

July 2020   Chenguang Chang
                                                                              Sun Yat-sen University
                                                                                Guangzhou, China

Preface

Our project of interviewing Christian Matthiessen started on September 30, 2016, in his office. By then, the three of us—Bo Wang, Yuanyi Ma and Isaac Mwinlaaru—had not yet graduated from The Hong Kong Polytechnic University (PolyU). As doctoral students supervised by him, we have benefited tremendously from his lectures, face-to-face discussions and many insightful email exchanges. The idea of interviewing him sprung out of these interactions. They started as informal personal and intellectual conversations in his office at the Department of English in PolyU and, along the line, we decided to turn this into semi-formal interview sessions that could provide other researchers and students a chance of semiotic engagement with Christian Matthiessen on his ideas about language and linguistics.

We did not make much preparation. All we did was to brainstorm a list of questions, to bring our old camera purchased more than ten years ago, and to order a brand-new tripod from an online shop. We met him weekly throughout the semesters since 2016. By the end of 2017, we had graduated and Isaac Mwinlaaru had returned to Ghana. Bo Wang and Yuanyi Ma continued with the exchanges during their weekly visits to Hong Kong from Zhuhai to attend Christian Matthiessen's postgraduate lectures. Our collection of interview videos gradually expanded. So far, we have conducted 39 interviews with him, covering a wide range of topics. For this book, we select ten interviews from our inventory, including (i) some background information on Christian Matthiessen's life and work, (ii) his interpretation of Systemic Functional Linguistics (SFL) and (iii) his discussions on the applications of SFL. Many of the topics covered here reflect our individual research interest areas and biases. We also plan to upload the videos on YouTube and Youku. You may search for "Interviews with Christian Matthiessen" at the two websites to watch the videos.

We should also mention that some of the interviews have previously been published in journals. Chapter 1 on Christian Matthiessen's early interest in SFL was recast as a short report and published in *Functional Linguistics.* Chapters 8–10 have also been published in *Linguistics and the Human Sciences.* In preparing the interview transcripts for this book, we have invited Christian Matthiessen to revise and reword some of the content to make them more reader friendly, including the addition of figures and tables to clarify and extend some issues in the discussion.

There has been other back and forth editing between him and the three of us. Thus, the chapters embody characteristics of both spoken and written language in some parts.

We would like to thank Professor Huang Guowen and Professor Chang Chenguang for kindly including this book in their book series. We are also grateful to Rebecca Zhu, Carolyn Zhang and Vidyaa Shri Krishna Kumar from Springer for their generous help. We thank our friends and colleagues who helped us during the process of writing this book, including Dr. Pattama Patpong, Prof. Juliane House, Prof. Peter Fries, Prof. Fang Yan, Dr. Abhishek Kumar Kashyap, Dr. Mark Nartey and Dr. Zhang Yanan.

We thank Equinox Publishing for permission to reprint the three interviews previously published in *Linguistics and the Human Sciences.*

Zhuhai, China   Bo Wang
Zhuhai, China   Yuanyi Ma
Cape Coast, Ghana   Isaac N. Mwinlaaru
July 2021

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About the Authors

Christian M. I. M. Matthiessen is Distinguished Professor, University of International Business and Economics. He has degrees in linguistics from Lund University (BA), where he also studied English, Arabic and philosophy, and in linguistics from UCLA (MA, Ph.D.), and has previously held positions at USC/Information Sciences Institute, Sydney University, Macquarie University, and the Hong Kong Polytechnic University. He has held visiting appointments at, e.g. the University of Hamburg and the Brain Science Division of the RIKEN Institute in Tokyo. He is Honorary Professor, Beijing Normal University, Beijing, the Australian National University, Canberra, and Guest Professor, University of Science and Technology, Beijing.

Matthiessen has been involved in major text-based research projects informed by Systemic Functional Linguistics since 1980, starting with the Penman project at USC/Information Sciences Institute, which produced a large-scale systemic functional grammar of English (the Nigel grammar). His research has covered a wide range of areas (all informed by Systemic Functional Linguistics), including analysis of many kinds of discourse, corpus compilation and corpus-based studies, register analysis and context-based text typology, the development of Rhetorical Structure Theory (jointly with Bill Mann and Sandy Thompson), the description of English and other languages spoken around the world, language typology and comparison, translation studies, multisemiotic studies, institutional linguistics, computational linguistics, the evolution of language, and systemic functional theory.

Matthiessen has authored and co-authored over 150 book chapters and journal articles. His books include *Text Generation and Systemic-functional Linguistics: Experiences from Japanese and English* (with John Bateman, 1991), *Lexicogrammatical Cartography: English Systems* (1995), *Working with Functional Grammar* (with J. R. Martin and Clare Painter 1997), *Construing Experience: A Language-based Approach to Cognition* (with M. A. K. Halliday 1999), *Halliday's Introduction to Functional Grammar* (revised version of Halliday's book, with M. A. K. Halliday 2014), *Functional Typology* (edited, with Alice Caffarel & J. R. Martin 2004), *Continuing Discourse on Language* (edited, with Ruqaiya Hasan and Jonathan Webster, 2005 and 2007), *Systemic Functional Grammar: A First Step into the Theory* ([in English and Chinese] with M. A. K. Halliday, 2009, with an introduction by Huang

Guowen), *Key Terms in Systemic Functional Linguistics* (with Kazuhiro Teruya and Marvin Lam, 2010), *Deploying Functional Grammar* (with J. R. Martin and Clare Painter 2010), *"System" in Systemic Functional Linguistics: A System-based Theory of Language* (in press, with Equinox), *The Texture of Casual Conversation* (with Diana Slade, forthcoming, with Equinox), *A Guide to Systemic Functional Linguistics* (with Kazuhiro Teruya, forthcoming with Routledge), *Rhetorical System and Structure Theory: The Semantic System of Rhetorical Relations* (forthcoming), *The Architecture of Language According to Systemic Functional Linguistics* (forthcoming), and *Systemic Functional Linguistics, Part I* (2021), Volume 1 of *The Collected Works of Christian M. I. M. Matthiessen*, in 8 volumes with Equinox, edited by Kazuhiro Teruya and team.

Bo Wang and Yuanyi Ma received their doctoral degrees from the Hong Kong Polytechnic University. Their research interests include Systemic Functional Linguistics, translation studies, discourse analysis and language description. They are co-authors of *Lao She's Teahouse and Its Two English Translations: Exploring Chinese Drama Translation with Systemic Functional Linguistics* (Routledge, 2020), *Systemic Functional Translation Studies: Theoretical Insights and New Directions* (Equinox, 2021), *Translating Tagore's Stray Birds into Chinese: Applying systemic functional linguistics to Chinese poetry translation* (Routledge, 2021) and *Introducing M.A.K. Halliday* (Routledge, 2022). Bo Wang is Lecturer at the School of Translation Studies, Jinan University, China. Yuanyi Ma is Lecturer at the School of International Cooperation, Guangdong Polytechnic of Science and Technology, China.

Isaac N. Mwinlaaru holds a Ph.D. from The Hong Kong Polytechnic University and is Senior Lecturer at the University of Cape Coast, Ghana. His research areas include systemic functional linguistics, language description and typology, grammaticalization, literary stylistics, and English for Specific Purposes. He has published in internationally recognized journals such as *Functions of Language*, *Language Sciences*, *Journal of Pragmatics*, *Corpora*, *Journal of English for Academic Purposes, Ibérica*, and *Research in African Literatures*. He is a contributor to *The Routledge Handbook of African Linguistics* (2018) and co-editor of *Approaches to Specialized Genres* (Routledge, 2021).

Abbreviations

AAAL	American Association for Applied Linguistics
AI	Artificial intelligence
AILA	Association Internationale de Linguistique Appliquée (International Association of Applied Linguistics)
AMH	Anatomically Modern Human
ANU	Australian National University
ASFLA	Australian Systemic Functional Linguistics Association
ATN	Augmented Transition Network
CA	Conversation Analysis
DALS	Doctor of Applied Language Sciences
DDG	Daughter Dependency Grammar
DTS	Descriptive Translation Studies
ESFLC	European Systemic Functional Linguistics Conference
ESL	English as a second language
FG	Functional grammar
FUG	Functional Unification Grammar
HPSG	Head-Driven Phrase Structure Grammar
IFG	*Introduction to Functional Grammar*
ISFC	International Systemic Functional Congress
ISI	Information Sciences Institute
KPML	Komet-Penman Multilingual
LA	Los Angeles
LACUS	Linguistic Association of Canada and the United States
LASS	*Language as Social Semiotic*
LexiCart	*Lexicogrammatical Cartography*
LFG	Lexical Functional Grammar
LMH	Linguistically Modern Human
LSA	Linguistic Society of America
MSA	Modern Standard Arabic
MT	Machine translation
PolyU	The Hong Kong Polytechnic University

RRG	Role and Reference Grammar
RST	Rhetorical Structure Theory
RTN	Recursive transition network
SFG	Systemic Functional Grammar
SFL	Systemic Functional Linguistics
SIL	Summer Institute of Linguistics
TAG	Tree Adjoining Grammar
TPR	Translation process research
UCB	University of California Berkeley
UCLA	University of California Los Angeles
UG	Universal Grammar
UNSW	University of New South Wales
USC	University of Southern California
UTS	University of Technology Sydney
WALS	The World Atlas of Language Structures

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Part I
Personal History

Chapter 1
The Way into Systemic Functional Linguistics

Abstract This chapter discusses Christian Matthiessen's early experience in linguistics and his motivations for working with Systemic Functional Linguistics (SFL). It also sheds light on the interaction between SFL and other schools of linguistics in the European and American traditions, and indicates the distinctive contributions of SFL to linguistics.

1.1 Introduction

This first chapter begins a series of interviews probing Christian Matthiessen's experiences of linguistics from the 1970s to the early 2000s, focussing on the developments of Systemic Functional Linguistics (SFL) within the ecology of the metalinguistic landscape of the period. The chapter provides a personal angle on contemporary developments in linguistics. It commences with Christian Matthiessen's reflection on his early interest in language and his encounter with SFL in the 1970s. Notably, it discusses the nature of training and scholarship in linguistic science in Europe and the US in the 1970s and the 1980s and the contributions made to different aspects of linguistics by several scholars. Beyond the personal histories, this chapter also reflects on the distinctive characteristics of SFL, such as the paradigmatic orientation in the theory of language as a resource for making meaning, as well as the interaction between ideas in SFL and other functional approaches to language (e.g., Tagmemics, Glossematics and the Prague School of Linguistics).

1.2 Christian Matthiessen's Early Interest in Systemic Functional Linguistics

Isaac Mwinlaaru: What motivated you to move into Systemic Functional Linguistics? How did you get to know about SFL, and why do you have an interest in this linguistic tradition and in pursuing this kind of research?

Christian Matthiessen: It is interesting to be given the opportunity to think back to the time when I first became aware of SFL and realized how central it was to my interests. This of course takes us back to the 1970s, which was a very different period of time in linguistics from now. One of the extraordinary differences is precisely the Internet and the technology that enables us to rapidly find out about a field. That was not possible at all in those days unless you had special connections, which was partly how I got into SFL.

My way into linguistics came early, when I was still in my mid-teens: by the time I started high school (Nicolaiskolan in Helsingborg[1]), I had become interested in descriptions of language.[2] In high school, I liked reading more in-depth material in various subjects beyond what the curriculum required in terms of textbooks. This nudged me in the direction of linguistics even though my strand in high school was mathematics and natural sciences. On the one hand, I found that in physics, chemistry, mathematics, going beyond the textbooks was very hard, because I couldn't find the kind of intermediate material just a bit beyond the high school curriculum that would have been accessible to me; whereas with grammar, it was possible. But then, on the other hand, I was very dissatisfied with the kinds of grammar we were provided with in high school. I thought they were not systematic and not explanatory. That was another reason why I was prompted to read around. So I discovered certain linguistics books like Otto Jespersen's work on grammar (e.g., Jespersen 1924, 1933a) and also an introduction to Generative Semantics by a Swedish linguist, Alvar Ellegård (1971), whose book was very interesting because he took the aspects of grammar that were presented as arbitrary in traditional accounts, and explained them in terms of pseudo-semantic structures, drawing partly on predicate logic, which I learned about in another subject in high school—philosophy. I was lucky to have a very dedicated and enthusiastic philosophy teacher; he took us beyond the textbook, e.g., giving us a more in-depth account of propositional calculus and predicate logic and also introducing us to George von Wright's work, which developed some strands of his teacher Wittgenstein's insights.[3] His deep engagement with philosophy was one of the reasons I went on to study it later at Lund University. In psychology, we

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[1] https://nicolaiskolan.helsingborg.se/skolan/.

[2] Apart from my multilingual family background, this was, to a large extent, motivated by my German teacher in years 7 through 9, Birgitt Kronzell. As luck would have it, I had another brilliant teacher of German in high school (and of Swedish), Helge Jahn.

[3] I think we were given excerpts from von Wright's book *Logik, filosofi och språk* (Logic, philosophy and language). Our teacher emphasized the significance of the fact that von Wright had written philosophical works in Swedish (although he was Finnish). Sweden did not stand out for its contributions to the history of philosophy. Later, when I studied philosophy at Lund University, I was told

also learned about meaning—interpersonal meaning; our teacher introduced us to Osgood's semantic differentials as a method of studying connotative meaning (e.g., Osgood 1964; Osgood et al. 1957), which in a way primed me for the work by Jim Martin and others in SFL on appraisal.

One day, our psychology teacher told us he would have a reunion with his classmates from high school and asked us if we would be happy to meet them and chat with them—I think it must have been the 30th anniversary of their graduation. We all agreed, and when the day came, I was completely taken by surprise to see my half-brother Tryggve Emond[4] join the group. I had not known that he and my psychology teacher had been classmates! Tryggve and I didn't grow up together; he was born in the same year as Michael Halliday, and passed away one year before him. Tryggve also influenced my interest in philosophy; he had done a PhD thesis in aesthetics (Emond 1964), *On Art and Unity*, getting help from our father, Martin Emond, who was an artist. When I was old enough to appreciate it, he gave me a copy of his thesis; and his insights primed me for the later development of multimodal studies in SFL. One day, I hope I'll be able to translate some of his key insights into the systemic functional approach to visual art. Tryggve inherited our father's visual artistic potential (or PP, "painter potential"), unlike me—as far as I can tell; and he sketched and painted throughout his life, having his first public exhibition when he was 70. Tryggve's academic field, aesthetics, was considered part of philosophy, but, unluckily for him, no academic position in this area ever materialized in Sweden, and he worked as a lecturer at a high school in Lund, teaching philosophy, English and French. After retiring, he co-authored a textbook in philosophy for high school students, *Vad är filosofi* (What is Philosophy). It's excellent, and if I ever get the time, I'd like to translate it into English—after attempting to translate Vilhelm Moberg's play *Din stund på Jorden* ("Your time [or moment] on earth"), which I think should be part of the canon of world literature in the sense of the treasure trove of our extended human family's cumulative experience and wisdom. Speaking of translation (see Sect. 8.4), Tryggve and his family were again a source of insight, since three of them were very active in translation from English, French, Italian and Japanese into Swedish (cf. Matthiessen 2001).

But in any case, returning from this tangent, when I began to see grammar as essentially a construction of meaning, thanks in large part to Ellegård's work—at the time, still in syntagmatic terms—I was seriously impressed. Around the same time, I came across a book in Swedish (but translated into English as *New Trends of Linguistics*) by Bertil Malmberg (1969), who was the professor of linguistics at Lund University at the time. His book included a very interesting chapter on the European structuralist tradition—Saussure, Jakobson, Hjelmslev and European structuralism

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that Sweden's only "contribution" to European philosophy was due to Queen Kristina: an enlightened scholarly monarch, she had invited Descartes to Stockholm, but her palace proved too cold and draughty for him during winter, and he died of pneumonia. She is said not to have cared for his kind of philosophy.

[4] https://sv.wikipedia.org/wiki/Tryggve_Emond.

in general. That, to me, also seemed fascinating. I came to understand and appreciate the notion of the Saussurean sign and also the axial differentiation between syntagmatic and paradigmatic patterns. From Ellegård, I had the insight into the semantic underpinnings of syntagmatic grammatical patterns—into syntactic structures, including negatives as higher predicates and other analyses based on Generative Semantics. Then from Malmberg, I gained access to the European insights, which seemed very useful in the study of phonology (like Nikolai Trubetzkoy 1939a, b), and of lexical semantics (Jost Trier's 1931 field theory). But to me, these strands of insights coming from different traditions seemed incompatible; I could not see how they could be related to one another since I had no map of the overall territory of linguistics. Then, when I had to do an awful eleven months of military service in the Swedish air force as a guard soldier protecting a secret radar monitoring installation in the middle of nowhere, I entered university through a correspondence course. What kept me intellectually alive was this correspondence course in English linguistics,[5] and then I got to do a bit more reading in linguistics, including John Lyons' (1968) *Theoretical Linguistics*—fascinating to me, but not exactly an accessible reading to someone fresh out of high school. But in any case, even when I arrived in the Department of Linguistics at the university, I still had the sense that I could not put these two insights together, i.e., the **syntagmatic** and **paradigmatic** insights.

Once I got into linguistics at the university, I started doing both English linguistics and General linguistics, and I also studied philosophy and began to learn Arabic in the Department of Oriental Languages. In linguistics in our Department and also more generally, the prevailing approach at the time was Chomsky's (Extended) Standard Theory, so the basic textbook for syntax was Akmajian and Heny (1975), a thick book introducing this theory, and it included little examples of how to write rules for tag questions, passive formation and so on. But we were encouraged to read around, and the Department was not dogmatic at all. Our Linguistics Department was housed in the former villa of the Rector Magnificus—the president or vice chancellor of the university. So, it was really like a home, with a large garden; and even as an undergraduate student, I felt included in a kind of academic family, which I think was helped by the fact that a number of our teachers were also PhD students, who shared their research topics and enthusiasm with us—Sven Platzack, Kenneth Hyltenstam, Christopher Stroud and Eva Larsson. I was invited to take part in a regular seminar called "diskuteket", where researchers presented on a variety of topics; and I presented at two of them, one presentation being an overview of linguistic approaches to the analysis and modelling of negation.[6] What used to be the huge formal dining room in the villa had been turned into a library—quite an extensive one. Even when I was an undergraduate student, I was allowed to stay there for hours, and I would just browse and browse late into the evening. That was

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[5] And also, our wonderful Alsatian guard dogs—a great sort of comfort as I traipsed around the spooky wilderness around the installation at night, going from one check point to another.

[6] One of the participants, Sven Platzack's brother Christer Platzack, who was already an established academic, said to me after my negation presentation that it was "djävla bra", literally "devilishly good"—high praise indeed in Swedish, at least at that time.

the equivalent of Googling topics, walking around the library and looking things up. As I said, we were encouraged to read around, so I read different approaches to language, different theories, and became familiar with Chomsky's theory. I was very disappointed because it had nothing to do with meaning. What I discovered later was that the version of Transformational Grammar I had met in Ellegård's book while I was still in high school was Generative Semantics, not Chomsky's version. In the library, I also came across other traditions, including Stratificational Linguistics, which seemed visually intriguing, and Tagmemics. We had a PhD student, Milan Bílý, who was from Czechoslovakia and represented the Prague School.

It was when I came across some writings by Michael Halliday that things finally clicked! The first work I came across was his collection of papers called *Explorations in the Functions of Language* (Halliday 1973), a very recent publication, when I encountered it. One of the things that clicked was that I suddenly saw the connection between the **European structuralist** insight into the differentiation of the paradigmatic and syntagmatic axes, and the ability to describe structure in a semantically transparent way. The connection was provided by Halliday's **system network** representing paradigmatic organization and the associated **realization statements** specifying (fragments of) structure, representing patterns along the syntagmatic axis in functional terms. I suppose I had a visual orientation, so the system network was very appealing to me. I sensed that I could map something out in terms of the paradigmatic organization. At the time, the network metaphor was nowhere near as prevalent as it is today as a model of organization (also now in the emergent discipline of network science, as in Barabási 2016); we were still largely in the era of trees, i.e., tree diagrams.[7] But for me, the system network was a revelation—a gateway to a greater understanding of the organization of language. In hindsight, this is of course an indication of how helpful "multimodal" accounts of linguistic theory and description can be.

In addition, there were other nudges and pushes. At the time, in the Department of English of Lund University, there were two professors, one in English linguistics and the other in literature—the one in English linguistics had arrived fairly recently. For decades, English at Lund had been very well-known for historical studies of English—in particular, place name studies, which went back to the 1920s. But the new professor of the English language, **Jan Svartvik**, was a corpus linguist. He was famous as one of the team members of the grammars produced by Randolph Quirk and his team drawing on the **Survey of English Usage**, the major achievement being Quirk et al. (1985). In addition, Quirk and Svartvik collaborated on the development of the London–Lund corpus—the first extensive corpus of spoken English, which had been recorded by Randolph Quirk in London, and was given to Jan Svartvik and his team at Lund University to transcribe. Various PhD students were involved

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[7] Tree diagrams followed the tradition of representing syntagmatic composition vertically, with the "root" at the top (although tree diagrams were not used for a long time in the American Structuralist tradition, as noted and discussed by Seuren 1998). In contrast, system networks represent paradigmatic organization horizontally, laid out from left to right. Years later, Michael Halliday told me that he had, naturally, chosen this rotation intentionally to contrast the paradigmatic representation of organization with the established syntagmatic representation.

in this—I remember visiting them in an office created for the project in the old part of the university hospital, and they were also doing research based on this unique new corpus, published as Svartvik and Quirk (1980). One of the sources for this research into spoken English was Michael Halliday and Ruqaiya Hasan's (1976) book *Cohesion in English*. Through their book, which really had an enormous impact since it provided researchers with a comprehensive resource for analysing patterns of cohesion in text, I also came across another aspect of SFL and the way it enabled the linking up of insights into grammar, discourse and discourse patterns. It was quite an exciting time.

Then, in 1978, Halliday (1978) published another collection of papers transformed into a book—*LASS* (*Language as Social Semiotic*), and its appearance was very timely for me. As students in the Linguistics Department, we were encouraged to undertake and write up a project. I chose to do SFL, and I wrote a mini-thesis called **Hallidayan Linguistics**. To me, this was a fascinating undertaking involving a great deal of detective work, piecing together an account of SFL from a variety of sources because there were no overviews available. (At some point, I did get access to Margaret Berry's (1975, 1977) two valuable volumes of *An Introduction to Systemic Linguistics*; but she had produced an overview essentially of scale-&-category theory, so I was struggling to relate her account to what I had read about the more recent developments—the concept of the **meaning potential** and organization in terms of **system networks** and the theory of **metafunctions**.) So I tried to do this in my account of *Hallidayan Linguistics*.

That was really the foundation, and I took it with me when I got the scholarship to study linguistics for a year at UCLA. Linguistics at UCLA was a totally different environment from linguistics at Lund University—no SFL at all, but certainly generative linguistics and early "**West Coast Functionalism**", and very strong in **African linguistics**. That was exciting to me because I was very keen to learn more about various languages and to gain more insights into **language typology**. (During my first year at UCLA, 1979/80, I took a course with Bill Welmers that provided an overview of languages spoken in Africa (cf. Welmers 1973), which included the genetic classification ultimately based on Joseph Greenberg's (1963) work,[8] and I tried to learn Zulu during that year, taking a course taught by a wonderful poet from South Africa. Later, I did a one-year field methods course with Paul Schachter, where we worked on Akan). In those days, linguistics at UCLA was a very rich environment, and was very strong in phonetics with Peter Ladefoged, Ian Maddieson (e.g., Ladefoged & Maddieson 1996) and other members of the phonetics team. While phonetics wasn't

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[8] I was already aware of and interested in Greenberg's work on language typology; I had drawn on it in a course I taught in Linguistics, Lund University, in the first half of 1979. I was very happy when I had an opportunity to listen to Greenberg at a conference later in the 1980s. I remember a detailed comment he gave on evidence for the genetic classification of languages in one area in Africa presented in a talk based on observations of patterns in "word order". Greenberg noted that such patterns are likely to change due to language contact, so they are not reliable as indicators of genetic relationships. This was related to something Michael Halliday had told me earlier when he suggested that patterns within the textual metafunction are likely to change due to language contact, citing English and Japanese as examples.

my main area of interest, the team was very high-powered and it was stimulating to be asked to do recordings for them of my dialect of Swedish; I think they were keen on my uvular r's /R/, which amused me a little because a speech therapist who met with all the students in my high school class wasn't happy with them—perhaps they had been influenced by my mother's North-German r's.

While linguistics at UCLA was extraordinarily rich and varied, there was nobody working with, or interested in, SFL. There was, in fact, an indirect connection: **Paul Schachter** had engaged very seriously with **Dick Hudson's** (1976) Daughter Dependency Grammar (DDG) (e.g., Schachter 1981); but while SFL was a major source for DDG, it had already moved in a different direction—understandably, since Hudson had set out to provide answers to Chomsky's questions about language using first SFL (Hudson 1971) and then DDG (Hudson 1976) as non-transformational alternatives to Chomsky's Transformational Grammar.[9] When I talked to Schachter and Hudson about the connection, I found out that they had not met, and I remember being happy to be able to convey to them their mutual admiration for each other. Hudson had generously met with me a few times in the late 1970s when I had found cheap January flights from Copenhagen to London to enjoy the brilliant offerings at theatres there—grateful for opportunities to see plays with John Gielgud, Alec Guinness, Tom Conti, Douglas Fairbanks Jr., and Penelope Keith.

But then, in the second half of my first year at UCLA, by pure chance, I met **Michael Halliday**. Again, it was striking in those days that unless you were part of the network of academics, it was virtually impossible to find out where people were and what academic activities they were engaged in. I did not know where Michael Halliday was; all I had were hints from publications (e.g., he had written the foreword to *Cohesion in English* at Stanford University)—but I am sure I did not know that he had moved to Sydney University to take up the first chair in linguistics there. But during my first year at UCLA, I thought I should try to be an academic tourist and visit different universities. I made one trip from Los Angeles to Stanford University just to get a sense of what that famous university was like and what the campus was like—it turned out to be a beautiful campus. So, when I had arrived at Stanford University, I looked up a well-known linguist—Tom Wasow—in his office. He very kindly invited me in and talked about what they were doing in linguistics; and noting recent activities, and he showed me the programme of the special workshop on intonation. There were various well-known names like Dwight Bolinger, Ken Pike and Michael Halliday, so I said: "Wow, Michael Halliday was here?" Tom Wasow said, "yes, in fact, he is still with us, he's here as a visiting scholar". He could tell that I suddenly got very interested, and then he told me Halliday was giving a series of lectures. He looked up the timetable of lectures. Amazingly, it turned out that Halliday was giving a lecture just at that time, one that would finish in about ten minutes, so I asked Wasow: "Where, where, where?" He told me, and I dashed out

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[9] Later Hudson developed Word Grammar (Hudson 1984), and by then he had, as he told me on an occasion when I met him in London, come around at a higher level to Halliday's richer and more well-rounded conception of language and of linguistics than Chomsky's, partly as a result of his project of producing a textbook on sociolinguistics (Hudson 1980).

of his office, found the classroom, then I stood outside and waited for Halliday to
come out. Then he came out together with Ruqaiya Hasan. I sort of shyly introduced
myself, and said I was very interested in his work. He immediately gave me the
impression of somebody very approachable and easy to talk to, and told me that for
the second part of his sabbatical he would move from Stanford University to UC
Irvine and said: "Well, why don't you attend my ten-week lecture series there?". He
also gave me the name of the host and organizer, Benjamin Colby, a Professor of
Anthropology. So having made contact with Benjamin Colby, I started to attend the
lecture series. It took me four hours commuting by bus going down from LA, and
four hours going back.

But then, about three weeks into Halliday's course, I noticed a job ad on the bulletin
board of the UCLA Linguistics Department; it said somebody in a computational
linguistics project was looking for a research assistant familiar with SFL. Of course,
in all of Los Angeles, there was not really anyone familiar with SFL except for me.
So I called the contact number. The contact person turned out to be David Webber, a
brilliant linguist and world expert on Quechua (e.g., Weber 1989), and he arranged for
me to meet the project leader, **Bill Mann** (Fig. 1.1). It turned out that Bill was leading
research into text generation by computer at the Information Sciences Institute (ISI),

which was located at Marina del Rey, and was part of the University of Southern California.

Bill had initiated a survey of linguistic frameworks that could provide a central resource in a new project focussed on text generation by computer, including different variants of Generative Linguistics and SFL. (Bill had been impressed by Davey's (1978) text generation system that used Systemic Functional Grammar.) He and his research team had done some exploratory work on text generation by computer in the second half of the 1970s. Moving into the 1980s, researchers were beginning to establish text generation as part of computational linguistics, which had been mostly concerned with machine translation and text understanding, including parsing, in the 1960s and 1970s. But Bill wanted to do text generation in a new and fresh way, so he and his team had undertaken the survey I just mentioned, and they determined that by far the best approach for what they needed was SFL. One reason was the systemic functional commitment to **comprehensive descriptions of grammar**, and another was the systemic organization of the description of the grammar as a **meaning-making resource** organized around **choice** (e.g., Mann 1983).

When I met Bill Mann, he asked me about my knowledge of SFL. I told him about my work on *Hallidayan Linguistics*, and asked him if he knew that Michael Halliday was a visiting professor at UC Irvine. He said he had no idea. So, for the remaining seven weeks, Bill Mann also went to the Halliday lecture series, and I got a ride with him. By car, the journey was only around forty-five minutes—an improvement over my four-hour bus journey! After the lecture series had ended, Bill invited Michael to ISI for a day—Michael flew up by a commuter plane. I remember the three of us had lunch at The Warehouse, a restaurant in Marina del Rey designed to look like a trading place on an island in the South Pacific. Using his paper placemat, Michael drew two contrasting diagrams to illustrate the difference between the generative approach to the description of a language and the systemic functional one. (The lunch meeting went very well, but I think Bill had intended to impress Michael with the restaurant. That didn't work; in keeping with the warehouse design and décor, it was only dimly lit, and an important Hallidayan principle, Michael told me later, was that one should be able to see the food one eats.)

Through his discussions with Michael, Bill succeeded in convincing him to join the project as a consultant, and he hired me as a research assistant. That was really the start of what came to be known as the Penman Project, which, critically in terms of SFL, was the environment in which we developed a systemic functional grammar for text generation in English. Bill suggested naming it the **Nigel grammar** after Nigel in Michael's case study of how a child learns how to mean (Halliday 1975), and Michael agreed to his suggestion. We actually got the core of the description of the clause grammar from a research project at UC Irvine initiated by Benjamin Colby: working with a computer programmer, Mark James, Michael produced a systemic clause grammar of between 80 and 90 systems (reproduced in Halliday 2005: 268–284). Eventually, we expanded it to over a thousand systems, which I drew on when I produced an account of the grammar of English organized as a system network map (Matthiessen 1995).

The reason why Benjamin Colby had initiated the project at UC Irvine was very interesting. He had the notion that he wanted to explore culture through text, thus using text as a gateway into culture (anticipated by Colby 1966; cf. also Colby & Colby 1981). But, in order to do that, he needed to be able to process large volumes of text automatically, so he started this project designed to develop a computational parser for Systemic Functional Grammar. That was his long-term vision—a system for parsing and understanding text that would produce culturally interesting relevant analyses. The Irvine project remained a vision, but we developed the initial systemic clause grammar into a generation grammar.[10]

It was useful for me to be in the context of linguistics at UCLA, once I finally got to meet Michael Halliday and attended his series of lectures. Because by the time I attended the lecture series, I had seen quite a few really top US-based linguists. On the one hand, UCLA Linguistics was very good. It was judged the second-ranking department of linguistics in the US after MIT, and it was much more varied than MIT, which I think is significant in assessing its status. On the other hand, there had been a Linguistic Society of America meeting in LA in December in 1979. As a student, I was roped in to help at the conference, so I saw a number of quite well-known linguists, including, for example, Jim McCawley. I was asked to assist him when he gave his talk. He was a very nice man. After the talk, I told him I heard he was writing a book on language and logic. He grinned and said: "Yes, I happen to have a manuscript of this in my bag here, since you are interested, why don't I give you the manuscript to read?" It was published as McCawley (1981), with an interestingly inter-textual title. That was my first experience of Jim McCawley, who had been instrumental in the Generative Semantics movement about a decade earlier and then charted his own way forward (e.g., McCawley [1998]—and of course, McCawley [1984]).

So I was lucky to meet a great many linguists. (Later I would also meet outstanding linguists who had been marginalized by the dominance of Chomskyan linguists, e.g., Ken Pike, Robert Longacre, Joseph Grimes, Harold Gleason, Sydney Lamb; they would turn up at LACUS meetings.) As I said, the local ones at UCLA were very good, as were USC linguists; and of course I'd already encountered major linguists in Europe. Against this background, my first and lasting impression of Michael Halliday talking about language was revelatory—he transcended the linguistic scholarship I was familiar with. The sense I got when I started attending his lecture series at UC Irvine was this: for the first time ever, here I met a linguist who had an extraordinarily deep insight into language, and whose work was in the first instance about language, not about linguistics. That meant he was not trying to solve jigsaw puzzles designed according to linguistic theories. He was trying to achieve a rich comprehensive understanding of language. To me, that was one of the really fundamental experiences in terms of giving me an appreciation of what set Halliday apart among

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[10] It is possible to recognize similar aspirations in the last decade or so coming from information science researchers behind Google's Ngram viewer and Google Trends (e.g., Michel et al. 2010). It would be interesting if such efforts could be combined with insights from SFL and Benjamin Colby's anthropological vision.

leading linguists. Later, when I went down to Sydney to work, I reflected on how lucky and privileged the students in Linguistics at Sydney University were since they had had Michael Halliday as one of their teachers for years, but I also realized they had not experienced the magnitude of the difference between him and other big names in linguistics.

My sense of their experience as Halliday's students resonated with something that he and I discussed later—his view of **what you do in first-year linguistics**. What I have seen in many places is that in first-year undergraduate linguistics, students are introduced to jigsaw puzzles based on rule systems, and at the time of my undergraduate days, it was some form of Transformational Grammar or Generative Phonology (as in one of our textbooks in Linguistics at Lund, Akmajian & Heny 1975). This may be good for students who like games, but it gives them no sense of engagement with language. Michael said to me: "That's not what you do in first-year linguistics; what you do in first-year linguistics is to enable the students to get a sense of language, to reflect on language, and to really observe language". So, you do not talk to them about linguistics. You talk about **language**, and you enable them to engage with language and become aware of it, since to them it has been part of the background of life. As a linguist, Michael somehow had a broad channel of communication with language itself, and really understood its central nature as a resource and how it is organized. That led to his gradual development of SFL.

1.3 The Distinctive Properties of Systemic Functional Linguistics

Isaac Mwinlaaru: You have given us the landscape of linguistics in the 1970s and the 1980s, which was the background of SFL. What are the distinctive properties of SFL against the other frameworks of grammars and linguistics that you have indicated?

Christian Matthiessen: It is a very central question, and I think there may be different answers depending on where one is coming from. (1) In the context of modelling language for text generation by computer, as I mentioned earlier, Bill Mann once did a survey with the help of David Webber, who was a full-blown descriptive typological linguist who did his PhD thesis on one of the varieties of Quechua in Peru (Webber 1989). To them, what was distinctive was that high priority was given to *comprehensive descriptions that are meaning-oriented and organized around choice*. That was a very good answer because it was not really part of the agenda at the time for many approaches. It was very different from Chomsky's agenda of taking small areas of language and developing very sophisticated accounts in order to be able to arrive at something you could plausibly claim to be part of universal grammar in one manifestation or another (the manifestations becoming more abstract over the decades).

This narrow focus was highlighted at the end of the 1970s in an article in *Language* by Maurice Gross (1979), a very good French linguist who had done his PhD with

Zellig Harris, written about mathematical linguistics, and was also committed to corpus-based linguistics, i.e., the linguistic research based on authentic evidence. The title, "On the Failure of Generative Grammar", was quite provocative at the time. Gross explained what the failure was: generative linguists had failed to produce anything comprehensive by way of description. He cited as an attempt known as the UCLA grammar, which was put together from different fragments in the early 1970s at UCLA by Bob Stockwell et al. (1973), but he did not think that the attempt at integration had succeeded. So, in that kind of context, you could say yes, generative grammar was a failure in terms of the commitment to comprehensive descriptions of language as a meaning-making resource. [Stockwell et al. (1973) is an interesting attempt, and part of the challenge the authors faced was settling on a framework in generative linguistics that would enable them to weave together descriptive fragments from different sources.]

As an anachronistic aside, but a very important one, since the 1970s, linguists have also identified other ways in which the Chomskyan research programme has failed; for example, Evans and Levinson (2009) expose "the myth of language universals". And neuroscientists such as Gerald Edelman and Terrence Deacon have illuminated problems with other aspects of the foundation of the Chomskyan programme. Similarly, Michael Tomasello has adopted certain key positions similar to ones earlier articulated by Michael Halliday.[11]

(2) From another vantage point, Michael Halliday is the only linguist who has given priority to the **paradigmatic axis** and organized the theory and description of language around this axis rather than the syntagmatic axis, demonstrating that this gives us the insight into language as a complex adaptive system. In my chapter in *The Bloomsbury Companion to M.A.K. Halliday* on Halliday's theory of language (Matthiessen 2015: 151), I call it "the axial rethink". He does not start from structure—from syntagmatic patterns, but rather from system—from paradigmatic patterns (see e.g., Halliday 1966). This gives us a unique insight into language. A number of further insights flow from that fundamental insight (see Fig. 1.2); but at the same time, he maintains the connection with the syntagmatic, by means of realization statements specifying fragments of structure, and the account of structure he developed is a sophisticated unification of different metafunctional strands of organization. To me, one of these foundational insights into the organization of a system of some kind is, in this case, language. So, I would cite that, too, as distinctive.

(3) I would also pick up a third aspect. If you look at the history of linguistics in the twentieth century, there has been a considerable tension between the people focussing on text, on *parole*, on performance, or on discourse, and the people trying to focus on the system, on *langue*, or on competence. We have this tension, and there has always been a chasm between the two because nobody has theorized the connection between them. Here, I would say that the one person, the one linguist, who has

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[11] At an invited talk Tomasello gave at AAAL (American Association for Applied Linguistics) in Atlanta on March 9, 2010, "Constructing a language", Peter Fries pointed out to Tomasello certain key similarities with the earlier systemic functional work by Michael Halliday. As a member of the audience, I didn't get the sense that Tomasello picked up on Peter's very helpful comments; but perhaps he has done in subsequent work.
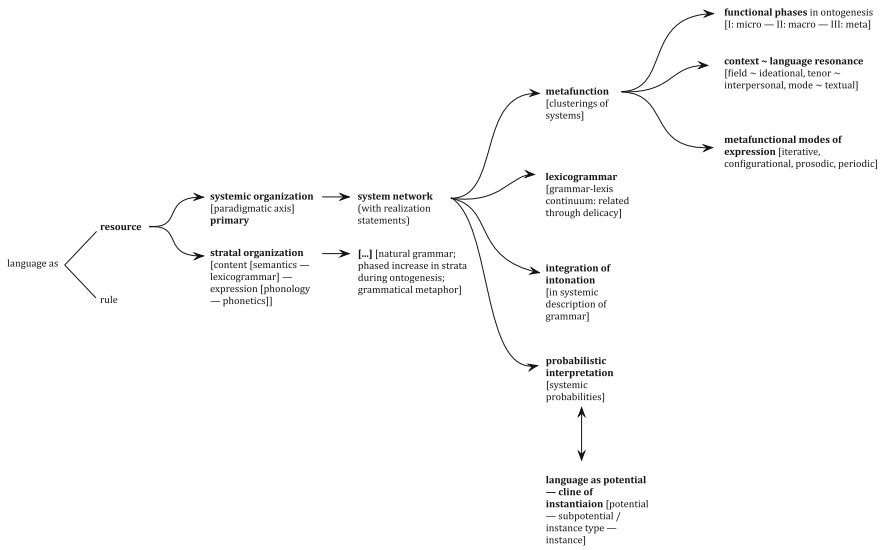
Fig. 1.2 The axial rethink: The Hallidayan insights on language

done this, who has theorized the connection between system and instance is Michael Halliday, with his notion of the **cline of instantiation**. That again makes possible a number of additional insights, like being able to understand language in probabilistic terms by looking at relative frequencies in text and interpreting them as **systemic probabilities**. That also gives linguistics a solid evidential base, grounding theoretical and descriptive generalizations at the potential pole of the cline of instantiation in extensive samples of text at the instance pole.

1.4 Connection Between Systemic Functional Linguistics and the European Linguistic Tradition

Isaac Mwinlaaru: We have talked about a meaning-oriented and comprehensive description of language, the paradigmatic axis against the syntagmatic axis, and bridging the gap between system and instance. Since you evoke some Saussurean terms, a natural follow-up would be the connection between SFL and Saussurean linguistics, and by extension, other European schools in linguistics such as the London School and the Prague School. What are their connections with SFL? How did SFL depart from these frameworks?

Christian Matthiessen: I will start with the first part of these **Saussurean connections**. We certainly have had a number of systemic functional linguists writing about Saussure's posthumous *Cours* (1916, 1974) and making connections, important contributions being Ruqaiya Hasan (e.g., Hasan 1987, 2014, 2015), David Butt

(e.g., Butt 2001, 2015) and Thibault (1997)—and also one of Michael Halliday's few book reviews, i.e., his review of Jonathan Culler's (1977) introduction to Saussure.

To me, it was interesting because I felt that in my linguistic "kindergarten", I had been brought up with Saussure, to a large extent thanks to **Bertil Malmberg** (1913–1994).[12] On the one hand, there were the readings that I did before I studied linguistics at Lund University—centrally, Bertil Malmberg's (1969) *New Trends in Linguistics*. On the other hand, once I started at Lund University, I met him; he was the first Professor of Linguistics at the university (having moved from Phonetics in 1969, after many years as professor there [1950–1969]). Bertil retired while I was there and was succeeded by his student Bengt Sigurd (1928–2010), but I had the opportunity to benefit from the fact that he was the one who introduced Saussurean linguistics to the Swedish linguistic scene, starting in the 1930s.

While he had not met Saussure himself, having been born in 1913, the same year that Saussure died, he had known a number of the major representatives of European structuralism—half a generation before him, Louis Hjelmslev (1899–1965), and Roman Jakobson (1896–1982) and, from the same generation, André Martinet (1908–1999).

He was a young scholar in the 1930s, specializing in Romance languages and his PhD thesis was *Le roman du Comte de Poitiers* (Malmberg 1940). He was well versed in the Romance tradition, including not only Saussure, but also the Geneva school of commentary on Saussure, and various contributions by Spanish-writing and Spanish-speaking linguists.

To him, Saussure was the master. He also rated **Roman Jakobson** very highly, and must have known him fairly well. I remember once a group of us visited Bertil in his vacation home on the east coast of Skåne. He told us he had hosted Jakobson there, and Jakobson had looked out the window across the Baltic Sea towards Russia, and said to Bertil "I had to flee, and I will never return." But Roman Jakobson did, in fact, return towards the end of his life. Naturally, Bertil Malmberg also knew **Louis Hjelmslev** and his group—including Eli Fischer-Jørgensen (1911–2010) (whose encyclopaedic [1975] history of phonology, mentioned above, I read as part of my

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[12] He was of the same generation as my mother; in fact, they were born and died in the same years. My half-sister-in-law, Ingrid Emond, had gotten to know Malmberg when she was a student, and would tell me stories about him. He was kind and helpful to me. In the year he retired, he was given the huge honour of being asked to deliver the keynote address at the graduation ceremony in Lund Cathedral. He hovered above us, in the archbishop's pulpit. A motif in his talk was language and thought, and he quoted Esaias Tegnér (1782–1846), *Upp flyga orden, tanken stilla står. Ord utan tanke himlen når.* ("Up words fly, thought remains stationary. Words without thought reach heaven.") On the same occasion, Eli Fisher-Jørgensen was given an honorary doctorate by Lund University—sadly for me, the only occasion I had an opportunity to see her in person; I had admired her since I had read her still unsurpassed history of phonology one or two years earlier (Fischer-Jørgensen 1975). After the graduation ceremony, the attendants were milling around outside the cathedral. Malmberg spotted me, walked up beaming happily, stretched out his hand and said "Let's be brothers". I had no idea what he meant, and as he realized how bewildered I was, he explained that he suggested that we should "lägga bort titlarna", literally "put our titles away", i.e., call each other by our given names. I thanked him profusely and politely, and after then he was "Bertil"—quite an honour for me as an undergraduate student.

undergraduate studies). Bertil was part of the group of linguists who understood and appreciated Hjelmslev's Glossematics. (It turned out later that my exposure to Hjelmslev and Glossematics was helpful not only to my involvement in SFL but also to my understanding of Sydney Lamb's Stratificational Linguistics (e.g., Lamb 1966, 1999; Lockwood 1972; García et al. 2017), Lamb being fairly unique among US American linguists in taking Hjelmslev seriously.[13]).

That was part of Malmberg's background, and I absorbed as much as I could when I grew up linguistically. I took a reading course dealing with classics in linguistics, and fortunately for me, the year I took the course, Thore Petterson (a docent in the linguistics department) had chosen to guide us through Hjelmslev's (1943) *Omkring sprogteoriens grundlæggelse*, translated into English with a somewhat fancier title: *Prolegomena to a Theory of Language* (Hjelmslev 1953); but we read the Danish original.[14] At some point, Thore organized an excursion for us to Copenhagen, and introduced us to linguists who had known Hjelmslev and were happy to share stories with us.

As he was about to retire, Malmberg gave us a special intellectual treat—a course on the history of theories of the sign; it was designed for research students and scholars, but even though I was still an undergraduate student, I was allowed to sit in. He covered the whole period in the Western tradition from the Stoics to contemporary European structuralists, with a rich selection of readings. I remember he gave us a reading in Spanish from Sanchez or one of his contemporaries, remarking that it was easy to read even if we hadn't studied Spanish.[15]

So the Saussurean syndrome of insights and stances was very much part of the tradition I grew up in and thus part of my background; but I was also aware of critical views, including his contemporary Otto Jespersen's (1917, 1933b) review of his posthumous *Cours*, which I read while at Lund (cf. also Koerner 1999: Chapter 6, on Jespersen's reading of Saussure). For example, Jespersen's critical review of Saussure's sharp distinction between synchrony and diachrony helped me understand the pendulum swings in intellectual history in general between different positions, and the potential need at certain points in history to emphasize, or event exaggerate, certain distinctions in order to create the space needed to move ahead. One could argue that Labov's much later work on change in progress shed light on Jespersen's objection to the sharp dichotomy in *Cours*.

The relationship of Saussure's work to Firthian linguistics is, of course, quite interesting in our context of discussing SFL. I remember Michael Halliday telling me that Firth thought Saussure was a bit overrated, and having grown up in a partly

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[13] Cf. Einar Haugen's (1951) presidential address to the LSA (Linguistic Society of America), where he drew attention to the relative lack of interaction between the linguistic traditions on the two sides of the Atlantic.

[14] While not necessarily mutually intelligible in speech, Swedish and Danish are close enough that it was possible for me to read Hjelmslev's book in Danish—though hard work; around the same time, I read Aarne Næss's history of philosophy in Norwegian as part of my studies in the Department of Philosophy. I found Bertrand Russell's history of Western philosophy in English easier to read!.

[15] It can't have been from Sanchez's (Sanctius') well-known *Minerva*, since it is in Latin; but we learned about his contribution to the theory of the sign (cf. Breva-Claramonte 1982).

Saussurean environment, I could actually understand the point. Similarly, I have sometimes taken the Firthian line that people tend to overrate Saussure a bit.[16] That was against the background of having had a Saussurean "kindergarten", instead of being very much part of that and very much valuing it. But Saussure died too young at 56. The European structuralists were not able to take it much further. Part of it was that they did not have a theory that would allow them to organize around the paradigmatic axis in any extended kind of way—a challenge that was addressed by Michael Halliday rather than by any of the Continental European structuralists, when he gave priority to the paradigmatic axis and developed the system network as a theoretical representation of paradigmatic patterns (e.g., Halliday 1966; cf. Matthiessen 2015 on Halliday's "axial rethink"). I felt that Continental European structuralists were a bit stuck. Malmberg himself said he saw it as a great tragedy—that the Geneva School turned out to be "metalinguistics". In the Geneva School, they kept talking about reading Saussure, re-reading Saussure and reading new notes and so on. Malmberg, while he himself contributed to the understanding of Saussure, felt that what needed to be done was to develop Saussure. That was what Hjelmslev, Martinet and other scholars influenced by Saussure but also striking their own path were in fact doing.

While it is important to make the connection, I think one does not have to go back to Saussure (1916, 1974) every time to quote him and ground one's work in his. A couple of years ago, I went back to re-read Saussure's work. What struck me was this: it would be such a nice source for an analysis of evaluation and appraisal because he was very dismissive of various ideas and approaches. It is an interesting kind of discourse used strategically; he tries to create a new space for the study of language and other semiotic systems.

Mathesius and the other early **Prague School functionalists** were more interesting because they actually got on with the work of describing languages; the combination of theory and extensive description made their work very powerful. They were able to shed new light on a number of different regions of language. Interestingly, Michael Halliday has told me he worked out the textual organization of the clause when working on Chinese before he became aware of the Prague School work on their Functional Sentence Perspective. He studied in China; and without the World Wide Web, access to scholarly information and also to the flow of translations was much more restricted. The Prague School got isolated during WWII and then the isolation continued during the Stalinist period and its aftermath during the Cold War. It was very difficult for people to travel, and conference participation was much more restricted. But once Michael Halliday saw the parallels, he engaged with the work of the Prague School (e.g., Halliday 1974) and made sure that he referred to and promoted their work in the West.

Importantly, we also need to make a connection with the US American anthropological linguistic tradition, as Michael Halliday has done—and let me add this

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[16] Saussure is, of course, often characterized as the "father of modern linguistics"; but on the one hand it is helpful to view him in the context of European scholarship at the time (including Mathesius 1911/1964), as Seuren (2016) does, and on the other hand it is very important to recognize the large-scale descriptive effort initiated by Franz Boas and followed up by US American anthropological and descriptive linguists. On this tradition, see further below.

important substantial body of descriptive work to the consideration of Saussure's contributions. When people say he is the founder of modern linguistics, I say yes, well, ***but***, by which I mean it is important not to undervalue descriptive work, in thinking about what got linguistics in the twentieth century energized. After his earlier work in historical linguistics, Saussure did not do much by way of description, and his *Cours* was rhetorically located within the tradition of Indo-European historical linguistics—in particular, the Junggrammatiker strand (e.g., Koerner 1999), but in the US, **Franz Boas**, **Edward Sapir**, **Mary Haas**, **Benjamin Lee Whorf** and others undertook a very extensive programme of language description, ranging over many different languages. I would give them equal weight in energizing and pioneering linguistics in the twentieth century. To those who keep celebrating only Saussure as ***the*** founder of modern linguistics, I would say: read Boas's work on Kwakiutl, and consider his collaboration and co-authoring with George Hunt—a speaker of the language (e.g., Boas & Hunt 1921; Boas 1935, 1976). Boas made much more of an institutional contribution than Saussure; they were born within two years of each other, but Boas had the fortune of living until 1942, and was able to train a couple of generations of anthropologists and anthropological linguists. The flow of **American anthropological descriptive linguistics** into Michael Halliday's thinking was important. Significantly, this is also a connection with Sydney Lamb's development of Stratificational Linguistics, since it was grounded in the American descriptivist tradition with Lamb's (1957) PhD thesis at UC Berkeley on Mono, supervised by Edward Sapir's student Mary Haas.

1.5   Connection Between Systemic Functional Linguistics and the American Linguistic Tradition

Isaac Mwinlaaru: You have mentioned Tagmemics when talking about the European school of linguistic structuralism. I would like to know the connection between SFL and Kenneth Pike's (e.g., 1976) Tagmemics, and whether there was any collaboration between scholars of SFL and scholars in the American tradition like Kenneth Pike at some point during the early stages of SFL.

Christian Matthiessen: There are certainly resonances having to do with, very broadly, orientation to meaning, with the interest in text and culture and with a sense of the centrality of description. That makes sense if one thinks of **Tagmemics** as continuing the American anthropological tradition from Sapir to Pike. Also, Mary Haas, as Sapir's student, and Pike are roughly contemporaries on the American scene (in terms of personal recollections, see Pike 2001). And there were connections of family and friendship going back a long time. Importantly, Ken Pike was supervised by Charles C. Fries at Michigan University, and C.C. Fries's son Peter Fries got to know Pike when he was still a boy, and then later also as a fellow linguist once Peter had obtained his PhD at the University of Pennsylvania (Fries 1964; see also Fries 1970). Michael Halliday knew Ken Pike, of course; and thanks to Peter Fries and his

wife Nancy, I got to know Ken Pike and also other members of his family, and of his group, like Ruth Brend. LACUS, the Linguistic Association of Canada and the US,[17] also played an important role in providing a forum where linguists who found themselves struggling outside the Chomskyan "mainstream" for a few decades could meet, knowing that they would not be attacked for being outside the mainstream.[18] In this way, LACUS provided a welcome oasis for Tagmemicists, Stratificationalists and also for Systemicists.

Then another component of Tagmemics is the Summer Institute of Linguistics (SIL)[19] and the Wycliffe Bible Translators.[20] In that engagement with the description of languages that are typologically very varied, there is certain resonance in terms of the ethnographic orientation and the support of application (though there is little or no application to Bible translation in the case of SFL although members of SIL in Britain have worked with SFL). Anecdotally, I remember Michael Halliday telling me that in the 1950s, Kenneth Pike was invited to London by J.R. Firth (cf. Bendor-Samuel 2002). When Ike (Eisenhower) was running for an election or re-election, there was a political slogan—"We like Ike". The London linguists turned this into "We like Pike". I have been fortunate enough to meet Pike a number of times and to chat with him, learning from him about his sense of the key differences between Tagmemics and SFL.

Also, there was one very important meeting of the LACUS Society in 1983 at Quebec University in Montreal, where the organizers had invited the founders of three great traditions in linguistics—Ken Pike (Tagmemic Linguistics), Michael Halliday (Systemic Functional Linguistics) and Sydney Lamb (Stratificational Linguistics, later renamed Cognitive Linguistics, and now known as Relational Network Theory, cf. García, Sullivan & Tsiang 2017). If you consider these three traditions in relation to the notion of **appliable linguistics** (cf. Halliday 2008), you could say all three are appliable. Certainly, in Tagmemics, the notion of being able to apply linguistics was important in the context of the work on Bible translation, developing orthographies and so on; and it has contributed significantly to the understanding of the relation between grammar and discourse patterns, discourse organization and translation.

But if you compare Tagmemics with SFL, SFL has a much broader **range of applications**. The applications of Tagmemics were largely restricted to the context

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[17] http://lacus.weebly.com/.

[18] At one LACUS conference, the organizers had, amazingly, succeeded in getting Ken Pike, Michael Halliday and Sydney Lamb as keynote speakers: see below. I remember Lamb's perceptive and eloquent deconstruction of the notion of the "mainstream" in linguistics; through his analysis, he was able to dismiss the notion as misguided. To me, the combination of Pike, Halliday and Lamb represented a far deeper understanding of, and insight into, language than Chomsky's; and I would say that this is also true of them individually. One of the characteristics they shared was the extensive experience with, and commitment to, the significance of language description (cf. e.g., Sampson 1980; Seuren 1998, 2004). And the empowering quality of this kind of experience has, during certain periods of linguistics, not been sufficiently recognized or even dismissed—cf. my discussion above about the founders of modern linguistics.

[19] https://www.sil.org.

[20] https://www.wycliffe.org.

of the Summer Institute of Linguistics, but while the strongest early application of SFL was undoubtedly in education, many more have been added. In terms of the range of applications, SFL has covered many more different institutional settings than Tagmemics (see Matthiessen 2009; Matthiessen & Teruya in press). Of course, Tagmemics emphasizes the descriptions of many different languages, but this multilingual orientation has also been part of the Firth-Halliday tradition from the start, and systemic functional linguists have been adding descriptions of an increasing range of languages since the 1960s (e.g., Kashyap 2019; Mwinlaaru & Xuan 2016; Teruya & Matthiessen 2015).

I had a sense that in terms of the **influences** between SFL and Tagmemics, it was more a case of maintaining a dialogue. It would perhaps be harder to point out anything in SFL where there was a direct influence or borrowing from Tagmemics. The dialogue was more about the recognition of parallel notions, like the notion of levels in Tagmemics, and the notion of rank in SFL (cf. Pike 1967); and Pike (2001) comments on how often he has mentioned Firth in his work. One could compare the notion of the tagmeme in Tagmemics (originally taken from Bloomfield 1933) with the notion of a function and its realization by class in SFL. Tagmemics, unlike SFL, never moved in the direction of taking the paradigmatic as the primary organizing axis—a significant difference.

Sadly, I would say Tagmemics is now **a dead metalanguage**, and is no longer really used except in a few places outside the US around the world. I could see the beginnings of this already when Pike was still alive in the late 1970s, and certainly in the 1980s when it ceased to be the primary kind of linguistics taught within the Summer Institute of Linguistics. I remember talking to one of Pike's followers, Ruth Brend, who did a lot of editing of tagmemic work (e.g., Brend 1974; Pike & Brend 1972). When she was at University of East Lansing, she had PhD students in Tagmemics, but then she stopped supervising works using Tagmemics because she felt if she continued, they would not be able to get an academic position and they would not be able to take this into institutional linguistic contexts. That was the beginning of the end of Tagmemics. But it continued to be used elsewhere outside the US, e.g., in important descriptive work at Mahidol University on various languages spoken in Thailand. This does not mean, of course, that we should stop reading the Tagmemic literature—on the contrary; for example, Pike (1967) is still a great source of deep insights, also in the current context of interest in language and other semiotic or social systems (Table 1.1).

One would have to be very careful not to attribute its gradual disappearance to Tagmemics itself, or at least only partly, because at the time the context was that of the dominant generative linguistics. People's interest in language was so different from Pike's that they did not engage with Pike, and they did not understand the importance of his contributions.[21] But other folks did. If you look at Talmy Givón's (1979) introduction to his book, *On Understanding Grammar*, he cites Pike as one

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[21] I remember one occasion when Pike had been invited to give a Friday colloquium in Linguistics at UCLA. His talk was inspired and inspiring—and also entertaining: he played the flute as an illustration of one of his points. But I had the very uncomfortable feeling at the time that none of

Table 1.1 Christian M.I.M. Matthiessen's linguistic encounters

Period	Institution	Languages studied	Linguistics teachers	Other linguists encountered	Doctoral students
1972–1975	Nicolaiskolan	English, German, French	Helge Jahn (Swedish, German)	Otto Jespersen, Bertil Malmberg (1969), Alvar Ellegård (1971)	
1975–1976	Lund University (correspondence course)	English		John Lyons (1968)	–
1976–1979	Lund University, Fil. kand. (BA)	English, Arabic	Bertil Malmberg, Thore Pettersson, Bengt Sigurd, Sven Platzack, Christopher Stroud, Kenneth Hyltenstam; Jan Svartvik, Bengt Altenberg	[Talks and public defences, e.g.,] André Martinet, Geoffrey Leech, John Sinclair, Nicholas Ruwet, Alvar Ellegård, Östen Dahl, Pit Corder	–
1979–1984	UCLA, MA	Zulu	Sandy Thompson, Ed Keenan, Talmy Givón, Bill Welmers, Susie Curtiss, Vicky Fromkin, Stephen Anderson, Peter Ladefoged, Paul Schacter; Bernard Comrie	[Talks, e.g.,] Ken Pike, John Haiman, George Lakoff, Haj Ross, Noam Chomsky, Jack Hawkins, Deirdre Wilson, Ray Jackendoff [LSA Institute, e.g.,] John Goldsmith, Gerald Gazdar, Geoff Pullum [Visits to ISI, e.g.,] Martin Kay, Igor Mel'čuk, Aleksandr Žolkovsky, Elinor Ochs, Joe Grimes, Jim Martin, Ruqaiya Hasan, Evelyn Pike	–
1984–1988	UCLA, PhD	(Akan)			

(continued)

Table 1.1 (continued)

Period	Institution	Languages studied	Linguistics teachers	Other linguists encountered	Doctoral students
1988–1994	Sydney University	Mandarin	–	[Colleagues:] Jim Martin, Clare Painter, Di Slade; John Gibbons, Michael Walsh	Marilyn Cross, Keizo Nanri, Mick O'Donnell, Arlene Harvey
1994–2008	Macquarie University	–	–		Zeng Licheng, Kazuhiro Teruya, Ed McDonald, Minh Duc Thai, Pattama Patpong, Abhishek Kashyap, Maria Herke, Mohamed Ali Bardi, Ernest Akerejola
2008–2021	The Hong Kong Polytechnic University	–	–		Winfred Xuan, Nancy Guo … (see Table 3.2)

of the key influential linguists together with Dwight Bolinger, and emphasizes the importance of Pike's work. If you look at more typological works, Pike has continued to influence scholars like Robert Longacre (e.g., 1996). And in practical approaches to language description, Pike's "monolingual" continues to be a fascinating model and source of inspiration.[22] I had an opportunity to witness one of his monolinguals sometime around the mid-1980s, and it certainly represented an important strategy in field work—but at the same time, it also demonstrated the importance of tenor considerations since Pike's engaging persona was certainly an important aspect of the success.

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my fellow research students "got it"—they just hadn't got the background to appreciate and value Pike's insights.

[22] For demonstration, from 1966, see: https://archive.org/details/languagebygesture And Dan Everett following up the Pikean tradition: https://www.youtube.com/watch?v=lVo2WJFeFe4 and https://www.youtube.com/watch?v=sYpWp7g7XWU.

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Chapter 2
Life and Work of Christian Matthiessen (Part I)

Abstract This chapter briefly sketches Christian Matthiessen's life and work since the late 1970s. We first introduce the training that Christian Matthiessen has received at Lund University in Sweden. Then we report on his experiences of studying linguistics at University of California, Los Angeles. His Ph.D. thesis on text generation and the influences on him by West Coast Functionalism are also related. Finally, we present some background information on his work experience at the University of Sydney and later at Macquarie University.

2.1 Introduction

This chapter continues Christian Matthiessen's personal experiences of linguistics begun in Chap. 1. It identifies specific institutional experiences noted in Chap. 1 and examines their influences on him and his scholarship. The institutions considered comprise Lund University, University of California Los Angeles (UCLA), University of Sydney and Macquarie University. Within each institution, we are interested in a detailed description of the kind of linguistic training that was provided to students, research commitments and approaches to language studies. Christian Matthiessen also reflects on the influences of these institutional dynamics on his scholarship and interests. Further, he introduces the outlines of his MA thesis on the system of tense in English and his over 1000-page Ph.D. thesis on text generation as a linguistic research task.

2.2 Studying Linguistics at Lund University

Bo Wang: In our first interview (see Chap. 1), you talked about how you read extensively in the library of the Linguistics Department at Lund University[1] and how

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[1] https://www.lunduniversity.lu.se/home.

you decided to become a linguist. What kind of training did you receive from Lund University?

Christian Matthiessen: I was lucky in that it was a good period to be there because I was actually in four departments where language played a role in one way or another. In addition to the Department of Linguistics, they were:

- the Department of English, where I was focussed on the **English linguistics** strand rather than English literature although I enjoyed courses in British and American literature as well (this was really before the breakthrough to "post-colonial" literature)—particularly lectures given by Rick Fischer and a private study group he organized, where we explored certain works in some detail, like Faulkner's writing;
- the Department of Oriental and Semitic Languages, where I was studying **Arabic**, trying to learn MSA (Modern Standard Arabic) since I wanted to learn a non-Indo-European language, but also studying relevant culture and history;
- the Department of Philosophy, where **natural language philosophy** and **logic** were part of the curriculum, and I also learned about the history of Western Philosophy, epistemology, and the philosophy of science.

Consequently, I came across different traditions engaging with language, in particular in the Department of English and the Department of Linguistics. I was lucky that in the Department of Linguistics, we still had the founding professor, **Bertil Malmberg**. He had been appointed to the first chair in general phonetics in Sweden in 1950 at Lund University,[2] and then he went on to start the first department of general linguistics in 1969 by taking it over from Indo-European language studies. In terms of his background and training, he was born in 1913, so he was a young university student in the 1930s and was very much in the **European structuralist tradition**. To him, the heroes were **Ferdinand de Saussure** (1858–1913) and **Roman Jakobson** (1896–1982). Of course, he did not meet Saussure because he was born in 1913—the year that Saussure died. But he did meet and get to know Roman Jakobson (see Chap. 1). Bertil also knew **André Martinet** (1908–1999), who came to give a talk at Lund University; I was certainly thrilled to have the opportunity to listen to a lecture by a great linguist, a proponent of European Structuralism influenced by and contributing to the Prague School, but actually rather disappointed in the lecture itself, which was on functional syntax but did not seem to offer any particular insights. However, Martinet's contributions had, of course, focussed largely on phonology. Bertil had, quite naturally, also known Louis Hjelmslev (1899–1965), who had been a "neighbour" at Copenhagen University. But at the same time, the kind of linguistics taught in the undergraduate courses in the Linguistics Department was really Chomskyan, i.e., Standard Theory and Extended Standard Theory and the Chomsky & Halle (1968) stage of generative phonology (the "Sound Patterns of English"). Of course, the programme also included a great deal beyond syntax and phonology, e.g., the history of linguistics and contrastive linguistics, which I taught as

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[2] See e.g., https://www.lunduniversity.lu.se/lup/publication/b732f1f3-571d-4793-beeb-55af20a2916f.

a visiting lecturer to other unsuspecting students in my last year as an undergraduate student there in the spring of 1979.

I got to know both the European structuralist tradition, which I had already encountered in high school, and the **Chomskyan generative linguistics** of the day. And, fortunately, one of the Ph.D. students in Linguistics at Lund, Milan Bílý from Czechoslovakia, was very much familiar with the **Prague School**, and he was doing his own Ph.D. research in the Prague School tradition. He happened to be Professor Malmberg's son-in-law. In general, the Linguistics Department often felt like a happy extended family.

Then, in the English Department, **Jan Svartvik**[3] (1931–) had recently been appointed to the chair of English linguistics. He had changed the historical tradition that had been dominant. (Lund University was very well-known for place name studies, and there were still one or two people around from this *Ekwall* tradition.[4]) But Svartvik brought **corpus linguistics** with him, including the development of the London–Lund corpus, which had been initiated by Randolph Quirk as part of the Survey of English Usage.[5] (This research group is partly covered in Crystal's [2009] autobiography.) Being exposed to this tradition as well was very beneficial to me. A number of the Ph.D. students in linguistics that I got to know as an undergraduate student were working on the London–Lund corpus, including Lars Hermerén (e.g., 1978), Anna-Britta Stenström (e.g., 1984), and Bengt Oreström (e.g., 1983). Some of them were transcribing this first extensive corpus of spoken British English; it was called the London–Lund corpus because it was recorded in London and transcribed in Lund. As part of their transcription effort, they might be sent to London. Jan Svartvik gave me a letter of introduction to Randolph Quirk as I was about to set out on one of my January trips to London. Randolph Quirk received me graciously, and introduced me to Anna-Britta Stenström, who was on a visit from Lund.

The result of this extensive work was published as Svartvik & Quirk (1980). This corpus was really a pioneering sample of dialogic spoken English. I have used it a great deal myself, although this has as yet not really been reflected in my publications, or rather only as examples cited in various places (as in my *Lexicogrammatical Cartography*). The first time I used it was when Jan Svartvik gave me a printed sample of concordance lines with modals in around 1978, so that I would be able to conduct a corpus-based study of modality. But since I left for UCLA in 1979, I did not complete it. The corpus is certainly a unique pioneering contribution, but it suffers from the way the transcript is represented. It is not separated from the prosodic annotation, largely based on Crystal's (e.g., 1969) system (which is of course linguistically much more sophisticated than the conventions used in Conversation Analysis [CA]), so it is difficult to read and even the electronic version is difficult to search. This would have been before the development of stand-off markup annotation. I have spent countless

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[3] See: https://sv.wikipedia.org/wiki/Jan_Svartvik.

[4] See: https://en.wikipedia.org/wiki/Eilert_Ekwall and *Lund Studies in English*, established by Svartvik in 1933.

[5] See e.g., https://www.ucl.ac.uk/english-usage/about/svartvik.htm.

hours cleaning up the transcript by removing the annotation so that I could search for items and use the transcript for lexicogrammatical analysis.

Through my study of Arabic (Modern Standard Arabic, MSA), I got a sense of a language that was very different from Indo-European (IE) languages, including the ones I had studied myself. Studying Arabic was a marvellous experience, very enriching. I benefitted greatly from studying linguistics at the same time—particularly as I engaged with the phonology and phonetics and the grammar of Arabic. I could use my growing linguistic expertise to help myself master the pronunciation of Arabic, since I had learned articulatory phonetics; and I could make sense of certain important features like the allophonic variation of the three vowels of Modern Standard Arabic (MSA) in the environment of different consonants (in other words, a reflection of syllabic properties). My background in linguistics also helped me with features of the grammar that might otherwise have seemed obscure, like the temporal system, which I think is a mixture of tense and aspect, and the variation in "word order" in MSA—predominantly VSO but also under certain conditions SVO. At some point, I realized the significance of the beginning of clauses—thematic prominence mapped onto interpersonal overture. There were certainly puzzles, like the "tension" between clause-based transitivity and word-rank derivational system of verb classes, which appeared to me to have patterns that were motivated "from above" but also involved unpredictable lexicalizations. Lexis was indeed the one area of Arabic where my linguistic studies did not provide me with much help— and unlike in my study of Germanic and Romance IE languages, I could not rely on cognates (just a few loan words in both directions). So vocabulary learning was tough. As I struggled, I realized that I had a visually oriented memory—memorization became somewhat easier once I had mastered the Arabic script. However, even when I had mastered the script, one significant challenge remained. In Arabic texts, vowels are normally not specified (except for length), since they are predictable if one has mastered the language; similarly, ligatures often defeated me. So I depended on readers prepared for learners, and I bought a bilingual copy of the Koran (Qur'an), and tried to read passages since vowels are specified in the Koran. And at some point, I got some books designed for children learning to read Arabic.

In the department, there were only a few teachers of Arabic, and it was a very friendly academic environment. One of the teachers, Kjell Norlin, was particularly helpful. At some point he gave me a copy of Wright's grammar of Arabic, a classic from the nineteenth century. And I bought Cantarinos's three-volume grammar of MSA. Since then, more descriptions of a fairly comprehensive nature have been published (e.g., Badawi, Carter & Gully 2016).

Then, in philosophy, we read some of the classics like *Theaetetus*, which is the Socratic dialogue about language. We also learned about Russell, Wittgenstein (both manifestations), Carnap and other philosophers who had been concerned with aspects of language; and we got a good dose of the philosophy of language and the history of philosophy (I read both Russell's history of Western Philosophy in English and Aarne Naess's two-volume history in Norwegian, partly because my high school philosophy teacher had recommended him—later I came across Aarne Naess's philosophical contributions to ecological thinking, and realized that this work would also relate

to the development of ecolinguistics). The course on the philosophy of science was also very interesting, and later helpful to me in thinking about the conditions for SFL as a science of language. It was a good period to be there and to get a sense of the different strands in philosophy—largely Anglo-American philosophy at Lund University at the time. Then of course, I had to try to make sense of it all, and relate it to what I was learning in linguistics. One helpful book that I consulted at the time was actually by a team of Swedish linguists, Allwood, Anderson & Dahl (1977), which was really one of the first introductions to logic for linguists.

When I studied linguistics at Lund University, there was still an echo of **Hjelmslev**. As an undergraduate student, I joined a study trip to the University of Copenhagen, with one of our teachers, Thore Pettersson, as a guide. We talked to people there who had known Hjelmslev, and I was fortunate to get a sense of what the Hjelmslevian period had been like. He was still a rich source of insights—theoretical ones mainly, but he was of course still also valued for his work on case (Hjelmslev 1935; cf. the alternative presented by Bílý & Pettersson 1984). While I was still there as an undergraduate student, a new professor named **Bengt Sigurd**[6] (1928–2010) arrived as Malmberg's successor. His Ph.D. was on phonotactics, but he had developed a wide range of interests and he used the computer as a tool in his work and computational linguistic representations (e.g., Sigurd 1987, 1991). After I had moved to LA to study at UCLA, I met him a few times at computational linguistics conferences and workshops. At a generation workshop on Catalina Island, he entertained us on the piano with jazz music; and, in connection with another conference, we toured the area around Stanford University in my old Ford Fairlane (cf. Figure 3.3). I was quite lucky in having these intersections of different traditions.

Bo Wang: Did you face the dilemma of choosing whether to study literature or linguistics, like Michael Halliday did?

Christian Matthiessen: No, I had already got hooked on **linguistics** in high school, where the course ahead for me was set. After high school, like most young men in Sweden at that time, I had to do military service for almost one year—eleven torturous months.[7] What helped me through that awful period was a correspondence

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[6] https://sv.wikipedia.org/wiki/Bengt_Sigurd.

[7] It was a truly miserable experience. Listening to the officers in charge of our basic training, I remember thinking "if these are the values they embrace and spout, what is it that we are expected to defend?"; a very real-life illustration of the significance of the tenor parameter of context—power play, put downs, misogyny ... well, all the expected stereotypes. One lieutenant explained to us that Sweden was a neutral, non-aligned country; but we should understand where the actual threat came from: The Soviet Union. He also noted that there was no way that we could overcome the might of their military, but our task was to hold out for two weeks or so, making the prospect of attacking Sweden as uncomfortable and unpalatable for them as possible. A few years later I hiked down and up the Grand Canyon in one day with a former Israeli paratrooper—who showed no signs of fatigue after we'd returned to the top, and I realized what a picnic in the park my military service had been in comparison. Since then I've met men who've served in similarly awful environments, all in the name of patriotism. So I became aware of hideously twisted value systems early on. On the day I took the train from my hometown to report for duty at the air force base I had been assigned to, I felt miserable and sorry for myself. My mother's elder sister, Aunt Inga, happened to be on a visit

course with an institute called Hermods.[8] They delivered the first year in English linguistics—a component of it in a correspondence course and then intensive face-to-face teaching at the end of the year. As part of that, I was able to study English linguistics and some literature, British and American at the time (since this was before the general recognition in the educational system of the treasure trove of post-colonial contributions around the world).

Then, in the summer, when I had been liberated from the drudgery of doing military service, there was at least three or four weeks of intensive coursework at Lund University, which was absolutely lovely. They brought over some scholars from the UK, supplementing the local scholars. It was such a wonderful learning experience. I felt very much part of the intellectual family, more than I would have if I had just come in as a regular undergraduate student at the beginning of the new academic year. During that period, I read John Lyons's (1968) *Introduction to Theoretical Linguistics*, which was not easy-going for a 19-year-old, who was straight out of high school, but it was very intriguing. I worked my way through his book, gaining many insights (including his discussion of ergativity and of categorial grammar, which turned out to be a prescient inclusion on his part in view of later developments in linguistics and computational linguistics—among various contributions, let me mention Mark Steedman's work, e.g., (2000), since he drew on Michael Halliday's account of information structure). While I enjoyed literature and was interested in it—it was very central in my family background, there was never any question of my direction, which was into linguistics and language.

Bo Wang: How was linguistics in those days back in Sweden? Were there many linguists in Sweden?

Christian Matthiessen: There were quite a fair number of them. There was an interesting situation, and it struck me years later that it was very similar to the situation in China. The departments of the mother tongue (the national language, i.e., Chinese or Nordic languages) were very conservative and traditional. In Sweden, they even continued the pre-structuralist tradition, philology and so on. Whereas in the departments of linguistics and of foreign languages in Sweden, that was where you would find influences from abroad. There were various reasons for this, but one was simply what languages the scholars controlled in terms of reading. If you were in a Department of Nordic Languages or a Department of Chinese, chances were you did not, on the one hand, actually know the foreign languages that could be carriers of new ideas to be taken on board; on the other hand, such literature would largely not

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from Germany, where she had been born in 1903 and spent all her life. She told me not to feel sorry for myself. She had lived through two world wars, and lost a son on the Eastern Front in The Soviet Union, as well as her husband. Somehow her words didn't comfort me!—I have thought of Aunt Inga in the context of your interview. When she was in her 60s, she told me that she had wanted to write her autobiography, just for the family, chronicling her own life through a period of dramatic terrible European history. But, she said to me, when she had mentioned this idea to her daughter Christa, her daughter had said—"Why, who would be interested?"; so my aunt abandoned her idea, which I think is very sad.

[8] It still exists today: https://www.hermods.se.

engage with Chinese or Nordic languages. The innovation happened in linguistics departments and in the departments of foreign languages, e.g., English departments.

Bertil Malmberg once said, thinking back as far as the 1930s: "I had to fight for the new ideas and new insights coming from European structuralism. One day I was standing on the barricades of the revolution of European structuralism to make it really energizing and stimulating in its engagement with language. But the next day I was *passé* because of the so-called Chomskyan revolution." He felt that people in many places leapt over European structuralism and went straight into Chomskyan linguistics, but he maintained that it would have been very helpful for them to go through a structuralist stage first. But certainly, in the Nordic departments, there were also people who were into generative linguistics of the day, like Christer Platzack.

2.3 Studying Linguistics at University of California Los Angeles

Bo Wang: After studying at Lund University, you went to the US and studied at the University of California Los Angeles (UCLA).

Christian Matthiessen: Yes. I was lucky in that I got a scholarship through an exchange programme between Lund University and the University of California, and I was awarded a one-year scholarship to study at one of the branches of the University of California. I looked at the linguistics programmes of the different branches, and for me the essential choice was between Berkeley and UCLA. Berkeley would also have been intellectually rewarding because there were interesting linguists like Wallace Chafe, Charles Fillmore and George Lakoff. But I was very keen on **language typology**, **African linguistics**, and the indigenous languages of North America, and these areas were strong at UCLA. That was a key reason why I chose UCLA. I was lucky to get admitted. I guessed they felt that they would just admit me for a year, which would not be too much of a risk.

Because I was admitted in this way and since **Peter Ladefoged** (1925–2006) was head of the department at the time, I was automatically assigned to him as my supervisor. I felt very lucky because even though I did not go into phonetics, I always found it interesting. Peter Ladefoged was a great scholar and a very interesting and nice man. I was very lucky to get to know him in that way.

Peter Ladefoged had moved from Edinburgh, which had been a centre for exporting great linguists. In phonetics, David Abercrombie was there, whom sadly I had never met, but he was obviously an important and inspiring phonetician. There was a period when Michael Halliday, Ruqaiya Hasan (as a Ph.D. student), Ian Catford and Peter Ladefoged were all there at Edinburgh. Allan Davies went there a bit later after Halliday and Catford had left. Allan Davies continued to develop applied linguistics at Edinburgh University, staying there after his retirement. To the extent that they "exported" Halliday to London—and later to Sydney, Ladefoged to Los Angeles, and Catford to Michigan, it can be said that they have really made a huge

contribution to linguistics around the world. And Allan Davies also moved around, spending time in Melbourne and coming to visit us at PolyU when I was Head of Department.

Bo Wang: What was your MA thesis about?

Christian Matthiessen: It was about the grammatical and semantic interpretation of the system of tense in English, but let me give some background first. I knew SFL from my days at Lund University, and, in fact, the new professor of linguistics, Bengt Sigurd, encouraged my interest. He said it would be very good to have an expert in this area. He thought that studying SFL would also be good for the linguists in Denmark, who were intellectually still suffering from what he called a "post-Hjelmslevian hangover" after Hjelmslev's powerful intellectual presence. That was very prescient of him, and he encouraged me to write a mini-thesis of around 60 pages, which I named *Hallidayan Linguistics*. I am not sure if I was aware at the time that SFL was the general name. In her two-volume introduction, Berry (1975, 1977) had used the term "systemic linguistics" and her account still reflected the state of development around the mid-1960s, before the functional organization had become pervasive in the systemic functional architecture of language.

It is now difficult to imagine the challenge of finding information in the pre-Wikipedia days and pre-WWW days more generally. Of course, there were libraries, but there was no equivalent of instant web searches enabling one to do an armchair rapid profiling of fields of knowledge in five minutes. At that time, one might have taken days or weeks, and the picture would still be incomplete—partly because much of the crucial information providing bridges between islands of knowledge was only available in the oral cultures of different communities of scholars. I did not know where Michael Halliday was, nor did I know that he had been a linguistic vagabond for a number of years. I had to piece everything together. If I had had somebody around with insider knowledge, then I could have asked, but I was all on my own since there was no-one pursuing SFL in particular. So, that was part of the background. In my case, I was only able to make certain connections and obtain certain mimeographed papers when I took advantage of cheap flights from Copenhagen to London in a few consecutive Januaries in the late 1970s before heading for LA and was lucky enough to meet Dick Hudson at UCL—and to see Douglas Fairbanks Jr., Penelope Keith, Tom Conti, Alec Guinness, John Gielgud and some of the other great actors of that era. (One highlight was seeing John Gielgud at the National Theatre in Julian Mitchell's play called "Half Life" in 1977.[9] To me, the play was fascinating not only because of Gielgud—much admired in our family; my mother had seen him as Hamlet staged at Elsinore Castle in the 1930s,[10] but also because of the theme—the

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[9] See   e.g.,   https://www.reportdigital.co.uk/stock-photo-half-life-written-by-julian-mitchell-starring-john-gielgud-national-photojournalism-image00083053.html.

[10] Michael and I both loved plays and the experience of going to the theatre, and we discussed the staging of Hamlet at Elsinore Castle. Once he had had an opportunity to visit it, he laughed and said that it was of course nothing like what Hamlet's castle would have been like—which is absolutely accurate, historically. Still, it must have been a great setting for the play, and years later after WWII, Hamlet was again staged at "his" castle in Elsinore.

connection between scholarship and technology in research, which has been very relevant during my life in linguistics. Another highlight was getting Alec Guinness' autograph after a play in the West End (1977), Alan Bennet's "The Old Country". Guinness was very gracious, and it was a great thrill to meet him; but I got a sense of dignified reserve. Before he emerged from the theatre, a couple of us had been talking to another actor, John Phillips, also quite well-known, and he was very loquacious, telling me about appearing in Malmö, commenting that the city theatre stage was really far too large—which I could understand, since I had seen productions there.[11] When Guinness emerged, our attention turned to him, and this other actor melted into the background, giving me the impression that he knew that we were really all there for Guinness … That will always be a reminder to me of the fundamental importance of attending to everybody, not only the "stars".)

I had become familiar with Halliday's treatment of the system of **English tense**. While his systemic functional descriptions often depart from traditional descriptions, in this case, his description of tense made sense of the tradition in Europe, where in the description of various European languages (including English), the traditional grammarians had produced a fairly rich account of tense because they had moved across from Latin drawing on its verb conjugation (cf. Michael 1970). The different verbal categories had then been projected onto English, which brought out certain features of English. It was also misplaced by missionary grammarians and others in trying to find the Latin tense system in languages around the world, just as they looked for the Latin case system. That is why people coming from the Western linguistic tradition did not understand how the modelling of time works in Chinese. (It is also, incidentally, why linguists describing "tense languages" such as English got it wrong once they had learned about aspect, and interpreted secondary tenses in English in aspectual terms.)

By 1980, I had met Michael Halliday, and had attended an intensive ten-week lecture series he gave at UC Irvine in the spring of 1980 (see Sect. 1.2). At some point, I was beginning to get the draft versions from him of the *Introduction to Functional Grammar* (*IFG*) (Halliday 1985). Also, in my readings in the philosophy of language and in logic, I had come across work on temporal logic, including a book by **Hans Reichenbach** (1947). I explored the interpretation of tense in **philosophy and logic** further, and understood how he had treated tense in terms of an embryonic temporal logic (prior to Prior 1968). He had taken the account from traditional grammar influenced by the Latin system (cf. Michael 1970), and then interpreted it in terms of the three times, i.e., S (speech time), R (reference time) and E (event time), which was a kind of proto-tense logic or proto-temporal logic. However, he did not go beyond the tense system recognized by traditional grammarians (nor did the many scholars who built on his account later); the traditional account was not tested against actual texts in context.

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[11] This is, incidentally, one of the public places where my father's work is on prominent display in the upper foyer of the Malmö Stadsteater (now Opera)—a wall of scenes from Shakespeare's plays. See e.g., https://www.malmoopera.se/en-midsommarnattsdrom-pa-malmo-opera.

My sense of breakthrough was this: coming to understand Halliday's (e.g., 1976, 1992) grammatical account of tense as a logical system—serial tense, I realized that the Reichenbachian model was a constrained special case of a semantic model of serial tense based on serial time—built up from the "now" of speaking (Reichenbach's S) as a series of temporal relations, with an unlimited number of reference times. As this realization dawned on me, I had the sense of an "a-ha Erlebnis"—a sudden revelation. This was why Reichenbach (e.g., 1928, 1947) was on the right track; but he was wrong because he constrained it to configurations of three times instead of a logical series of tenses. There were people around who were doing a lot of work with Reichenbach in computational linguistics. They never problematized what the actual system of English was like. One reason for that was: unlike Halliday, they did not attempt to work with naturally occurring text, in particular casual conversation, where Halliday found the most intricate tense selections (e.g., Halliday 1992/2003: 369). And I went through Svartvik and Quirk's (1980) London–Lund corpus, identifying various fairly intricate tense selections. Here are some examples that I included in Matthiessen (1995: 740):

present-in-past-in-present

*A : So when is this thing scheduled to produce results, Frank? B: Oh, it's **been producing** results for a long time*

(CEC: 482)

present-in-future-in-present

*Yes, but he couldn't arrange for possible timetabling because the timetables of these students is not worked out until well into the term, until they know what they're doing, you see. They never know in the long vac or in the summer what they **are going to be doing** the next year.*

(CEC: 839)

present-in-past-in-past

*when I'**d been teaching** apprentices at Vauxhall I could have gone straight there but I just couldn't get there*

(CEC: 511)

To illustrate the interpretation of the tense system as a logical system in terms of serial time, I'll use Halliday's well-known example, *it'll have been going to've been tested every day now for a fortnight soon*, presented in Halliday (1992/2003: 369) (see Table 2.1):

The verbal group of the clause that the dialogic interaction leads up to is analysed in Fig. 2.1. To interpret the serial tense semantically, we need six times (t_0 through t_5) rather than Reichenbach's three times (S, R, E). In this example, his S corresponds to t_0 and his E to t_5; and his reference time, R, could in principle be any of the times in between, but we translate it as t_1, indicating that this is where his model runs out of descriptive steam when it is being tested against naturally occurring text.

In contrast with Reichenbach, Bull's (1963) approach to the **semantics of tense** involved more than just a configuration of three times, as can be seen in his description of Spanish and other languages, and his account was closer to Halliday's in bringing out the serial nature of tense. However, in spite of a good beginning, he went off the

Table 2.1 Example of the tense system as a logical system in terms of serial time

	Tense
Can I use the synthesizer?	
Well I'm afraid we *use* it ourselves in the morning.	Present
What, every morning? *Are* you *using* it now?	Present in present
Yes,'fraid so.	
How about this afternoon? *Are* you *going to be using* it then?	Present in future in present
No. But this afternoon's no good.	
Why not?	
It'*s going to be being tested*.	Present in future in present
Come off it! It'*s been going to be being tested* for ages.	Present in future in past in present
It'*ll've been going to've been being tested* every day for about a fortnight soon.	Present in past in future in past in present

track at some point. I talked to Halliday about this, and he felt so too. Continuing contributions from philosophy and logic, Prior (1968) proposed a temporal logic, supplementing work on modal logic.

Thus against the background of contributions by linguists and logicians, I got involved in the late spring of 1980 in research into the semantics of tense as a researcher in the computational linguistic project at the Information Sciences Institute (ISI) associated with University of Southern California. I had to think about the more explicit modelling account of the semantics of the different parts of the grammar. So, I chose that as a topic for my MA thesis, which I completed in 1984.

When I was doing my MA thesis, I had two supervisors—one was **Sandy Thompson** and the other was **Ed Keenan**. Ed Keenan was well-known as a language typologist and a specialist of Malagasy (e.g., Keenan & Ochs 1987; Keenan 1976— among many publications), the westernmost Austronesian language, spoken on the island of Madagascar, but he was also a leading linguist in the area of formal semantics (e.g., Keenan & Faltz 1984; Keenan 2018). Ed had been influenced by **Richard Montague** (1930–1971), who had worked in the field of logic at UCLA in the Philosophy Department.[12] Montague (e.g., 1974) set out to develop a kind of formal semantics for a fragment of English, and took over the approach that **Alfred Tarski**

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[12] I never studied there, but I knew it was an interesting productive department. A fellow linguistics PhD student, Filippo Beghelli, once took me there and introduced me to the great Alonzo Church (1903–1995), the inventor of lambda calculus. I was impressed—I knew that this calculus could be used in providing a formal semantic analysis of relative clauses. Filippo was a wonderful friend, and I was always amazed at how easily he mastered the representational system of formal semantics. I still remember one occasion vividly. Ed lived in Venice Beach, and Filippo had driven to his place to pick him up to go to the airport, with me tagging along—Ed was off somewhere, maybe to Israel, where he also had an academic position (until, as I recall, he was disinvited because he'd spoken up in support of the Palestinians). Filippo's little car was full of animated linguistics discussion, and he dashed across an intersection although the light had turned red. Ed pointed this out to him, and

will	have	been going to	have	been	being	tested
α	β	γ	δ	ε	ζ	η
future	**past**	**future**	**past**	**present**	passive	event

t0

future ↘ *will*

t1

past ↘ *have v-en*

t2

future ↘ *been going to*

t3

past ↘ *have v-en*

t4

present ↘ *been being*

t5
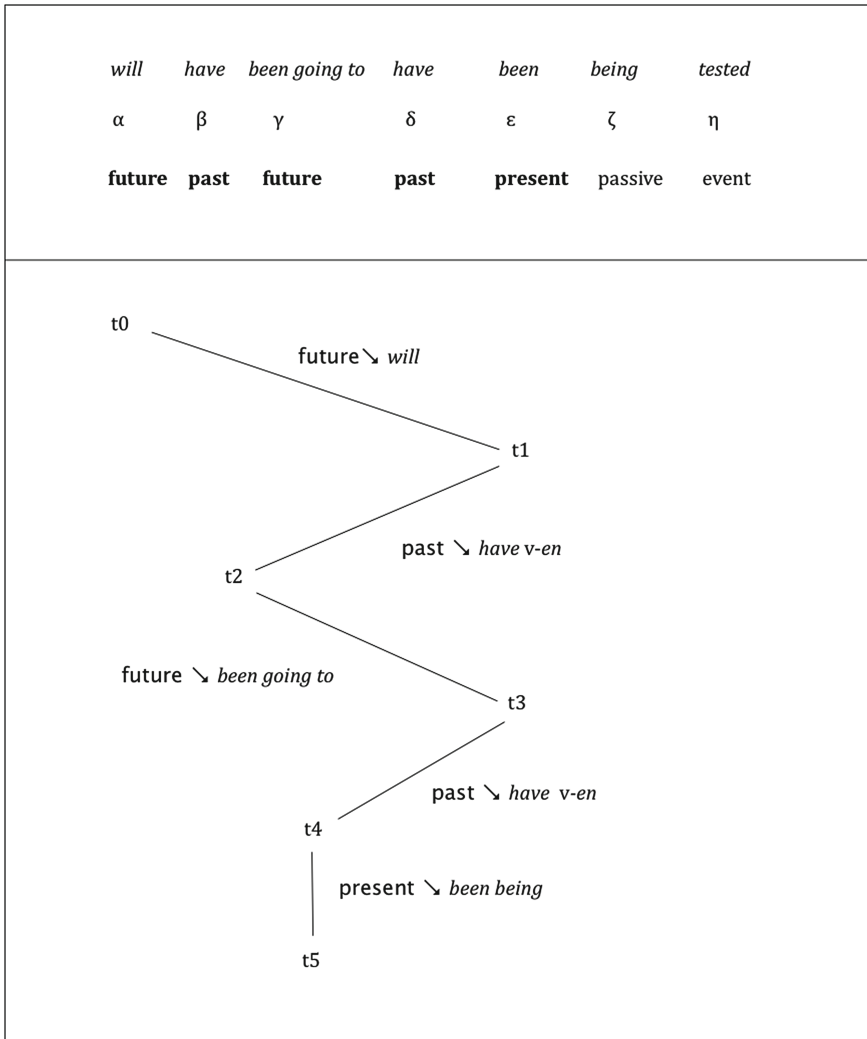
Fig. 2.1 Analysis of *will have been going to have been being tested*—logical grammatical analysis of the verbal group and semantic analysis of the serial tense in terms of serial time, with t$_0$ as the "now" of speaking and t$_5$ as the time of the event of testing

(1901–1983), who had also lectured at UCLA, had developed in the 1930s to form a model of theoretical semantics for predicate logic. Montague was convinced that he could do this at least for a fragment of English. Montague was murdered in 1971, so he did not live to develop his seminal ideas. But, in any case, Keenan was influenced

Filippo stopped and began to apologize profusely. Ed grinned jovially, and said: "I didn't mean for you to stop—just honk!".

by his work, and he was one of the people who made a linguistic sense of the formal model of theoretical tools, which was a really great contribution.

I only had one session with Ed Keenan when he was my MA supervisor, but it was such an enlightening session because he immediately saw that I was interpreting Halliday's grammatical account of tense in semantic terms of temporal variables. I remember this and I always think about this in terms of supervision of students doing thesis. Students should not only think about the quantity of interaction with supervisor, but also the quality, which depends on the supervisor and very much on what the students do. So, I would never have wasted Ed's time with all sorts of admin stuff, but he was certainly spot-on with the research issues. I remember Ed said: "The thesis fits very well, and it is very neat for tense, but I wonder what could be done for aspect." I was not able to take the exploration of aspect further, but it is certainly an interesting and open question. Naturally, a great deal of research has been done on tense and aspect since the early 1980s!

My MA thesis was actually published as a special kind of report by the Information Sciences Institute (ISI) (Matthiessen 1984). Then, thanks to Sandy Thompson, one aspect of it was published in *Studies in Language* (Matthiessen 1983). The editor, John Verhaar, whom I have never met, wrote to Sandy Thompson: "We have a gap. Do you have an article that could fill the space?" Sandy replied: "Yes, how about this?" .

I had intended to continue working on tense, not in terms of my Ph.D. thesis because I was actually keen on doing something more typological, but I was thinking of a book, and I wanted to locate this systemic functional interpretation that resonated with some informal variant of tense logic in relation to the other accounts of tense that were around. Michael Halliday was very supportive of this idea; I would certainly have to take account of all the work that has been done since the 1980s—but his account of serial tense representing serial time still stands out as a unique contribution.

The history of the interpretation of tense in English is quite interesting, reflecting different phases of linguistic theory. As already noted, **traditional grammarians** recognized a reasonable number of tenses in English thanks to their experience with descriptions of Latin, which is very well documented by Ian Michael (1970) in his book dealing with the history of the tradition of English grammars up to the early nineteenth century. Then, when the **structuralists** (in particular the US American structuralists) took over, they adopted a narrower view of the tense system, based on considerations "**from below**" (in the sense of Halliday's [e.g., 1978] trinocular vision on language). Their view was focussed on word rank; they only saw the morphological contrast between "past" and "present" (or "past" and "non-past"). Thus the three-term contrast in primary tense—past, present and future—was reduced to two terms, past versus present (or past vs. non-past). Because future tense was expressed in a different way—not by verbal morphology, but by an auxiliary, this was a major distinction in their view. In contrast, if you move in **systemically** and view the system of tense trinocularly, it does not matter whether systemic contrasts within the verbal group are realized at group rank or at word rank. There are many systems like tense where the realization is spread across ranks, and this is likely to change over time: the literature on grammaticalization has illuminated the drift down the rank

scale of items realizing terms in grammatical systems—an insight already reflected in Givón's (1971) well-known adage "today's morphology is yesterday's syntax". Halliday and other linguists have pointed out that languages go through cycles (see e.g., Dixon 1997). So, it is quite interesting to get a sense of what different theoretical angles will allow you to see or not to see aspects of a certain phenomenal domain.

After the submission of my MA thesis in 1984, I thought I should stay on at UCLA to do a Ph.D.. Sandy Thompson kindly agreed to be my chief supervisor, and I was very excited to do something in the area of functional typology.

2.4 Christian Matthiessen's Ph.D. Thesis on Text Generation

Bo Wang: Could you introduce your Ph.D. thesis *Text-generation as a Linguistic Research Task* to us (Matthiessen 1989)?

Christian Matthiessen: Well, it was not the most exciting of titles. I was pushing on with this notion that the Ph.D. should be in the area of **language typology**. For example, in 1979 and 1980, there was a seminar taught by **Sandy Thompson**, which was on the work she had done with **Paul Hopper** on their **transitivity hypothesis** (Hopper & Thompson 1980, 1982). I found it very interesting. They presented evidence from different languages, and their hypothesis was intended as a typological generalization based on ten transitivity parameters that tended to be correlated in their values. When I read it, I thought that this was so systemic because I had read Michael Halliday's (1967/1968) "Notes on Transitivity and Theme". So I produced reflections on this connection—ten or eleven pages of handwritten notes—and gave my observations to Sandy, pointing out the similarities. She said that my comments were interesting, but there was no further development.

I worked on the manifestation of these transitivity parameters in nominalizations in languages around the world—more specifically in **process nominalization**, and I wrote up a paper documenting my findings. Then, based on their seminal article, Paul Hopper and Sandy Thompson (1982) were co-editing a volume on transitivity, and those of us who had taken part in Sandy's year-long seminar were encouraged to contribute. But the publisher, Academic Press, said that they only wanted famous names on the whole, reducing the number of student contributions to one or two. But both Paul Hopper and Sandy Thompson encouraged me to revise my paper as a journal article and to submit it somewhere. Unfortunately, I never got the time because by then I had to earn my living by working as a research assistant and then a research linguist on a project on text generation (see Sect. 5.3). I still regret that I did not revise the paper for publication because it would have been quite a unique, interesting and substantial contribution to the typological literature. Essentially what I was able to show was that nominalization lowers the transitivity values along these

different parameters, and now I could make a systemic functional sense out of it.[13] I suspect that I have lost the paper because I would not have an electronic version and I have moved around so much. So, I did not try to look for it for inclusion in my collected works.

At some point, I had to come to the realization that I did not have the time and the energy to pursue a Ph.D. that was so different from my work on the **text generation** project at ISI. If the job had been in McDonald's, it would not have been that stimulating, but it would not have occupied much of my brain space. So, I had to decide to switch the Ph.D. to something that resonated with what I did in the research at ISI. My sense was that the work in computational linguistics and text generation could be more informed by linguistics and in my case by SFL, so I made that part of the focus of my Ph.D. research, calling it *Text-generation as a Linguistic Research Task*. Importantly, I thought about the account of language not only from the point of view of analysis of texts and description of the linguistic system, but also from the point of view of the generation of texts—of how one would synthesize texts, modelling the generation of text in a way informed by linguistic theory (cf. the method of analysis by synthesis in certain disciplines). So, it was intended as a dialogue between computational linguistics and SFL. In terms of Michael Halliday's (1978) trinocular vision, my vision for the Ph.D. was from below and from above. What was written up in the Ph.D. thesis was **from below**, moving stratally from lexicogrammar to semantics (cf. also Matthiessen 1987); but I was also working on the move **from above**, i.e., from context to semantics. Again, I had to take a decision to make the thesis manageable, so I completed and submitted the first part, not the second part based on the view from above, from context. But I was very interested in moving in from above, and I also wanted to show the value of the complementarity of the two approaches (cf. Matthiessen 1990). But 2,000 pages would have been even more absurd than 1,000 pages. (The submitted version of Ph.D. thesis, Matthiessen 1989, was a little over 1,000-pages long).

I was extending the thesis-study period year by year to retain my visa status and to remain in LA. I was not on the path to green card, partly because otherwise I had to have a full-time job, so I did not get a normal student visa, but a J visa. As long as I did not submit my Ph.D. thesis, I could then retain this visa. Suddenly the opportunity of a job at Sydney University came up, and that changed the situation entirely, because that meant I should complete the thesis as quickly as I could. That is part of the explanation for why the thesis stretched to 1989, because that was my way of staying in the US.

I remember discussing this with Sandy Thompson, and she said it was good to think about two versions of the thesis, the ideal thesis and the expedient one. She said it was absolutely fine if I chose to do an expedient thesis. I did not cut corners, but scaled back on what I envisaged, and produced a thesis that could pass, but of course it was different from my own original goals.

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[13] I undertook my study before I had learned about Michael Halliday's account of grammatical metaphor, which provides additional insights into process nominalizations (cf. Matthiessen 1989).

2.5   Influence from West Coast Functionalism

Bo Wang: Have you been influenced by **Sandy Thompson** and **West Coast Functionalism**?

   Christian Matthiessen: You could say the influence is ongoing. Sandy was a wonderful supervisor and is a great person. She has been very supportive in various ways, and has continued to produce very interesting work. As I said, I was very keen on working on different languages, empirical evident-based typology, and generalizations about languages, and this was an important strand in her research. We were very excited when her co-authored *Mandarin Chinese: A Functional Reference Grammar* appeared (Li & Thompson 1981). I had a copy, with a striking red cover, and Sandy said, alluding to Mao's Little Red Book: "Of course, the cover had to be red". That was exciting, as was the much later publication of Thompson et al. (2006) description of the grammar of Wappo, the result of a project that had started much earlier. Sandy has, of course, published widely in the areas of the **description of Chinese**, **functional language typology** and **discourse and grammar** (including grammar and interaction). And Bill Mann, the project leader, brought her in as a consultant on the text generation project to work on text planning. That was how we came to work together and to develop **Rhetorical Structure Theory** (RST; e.g., Matthiessen forthcoming). It was a very productive period (Fig. 2.2).

Fig. 2.2 Sandy Thompson (left), Michael Halliday (middle) and Jack Du Bois (right) at Santa Barbara

For quite a long time, in the 1980s and into the 1990s, I thought that there would be a great potential for a dialogue between West Coast Functionalism and SFL. I did feel frustrated at the lack of interaction (not because of Sandy, who made various dialogic contributions, but because of the community in general). I wondered why the West Coast Functionalists did not refer to systemic functional work and why they did not refer to Michael Halliday, going back to his seminal work in the 1960s. Even in works on the transitivity hypothesis (Hopper & Thompson 1980, 1982), there was no engagement with Halliday's (1967/1968) conception of transitivity. Interestingly, West Coast Functionalists were happy enough to go to the Prague School and, to some extent, to Tagmemics.

Part of the reason for the lack of West Coast Functionalist engagement with SFL was that West Coast Functionalists and Systemic Functional Linguists had different goals. Systemic Functional Linguists studied text (discourse) in context because they were interested in text in context. They took text as the primary unit of language because if one conceived of language as a meaning potential, text was then very significant as the unit of meaning in context. For the West Coast Functionalists at that time, they studied text or discourse to explain grammar, but text or discourse was not the primary focus in its own right. (In that sense, RST was an exception.) If you look at the works at the time, they got into narratives because that was the way of understanding tense and aspect systems (e.g., Hopper 1982) and arguably the later engagement with CA (Conversation Analysis) was also characterized to a large extent by attempts to explain grammatical properties (e.g., Ochs et al. 1996).

Another difference was that SFL has always been an **appliable** kind of linguistics (going back to its roots in the 1950s, when Halliday had not yet introduced the term) (e.g., Halliday 2002a, b, 2008; Matthiessen 2012, 2014). So, Michael Halliday, Ruqaiya Hasan, Jim Martin and others were very interested in developing a kind of linguistics that could actually be used outside linguistics, even outside the institutional realm of the university, to address various problems in the community (centrally including educational problems but also many other problems). The computational work can also be interpreted along these lines. In contrast, appliability was not on the agenda of the West Coast Functionalists, who were not in the business of developing a kind of linguistics that could be used to address problems arising in different areas of the community. That was not their primary or even secondary concern.

Also, there was the tradition of **Conversation Analysis** (CA) at UCLA. To a large extent, CA had been developed in sociology, first with Harold Garfinkel (1967) at UCLA and then in the next generation with Harvey Sacks (e.g., 1974, 1992a, b), Emanuel Schegloff (e.g., 1979) and Gail Jefferson—a foundational oft-cited contribution being Sacks et al. (1974). When I arrived at UCLA, Garfinkel was still working in the Department of Sociology, and his work inspired the development of CA, with its emphasis on micro-analysis as part of his framework of Ethnomethodology, although he was not doing Conversation Analysis per se. Sacks, who Michael Halliday thought was the most creative person involved in Conversation Analysis, had tragically died in a traffic accident in 1975. So, it was really Emanuel Schegloff, who had a very

close attention to detail and a kind of aversion to theorizing, that developed Conversation Analysis with UCLA as his base. He did the micro-analysis of encounters, and was careful not to posit anything that you could not demonstrate based on the data. In positing categories, you had to demonstrate the interactants were actually oriented towards them; they could not be assumed for analytical convenience (or reasoning in the philosopher's armchair, as in the case of speech act theory). Coming from functional linguistics of the systemic kind, one would of course take this important attention to authentic data for granted (cf. Firth's [e.g., 1957, 1968] insistence on the need for **renewal of connection** with data). But if one came from macro-sociology—still dominant in the early 1960s, then taking the micro events and micro activities seriously was something you had to argue strongly for. (Compare also the later debates between CA and CDA in the second half of the 1990s.)

While CA and West Coast Functionalism developed independently of one another in and around UCLA, they began to interact around the mid-1980s. Some Ph.D. students of Sandy's sat in Conversation Analysis classes in the Department of Sociology with Emanuel Schegloff, and built this into their Ph.D. research: Fox (1987) and Ford (1993). This led to a strand in West Coast Functionalism, which was very much concerned with looking at the grammar in terms of micro-encounters, i.e., developing over seconds in time.[14] If you look at the book co-edited by Elinor Ochs, Emanuel Schegloff and Sandy Thompson (1996), it is not so much about the interpersonal, but is really about the grammar and what happens when you follow it in turn-taking.

The engagement with Conversation Analysis meant that to some extent, West Coast Functionalism moved away from theorizing language as a system since conversational analysts have been very wary of making generalizations that depart too far from the instantial patterns they observe in texts in contexts and also of developing general theory, emphasizing methodology instead. (One could certainly engage with this systemic functionally, but systemic functional linguists might have thought that they were losing the plot. At least they were not doing the kind of linguistics that was useful beyond itself.) West Coast Functionalism was not something that you could take to education and address educational issues or to healthcare to address communication problems or to other institutional sites where problems that arise can be diagnosed and treated by means of linguistics. (Around the mid-1980s, there emerged a tension in the community of conversational analysts who were keen to apply CA in various institutional settings and those who resisted potentially premature application. In the decades that followed, CA has of course been applied to the study of dialogue in a variety of institutional settings, including applications in healthcare.)

So, there was another kind of divergence that emerged in the 1990s. It took quite a while for me to discover that it was actually not all about the **field** of doing science

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[14] In this context, it is helpful to draw attention to Halliday's (1961: 254) observation that language is "patterned activity", e.g., "The unit being the category of pattern-carrier, what is the nature of the patterns it carries? In terms once again of language as activity, and therefore in linear progression, the patterns take the form of the repetition of like events." He argued against the "bricks and mortar" conception of structure. This insight has guided systemic functional work since then.

in general and of doing linguistics in particular. I had naively thought that if things fit together in terms of field from different traditions, then people from these different traditions would interact and refer to one and another. It also took me quite a while to realize that **tenor** was very important, i.e., the tenor of relations in networks of people in a given tradition and the citation networks they create, was very important—sometimes or even often having more influence than considerations of field. If you are not in a given academic network, then it is much harder for you to be heard by members of that network. And as soon as we take tenor into consideration, we also begin to recognize differences in access to academic power (reflected in various ways, e.g., sources of funding, Ph.D. positions with scholarships, and publication venues like journals and book series), dominant groups having much more access. In some publications, Sandy Thompson and I tried to make references showing resonances between West Coast Functionalism and SFL. Maybe it was a valiant attempt, but I can't say that it was successful.

But reasons for trying to nurture dialogue across tenor-based networks still exist. For example, at the symposium on language typology at Shanghai Jiao Tong University in December 2015, Jim Martin had invited Nick Enfield from the descriptivist tradition in Australia that had been influenced by West Coast Functionalism as a way of trying to make connections. They drew my attention to an edited book by Ameka et al. (2006)—*Catching Language*. It includes a helpful introduction by Evans & Dench, who write (p. 15):

> Within the broad characterization given above, one asymmetry is particularly important: that between analytic / decoding / semasiological / form-based grammars, that take as their starting point forms or constructions in the target language, and synthetic / encoding / onomasiological / meaning-based grammars, that start from particular meaning categories (e.g., tense, or space, or causality) and show how grammars – in conjunction with the lexicon, where necessary—allow meanings within these fields to be expressed.
>
> The ideal of having both types of grammar for any given language goes back to Gabelentz (1891), and Mosel's chapter discusses the history of this bidirectional conception, and the problems faced in seeking to implement it. But for many reasons comprehensive meaning-based grammars have hardly ever been produced, with the honourable exception of Leech and Svartvik's (1975) 'communicative grammar of English', although some descriptive grammars—such as Seiler (1977) on Cahuilla or Wilkins (1989) on Mparntwe Arrernte—contain a sample chapter or chapters written from a meaning-based perspective, and there have been other interesting experiments in grammatical organization, such as Newman's 'encyclopaedic reference grammar' of Hausa, that includes some entries based on form and others based on meaning.

(This reflects two of the angles of approach in Halliday's, e.g., 1978, trinocular vision: "from below" and "from above"; SFL invites a third angle of approach, viz. "from roundabout".) It is interesting that they do not mention Halliday's pioneering work on functional, meaning-oriented accounts of grammar. The "honourable exception" that they mention, Leech and Svartvik (1975) was, in fact, inspired by Halliday's functional theory (e.g., Halliday 1973), as Leech and Svartvik point out.

In general, the lack of reference to Halliday is more than not just picking up on particular insights. It is more active than that in terms of maintaining the different communities and networks within different communities. You see this very strongly

in Chomsky—the tenor aspect of academic communities. If you are struggling, if you are trying to be heard and recognized, then there is an incentive for engaging with different folks. But if you have reached some degree of academic power, that incentive disappears. In terms of field, there could still be the incentive, like thirst for knowledge, but that often seems to be pushed into the background under the weight of tenor considerations. If it is career considerations that come to dominate, which is of course understandable, then it may not do you any good to refer to "quaint" scholars from another tradition or if you do, you brush them aside as being "idiosyncratic", "arcane", too complex or the like (cf. discussions by Jim Martin [e.g., 1992] of the "dismissal genre").

One could also make the case that Halliday and more generally SFL have come to be treated as **taboo** in terms of the experiential aspects of Mary Douglas's (e.g., 1966) theory of taboo as a way of dealing with phenomena that do not fit into established taxonomies (like shellfish and pigs). Thus SFL is an anomaly in the classification of approaches to languages: it is both theoretical and applied, it is both functional and formalized, it is both system-oriented and text-oriented, and so on. Given this status, SFL can be dismissed or effaced.[15]

2.6   Working at Sydney University

Bo Wang: After studying at UCLA, you went to the University of Sydney in 1988, and worked there as lecturer and later as senior lecturer from 1988 to 1994. What was the job like?

Christian Matthiessen: By the time I moved to Sydney University, Michael Halliday had just retired at the age of 63. He retired quite young, and he could have continued in his position longer. He told me that the dean had said to him he was very young to retire; but Michael wanted to create space for others and also to retire to be able to get on with research and writing,[16] academic activities already at that time being threatened by administrative demands. Luckily for me, I was his replacement (at the entry rank, of course, not at the professorial level). There was a position of lecturer in SFL to teach the courses he had taught. I knew a number of the other people who also

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[15] There are many instances of this throughout linguistics since the 1960s; here is just one fairly recent example provided by de Bot (2015: 58): "On the basis of comments made by the informants, a spectacular growth of SFG in AL (applied linguistics) is not expected, though Tim McNamara feels that the United States is ready for SFG. William Grabe disagrees: "SFG is not the solution. There is not enough empirical evidence. The theory is arcane, the terminology complex and the texts are often painful to read." When asked why Halliday never took off in the United States, he remarked jokingly: 'Because he moved to Australia!'" The remark about Halliday's move to Australia is, of course, quite revealing.

[16] I remember one of the occasions when he discussed his plans. We were on one of our hikes in the Santa Monica Mountains, and Michael told me hopefully he thought Jim Martin had a very good chance of succeeding him.

had an SFL background were interviewed for the same position I was in competition with, all fantastic SFL scholars.

It was a very interesting change of gears from my position as a research linguist at ISI. In addition to courses on grammar, register, context and discourse informed by SFL, I was given a computational linguistics course to teach. And only a couple of years after arriving in Sydney, I managed miraculously to get a funding for a project in text generation. The project was not a direct continuation of the text generation research at ISI, but I continued working on what I had started at ISI. First, I had produced a little document called "What's in Nigel" (Matthiessen 1988), in which I tried to document the systems in the computational grammar that we called the **Nigel Grammar**. The name was Bill Mann's idea. Nigel, which was the dialectal version of the real child's name, was the official name of the child in Halliday's (1975) ontogenetic case study of "learning how to mean". Bill Mann asked Michael Halliday if it would be OK to call the system's grammar the Nigel Grammar, and Michael agreed. I continued working on that document, and it eventually grew into *Lexicogrammatical Cartography* (Matthiessen 1995).

When I moved to Linguistics at Sydney University, what struck me very strongly was that for the first time I was in a linguistics department where **text analysis** was an absolutely central component of doing linguistics. (Thanks to the Hallidayan tradition, further developed by Jim Martin.) That was not the case at Lund University. They engaged with text using cohesion analysis in the research in the English Department, but they did not do the kind of text analysis that is now one of the mainstay activities of SFL. Also, in West Coast Functionalism, there was really no notion of that kind of text analysis; as I noted earlier, they engaged with text or discourse in the first instance in order to explain grammar, not as part of a development of a theory of text. (In that respect, our work on Rhetorical Structure Theory was different from the prevailing orientation in West Coast Functionalism. In the US context, it was more comparable to work in and around Tagmemics.)

Apart from the course of computational linguistics, I was teaching courses on grammar, using *IFG* (Halliday 1985), on discourse in context and on register. I co-taught some of the courses with Jim Martin, and this collaboration was truly helpful and wonderfully enjoyable, and it got me into the system there. But it was the tradition of engaging with **text** by taking text as an object in its own right that was really striking and fantastic. To me, there was a continuity with my earlier work: since at ISI I was trying to model text generation, I had to treat the text as an object in its own right, to exhaust the text in the account, not to pick out only certain features, but to address all the features—as in Martin's (1992) *English Text*.

The educational interest at Sydney University was an orientation I had not experienced before in linguistics, neither at Lund University nor at UCLA. At UCLA, there were two different linguistics departments, Linguistics and Applied Linguistics. By the time I left for Australia, Sandy Thompson and the other functionally oriented linguists had left UCLA and gone to Santa Barbara (Jack Du Bois, and later Sandy's Ph.D. student Susannah Cumming) and to Oregon (Talmy Givón). If you wanted to study linguistics of the sentence and below, you went to the Linguistics Department; if you wanted to study anything above the sentence, you went to the

Table 2.2 MA theses on areas of Chinese supervised by M.A.K. Halliday at Sydney University

SYSTEM	Reference
COHESION	Hu, Zhuanglin. 1981. *Textual cohesion in Chinese.* MA Honours thesis, University of Sydney, Sydney
CLAUSE COMPLEX	Ouyang, Xiaoqing. 1986. *Clause complex in Chinese.* MA thesis, University of Sydney, Sydney
TRANSITIVITY	Long, Rijin. 1981. *Transitivity in Chinese.* MA thesis, University of Sydney, Sydney
CIRCUMSTANTIAL TRANSITIVITY	Hua, Tsung Tie. 1986. *Circumstantial elements in Chinese.* MA thesis, University of Sydney, Sydney
MODALITY	Zhu, Yongsheng. 1985. *Modality and modulation in English and Chinese.* MA Honours thesis, University of Sydney, Sydney

Applied Linguistics Department. But in Michael Halliday and Jim's section of the department in Sydney, it was all there together—language was viewed ecologically as a whole in its environment. That was a very valuable experience, and that kind of focus continued after I moved to Macquarie University.

Bo Wang: By the time you moved to Sydney University, had the Chinese students supervised by Michael Halliday already returned to China?

Christian Matthiessen: Yes. Michael Halliday had quite a group of Chinese students in the early 1980s from mainland China, they later returned to their various universities, and really built up linguistics in China. They were themselves from the generation that had been quite badly affected by the Cultural Revolution, and some had been sent to do agricultural work in the countryside. In the late 1970s and the early 1980s, as China opened up again, they began to be allowed to go abroad. A number of them went to Sydney University because of Michael Halliday. They did their MA theses with him, which were very good ones covering a number of key systems of the grammar, as shown in Table 2.2. During one of my early visits to Sydney, I copied them all at Kinko's. There was not enough money or time for them to go on to do Ph.D.s, so they returned to China with MA degrees. These scholars include Hu Zhuanglin, Zhang Delu, Zhu Yongsheng and Fang Yan (she did not do her MA thesis, but she was part of the group, and developed the description of theme in Chinese, e.g., Fang 1989, 1993; Fang et al. 1995), whom I all came to know as friends and colleagues later on, and also Long Rijin, whom unfortunately I had never met. He had produced one of the best MA theses, on transitivity (see Long, 1981; Long & Peng 2012). These Chinese students built up linguistics in departments of English and Foreign Languages in China. They had returned by the time I arrived in Sydney. Then in the 1990s, more Chinese people arrived in Sydney partly because China was getting richer and people could afford to send their children abroad, even if they did not have full scholarships. But that first generation was very important, laying the foundation for future developments both academically and institutionally.

2.7   Working at Macquarie University

Bo Wang: How was your work at Macquarie University?

Christian Matthiessen: The switch from Sydney University to Macquarie was again because of the opportunity created by retirement; first Michael Halliday retired from Sydney University in 1987 and then Ruqaiya Hasan retired from Macquarie in 1993. They needed somebody to teach courses in her area, and they advertized the position. Fortunately for me, the job was at the level of associate professor, so I moved from Senior Lecturer at Sydney to Associate Professor at Macquarie.

Linguistics at Macquarie University was certainly quite attractive, but at the same time, my time at Sydney University had been difficult because the linguist who replaced Michael as professor, and also became head, Bill Foley, seemed determined to get rid of SFL. When he moved to Sydney, he had been at ANU several years after graduating with a Ph.D. from Linguistics at UC Berkeley, and it seemed as if he was on a mission to replace SFL with the kind of descriptive linguistics Dixon had helped develop in Australia. Two brilliant systemic functional linguists, Di Slade and Clare Painter, lost their positions after he had arrived. When I applied for promotion from lecturer to senior lecturer, he opposed the promotion, but was overruled by the university staffing committee of Sydney University.

When I moved to Macquarie, I naturally had mixed feelings. On the one hand, I felt bad because I left Jim Martin there on his own and of course also because working with him had been fascinating and very productive; but on the other hand, I had to think more generally about what I could do and what it meant for the community. My experience had been that in particular universities or departments, things would go up and down, you had to take a broader view to consider the dynamics and interplay of members at a whole group of universities within an area. As a community, Sydney was like Hong Kong, there was this extraordinary concentration of universities, where there were initiatives and activities that bud and blossom not necessarily in the same place. Just as in Hong Kong, scholars have moved from one university to another in the same city while remaining active and networked in the community.

When things were difficult in linguistics at Sydney University, Geoff Williams and Len Unsworth continued to forge ahead in education as did Rosemary Huisman in English. And Alice Caffarel got a position in French, and was able to develop systemic functionally informed courses, and do great research and publish her systemic functional description of French (e.g., Caffarel 2004, 2006). At the same time, we were able to continue to develop SFL at Macquarie University, building on Ruqaiya's foundation, and for quite a while the University of Technology Sydney (UTS) was also an important centre for SFL work, and University of New South Wales (UNSW) has emerged as a productive systemic functional university. What was important was to find ways of maintaining the momentum within the extended Sydney community, thinking of ways of developing collective knowledge (the collective discourse), and ensuring that if somebody left one of the places, you would not have a gap and you would have ways of having a more abstract conception of what constituted a working group.

It was not easy to maintain the momentum in Sydney and we all face similar problems all the time. The problem of continuity has been researched within the military—interestingly but unsurprisingly, since given their system of rotation and promotion, the amount of time that people spend as members of teams tends to be fairly short, which leads to the question of how teams can continue to operate smoothly and productively, retaining the local knowledge and cohesion. This is an interesting research problem in its own right, and one worth keeping in our focus of attention as academic teams can become more dispersed globally, thanks to technological developments, and must operate virtually under conditions such as those created by COVID-19.

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Chapter 3
Life and Work of Christian Matthiessen (Part II)

Abstract **Chapter 3** continues the discussions on Christian Matthiessen's life and work started in Chap. 2. We first highlight the works that Christian Matthiessen produced during when he was in Australia. Then we discuss the Ph.D. students that he supervised there and the motivations of relocating to The Hong Kong Polytechnic University. We also report on the influences on Christian Matthiessen by Michael Halliday and other linguists, Christian Matthiessen's research areas throughout the decades, and his unpublished works. Finally, we summarize his contributions to Systemic Functional Linguistics.

3.1 Introduction

This chapter focusses on the scholarly contributions of Christian Matthiessen. First, it discusses his scholarly output and research trajectories since the late 1980s, when he got his first academic position in the University of Sydney. It also discusses his contributions through postgraduate supervision in the University of Sydney, Macquarie University and The Hong Kong Polytechnic University. The chapter provides a comprehensive map of the key publications and Ph.D. theses supervision by Christian Matthiessen and outlines his ongoing projects. Notable issues discussed also include his critical reflection on institutional politics, the politics and power dynamics in citations, career decisions, institutional research evaluation and how these have affected both the development of SFL and his personal career.

3.2 Productive Years in Australia

Bo Wang: As you said, you have spent twenty years in Australia altogether. Were those years productive?

Christian Matthiessen: Yes, it was a productive time, 1988–2008. Interestingly, given the change in how universities measure productivity today in terms of superficial metrics, in those days, you could afford to be productive in a more fundamental way. It was a self-directed kind of productivity, one that allowed for long-term research programmes. I was productive because I loved what I was doing and found it interesting and rewarding — obviously very aware of Michael Halliday's (e.g., 1984) notion of **social accountability**, attention to which was very evident in SFL activities at both Sydney University and Macquarie University, e.g., in educational and clinical applications. If something new came up, I was able to go for it. For example, I was able to accept invitations to contribute to book chapters without a single thought about whether the books were published by a prestigious publisher or not. If I submitted something to a journal, I would only consider the quality and the potential contribution in terms of the ideas of the article. There was no sense of journal ranking in terms of A-star down to Z, citation indices, impact factors, and other forms of metrics that might be dreamt up. It was a time when I concentrated on the ideas and the development of research-based insights into language. Of course, we applied for research project funding, but that was driven by the interest in doing some piece of research, not because it was mandated by the university that we had to submit an application at certain intervals (like every year). Thus I was able to pursue ideas that had some lifespan — the notion of developing a comprehensive map of the grammar of English based on systems, i.e., *Lexicogrammatical Cartography* (Matthiessen 1995); see Table 3.1 for a chronological overview of my areas of activity since the 1980s. I also explored the idea of logogenesis and instantial systems (e.g., Matthiessen 2002, 2009). It was possible to explore a number of phenomena and issues without worrying about "the assembly line of publications".

It was a very different time. For me, this period lasted until 2008, when I moved to Hong Kong. It was only when I arrived in Hong Kong that the increasingly dominant emphasis on metrics really began to bite, Hong Kong following the UK in this particular aspect of academia. I am not saying that it is not happening in Australia now — it is (cf. Hil 2012). You often find that when you move around the world, you experience different phases of overall academic development in different places. One just has to be careful not to say that it is specifically Australian or specifically in Hong Kong because when one moves, one of course also moves in time.

In terms of major publications, there was the early work with John Bateman on documenting what we had done in the 1980s in **text generation** with English and Japanese (Matthiessen & Bateman 1991; Bateman & Matthiessen 1991; see Fig. 3.1 for a picture taken by John Bateman during a conference trip to Xi'an, China in 1989). That was very exciting. Then, my agenda was to expand the early notes I had in "What's in Nigel" (Matthiessen, 1988) — the computational Nigel grammar of English, and that turned out to be *Lexicogrammatical Cartography* (Matthiessen 1995). Concurrently with that, I worked with Michael Halliday on *Construing Experience through Meaning* (Halliday & Matthiessen 1999/2006), which came out of what we had been asked to do on ideational semantics in the 1980s by Bill Mann — the project leader of the text generation project at the Information

Table 3.1 Areas of research activity and publication

Area	Sub-area	1980s	1990s	2000s	2010s	2020s
SFL theory	General	[1979–1988: Los Angeles]	[1988–2008: Sydney]	M (2007) "Architecture"	[2008–: Hong Kong]	M (forthc.) "Architecture"
	Metafunction		M (1992) "Textual metafunction"	Halliday & M (2006) "Construing experience"		
	Instantiation: probability		M (1999) "Transitivity selection"	M (2006) "Systemic probabilistic profiles"	M (2015) "Halliday's probabilistic theory of language"	
	Stratification of metalanguage					
	Computational modelling	[1980–88: Penman project]		[2000–2005: Sugeno's everyday language computing project]		
		M (1988) "Representational problems"	M & Bateman (1991) "Text generation"			
Language description	English	M (1988)	M (1995) "LexCart"	IFG 3	IFG 4	
		M (1983) "Primary tense"	M (1996) "Tense"			
			M (1999) "Transitivity"		M (2014) "Process type"; Zhao (2016) "Identifying clauses"	

Table 3.1 (continued)

Area	Sub-area	1980s	1990s	2000s	2010s	2020s
		M & Thompson (1988)	M (1995) "LexCart"	M (2002) *Stuart-Smith (2001)*	M & Teruya (2015); *Zhang Peijia (2018)*	M (forthc.) Systemicized RST — book MS
	Niger-Congo: Akan, languages in West Africa	M (1987) "Akan lexicogrammar, phonology"		*Akerejola (2006): Òkó*	Mwinlaaru, M & Akerejola (2018) "Mood"; *Mwinlaaru (2017): Dagaare*	
	Semitic: Arabic			*Bardi (2008): Arabic*		
	Romance: French		*Caffarel (1996): French*			
	Indo-Aryan: Bajjika, Marathi			*Kumar [Kashyap] (2008): Bajjika*	[M: sketch of Marathi, based on secondary sources]	
	Sino-Tibetan: Chinese		*McDonald (1998): Chinese*	Li (2002): Chinese	*Fung Ka Chun (2018): Questions in Cantonese*	
	Japanese		*Teruya (1998): Japanese*		Ochi (2013)	
	Austroasiatic: Vietnamese		*Thai (1998): Vietnamese*			

(continued)

Table 3.1 (continued)

Area	Sub-area	1980s	1990s	2000s	2010s	2020s
	Tai-Kadai: Thai			*Patpong (2006): Thai*		
Multilingual Studies	Language comparison and typology		M (1995): typological outlooks in "LexCart"; Batman, M & Zeng (1999)	M (2004) "Descriptive motifs and generalizations"; Teruya et al. (2007)	Teruya & M (2015) M (2018) "Multilingual system networks"	M (in prep.) MS of multilingual IFG
	Translation Studies			M (2001) "Environments of translation"	M (2014) "Choice in translation"; Kim & M (2017); *Wang Yan (2015); Zhang Daozhen (2016); Wang Bo (2017); Ma Yuanyi (2018); Macdonald (2019)*	M (in prep.) MS text analysis for translation M, Arús-Hita & Teruya (2021)
Register Studies			M (1993) "Register in the round"		M (2014) "Registerial cartography"; M (2019) "Register in SFL"	*Liu Xiangdong (2020): projection across registers*
Multimodal Studies		M et al. (1998) "Multimodal generation"	M (2006) "Printed page", M (2009)		*Zhang Peijia (2018): multimodal analysis of public health posters*	

Table 3.1 (continued)

Area	Sub-area	1980s	1990s	2000s	2010s	2020s
Educational Linguistics				M (2006), M (2009)	M (2015b) "Reflections on Chinese"; *Xuan Wenhui (2015): longitudinal study of 2nd language writing; Guo Songdan (2015): multimodal analysis of textbooks of different grades; Zheng Yaofei (2019)*	M, Yousefi & Mardian (2022)
Healthcare Communication Studies			M et al. (1998)	Slade et al. (2008)	M (2013); Slade et al. (2015); *<Zhang Peijia (2018): public health posters>*	

Fig. 3.1 Christian Matthiessen (right) and David Gil (left) in Xi'an in April 1989 for the international conference on texts and language research (photo taken by John Bateman)

Sciences Institute (ISI). We had been working on it for a long time, starting in Bloomington, Indiana, in 1986.

Michael Halliday and I were also working on other projects. Fred C.C. Peng,[1] the publisher of *Lexicogrammatical Cartography*, had a grand publication plan lined up, which included a number of books. So, Michael Halliday and I worked on a book that was supposed to be an overview of Systemic Functional Linguistics (SFL). Unfortunately, Fred's project collapsed, but a couple of books were published, including my own *Lexicogrammatical Cartography* and a very good book on morphology by Lockwood (1993), who was one of the few people working with stratificational linguistics.

Other publications were planned. An offshoot from our overview of SFL was ***Systemic Functional Grammar: A First Step into the Theory***, which was uploaded on the web in the late 1990s, and then thanks to our friend and colleague Huang Guowen, the book was published in China in English with a Chinese translation and additional material by him (Matthiessen & Halliday 2009). But there were still things in that project that did not get published. Separately, Michael Halliday and I were working on ***Outline of Systemic Functional Linguistics*** in two volumes (Halliday & Matthiessen in preparation), which is on my publication agenda. We set the *Outline* aside to work on the third edition of *An Introduction to Functional Grammar* (*IFG3*)

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[1] See e.g., https://www.researchgate.net/scientific-contributions/Fred-CC-Peng-71843789.

(Halliday & Matthiessen 2004). That kind of interleaving of publication projects is, of course, typical of academic life.

There are also other projects Michael and I had started on or sketched. Before I return to them, I need to finish *A Guide to Systemic Functional Linguistics* with Kazuhiro Teruya (Matthiessen & Teruya in press), the first volume on my systemicized version of **Rhetorical Structure Theory**, which is done (Matthiessen forthcoming a), a book on the **architecture of language** according to systemic functional theory (Matthiessen forthcoming b), which is almost done. There are a number of book projects that are sitting around, having reached different degrees of completion.

Early on, during my years at Sydney University (1988–1994), I had the great opportunity to work with Jim Martin. We published one paper together on **typology and topology** as complementary ways of viewing paradigmatic organization (Martin & Matthiessen 1991). We had other things planned, including a comparison of Rhetorical Structure Theory (RST) and his approach to conjunctive relations (e.g., Martin 1992), for which we had a partial manuscript. Then, with the move from Sydney University to Macquarie University, I worked with David Butt and, with Annabelle Lukin's help, we put together a book on systemic functional semantics that was circulated in mimeo form by the turn of the century. There were also funded research projects, in computational linguistics. It was a very productive period.

3.3 Ph.D. Supervision in Australia

Bo Wang: Did you start to supervise Ph.D. or MA students when you were in Australia?

Christian Matthiessen: Yes, I did. (For a summary of doctoral students I have supervised, see Table 3.2.) The first Ph.D. student I supervised was Marilyn Cross (1991) — as an external associate supervisor. Her main supervisor, Ruqaiya Hasan, who was at Macquarie University, asked me if I would come in as an external supervisor while I was still at Sydney University. Marilyn finished her Ph.D. in 1991. The key reason for Ruqaiya to ask me was that Marilyn was modelling text generation from a systemic functional point of view. It was a very interesting work. Mick O'Donnell was already very active when I arrived in Sydney, and we had corresponded while I was still at ISI; he was working in the area of computational SFL, and I arranged for him to visit ISI to take part in the research activities I had left in 1988. So he also became a highly valued member of the network that included John Bateman, Ed Hovey, and Cécile Paris. Then Nanri (1993) started his fascinating Ph.D. research into the evolution of the register of hard news, and I served as his chief supervisor (see Table 3.1).

Table 3.2 Supervision of research students by Christian M.I.M. Matthiessen

Name	Year	Title	Research area	Degree	Institution	Supervisory status
NANRI, Keizo	1993	*An attempt to synthesize two systemic contextual theories through the investigation of the process of the evolution of the discourse semantic structure of the newspaper reporting article*	Register studies: evolution of news reporting	Ph.D.	University of Sydney	Chief
NESBITT, Christopher	1994	*Construing metalanguage*	Theory	Ph.D.	University of Sydney	Chief
O'DONNELL, Michael	1994	*Sentence analysis and generation: A systemic perspective*	Computational linguistics	Ph.D.	University of Sydney	Chief
SEFTON, Petie	1995	*State-potentials and social subjects in systemic-functional theory: Towards a computational socio-semiotics*	Computational linguistics	Ph.D.	University of Sydney	Chief
CAFFAREL, Alice	1996	*Prolegomena to a systemic functional interpretation of French grammar: From discourse to grammar and back*	Language description, French grammar	Ph.D.	University of Sydney	Chief

Table 3.2 (continued)

Name	Year	Title	Research area	Degree	Institution	Supervisory status
HARVEY, Arlene	1996	*Equivalence and depersonalisation in definitions: An exploration of lexicogrammatical and rhetorical patterns in English technical discourse*	Register studies	Ph.D.	University of Sydney	Chief
SLADE, Diana	1996	*The texture of casual conversation in English*	Register studies: gossip	Ph.D.	University of Sydney	Chief [taking over; joint with Rosemary Huisman]
ZENG Licheng	1996	*Planning text in an integrated multilingual meaning space*	Computational linguistics	Ph.D.	University of Sydney	Chief
MCDONALD, Edward	1998	*Clause and verbal group systems in Chinese: A text-based functional approach*	Language description: Chinese grammar	Ph.D.	Macquarie University	Chief
TERUYA, Kazuhiro	1998	*An exploration into the world of experience: A systemic-functional interpretation of the grammar of Japanese*	Language description: Japanese experiential clause grammar	Ph.D.	Macquarie University	Chief
THAI, Minh duc	1998	*A systemic functional interpretation of Vietnamese grammar*	Language description: Vietnamese grammar	Ph.D.	Macquarie University	Chief
WU Canzhong	2000	*Modelling linguistic resources: A systemic functional approach*	Computational linguistics	Ph.D.	Macquarie University	Chief

Table 3.2 (continued)

Name	Year	Title	Research area	Degree	Institution	Supervisory status
STUART-SMITH, Virginia	2001	*Rhetorical Structure Theory as a model of semantics: A corpus-based analysis from a systemic-functional perspective*	RST	Ph.D.	Macquarie University	Chief
LI, Eden	2002	*A text-based study of the grammar of Chinese from a systemic functional approach*	Language description: Chinese grammar	Ph.D.	Macquarie University	Chief
AKEREJOLA, Ernest	2006	*A systemic functional grammar of Òkó*	Language description, Òkó grammar	Ph.D.	Macquarie University	Chief
HERKE-COUCHMAN, Maria	2006	*SFL, corpus and the consumer: An exploration of theoretical and technological potential*	Register analysis and semantics for Natural Language Processing	Ph.D.	Macquarie University	Chief
PATPONG, Pattama	2006	*A systemic functional interpretation of Thai grammar: An exploration of Thai narrative discourse*	Thai grammar	Ph.D.	Macquarie University	Chief
BARDI, Mohamed Ali	2008	*A systemic functional description of the grammar of Arabic*	Language description, Arabic grammar	Ph.D.	Macquarie University	Chief

Table 3.2 (continued)

Name	Year	Title	Research area	Degree	Institution	Supervisory status
KUMAR, Abhishek	2008	*Mood, transitivity and theme in Bajjika in a typological perspective: A text-based description*	Language description: Bajjika grammar	Ph.D.	Macquarie University	Chief
SMITH, Bradley	2008	*Intonational systems and register: A multidimensional exploration*	Intonation and register variation	Ph.D.	Macquarie University	Chief
KIM, Mira	2008	*Application of SFG-based text analysis in translator education: A case study of English/Korean translation*	Translation studies: translation between English and Korean	Ph.D.	Macquarie University	Chief: Co-supervisor (with Anne Burns)
OCHI, Ayako	2013	*A text-based study of the interpersonal grammar of modern Japanese: MOOD, MODALITY and EVIDENTIALITY*	Analysis of news reports in English and Japanese	Ph.D.	Macquarie University	Chief: Former Supervisor, then external
GUO Songdan, Nancy	2015	*The ontogenesis of multiliteracy scaffolding in textbooks: Multimodal analysis of English language teaching textbooks of different grades*	Educational linguistics, multisemiotic analysis	Ph.D.	The Hong Kong Polytechnic University	Chief
WANG Yan, Constance	2015	*A systemic perspective on the translation of detective stories*	Translation studies	Ph.D.	The Hong Kong Polytechnic University	Chief

(continued)

Table 3.2 (continued)

Name	Year	Title	Research area	Degree	Institution	Supervisory status
XUAN Wenhui, Winfred	2015	*A longitudinal study of Chinese high school students learning English based on systemic functional text analysis*	Educational linguistics, second language writing	Ph.D.	The Hong Kong Polytechnic University	Chief
ZHANG Daozhen, Russell	2016	*Translating Alice in the Chinese context — a critical stylistic approach to characterization in the Chinese translation of Alice in Wonderland by Chao Yuen-Ren*	Translation studies	Ph.D.	The Hong Kong Polytechnic University	Chief
ZHAO Ruihua, Flora	2016	*The systems and functions of identifying processes — a semiotic perspective*	English grammar, identifying clauses	Ph.D.	The Hong Kong Polytechnic University; joint Ph.D. with Sun Yat-sen University	Chief
MWINLAARU, Isaac N	2017	*A systemic functional description of the grammar of Dagaare*	Language description, Dagaare	Ph.D.	The Hong Kong Polytechnic University	Chief
WANG Bo	2017	*Lao She's Teahouse and its English translations: A systemic functional perspective on drama translation*	Translation studies	DALS	The Hong Kong Polytechnic University	Chief

Table 3.2 (continued)

Name	Year	Title	Research area	Degree	Institution	Supervisory status
FUNG Ka Chun, Andy	2018	*Analysing Cantonese doctor-patient communication: A semantic network approach*	Questions in Cantonese in healthcare communication	Ph.D.	The Hong Kong Polytechnic University	Chief
LAW Lok Hei, Locky	2018	*House M.D. and creativity: A corpus linguistic systemic functional multimodal discourse analysis approach*	Creativity in TV medical drama	Ph.D.	The Hong Kong Polytechnic University	Chief
MA Yuanyi, Helen	2018	*A systemic functional perspective on Rabindranath Tagore's Stray Birds and its Chinese translations*	Translation studies	DALS	The Hong Kong Polytechnic University	Chief
ZHANG Peijia, Kaela	2018	*Public health education through posters in two world cities: A multimodal corpus-based analysis*	Healthcare communication research	Ph.D.	The Hong Kong Polytechnic University	Chief
MACDONALD, Kathleen	2019	*Construing musical discourses: Axial reasoning for a contrastive description of habitual ideational resources in English and Korean, with reflection on translation*	Ideational meaning in English and Korean (translation)	Ph.D.	The Hong Kong Polytechnic University	Chief

Table 3.2 (continued)

Name	Year	Title	Research area	Degree	Institution	Supervisory status
ZHENG Yaofei, Jennifer	2019	*Construing pharmaceutical research: A social semiotic perspective*	Educational linguistics	Ph.D.	The Hong Kong Polytechnic University	Chief
LIU Xiangdong, Charlie	2020	*On the language of projection across registers*	Registerial variation in projection	DALS	The Hong Kong Polytechnic University	Chief
GUERRA LYONS, Jésus David	2020	*Making meaning throughout writing trajectories: Towards a social semiotic account of language change in scholars' theory construction*	Semogenesis: ontogenesis	Ph.D.	The Hong Kong Polytechnic University	Chief
COFFIE, Joseph	2022	*Sexual language in television advertising: A systemic functional multimodal discourse analysis of Ghanian alcohol commercials*	Register studies, multimodal studies	Ph.D.	The Hong Kong Polytechnic University	Chief

In the Australian system, another important component was the fourth (honours) year in undergraduate programmes. Students could get a BA in three years, but they could go on and do an honours year, which was really geared towards an **honours thesis**, and there were courses on research methods. That honours thesis was worth more than the capstone project (six credits) at The Hong Kong Polytechnic University (PolyU). Students could really get into a significant research project, which would serve as a great way of getting experience with the whole process of running a project oneself, and it was a springboard for many to go into an MPhil or a Ph.D. programme. With an honours thesis, students really had the training to go straight into the Ph.D. track. I would supervise honours theses, MA students who chose to do a thesis, and MPhil and Ph.D. students.

There were a number of very good honours students. Some continued to do their Ph.D.s. You could see the fantastic development of the students from the first three years of undergraduate studies, then into honours, and then into Ph.D. studies. It was often good for students to move around, doing their undergraduate and honours at one university and then moving to another to do a Ph.D. Alternatively, students can get a tremendous research trajectory by staying in the same place and working with the same group of people. One example was Peter White (1998), who did a great honours thesis with us at Sydney University and then continued to do an excellent Ph.D. thesis with Jim Martin.

Bo Wang: Which were the areas that you preferred to supervise?

Christian Matthiessen: The common thread was SFL, involving quite a wonderful spread of research topics: see Table 3.2. **Marilyn** Cross's (1991) thesis aimed to model text generation with a view to computational linguistics. **Keizo Nanri's** (1993) thesis was, at the time, quite a unique contribution, and it was on the development of the register of news reporting with a specific focus on reports on assassinations or assassination attempts over 200 years. It was a detailed linguistic analysis, which could also be linked to **media discourse** and historical media discourse. Through the lexicogrammatical analysis, it revealed the change in English-speaking media around the 1860s in the news of American Civil War. People in the SFL community tended to refer to another great study of media discourse that came out of a disadvantaged school project that appeared around the same time (Iedema et al. 1993). Keizo Nanri's (1993) Ph.D. thesis was somehow overlooked in the SFL community in terms of the contributions to the understanding of media discourse. It was a pity that he did not have the opportunity to turn that into a book, which would have been a fantastic contribution. There is every reason to keep going back to earlier works, including Ph.D. theses, even if they had not been published by major publishers. A long time ago, I heard that people should not refer to Ph.D. theses because they had not been published by major publishers. I don't know where that idea came from, but it is bizarre because in documenting research you should always bring out and draw on whatever is available.

3.4   Moving to Hong Kong

Bo Wang: After staying in Australia for 20 years, you came to Hong Kong in 2008. Why did you move to Hong Kong?

Christian Matthiessen: It was not a move that I had anticipated. I was given a professorship in linguistics at Macquarie University in 2002. That was a very comfortable position. I had no heavy administrative duties. If I had stayed, there would have been difficult periods with a new vice-chancellor who was predictably disastrous for Macquarie University, but I could have continued to prosper. I would not have been subjected to these kinds of assessment exercises, the annual appraisals and all the administrative duties I have had in Hong Kong. I could have had a very comfortable existence.

I discussed the possibility of the move with Michael Halliday to weigh the pros and cons. He thought I should go to Hong Kong. One consideration was certainly the sense of being part of helping the development in another region. In Sydney and around Sydney, there was this tremendous concentration and a critical mass of systemic functional linguists. It seemed that Hong Kong could be an interesting area to work in. I knew some people quite well like Huang Guowen at Sun Yat-sen University and Jonathan Webster at City University of Hong Kong. I had done some review programmes when Jonathan was head of CTL (the Department of Chinese, Translation and Linguistics). So, I had some sense of the scene, though not very detailed.

I did know that the Pearl River Delta had an extraordinary concentration of brainpower. There would be interesting opportunities for further development, which were certainly a consideration. If I compared with some friends and colleagues who essentially stayed put where they were born, where they went to school and university and where they stayed throughout their lives (except for some excursions), my life turned out to be that of an academic vagabond. I moved around and did not stay for very long extended periods. Of course, it was interesting to have the opportunity to work in a different city, a different culture, and a different context. I was very much conscious of the fact that in Hong Kong, there was considerable investment in and support of universities.

I had hoped that I would learn Chinese. I had some exposure to Mandarin for half a year of evening classes around 1989 (cf. Matthiessen 2015b). There was the lack of opportunity of taking that further in the Hong Kong context because Hong Kong was after all such a solidly Cantonese city. I was being pulled into different directions. It would have made sense to try to learn Cantonese, but Mandarin was really what I primed to learn. I had some preparation for that, but there was not enough pressure for me. In the end, it turned out that when you are incredibly busy and when you work for seven days in a week, the practical incentive to learn Cantonese wasn't strong enough, because one could get away with using English most of the time.

Bo Wang: Michael Halliday also came to Hong Kong after you came.

Christian Matthiessen: Yes. We discussed this possibility during our weekly meetings at my place in Sydney before I left for Hong Kong. I had a very nice place on the third floor in an old building in Neutral Bay, which is a very pleasant area in Sydney. He smiled and said: "I might come and help you and get you started." He was 84 at the time. That was tremendous. He did come and help us (see Fig. 3.2). He made a big difference in terms of setting an academic tone in the department and the faculty. He gave a series of major talks. One was on scientific English across different disciplines, including natural science and the humanities. The other was on Chinese. That was intentional: one was directed at the Department of English,

Fig. 3.2 Michael Halliday (front) and Christian Matthiessen (back) in Hong Kong in 2009

and the other was directed at the Department of Chinese and Bilingual Studies. We started a series of symposia on language education as well as on SFL in the Pearl River Delta (though that was not continued), and we added one on translation later on, organized with the central help of Elaine Espindola.

Bo Wang: How did you collaborate with him in this period?

Christian Matthiessen: *IFG3* (Halliday & Matthiessen 2004) was published when I was in Australia. By the time *IFG4* (Halliday & Matthiessen 2014) was conceptualized, he said to me: "You take it over. I have contributed what I can". For him, the major project was to see through the completion of his collected works, which included 10 volumes. Later, there turned out to be another volume — *Halliday in the 21st Century* (Halliday 2013). We laughed about the title. There was another book — *The Essential Halliday* (Halliday 2009), which was a selection taken from the *Collected Works*. Again he felt this was funny, and I said: "I think we should have another book — *The Inessential Halliday*". But in any case, that had certainly been a major project.

I would have liked to return sooner to **The Outline of Systemic Functional Linguistics** in two volumes (Halliday & Matthiessen, in preparation). We also had other discussions, and we were planning a book on the introduction to **English discourse**. The idea was to have something that started with the phenomenon of discourse itself so that readers can get a sense of what discourse is. We were thinking of doing this by using a kind of ontogenetic framing so that you can follow discourses the way you meet and expand them as a person grows up (cf. McCabe 2021). I would like to pursue and develop this, but that will obviously have to be on my own. In addition to this book, we started on a book concerned with the status of language and languages around the world.

Another thing we talked about was **Games with Grammar and Phun with Fonology** — the provisional title he suggested for the book, and we planned it as a book that illustrates how people play with language. He was very good at crossword puzzles; they never got me interested. But there were other ways in which we could explore the potential in language for playing (even seriously) with language. We both enjoyed puns, for example — a sense of humour that seems to split groups of friends and also families into different camps! Some of us enjoy them, while others dislike puntification, and don't like pundering to us — if we persist, the result can be pundemonium. Both Michael and I had punctiliously collected a number of examples of what might be humorous or interesting. Like the other partially completed and planned books, this one is still on my agenda.

One thing I had not taken in was how much I would miss the glorious time of working with Michael Halliday in the same place. To me, that was a great source of sadness of not being possible to continue in the same manner. It was also the period when he had to think about winding down. A couple of years ago, we were talking about new ideas, and it would have been possible to pursue these ideas if Ruqaiya Hasan had not passed away. In any case, you have to look for new opportunities.

Interestingly, before I decided to move to Hong Kong, I asked a friend — Frances Low, who is a bi-city person, knowing Sydney and Hong Kong equally well, having lived in both places. He came to Hong Kong just before the handover in around 1996, but he still had his family in Sydney. I said: "Which is the best place to live?" Without blinking and without hesitating for a second, he answered "Sydney". So, I was aware of this when I made the decision.

Such decisions are always so complex because of the many factors involved. In terms of the community of students and academics, Hong Kong has turned out to be unprecedented — a community being developed and built up. We are just at the beginning of the extraordinary possibilities. Hong Kong is a very good place as a hub, which in some sense has been over-used in promoting Hong Kong as Asia's world city and as a knowledge hub. But it is certainly true that people visit Hong Kong and also fly through Hong Kong, so it is possible to grab them as they are on their way to somewhere else. Sydney, on the other hand, is more of a terminal destination, unless somebody stops over on the way to Melbourne. That is a significant difference. Also, in terms of bringing people in, it is easier to bring people from the Americas and Europe to Hong Kong than to Sydney — something fairly significant in our development here.

3.5 Influence from Halliday and Other Linguists

Bo Wang: In a previous interview (see Chap. 1), you mentioned how you met Michael Halliday and how you worked together with him on a project led by Bill Mann. Can you tell us how you have been influenced by Michael Halliday?

Christian Matthiessen: I was, of course, influenced by him long before meeting him. It would be interesting to compare that with the lucky folks who grew up with him academically in Sydney. While I was at Lund University (see Sects. 1.1 and 2.1), the undergraduate teaching of linguistics (phonology and syntax) was influenced by the generative linguistics of the day. In phonology, there was *The Sound Patterns of English* (Chomsky & Halle 1968). In syntax, there was the (extended) standard theory (Akmajian & Heny 1975). But at the same time, there was the lasting sense of the significance of European structuralism, thanks to Bertil Malmberg, and the Prague School, thanks to his son-in-law Milan Bílý. In the English Department, there was corpus linguistics and the Quirkian tradition, which had also been influenced by Halliday (cf. 1979). In the Linguistics Department, we were encouraged to read around and during one of my discursive expeditions, I came across SFL and something suddenly clicked somewhere in my brain. I thought that it was truly interesting, so I kept reading, looking for more material.

Fig. 3.3 Bengt Sigurd (middle), his wife and Hans Lindqvist (on a conference trip to the US)

When I did my final year project, Professor Bengt Sigurd (see Fig. 3.3) encouraged me to pursue my interest in what I called Hallidayan linguistics. I do not think that we were even aware of the term "Systemic Functional Linguistics". Based on what I could extract from the literature, I wrote some 60 or 70 pages. So I was very much primed to appreciate SFL when I met Michael Halliday and Ruqaiya Hasan at Stanford University around February 1980. Starting later that spring, Michael Halliday gave a lecture series at UC Irvine over the ten weeks, and I attended, spellbound. By then, I had had a very good part of an academic year at UCLA, which was a place of important linguists, as was USC, and had encountered various "big" names in linguistics. When I began to have the first lectures given by Michael Halliday, I was struck that this was the first time I met somebody who was interested in **language** in the first instance rather than in linguistics. He was somebody who seemed to have a hotline to language, who really had a sense of what language was all about. His theories were not about games in linguistics, but **a holistic theory of language in context** designed to support the development of comprehensive descriptions of particular languages. That was quite mesmerizing (see Fig. 3.4).
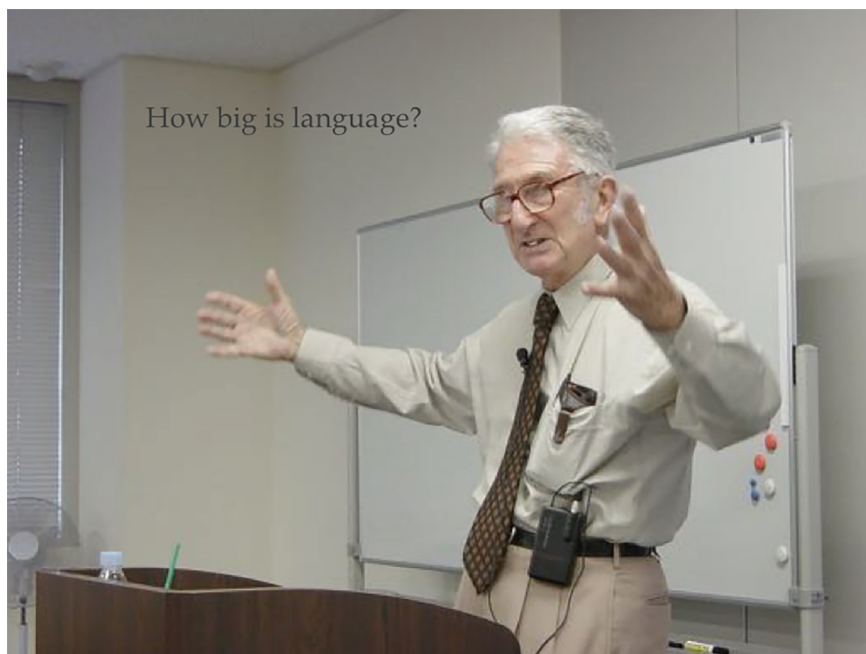
Fig. 3.4 Halliday talking about language, raising the question of how big language is at Columbia University's Teachers' College in Tokyo

At some point during one of our conversations, he told me about his approach to teaching first-year linguistics students. He said he would teach them about **language** in the first year, and then move on to **linguistics**. The typical pattern since the late 1960s had been to teach first-year students about linguistics, like solving little jigsaw problems in syntax and writing rules in phonology. But Michael Halliday was very adamant that beginners should learn to observe language, and become able to bring it to consciousness. In first-year linguistics, he helped them develop a sense of what kind of phenomenon language is (e.g., rhythmic patterns and other prosodic patterns in everyday life) and reflect on this. That was quite an eye-opener for me. I would say to people who had grown up with Michael Halliday academically: "What you haven't experienced is the difference between his approach to language and that of many well-known linguists". That was a fundamental difference.

Bo Wang: Besides Michael Halliday and Ruqaiya Hasan, are there other linguists or works that you think are significant to your career?

Christian Matthiessen: In the early days at Lund University, I learned about European structuralism through the works of Bertil Malmberg and the corpus-based work that used Halliday and Hasan's (1976) book on cohesion as a way of analysing

discourse. After reading around, I also got interested in **Stratificational Linguistics** (e.g., Lamb 1966; Lockwood 1972). I was always easily charmed by visuals, so I was intrigued by the diagrams of Stratificational Linguistics and also in **Tagmemics** — in addition to those in SFL. (Sydney Lamb had actually developed his graphic representation of stratificational networks inspired by Michael Halliday's system networks.)

I found accounts of language typology quite interesting. Even before I went to UCLA, I had read, and been influenced by, some works by Joseph Greenberg (e.g., 1966, 1978). At UCLA, **language typology** was very much part of experience because there were various linguists working on typology, including Sandy Thompson, Talmy Givón, Paul Schachter, Ed Keenan. There were linguists working on the description of particular languages. UCLA was strong in studies of North American Indian languages (e.g., Mithun 1999), especially thanks to the contributions by Munro (e.g., 1974) and languages spoken in Africa (e.g., Welmers 1971) and also Austronesian languages (e.g., Keenan & Ochs 1987; Keenan 1976; Schachter & Otanes 1972). That was important in my thinking and my development.

I also found computational linguistics and knowledge representation quite interesting. Because I had to earn money to keep on studying, once I got my job at the Information Science Institute, I became interested in computational linguistics and how that could contribute to the understanding of languages and the whole notion of modelling language. Drawing on that, I felt the need to analyse, describe and explicitly model language. I also felt the need to think about linguistic processes as well as linguistic states (of either the linguistic system or texts), including the process of generating texts. That engagement with computational modelling and process specification has been most valuable to me since it is fairly far removed from the typical domains of linguistics.

3.6   Christian Matthiessen's Research Areas

Bo Wang: Which areas of linguistics have you worked most extensively in? Your collected works are now being published, how many volumes will there be?

Christian Matthiessen: There will be eight volumes, and the cut-off for inclusion is around 2016. I hope I will be around to produce much more material for future volumes.

In terms of research areas, one of the first was certainly the **computational linguistic work**; but I have been a linguist working in a computational environment,[2] unlike John Bateman — a contemporary of mine who is bi-metalingual and

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[2] Actually, Bengt Sigurd had already shown us students at Lund University how the computer could be used as a tool in linguistics — beyond its use in corpus linguistics. As a result, I signed up for a non-credit bearing course given by a computer programmer at the Department of Phonetics to a group of faculty members, research students — and me. Unfortunately, for us, his idea was to

has been equally home in linguistics and computational linguistics, and later, based on computational linguistic work on multimodal documents, branched out into multi-semiotic studies and film studies. I never managed to get beyond being a linguist talking to computational linguists, software engineers and programmers (cf. Patrick 2008 on dialogic interfaces in computational linguistic projects). It turned out that I was more valuable as a decent linguist than as a bad programmer! I would have loved to learn programming so that I could implement ideas myself. If I had started out now, it would have been more feasible since researchers have designed and developed object-oriented and higher-level programming languages such as Java, Python and Apple's Swift.

Throughout the 1980s, I was in a double bind. To support my studies, I worked at ISI as a research linguist, from 1980 to 1988, when I left for Sydney University to take up a lecturer position there. The work in this research institute was fascinating. But what had attracted me to UCLA in the first place was my interest in **language description** and **language typology**, and I wanted to pursue research in this area informed by SFL. I found field methods fascinating: for a year, I worked on Akan under the guidance of Paul Schachter, and tried to develop a systemic functional description of the grammar of Akan (Matthiessen 1987a), which was an interesting challenge partly because I didn't have any models to draw on (at that stage I hadn't seen the early systemic descriptions by Barnwell (1969) on Mbembe, and Mock 1969 on Nzema) and partly because the other members of the field methods course were interested mainly in what was fashionable at the time in Chomskyan generative linguistics, including long-distance movements. In a sense, it was even more challenging when working out the description of the **phonology** (Matthiessen 1987b), since there was virtually nothing around on systemic functional phonology, so I had to develop the framework from first principle. That was truly exciting. I would have loved to continue that; but at some point, I was being pulled in two directions and could not maintain both. The direction for which I was being paid as a researcher was the computational work.

As I moved from the 1980s into the 1990s, I was for the first time in the context of teaching in a programme of linguistics, where actually engaging with and systematically analysing text was part of what the students had to learn from a very early stage (whether they were undergraduate or MA students). That was extraordinary; I had never seen that anywhere else. That influenced me in developing the system-based description that turned into *Lexicogrammatical Cartography* (Matthiessen 1995).

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give us a sense of how computers really work at "low levels", so he set out to teach us assembly language. Not surprisingly, our interest waned fairly quickly. This was my second attempt to learn programming; in my final years of high school, I took a guided studies course with our mathematics teacher to learn to programme the school's newly acquired computer — using punch cards! That turned out not to be exactly thrilling. My third attempt to learn a programming language started in 1980, when our project leader, Bill Man, set me up to learn LISP through an online course at Rutgers University. I was "rescued" from my slow online struggles with the LISP tutorial programme when Bill decided that my time as a linguist was more valuable than as a slow-coding programmer. He asked me to teach a course to researchers at the Institute on theme, cohesion, and other textual systems. But these experiences were quite helpful in giving me a sense of how to interact with computers through programming.

After the computational linguistics projects during the 1980s, I started working more systematically on other research topics. One early publication that was important to me was **"The Grammar of Semiosis"** (Matthiessen 1991a), where I explored the way in which the grammar embodies a theory of itself. It was published in one of the early issues of our new journal *Social Semiotics*. I also continued my work on **tense** in English (Matthiessen 1983, 1984), part of which was published in one of the thematic SFL Festschrifts for Michael Halliday (Matthiessen 1996).

Stimulated by our work on text generation at ISI (cf. Matthiessen & Bateman 1991), I was interested in the development of text in context, i.e., **logogenesis** — the creation of meaning as a text unfolds. I wrote a paper on logogenesis and instantial systems after giving a talk on this topic at a conference hosted by Professor Ren Shaozeng at Hanghzhou University in China — Matthiessen (1993). While circulated fairly widely, this paper was never published; but I followed it up with another paper, also originally presented in China, at Sun Yat-sen University: Matthiessen (2002).

Drawing both on what I had learned about representing grammars metafunctionally and my interest in language typology, I also wrote a paper on the **metafunctional modes of meaning** and **modes of expression** in different languages (Matthiessen 1991b); I conceived of it as an extension of Halliday's (1979) paper on modes of meaning and modes of expression. I sent it to one journal, but it was politely returned by the editor, Bernard Comrie, whose fascinating course on tense I had audited sometime in the early 1980s, when he was still at USC. Then I never got around to revising it for submission to another journal; it was a pity, and I should have pursued it. Much later in the functional-typological literature, people would have been more receptive to it. I do not think that I had learned to put it in terms that would make it accessible and interesting to people outside SFL, particularly in the functional-typological community. But I continued to explore the relationship between modes of meaning and expressive resources, and added "**means of expression**" in my typology chapter (i.e., Matthiessen 2004) in Caffarel, Martin & Matthiessen (2004), as set out schematically in Table 3.3. For example, intonation and sequence are **means of expression**, and they can be used by different metafunctional **modes of expression**, e.g., textually to create wave peaks of prominence and interpersonally to create prosodies.

One reason why I didn't pursue these unpublished topics was that I was fortunate early on in being asked to give talks or contribute publications. I found it very interesting to be given such tasks. Even though I had not particularly published in the area of **multimodal studies** (our publications on multimodal text generation in the 1990s, e.g., Matthiessen et al. 1998, were not widely accessible), Wendy Bowcher and Terry Royce asked me to contribute a chapter to their edited book on multimodality (Matthiessen 2006). I was grateful for the opportunity to collect my thoughts. Erich Steiner and Colin Yallop had organized a symposium within an ISFC and asked me to do something on **translation**. Again, that was an opportunity to collect my thoughts, read up, and do some work that was relevant (Matthiessen 2001).

I remember talking to Michael Halliday when he was in the process of planning his *Collected Works*. He said: "It was interesting that many of the things I have done were in response to requests by other people." I understood what he meant; quite

Table 3.3 Relationship between modes of meaning and means of expression in relation to the metafunctions

		Metafunction		
		Logical	Experiential	Interpersonal	Textual
Modes of expression		Serial	Configurational (particle)	Prosodic (field)	Undulating (wave)
Means of expression	Segmental	Repeated segments (e.g. structural conjunctions)	Configurationally distributed segments marking parts of configuration (e.g. case markers)	Prosodically dispersed segments or segments with prosodic scope (e.g. modal particles)	Segments marking prominence (e.g. focus particles)
	Sequence		Configurational sequence of elements, e.g. doer > process > done-to	Prosodic use of sequence, e.g. at junctures of units as interactive hot spots or (in English) Subject ^ Finite vs. Finite ^ Subject	Prominence-creating use of sequence, e.g. initial vs. end prominence
	Intonation	Tone sequence, tone concord	–	Tone (direction of pitch movement)	Tonicity (location of major pitch movement)

a few of my publications were initiated by people's interest in certain areas. I may have been biting off more than I was able to chew, but I still, found these assignments very stimulating. I always loved reading on, and going into new fields to get a quick sense of the possible connections and possible contributions by using SFL as a key resource.

3.7 Christian Matthiessen's Unpublished Works

Bo Wang: You have talked about works that are not published, like the book on discourse. Are there any other similar projects?

Christian Matthiessen: There was one on the **architecture of language** (Matthiessen forthcoming a), one on the **systemicized version of RST** — actually two volumes, the first of which is now complete (Matthiessen forthcoming b), and one on the **multilingual version of *IFG*** (see Sect. 4.4). There was also one on **discourse analysis for translators** (see Sect. 10.2). I have tried to put a team together, but people were busy with their various agendas. I will see if I can complete that on my own. Then, I have been thinking of something like a registerial overview of **registers in healthcare and medicine**. I also thought about a book that will be a handbook in linguistic and contextual analysis of **discourses in healthcare and medicine**.

But new ideas emerge, and may overtake earlier ideas. In the case of co-authored books, Kazuhiro Teruya has coordinated a book on **projection in different languages**, which will be very exciting. He is also coordinating a book that is being published in Japan in Japanese (Teruya in press), with contributions by different authors, including himself, myself, Michael Halliday, Heidi Byrnes and some other scholars. We need to do more of this. People appreciate and find it helpful to read and engage with thematic books and volumes presented to readers in different languages.

I have thought quite seriously about returning to *Construing Experience through Meaning* (Halliday & Matthiessen 1999/2006). When we wrote this book, Michael Halliday and I positioned it as a contribution to people who were in and around cognitive science, which is reflected in the works we cite. The response we got was resounding silence from that community; virtually nobody seems to have thought it worth their while to engage with it from the standpoint of cognitive science although Sowa (2001) wrote a very thoughtful review[3] and now work by Adolfo García and his colleagues make it possible to develop the work in relation to what they call **neurosemiotics** (e.g., García & Ibáñez 2017; Trevisan & García 2019; cf. also Matthiessen forthcoming c). I plan to return to our work on construing experience, taking account of all the new developments and insights, but this time with a targeted readership of people in education and discourse analysis more generally. In this

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[3] He pointed out the relevance of Peirce's (1992, 1998) work. Interestingly, Bateman (2018) has now engaged with Peirce in the context of multimodal studies.

context, treating knowledge as meaning would be a valuable contribution. There are various ideas bubbling around up there.

When Michael Halliday retired at the age of 63, the vice chancellor said to him that he was awfully young to retire. But Michael told me that he had to retire to get work done. That is why we have to win lottery tickets to become independently wealthy! I will also have to write books to earn royalties because regular academic books do not. On one occasion, in the book on translation edited by Erich Steiner and Colin Yallop (2001), I included a preamble in my chapter on the environments of translation (Matthiessen 2001), which was about translation across semiotic systems, because I thought this topic had not really been discussed in SFL and it was interesting in its own right and would also shed additional light on the whole question of translation (see Sect. 8.4). As an illustration, I had chosen a painting by William Turner, *The Slave Ship*, which was based on the report of an unimaginably cruel and craven event during the transatlantic slave trade, because John Ruskin, who was the first owner of the painting, had described, or "translated", Turner's painting in a vivid evocative passage. So I needed a reproduction of the painting for my chapter, and I had to find out where this painting was displayed. It turned out to be the Museum of Art in Boston. So, I got in touch with somebody there, and he asked me how many copies of the book would be published. I had no idea, so I checked with Colin and Erich, and they said 500 copies. I sent this message back to the person at the museum. His response was that there was no need to pay anything. He must have had a good laugh at the very modest number. Coffee table books on art are printed in ten thousands of copies, but on the whole academic books do not make money. You have to be in the textbook market in applied linguistics or ESL to actually make money. That would be an alternative to winning a lottery ticket.

3.8 Christian Matthiessen's Contributions to Systemic Functional Linguistics

Bo Wang: How have you developed SFL? What are your contributions?

Christian Matthiessen: SFL has continued to develop both by design and by evolution (see Sect. 4.2); it is very important to understand the complementarity of these two processes of development. On the whole, its path of development has been very different from that of Chomskyan linguistics. From my point of view, there have been very few revisions of SFL; the theoretical and descriptive contributions over the decades are cumulative in nature. You can pick up anything from the 1960s, and it is still valid — perhaps with certain revisions and adjustments and additions, but the overall picture will still be useful (as I have tried to show in reference to the description of the system of TRANSITIVITY in English in Matthiessen 2018).

If it is early SFL, you will not have metafunctions and systems organized as system networks. But there is nothing that has really been negated because systemic

functional development has been one of expanding around central insights, recontextualizing earlier contributions rather than discarding them (Matthiessen 2007a, 2007b, 2010, 2015a). One reason for this is that from the start Michael Halliday knew so much about language, and also different particular languages. Engaging with language, he got it right in the first place. If you combine that with a holistic approach to theorizing, the development of comprehensive descriptions does make sense — comprehensive descriptions being valued both because they support "systems thinking" and because they are needed for many applications. So, the development of SFL has been a process of filling in gaps rather than one of constant replacement of earlier accounts. There are of course revisions of some quite significant details, but on the whole (thanks to this kind of development), it has been a creation of cumulative knowledge about language. In that sense, it is more like the physical sciences and the natural sciences generally[4] than linguistics as a whole during the second half of the twentieth century. In the Chomskyan era, linguistics was characterized by the ongoing arguments for and against alternative accounts, with one replacing another (cf. the discussion in Sect. 1.2 of Gross's 1979 claims about the failure of generative grammar; see also e.g., Seuren 2004). Every proposal had a certain lifespan and then it became obsolete. It has been a great pleasure for me to contribute to the collective and cumulative development of "knowledge" in SFL.

This body of "knowledge" is, naturally, not monolithically homogeneous but it is rather varied according to different theoretical orientations within SFL (see e.g., Bartlett & O'Grady 2017; Thompson et al. 2019; Matthiessen & Teruya in press: Chap. 5). Thus Robin Fawcett (e.g., 2008) has developed what he calls the Cardiff Grammar, together with a great team, often in computational applications, and he contrasts this with "the Sydney Grammar", Halliday's *IFG* framework (cf. Matthiessen 2007b). (I do not agree with the characterization of it as the "Sydney Grammar" because the grammar is used and developed all around the world.) But SFL has remained a flexible or elastic resource encompassing such theoretical variation, and of course also descriptive alternatives. I have found the elastic space of SFL most productive; it has provided the conditions for the longer-term potential of developments and insights.

It is important to work together to develop a big quilt work of contributions instead of creating one's own space or turf as has characterized a great deal of linguistics from the 1960s onwards. If you take a step back and look at the development of knowledge in different disciplines, linguistics has reached the point where it cannot be a one-person operation any longer. You must have teams working together and supporting one another. In China especially, I have been asked a number of times: "Who is Michael Halliday's successor?" People wanted an answer, but I know Michael Halliday did not like this line of questioning. I think the answer is: we are now in a different era and taking part in a different phase of development, and it needs to be

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[4] For example, Newtonian physics is still taught in secondary school — in particular his laws of motion; his laws of motion have not been discarded but rather contextualized by later developments, and in the process interpreted as a special case with certain parameter settings. At the same time, there are certainly accounts in the natural sciences of a more speculative nature that have been replaced such as the notion of ether.

collective. There will clearly be academic leaders, but they will have different traits of the kind that researchers into leadership have identified and described, and they will be complementary. People must work together, which calls for a different mode of operation. That is one of the reasons why I heartily dislike appraisal systems in universities that target individuals. In some sense, such appraisal systems unnecessarily lead to unhealthy competition among individuals because, in certain societies, competition is already embodied in corporate capitalism. People do not need extra incentive to compete through appraisal systems. What they need actually incentive for is collaboration. That is an important part of research work going forward. This is the **tenor aspect of academic processes** — including centrally the role networks, and issues of hierarchies in their organization.

But how can one achieve this? It is easy to say, but it is not actually straightforward. How do you enable teamwork? If you have a comprehensive description of a given language, what kind of framework do you need to enable people to work together on it? This is an interesting meta-theoretical and also a practical problem, but it is something that needs careful thought though given attention in its own right. As we move more towards team-based research, this is an important problem to tackle. In SFL, we see very good teams of people, and we have models, so we can reflect on what has made them successful. What made the fantastic teamwork led by Martin's (e.g., 1992, 1999) development of genre possible? What can we learn from that in terms of developing large-scale accounts that have a lifespan beyond a couple of years? What accounts for the success and remarkable longevity of the Penman to KPML line of development? What will be the components needed to break through to a modern scientific approach to semiotic systems? Questions of this kind are a very important prelude to future activities. (As long as universities are structured hierarchically into different faculties with different schools or departments, one way forward is to form teams with members who are not set up to compete within the same academic units.)

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Part II
Systemic Functional Linguistics and Its Applications

Chapter 4
Some Conceptual Issues in Systemic Functional Linguistics

Abstract In this chapter, we examine some conceptual issues in Systemic Functional Linguistics (SFL). We first interpret the meaning of "systemic" and "functional". We then examine the phases of development of SFL and comment on the different names of the theory in the course of its evolution. We also discuss the theoretical aspects of SFL, delineate the term "metafunction", and introduce the multilingual version of *Introduction to Functional Grammar,* which Christian Matthiessen is developing. Finally, Halliday's unfinished works on systemic functional theory and Halliday's conventions of technical terms are introduced.

4.1   Introduction

This chapter examines some key terms and concepts in Systemic Functional Linguistics (SFL) and their historical developments. The chapter begins with a discussion of the terms "systemic" and "functional", situated within the context of other functional theories of language and formal approaches. The goals and scope of SFL are delineated in relation to a family of functional theories and the formal approaches emerging from generative linguistics. The chapter then examines the historical development of SFL, beginning with scale-&-category theory emerging from Halliday's (1961) "Categories of a Theory of Grammar", systemic linguistics, Systemic Functional Grammar and subsequently Systemic Functional Linguistics. We also discuss common notations in representing categories in SFL descriptions. Controversial issues in linguistics such as the difference between theory and description, and the criticism of SFL as an Anglocentric theory are also examined. Christian Matthiessen finally outlines his ongoing project on a multilingual version of *IFG* (*Introduction to Functional Grammar*) and his collaboration with Michael Halliday on *Outline of Systemic Functional Linguistics*. As with other chapters, the chapter provides interesting issues behind-the-scenes in the development of SFL by Michael Halliday and in his collaboration with Christian Matthiessen.

4.2 "Systemic" and "Functional" in Systemic Functional Linguistics

Isaac Mwinlaaru: What is Systemic Functional Linguistics? Why is it "systemic"? Why is it "functional"?

Christian Matthiessen: Functional linguistics is both the name of the class of approaches to language and the specific brands of linguistic theories. There are various manifestations of functional linguistics, but there is only one **Systemic Functional Linguistics (SFL)** (see Table 4.1). "**Functional**" is general and technical in terms of the value systems, the *valeur*, the contrast, and the oppositions of the theories. In this case, "functional" contrasts with "formal". The important point is that "functional" does not necessarily exclude the other theories. It is used to foreground the functional approach to language — the approach that is able to analyse, describe, and explain language. Functional linguistics is a set of these different approaches.

In *Ideas about Language*, Halliday (1977) traces the contrast between functional and formal approaches back to ancient Greece in the European tradition, where functional and formal have been the motifs of thinking about language. Sometimes functional is foregrounded, and at other times formal is foregrounded. Halliday (1977) talks about this in terms of the conception of language as **resource** — the way that functional theory tends to conceptualize language. In contrast to language as resource, there is language as **rule**, which is the formal model. The Sophists in ancient Greece are more functional in orientation, they conceive of language as a resource; whereas Aristotle is more formal in seeing language as rule. Seuren (1998) makes a very similar distinction in his *Western Linguistics: An Historical Introduction*, where language as resource is characterized as **ecologism**, and language as rule is considered **formalism**. It is essentially the same kind of contrast (see Fig. 4.1).

Viewed in that way, functional linguistics and formal linguistics both have a very long history. In the first half of the twentieth century, in the European context, **Vilém Mathesius** (1882–1945) and his junior colleagues developed the **Prague School** as

Table 4.1 SFL in relation to other functional approaches and formal ones

Orientation towards	Axis	
	Systemic (paradigmatic)	Structural (syntagmatic)
Function	SFL	Functional approaches in general: SFL, Prague School, FDG (Functional Discourse Grammar), LFG (Lexical-Functional Grammar), RRG (Role and Reference Grammar), etc
Form	–	Formal approaches in general: TG (Transformational Grammar), EST (Extended Standard Theory), Minimalist Program, GPSG (Generalized Phrase Structure Grammars), Categorial Grammar, etc
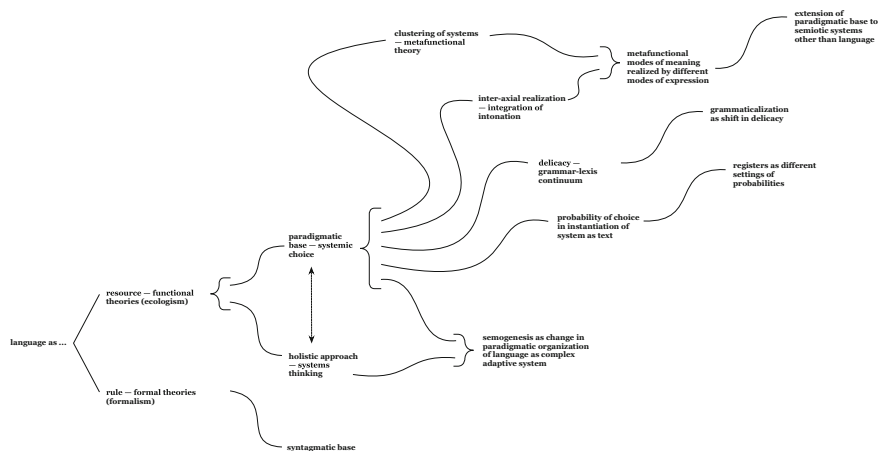
Fig. 4.1 The contrast between language as resource (ecologism) and language as rule (formalism)

a functional orientation to language (see Halliday 1974; Davidse 1986). An early important contribution was Mathesius (1911), but the articulation of the Prague School programme came in the late 1920s. The work by Mathesius and others on "Functional Sentence Perspective" shed new light on the "Subject-Predicate debate" from the late nineteenth century into the 1920s (e.g., Seuren 1998, 1999).

On the North American side of the Atlantic, there emerged a functional-anthropological approach, due to **Franz Boas**, his students in linguistics and anthropology, and their students: Boas (1911, 1940) — including his descriptions of the Kwakiutl language (grammar and vocabulary) and society over a number of years and in a number of publications — and Sapir (1921), Whorf (1956), Haas (1944, 1969), and Pike (1948, 1967). This tradition was functional in the very general sense of being oriented towards descriptions informed by anthropological fieldwork, drawing on text and paying attention to culture. Pike's Tagmemic Linguistics is part of this tradition, as is the later development by Hymes (e.g., 1967) and his colleagues of the Ethnography of Speaking (e.g., Saville-Troike 1982).

Alongside the Boas-Sapir tradition of anthropological linguistics, we can recognize a parallel current in US linguistics, developed by **Bloomfield** (e.g., 1914, 1933) and "post-Bloomfieldians",[1] prominently Harris (e.g., 1951, 1954, 1970), Chomsky's teacher — the latter group providing a starting point for Chomsky's generative linguistics (cf. Matthews 1993).

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[1] Cf. Blevins (2006), who includes: "Bernard Bloch, Zellig Harris, Archibald Hill, Charles Hockett, Eugene Nida, Kenneth Pike, Henry Smith, George Trager, and Rulon Wells". But it's helpful to recognize some of them, at least Ken Pike, as being more directly related to the Boas-Sapir tradition of anthropologically oriented linguists.

In the US, Chomsky's **generative linguistics** became increasingly prominent and even dominant in the 1960s[2]; and although this generative strand has split and diversified, it is still active. Starting in the 1970s, there was a functionally oriented reaction against Chomsky's generative linguistics; the leaders were a generation of linguists who obtained their PhDs during the second half of the 1960s when Chomskyan formal linguistics was very strong. Often feeling that Chomsky's kind of theory was a straitjacket and his explanations unsatisfactory, these linguists rebelled against Chomsky's formal linguistics and developed an approach often called **West Coast Functionalism**. Key representatives of the first generation are **Sandy Thompson**, **Talmy Givón**, and **Paul Hopper** (e.g., Givón 1979; Hopper & Thompson 1980; Li & Thompson 1989). They did make connections with the Prague School in certain respects, though much less so than systemic functional linguists did. We can also regard Tagmemic Linguistics (e.g., Pike & Pike 1983; Pike 1967) as functional. So, functional is a general approach to language that has had many manifestations.

In the European context, Simon Dik (1978) developed his **Functional Grammar** starting with his Ph.D. on coordination in 1968. His work was essentially functional (though it was not really text-oriented) and followed the European functional tradition. He was certainly influenced by the Prague School (in particular by František Daneš); the Praguian functional sentence perspective (e.g., Daneš 1964, 1974; Mathesius 1928) became pragmatics in Simon Dik's work. Dik was particularly concerned with functional accounts as a contribution to linguistic typology (so in that respect comparable to Role and Reference Grammar, e.g., Van Valin 1993), and the development of comprehensive descriptions of particular languages was not high on the agenda (Dik pc in 1986). In developments in the last two decades or so, "discourse" has been added to the realm of phenomena; this revised version of Functional Grammar has been called "**Functional Discourse Grammar**" (FDG; e.g., Hengeveld & Makenzie 2010).

This is the tradition of "**one-dimensional**" functional linguistics, where functional diversification and stratification are not independent dimensions but are as it were fused in one hierarchy of levels or components. In contrast, Halliday's (e.g., 1992a) functional approach is "**two-dimensional**": **stratification** and **functional diversification** (metafunction) are independent dimensions that intersect. That has been a major difference within the functional approaches. For instance, in Simon Dik's work, you essentially have one dimension, and semantics is the deepest or the highest component. In systemic functional work (e.g., Halliday 1973a, 1979), the metafunctions permeate the content plane and are thus manifested within both the semantic stratum and the lexicogrammatical one. That has been very important.

There is a whole family of functional approaches to language, and you get different combinations of traits. For example, Lexical Functional Grammar (LFG, e.g., Bresnan & Kaplan 1995) is functional in the sense of **micro-functional** (using

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[2] In Matthews's (2001: p. 142) assessment discussing structuralism: "Its heyday lasted from the 1930s, when it was named, to the end of the 1950s; and, throughout that period, linguistics was dominated by it. But structuralism in America is said to have been overturned by Chomsky, and by the 1970s his hegemony was world-wide.".

this term here in a meta-linguistic sense), i.e., functional in the representation of structure. There was a good deal of discussion about the representation of structure in the generative tradition, including whether one could derive structural functions from the class-based constituent structure or not. Chomsky's (1976, 1981) position was: yes, you can. But in LFG, as in Bresnan (1982), both function structure and constituent structure are posited to handle different aspects of (in our terms) syntagmatic patterns. Other functional approaches, like the Prague School, have gone beyond structural micro-functions with some kind of notions comparable to aspects of metafunction in SFL; for example, in the Prague School, structural functions like Theme and Rheme are manifestations of Functional Sentence Perspective. That was also an important distinction.

SFL is unique among functional approaches. In fact, it is unique in general among approaches of any kind. In the systemic part, the primary organization is **paradigmatic** and **systemic** rather than syntagmatic and structural, as already shown in Table 4.1 above. The prioritization of the paradigmatic axis was introduced in the 1960s by Halliday (1966a). The combination of the paradigmatic orientation and the metafunctional diversification is a unique property of SFL.

Interestingly, the prioritization of the paradigmatic axis followed a number of insights. In one of my chapters, I called it "**the axial rethink**" (Matthiessen 2015; cf. Matthiessen et al., 2018). If you see language as a resource for making meaning, how can you represent it as such — as a resource rather than as a rule system, which has been the common way of representation in syntagmatically oriented theories? Michael Halliday's answer was his design of the **system network** (Halliday 1966a). Once he had taken that crucial step, other fundamental insights followed; by representing language paradigmatically by means of system networks, he was able to discern phenomena that are less immediately visible from a syntagmatic vantage point and he was also able to model aspects of the organization of language that are syntagmatically tricky, like the integration of intonation and the relationship between grammar and lexis. That is how Halliday (e.g., 1969, 1973a, 1979) discovered the metafunctions. It is also what has enabled him to incorporate intonation without having to force it into a constituency model (e.g., Halliday 1967c, 1992b). While linguists drawing on European structuralist linguistics have emphasized **paradigmatic** organization, SFL is the only paradigmatically based theory of language — hence "systemic" in the name.

As just noted, other linguists have certainly explored paradigmatic organization — typically, as I said, informed by European structuralism (e.g., Trier's 1931 lexical field theory, Trubetzkoy's 1939, 1969 phonological paradigms). In the 1960s, Heller and Macris (1967) proposed **parametric linguistics**, but it does not seem to have attracted much attention. Their framework shows systemic thinking in terms of phonology and morphology. It follows from the Saussurean tradition of recognizing the two axes, but it does not elevate the paradigmatic as the primary mode of organization.

4.3 Historical Development of Systemic Functional Linguistics

4.3.1 Different Terms for Systemic Functional Linguistics

Isaac Mwinlaaru: It is evident from the literature that from the 1960s, there were different terms for SFL, including scale-&-category theory, systemic theory, and Systemic Functional Grammar. What motivated these changes?

Christian Matthiessen: Scale-&-category theory was the first step in trying to develop Firth's (e.g., 1957a, 1968a) **system-structure theory** — the British version of a paradigmatic and syntagmatic theory where the two axes are "balanced": paradigmatic organization is represented by system, and syntagmatic organization by structure. But there is the special Firthian twist. As a manifestation of his **contextualism**, he "dispersed" the phonological system according to **places** in phonological structure: instead of positing a single system of phonemes (or phonematic units), he posited distinct consonant and vowel systems operating in different places. For example, in a language that has consonant clusters as the onset of a syllable before the vocalic peak, there might be three consonant systems before the peak one after the other: $C_1C_2C_3VC_4$. One of the points is of course that these systems are distinct in terms of their members, so the members have different values (*valeurs*) even if the same "phoneme" in phonemic theory would happen to operate in more than one place. That has interesting consequences for phonology. One could explore the possibilities in the following dialectic way. The thesis is that there is one phonological system, one system of phonemes. The antithesis is that there are distinct systems for different places in phonological structure. (In Firthian phonology, this went hand in hand with his prosodic analysis.) But what about the synthesis? In my view, this is **systemic functional phonology**, at least as I envisage it (Matthiessen 2021). Uniquely among phonological theories, SFL has adopted a system-based (i.e., systemic) approach to phonology as opposed to a syntagmatic one. (One of the many consequences of this is that phonological "features" are interpreted as paradigmatically as terms in phonological systems, not syntagmatically as components of phonemes — Jakobson's distinctive features, taken over by generative phonology.)

As he worked with classical Firthian system-structure theory, Michael Halliday (e.g., Halliday 1961, 1992a) found that he had to make a number of changes. Firth had posited different **levels of analysis**, but he treated them as simultaneous perspectives on linguistic patterns without any ordering. In contrast, Halliday introduced ordering among the "levels" (later to be renamed "strata" to avoid confusion with other uses in linguistics of the term "level"). Part of Halliday's motivation was to focus on the inner level of language — grammar — to supplement Firth's focus on the outer levels, viz. phonetics and phonology, below grammar (e.g., Firth 1948) and context, above grammar (e.g., Firth 1957b). Firth had published very little on grammar, apart from the syntagmatic concepts of collocation (for lexis) and colligation (for grammar) (Firth 1968b). Colligation seemed dormant for a long time but was later picked up

Table 4.2 Paradigmatic and syntagmatic organization represented within the grammatical and lexical zones of lexicogrammar

Axis:	Grammar	Lexis
Paradigmatic	Grammatical system	Lexical set
Syntagmatic	Colligation > structure	Collocation

by certain corpus linguists (e.g., Sinclair 1987, 1991). However, collocation really took off in the 1960s since it lent itself immediately to corpus-based investigations. In any case, both are syntagmatically based conceptions, and need to be supplemented by paradigmatically-based ones (cf. Halliday & Matthiessen 2014: Chap. 2): see Table 4.2.

In his attempt to develop the account of grammar, Michael Halliday (e.g., 1956, 1959) started with Chinese and that led him to scale-&-category theory (Halliday 1961). Halliday (2013) said that he had given this manuscript to Firth and was all set to receive Firth's comments on it at a conference in 1960 but Firth passed away the day before they had arranged to meet at the conference. So Halliday never got to hear Firth's reaction to his manuscript.

In his 1961 paper ("Categories of the Theory of Grammar"), Halliday still retained the balance between **system** and **structure** (paradigmatic and syntagmatic); they still had equal status. Then later in the 1960s (see Halliday 1966a), he pushed this further and explored the possibility of deriving structure from system (to put things in slightly generative terms). That paved the way for systemic theory, where system was made primary. That was a way of theorizing the notion of language as a resource: the organization of a language as a resource is brought out most clearly in the representation of paradigmatic patterns by means of system networks.

Isaac Mwinlaaru: I have been going through the literature to find when the term "Systemic Functional Linguistics" was first used. The term "systemic theory" was first mentioned in Michael Halliday's (1966a) "Some Notes on Deep Grammar". When was the functional component added? When was the theory called "systemic functional"?

Christian Matthiessen: That is an interesting question. If we take a corpus-based approach using Google's Ngram Viewer to examine Google Books from 1950 to 2019 (Fig. 4.2), we find that the term "**systemic linguistics**" began to appear around 1970, and "**Systemic Functional Linguistics**" around 1985, the year the first edition of Halliday's (1985) *IFG* was published. The term "systemic linguistics" peaked around 1990, and was overtaken by "Systemic Functional Linguistics" at the turn of the century. Of course, this is a crude picture — on the one hand, we would need to check the books in which the terms appear, and the other issue is that I have left out acronyms, in particular "SFL", since it can stand for many names, including "Students For Liberty", "Southern Football League", "Swiss Football League", "Société Foncière Lyonnaise", "Space Flight Laboratory" and "Solid Fuel Generators".
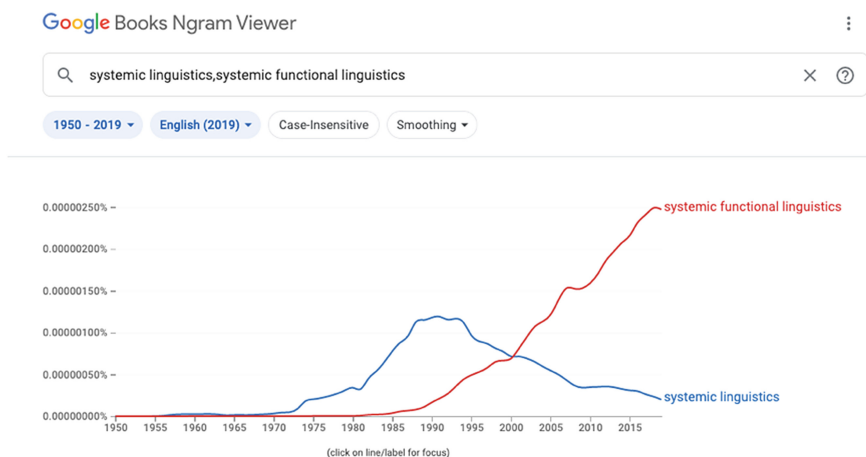
Fig. 4.2 The occurrences of "systemic linguistics" and "Systemic Functional Linguistics" from 1950 to 2019 according to Google's Ngram viewer

The paper Halliday published in 1966 aimed to explore the view that the systemic organization of language is primary (Halliday 1966a). He characterized it as "deep" since this might have made it accessible to linguists accustomed to the Chomskyan metaphor of "deep structure"; but of course this metaphor was not part of the Firthian and Hallidayan tradition. The paper was concerned with the following questions: What mode of organization should be treated as primary? What was a theory in the representation that can be used as the environment for specifying the syntagmatic structural patterns? As I have noted earlier, it was the introduction of systemic organization and the representational tool, i.e., the system network, that allowed him to adopt new patterns of organization — clusterings of systems in the system network that he interpreted by means of **the theory of metafunctions** that he developed. He had actually reached this point before 1966, because he had begun to outline a systemic description of the grammar of English a couple of years earlier. This description included sketches of major clause systems (cf. the system networks for the clause and groups in Halliday 1976), and has been called the **Bloomington Grammar**, because he presented his descriptions in Bloomington, Indiana. About 20 years later, Michael and I worked together in Bloomington during a six-week semiotics symposium, where he gave a course on "the grammar of daily life"; often sitting at outdoor cafés, we prepared the initial sketch of our description of the ideational semantics of English. We called it the **Bloomington Lattice** (Fig. 4.3), and documented it years later in our book on ideational semantics — *Construing Experience through Meaning* (Halliday & Matthiessen 1999/2006). That was the Bloomington connection.

The "systemic" part of Systemic Functional Linguistics was, as I have noted, the focus in Halliday (1966a); and it was illustrated in Halliday's (1969) brief overview of the systems of the English clause (see further below). The "functional" aspect
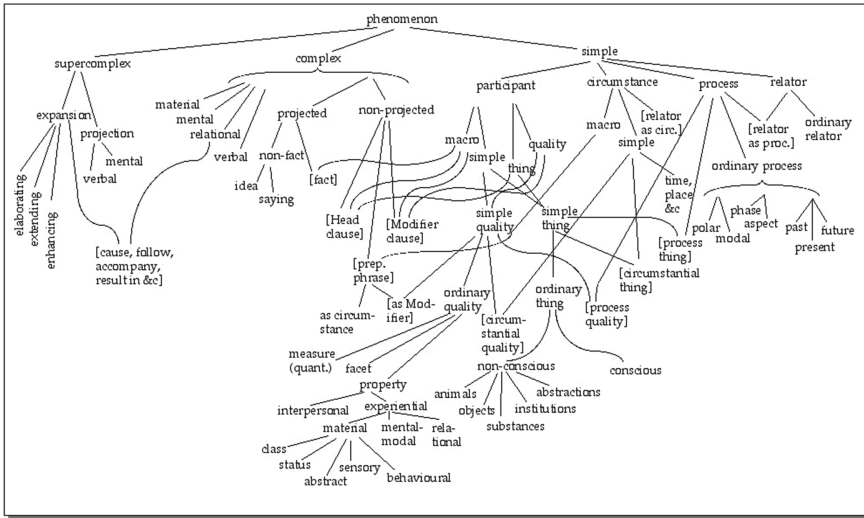
Fig. 4.3 An early sketch of the "Bloomington Lattice"

was initially foregrounded in a number of publications between 1967 and 1970. The experiential and textual metafunctions were illuminated in his "Notes on Transitivity and Theme in English" (Halliday 1967a, b, 1968), and the interpersonal metafunction was illuminated in his earlier description of mood and modality (Halliday 1970a).

By then, the structural aspect had also been worked out, as can be seen in another 1970 paper (Halliday 1970b). If you trace syntagmatic representations of the different metafunctional "strands" from the mid-1960s into the early 1970s, you will see his attempts to accommodate the metafunctional strands in the structure. For example, in the "Bloomington Grammar" from 1964, published in Halliday (1976) and then in Halliday (2002), the interpersonal functions of Subject, Predicator, and Complement are annotated experientially by means of transitivity superscripts (as shown in Fig. 4.4; see also Huddleston 1965, 1988). The representation of clause structure in terms of S, P, C, and A was used also outside SFL — or variants of it such as S, V, O, and A (cf. the Quirk grammars[3]). However, later in the 1960s, the metafunctional strands of the clause were represented as separate layers, each in its own right (e.g., Halliday 1968, 1973b).

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[3] I remember Jan Svartvik commenting in class in the second half of the 1970s that Halliday advised against mixing class and function labels — hence P for Predicator rather than V for Verb. We find the mixed labels in the literature on "word order", of course, where the interpretation of S is particularly open to question.
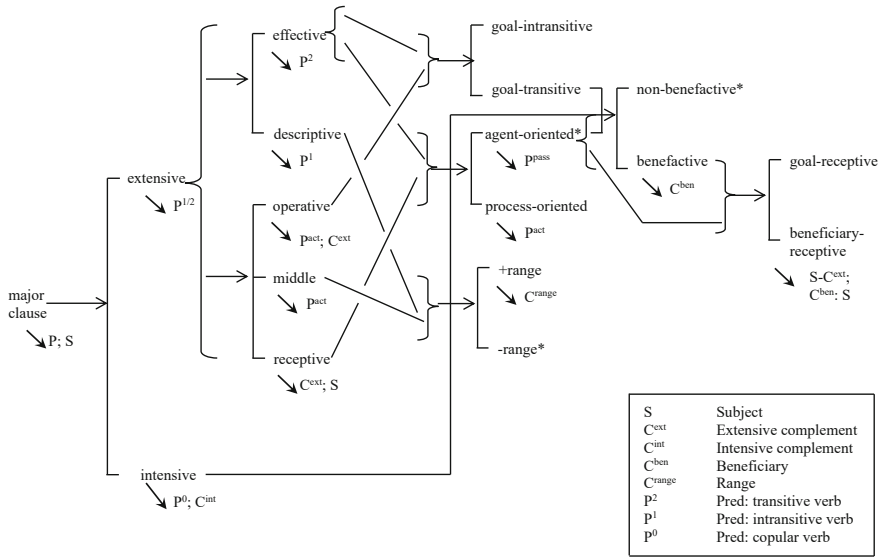
Fig. 4.4 The system of transitivity in the "Bloomington Grammar", with interpersonal functions (S, P, C) superscripted by transitivity specifications (from Halliday, 1976)

extensive: (effective/operative: (goal-intransitive))

John threw; Mary washed (sc. the clothes)

extensive: (effective/operative: (goal-transitive: non-benefactive))

John threw the ball; Mary washed the clothes

extensive: (effective/operative: (goal-transitive: benefactive))

John gave the dog a bone; Mary washed the boys their clothes

extensive: (effective/middle)

Mary washed (sc. herself)

extensive: (effective/receptive: (agent-oriented: non-benefactive))

the ball was thrown; the clothes were washed

extensive: (effective/receptive: (agent-oriented: benefactive: (goal-receptive)))

the bone was given to the dog

extensive: (effective/receptive: (agent-oriented: benefactive: (beneficiary-receptive)))

the god was given the bone

extensive: (effective/receptive: (process-oriented))

the books sold; the clothes washed

extensive: (descriptive/operative)

the sergeant marched the prisoners

extensive: (descriptive/middle: (+range))

Peter jumped the wall

extensive: (descriptive/middle: (–range))

Peter jumped; the prisoners marched

extensive: (descriptive/receptive)

the prisoners were marched

intensive: non-benefactive

Mary seemed happy; Mary made a good wife

intensive: benefactive

Mary made John a good wife

Thus from the early 1970s onwards, we find the kind of polyphonic, or layered, functional structure that we still use today. In this period, Halliday (1969) published "Options and Functions in the English Clause" in *Brno Studies in English*, a Czech journal. The paper was a sketch, providing a clear sense of the systems as well as the structures. Then, "system" also came through in his chapter on functional interpretation of language and grammar collected in Basil Bernstein's second volume of *Class, Codes and Control* (Halliday 1973b).

Interestingly, it was also in the late 1960s and the early 1970s that he had the challenging opportunity to explore the functional nature of the ontogenesis of language in *Learning How to Mean* (Halliday 1975), with the birth of his son in 1969 for the case study. He also had to develop an account of a functional precursor to the meta-functional organization of adult language, not treating the metafunctions of language as suddenly bursting onto the scene from nothing, but to show the development from **micro-functions** through **macro-functions** to **metafunctions**. That really enriched the functional part of systemic functional theory. Interestingly, among functional theories, his theory was the only one providing an evidence-based account of how the functional organization of post-infancy adult language would develop in the individual. You could also think of it in the species, not just ontogenetically, but also phylogenetically, as I have suggested elsewhere (Matthiessen 2004a).

4.3.2 From Scale-&-Category Theory to Systemic Functional Theory

Isaac Mwinlaaru: What are the most important changes from scale-&-category theory to systemic functional theory?

Table 4.3 The development from scale-&-category theory to systemic functional theory

Feature	Scale-&-category theory	Systemic (functional) theory	
		Systemic theory	Systemic functional theory
Axial balance	System and structure: equal status	System primary	
System	Systems located in places within the structure of units	Systems with units as their domains	
Structure	Structures consisting of elements	Structures consisting of elements, annotated with superscripts	Structures consisting of functional "layers"
Metafunction	–	–	Metafunction: ideational (experiential, logical), interpersonal, textual

Christian Matthiessen: Let me begin by summarizing the differences between scale-&-category and systemic (functional) theory: see Table 4.3. Analytically, we can identify two transitions: from scale-&-category theory to systemic theory, and then from systemic theory to systemic functional theory.

In scale-&-category theory, structures consisted of elements — which served as the places where systems operated (as in Halliday 1961), but structures were not polyphonic or layered since this development depended on the formulation of the theory of metafunction. Systems operated at places in the structures of units, as in Firthian system-structure theory. They had not yet been freed from places so that they could become the global principle of the organization of a given unit.

Naturally enough, there had always been a lag between the theoretical frontier of SFL and published introductions to SFL. For a long time, there was no introduction, so Margaret Berry (1975, 1977) produced two volumes in the 1970s. But they did not fully incorporate the systemic functional aspect and essentially only had the scale-&-category theory component.

But let me contextualize these developments. Going back to the 1960s and into the 1970s, a number of linguists of Michael Halliday's generation, and the one following, accepted Chomsky's agenda — his questions about language, and worked within his Transformational-Generative Grammar. For example, Hudson (1967, 1971) tried to answer Chomsky's questions in non-transformational ways by developing **a generative systemic grammar**. He attempted to show that you could generate structures from system networks with associated realization statements as an alternative to a transformational grammar with phrase structure and transformational rules. He continued working on this task, and that led to his **daughter-dependency grammar** (Hudson 1980; Schachter 1980), which had the systemic part of Systemic Functional Grammar but without the foregrounding of the functional organization, and which added dependency structure to the representation of structure. Dependency was part of the contribution by the Modistae to the theory of grammar (see e.g., Covington 1984), and it was explored and developed by Lucien Tesnière (1959) in his posthumous book on syntactic theory, making its way into linguistics again; and then it was

picked up by Hays (1964) in computational linguistics, where it has been applied successfully (see e.g., Jurafsky & Martin 2019: Chap. 15).

In contrast with many linguists in the 1960s, Michael Halliday did not accept Chomsky's questions about language and the agenda that it represented (nor did he accept the view that linguistics was only part of, or closely aligned with, cognitive science). Instead, he continued working with and developing the agenda for research and application that he and his colleagues had been formulating since the 1950s. The approach he later called **appliable linguistics** (e.g., Halliday 2008).

Scale-&-category theory was broadly functional in the European tradition in its treatment of language as a resource, text as something that should be accounted for as part of the description of language, and text analysis as an important task for linguists. You already see this in Michael Halliday's (1964) "Syntax and the Consumer" presented at Georgetown University. He contrasted some of the Chomskyan questions or goals for linguistics, and said that he was trying to develop an account that could be used in the analysis of text. The title of the paper revealed his attempt to create a space within linguistics that was expansive enough to accommodate different questions and approaches so as to help people realize that the kind of theory he was developing was really dependent on the "consumer needs", just as other theories were, including Chomsky's.

As an aside, I came across this paper long before I met Michael Halliday, and was intrigued by his approach to diversity in linguistics. Once I met him, I asked him about how the notion of "syntax and the consumer" had been received and how people at Georgetown in the US reacted. He said: "Well, I was laughed out of court." The reason why they laughed at him was that they were convinced that there could only be one true theory. The whole notion that theory was somehow adaptable or itself functional with respect to its context (the metacontext of doing linguistics) was not something that people could conceive of at the time. I suspect that the notion of "syntax and the consumer" would be accepted quite generally now. The acceptance of variation within linguistics has come a long way since the 1960s.

This is one of a number of areas where Michael Halliday was ahead of the thinking at the time, at least in what had become "mainstream" linguistics, so his ideas seemed "strange" to many other linguists.

4.4   The Meaning of "Theory" in "Systemic Functional Theory"

Isaac Mwinlaaru: SFL has been criticized for being Anglocentric (e.g., Butler 2005). In your publications (e.g., Caffarel, Martin & Matthiessen 2004; Matthiessen 2014a), you suggest that *Introduction to Functional Grammar* (*IFG*) (e.g., Halliday 1985; Halliday & Matthiessen 2014) is meant to be a description of English grammar rather than a representation of systemic functional theory. That makes SFL different from not only the generative works, but also the functional works that merge description
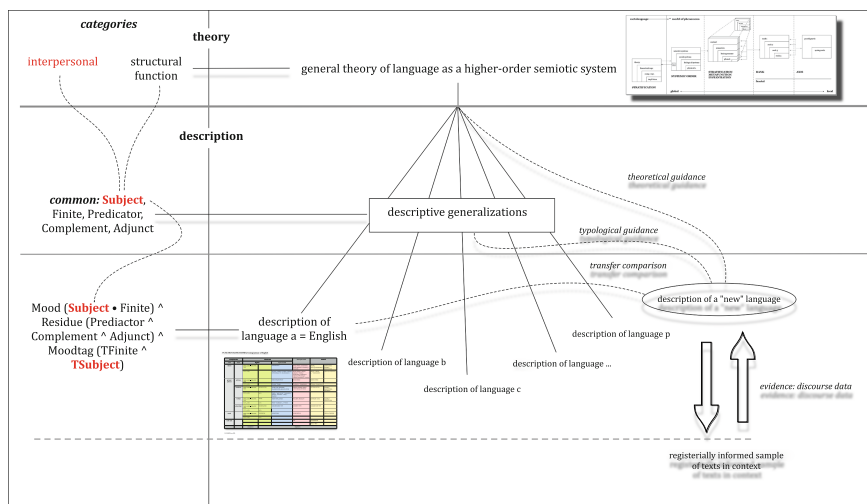
Fig. 4.5 The distinction between theory and description; descriptive generalizations across descriptions of particular languages

with theory (e.g., Dik 1978; Van Valin & LaPolla 1997). What then does "theory" in "systemic functional theory" mean?

Christian Matthiessen: I talked about the distinction between the general **theory** of language as a human semiotic and **descriptions** of particular languages at the conference on functional language typology organized by the Martin Centre for Appliable Linguistics in Shanghai[4] in December 2015. An invited speaker, Nick Enfield, asked some questions about it, and it was clear that coming from a different tradition, he was, understandably, struggling to make sense of it. In the most general terms, theory is the theory of language as a general human system. Description involves the descriptions of particular languages that manifest this general human system. In principle, the distinction is very clear: see Fig. 4.5.

You can trace the distinction back to Firth's (1957a, 1968a) as well as Hjelmslev's (1943) thinking in the European tradition. One key reason for making the distinction is to make sure that linguists do not foist the description of one language, typically a "dominant" one at the time, like Latin, Spanish or, since the beginning of generative linguistics, English, upon another. So, by ensuring that the theory is very general, we are trying to avoid what had happened up through the nineteenth century when people producing grammars of "new" languages around the world imposed Latin categories posited in traditional descriptions on them, looking for categories such as the Latin case system, vocative, nominative, accusative, oblative and the Latin tenses.

I have referred to a description of the grammar of Mayan by Tozzer (1921), who was part of the Boas descriptive tradition. In his preface, he emphasized how important it was to treat a language in its own terms (pp. 7–9):

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[4] http://martincentre.sjtu.edu.cn.

> The Spanish priest thought he had successfully written a grammar of a native language if he had found forms in that language to correspond to every term in his Spanish grammar. The desire to find words which fitted the different categories of thought expressed in his own grammar often outweighed his keenness in realizing that many grammatical forms used in Spanish could not be properly expressed in the native language. Parallels were sought for every form in the Spanish or Latin. […]

> The whole difficulty lies in the fact that it is impossible to build up a grammar of a primitive language by following a Latin or Spanish model. This rigid adherence to such a model leads to two defects. Forms are given to the investigator, often after repeated questioning, which only vaguely express corresponding forms in Spanish or Latin. These are often unnatural and are compounded so as to express in a most artificial way the idea desired. The second defect is the greater as scores of native expressions are entirely overlooked and are never recorded in the early grammars as there are no forms corresponding to them in Latin.

> The only possible method of approach to the study of a primitive language is an analytical one, working out the different thought units and the methods of expressing these entirely divorced from any model based on Latin or Spanish lines.

(We have to interpret "primitive language" as a technical term, as when we read other contemporary accounts, such as Malinowski's.) The emphasis on treating every language in its own terms was also very much part of the Firthian tradition. If you look at Firth (e.g., 1968c), you will find that he is very suspicious of universals. Here the metatheoretical distinction between theory and description is thus also very important. In view of the tradition of foisting "European" linguistic categories construed by means of traditional grammar, where there is no clear distinction between theory and description, onto other languages, we should be both cautious and humble. When I look at various approaches to language outside SFL, I find that what would be considered descriptive categories in SFL in the course of the description of particular languages are treated as part of theory — for example, in Lexical– functional Grammar (LFG) (e.g., Kaplan & Bresnan 1982; Bresnan et al. 2016), in Functional Grammar (e.g., Dik, 1978), and in Role and Reference Grammar (RRG) (e.g., Van Valin, 1993; cf. also various publications informed by RRG, e.g., Van Valin & LaPolla 1997). The reason for this is: you have to motivate a description in particular languages.

In terms of the differences between *IFG* and Simon Dik's (1978) Functional Grammar, *IFG* is an introduction to the systemic functional description of English, but also an introduction to at least the functional part of the general theory of grammar, with the description of English as an illustration. To complete that project, one needs a multilingual version of it — which I have been working on for a long time (cf. the typological outlooks in Matthiessen 1995 and also Matthiessen 2004a). When that gets published, it will be easier to see this clear contrast between theory and description. If you have that contrast, then you can talk about descriptive generalizations. You can begin to say: "It has turned out that this notion of the tense system is helpful in the description of a number of languages, so we can say that we can have a generalized characterization, but it is still not part of the theory".

What Simon Dik (1978) tried to do in his Functional Grammar was to provide a framework for typological generalizations about languages. We can consider such

efforts in the context in the 1970s between two approaches to language universals — the Chomskyan approach to universals and Greenberg's (1966a, 1978) more empirical approach to universals (which can be said to be resonant with the Prague School tradition). Dik's goal at the time was clearly not to provide comprehensive descriptions of particular languages; whereas for Halliday, that was very important. It was important for theoretical reasons because it is only when you begin to develop a comprehensive description of a particular language that you can actually reason about it systemically, for example detecting fractal patterns. I mean this not only in the sense of the systemic organization of language, but also in the sense of what has been explored in other sciences, theories, general systems of thinking, a holistic thinking about systems of different kinds (whether they are physical, biological, economic, linguistic or whatever), reasoning about the organization or principles, reasoning about emergent phenomena and so on. You can only do that once you have a comprehensive description of particular languages. That is one theoretical consideration. But there are also more applied considerations, including the ability to undertake text analysis to get empirical evidence for the description being developed. In general, comprehensive descriptions are appliable descriptions; they can serve as resource for a wide variety of projects.

In corpus linguistics, scholars have contrasted **corpus-based** and **corpus-driven** approaches (e.g., Tognini-Bonelli 2001). The corpus-based approach means that you have a description and then you look for examples, which is not seen as the ideal since you may overlook phenomena that have not been taken account of in the description but which are manifested in the corpus; in contrast, corpus-driven is seen as the ideal. Corpus-based is in some sense **deductive**: you have your description and you look for examples. Corpus-driven, if it truly exists, would be **inductive**. However, it is likely to be restricted to patterns that tools for automatic text analysis can identify and such patterns are still quite "low-level" (i.e., low in terms of both rank and stratum).[5] What I suggest is a third and important complementary approach to

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[5] The term "corpus-driven" seems to have become fashionable and frequently turns up in various titles of contributions to corpus linguistics; according to Google's Ngram Viewer, occurrences of this term began to increase significantly around the mid-1990s. However, it is important to emphasize the limitations of the "corpus-driven" approach. In a recent introduction to corpus linguistics Zufferey (2020: 8) writes: "This corpus-based research approach is opposed to an approach which considers corpus data as the only point of reference, both in a theoretical and a methodological sense. In this approach, linguists begin their research without an a priori and simply let hypotheses emerge from corpus data (this is called a corpus-driven approach). This approach is almost unanimous among linguists working with an empirical methodology. On this point, we agree with Chomsky's metaphorically explained opinion where he states that working with linguistics in this way would be the equivalent for physicists of hoping to discover the physical laws of the universe by looking out of their window. Observing data without a hypothesis often leads to not being able to make sense of data". The point about the fundamental importance of theory, say in the form of hypotheses, in the observation and then the analysis and interpretation of data has been part of systemic functional research methodology from the start. And if researchers approach corpora empowered by a holistic theory of language, they will also be able to show explicitly what regions of the overall system of language can actually be investigated using current tools and techniques in corpus linguistics — and it will turn out to be "low-level" regions.

methodology, viz. an **abductive approach**. Abduction means working with a gradually developing description and using it in exhaustive analysis of texts. Description gives rise to hypotheses. You test descriptive hypotheses against your data by means of **text analysis**, you revise your description, and this process of shunting between description and text continues, in principle indefinitely.

Text analysis thus feeds into the development of the description that it references, but it is also of value in its own right; it is a key to appliable linguistics. Many projects of addressing problems in human societies have text (discourse) analysis as a central component, and to undertake text analysis, you need comprehensive descriptions (cf. Matthiessen 2014b). But text analysis along these lines has not been on the agenda of Functional Grammar, of Role and Reference Grammar, and of Lexical Functional Grammar.

I remember talking to Simon Dik about this in 1986, as he gave me a lift from a workshop in Nijmegen we had both presented at (Kempen, 1987) to Amsterdam. I asked him whether there were any projects undertaken to produce comprehensive descriptions of the grammars in particular languages in terms of Functional Grammar. He said no. He confirmed that it was not on the agenda because it was not seen as leading to these typological generalizations. That is fundamentally different from Systemic Functional Grammar and SFL in general. In SFL, comprehensive descriptions are essential to typological generalizations. These different aspects are important if people want to compare Role and Reference Grammar or Functional Grammar with Systemic Functional Grammar.

To round this off, let me emphasize that the general theory is not — and should not be — centred on a particular language; but the descriptions of particular languages should be, e.g., the description of Chinese should the Sinocentric, just as the description of English should be Anglocentric — in the sense sketched above: the descriptions of particular languages should be based on empirical evidence from these languages and be designed to bring out language-specific features.[6] When such descriptions are developed, they in turn become data for evidence-based typological generalizations.

4.5 A Multilingual Version of *Introduction to Functional Grammar*

Isaac Mwinlaaru: You have mentioned a multilingual version of *IFG*. Does multilingual mean typological?

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[6] One difficulty here is terminological. While the fundamental distinction between theory and description has been articulated and highlighted again and again in SFL, there will inevitably be contributors who make the mistake of characterizing the description of a particular language as theory — e.g., treating the description of the system of TRANSITIVITY in English as part of the theory, or the description of the system of APPRAISAL in English as part of the theory. They are not; they are systems postulated in the description of a particular language.

Christian Matthiessen: Well, crucially the multilingual version of *IFG* includes reference to key systems in a range of languages, including the systems of THEME, MOOD and TRANSITIVITY. While it is not practically possible to present comprehensive text-based, meaning-oriented descriptions of these and other systems that are comparable to the description of English in *IFG* for a good range of languages — even at a low degree of delicacy, it is vitally important to draw on such descriptions. Fortunately there is a growing number of them, framed in terms of SFL (e.g., Teruya & Matthiessen 2015; Mwinlaaru & Xuan 2016; Kashyap 2019) and other frameworks that support descriptions of the kind I referred to. This is thus a core aspect of the multilingual *IFG*, and it is possible to base generalizations on the comprehensive descriptions that are referenced.

But where does typology come into this account? Generalization across existing comprehensive descriptions is clearly not language typology in a strict sense because the sample of such descriptions is too small and at the same time not representative in terms of methods of sampling — any method of sampling we might favour (there are different candidates to consider). Existing typological generalizations that seem to be reliable in these two related respects tend to be focussed on phonological features and on "low-level" lexicogrammatical features — features that are accessible in principled samples of large numbers of languages, usually because they are overt rather than covert in character so included in reference grammars and other descriptions that typologists rely on. Many of the very valuable books on language typology and books designed to help linguists in their descriptions of "new" languages — from say the Greenberg (e.g., 1978) volumes from the late 1970s, via the two editions of Shopen's (2007a, b, c) three-volume *Language Typology* to Dixon's (2010a, b, c) richly informative three volumes of what he calls "basic theory",[7] and all the volumes on particular areas such as ergativity, tense and aspect, voice and evidentiality — do not make very explicit the size and nature of the sample upon which their typological generalizations are based.[8] So often we make do with tentative generalizations, and we remain open to revisions as linguists add new descriptions, expanding the database.[9] Such tentative generalizations are included in the multilingual version of *IFG*. Their purpose is to guide students and researchers in their multilingual engagement. While there has been emphasis since the 1960s on developing constraints on

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[7] From a systemic functional theory, his "basic theory" can usually be interpreted as descriptive generalizations — rather than as the theory of the "architecture" of language (cf. Halliday, 2003; Matthiessen 2007).

[8] We can take WALS as a frame of reference; here samples vary in size (and so in typological representativeness) from a few hundred languages to around 1,400.

[9] An instructive example comes from the work on the typology of "word order" (i.e., the sequence of elements in different grammatical units). Greenberg's (1966b) "universals" were based on a sample of around 35 languages. Some of them have held up, like the correlation between the sequence of "V" and "O" in clauses and the adposition and its complement in adpositional phrases, but the correlation that Greenberg had found between "V" and "O" in clauses and "N" and "A" in nominal groups proved not to hold up, as shown by Dryer (2013) based on a sample of 1,316 languages.

what a possible language is, we also have to highlight the need to stimulate the ***imag-ination*** of new descriptivists — part of which is to provide them with "etic pools" (in the sense of Tagmemic Linguistics) to dip into.

In principle, the multilingual version of *IFG* will cover the different considerations represented in Fig. 4.5 — that is, general theory as a descriptive guide, descriptions of particular languages, typologically oriented generalizations across these descriptions. In the 1960s, Michael Halliday (1966b) talked about "descriptive transfer and comparison". You work with one language, and you seem to transfer this description from another language. (Elke Teich 2002 has taken this up.) I remember Michael Halliday commented on the first description of a "new" language (i.e., one that had not been well described before) as a Ph.D. thesis. Once they had completed their Ph.D. theses, he would tell them: "Great, but now do it again, as if English (or Chinese or whatever major well-described language had influenced the description) had never existed. Forget about all you ever knew about the description of that language." This was not at all a put-down, but an encouraging reminder of the awesome challenge of describing a language in its own terms, doing justice to it. In Jim Martin's (e.g., 1983, 1985, 1990) work going back to the 1980s, you can see how he described different aspects of Tagalog, like the system of TRANSITIVITY, in terms that are independent of English and empirically grounded in Tagalog — which of course means based on evidence from text, probed my means of proportionalities, focussed on reactances as well as overt markings.[10]

4.6   Michael Halliday's Unfinished Work on Systemic Theory

Isaac Mwinlaaru: Is there a plan to write a book on systemic theory without reference to any particular language, like outlining the theory in general?

Christian Matthiessen: Yes, and that was certainly the plan Michael Halliday and I formulated in the 1990s. We began writing two volumes of *Outline of Systemic Functional Linguistics* (Halliday & Matthiessen in preparation), but then other tasks intervened and slowed us down. We had to finish *Construing Experience through Meaning* (Halliday & Matthiessen 1999/2006). Then there was the exciting project of the third edition of *IFG* (Halliday & Matthiessen 2004). So, it is something that I feel is urgent to return to. A good part of it has been written, but I need to update it;

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[10] The term "reactances" relates to Benjamin Whorf's concept of cryptogrammar. There are certain grammatical meanings or feature oppositions that are not overtly marked in a language but once we probe the system we can find latent indicators of the hidden meanings. An example is the distinction between attributive and identifying clauses in English. While there is no overt marking for this distinction, identifying clauses allow an inversion between Token and Value (e.g., *The man is the teacher; The teacher is the man*) while attributive clauses do not allow this inversion (e.g., *The man is a teacher; *A teacher is the man*). These latent indicators of grammatical meanings and feature oppositions are referred to as reactances.

and I will revise our discussion about how to engage with criticism. Some criticisms, like the claim that SFL is Anglocentric, were fundamental misunderstandings of the roles played by the theory of language as a shared human semiotic, descriptions of particular languages and also the metatheory. The description of English in *IFG* should be Anglocentric. A description of Chinese should be Sinocentric. A description of Akan should be Akano-centric. That is important. The general theory, which does not contain elements or fragments of languages (like subject, tense or finite), should not embody aspects of any one particular language.

But in a way, for the community of descriptivists outside SFL, there should, in principle, be more resonance now with the systemic functional approach to descriptions. The great respect for the Sapir-Whorfian tradition and the awareness of the danger of rushing to posit universals have now led to calls for more meaning-oriented descriptions of grammar — an approach that is much more sensitive to, alert to, and aware of the particular languages (cf. Ameka et al. 2006; and also the frontal attack on the myth of language universals by Evans & Levinson 2009, a contribution which is arguably very Firthian and Hallidayan in spirit).

The intellectual environment in linguistics has developed in positive ways — often in the direction of SFL, even if this is not made explicit. This has happened across a wide range of areas. For example, if we compare positions that Michael Halliday took in the 1960s which were considered odd at the time and positions that are generally accepted or widely supported today, we can include (cf. Matthiessen 2015): the notion of language as a probabilistic system, the relationship between text and grammar, the continuity between grammar and lexis, the "semanticky" nature of grammar — its orientation towards meaning. The difficulty for people working in SFL is that once linguists from other traditions arrive at similar positions, they often do not refer to the systemic functional work because they are not aware of it since it has been marginalized for so long. So, you do not get the support you would hope to get from the other communities of linguists. But I am sure you could point to similar examples in other disciplines and other sciences when insights during a particular period are ahead of their time (one famous example being the theory of tectonic plates).

4.7　Metafunction in Systemic Functional Linguistics

Isaac Mwinlaaru: What does metafunction mean in SFL?

Christian Matthiessen: The term is, of course, unique to SFL; Michael Halliday introduced it (later apologizing jocularly for the Graeco-Roman combination) because there was no term devoted to the sense of function as an intrinsic function — as a principle in the overall organization of language (cf. Halliday & Hasan 1985; Martin 1991). One helpful way of exploring **metafunction** is Halliday's (1975) work on ontogenesis; he shows how a young child learns how to mean, developing a child tongue or protolanguage in interaction together with his or her immediate caregivers

starting around the age of 8 months. Protolanguage is organized in terms of a small number of **micro-functions** (increasing from 4 to around 7). As the child begins a transition from protolanguage to language, the micro-functions are first generalized into two **macro-functions** and the macro-functions are then transformed into abstract functions, the metafunctions of post-infancy adult language: see Fig. 4.6.

Micro-functions are local meaning potentials that are tied to particular situation types — particular contexts of use, so at this stage, the functions of language equal the uses of language. Then, as the system expands, children begin to generalize these micro-functions into two more general functions — **macro-functions**, i.e., the mathetic — resources for learning about the world — and the pragmatic — resources for getting things done. They are more general than the micro-functions, but they still cannot be combined; at this stage, children can only mean either mathetically or pragmatically.

As a child begins to move into the mother tongue, the breakthrough comes when the alternatives in meaning become simultaneous, paving the way for the alternative macro-functions to become simultaneous metafunctions. That is when language begins to be organized into simultaneous systems within the overall meaning potential, enabling speakers to mean more than one thing at the same time.

If we put the different notions of functions on a scale of degrees of functionality, ranging from functionality essentially limited to structural functions to functionality as a principle of the overall organization of language, Lexical-functional Grammar (LFG) will be at one end of the scale (i.e., structural functions) and Systemic Functional Grammar (SFG) at the other (functional organization); the Prague School will
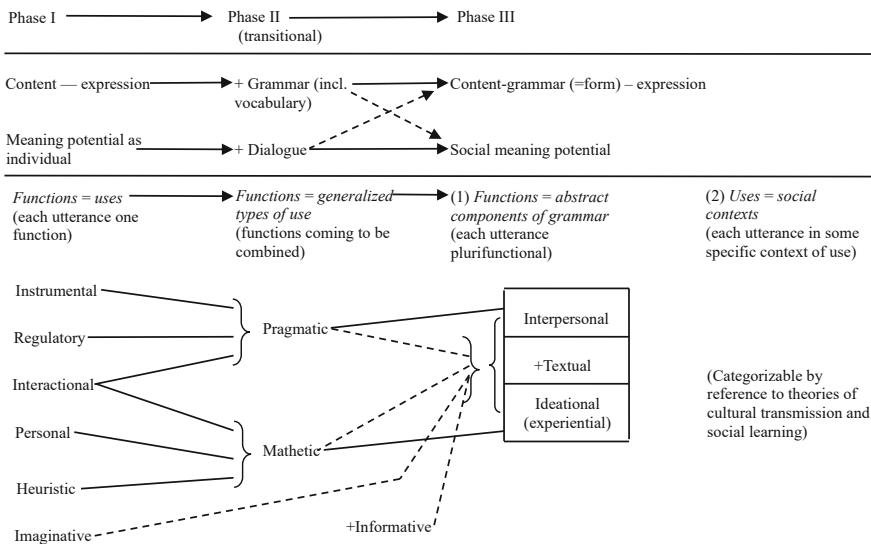
Fig. 4.6 Halliday's (1975) summary of the functional development from protolanguage (Phase I) via a transition into the mother tongue (Phase II) to the mother tongue (Phase III)

Table 4.4 Scale of functional pervasiveness — manifestations in different grammatical theories

"Base":	Structural functions	+ Functional components = levels	+ Metafunctions × strata
Systemic [paradigmatic]			SFG
Structural [syntagmatic]	LFG	FG, RRG — Prague School	

be located towards the other end (structural functions), and Functional Grammar and Role and Reference Grammar (RRG) will be positioned somewhere in between the two poles. (This is represented in tabular form in Table 4.4, where I have added the distinction between systemic base — priority given to the paradigmatic axis — and structural base — priority given to the syntagmatic axis). Historically, Halliday "discovered" the metafunctions precisely because he had chosen a systemic base — he had opted to prioritize the paradigmatic axis. This is where the metafunctional organization can be seen most clearly permeating the content plane — both semantics and lexicogrammar; but of course once this principle had been discovered, it was also possible to see that it was manifested in modes of structure (Halliday 1979, 1981; Matthiessen 1991, 2004b; Martin 1996).

One may ask why some linguists have not paid more attention to SFG or, more generally, to SFL. There are a number of factors relevant to the explanation, but let me just mention one here, by referring to Douglas's (1984) explanation of taboo. She suggests that phenomena may be treated as taboo in cultures if they pose problems for experiential taxonomies when they have mixed properties like shellfish and pigs. Thus pigs are taboo because they have features that crosscut established classes. They are hoofed, and yet they are not like the general class of hoofed animals. Mary Douglas regards this as a way of taking pressure off the classification and the taxonomy by removing the categories as well as the phenomena that are problematic.

In what sense is SFG like a pig? It is both **formal** and **functional**. It is functional in considering language as being fundamentally functionally organized and in explaining language in functional terms. But at the same time, it shares features with formal grammars in the special sense of being concerned with **explicitness**, i.e., explicit representation of the structure and explicit representation of systems. System networks and realization statements may not be sufficiently computationally explicit, but they are certainly much more explicit than anything you can get in accounts informed by West Coast Functionalism. It is difficult for people to see the value of SFL, which is a comprehensive theory and is used in different disciplines like educational linguistics and computational linguistics. So, how would you deal with SFL? In the same way, you deal with pigs and shellfish: you can declare it as a taboo, and then you do not have to deal with it.

4.8   Conventions of Technical Terms in Systemic Functional Linguistics

Bo Wang: The technical terms in SFL are written in different ways, e.g., in all caps (MOOD), in small caps (MOOD), with an initial upper-case letter (Mood), and in all lower-case letters (mood).

Christian Matthiessen: Yes, not everybody follows these conventions, but Michael Halliday and I do. I think it is helpful to follow them, even crucial to ensure that all users have clear understanding not only of the terms but also of the categories that are manifestations of. (One interesting problem is that people use initial capitals to signal technical terms of any kind, so this has to be discouraged; it leads to considerable confusion.) These conventions may seem like an additional roadblock that is not very helpful; however, once one adopts them, they will be a good way of checking whether a term is used correctly or not. I have found that if people are encouraged to follow the conventions, they become more sensitized to important distinctions and can achieve greater mastery of the theory.[11] In my work, it has been very important in producing descriptions to be used for explicit modelling on the computer. Part of it is necessitated by the fact that one and the same term may be used in related different ways. One example is "mood": MOOD is the name of the major grammatical system of the clause — the system for realizing the semantic system of SPEECH FUNCTION; the primary system is MOOD TYPE, with the two terms being "indicative" vs "imperative". It is also the Mood element in the interpersonal structure of the English clause. Then it is useful to distinguish between the two. "Mood" is not the only term that is used in more than one way: see Table 4.5.

The principle is: if it is a name of a system, then it is in all caps, or small caps, which may look more aesthetically pleasing, e.g., systems of MOOD (MOOD), TRANSITIVITY (TRANSITIVITY), THEME (THEME), FINITENESS (FINITENESS), TONE (TONE), and KEY (KEY), which applies to all strata. A structural function, e.g., Subject, Actor, Theme, in grammar, and Onset and Nucleus in phonology, is in initial upper case, and in initial upper case only. The names of systemic terms or features are in all lower case; if necessary, I will put single quotes around them to indicate that they are used technically as names of systemic terms, as with "positive", "negative"; "indicative", "imperative".

You also get deviations from the convention just sketched above. One issue is that, as mentioned above, people for some reason love initial capitals, so they will use the initial capital to mark any terms as a technical term, regardless of its theoretical status. Another thing is that people have invented other conventions, e.g., Suzanne Eggins's (2004: 153 ff.) capitalization of MOOD and RESIDUE as structural functions. You get pulled in different directions, but the convention I described is what you have in

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[11] This is an obvious point. Similar conventions used in linguistics in general have proved to be very helpful, e.g., the convention that technicalized grammatical items in interlinear glossing such as PERF, NEG, ACC should be written in small caps. This helps research students in training to master the skill of glossing grammatical items using terms from a technical vocabulary, and it also helps readers "decode" interlinear glosses more quickly, getting a sense of grammatical patterns.

Table 4.5 Examples of terms and capitalization conventions

Term	Name of system	Name of feature in system	Name of structural function	Other
mood	MOOD		Mood	mood Adjunct
theme	THEME	unmarked theme/marked theme	Theme	
finite	FINITENESS	finite/non-finite	Finite	
conjunction	CONJUNCTION			conjunction [word class]
phenomenon, phenomenality	PHENOMENALITY	phenomenal/macro-phenomenal/meta-phenomenal	Phenomenon	

IFG and other documents that follow these long-established conventions. Maybe it is an initial stumbling block, but once one begins to use it, it is actually quite helpful in sorting out what one is doing.

As I said, people have tried different conventions and different approaches to the choice of technical terms, and it is interesting to review them. There should be a whole paper on it to describe the way that linguists have explored strategies or tactics for making clear the metalanguage (language about language). Many years ago, Dick Hudson tried to use terms in French when he was using language to talk about language. In the foreword of *Semantics*, Lyons (1977) wrote that his initial intention was only to use technical terms that were different from the ordinary language, but he found it impossible. What he did was to have a star asterisk for all technical terms to clearly distinguish technical and non-technical expressions, as in "Peirce's definition of symbol* rests upon the conventionality or arbitrariness of the relationship between the sign and its signification." (Lyons 1977: p. 100). While this annotation alerts readers to the use of everyday terms in technical ways, it does not seem to have been taken up by other linguists. Hjelmslev's (1943) solution was to invent a great number of new terms, including *glossematics*; but they have tended to make his work fairly inaccessible.

In languages where you have lexical resources of different origins (e.g., Japanese with Chinese and native Japanese, English with native Germanic, Norman French, Latin and Greek), there is some potential for differentiation (cf. the exploitation of this in legal English, as with *pith and substance*). The question is how to exploit this effectively in the metalanguage. In the description of English in SFL, there is a certain tendency for grammatical terms to be Greek or Roman, but for semantic terms to be more Germanic. In the Japanese context, scholars adopted different approaches to the choice of terms in SFL used to describe Japanese or talk about language in general. One tradition was to go Sinitic, which means to use technical terms in Japanese that are shorter than terms in the ordinary vocabulary. Unlike English where technical and scientific terms from ancient Greek or Latin tend to be longer, the technical terms in Japanese come from Chinese and are shorter. In another tradition, following Okuda, Kazuhiro Teruya (2007) tried to use native Japanese terms wherever needed because they are more accessible in various contexts, like education. There are always various considerations, and they may push us in different directions.

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Chapter 5
Computational Linguistics

Abstract This chapter first summarizes the contributions of Systemic Functional Linguistics (SFL) to computational linguistics. It elaborates on Martin Kay's Functional Unification Grammar, highlights the achievements of the Penman Project on text generation directed by William C. Mann and comments on the influences from computational linguistics on SFL. The connections between Cardiff Grammar and Nigel Grammar are also discussed.

5.1 Introduction

Computational linguistics, to simplify greatly, is the application of linguistic theory and description to the interpretation, analysis and generation of linguistic units such as sentences and even whole texts in digital systems. Systemic Functional Linguistics (SFL), as an appliable theory of language, has been applied in computational contexts since the 1970s (with precursors in the 1950s and 1960s) in ways that have mutually shaped both the SFL theory and description and advances in computational linguistics. Some of the areas that computational SFL have covered include early attempts in machine translation, parsing and text generation. Of these, text generation represents the most successful endeavour, while parsing, for instance, remains a challenge for the extravagant, meaning-rich description of Systemic Functional Grammar (SFG). Short overviews of computational SFL work are provided by O'Donnell and Bateman (2005), Teich (2009), Bateman and O'Donnell (2015), and Bateman et al. (2019).

In this interview, we choose the 1980s as the point of reference for two reasons. First, it was a period that garnered the most interest in computational SFL. This was however a culmination of earlier efforts that began in the mid-1950s and into the 1960s and 1970s. This was also because, by the 1980s, sufficient SFL descriptive work on English grammar was available and could be implemented in computational settings. The second reason is that this was the time Christian Matthiessen became more actively involved in SFL research (as a Ph.D. student) and also in computational linguistics (as a research linguist at the Information Sciences Institute (ISI) in

Los Angeles). In this interview, we examine the contributions of SFL to computational linguistics and then consider Martin Kay's Functional Unification Grammar as an extension of his initial work in computational SFL. We also highlight the achievements of the Penman Project directed by Bill Mann at ISI, and comment on the influences on SFL theory by computational SFL. Connections between different computational implementations of SFL, notably the Cardiff Grammar and the Nigel Grammar are also discussed.

5.2   Contributions of Systemic Functional Linguistics to Computational Linguistics

Isaac Mwinlaaru: The 1980s was a period of sustained energy in the application of Systemic Functional Linguistics (SFL) in computational context. Can you please give us a little background that led to and shaped this development? And how did SFL contribute to computational linguistics in the 1980s? For instance, what influences did it make in the field and what developments came up from those influences?

Christian Matthiessen: In hindsight, one can relate the remarkably successful developments starting in the 1980s to Halliday's (e.g., 2007, 2008) notion of **appliable linguistics**. I say "in hindsight" simply because Halliday first articulated this conception of linguistics in these terms when he accepted the AILA (*Association Internationale de Linguistique Appliquée*, "International Association of Applied Linguistics") inaugural gold medal in Singapore in 2002. But in a sense, that is what he and others have been working on since the 1950s (with the 1980s being the beginning of a very active period).

In the 1950s, one of Michael Halliday's first experiences was with a very early machine translation project in Cambridge led by Margaret Masterman. There was a small group of people representing different areas of expertise, and Michael Halliday was a linguist in the team. One of the very original and creative members was Arthur Frederick Parker-Rhodes—polymath[1] from Yorkshire at Cambridge; one of his later contributions is a book called *Inferential Semantics* (Parker-Rhodes 1978), where he makes good use of lattices to represent meaning—it is still a source of insight today and well worth reading. In the late 1950s, Martin Kay joined the group, and he became an important figure in connecting SFL and computing in the late 1970s and the early 1980s (e.g., Kay 1979, 1985). Masterman's team explored the possibility of machine translation. One of Michael Halliday's contributions at that time was an article on the notion of a mechanical thesaurus (Halliday 1956a). The default way of thinking about lexis in machine translation (mechanical translation) was the model of the dictionary; whereas Halliday (1956a) suggested that the notion of **thesaurus** could be very relevant to the task.

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[1] See: https://en.wikipedia.org/wiki/Frederick_Parker-Rhodes.

The thesaurus itself has an interesting history. The person who produced the first comprehensive thesaurus of English lexis, Peter Mark Roget (1852), worked on it for fifty years—as a hobby. As a physician, he was influenced by the attempts to design artificial languages in the sixteenth and seventeenth centuries (thought of as universal languages), which led to this thinking about the organization of vocabulary as a resource (cf. Halliday & Matthiessen 1999/2006). Roget's organization of the thesaurus was a pre-systemic and taxonomic arrangement, as I have shown in Fig. 5.1.[2] Significantly, this arrangement gives us a sense of the organization of the lexis as a resource rather than an inventory of items, as in a dictionary. That was Michael Halliday's (1956a) point, which enabled him to foreground a kind of paradigmatic way of thinking about the organization of lexis. So, that was his first involvement in computational linguistics (or what later came to be computational linguistics). I followed up his point about the thesaurus as a model of lexis as a resource in a paper I first presented at a text generation workshop on Catalina Island (Matthiessen 1991). During the question time after my presentation, Aravind Joshi and I had a little debate. An aspect of his model of TAG (Tree Adjoining Grammar; e.g., Joshi & Schabes 1997) was the relationship between the grammar and the dictionary (as in other lexicalist approaches that had been developed by the late 1980s). But the thesaurus model of lexis does not displace the dictionary; rather I have suggested that the dictionary is a view on the canonical thesaurus-like organization of lexis that can be adopted (compiled out) for tasks associated with parsing and text analysis more generally (cf. Matthiessen 1995a; Matthiessen & Nesbitt 1996: 72–79).

Halliday (e.g., 1956a, 1962) published a couple of papers on machine translation, the first in 1956 was already on the notion of a mechanical thesaurus. (In the same year, Halliday 1956b also published an article on the categories in the grammar of Chinese.) Then later in the 1960s, he had a student in London, **Terry Winograd**, who has always had an important sense of ethical choices. When Winograd was young, he would have been forced to go to Vietnam to kill people there, so he went to the UK and studied with Michael Halliday. Winograd produced a pioneering thesis in artificial intelligence (AI), which included a systemic grammar. He developed the well-known SHRDLU system, and continued his interest in AI (e.g., Winograd 1972, 1983). But because he took the ethical position of not accepting funding from the military, which was one of the main sources of funding in the area, it was difficult for him to embark on big projects in AI. He was at Stanford University for many years and published a book in 1983, in which there was a very interesting chapter on the features of functional grammars and a documentation of the influence from Systemic Functional Grammar (SFG). He sent us a pre-publication draft, which was very helpful as we developed the Nigel generation grammar starting in the early 1980s.

There was another strand of early interaction with computational linguistics in Britain, where a Ph.D. student named Anthony Davey (e.g., 1978) created one of the few very early text generation systems in the 1970s. The system was able to

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[2] For some indications of the correspondences between Roget's lexical taxonomy and systemic functional descriptions of lexicogrammar, see Halliday (1976) and Matthiessen (1995a).

I abstract relations
— I existence
— II relation
— III quantity
— IV order
— V number
— VI time
— VII change
— VIII causation

VIII causation
— 1° constancy of sequence
— 2° connection between cause & effect
— 3° power in operation
— 4° indirect power
— 5° combination of causes

164 production
165 destruction
166 reproduction
167 producer
168 destroyer
169 parentage
170 posterity
171 productiveness
172 unproductiveness
173 agency
174 vigorousness
175 intertness
176 violence
177 moderation

II space
— I space in general
— II dimensions
— III form
— IV motion

III matter
— I matter in general
— II inorganic matter
— III organic matter

IV intellect
— division (I): formation of ideas
— division (II): communication of ideas

division (I): formation of ideas
— I intellectual operations in general
— II precursory conditions and operations
— III materials for reasoning
— IV reasoning process
— V results of reasoning
— VI extension of thought
— VII creative thought

division (II): communication of ideas
— I nature of ideas communicated
— II modes of communication
— III means of communication

V volition
— division (I): individual volition
— division (II): intersocial volition

division (I): individual volition
— I volition in general
— II prospective volition
— III voluntary action
— IV antagonism
— V results of action

division (II): intersocial volition
— I general
— II special
— III conditional
— IV possessive relations

VI affections
— I affections generally
— II personal
— III sympathetic
— IV moral
— V religious

Fig. 5.1 The taxonomic organization of Roget's Thesaurus

generate short reports on games of tic-tac-to, using a Systemic Functional Grammar. Thus he demonstrated the value of the orientation towards systemic organization in a grammar for a text generation system. This work impressed Bill Mann greatly.

Another strand was through **Martin Kay**. By the 1970s, Kay had gone to work in the US and was very much involved in computational linguistics as a leading scholar, especially machine translation and parsing. One of the tasks he set for himself as a goal was to develop a computational systemic functional grammar. He never achieved

that goal, but in pursuing it, he developed what he first called **Functional Unification Grammar (FUG)** (Kay 1979, 1985); his introduction of unification was quite influential, and led to a family of unification-based grammars (Shieber 1986; for a formal account, see Francez & Wintner 2012). His approach was to specify a grammar consisting of a large number of functional descriptions of different regions of the grammar, like the chapters in a grammar book, and such descriptions could be merged or unified, e.g., a description of theme could be unified with the description of mood. Unification is a well-known computational operation, and his FUG did not involve any changes in descriptions: unification operates without any transformations (in contrast with transformational grammar). One of his key goals was to create a representation of the resource of the grammar that was neutral with respect to generation and parsing. The parsers that had been developed in computational linguistics were all computational grammars geared towards parsing rather than generation; hence, they had things like Augmented Transition Network (ATN), which were all procedural grammars for the task of parsing.

Martin Kay was at Xerox PARC in Palo Alto, which was fairly close to Stanford University. He had a junior colleague named Robert Kaplan, who got quite interested in Kay's work on FUG and the notion of registers in ATN grammars. Meanwhile, Kaplan worked together with a linguist, Joan Bresnan, who had studied with Noam Chomsky at MIT, where she obtained her Ph.D., and together they developed **Lexical-functional Grammar (LFG)** (e.g., Bresnan et al. 2016; Kaplan & Bresnan 1982). Thus the conception of functional grammar in SFL actually had a direct line into LFG, although if you read the literature in LFG, it is not documented. But if you asked Martin Kay, he would tell you about it. In 1999, during the 12th AILA World Congress in Tokyo, Joan Bresnan was one of the invited speakers, and her brief was to talk about the developments in syntax. At the end of that talk, there was a brave delegate from Norway who asked her about the influence of Halliday's SFG. She answered: "Oh, yes, of course". Then she talked about Martin Kay, Robert Kaplan, and how Kaplan worked with her. It would have been very helpful if it had been easier for linguists who engaged with LFG, which became very dominant for quite a while (even more important than Chomskyan grammars), to trace certain key ideas back to SFG while at the same time attending to parallel developments in SFG.

We can identify a family of grammars that began to emerge in the late 1970s and grew in the 1980s. They were all **unification-based grammars** in the sense just noted above. They can also be characterized as **constraint-based grammars**, with descriptions specified in terms of features and functions (cf. Winograd 1983). They constitute a family of grammars that resemble one another at the level of representation (though less so at the level of theory) (see Sect. 4.1). Thus Systemic Functional Grammar (SFG) belongs to two families of grammatical frameworks ("grammatics" in Halliday's [e.g., 1996] sense), as shown in Fig. 5.2. In terms of the level of representation in the stratification of the grammatical metalanguage, it is a member of the unification-based family of grammars. However, in terms of the level of theory, it is a member of a functional family of grammars on the one hand and of a relational-stratal family on the other. The other members of the functional family
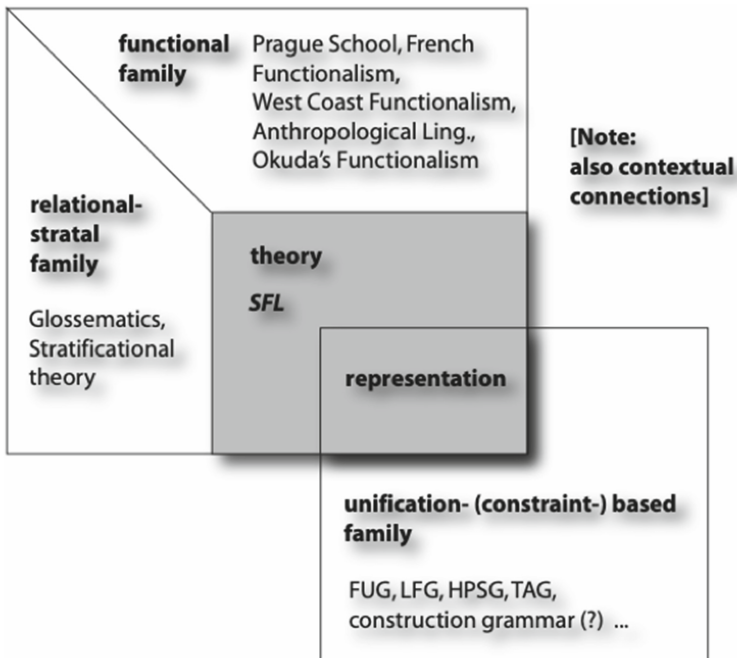
Fig. 5.2 Systemic Functional Grammar (SFG) within SFL as belonging to different families of grammar depending on whether it is viewed "from below" in terms of the level of representation or "from above" in terms of the level of theory

tend not to have a very highly elaborated explicit representational level, so it does not make sense to say that they belong to the unification-based family.

Later in the 1990s, the possibility of importing representational mechanisms from these other traditions was raised by a number of researchers, centrally by **Elke Teich** (1999), who asked how the representational power of SFG in a computational environment can be extended. She suggested that one could turn to other unification-based (constraint-based) grammatical frameworks. So, there was certainly an influence from SFG into computational grammars in this implicit way. Researchers also experimented with another form of representation, viz. **typed feature structures**, which had been developed as part of research into knowledge representation (e.g., Carpenter 1992; cf. Bateman & Momma 1991 on their use in computational SFL).

Throughout the 1980s and into the 1990s, the dominant computational linguistic application for SFL was **text generation**. This was to a large extent due to Bill Mann's initiative at the Information Sciences Institute/USC. Based on exploratory research into text generation in the late 1970s, he decided that the way forward should involve an approach informed by linguistic theory and that the resources developed in the course of a project should be developed over long periods of time, extending far beyond the period of individual projects (which might have a lifespan of two to three years). So with David Webber's help, he surveyed the different linguistic approaches

to grammar on offer in the late 1970s, and he decided that SFG was by far the best for the new text generation system he was planning to develop (see Sect. 1.1). On the one hand, according to Anthony Davey's (1978) demonstration of a very capable text generation system, the systemic organization was important for text generation because one could organize the grammar around choice rather than based on structure. On the other hand, there was the commitment to comprehensive descriptions in SFG (not just little fragments, as was almost always the case in generative models at the time; cf. Gross 1979—despite the efforts embodied in the so-called somewhat eclectic UCLA grammar—cf. Stockwell et al. 1973 [for discussion, see Sect. 2.4]).

Bill Mann thought that this functional orientation of grammar as a comprehensive resource would be important in the new text generation project he was launching in 1980 (when text generation began to be included in computational linguistics, e.g. McKeown 1982, 1985). He had taken stock of projects developed in computational linguistics by the late 1970s. He said that one problem was the short lifecycle of the development of resources for computational system. There was a tendency for new projects to start from scratch instead of building on earlier foundations. They might be informed by previous results, but there was no commitment to the development of large-scale re-usable resources. So, he wanted to contribute to the development of a large-scale generation grammar of English. His vision had certainly been borne out in the construction of the **Penman system** (e.g., Mann 1982; Mann & Matthiessen 1985; Matthiessen & Bateman 1991) and in John Bateman's development of the **KPML (Komet-Penman Multilingual) system**[3] (e.g., Teich 1995; Bateman 1996, 1997), which was another effort, originally based at a technical institute in Darmstadt, that was taken from the Penman system developed under Bill Mann's direction. John merged these to develop a multilingual workbench for creating generation grammars of different languages.

5.3   Martin Kay's Functional Unification Grammar

Isaac Mwinlaaru: Many thanks for the insights! I understand that the use of SFL in computational contexts is part of the theory's agenda as an appliable theory of language. And the early involvement of Michael Halliday was in the area of machine translation, starting in the 1950s—he was not doing the computational work himself but provided insights on the paradigmatic organization of language to guide the machine translation project. I also realize that the most important role SFL played in the 1980s is in the area of text generation, with Bill Mann as a key player here. You have talked at length about Martin Kay and unification grammars as well as his role in making connections between SFL and later computational grammars such as LFG. What does "unification" in Functional Unification Grammar mean? What are Martin Kay's goals?

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[3] See: http://www.fb10.uni-bremen.de/anglistik/langpro/kpml/kpml-description.htm.

Christian Matthiessen: What Kay (e.g., 1979, 1985) wanted to do was to create a purely **declarative representation** of grammar, a resource that could be used for both **parsing** and **generation**. This goal can be understood against the background of the way parsing grammars had been developed. Computational linguists had made some attempts to apply transformational grammar in the 1960s, and they had on the whole concluded that this was impracticable as a computational grammar (cf. Peters & Ritchie 1973). What they did instead was to develop grammars explicitly designed for computational linguistic applications—typically parsing grammar. Consequently, the orientation to parsing was built into these grammatical frameworks, with no clear separation of grammatical resources and parsing algorithms. Such grammars typically took the form of transition network grammars, either recursive transition networks (RTNs) or augmented transition networks (ATNs; see e.g., Winograd 1983: Chap. 5). Since they were tailored to the task of parsing "strings" of wordings, they were not reversible—the same grammars could not be used for generation. One response to this problem was to develop a separate transition network grammar for generation; in this approach, the grammar would "parse" semantic (conceptual) networks, as set out by Simmons and Slocum (1972) and Stuart Shapiro (1982). There was still the notion of parsing, but what people parsed was not a string of words (wordings), but a network of meanings to produce wordings. Such grammars thus fused the representation of grammatical resources and the orientation to a procedure of either parsing or generation.

As I said, Martin Kay's great conceptual contribution was to separate the declarative representation of grammatical resources from the procedural aspect of either parsing or generation—i.e., to remove considerations of instantiation from the representation of the resources (instantiation in analysis/parsing or instantiation in generation). In knowledge-representation terms, Kay's goal was to have a purely declarative representation of the grammatical resources. Declarative here means to separate it from procedures that operate with these representations, e.g., parsing procedures, generation procedures or translation procedures.

Unification is simply an operation over partial descriptions of the resources of the grammar—as the union of two or more sets would be. We can put it in somewhat systemic functional terms: you might have a description of the clause as a message, the clause as a move, and the clause as a figure, and then you unify them—you map them onto one another. That notion of unification is thus not a parsing or generation procedure but an operation over descriptions. That was Kay's way of moving towards the development of very comprehensive descriptions.

Kay's Functional Unification Grammar influenced the development of new grammatical frameworks in the 1980s. In addition, some of them were also influenced by the development of knowledge representation systems, in particular frame-based inheritance networks (importantly, Head-Driven Phrase Structure Grammar [HPSG]; e.g., Pollard & Sag 1993; Boas & Sag 2010). Here, the insights into knowledge representation also fed into certain computational grammars, and you got developments of computational representation schemes like typed feature structures, mentioned above. This then fed into the issue of how you represent SFG computationally in the late 1980s (e.g., Kasper 1988a) and into the 1990s (e.g., Teich 1999).

John Bateman and I wrote about this (Matthiessen & Bateman 1991; see also Matthiessen 1985, 1988a, 1991; Teich 1999). We tried to conceptualize the **metalanguage** by saying that like language itself metalanguage was also **stratified**. That was a way of making a much clearer sense of work on theorizing, modelling and representing language. In the organization of the metalanguage, you have a stratum or **level of theory**, followed by a **level of theory-oriented representation** (like system networks and realization statements). You then have a level of **computational-oriented representation** (like typed feature structures) and a lowest **level of programme representation**. But in principle, they are independent of, and variable in relation to, one another; hence, you could have typed feature structures representing different kinds of grammars, but the typed feature structures could be implemented in LISP, in Prolog, in C++, in Java, etc.

In the work on **knowledge representation** emerging from the 1970s, researchers were also arriving at a stratified interpretation of the metalanguage. Brachman (e.g., 1978; Brachman & Levesque 1985) had worked on a stratified interpretation of (or rather: what I would characterize as a stratified interpretation of) knowledge representation, partly as a way of addressing Woods' (1975) critique of the free-for-all use of semantic networks since researchers had begun to engage with them in the late 1960s. After the introduction of semantic networks in the 1960s, they were often used almost metaphorically to represent various kinds of relation—the "links" referred to in the title of Woods' paper. This development contains important insights also for linguists who conceive of language in relational network terms. That is one reason why I have kept insisting on the importance of differentiating system networks both from the theory of axial organization in SFL and from more explicit computational forms of representation (such as typed feature structures) (Fig. 5.3).

5.4   Achievements of the Penman Project on Text Generation

Isaac Mwinlaaru: Developing SFG in a computational form was one of Kay's goals and also part of your project with Bill Mann. What achievements did you make? Were you able to successfully develop SFG in a computational form?

Christian Matthiessen: I think the answer is yes, in text generation systems. People tried to develop parsers and also machine translation systems, which of course depended on both parsing and generation, but very broadly speaking, the task of parsing with SFG has still not been solved for the general case of full systemic-functional parsing of large volumes of text of any kind (i.e., belonging to any kind of register), although there have been important contributions. While there are a number of grammars available to the research community for parsing, like the Stanford dependency parser[4], systemic functional grammars impute more information to

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[4] See: https://nlp.stanford.edu/software/nndep.html.

Fig. 5.3 Christian Matthiessen (4th from left) and John Bateman (5th from left) at John's place in Kyoto in September 1986

a parse—adding functional analysis and also systemic analysis, so the lexicogrammatical analysis is richer but also more challenging to automate. Taking one of the examples provided by the developers of the output of a dependency parser, I have added a systemic functional analysis manually (focussing on the structural analysis but leaving out the systemic analysis) in Fig. 5.4.

Thus the challenge has to do with the higher level of information about the patterns of wording specified by SFG (in comparison with some forms of phrase structure grammar and dependency grammar). Systemic functional parsing has been possible for small sets of text that you tailor the parser to, but scaling up has proved to be difficult. Bateman (e.g., 2008a) has pointed to the need for representations of syntagmatic organization that can provide parsers with more "guidance" beyond what is immediately available through realization statements. In the systemic functional literature on parsing, there are a number of approaches that researchers have experimented with; they include the use of pre-parsing by means of a standard parser (e.g., Kasper 1988b), selective functional parsing of a treebank (parsed corpus; Honnibal 2004), and probabilistic parsing (e.g., Weerasinghe 1994). The most extensive work on systemic functional parsing has been undertaken by Mick O'Donnell (1994), who did his Ph.D. with me at Sydney University.

It would be great to have the opportunity to pull together all the experiences from work on systemic functional parsing and insights coming from other approaches. I
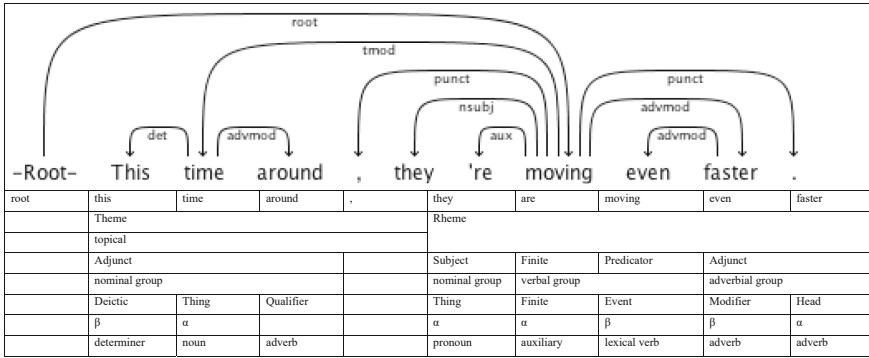
root	this	time	around	,	they	are	moving	even	faster
	Theme				Rheme				
	topical								
	Adjunct				Subject	Finite	Predicator	Adjunct	
	nominal group				nominal group	verbal group		adverbial group	
	Deictic	Thing	Qualifier		Thing	Finite	Event	Modifier	Head
	β	α			α	α	β	β	α
	determiner	noun	adverb		pronoun	auxiliary	lexical verb	adverb	adverb

Fig. 5.4 Comparison of dependency parse (by the Stanford Parser) and systemic functional grammatical analysis of a clause

would love to have the opportunity to work with a team of computational linguists and software engineers. While the "extravagance" of systemic functional lexicogrammatical analyses can be seen as a weakness, I think we can do more—much more—to draw on the insights from SFG, including getting more support from the systemic part and getting more mileage out of the different metafunctional strands in the organization of grammatical units (which will of course have to be adapted to different languages), e.g., harnessing the power of different metafunctional parses of a clause (i.e., textual parsing, interpersonal parsing, and experiential parsing). At the same time, lexicogrammatical parsing must ultimately be treated as part of the more general task of the analysis of text in context. This task will surely involve a great deal of **stratally interleaved analysis**, i.e., analysis moving up and down the strata (context > semantics > lexicogrammar); and it will surely also involve **probabilistic hypotheses** about the likely developments at a given point in the unfolding of a text in its context of situation (cf. Weerasinghe et al. 1993; Weerasinghe 1994).

Rounding off this subsection, let me return to generation. We developed SFG in the computational form of the Nigel grammar. If you look around to examine computational grammars, there is nothing close to the Nigel grammar in terms of both longevity and comprehensiveness. We started working on it in around 1980. Forty years later, it is still around in Bateman's (e.g., 1996, 1997) KPML system. It has also influenced the development of other systems along the way, like SURGE (e.g., Elhadad & Robin 1999). That is pretty extraordinary. This precisely reflects Bill Mann's vision of developing and expanding long-lasting resources instead of starting over and over again, and it also reflects the systemic functional commitment to the development of comprehensive descriptions of different languages.

5.5 Influence of Computational Linguistics on Systemic Functional Linguistics

Isaac Mwinlaaru: We have considered some of the influences of SFL on computational linguistics, notably early explorations in machine translation in the 1950s onwards and later applications in the area of parsing and generation. But what influences did computational linguistics bring back to SFL in terms of application, theoretical development and language description?

Christian Matthiessen: Computational linguistics has clearly had actual influences and there are also potential ones, yet to be taken up. In terms of actual influences, it really helps us to think through properties of the systemic functional architecture of language like realization and instantiation. I give some examples of theoretical and descriptive contributions to SFL deriving from computational linguistic work in Table 5.1. Let me comment on a few of these.

Consider the theoretical understanding of **instantiation**: linguists tend to focus on regions along the cline of instantiation, viz. the potential or instance poles or intermediate patterns but not on instantiation as a process. And if you think about what the vast majority of systemic functional linguists do, they may take existing representation of the potential from *IFG* or some other reference source, and apply it manually to the analysis of text in order to produce phonological, lexicogrammatical, semantic or contextual analyses of texts in their contexts of situation. Their representation of the instance may include box diagrams, tables and so on. (And the same applies to multimodal discourse analysis).

What they do not focus on and are not good at is the process of instantiation (e.g., in generation, analysis or the combination of the two in editing or translation). As I have already noted, one of the great advantages in the representation of SFL is that it is declarative; it does not mix procedures with resources, as was the case in transformational grammars and transition network grammars. But this then needs to be supplemented theoretically with strong accounts of processes of instantiation, and computational SFL has played a significant role. Still, the insights coming from computational modelling need to be incorporated more extensively in discussions of systemic functional theory—obviously in general investigations of logogenesis, i.e., the flow or development of meaning in text (e.g., O'Donnell 1990; Matthiessen 1993, 2002; O'Donnell & Sefton 1995; Zappavigna 2011), but also in specific areas such as translation studies, especially now that there is a dialogue between SFL and translation process research (TPR) (e.g., Alves et al. 2010) (examined in our conversation in the Chapter 10 about systemic functional translation studies, see Sect. 10.3).

But if you think about the discussions on instantiation and separate instantiation from stratification, I do think that we have been helped by the computational work (cf. Table 5.1). The conception of the stratification of the metalanguage is so fundamental. That was really in the greenhouse of the computational work. There would certainly be other activities and insights that one could point to. One was the conceptualization of semantics, and in some sense that was part of the concern of *Construing Experience*

Table 5.1 Examples of contributions to SFL originating in, and informed by, computational SFL

Domain	Domain	Nature of contribution	References
Global dimensions	Stratification	Explicit modelling of inter-stratal realization: lexicogrammatical realization statements in semantic system networks; the choose-&-inquiry framework	Patten (1988), Matthiessen (1988b), Matthiessen and Bateman (1991)
		The modelling of semantics as a meaning base (ideation base, text base and interaction base)	Matthiessen and Halliday (1999), Bateman et al. (2010)
	Instantiation	Processes of instantiation: explicit specifications of such processes	Matthiessen and Bateman (1991)
	Metafunction	The modelling of logical systems	Henrici (1965), Bateman (1989)
		The modelling of exchange	O'Donnell (1990)
		The modelling of textual statuses in relation to the ideation base	Bateman and Matthiessen (1993), Matthiessen (1995b), Halliday and Matthiessen (1999)
	Register variation	Interpretation of registers as solutions compiled for recurring contexts in problem solving	Patten (1988)
Local dimensions	Axiality: delicacy	The modelling of lexis as most delicate grammar, informed by considerations in the description of lexicogrammars for generation systems	Matthiessen (1991), Cross (1992), Wanner (1997), Tucker (1998), Neale (2002)
	Axiality: indeterminacy	The interpretation of systems in terms of fuzzy theory	Research within the Sugeno Lab at Tokyo Institute of Technology; Matthiessen (1995c)

Table 5.1 (continued)

Domain	Domain	Nature of contribution	References
Multilingual meaning potential	Axiality	The expansion of system networks to enable them to represent multilingual meaning potentials, both preserving the integrity of each language included and bringing out systemic commonalities	Bateman et al. (1991), Bateman et al. (1999), Matthiessen (2018)
Multimodality			Matthiessen et al. (1998), Bateman et al. (2002), Bateman (2008b)
Systemic functional metalanguage	Stratification		Halliday and Matthiessen (1999), Teich (1999)

through Meaning (Halliday & Matthiessen 1999/2006), which was developed from computational work.

We can compare computational SFL with other realms of activity within SFL—in relation to education, to administration, to healthcare, to forensic concerns, to translation and interpreting, to multimodal studies and so on; and as we consider them, we can ask about the dialogic interfaces between SFL and other disciplines and to what extent findings have fed back into SFL in each case. If they feed back into SFL as a general appliable linguistics, then the insights have the potential to benefit everybody; but if they remain fairly restricted to a particular realm of activity, then this won't happen. There is always the danger of the fragmentation of knowledge that Bohm (1979) warned us about based on his experience as a physicist (but also reinforced by his dialogues with Jiddu Krishnamurti); this tends to happen, naturally enough, when disciplines develop more specialized sub-disciplines over time. So we have to continue to find ways of counteracting this tendency towards fragmentation. It is not easy, partly of course because systemic functional linguists working at different disciplinary interfaces must present their work in such a way that it is interesting and accessible to members of the other disciplinary—or professional—communities that they work with. Now, researchers in computational SFL can most likely engage with, say, educational linguistic contributions; while such contributions are obviously specialized, there is no threshold of technicality to prevent students and scholars working in other areas from engaging with the educational linguistic work. In contrast, the degree of technicality in computational SFL may be too high to make it readily accessible to educational linguists (unless of course they already have the relevant background expertise). In terms of field of discourse, there is thus an asymmetrical relationship; and this is likely to be reinforced by tenor—by our tendency to form likeminded academic communities ("birds of a feather flock together"). So we

must look for areas where engagement is likely to be mutually beneficial already at a fairly early stage of dialogue—areas where the learning curve is not too steep for either side. One very interesting candidate is clearly subject or disciplinary knowledge—field-classified domains within the ideation base. In educational linguistics, researchers have contributed a fascinating range of studies of "domain knowledge", e.g., in geography, history and physics; and in computational SFL, researchers have modelled domain knowledge quite explicitly, also including multimodal considerations. So there is a considerable potential for productive dialogue; I can envisage very exciting collaboration in this area. I think it could really be of tremendous benefit to our twenty-first-century students. I dream of workbenches for students from primary school, or at least secondary school, onwards informed both by educational SFL and computational SFL. This kind of learning environment could really constitute a quantum leap—and it could include the insights from the research that Mohan (1986) reports on.

We need to think collectively about the general potential of SFL and how it can be extended as a resource for all communities using SFL. A number of people who have a computational background have been concerned with creating tools for doing linguistics. The goal was modelling language computationally, modelling how text has generated, and making explicit models. That was an attempt that could be used for analysis, parsing and translation. But since then, people have tended to devote time and effort to developing computational tools for doing linguistics, rather than to modelling language. Tool development is very valuable, of course, enabling and even empowering the general community of SFL researchers and users. You can think of Wu Canzhong's (2000) work on SysConc, the tools developed by Mick O'Donnell (2012), Webster's (1993) Functional Grammar Processor, and the tools developed by Kay O'Halloran in Singapore, first Systemics (O'Halloran 2003) and then the multimodal analysis software illustrated in O'Halloran et al. (2016). These are all tools for linguists or discourse analysts, but they do not constitute models of language and linguistic processes. To develop such models, you do need the right kind of environment, where you have people who are bi-metalingual or multi-metalingual. One great example is John Bateman; he is at complete ease with linguistics, computational linguistics, and computer science—all the relevant parts. So, if one could create those conditions, one could continue in very interesting ways the grand research programme of modelling language computationally, pushing theory in such a manner that it is explicit enough, and sorting out the aspects of the theory along the way. But there are a lot of aspects of the theory, so systemic functional theory would benefit from the increased attention to and development of more explicit representational model.

5.6 Connections Between Cardiff Grammar and Nigel Grammar

Isaac Mwinlaaru: To recap, we can say that computational SFL has led to more explicit representation in the theory such as the explicit modelling of inter-stratal realization. It has also shed insights on instantiation as a process and motivated the modelling of metalanguage as a stratal system just like language itself. Cardiff grammar is also one application of SFL in computational linguistics. Is there a connection between Cardiff grammar and the Nigel grammar you worked on with Bill Mann in the 1980s?

Christian Matthiessen: I think so. Before Robin Fawcett started his computational project in Cardiff, he came over to us at the Information Sciences Institute (ISI) in Los Angeles a couple of times. (Of course, we also met at conferences.) When he got the funds to start the project in a sustainable way in Cardiff, he sent over a research assistant who stayed with our project at ISI for some time. That was just about the time when I was completing my Ph.D. and preparing to go to Sydney, so I was not very much involved.

I think the Cardiff efforts got a boost from the Penman project, but they were freer to push things entirely systemic functionally under Fawcett's (e.g., 1981, 1988, 1994) direction, also in collaboration with Gordon Tucker. For example, while I tried but failed to convince our team at ISI to treat lexis as the most delicate grammar (e.g., Hasan 1987), they were able to push ahead. At ISI, Bill Mann did not agree with the systemic functional notion of lexis as most delicate grammar, partly because the "knowledge base"—what Michael and I re-interpreted as the ideation base (Halliday & Matthiessen 1999/2006)—was in fact extended in delicacy to include conceptual frames that are lexicalized in English. But in Cardiff, Robin Fawcett did not have that constraint because he was the director of the project, so he could push the modelling of lexis as most delicate grammar, as spelt out and documented in considerably descriptive detail by Tucker (1998) and Neale (2002). Similarly, Robin could also push the notion of probability, which was not incorporated in the Penman project (though Michael had attached systemic probabilities to the most general systems of the clause and they were helpful in testing the grammar in random generation exercises). But Robin's theory is importantly different from Michael Halliday's in certain respects. The kinds of computational implementation found in the two theories are also different.

Isaac Mwinlaaru: To recapitulate, we can say that you have traced the development of computational linguistic work in SFL from the early involvement of Michael Halliday in machine translation projects in the 1950s, where he emphasized the principle of the thesaurus—lexis as a resource and the sense of paradigmatic organization—as a more effective approach to modelling mechanical translations. We have focussed more on activities of the 1980s and into the 1990s, highlighting the important contributions SFL application made to work on text generation. In addition to yourself, you have also noted key scholars in computational SFL during this period, including

Martin Kay, Bill Mann, John Bateman, Mick O'Donnell, Elke Teich, Robin Fawcett, Gordon Tucker among others. Then, you have also noted that there is still potential to renew the implementation of SFG in computational parsing. You also recommend a conscious interaction between insights gained from computational SFL and other applied linguistics areas using SFL such as educational linguistics. Many thanks for these insights! We hope they will generate further explorations in this important research field.

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Chapter 6
Cognition in Systemic Functional Linguistics

Abstract In this chapter, we first place cognitive systems within the four orders of systems in Systemic Functional Linguistics (SFL). Then we discuss how cognitive linguistics can be approached within the SFL perspective, and differentiate between the knowledge-based approach and the meaning-based approach in studies on language and the brain. We also cover topics like Hopper and Traugott's discussions on language, instantiation and individuation, conceptual metaphor and grammatical metaphor, and the corroboration between SFL and cognitive linguistics. Finally, Christian Matthiessen gives some advice to young scholars in this area for future research.

6.1 Introduction

The present interview examines the place of cognition in Systemic Functional Linguistics (SFL) theory. SFL has often been criticized for not having a "cognitive commitment" (see e.g., Butler 2013) — "a commitment to providing a characterization of general principles for language that accords with what is known about the brain and mind from other disciplines" (Lakoff 1990: 40) or "a commitment not to isolate the study of linguistics from the study of the mind, but to take seriously the widest range of other data about the mind" (Lakoff 1990: 46). Following this commitment, several functional approaches to linguistics have emerged under the label "cognitive linguistics", including Cognitive Grammar, and the different versions of construction grammar. We may also add Simon Dik's Functional (Discourse) Grammar and Robert Van Valin's Role and Reference Grammar. The common argument among these approaches is that principles of linguistic structure need to reflect what is known about human cognition from other disciplines, notably philosophy and psychology. Many SFL scholars would however argue against explaining linguistic structure with reference to assumed cognitive processes. Until most recently, what many cognitive linguists use as evidence is experimental studies from psychology and principles of logic from philosophy rather than empirical observations of what is actually going on in the brain. We now know from neuroscience that language is not represented in the

brain as forms and structures but as networks of relations between neurons and that linguistic processes in the brain are not really divorced from sensori-motor processes (Lamb 1999; García, Sullivan & Tsiang 2017). This finding resonates with Edward Sapir's (1921: 8) earlier characterization of language as an "overlaid function" or rather "a group of overlaid functions" of the human biological system. Admittedly, some of these insights have now been incorporated into some cognitive linguistic approaches such as George Lakeoff's notable revision of his Conceptual Metaphor Theory as Neural Metaphor Theory (Lakoff 2014).

There is however one fundamental difference between cognitive linguistic approaches and SFL. Most of cognitive linguistics emerged to provide alternative answers to questions posed by Noam Chomsky and the generative linguistics school about language and the mind. SFL however did not evolve as a reaction against Chomsky but in response to a different set of questions — questions that border on social semiotics and social accountability of linguistic science. We can therefore characterize SFL as primarily having a "sociosemiotic commitment", to use a term proposed by Geeraerts (2016: 527). Thus, most of the issues investigated under the label cognitive linguistics are addressed in SFL but as resources of *meaning* rather than *cognition* and many instances of the term "cognitive" in the cognitive linguistics literature can normally be replaced with "semantic" from the SFL point of view.

Nonetheless SFL theory has a cognitive or rather biological agenda from a different point of view from cognitive linguistics. This agenda is to investigate how the brain or generally sensori-motor systems interact in the production, processing and perception of language using insights from neuroscience. In this sense, Halliday and Matthiessen (1999/2006: 606–610) use the term "bio-semiotic systems" to refer to sensori-motor systems. While little work has been done on this agenda directly under SFL (see Melrose 2005), it is fully compatible with Sydney Lamb's Relational Network Theory (Lamb 1999; García, Sullivan & Tsiang 2017), where language is truly treated as an embodied semiotic system. In this interview, Christian Matthiessen explains some of the issues highlighted here. While he recognizes the need for dialogue with other traditions, he discusses the complexities involved in such collaborations, notably the reluctance of scholars in cognitive linguistics to engage with related work in SFL (e.g., Halliday & Matthiessen 1999/2006). We hope that the discussion here will motivate interest in empirical research in the bio-semiotic agenda of SFL and promote cross-fertilization of ideas.

6.2   The Place of Cognitive Systems in the Four Orders of Systems

Isaac Mwinlaaru: Within the ordered typology of systems operating in different phenomenal realms, the four types of system are ordered in increasing complexity from physical systems to semiotic systems. Biological systems are made up of physical systems [physical systems + "life"], social systems are made up of biological
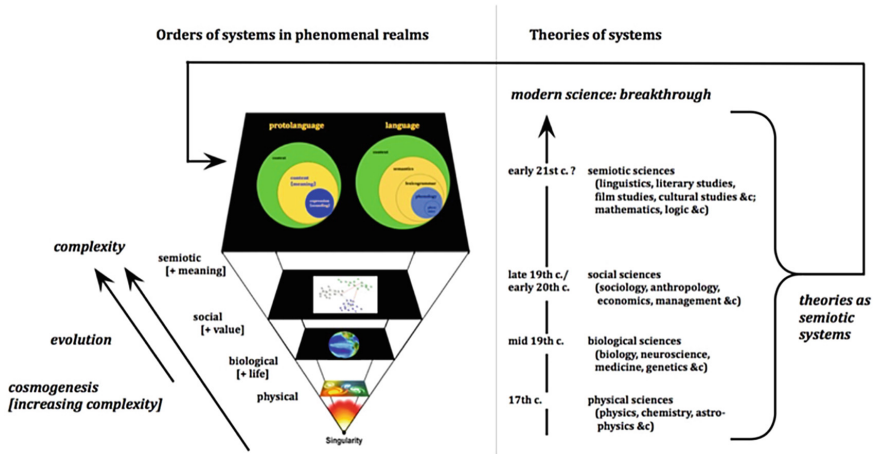
Fig. 6.1   Orders of system in phenomenal realms and theories of systems

systems [biological systems + "value"], and semiotic systems are enacted as of social systems [social systems + "meaning"] (see Fig. 6.1; see also Sect. 8.2). Where would you place the cognitive systems?

Christian Matthiessen: This is a very interesting and important question. If you look at mainstream cognitive science, which began to develop in the 1950s as a kind of macro-discipline (see e.g., Gardner 1985; Miller 2003; Bermúdez 2020),[1] it would be natural to expect that mainstream cognitive science would have been grounded in biological systems, specifically neurological systems. In principle, cognitive systems should be related to neural systems, except that the scientists did not actually engage with neuroscience within **mainstream** cognitive science in the first couple of decades. There were a few of related reasons. One was that the techniques of observation in neuroscience were fairly crude in the sense that there was still the tradition from the nineteenth century of observing dead brains with different injuries, identifying regions with different disorders like Broca's area and Wernicke's area. And it was not actually until the early twentieth century that Santiago Ramón y Cajal was able to provide more detailed accounts of neurons, producing very accurate drawings based on a new observational technique and thereby contributing to the foundations of modern neuroanatomy. In the late 1950s, Wilder Penfield (1958) very crudely put electrodes in the brain during brain surgery while patients were still conscious, which, while it was a very invasive technique, made certain observations possible. But it was not the ordinary happy living brain going through daily life. That was certainly a constraint.

Another reason was the development of computer science and the way it took over commonsense metaphors dealing with the mind from ordinary language, and then

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[1] The conceptual-temporal map of cognitive science created by Anna Riedl gives a good sense of the macro nature of the enterprise: https://www.riedlanna.com/cognitivesciencemap.html.

elaborated on them in the construction of cognitive models (e.g., Matthiessen 1993, 1998; Halliday & Matthiessen 1999/2006: Chap. 14). Thus computer science worked with the notion of memories as containers, suggesting that things were stored in and retrieved from memories. Of course, these computer-based models were powerful ones up to a point; they were more explicit and more developed than the folk model. But they were still grounded in the commonsense understanding of consciousness — of processes of thinking and other processes of sensing.

Mainstream cognitive science was developed in some degree of isolation from other relevant developments. (1) Neuroscience began to adopt ways of observation that were much more sophisticated (see already Sejnowski & Churchland 1989). Different scanning techniques were developed so that in principle you could observe the brain engaged in daily activities in real time with both the temporal and the spatial resolution necessary.

(2) At the same time, there were scholars who objected to the notion of cognition developed within mainstream cognitive science and emphasized the re-interpretation of the mind as the embodied mind (e.g., Varela, Thompson & Rosch 1991). If you talk about cognition and the mind, you have to realize that it is not just a free-floating kind of computational model, but is actually something embodied. This implies that we have to relate cognition to the biological order of system.

(3) In addition, scholars were beginning to talk about interaction between people, and that led to thoughts about social systems, interactive behaviour and so on — the social mind (cf. Wertsch 1985, who made the work by Lev Vygotsky from the 1930s accessible).

In terms of the ordered typology of systems, a more sophisticated understanding of cognition would indicate that cognition is not only directly related to the biological system (i.e., the brain), but also goes through some kind of mediation within social systems. This suggests that it could be explored as a 4th-order system. When you move to the 4th-order systems, you actually have two alternative interpretative views: the **semiotic** view (if you come from the semiotic tradition) and the **cognitive** one (if you take the sophisticated re-understanding of cognitive systems as embodied in organisms taking on roles in social interaction). So we can represent these views as two alternative conceptualizations of 4th-order systems (see Fig. 6.2).

How do we come to grips with this? In one way, they are just alternative perspectives on the same phenomena. This view is stated at the beginning of our book *Construing Experience through Meaning* (Halliday & Matthiessen 1999/2006) (cf. also Matthiessen 2021a, b). You can think of these 4th-order phenomena in cognitive terms as knowledge (as cognitive processes) or in semiotic terms as meaning (as semiotic processes). Then, one interesting question is: What are the consequences of adopting one view or the other? There is a tendency in cognitive science and cognitive linguistics to explain language in terms of cognition whereas what Michael Halliday and I were trying to say (Halliday & Matthiessen 1999/2006) was: "No, no, no, it is the other way around". The kinds of phenomenon that are evoked when cognitive scientists talk about cognition — how would you explain them? Our answer was that language plays a central role in explaining them. Here Painter's (1999) language-based study of the ontogenesis of "knowledge" is both uniquely important

interpreted as **cognitive**
(knowledge)
and/ or as **semiotic**
(meaning)

higher order organization
[4th-order]

knowledge and/ or meaning?
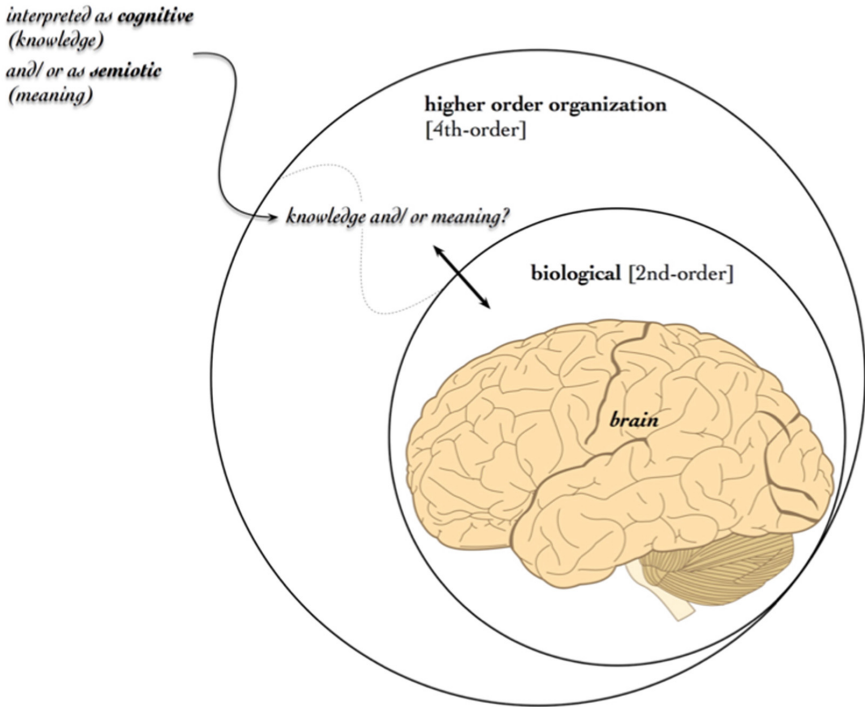
biological [2nd-order]

brain

Fig. 6.2 Interpretation of 4th-order systems: as systems of knowledge (cognitive systems) or as systems of meaning (semiotic systems)

in providing empirical evidence drawn from a couple of case studies and stimulating as a model of how to move forward with linguistic studies that shed light on the emergence of "general cognitive principles" and other notions from cognitive science.

There was another trail of exploration: scholars wondered about the source of the complexity of the human brain. There had been various proposals over the years, like tool-making. This proposal fell by the wayside when researchers discovered that lots of our fellow creatures (not only immediate cousins like chimpanzees and bonobos, but also species of birds) actually use tools or even make tools.

Another proposal people tried was general intelligence, but that in itself needs explaining — what could be the source of general intelligence? Among neuroscientists, Terrence Deacon (e.g., 1992, 1997) argues that the only phenomenon of the kind of complexity that could possibly explain the complexity of the brain is language. He holds that language and the brain co-evolved towards increasing complexity.

Here Michael Halliday's (1975a) account of ontogenesis can serve as a model of the gradual development of language as a powerful and complex semiotic system: he identifies three major phases in **ontogenesis** — the process of young children learning how to mean in interaction with their immediate caregivers. The phases are

set out in Table 6.1 together with the correlates I hypothesize in phylogenesis (see Matthiessen 2004). If we propose and explore a phased model of the evolution of language (and other complementary semiotic systems), we do not have to assume that language and the brain emerged full-fledged, with language in its current complexity from the start with a content plane stratified into semantics and lexicogrammar.

As indicated in Table 6.1, I suggest what the phases might have been in human evolution. Phase I, protolanguage, must have had a long history, stretching far back into our primate ancestry. After all, it seems that a range of species have evolved semiotic systems comparable to our Phase I, protolinguistic system, and like human protolanguage during ontogenesis, our ancestors' protolanguages and those of our fellow creatures are likely to have been "multimodal" throughout, e.g., involving both vocalizations and gestures as expressive resources.

The emergence of Phase II out of protolanguage seems to be likely to have occurred with the first burst in brain growth going from *homo habilis* to *homo erectus* around 2.2 to 1.8 million years ago.

Phase II is characterized by a generalization of the microfunctional organization of protolanguage into two **macrofunctions**, the mathetic and the pragmatic, and the gradual emergence of a split of the content plane into semantics and lexicogrammar and of the expression plane into phonology and phonetics (with vocalization taking over as the linguistic expression plane and gesture being transformed into the expression plane of a distinct but closely related and highly coordinated semiotic system, the semiotic system of gesture). During ontogenesis, Phase II is relatively short because young meaners have a model of Phase III to draw on — the mother tongue or tongues spoken around them. But obviously our ancestors did not have a model around; it seems plausible that the transitional Phase II would have lasted over a million and a half years. During this period there are certainly pieces of evidence suggesting continued evolution, like the evolution of our vocal organs (biological) and the "invention" of fire (social).

Then, Phase III language, modern language, emerged with Anatomically Modern Humans (AMHs), i.e., *Homo sapiens sapiens*, 150 to 250,000 years ago. This marks the emergence of "modern" humans — both semiotically modern and biologically modern. The account just outlined is consistent with, and supported by, Deacon's (e.g., 1992, 1997) hypothesis that language and the human brain co-evolved. I would thus interpret "modern humans" as a package deal; they were both AMHs and LMHs (linguistically modern humans).

Then there were of course other lines in early human evolution although only our line has survived, e.g., the Neanderthals, who eventually died out 30 to 40,000 years ago, probably because they lost ground when competing with our nasty ancestors, although there seem to have been many pre-historic versions of Romeo and Juliette. There are many details to take account of, and build into, a reasonably comprehensive account. For example, there has been a good deal of discussion in the literature of what we might interpret as evidence for a burst of socio-semiotic creativity during the Upper Paleolithic period. I touch on a number of these details in Matthiessen (2004), but it is an abridged version of the full manuscript and many new findings have emerged since then. However, while researchers have found evidence of early

Table 6.1 Linguistic phases in ontogenesis and hypothesized in phylogenesis

Phase	Properties	Properties	Ontogenesis	Phylogenesis	
I – protolanguage	Microfunctional	Bistratal [content > expression]	Around 5–8 months of age to middle of second year; starting to crawl	Millions of years ago (far back in primate history)	
II — transition	Macrofunctional	Emergence of stratified content and expression	Around middle of second year of life	Around 2 million years ago	*Homo habilis* (c. 2.2 million years BP), *Homo erectus* (c. 1.8 million years BP) — first significant brain expansion
III — modern language	Metafunctional	Quadristratal [content: semantics > lexicogrammar; expression: phonology > phonetics]	Starting to walk upright	Around 200 K years ago	*Homo sapiens sapiens* (cf. 200 K years ago): AMH — anatomically modern humans & LMH — linguistically modern humans

humans in new places — a process of discovery likely to continue, I don't see any findings yet that would be a reason to change the overall picture that I have suggested.

Even within semiotic systems in general, it is very interesting to continue to think about the emerging complexity and the orders of systems from primary semiotic systems to higher-order semiotic systems. Michael Halliday (e.g., 1995) used these terms, drawing on Gerald Edelman's (e.g., 1992) discussion on brain and consciousness and his distinction between primary consciousness and higher-order consciousness. In other words, the central idea is to link language and the brain without a cognitive intermediary level; it means that language is the human system through which you can understand the emergent complexity of the human brain, increasing with the increasing complexity of language. You could thus talk about the **language brain** or the grammar brain in this respect. In principle, this account is empirically grounded or groundable, which is one of its significant features. So, one of the interesting observations to me is: human language is the one and only human system that is pervasive in the brain, and thus human language (including both spoken and written language) integrates the different parts of the brain (cf. the reference below to Bickerton's 1995 feline example).

6.3 Studying Cognitive Linguistics from the Perspective of Systemic Functional Linguistics

Isaac Mwinlaaru: Scholars in other areas, for instance, Christopher Butler (e.g., 2013), think that Michael Halliday has conflicting and contradictory statements about language and cognition. In Halliday and Matthiessen (1999/2006), you have written that the human brain is the immediate environment of language. Also, Michael Halliday has foregrounded the importance of complementing the "intra-organism" perspective on individuals with an "inter-organism" one, since we are not bounded by the skin as individuals. Building on Firth's (1950) outline of persons and personae, he shows the development of individuals as persons in interaction with social groups (see in particular, Halliday 1978: Chap. 1). This fundamental point is also elaborated by Butt (1991), Lemke (1995) and Halliday & Matthiessen (1999), and challenges "the assumption that a human being is bounded by his skin" (Halliday 1975b). Does it mean that Systemic Functional Linguistics (SFL) is not interested in cognitive linguistics?

Christian Matthiessen: Let me begin by commenting on a central question identified by Butler (2013: 207):

> In my own recent work (see e.g., Butler 2008, 2009), I have advocated the last of these positions, on the grounds that the ultimate goal of functional linguistics should be an account of how people communicate using language, so that the question we should be asking ourselves is one which Dik (1997: 1) proposed was at the centre of functional approaches: 'How does the natural language user work?'. [fn: Hudson (2008: 91) even goes so far as to claim that "by the end of the [twentieth] century the focus had shifted from the language system to the individual speaker's cognitive system".] Dik himself shied away from attempting a full

investigation of this question, but I believe we should take it literally and work towards an answer in collaboration with colleagues from the full range of disciplines concerned with the scientific study of language.

The question "How does the natural language user work?" is interesting. However, it is certainly not at the centre of functional approaches in general — demonstrably not at the centre of SFL, but one can't claim that it is at the centre of the Prague School — which has had a very rich and varied agenda, and it is not at the centre of Okuda's functional school in Japan. There are many fascinating and urgent questions about language addressed in functional linguistics approaches that are only very indirectly related to this so-called "central question". And to address this "central question", we need to adopt a holistic view of language in the ordered typology of systems operating in different phenomenal realms (Fig. 6.1), and also answer other key questions, e.g., "How do groups of speakers work in speech fellowships?" (cf. the reference to Malinowski's 1935 insights in Sect. 6.6). One of the hallmarks of Halliday's approach to language woven into SFL is the "commitment" to answering a wide variety of questions about language, which means (among other things) that SFL has not been driven by questions that have been claimed to be central at one time or another by one particular linguist or group of linguists. Halliday never accepted Chomsky's central questions about language, and there is absolutely no reason why we should accept "How does the natural language user work?" as being "at the centre of functional approaches". If that question had been at the centre of SFL, there is a vast amount of critically important work that may not have been carried out if researchers had been preoccupied with this "central question". If one linguist or group of linguists have a central question, that's great; but they should not impose it on other linguists. There has been far too much of this kind of attempt to control the agenda since the mid-twentieth century.

Now, let me turn to the question you lead up to: "Does it mean that Systemic Functional Linguistics (SFL) is not interested in cognitive linguistics?" It would be a bit pointed to say that SFL is not interested in cognitive linguistics — and even wrong if we survey the whole varied range of contributions to SFL. There is of course an important difference between interest in the ***phenomenon*** of cognition and the ***framing and study*** of that phenomenon in cognitive linguistics. Many systemic functional linguists have taken an interest in phenomena that have been conceptualized in terms of cognition — including centrally developmental studies, explicitly foregrounded by Painter (1999). Here the approach to phenomena interpreted in terms of cognition is language-based — as also in Hasan (1992), Halliday (1993a, 1995) and Halliday & Matthiessen (1999/2006).

In addition, there is also the strand within SFL pioneered by Robin Fawcett, and clearly reflected in his first major book (Fawcett 1980): *Cognitive Linguistics and Social Interaction.* This strand can be said to represent an interest in cognitive linguistics as a way of framing and studying phenomena that have been conceptualized in cognitive terms. There are certainly opportunities for dialogue — a theme brought out by the extensive metalinguistic study of the functional-cognitive space by Chris Butler and Francisco Gonzálvez-García (Gonzálvez-García & Butler 2006; Butler &

Gonzálvez-García, 2014) and also, already mentioned above, by Butler (2013). It is possible to discern commonalities between functional theories and cognitive ones that are "usage-based" (e.g., Bybee 2010; Goldberg 2006, 2019). At the same time, it is also important to note the complementarity since the 1960s between Halliday in his development of SFL and Lamb in his development of Stratificational Linguistics (with some name changes over the years, more recently "Relational Network Theory"). Lamb's orientation has been towards cognitive and neurological considerations — see e.g., Lamb (1999) and García, Sullivan & Tsiang (2017). Importantly, García and his fellow researchers have also turned to SFL, showing how it can be an active partner in neurolinguistic studies, e.g., García and Ibáñez (2017) and Trevisan and García (2019). Their work goes much further than a general cognitive model that is not grounded in empirical evidence or some notion of "cognitive commitment" (see also Sect. 6.3 below, on different kinds of "commitment").

To understand Halliday's stance, we need to go back to the 1960s, when he faced the growing dominance of cognitive science as a macro-discipline, including the development of cognitive psychology, formal linguistics and psycholinguistics. Psycholinguists during this period tried to investigate those kinds of phenomenon that were often conceptualized in formal generative linguistics through typical laboratory-style experiments. This intellectual environment was not conducive to Halliday — neither methodologically, since he favoured authentic data for various excellent reasons, nor metatheoretically, since he pursued what we can characterize as "systems thinking". He had a much more holistic view of language in relation to other (human) systems, including centrally social ones. You will find this in his *Language as Social Semiotic* (*LASS*) (Halliday 1978), which is a direction-setting collection of papers from the 1970s. This book sets out a thematic area of studies, **social semiotics** — one that is really a complement to cognitive science, and it led to very productive developments, starting in the 1980s (see e.g., Andersen et al. 2015; Matthiessen 2017). Halliday was keenly aware of the need to engage with language also as a social system — more specifically, as a semiotic system that is also a social system, e.g., involving social interactive behaviour, social role systems, division of labour, social hierarchies and value systems. Theoretically, this was a fundamental aspect of language (as it had been for J.R. Firth, e.g., 1950) — not an optional extra to be studied only in a hyphenated branch of linguistics; and, crucially, this conception was needed to support a range of applications, including educational ones. At the time, the realm of social phenomena was largely or even totally absent from, or effaced in, mainstream cognitive science. This is what Jackendoff (1992) has called the "mentalist stance". Let me quote him at some length since the choice of "stance" is of fundamental importance (1992: 2):

> The basic stance of generative linguistics is that we are studying "the nature of language," not as some sort of abstract phenomenon or social artifact, but as the way a human being understands and uses language. In other words, *we are interested ultimately in the manner in which language ability is embodied in the human brain*. Chomsky makes this distinction nowadays by saying we are studying "internalized" language (I-language) rather than "externalized" language (E-language). Generative grammar is not the only theory of language adopting this stance. The tradition of Cognitive Grammar adopts it as well, Lakoff (1990), for instance,

calling it the "cognitive commitment". On the other hand, a great deal of work in formal semantics does not stem from this assumption. For instance, Bach (1989) asserts Chomsky's major insight to be that language is a formal system — disregarding what I take to be the still more basic insight that language is a psychological phenomenon; and Lewis (1972), following Frege, explicitly disavows psychological concerns.

What about the abstract and social aspects of language? One can maintain a mentalist stance without simply dismissing them, as Chomsky sometimes seems to. It might be, for instance, that there are purely abstract properties that any system must have in order to serve the expressive purposes that language serves; and there might be properties that language has because of the social context in which it is embedded. The mentalist stance would say, though, that we eventually need to investigate how such properties are spelled out in the brains of language users, so that *people* can use language. It then becomes a matter of where you want to place your bets methodologically: life is short, you have to decide what to spend your time studying. The bet made by generative linguistics is that there are some important properties of human language that can be effectively studied without taking account of social factors.

Similar remarks pertain to those aspects of language that go beyond the scale of the single sentence to discourse and narrative. Generative grammar for the most part has ignored such aspects of language, venturing into them only to the extent that they are useful tools for examining intrasentential phenomena such as anaphora, topic, and focus. Again, I am sure that the construction of discourse and narrative involves a cognitive competence that must interact to some degree with the competence for constructing and comprehending individual sentences. My assumption, perhaps unwarranted, is that the two competences can be treated as relatively independent.

It is not hard to understand why the "mentalist stance", as articulated by Jackendoff, was impossible for Halliday to work with — in the 1960s, or in subsequent decades. The "properties that language has because of the social context in which it is embedded" are left out of the picture completely, and the engagement with "those aspects of language that go beyond the scale of the single sentence to discourse and narrative" is, at best, postponed for later consideration. To Halliday, and to systemic functional linguists in general, discourse in context is part of the core of a holistic theory of language. It made absolutely no sense at the time to ignore the social enactment of language. Its social manifestation was, of course, of considerable interest in its own right — in line with the intellectual tradition of Malinowski and Firth. However, if one is interested in the core properties of language, the "nature of language", then bypassing its nature as a social system (as well as a semiotic and a biological system) makes absolutely no sense. Central properties of language are due to its social nature (as Saussure and those following him recognized). This obviously involves the nature of the interpersonal metafunction and its relation to the tenor parameter within context; but it suffuses language in context as a whole.

Of course, there have been significant developments in cognitive science since the 1960s, and even since Jackendoff (1992) characterized the "mentalist stance" three decades ago. Scholars have introduced alternatives to the mainstream, e.g., emphasizing the "social mind" and the "embodied" mind; and the mainstream is arguably quite different from that of the first couple of decades of cognitive science. One source of inspiration and support was Lev Vygotsky (1896–1934), a Soviet psychologist specializing in child development — of the same generation as Benjamin Lee

Whorf (1897–1941), and also a creative scholar who died quite young. While his work had appeared in English already in the early 1960s (Vygotsky 1962), it was not until the 1980s that it began to influence thinking within cognitive science in the West more generally, in large part thanks to Wertsch (1985). (By then, the work on the brain by his student Luria [e.g., 1976] had become well-known in the West, including his theory of the non-localist organization of the brain.)

Vygotsky's view of the relationship between language and cognition was more resonant with Halliday's approach, including the central role he gave to language, related to the view that we are not bounded by our skins. This parallel is brought out in an important volume conceptualized and edited by Heidi Byrnes, *Advanced Language Learning: The Complementary Contribution of Halliday and Vygotsky* (Byrnes 2006) — a point also made by Wells (1994) in relation to Halliday's (1993a, b) steps towards a language-based theory of learning (see further e.g., Hasan 1992; Han & Kellog 2019).

In order to understand the relationship between the development of cognitive science and the Hallidayan strand of SFL, we can contrast the 1960s and the 1990s: see Table 6.2. While there was not much for Halliday and other systemic functional linguists to engage with during the 1960s, the situation had changed significantly by the 1990s. On the one hand, the technology for observing the brain "in action" in non-obtrusive ways had improved dramatically (already by the late 1980s, e.g., Sejnowski & Churchland 1987), therefore new data became available as empirical evidence for neuroscience — and neurolinguistics. Consequently, cognitive models of the mind could be grounded in studies of the brain, and could move away from

Table 6.2 Comparison of the 1960s and the 1990s in terms of emergent activity in cognitive approaches relevant to language (highly selective) and related SFL publications

Decade	Psycholinguistics — language and mind	Neurolinguistics — language and brain	SFL
1960s	Growth of psycholinguistics: experimental studies investigating "performance" (stimulated by generative linguistic accounts of competence)		—
1990s		Improved technology for observing the brain processing language (e.g., Sejnowski & Churchland 1987); central role of language in brain development & evolution: Edelman (1992); Deacon (1992, 1997); Dunbar (1996)	Halliday (1995); Painter (1999); Halliday & Matthiessen (1999/2006)

disembodied accounts based on computational models (cf. Edelman's 1992 incisive critique).

On the other hand, cognitive science had become more theoretically aware of the fundamental importance of the embodiment of the "mind" (e.g., Varela et al. 1991) and of its social construction in interaction with people in social groups (cf. Halliday 1978). I remember Michael and me discussing these developments on various occasions, both leading up to Halliday & Matthiessen (1999/2006) and after its publication. We both liked the formulation by Susan Greenfield, the neuroscientist, who said that if one needed to speak of the mind, the best approach was to think of it as a **personalized brain**.[2]

The developments during the 1990s and the publications they led to — key ones being listed in Table 6.2 — created an aspect of cognitive science that was much more in tune with Hallidayan SFL than the work during the 1960s. We worked on *Construing Experience through Meaning: A Language-based Approach to Cognition* through a good part of the 1990s (going back to joint research we had begun in 1986; see Sect. 4.2). We discussed the developments in cognitive science just noted above and referred to them in our book — also contextualizing them in terms of SFL. This included a section on cognitive linguistics of the Berkeley variety, developed within linguistics by George Lakoff. (In parallel, Kristin Davidse engaged with Ron Langacker's Cognitive Grammar. This has turned into an interesting long-term project, reflected in a number of publications over the years, e.g., Vandelanotte 2009.)

Before leaving your question, let me return to the central role played by experimental studies in psycholinguistics. Here we can consider a Hallidayan principle. It is not just Hallidayan; it is also manifested within the study of phenomenal realms other than that of semiotic systems, as in Heisenberg's (e.g., 1930) observer's paradox — articulated in reference to the observation of physical systems. Once you begin to observe a system, you disturb it. If that is true of physical systems at the quantum level, it is even truer of human systems. So, the techniques, paradigms, hypotheses and experimental methodology that were developed in material sciences cannot be taken over when we move to higher-order systems, as occurred in psycholinguistics. Observation of naturally occurring phenomena becomes much more important, and that was of course established as the central tradition in anthropology, i.e., the ethnographic approach Malinowski pioneered in his fieldwork. Experimentation has very limited value when you investigate human systems. What you will learn (if you do experiment) is what people do under experimental conditions. It may be necessary to set up experimental conditions, especially when we want to investigate the embodiment of language as a biological system. But that is a constraint — although observational techniques have been improving (e.g., Kuhl 2010; Trevisan & García 2019), and we are interested in what they do under natural conditions. As Halliday has pointed out, it is even worse when you experiment with children: you are not likely to get anywhere near the natural development frontier under experimental

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[2] See e.g., https://www.youtube.com/watch?v=_QJilnXBcPc.

conditions. So, the kind of experimental approach in the study of language development under the metaphor of "language acquisition" was not likely to yield very interesting results. That was also part of the picture.

6.4   Knowledge-Based and Meaning-Based Approaches Towards Language and the Brain

Isaac Mwinlaaru: In *Construing Experience through Meaning*, there is a statement of your approach towards language and the brain: "our approach contrasts with representations of knowledge in that in our own work the experiential environment of the grammar is being interpreted not as knowledge but as meaning" (Halliday & Matthiessen 1999: 2). You have referred to this as meaning-based instead of knowledge-based. What are the knowledge-based and meaning-based approaches and why do you make this distinction?

Christian Matthiessen: The notion of a **knowledge base** came out of artificial intelligence and computational linguistics — as strands within cognitive science — in the 1960s and it was certainly advanced in the 1970s when people began to find ways of representing knowledge in earnest (for a selection of fairly early influential contributions, see e.g., Brachman & Levesque 1985). The notion of a knowledge base in such computational models can be seen as a conception based on our folk theory of knowledge, and I have tried to demonstrate the same for the conception of the mind in a few different places by analysing the discourse of mainstream cognitive science (Matthiessen 1993, 1998; cf. also Halliday & Matthiessen 1999/2006, in particular Chap. 14). This concept of knowledge comes from generations upon generations of people building up folk models — commonsense models of our experience of our own processes of consciousness (cf. contributions to cognitive anthropology, e.g., D'Andrade 1987; Holland & Quinn 1987). What I suggested was that mainstream cognitive science just took over the folk model rather unquestioningly, and added the computational model of memory, processing, reasoning and inferencing. This model depends centrally on ideational grammatical metaphor, one consequence of which is that the "knowers" are typically effaced. Now, we must obviously make sense of the folk model as part of the world view construed unconsciously over generations within a given speech community and culture. It reflects and construes lived experience and is negotiated by speakers in innumerable exchanges; but it is not a scientific model — anymore than folk models of our experience of the material world are.

If you want to look at our construal of experience scientifically, you of course take seriously the understandings coming through from the collective wisdom embodied in folk theories, in commonsense theories. But then you take a step back and say: "Well, now let's try to understand it scientifically". What we are saying is: "If you try to understand it scientifically, that means understanding it in terms of the resource that enables us to construe our experience of the world around us and inside us, i.e., language". Cognitive scientists try to approach language in terms of cognition;

they try to explain language by reference to cognition. However, we would put it the other way round: the only way that they can explain cognition is by reference to language and, of course also to other semiotic systems (including bio-semiotic ones), but language is the most powerful, so most complex, human semiotic system.

Again, the fundamental question we face is where the construal of experience comes from in the life of an individual. Here, one could make more connections with Vygotsky (e.g., Vygotsky 1962; Wertsch 1985; Byrnes 2006). The folk notion is in some sense "inside-out" — reflected in a whole cluster of lexicogrammat-ical metaphors like putting thoughts or feelings into words (e.g., *It is never easy to put thoughts into words.*), which means that cognitive structures are externalized — i.e., we can analyse them by observing their "products" such as language. The Vygotskyan notion is "outside-in", which means that children develop their cogni-tive structures with the resources of language and other semiotic systems mediated through social systems (cf. Hasan 1992). That is how they build up a more internal representation always in interaction with others. Thus, children build up an internal representation of experience always in interaction with others — from the "outside".

This is exactly what the ordered typology of systems — semiotic > social > biological > physical — enables us to apprehend and theorize because the internal organization is not just biological, but also social, which means constructed through interaction in groups. Thus, the semiotic system involves not just an organism with a brain but a person in different role relationships with other persons in a range of social groups. This follows from the ordered typology of systems we have proposed (see Sects. 6.1 and 8.2). That is, semiotic systems have properties unique to them, but they also "inherit" the properties of social systems (since they are enacted in social systems). And they inherit the properties of biological systems too (since they are embodied in biological systems) and of physical systems (since they are ultimately manifested physically). This feature of the ordered typology of systems is foregrounded in Fig. 6.3.

In the literature, we find many titles that include "the social self" and "embod-iment". They generally reflect significant advances over "mainstream" cognitive science as it began to take shape in the 1960s after the pioneering contributions in the 1950s by Herb Simon, George Miller, Noam Chomsky and other scholars who were instrumental in getting it started. However, these advances could actually have been part of the picture from the start if something along the lines of an ordered typology of systems had been considered — cf. the contributions by Lev Vygotsky, George Herbert Mead, Gregory Bateson that were, in principle, available during the early stages of cognitive science (and by another route, Bronisław Malinowski and J.R. Firth, with the emphasis on context and on persons as aggregates of personae).

Working with the account diagrammed in Fig. 6.3, we can have certain "com-mitments" that can be made in developing a theory of language (and descriptions of particular languages). I'm using Lakoff's (1990) term here simply because it may be helpful to compare what I'm about to say with what he says (p. 40):

> For me, cognitive linguistics is defined by two primary commitments, what I will call the Generalization Commitment and the Cognitive Commitment. The generalization commit-ment is a commitment to characterizing the general principles governing all aspects of human
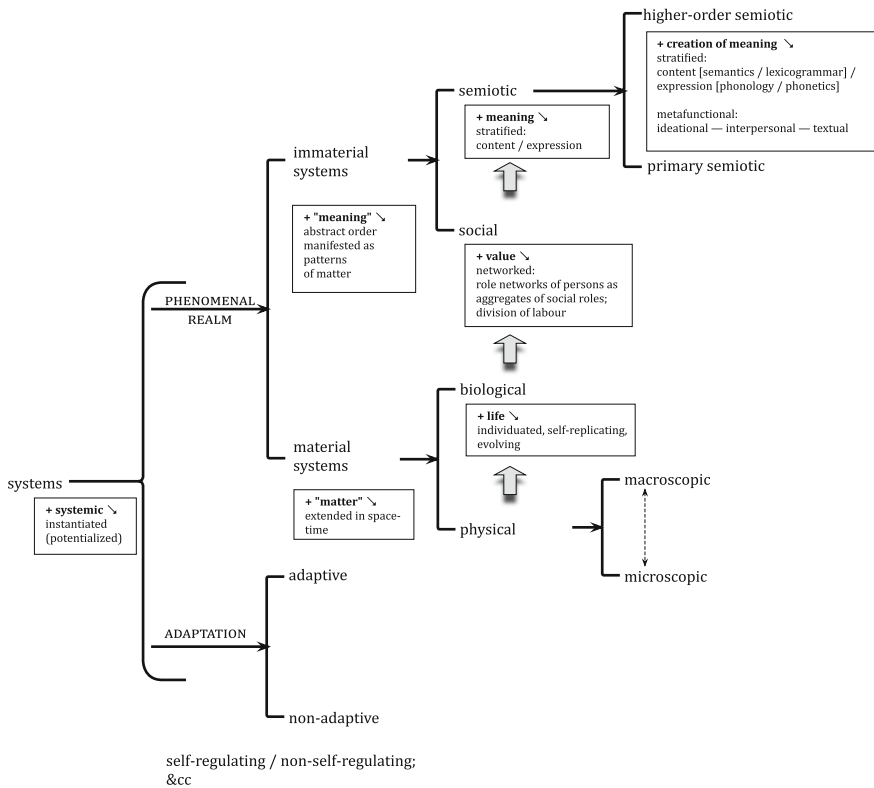
Fig. 6.3 Ordered typology of systems (material: physical, biological; immaterial: social, semiotic), showing that higher-order systems inherit the properties of lower-order ones

language. I see this as the commitment to undertake linguistics as a scientific endeavour. The cognitive commitment is a commitment to make one's account of human language accord with what is generally known about the mind and the brain, from other disciplines as well as our own.

The generalization commitment comes with a phenomenological characterization of subfields in terms of the kinds of generalizations required:

In syntax: Generalizations about the distribution of grammatical morphemes, categories, and constructions.

In semantics: Generalizations about inferences, polysemy, semantic fields, various kinds of semantic relationships, conceptual structure, knowledge structure, and the fitting of language to what we perceive, experience, and understand.

In pragmatics: Generalizations about speech acts, discourse, implicatures, deixis, and the use of language in context.

And so on, for morphology, phonology, etc. Of course, no a priori commitment is made as to whether these are separate subfields. It is an empirical matter, and empirical considerations suggest that they are not — that, for example, generalizations about syntax depend on semantic and pragmatic considerations.

The cognitive commitment forces one to be responsive to a wide variety of empirical results from a number of disciplines. Examples include:

Categorization results from cognitive psychology, developmental psychology, and anthropology that demonstrate the existence of basic-level categorization and prototype effects.

Psychophysical, neurophysiological, anthropological results about the nature of color perception and categorization.

Results from cognitive psychology concerning human imaging capacities and the association of conventional imagery with language.

Results from cognitive neuroscience and connectionism regarding the computational mechanisms of the brain.

The ordered typology of systems diagrammed in Fig. 6.3 above enables us to derive "commitments" we can make in theorizing language and describing particular languages; let me put this in terms of the general theory of language:

- **the systems "commitment"**: we theorize language in such a way that we take into account the properties of complex adaptive systems in general. These properties are **fractal** in the sense that they are manifested within the different phenomenal realms in which systems operate, and they are **emergent** in the sense that they emerge with increasing complexity.
- **the semiotic "commitment"**: we theorize language according to its own systemic order as a semiotic system with the general properties of semiotic systems and properties that are unique to language such as the metafunctional diversification and the internal stratification of the content plane into semantics and lexicogrammar and the expression plane into phonology and phonetics. We strive to develop a holistic theory of language, supporting the development of comprehensive descriptions of particular languages.
- **the social "commitment"**: we theorize language according to its enactment in society as a social system, ensuring that the properties of language we postulate are consistent with the nature of social systems, e.g., the meaner (speaker) as a person playing different roles within a wide variety of social groups and language as a socially distributed collective meaning potential, transmitted or re-created across generations of persons, and that the semiotic account can interface with the social one.
- **the biological "commitment"**: we theorize language according to its embodiment in the human organism as a biological system, ensuring that the properties of language we postulate are consistent with the nature of biological systems — of course, in particular those aspects of the human body directly involved in language, e.g., the semantic system as an interface to sensorimotor systems (bio-semiotics systems) and more generally language as a relational system integrating different regions of the brain.
- **the physical "commitment"**: we theorize language according to its manifestation as a physical system, taking both physical affordances and constraints into consideration. This includes, then, what scholars pursuing social semiotics have discussed in terms of "materiality", but also the kinds of issue that come to the fore in e.g., speech processing.

The general point is that these "commitments" are metatheoretical principles directly grounded in our theory of the ordered typology of systems operating in different phenomenal realms. Thus they are in fact not separate or separable "commitments"; they are ***inherent in the theory***. Lakoff's "cognitive commitment" seems to be dispersed into what I called, using his term "commitment", the semiotic commitment and the biological commitment. As an alternative to what I just sketched above, we could follow Geeraerts (2016), whom you referred to above, and add what he calls "the sociosemiotic commitment" to "the cognitive commitment". He characterizes it as follows (p. 537):

> To complement the Cognitive Commitment, we define a **commitment to make one's account of human language accord with the status of language as a social semiotic, i.e., as an intersubjective, historically and socially variable tool, and to base that account on a methodology that likewise transcends the individual**. [Bolding in original]

This relates directly to the third-order systems in our ordered typology, i.e., to social systems. Thus one might argue that the combination of the "cognitive commitment" and the "sociosemiotic commitment" covers the ground not covered by Lakoff's (1990) "general commitment". However, I prefer to relate the consideration of "commitments" to the theory itself, in fact deriving them from the theory of the ordered typology of systems operating in different phenomenal realms. This means that each higher order of "commitment" must be responsive to, and responsible for, lower-order "commitments"; in other words, the "commitments" are ordered according to the orders of the ordered typology.[3] Further, I think that the general systems "commitment" is of fundamental importance, and will grow in importance as we learn more from developments within different strands of **general systems theory** (e.g., Bertalanffy 1968; Boulding 1956; Gell-Mann 1994; Larsen-Freeman & Cameron, 2008; Skyttner 2001), including **network science** (Barabási 2016). We can of course also foreground considerations of semogenesis: the theory of language must bring out its characteristics as a learnable evolved system.

As an aside — but an important one, we can take a step back to consider the postulation of "commitments" in the light of the context in which we do science. Taking this step back, we can see that the concern with "commitments" relates to the field of doing science, directly to the field of experience — the phenomenal realms we investigate and theorize — but also to the field of activity — doing science as an activity. However, we can complement these field-oriented considerations with **tenor-oriented considerations**. Such considerations include the roles we take on as scientists in different groups, including roles working with professionals and other members of the community outside universities, and the value systems we adopt and enact.[4] Considerations of this kind have, of course, been highlighted by Michael

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[3] There's much more to be said about this, but I'll leave it for another occasion. However, it is interesting to note Kuhl's (e.g., 2010) emphasis on social considerations, including her "social gating hypothesis (Kuhl 2007).

[4] One example we have been involved with is *The International Charter for Human Values in Healthcare Initiative.* See: https://charterforcompassion.org/healthcare-partners/international-charter-for-human-values-in-healthcare-initiative.

Halliday in his discussions of **social accountability** (see Sect. 3.1). The field-oriented considerations and the tenor-oriented ones come together in his conception of **appliable linguistics** (e.g., Halliday 2008; Matthiessen 2014). Halliday (2008: 7) characterizes it succinctly as follows:

> a comprehensive and theoretically powerful model of language which, precisely because it was comprehensive and powerful, would be capable of being applied to the problems, both research problems and practical problems, that are being faced all the time by the many groups of people in our modern society who are in some way or other having to engage with language.

What Michael and I were suggesting was to look at the highest order of phenomena in the ordered typology of systems in semiotic terms rather than only in cognitive terms (cf. Fig. 6.2 above). Interestingly, if you look at the later development of cognitive science, you would notice the moves of saying "No, no, no, it's not disembodied minds, it's embodied minds — embodied in brains as part of biological organisms", and "No, no, no, it's not just isolated individuals, it's individuals in interaction with others" — cf. Geeraerts (2016) on the "social turn" in cognitive linguistics.

If you take a semiotic approach — i.e., a meaning-based approach, you could say that cognitive scientists have taken a huge detour, which is a bit disappointing after the decades following the launch in the 1950s. But if you follow through the trail from Malinowski to Firth (Europe), from Sapir to Whorf (the US) and then to Halliday (possibly in dialogue with Vygotsky in Russia), the semiotic approach is there — evolving throughout these generations of ideas. One has to find ways of having dialogues around these two developments — that of cognitive science and that of the semiotic approach.

One of the most powerful demonstrations of the role played by language in construing our experience of phenomena in the world around us and inside us — phenomena that are usually conceptualized in cognitive terms — is Clare Painter's (1999) work, where she demonstrates that the development of what our cognitive friends would discuss in terms of cognition is fundamentally a semiotic development, and more specifically a linguistic development (cf. Halliday's 1993 notion of a language-based theory of learning). Painter (1999) chronicles this development from the second year of life up to the age of two, three and four.

There are breakthroughs in what children are able to construe for themselves when they master the part of the grammar that provides them with the appropriate resources for construing particular domains of experience. Here relational clauses (i.e., clauses of attribution and identification) play a central role; as children master the resources step by step, they are able to move from labelling to taxonomizing to defining:

- labelling material phenomena by means of cataphoric reference, using attributive relational clauses (e.g., *that's a circle*);
- taxonomizing meanings that have been construed in this way, using relational clauses, either attributive (e.g., *frogs are amphibians*) or identifying (e.g., *frogs, toads and salamanders are amphibians*);

- defining meanings, "concepts", including abstract ones, using identifying relational clauses (e.g., *balance means you hold something in your hand and it doesn't fall*).

This gradual mastering of more of the linguistic system enables children to expand their semogenic potential — always grounded in authentic data and empirical investigation.

6.5 Robin Fawcett's Studies on Cognition

Isaac Mwinlaaru: Robin Fawcett (e.g., 1980) is a key person in SFL who engages with discussions in cognitive linguistics and attempts to develop cognitive models in his version of SFL theory. He has mentioned the "communicative mind" and "belief system" and has tried to model how language production relates to the brain in terms of processing. How do you relate Robin Fawcett's ideas to your own work?

Christian Matthiessen: In his work, which goes back to the 1970s, he attempted to put the "social" and the "cognitive" together. I don't know to what extent we can say that he studied "cognitive phenomena" since this might be taken to imply that he carried out psycholinguistic experiments or grounded the cognitive aspects of the model in neuroscience (cf. Lakoff's 1990 "cognitive commitment", referred to above). But it seems to me that his notions were from mainstream cognitive science, like the notion of belief models. The Cardiff model, developed by Robin Fawcett, Gordon Tucker and their great team of students and scholars, looks like the architecture of a computational linguistic/AI system from the 1970s or 1980s. I am not saying that this is wrong. But it is very different from our systemic functional architecture of language in context — a **relational architecture** that is based on intersecting **semiotic dimensions**, each of which is the domain of relations of particular kinds. I myself was — and am — more interested in the reconceptualization of what our cognitive friends talked about in terms that are semiotically informed, in other words empowered by an understanding of language and other semiotic systems.

We certainly need to address the phenomena that cognitive scientists consider when they talk about "belief systems" and similar abstractions in cognitive models, but Michael Halliday and I were keen to do this in a way that incorporates insights from the interpersonal realm of meaning in relation to the contextual parameter of tenor. That was, more generally, why we developed the meaning base approach rather than the knowledge base approach in *Construing Experience through Meaning* (Halliday & Matthiessen 1999/2006). The meaning base is multifunctional — the **ideation base**, the **interaction base** and the **text base**. We focussed on the ideation base aspect of the meaning base, but we also discussed the text base and the interaction base.

Sketching aspects of the **text base**, we showed how **textual statuses** such as identifiability, thematicity and newsworthiness can be represented as partitions within the ideation base (see Halliday & Matthiessen 1999/2006; Bateman & Matthiessen 1993

and cf. also Matthiessen 1992). Up to a point, the notion of partitions representing different textual statuses works quite well, and one can try to model **textual transitions** from one textual partition to another along the lines we sketched (cf. also Matthiessen 1995, where I try to suggest how this approach might be used to explore thematic progression in text).

In terms of the **interaction base**, we thought in interactive or **intersubjective** rather than subjective terms; for instance, the interactant's ways of projecting models of other interactants in a way that considered the tenor of their relationship. Thus, we went beyond the standard belief models in AI and computational linguistics at the time, models that did not foreground the interpersonal aspect of modelling one's relationship to other meaners in terms of belief, feelings — a history of sharing meanings, often updating the record in casual conversation (cf. Eggins & Slade 2005; Eggins 1990). We were influenced by creative pioneering thinkers who were not part of the cognitive science mainstream, in particular Colwyn Trevarthen (e.g., 1974, 1987, 2009, 2011) from Edinburgh University and his notion of **intersubjectivity**.

One of Trevarthen's contributions is included in Margaret Bullowa's (1979) edited volume of new insights into early development, *Before Speech: The Beginnings of Interpersonal Communication.* This volume also includes Halliday's (1979) chapter on the protolinguistic precursors to the later emergence of dialogue, complemented by his account in Halliday (1984b), and a chapter by Catherine Bateson (1979), who looked at the interaction between mother and child, like proto-conversation. Bateson's chapter developed from videotaping very young children and their mothers, which opens up the possibility of watching the proto-conversation in slow motion. The study demonstrated that the mother and the child were already in some kind of proto-dialogue even right after birth, described as a kind of dance, with the child usually initiating and the mother responding. Colwyn Trevarthen (1979) theorized the phenomena being observed in terms of his notion of **intersubjectivity** (which he had taken from Jürgen Habermas). This is a fundamentally important alternative way of viewing a wide range of phenomena that have traditionally been interpreted in more subjective terms in cognitive science. I was surrounded by belief systems and knowledge-based perspectives for almost a decade in daily work, in my work environment at the research institute (ISI) (see also Sects. 1.1, 2.3 and 2.5). So, I was thinking of a way to re-conceptualize language development in more linguistic, semiotic and metafunctional terms.

6.6 Paul Hopper and Elizabeth Traugott on Semantics

Isaac Mwinlaaru: In the book on grammaticalization, Hopper and Traugott (1993) referred to Michael Halliday's work on stratification. They talked about the semantic stratum, which interfaces with context and lexicogrammar, and pointed out that the stratum could be learned or interpreted procedurally in terms of language production. In this way, the individual speaker tried to cognitively interact with the environment through semantics, and then transferred that to lexicogrammar in terms of the

cognitive model of processing. What do you think of that interpretation of semantic stratum?

Christian Matthiessen: There are a number of interesting points there. One has to do with the stratal location of semantics within the overall system of language in context: we can return to Michael Halliday's (e.g., 1973, 1978) early formulation of semantics as interlevel or **interface**. In a stratal theory and model of language, lexicogrammar and phonology are purely internal to language, and semantics and phonetics (in spoken language) belong to the interface strata or levels, phonetics interfaces with the articulatory and auditory systems, and semantics interfaces with the perceptual systems and motor systems of the brain: see Fig. 6.4.

In Hjelmslevian terms, the inner strata are **form strata** and the outer strata are **substance strata**. The substance strata interface with "substance" in other semiotic systems, both other social semiotic systems such as gesture and drawing and **bio-semiotic systems**, to use the term Halliday and I suggested for sensorimotor systems (Halliday & Matthiessen 1999/2006: 606–610). In the case of semantics, the interface is concerned with meaning — broadly conceived. This includes the interface with the higher-order, or connotative, semiotic system of context (not shown in Fig. 6.4, but see Fig. 6.3).
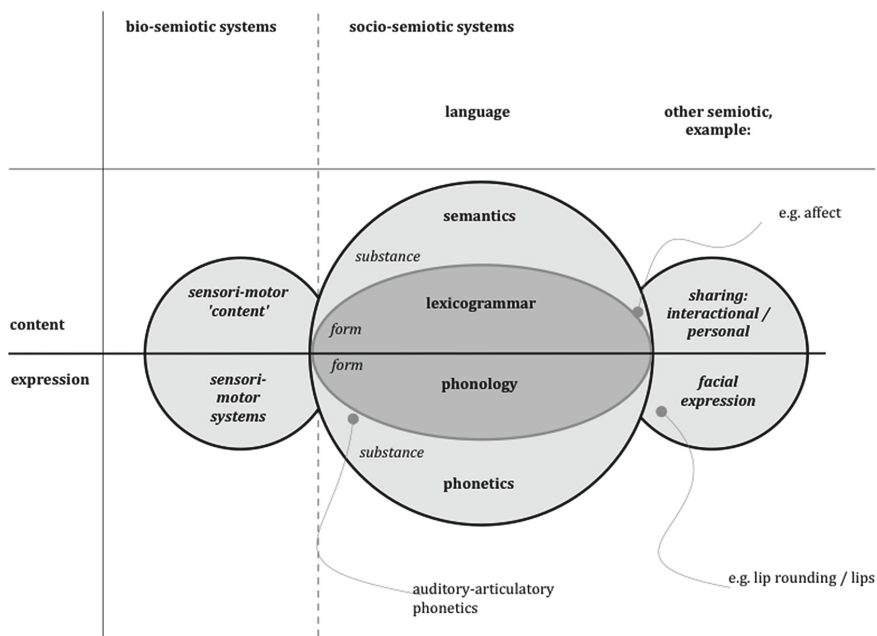
Fig. 6.4 The stratification of language interpreted in terms of form and substance, with the substance strata as interfaces to bio-semiotic systems and denotative semiotic systems other than language

In terms of the brain, the other systems include perceptual systems and motor systems — bio-semiotic systems, but language is unique as a human system in that it is pervasive in the brain, serving to integrate other systems within different regions. To illustrate this point, I have referred to Bickerton's (1995) example showing that the "linguistic cat" is the "holistic cat": if you think about the cat, the linguistic cat is the cat that integrates all our experiential engagements with cats. The concept of cat in our experiential semantic network is linked to our visual image of cats, our auditory image of cats (what they sound like), possibly to our tactile experience (that they are soft and furry, and may scratch or bite), our experience of lifting cats, cuddling cats, and so on. Our whole feline experience is accessible through the cat node in our experiential semantic network, which illustrates the insight that language is the one human system that integrates different parts of the brain, e.g., auditory cortex within the temporal lobe and the visual cortex within the occipital lobe. This relates back to the meaning-based versus the knowledge-based distinction. Here there is a kind of real resonance between Bickerton and the Hallidayan systemic functional insights.

In AI, there was a longstanding question about the conception of knowledge (or meaning, in our semiotic interpretation) — the distinction between declarative knowledge and procedural knowledge, which came into focus in the 1970s.

There was quite a debate about this distinction, and part of the picture was related to what Terry Winograd[5] (an AI researcher who studied with Michael Halliday in London in the late 1960s) did when he built his SHRDLU system, where he introduced a procedural way of thinking about meaning in terms of the processes undertaken to do something. You could trace that back to ideas in Western philosophy, e.g., Wittgenstein's objection to the early Wittgenstein — the *Tractatus* model in the tradition from Frege and Russell, which was focussed on declarative, propositional, knowledge or meaning, as opposed to meaning as a way of doing (action and interaction).

There is a whole complex way of sorting out the complementarity between declarative and procedural representations. On the one hand, there are the phases along the cline of instantiation — potential, subpotential/instance type, instance — and the process of instantiation itself. On the other hand, there is the metafunctional one, where the ideational invites the declarative way of thinking about it, versus the interpersonal inviting the procedural one (language as a mode of action and interaction), which as Geoffrey Sampson (1980) pointed out, Malinowski (1923) said quite a long time before Wittgenstein became famous for it — the interpretation of meaning in terms of use. So, you have these tensions built in the history of human thinking — our intellectual history. The different interpretations become thesis-antithesis pairs, and often the appropriate intellectual way forward is to turn to a theory that allows us to conceive of a synthesis. That is what systemic functional theory does. But it takes a great deal of work. With the growing work in the area of semantics and neuroscience, there is an additional insight based on the relationship between different domains of meaning within semantics and sensorimotor systems, e.g., the relationship between

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[5] e.g., https://en.wikipedia.org/wiki/Terry_Winograd and https://profiles.stanford.edu/terry-win ograd.

doings-&-happenings within the semantics of figures and bodily action — explored by García and Ibáñez (2017) in systemic functional terms (see further e.g., Kemmerer, 2015: Part V on "grounded cognition" and the "hub and spoke model").

As an expert in the history of English and pioneer in research on grammaticalization, Traugott, from Stanford University, has produced very interesting and important work (e.g., Traugott 1985, 1997; Hopper & Traugott, 1993/2003), and drawn some insights from Michael Halliday (e.g., Traugott 1982). Halliday has been to Stanford for extended periods twice: one was in the early 1970s and the other was in 1979, which was when I met him there (see Sect. 1.1). There had been interactions and discussions between Halliday and Traugott. As Randy LaPolla has pointed out in the discussions on grammaticalization, Traugott (e.g., 1982, 1997) adopted some insights from the theory of metafunction in SFL.

6.7   Instantiation and Individuation

Isaac Mwinlaaru: At the ESFLC (European Systemic Functional Linguistics Conference) in Paris in 2014, Margaret Berry (2014) gave a plenary talk, where she mentioned the notion of choice in relation to system network and instantiation. As you have pointed out, instantiation will involve different phases, and is the selection of systemic features from the systems that make up the resources of the linguistic system. Her point was: SFL has talked about language, but the *speaker* (or user) has been backgrounded. If we bring in the speaker here, how would you relate this to the process of the brain in terms of instantiating text?

Christian Matthiessen: Unluckily, I missed the talk. Did she talk about computational modelling in SFL?

Isaac Mwinlaaru: Not really.

Christian Matthiessen: No, that unfortunately gets overlooked in the main currents of SFL again and again, but computational modelling in SFL is of crucial importance in relation to your question because that is where you have to come to terms with both the speaker and the addressee as part of modelling the exchange of meanings. What we were doing through the 1980s was modelling not in the sense of psychological realism (whatever that would actually mean), but modelling the processes of generating text. I do not see how you could say the speaker was not taken into account in that body of work. That was precisely what we investigated and modelled. And the same applies to the computational work based on SFL by Robin Fawcett and his team at the University of Cardiff.

But I would also say: go to the literature on ontogenesis (e.g., Halliday 1975a, 2004; Painter 1984, 1999; Torr 1997, 2015; Painter, Derewianka & Torr 2007) because to arrive at deep insights into language development, you have to do case studies — longitudinal studies of individual children. The interpretation of ontogenesis focusses on the growth of individual meaners, but it does not lose sight of

the group because children always learn how to mean in interaction with others — initially with the members of their most immediate meaning group (cf. Halliday 1978 on the development of persons and personalities through interaction with members of groups, discussed below). I agree that it is very important to engage with speakers, and I agree that this has not been the focus in a good deal of SFL works, but one can go to sources where it has been the focus. I think this is crucial to really open up the dialogue with the cognitive folks, and it relates very directly to the longstanding puzzle in the twentieth/twenty-first century of what the relationship between the individual and the group or collective is.

You can go back to how Malinowski (1935) problematized the notion that comes through from Durkheim (1898, 1965) of the collective consciousness. Malinowski was influenced by him, but in the sense of reacting against this, he was using the *kula* exchange system in the Trobriand Islands and what he had experienced as a participant observer. He said: nobody knows the system in the sense of having a comprehensive understanding of the system, and yet all the members of that society together re-enacted it regularly. He problematized the question of who knew this system, and the answer was the ethnographer. So, we need to face it head-on because it is central to a number of tensions that are still around. That is one aspect and there are different attempts at this.

Firth (1950) made a very important contribution in his paper on person and personality. Michael Halliday (1978) continued this discussion in his book on language as social semiotic, illustrating how the person emerges from the group as an assemblage of personae or social roles taken on in different social groups: see Fig. 6.5. There is a kind of dialectic between the individual and the group. Going back to Firth and then Halliday, David Butt (1991) picked up the conceptualization and investigation of person and personality, and this framework has also been applied productively in the investigation of the self in psychotherapy, as in Henderson-Brooks (2006).

The tension between the focus on the individual and the focus on the collective has run through the twentieth-century linguistics and also other related disciplines within the field of "human sciences". It is very significant and quite interesting. In my own work since 1980, I found myself in a hot-zone where the AI and cognitive science conception clashed with the more ethnographic and social-oriented approaches, including SFL.

In 2003, there was an Australian SFL (ASFLA) conference in Adelaide, where I explored the relationship between the individual and the collective. Since we have Halliday's theory of the cline of instantiation, how do we conceptualize the individual in relation to instantiation as always being constructed through instances in interaction with people, observing how individuals emerge over time as persons with personalities (Fig. 6.5) — the process of **individuation**? Jim Martin and others (e.g., Martin 2008, 2009) have also worked on this problem; there are relevant contributions in Bednarek and Martin (2010). They used notions from sociology and psychology — including **affiliation** and **alignment**. This perspective complements that which is foregrounded by the notion of individuation. Individuation emphasizes the development of persons as aggregates of personae emerging as they interact with people in different groups, most likely starting with the institution of the family (cf. Halliday,

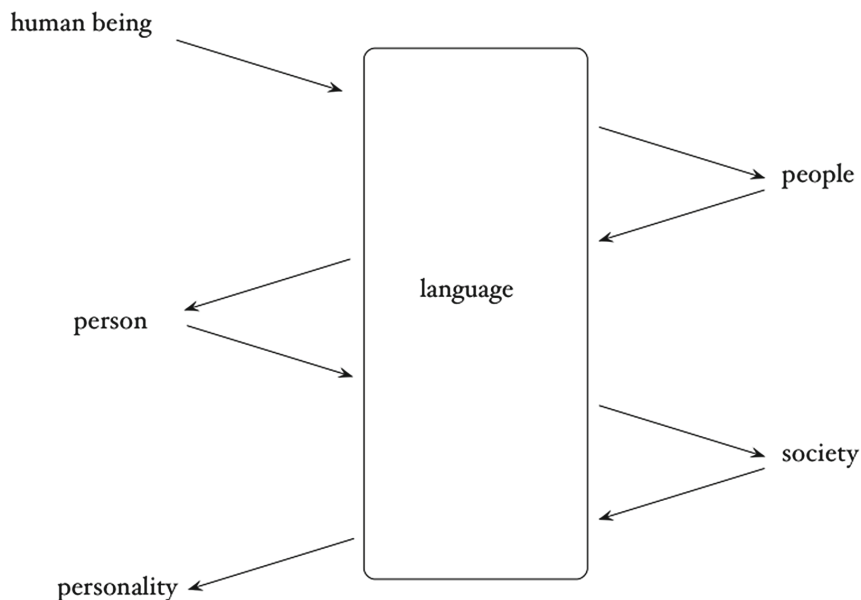INDIVIDUAL   GROUP

Fig. 6.5 Halliday's (1978: 15) schematic representation of individuals emerging as persons and their personalities through interaction with groups of people and members of societies

1978). The individual affiliates with the group, or as in the work by Martin Pickering, a psycholinguist, alignment is a process in dialogue of interactants aligning with one another (e.g., Pickering & Garrod 2006).

It would be helpful if we take the challenge of modelling this explicitly and seriously as is done in computational linguistics and AI. Here we can draw on the work by Colwyn Trevarthen, coming from, and thus grounded in, biology. As mentioned above, he and his group have done extensive research shedding light on **intersubjectivity**, proto-conversation and related aspects of infants seeking to commune and communicate (e.g., Delafield-Butt & Trevarthen 2015; Trevarthen & Aitken 2003; Trevarthen 1979, 1987, 2009, 2011). Trevarthen's interpretation of infancy and early childhood resonates with Halliday's pioneering work on learning how to mean, as can be seen from the references they make to each other's contributions (cf. also Smidt 2017).

In addition, for considerations of the speaker, systemic functional linguists can turn to the work by Robin Fawcett and his group (originally modelled by Fawcett 1980 — a model which has informed subsequent research).

As we grapple with tasks such as relating the system to the instance along the cline of instantiation and the meaning group to individual meaners, we can ask general

metatheoretical questions. What kinds of model do researchers use when they extend their territory of theorizing, modelling and observing? How do you go about it? One approach (or methodology) that has been very dominant in the last few decades is the macro one — a kind of **eclecticism**. In fact, the notion of the eclectic account has been given high value, and that is positive and fair enough. But you could also look at eclecticism from another vantage point, adopting a different approach — the one I have worked with throughout my career, **meta-translation**. Eclecticism is like quilt work, or it is like Frankenstein's monster. It is difficult then to know how things fit together, so I prefer to do meta-translation, i.e., take insights from a wide range of frameworks and disciplines, absolutely acknowledge them and learn from them, but then translate them into Systemic Functional Linguistics so that I can see how everything fits together in terms of the dimensions of the systemic functional architecture of language and other semiotic systems and so that I can reason about the whole (see also Sect. 9.8). This approach supports systems thinking (e.g., Capra 1996).

Isaac Mwinlaaru: I think for research purposes, it is good to talk about instantiation and individuation, where people can clearly focus on different aspects of individuals and societies interacting with one another to create meanings. Theoretically, do you think instantiation is separate from individuation in terms of language production? I sense that once individuals are involved in interaction, they are instantiating text and at the same time they are individuating themselves. Do you think this is theoretical? For research purposes, we may have to separate these, do you think it would be theoretically valid to say instantiation and individuation are different?

Christian Matthiessen: Yes, absolutely. I agree with you. My sense is that "individuation" has not yet been sorted out — nor its relation to instantiation, and there is a very strong pressure to theorize this area so that we can get on with a wide range of tasks. I feel a bit frustrated with myself in that this area has been there on the agenda for quite a long time — I could see the tension between cognitive and socio-semiotic approaches quite clearly already in the 1980s as we were developing models of text generation. However, it is not a trivial challenge; it is reflected in various explorations throughout the twentieth century and two decades into the twenty-first century — including the tension between macro- and micro-views, the tension between the individual and the collective, the tension between social and cognitive conceptualizations, the relationship between the system and users of the system. We can of course posit individuation as another dimension — a cline like the cline of instantiation (cf. the contributions in Bednarek & Martin 2010); but I don't think this will work out. Individuation is not a "dimension" in the same way that, say, the cline of instantiation and the hierarchy of stratification are. I explore some of the options in Matthiessen (forthcoming) as part of the discussion of the architecture of language according to systemic functional theory.

How far up the cline of instantiation towards the meaning potential and the culture potential can an individual person, an individual meaner, move? That may partly depend on the nature of the society — its size and organizational complexity. But you must have an account of this. When you move towards the potential, it begins

to become a group of collective enterprise. How are they related to one another? That is also why I resist the notion from Jim Martin and his group that reading a text (reading position or reader position, listening position or speaker position) is even more instantial than the text because reading (or listening) positions derive from the **angles on the system** that different groups of people adopt. It is not just located at the instantial end, but is something that extends up the cline of instantiation. There is a lot of interesting work to be done there, including modelling not only registers but also the Bernsteinian codes — what Ruqaiya Hasan (e.g., 1973, 1989, 2009) worked extensively on.

Again, the problem goes back to these very fundamental questions that have bedeviled human sciences for a very long time. You get different manifestations of them, including the cognitive versus the social, the individual versus the group (the collective), the system versus the instance and the micro versus the macro. If we are going to benefit from the dialogue between the social and the individual scientists (people who focus on it), this needs to be sorted out. There is another thing that has to do with the individual focus: you tend to get people who work with more explicit models, whether such models can be found in speech act theory or in AI/ computational linguistics or somewhere else like knowledge systems or belief models, whereas those who work with social interaction are more collective and do not tend to develop these explicit models. Unfortunately, those who work with the more explicit models do not seem to value the contributions by scholars who do not produce or use explicit models. We need to instill a sense of valuing the latter in order to move on because as long as there is the notion that what some people are doing is not valuable, it will be difficult to make progress. I have felt this for 40 years because I have been involved in this interface between computational modelling and discourse analysis since 1980.

6.8   Conceptual Metaphor and Grammatical Metaphor

Isaac Mwinlaaru: Metaphor has been a key topic in cognitive linguistics right from the beginning. It has been theorized and discussed by many scholars interested in language and cognition. The notable one was Lakoff and Johnson's (1980) *Metaphors We Live by* on conceptual metaphor. In grammaticalization, Bernd Heine and his colleagues (e.g., Claudi & Heine 1986; Heine & Kuteva 2007) have related metaphor to the development of grammar and the transfer of meaning of a lexical item, which is always more concrete to abstract environments. In SFL, we have grammatical metaphor. Could you tell us what grammatical metaphor is? How would you relate it to the works on metaphor by scholars in cognitive linguistics?

Christian Matthiessen: I think that is a very crucial question. The potential for metaphor emerges in language with the split of the content plane into semantics and lexicogrammar — into meaning (semantics) and meaning constructed as wording (lexicogrammar). Thus, the potential is a characteristic of higher-order semiotic
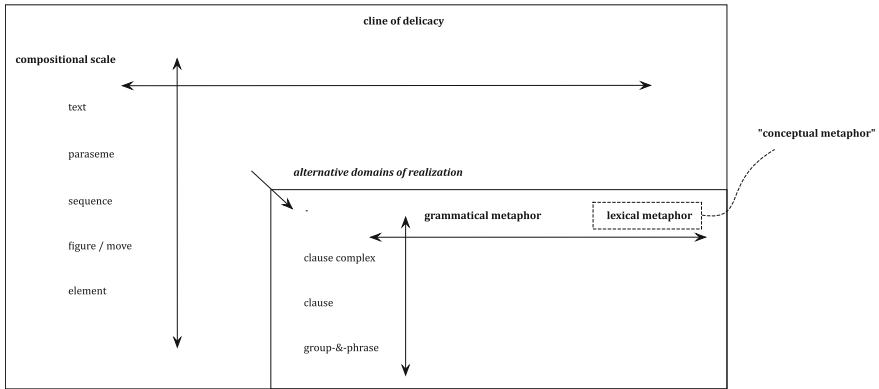
Fig. 6.6 Lexicogrammatical metaphor as a realizational relationship between semantics and lexicogrammar extended along the cline of delicacy

systems, i.e., ones where the content plane is stratified into two content strata. Language is the prototypical higher-order human semiotic; while there may possibly be other kinds, this has yet to be demonstrated. Of course, in the course of ontogenesis, once young children make this split during their second year of life, as they move into the mother tongue, they do not take up this potential for metaphor immediately, but it is there for them as a semogenic resource, and when they begin to take advantage of it, they start with the interpersonal metafunction — with metaphor (this being one manifestation of Halliday's 1993 interpersonal-first principle).

Now, since metaphor depends on the relationship between semantics and lexicogrammar, it is inherently **lexicogrammatical metaphor** — extending from the very general systems of grammar to the very delicate systems of lexis.[6] This is shown in Fig. 6.6: being a relationship between semantics and lexicogrammar, metaphor covers the full range of the cline of delicacy from the grammatical zone to the lexical zone, and its upper bound is the most extensive domain of lexicogrammar, i.e., the clause complex. Lexical metaphor was, of course, the traditional focus of studies of metaphor; and, in cognitive linguistics, it has been conceived of as "conceptual metaphor". In principle, "conceptual metaphor" could cover all of lexicogrammar, including the grammatical metaphor; but the accounts in Lakoff and Johnson (1980) and later publications always seem to focus on lexical metaphor. However, it is still of interest and relevance to systemic functional work on lexicogrammatical metaphor

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[6] This theoretical insight into the conditions for and nature of lexicogrammatical metaphor can be contrasted with Black's (1962: 28) characterization: "To use a well-known distinction, "metaphor" must be classified as a term belonging to "semantics" and not to "syntax" — or to any *physical* inquiry about language.)" He adds "pragmatics" a few pages later; but the fundamental point is that *metaphor depends on the stratification of the content plane* into semantics and lexicogrammar, and exploits the realizational relationship between the two.

(cf. our comments in Halliday & Matthiessen 1999/2006).[7] And Mark Turner's (e.g., 1990, 1992) proposal for representing and modelling (lexical) metaphor can serve as one source for further work on the explicit representation of metaphor in SFL.

In SFL, metaphor — lexicogrammatical metaphor — has a clear location in the overall content system of language (alongside other figures of speech). This is related to the holistic nature of systemic functional theory and the goal of developing comprehensive descriptions of particular languages. Since most attention had traditionally been devoted to lexical metaphor, Michael Halliday needed to fill a gap and to shed light on grammatical metaphor, an investigation that can be traced back at least to his "Grammar, Noun and Society" (Halliday 1967). This was the period of the first sustained project on scientific English directed by him and reported on by Huddleston et al. (1968).[8] To describe scientific English, and also scientific registers of other languages (e.g., Halliday 1984a, 1993b), Halliday needed to flesh out the account of grammatical metaphor — and of course, he thought it through systematically, netting in not only **ideational metaphor** but also **interpersonal metaphor** (which follows from systems thinking based on intersecting semiotic dimensions, in this case the hierarchy of stratification and the spectrum of metafunction[9]). At the same time, we also recognize that scientific discourse depends on both lexical and grammatical metaphor — in other words, on lexicogrammatical metaphor. Various domains of our experience of the world are construed by means of lexical metaphor (as Whorf 1956 showed a long time ago) in everyday discourse, and when they are reconstrued scientifically, this reconstrual of our experience involves the full continuum from grammatical to lexical metaphor, as we illustrate in Halliday & Matthiessen (1999/2006, in particular in Chap. 14) (cf. also Matthiessen 1993, 1998). Within lexicogrammar, lexical patterns tend to be more exposed, more easily accessible to speakers of a language, than grammatical patterns — certainly than cryptogrammatical ones, and the same holds true of lexicogrammatical metaphor. People are more likely to notice lexical metaphors than grammatical ones. For example, people have become aware of various lexical metaphors used in construing the outbreak and spread of COVID-19 — metaphors mapping aspects of the virus and the pandemic onto the experiential domains such as those of warfare, fire and flooding, as noted by Elena Semino in a valuable contribution to *The Guardian* on July 5, 2021: *Fire, waves*

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[7] I remember visiting George Lakoff in his office at UC Berkley and asking him about his opinions on SFL in the mid-1980s. I do not think he saw the connections there, but I do think that there are very interesting connections.

[8] They tried to get a book version of the report published, but formal linguistics had become so prominent in those days that publishers were not interested in text-based studies, so a number of studies like this one fell by the wayside and never got published. Huddleston (1971) did put together a book on his own that reflected some of the findings, but that report would have been an early example. The project clearly showed that it was important to have a way of understanding what happened in scientific English, and there was not really anything around. That was a real impetus for Michael Halliday's work on grammatical metaphor.

[9] So we can add one more dimension to Fig. 6.6, giving it perspectival depth to represent the addition of the metafunctional distinction between the ideational and interpersonal modes of meaning.

and warfare: The way we make sense of Covid. But the grammatical contribution to our metaphorical construal of Covid is equally important.

If one reasons about metaphor in a well-rounded way — trinocularly, holistically and systemically in the sense of systems thinking, one would ask about the metafunctions organizing both semantics and lexicogrammar within the content plane; and it turns out that lexicogrammatical metaphor operates not only within the ideational metafunction but also within the interpersonal one.[10] Halliday indicated that what linguists and philosophers conceptualized as indirect speech acts in speech act theory was a metaphor in the environment of the interpersonal (Halliday 1984b). You can put this together with his later work on interpersonal first principle (e.g., Halliday 1993a) and you would realize that a number of phenomena in language were first developed in the interpersonal environment. So, that was one reason for focussing on the grammatical zone within lexicogrammar.

To make my comments a bit pointed, when *Metaphors We Live by* appeared in 1980, I was already studying at UCLA and I remember the attention that their book received; but I did not think that there was fundamentally anything new in their work. This was partly because of the tradition of the study of metaphor in lexical semantics and in stylistics, but also because of Whorf (1956), who talked about this in the 1930s and early 1940s in detail in terms of the phenomena themselves and the broad outlines (cf. also Black 1962). And the same would have been the case in a number of other traditions, including linguists who had worked in historical linguistics. I remember one occasion when George Lakoff gave a talk on conceptual metaphor at UCLA (sometime in the early 1980s), and Robert Stockwell (from UCLA linguistics) asked him during the Q-&-A period when he thought metaphors became "dead metaphors" in the history of languages, and when one language borrows from another — Stockwell cited *arrive* from Latin "to" plus "shore" as one example of an item that was metaphorical in the original language but might not be recognized as such in English. Stockwell was, of course, very familiar with the role metaphor plays in the history of languages.

Interestingly, after Lakoff and Johnson (1980) had been published, Lakoff and others working with the general notion of conceptual metaphor and systems of conceptual metaphor have taken this in the direction of a description that was grounded in text but also applied in the analysis of text. As the US and its allies moved towards the first Gulf War, Lakoff circulated a paper pointing out how certain metaphors were quite misleading but useful to the war mongers, like the metaphor of war as surgery. This development in the engagement with metaphor was important, and it has continued in productive ways. If you talk to scholars like our colleague

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[10] Some systemic functional scholars have suggested that there are also textual grammatical metaphors; but when we discussed such proposals, neither Michael Halliday nor I found these suggestions convincing. There was no "as if" aspect present in the cases cited as examples of textual grammatical metaphor. Metatheoretically, this would seem to be an interesting area: if there are ideational and interpersonal metaphors, why not textual ones? To address this issue, we have to go deeper into the nature of the metafunctions, and take into consideration the distinct nature of the textual one as an enabling metafunction (cf. Halliday 1978; Matthiessen 1992).
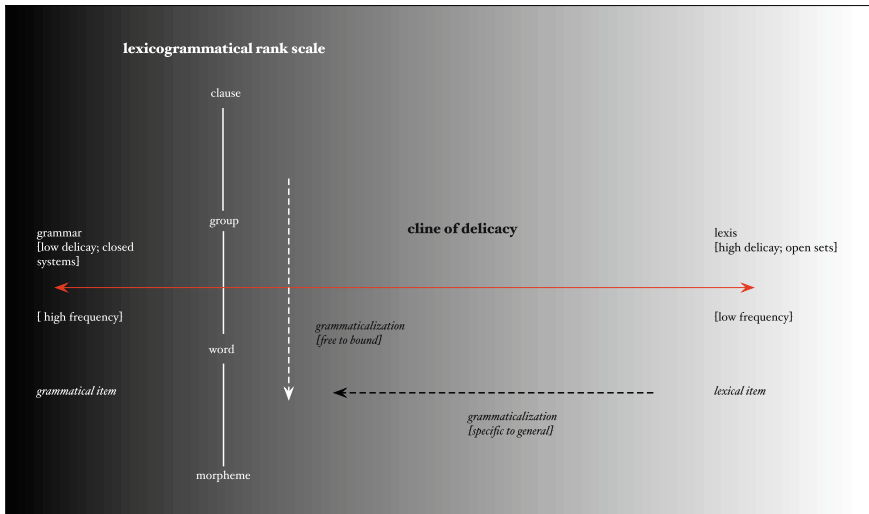
Fig. 6.7   The continuum from grammar to lexis

Dennis Tay (e.g., 2010, 2011; Tay & Jordan 2015), he will say that, like SFL, Conceptual Metaphor Theory is usage-based theory. By now, a number of strands in cognitive linguistics are usage-based (cf. Geeraerts 2016).

   Just to round off our discussion of this topic: there is still very interesting work to be done on lexicogrammatical metaphor (see Fig. 6.7) — i.e., the continuum from grammatical to lexical metaphor — in different registers, within both the ideational and interpersonal metafunctions, as part of the description of an ever-wider range of languages. We need to bring out and network the way in which lexicogrammatical metaphor is a semogenic resource that serves to expand our ideational semantic strategies for construing our experience of the world as meaning and our interpersonal semantic strategies for enacting our roles, relationships and values as meaning. In this context, it will be helpful to consider lexicogrammatical metaphor alongside other semogenic strategies, including those that have also traditionally been interpreted as "figures of speech" — simile and synecdoche (see Halliday 1985b: Chap. 10).

6.9   Collaboration Between Systemic Functional Linguistics and Cognitive Linguistics

Isaac Mwinlaaru: There is a paper by Butler (2013), in which he gives an overview of SFL and the areas that can be combined, including cognitive linguistics in general, and particularly construction grammar and cognitive grammar. He has suggested a close interaction between SFL and some other frameworks. In which areas can

SFL collaborate with these traditions in the pursuit of knowledge on language and cognition?

Christian Matthiessen: One interesting academic and political question is: are they interested? Because collaboration suggests dialogue, and dialogue suggests a kind of give and take — it is based on the mutual exchange of meanings, on reciprocity. It is striking how little dialogue there has been. One person who made a very big effort to connect with, and draw on, Ronald Langacker's work in cognitive linguistics was Kristin Davidse (e.g., 1992, 1996), and she has continued to develop this framework together with other members of her group, an interesting example being Vandelanotte's (2009) "cognitive-functional" conception of projection.

How can you understand this? In different ways. In terms of the field parameter of context, what is your experience? What part of the literature do you control? Do you stop developing your own research tradition to look at this or not? But there is also a tenor aspect of it. The moment you go to somebody else's work in another tradition, you are potentially in some sense abdicating your status position in a hierarchy of power. It took me a long time to realize this, so I was getting impatient with people. I wondered why others did not engage with SFL even when there was so much resonance with their work. As it turned out, I was thinking only in field terms. At some point, it clicked. It was also about tenor — interpersonal networks, positions of academic strengths, the ranking of channels of publication and so on.

Has this been a reciprocal dialogue? I remember meeting Ronald Langacker in the US in Washington DC at the Georgetown University Roundtable in 2006. We had both been invited as plenary speakers. We discussed the interaction between his cognitive grammar and SFL, and I emphasized the potential for dialogue. He said: "Yes, of course, Kristin Davidse was trying to do this." He was very much aware of it, but I do not think you will see a flood of references to her work or a kind of reciprocal attempt to do further the dialogue (cf. the lack of references in Langacker 2008, 2013).

So, a reasonable practical test is this: when you are thinking of having a dialogue with somebody, do they actually need to refer to your work? The answer is often "no" because they have enough infrastructure and enough status and power in place in their own community to ignore you. One of the fairly late discussions I had with Geoff Thompson was about what scholars are included in citations, in particular in citations across frameworks and traditions. In this context, I talked about the effacement of Michael Halliday. Since Halliday was outside the dominant current within linguistics for a long time, out of phase with Chomskyan linguistics, scholars and students in linguistics tended not to engage with his work, either not seeing its significance in the context of mainstream theoretical linguistics or ignoring it altogether. Once linguists started to rebel against the Chomskyan paradigm and explore positions closer to Halliday's, they did not generally refer to him or to scholars who had followed up on his work. This tendency to overlook his work is clear in development in the last couple of decades concerned with the nature of language as a probabilistic system, with the continuity between grammar and lexis, with the intrinsic functional organization of language. As I sometimes say, Michael Halliday should have got himself born in

Texas rather than Yorkshire (once described by Mary Abercrombie as "the Texas of England") (cf. Halliday 1985a). On the other hand, if he had been born in Texas, he would not have had the kind of fertile linguistic academic soil he had in Britain.

6.10   Some Advice to Young Scholars on Future Research

Isaac Mwinlaaru: What is your advice to young scholars in SFL who hope to explore the links between the social semiotic perspective on language and language as something that interacts with the brain? What areas need to be focussed on? What areas have been neglected?

Christian Matthiessen: Heidi Byrnes has made a very important point about what to do if you want to be heard in North America and especially in the US context. In terms of SFL, there are a number of people contributing to it and engaging with it in the US. While the numbers are growing, there are not so many of them, but globally that is not critically important, because there are many people in Asia, in Australia, in Latin America, in Europe and a growing number in Africa. You have to answer this question in terms of what it is that you want to achieve within different fields of activity, but (as noted above) tenor considerations obviously also matter — in particular, in terms of power structures. Heidi Byrnes said to me: "It is all very well to do the work in applied linguistics, the genre model and so on, but you will not have a breakthrough until you engage with theoretical linguistics in the US, focussing on issues those theoretical linguists are concerned with." She urged me to address research questions that theoretical linguists are concerned with in the US. I think she was right.

It comes back to your question about **the construction of dialogic interfaces**. How do you get people in another camp, another community, another tradition to take an interest in dialogue and really engage in it? Probably, it can only happen with new generations, i.e., people who are more intellectually and institutionally mobile and who can benefit career-wise from taking on and developing new insights.

But one has to be very cautious and careful. In another of our discussions, I mentioned what Ruth Brend said to me in around 1987 (see Sect. 1.4): even though she found Tagmemic Linguistics and Pike's work absolutely invaluable, she would not take on new PhD students wanting to use Tagmemics because by then she considered Tagmemics a dead metalanguage. If students used it in their PhD research, they would not have any career opportunities. That is obviously an important consideration (although it can be a self-fulfilling prophesy).

Unlike Tagmemics,[11] SFL has survived the very difficult period in linguistics when Chomskyan linguistics dominated from, say, the second half of the 1960s into the 1990s, and it has even flourished and expanded (though mostly outside linguistics departments, and certainly outside linguistics departments in the US — which is related to Heidi's point), but research students still have to consider career paths very carefully. That puts a constraint on what you can do.

6.10.1   Passionate Interests and Career Considerations

However, let's put career consideration aside for a moment. If someone is interested in linguistics, then this is already an unusual choice in terms of study paths. If they still go ahead and study linguistics, but at some point decide to pursue areas that are likely to be strategic in terms of career opportunities, then maybe it would make sense to re-think about the decision to go into linguistics in the first place or even to pursue an academic path at all. There are so many obstacles nowadays for anyone starting out on an academic career that unless they are passionate about what they want to do, they are likely to find it very difficult to sustain the effort and persevere despite all the difficulties involved in securing an academic position (and then, contract renewal, tenure, promotion).

So I would say that while it is important to keep practical career considerations in view, one should do what seems truly energizing, exciting and effective — what really fires you up. That was what I did myself, but arguably "nerdily" rather than strategically. If I had known what it would be like career-wise, maybe I would have thought twice. But fortunately I did not think about such issues at all. Thankfully, all the metrics that have now been introduced were not around, so I was not constrained in my imagination by these ghastly anti-intellectual metrics that are supposed to guide us in decisions about where to publish. I was still part of the generation where publications were valued in their own right, and would be judged in terms of the quality of the contribution they made, not by the number of stars of the journal, of the publisher based on citations and impact factors. The constraints that have been introduced based on such superficial metrics — features that are easy to measure but very likely totally trivial as easy-to-measure features usually are — are a true tragedy as far as research and scholarship are concerned. The people who have enabled this syndrome of deeply depressing developments should be encouraged to take a step or two back so that they can get a clear sense of the devastating long-term effects of this infatuation — this dangerous liaison — with superficial metrics. How did we get to

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[11] Cf. Pike (2001), completed in the month before he died on 31 December 2000: "A second major change was the paradigm shift in linguistics from descriptive (or structural) linguistics to Chomskyan transformational linguistics. While this was good for anthropological linguistics — all linguistics is anthropological, by the way — it was unsettling for me personally because I came out of the Bloomfieldian descriptive linguistics school, and especially because the transformational revolution shoved my own tagmemics theory to the back-burner. A humbling experience for me, but not surprising when we think of Thomas Kuhn's (1970) model.".

this point? Well, I think the pattern is common: enablers make small decisions, each of which probably seems harmless enough, but the cumulative effect of apparently innocuous decisions can turn out to be an existential threat. A creeping crisis that may only be noticed when it is too late.

But on the other hand, it meant that I did not accumulate publications in "A" journals because I got invited to contribute to books, edited volumes, etc., which was wonderful. Even now I have to pay the price for that. But if one is starting out, then one has to think about publication options very seriously. But again, if that is all one is doing, then why be an academic? It is not that academic positions pay better than other positions. It is not that the working conditions are better. (Not really actually. To succeed as a junior academic at The Hong Kong Polytechnic University, and at many other universities, people have to work seven days a week, without any real breaks during the year from obligations and deadlines.) Why become an academic if you do not do what you really consider to be exciting, if you do not feel that you are doing illuminating and life-enhancing work? One has to somehow balance these considerations.

6.10.2   Areas to Focus on — Areas that Need Work

I feel that SFL, as developed by Michael Halliday and others working with him, has given me so much beyond a job and a career on so many fronts one meets going through life. If you are in this kind of position, that is very important.

Now, if you decide that you would really like to be part of the development of SFL, then it is completely rational and productive to try to get a sense of the trajectories of development and to identify gaps that need to be addressed. What has not been covered? Fig. 6.8 shows the attention given to the different strata of language in context by scholars in the Firthian-Hallidayan tradition (which Sampson 1980 calls the "London School"). One can think about language itself, moving from Firth to Halliday, and then try to flesh out the agenda for further work as far as stratal coverage is concerned (see e.g., Matthiessen 2009).

As Fig. 6.8 indicates, Firth worked mainly on the outer strata, i.e., context and phonology and phonetics. When Halliday began to develop what was to become SFL, he saw that he had to work on the inner ones, in particular the strata of the content plane. That was why he worked on lexicogrammar, starting in the 1960s and summarized initially in the first edition of his *Introduction to Functional Grammar* (Halliday 1985a, b). Then based on the work on lexicogrammar, it became possible to do more work on semantics and relate the metafunctional account of semantics to context (e.g., Halliday 1973, 1978, 1984b).

One can see this kind of trajectory, but work on the inner strata (lexicogrammar and phonology) has largely taken a backseat in recent times — in one way, naturally enough, since there are other areas that have needed attention. Part of the reason is the question of what pays off in doing linguistic discourse analysis or applications that involve discourse analysis, as in education, healthcare communication and forensic
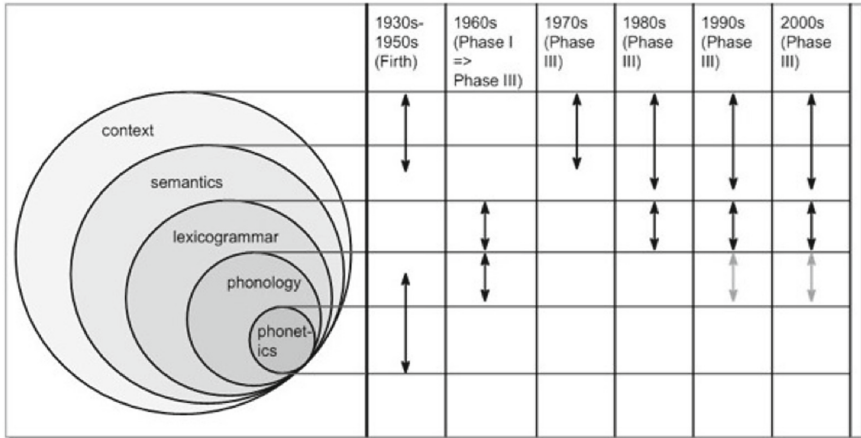
Fig. 6.8 Phases in the development of SFL out of Firthian linguistics, represented as expansion of coverage of the strata of language in context

linguistics. There are many applications where discourse analysis is central, and that already draw your attention to the aspects of the overall system of language. People have paid much or less attention to phonology since Firth and Halliday's earlier work on phonology, but here one has to clarify the different domains within phonology. There has been more work on prosodic phonology, but there is very little work on articulatory phonology (cf. Matthiessen 2021a, b). Systemic functional theory offers a very unique and considerable potential for the development of new insights into phonology, and the phonological systems of particular languages. Phonology would be a very exciting area to return to, building on what Firth and Halliday (in his early years) worked on and of course on subsequent developments (e.g., Bowcher & Smith 2014; Tench 1992).

6.10.3   Morphology

The focus on morphology in various linguistic traditions is partly an accident of the languages that these traditions were concerned with, as with traditional grammarians in the West focussing first on Ancient Greek and then Latin, and starting in the early twentieth century, American Descriptivists working on various indigenous languages with rich word grammars (e.g., Boas on Kwakiutl, now Kwak'wala, and Sapir on Takelma, a so-called polysynthetic language). On the whole, systemic functional linguists have been working on languages that do *relatively* less work at word rank, i.e., that have relatively less elaborated word grammars — even considering the systemic functional descriptions of languages like Japanese, Korean, Arabic or Finnish. I would love to see work on so-called polysynthetic languages. That would

be very valuable. Recently, I was asked by Edson Rosa de Souza, a scholar in Brazil, to contribute a chapter on systemic functional morphology (Matthiessen 2015a) to a book organized around questions asked of proponents of different linguistic theories, and I enjoyed the task he gave me thoroughly. Of course, I had to do it sketchily under time constraint, but it was very interesting to go back to morphology. When I studied Modern Standard Arabic in the second half of the 1970s and tried to learn the language, I began to think about the interesting challenges involved in developing a systemic functional description of Arabic word grammar, as part of the overall description of the grammar (see Bardi 2008).

6.10.4   Phonology and Graphology

Focussing on the expression plane of language, I think it would be fantastic to have the analogue of the accounts on phonology and phonetics for written language — i.e., systemic accounts of graphology and graphetics (cf. Sefton 1990). That would dovetail beautifully with multimodality, including all the work on images; and it would of course be very helpful in the analysis and interpretation of art made of graphology, as in the cases of Chinese and Arabic calligraphy — **graphological art** as a special case of verbal art (e.g., Hasan 1985), informed also of course by work on "visual semiotics". How will you work that out? You can start with John Bateman's (2008) stratification of the expression plane (layout) and content plane (e.g., RST [Rhetorical Structure Theory] analysis).

6.10.5   Semantics as Interface

Focussing on the content plane of language, I do think that semantics needs a great deal of further work. While there have been valuable descriptions of the internal organization of semantic systems — with most attention having been devoted to English, these contributions need to be supplemented in various ways. On the one hand, they need to be "upgraded" to ensure that they can support various kinds of reasoning — since reasoning depends on the natural logic of semantics, and, on the other hand, they need to be replicated for a much wider range of languages so that the great advances in the descriptions of the lexicogrammatical systems of a growing number of languages are gradually matched by semantic descriptions.

In addition, semantic accounts need to be developed to reflect the nature of semantics as an interlevel (e.g., Halliday 1973) — an interface to systems that operate outside language, both other social semiotic systems and what Michael Halliday and I in *Construing Experience through Meaning* (Halliday & Matthiessen 1999/2006) called "bio-semiotic systems", i.e., not only sensory systems, but also motor systems — sensorimotor systems. Here recent work by Adolfo García and his colleagues

can show the way, providing not only significant findings but also models of how to proceed (e.g., García & Ibáñez 2017; Trevisan & García 2019).

This last enterprise can be informed by the work that John Bateman started over a decade ago in Germany in the area of robotics working with people from Vortex (e.g., Bateman & Farrar 2005). If you try to link the kind of model that Michael Halliday and I sketched in *Construing Experience through Meaning* to other models of the same domains of experience, as John Bateman has done for the domain of space, you need to link the language-based model of space (the ontology of space as Bateman put it) to a model of space that can enable robots to navigate around space — modelling space in such a way that the robots can interpret it visually and use the model to move around it. In other words, the semantic model of space in language needs to be such that it can interface with the model of space designed to meet sensorimotor demands. The linguistic model of space must be able to construe visual information as meaning and it needs to be able to enact linguistic meaning as motor programmes. This view is comparable to that of "grounded cognition" and the "hub and spoke model" of meanings in semantics referred to briefly above. Bateman's line of research happens to involve robotic systems rather than human systems, but we can learn a great deal about demands on semantics as an interlevel form. At PolyU, we had discussions with researchers in geoinformatics about joint projects, but we did not succeed in attracting research funds. At the same time, we have carried on with linguistic research into the construal of our experience of space in different registers where space figures prominently: Abhishek Kashyap and I have published a number of papers on our exploratory research (Matthiessen 2015b; Matthiessen & Kashyap 2014; Kashyap & Matthiessen 2017, 2019).

6.10.6   Areas and Institutional Settings

We can also think about areas that need work — theoretical, descriptive, applied — in terms of institutions, and the settings or sites that relate to different areas of language in context. Systemic Functional Linguistics has, of course, been developed in such a way that it provides us with the resources for identifying what kinds of work is needed in different institutional settings — then planning it and carrying it out. Here we can still benefit from Malinowski's (1944) focus on institutions as the primary isolates of culture, from his conception of cultures as aggregates of institutions, and of course from more recent contributions such as Turner's (1997) account of institutional order. Some institutions have been part of the long-term programme of research and application from the start, institutions of education being a key example; but institutions have kept being added through the decades. There are institutions moving across cultures and languages, like translation and interpreting, which have been there for a long time. Much more work needs to be done, and can be done. Healthcare communication and the forensic contexts are also examples (e.g., Matthiessen 2013). The forensic area is one where not only discourse analysis, but also phonology and phonetics (graphology and graphetics) are involved. Other

institutions, like marketing and advertising, can also be involved because branding is so important nowadays (see Esterina Nervino's 2018 thesis that links SFL to this area). In these areas, there are interactions with the professional, which are very important.[12]

6.10.7   Areas of Engineering and Societal Significance

I would love to see a return to **computational modelling** because you can do things with computational models in terms of important applications and, because in certain intellectual contexts, it is only when you are forced to do computational modelling that you can really think through things theoretically. Computational modelling tends to be undervalued. I would also love to see this at a metalevel, like Wu Canzhong's (2000) and Mick O'Donnell's (1994) work. There is so much that can be done here. The software tools in corpus linguistics come out of language (or linguistics) department, and tend to be one-person efforts, which is a constraint (cf. McEnery & Hardie 2012: 43). But you cannot push the boundaries unless you get teams of people together in teams as in computational linguistics and Natural Language Processing. We should be part of that. I have the notion of a workbench for linguists, providing broad-range support for doing linguistics. I have tried to simulate with the suite of FileMaker Pro databases. Very interesting work can be done on so many fronts (see Sect. 7.7).

What seems very urgent now is **ecolinguistics**, which draws on Halliday (1990). Huang Guowen (e.g., 2016) is supporting the development of this at South China Agricultural University. That is what critical linguistics and critical discourse analysis have been engaged more with on the political stage. But we have to do this in a much more effective way so that those engaged in the work do not simply "preach to the converted" but rather provide strong and robust evidence that can influence people who are not yet convinced. At the same time, the framework has to be developed to the point where it can be introduced to pupils and students in the educational systems, empowering them as discourse analysts. This relates directly to the next topic.

There is also the scourge of **the post-truth society**. What do we do about this? How can people lie publicly — and visibly in everybody's view — and get away with it? How do we understand this? How do we analyse it? How can we give people the tools to resist it? Citizens and journalists can make positive contributions, but there

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[12] If you examine different disciplinary boundaries, you can see that the roles you can play vary considerably. In some boundary areas, you can go in as an amateur; but in others, you really need to develop expertise across the boundary areas. One of the reasons for the really phenomenal success in educational linguistics was this: professional teachers came from education and did a PhD in linguistics, so they really became bimetalingual. Similarly, in computational linguistics, John Bateman is bimetalingual in computer science and linguistics, but that has been relatively rare. In some areas, you can come in from linguistics being a bit of an amateur, although you will have to learn to dialogue with the experts across the border; but if you move into something like neuroscience, you really need to have the professional expertise there.

can also be fake news. New semiotic technology can enable us to reveal patterns indicative of problems with the quality of discourse in large volumes of discourse — big data in the form of large fixed corpora or flow-through monitor corpora. Based on low-level patterns accessible through automated analysis, we can get some diagnostic indication of problems with veracity, bias, discrimination, and other current problem areas.[13] We can finally have the microscope or the telescope in linguistics, and we should use that opportunity.

But this changes the conditions for meaning-making, and we must think of ways to get at fake news, the Disneyfication of discourse and the post-truth societies. There is no shortage of work that we need to undertake. Of course, that always links back to education. That means we try to turn this into something to be put in the hands of everybody. In the early 1980s, Bill Mann had the notion of turning artificial intelligence into something that everybody could do in their garage (in a society less dominated by cars than the US, one might choose another place, of course!). In a real sense, that is now actually happening, including the continued development of high-level programming languages appropriate to such tasks and protocols for collective cumulative developments. (Our doctoral students may now undertake to learn Python to do their own programming work.) But by the same token, we need the same conception of equipping people linguistically so that they can deal with the complexities of the phase of human history that we find ourselves in.

Currently, although quite a few systemic functional **descriptions** of a fairly wide range of languages have been produced since the 1990s, there is an urgent need for descriptions of languages that have not yet been described in systemic functional terms and also for expansions of the descriptions that have already been produced, often involving the move from lexicogrammatical descriptions to include semantic accounts. Interestingly — but not surprisingly, there have arguably been more contributions in the last couple of decades to multimodal studies than to multilingual ones: multimodality has become very fashionable, and it is so much easier to do investigations of multimodality than of multilinguality[14] because the fact reminds that the most complex semiotic system ever evolved is language. So, please engage with different languages! Communities around the world urgently need appliable descriptions of their languages — descriptions that can serve as resources in education, healthcare, local media and administration, and many other community activities where the local modes of meaning are central to the living of everyday life. This takes us back to

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[13] One important example is the work by Linus Ng in his final year project in our department at PolyU. He compiled a corpus of Brexit debates leading up to the referendum and used LIWC (http://liwc.wpengine.com) try to identify possible lies. He produced a report entitled "'Let's deal with this big fat lie once and for all': A linguistic analysis of inaccurate claims in four Brexit debates", and he presented part of his results at ESFLC at the University of Salamanca.

[14] I realize that this may come across as provocative, and I don't normally try to be provocative; but one way of getting a sense of what I'm suggesting is to contrast the task of describing a language that has not yet been described, at least not in anything approaching a comprehensive way based on text in context, with the task of describing a semiotic system other than language that has not yet been given adequate descriptive attention. The two are certainly not mutually exclusive; it makes sense to imagine future studies where linguists turn to the task of describing a "new" language and pay attention to accompanying semiotic systems in face-to-face interaction from the start.

the discussion of "commitments". The attempt to produce comprehensive descriptions of different languages spoken around the world is a field-oriented commitment, but the effort to make the description appliable as a resource for the community of speakers is a tenor-oriented commitment. When we reach this level of insight into what linguistics can do, we have gone far beyond the notion that the question "How does the natural language user work?" lies at the centre of functional linguistics — as I have argued, it does not, and also far beyond the "cognitive commitment".

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Chapter 7
Systemic Functional Language Typology

Abstract In this chapter, we first discuss Christian Matthiessen's interest in studying language typology. Then we characterize systemic functional typology, relate language typology to its neighboring areas, recommend Christian Matthiessen's descriptions of Akan lexicogrammar and phonology and investigate the way of interpreting Joseph Greenberg's work from the perspective of Systemic Functional Linguistics. We also provide some background information of Christian Matthiessen's work and introduce some computational tools for studies in language typology. Finally, we examine some challenges for systemic functional typology, and suggest some ways of moving forward.

7.1 Introduction

Language typology is a sub-discipline of linguistics that aims to describe and explain the common characteristics and diversity of the languages of the world. In one sense, it is the study of universals in human languages—not in the sense of the pursuit of universal innate principles of language as is done in generative linguistics but in a sense of identifying generalizations and motifs across languages through empirical analyses of the descriptions of large samples of languages. In another sense, it has to do with the classification of linguistic systems such as tense and aspect systems, voice systems and mood systems. Language typology as a field of study was given a significant boost by the work of the American linguist Joseph H. Greenberg, starting from the mid-1960s (see Greenberg 1966); but earlier work has been significant in SFL, including Firth's perspective on language universals and contributions from the Prague School (see Sgall 1995), e.g., Mathesius's notion of characterology and Trubetzkoy's work on phonology.

Currently, the research on typology is pursued within the framework of several linguistic traditions, including West Coast Functionalism, cognitive linguistics, construction grammar, Functional Discourse Grammar and Role and Reference Grammar. Although language typology has been one consideration in the development of SFL theory, there has been little empirical research on language typology

using SFL. Christian Matthiessen has been a leading scholar in motivating work in systemic functional typology through the development of resources and theoretical maps and through the supervision and involvement in the description of the grammars of several languages. SFL, with its text-based, meaning-oriented and paradigmatic approach as well as its commitment to social responsibility, has enormous prospects in filling research gaps in language typology. In this interview, we get to understand Christian Matthiessen's personal interests in this field of research, the nature of language typology in systemic functional terms, his contributions to the field, the challenges involved in doing language typology in SFL and the way forward (see Caffarel, Martin & Matthiessen 2004: 1–61; Teruya et al. 2007; Mwinlaaru & Xuan 2016; Teruya & Matthiessen 2015; Kashyap 2019 for overviews of systemic functional language typology).

7.2   A Keen Interest in Language Typology

Isaac Mwinlaaru: In your early career in linguistics, your research focussed on descriptions of English, computational linguistics and modelling the systems of English grammar computationally for text generation purposes. How did you come to work on language typology?

Christian Matthiessen: That actually started back in Sweden. Part of the background was my interest in various languages (see Fig. 7.1).

Through my pre-university school years, I studied as many languages as the curriculum would allow me to — English, then German, then French (and along the line we got a dash of Danish). English was obligatory, and then there was a choice between German and French, and although I would have loved to study Spanish, it was not an option at the time. When I started at Lund University, I was keen to learn various non-Indo-European languages as well as doing linguistics. I looked around to find what non-Indo-European languages were being offered by the university. It turned out that they only offered Hebrew, Arabic and Mandarin — Japanese was added a few years later. I chose Arabic for a variety of non-linguistic reasons, but my choice was also informed by linguistic considerations — or rather, ill-informed. I found the complexity of the Chinese graphology (the writing system) daunting. Also I thought I had no ear for music, and I knew that Chinese was a tone language, so I chose Modern Standard Arabic — but there were positive considerations as well, including the fact that the Arab world was much closer to Sweden than China.

As it happened, if I had chosen Mandarin Chinese, I would have been closer to Michael Halliday in terms of linguistic experience, and also to Sandy Thompson — my Ph.D. supervisor, who was also an expert in Mandarin Chinese (e.g., Li & Thompson 1981). And I ended up living first in Los Angeles and then in Sydney, both cities with large and vibrant populations of Chinese speakers. If I had studied Mandarin Chinese, I would probably have been in an environment where I could

Fig. 7.1 Christian Matthiessen's early interest in language

continue with it.[1] Years after I had made my decision in the mid-1970s, I told Michael Halliday about my reasoning and choice. He laughed and said: "Phew, the tones of Mandarin. Arabic is much harder to pronounce because of the pharyngeals". In any case, I did study Arabic, and it has been fascinating. If one starts from Swedish, or English, one will meet a similar obvious challenge with Mandarin and Arabic — learning lexis (cf. Halliday 1978). Since I was studying linguistics, I had great help in learning Arabic phonology and grammar, but apart from the linguistic insights into Arabic derivational patterns, memorizing Arabic lexical items was a struggle without help from cognates.

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[1] However, I have derived great insights and benefits from my study of Arabic, including but not limited to the exposure to the fascinating word grammar — the morphology (cf. Matthiessen 2015a), and of course learning both the graphology and the phonology. During trips to Morocco, UAE, Qatar, Tunisia and Lebanon, I have not been able to converse in Arabic or even eavesdrop because I studied Modern Standard Arabic rather than one of the regional varieties, but I have been able to decode in part public signs. For example, I was amused when I decoded the name of a bank on a walk around Dubai, and realized that it was simply a transliteration of HSBC (Hong Kong and Shanghai Banking Corporation). And I have been lucky enough to supervise one PhD project on Arabic, Mohamed Ali Bardi's (2008) penetrating systemic functional description of Modern Standard Arabic.

More generally, from the point of view of a language learner, different languages will turn out to be easy or difficult to learn in many different ways and probably during different phases of the language learning process. I spent one semester in the late 1980s trying to learn Mandarin as part of the Sydney University extension programme, but I was working full time and just didn't have enough time to do the homework. Still, I did get a good sense of the different challenges involved in trying to learn (Modern Standard) Arabic or Mandarin — and by then I had the experience of trying to learn Zulu as a language learner at UCLA for a year, and of picking up aspects of Akan as a linguist during a year of fieldwork studies. (And I had flat mates from India, from one of whom I learned a tiny little bit of Telugu.)

That was part of my interest in languages. Then in my last semester at Lund University as an undergraduate student, I applied for a scholarship to study at one of the University of California campuses. I chose UCLA because its Linguistics Department had great strength in the area of typology, and in those days, they had a very good representation of languages spoken in Africa. My established interest in different languages was further strengthened when Bengt Sigurd hired me as a visiting lecturer at Lund University to teach a course on language comparison, contrast and typology in the first half of 1979. So I really had to engage with some of the literature to master it enough to teach it especially since I was still an undergraduate student teaching other undergraduate students! Actually, one of my teachers of Arabic had enough confidence in me to audit the course. In any case, UCLA turned out to be the right choice, although I remember Bertil Malmberg, who had written a very supportive letter of reference for me when I applied for the scholarship, expressed surprise that I had not chosen UCB rather than UCLA.

In other words, my interest in a wide variety of languages goes back to my days in Sweden. It is also due to my mother because she grew up in Husum, Germany, learning German, with a German father and a Swedish mother. I grew up with Swedish. My mother did not want to teach me German for various reasons. One reason was that the misguided notion of "halvspråkighet", semilingualism, was around (see e.g., Martin-Jones & Romaine 1986). Also, she still had a sort of visceral reaction against the Germany of the 1930s and the 1940s. When still a teenager, she went to England as an au pair and learnt fluent English, and then when the authorities refused to give her a visa after three years of her working there, she moved to Sweden, sponsored by her mother, who had returned after her husband died in the late 1920s. So she learned Swedish, and she was also interested in various other languages. She knew French and Spanish. She studied Italian. She studied Russian in the 1950s because, during that phase of the Cold War, it seemed the Soviet Union might invade Sweden and she decided she had better be prepared. Before I came along, she had spent vacations hiking in the mountains of northern Sweden during the war years when it was not possible to travel outside Sweden. There she met people from the Saami community, so she also studied Saami, which, like Finnish, belongs to the Uralic language family; I have inherited her Saami grammar. I was also exposed to the language in its speech community. One summer (1967), she took me to the far north of Sweden, and I met some of the people who spoke this language, including

Saamiland (Lapland) — Sápmi, July 1967

Fig. 7.2 Christian Matthiessen in Saamiland in July 1967

her guide from the war years, who had a "kåta"[2] he rented to tourists, which we stayed in (see Fig. 7.2). So the multilingual orientation was also part of my childhood in various ways even though I grew up as a monolingual speaker of Swedish. I do remember the excitement of my mother reading letters from relatives in Germany and Mexico, translating them for me into Swedish — my first experience with sight translation.

As already mentioned, when I was studying Arabic, I found that I had a lot of help from doing linguistics and having some knowledge of typology. It helped me with the phonology and phonetics of Arabic. It helped me with the grammar (though the lexical part of the lexicogrammar was still difficult). Then I went to UCLA, and one key reason was precisely the range of languages on offer there. In the first quarter, I took a course surveying African languages with Bill Welmers (cf. Welmers 1973). I had one year of Zulu with a Zulu poet from South Africa (a refugee from the apartheid regime). Then I did a course that had typological content with Sandy Thomson, based on the transitivity hypothesis she and Paul Hopper had formulated (Hopper & Thompson 1980, 1982), and another on topic continuity with Talmy Givón (cf. Givón 1983), both at my home institution, UCLA,[3] and also one on tense

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[2] See https://en.wikipedia.org/wiki/Goahti.

[3] Both courses were designed to encourage us students to write publishable papers. Paul Hopper and Sandy Thompson had a contract with Academic Press to publish an edited volume based on

at University of Southern California with Bernard Comrie. Fellow Ph.D. students worked on a variety of languages (e.g., Weber's 1989 description of Quechua — very interesting for a variety of reasons, including his description of the system of evidentiality).

The faculty of Linguistics at UCLA had expertise in languages from a number of different language families and linguistic areas, including languages from different families in Africa, Sino-Tibetan, indigenous languages from North America and Austronesian. Their descriptive work enriched the academic environment significantly, and because of this orientation of the Department, many visitors came to give talks on a wide range of languages. This descriptive expertise in the Department was also of great value to us as Ph.D. students because one of the obligatory courses was a one-year course on field methods, and every year a different language was on offer. I started my field methods course with Pamela Munro, who specialized in American Indian languages — especially Mohawk languages (e.g., Munro 2016); but then I just did not have the time that year, and next time around, it turned out to be Akan with Paul Schachter. That proved to be fortuitous for various reasons, including the fact that it prepared me to appreciate your work on Dagaare (Mwinlaaru 2017) and Ernest Akerejola's (2007) on Oko. And happily you and I are now pursuing a long-term research programme into Akan and other languages spoken in West Africa (cf. Mwinlaaru, Matthiessen & Akerejola 2018).

I was very keen to do my Ph.D. in the area of language description and typology, but after my one-year scholarship came to an end, I had to earn my keep, so I got a job in the second half of my first year as a research linguist in computational linguistics (see Chap. 5). That was interesting and exciting, and it involved Systemic

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their account of transitivity, and a number of us submitted versions of our term papers for possible inclusion. Then Sandy told us that the publisher wanted predominantly established scholars, and I think only one of our group of students got included, David Gil — a brilliant scholar. I had produced a study of the transitivity of nominalizations in a wide range of different languages, couched in terms of the Hopper-Thompson transitivity hypothesis, and Paul Hopper very generously read it and kindly encouraged me to submit it to a journal. Unfortunately, I didn't get the time to revise it for journal publication — by then I was busy supporting my studies through work in computational linguistics at the Information Sciences Institute and had no spare time. I would like to return to this investigation because my findings really resonated with Michael Halliday's account of nominalization as part of grammatical metaphor. Talmy Givón edited a volume on "topic continuity in discourse" based on his course, and kindly invited us to contribute. So I submitted a study of topic continuity in Swedish discourse; but based on the insights I had gained from SFL, I felt that Givón's low-level measure had to be supplemented by references to observations about the semantic organization of discourse. I don't think this fit his conception of the book and it probably clashed with the other contributions, so he rejected my contribution, but in a kind way, saying that similar languages were already represented in the book. As it happens, I was staying with Ruqaiya and Michael for an extended period and had given Talmy Givón their home address, so when his letter of rejection arrived, I was in the best of intellectual environments and took it in stride. I don't think I told Michael at the time, but at some point, he told me that when Givón was putting together his volume on syntax and discourse (Givón 1979a), Michael had sent him his paper on modes of meaning and modes of expression that was later published as Halliday (1979) after Talmy Givón had rejected it. Givón told Michael that there was nothing new in it and that Michael had written about this topic before. This was so obviously completely wrong, and Michael and I laughed about us both having been rejected by Givón.

Functional Linguistics (SFL) quite centrally, so I could draw on and develop my expertise in SFL (see Sects. 1.1 and 2.3). But I still wanted to do something on language typology or language description. Over time, I found that it was difficult to be pulled in these two different directions. I spent so much time thinking about modelling language in a computational context, and there was not enough time to pursue language description (apart from English, the focus of the computational linguistics project) and language typology. So, in the end, I did my Ph.D. in text generation and computational modelling but very much focussed on SFL. In some sense, I always regretted that. It was really because of financial considerations. If I had a job doing something in language typology, I could have done that. Michael Halliday also had to give up on his first choice of Ph.D. topic, which would have drawn on his research on Wang Li's project. In his case, it was because there was nobody who could supervise the research area he had chosen, but his Ph.D. research turned out to be fascinating, and led to a unique and insightful publication (Halliday 1959).

But I always maintained the interest and continued to try to keep up with the literature. One burning passion and interest was to suggest how language typology could be empowered by systemic functional theory and based on systemic functional descriptions, in some sense complementing the empirical work in typology done by Joseph Greenberg's (e.g., 1978, 2005) generation and other scholars with the same orientation to typology like Bernard Comrie (e.g., 1981). Part of my engagement with that area was a vision of a unified area of multilingual studies (Matthiessen, Teruya & Wu 2008) because I could see how different regions of multilingual studies, like language typology and translations studies, could be in much more mutually beneficial interaction. See further below, and Fig. 7.3.

7.3   Features of Systemic Functional Typology

Isaac Mwinlaaru: So, we see that your multilingual orientation grew gradually from different experiences since childhood and that linguistics at UCLA has played a crucial role in your knowledge of a range of languages and your interest in language typology and description. The connection you make between language typology and translation studies and your suggestion of the cover term multilingual studies are both revealing. We will return to this point later for more details. But how would you characterize systemic functional typology? What are its methodologies and its goals?

Christian Matthiessen: It certainly includes the empirical orientation to the development of typology in the tradition of the Prague School, as in the work by Trubetzkoy (1939) — see Sgall (1995). Systemic functional typology really got a boost from the work in the Prague School and in particular Trubetzkoy's (e.g., 1939) work

cline of instantiation
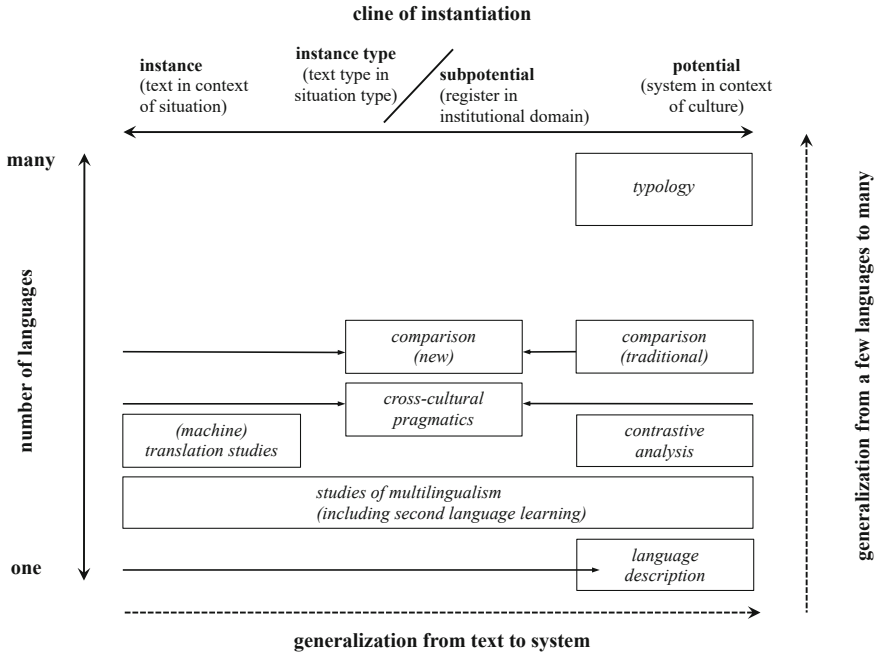
Fig. 7.3 Phenomenal realm explored in multilingual studies in terms of the number of languages in focus and in terms of the cline of instantiation

on phonology early on and Mathesius's (e.g., 1928, 1975) notion of the **characterology** of a language, which tried to profile the characteristics of language against the background of what is possible in language (developed by Halliday 2014).

It is also resonant with the orientation of Greenberg (e.g., 1978), Comrie (e.g., 1981), and other people behind *The World Atlas of Language Structures* (*WALS*) (Haspelmath et al. 2005),[4] also combined with the West Coast Functionalism in functional language typology, which adds discourse-based studies to the empirical foundation (early formative studies including Hopper & Thompson 1980, 1982; Hopper 1982; Givón 1983; Haiman & Thompson 1989; Ochs, Schegloff & Thompson 1996) rather than the kind of approach to universals formulated as part of Chomsky's (e.g., 1965) conception of universal grammar, or "UG". Given the relatively small number of languages that have been described in reasonably comprehensive terms and the variety and number of languages still spoken, it behooves us to be very humble in making claims about language universals. Evans & Levinson (2009) detail the "myth of language universals", stressing the kind of diversity that has been explored and respected in the "descriptivist" tradition and also in SFL, note (p. 445): "Once the full diversity is taken into account, the UG approach becomes quite implausible — we would need to stuff the child's mind with principles appropriate to thousands of

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[4] For the online version, see https://wals.info.

languages working on distinct structural principles". Compare Firth's "universalist fallacy" immediately below.

Michael Halliday was very interested in language typology (see e.g., Halliday 1959–60, 2014), and this resonated with the work by a few other linguists from the same generation contributing to SFL (Ellis 1966, 1987).[5] We discussed language typology on a number of occasions, and drafted a long section on it for one of our books (still in manuscript form now). One of Michael's unpublished projects focussed on areal typology; it was a survey that included 37 features of languages in Southeast Asia, which had not been documented anywhere. He had determined that there is a continuum from northern China (the Mongolian territory) down through the southern part of China into Vietnam, and then further south down the Malay Peninsula to include Austronesian languages. He wanted to show the 37 parameters and the values over the region and space. He might have lost the manuscript, because he had moved around quite a bit like me.

This project adopted a **parametric approach** to language typology, and this approach is part of the response to your question about what is specific about systemic functional typology. Parameters are systems, or are derived from systems, and this foregrounds the central point that systemic functional typology is precisely systemic and not only structural. This systemic orientation was foreshadowed in Trubetzkoy's (e.g., 1939) work because he was interested in phoneme systems. Obviously, you can do that more easily for phonology. So, a number of insights, like the distinction between privative versus equipollents systems, go back to that time. Then you combine it with the notion that you base typological generalizations on comprehensive descriptions of particular languages; you do not do the "tour-around-the-world-in-80-days" of languages by looking just at some areas like causation and just ripping that out of a number of languages.

Systemic functional typology should also be based on comprehensive descriptions of particular languages. When you talk about how systems of evidentiality, causation or time are organized in one or another language, you do this not just with a fragment that you collect, i.e., "tour around the world in 80 days", but you can actually reason about such systems within the overall system that they are part of — you approach them ecologically within their own systemic environment.

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[5] But it is important to note that Halliday took the same critical view as Firth of positing language universals based mostly on a few languages rather than on extensive empirical evidence from a wide variety of languages. Firth (1957: 21) writes: "Linguists are only just beginning to realize the dangers and pitfalls of 'personification' of categories as universal categories. [footnote omitted] There is a constant need to beware of such bogus philosophizing in linguistics. There is always the danger that the use of traditional grammatical terms with reference to a wide variety of languages may be taken to imply a secret belief in universal grammar. Every analysis of a particular 'language' must of necessity determine the values of the ad hoc categories to which traditional names are given. [footnote omitted] What is here being sketched is *a general linguistic theory* applicable to *particular linguistic descriptions*, not *a theory of universals* for *general linguistic description*. Though it is found convenient to employ the words *noun, verb, pronoun, particle*, for example, it must not be assumed that in all languages, nouns and verbs *are to be found* as the universalists might express it. It has been held that in some Melanesian languages the noun–verb distinction is unnecessary. The 'universalist' fallacy is constantly with us".

Evidentiality is an interesting example of what kind of access we have to information about languages. According to *WALS*, there are no evidential markers in African languages but there are lots of them in the Americas (de Haan 2013). Having obtained this information from *WALS*, you have to pull back, so that you can examine what happens in texts belonging to registers where there is a premium on the validity of information. You can say that in terms of what you are doing in the creation of meaning in text or discourse used in languages spoken in West Africa (e.g., Akan), people still do it in some way even though it may not be selected for obligatorily as in Quechua text (cf. Weber 1989). You may not have to do it obligatorily for each declarative clause by indicating whether you have seen it yourself or heard yourself from somebody else, etc. But the resources are still there.

That goes back to the difference between doing systemic functional typology and Functional Grammar typology in Simon Dik's (e.g., 1978) sense (see Chap. 4). In 1986, I asked Simon Dik if, using Functional Grammar, they were working on comprehensive descriptions of any language. The answer at that time was no. So, the assumption was: you do typology, but not based on comprehensive descriptions of particular languages. The same was true at the time of Role and Reference Grammar (e.g., Foley & Van Valin 1984).

If you develop a comprehensive functional description, it has to be text-based. That introduces another interesting challenge in systemic functional typology. Because in some sense, you feel this tension between working on typology of the systems that make up the overall meaning potential of a language and what translators would do with an individual text. My view was: it would be very productive to bring the two together in some way, and to make typology more sensitive to text (cf. Matthiessen, Teruya & Wu 2008). Then, the mid-way station is doing language contrastive work based on texts in particular registers, as argued and demonstrated by Teich (1999), Lavid (2000) and Murcia-Bielsa (2000). These are part of the ingredients that would enable SFL to make a distinctive contribution to language typology (cf. Teruya & Matthiessen 2015).

Let me summarize my sketchy characterization of systemic functional language typology by means of Table 7.1. The table sets out the central considerations where we can compare systemic functional language typology with conceptions that have been adopted in various other approaches.

The typology of particular systems is supported by our development of multilingual system networks to represent multilingual meaning potentials (e.g., Bateman, Matthiessen & Zeng 1999; Matthiessen 2018). They can be used as a basis for comparing and contrasting a few languages, and then extended towards language typology. A simple multilingual system network for Chinese, English and Korean is presented in Fig. 7.4. It shows that unlike Chinese and English, Korean has a system simultaneous with the system of mood type — a system for enacting the tenor of relationship between speaker and addressee. This system network is, of course, simply a comparison of a couple of languages, but by expanding such multilingual system networks, we can move towards typological generalizations, as illustrated by Teruya et al. (2007).

Table 7.1 Key considerations in the systemic functional approach to language typology

Consideration	SFL	Rather than
Language	As resource for making meaning in context, represented as a system where structural patterns are specified by means of realization statements	Inventory of rules or structural patterns
Typology of	Particular systems (with inherent probabilities) such as mood, modality/evidentiality, tense/aspect, person, number	Whole languages
Axial orientation	Both systemic (paradigmatic) and syntagmatic (structural), but with priority given to systemic patterns	Primarily structural (like "word order typology")
Focus on	Content plane (metafunctionally organized), so lexicogrammar (clause and below) in relation to semantics (text and below)	Morphosyntax: syntax, morphology
Conception of semogenesis	Semanticization and lexicogrammaticalization	Grammaticalization
Descriptive base	Comprehensive descriptions of particular languages, each interpreted in its own right	Anything from reference grammars to descriptive sketches
Nature of data	Primary: texts (from a range of registers) in contexts; secondary: descriptions of systems behind texts	Descriptions of structural patterns, and of systems

In addition, in this very condensed sketch, I would like to mention one other property of SFL that is crucial in exploring typological variation in particular linguistic systems around the languages of the world — the notion of **complementarity**. Halliday (2008) illuminates the notion in general, and when we look across languages we find complementary models achieving semantically comparable grammatical tasks, e.g., the logical and experiential models for construing our experience of the flow of events, the transitive and ergative models for construing a quantum of change in our experience of the flow of events, the tense and aspect models for construing process time and the modality and evidentiality models for assessing the validity of information. Languages vary considerably in how they divide the labour between such complementary models — one or the other model may dominate in a given language or they may appear in a mixture; and this variation is also related to register variation. For example, in Eurasia, some variant of the tense model tends to be foregrounded in the West and some variant of the aspect model in the East, and in between we find mixtures in Slavic languages and Indo-Aryan languages, and also in Semitic languages, or at least in Modern Standard Arabic in my interpretation, as shown in Fig. 7.5.
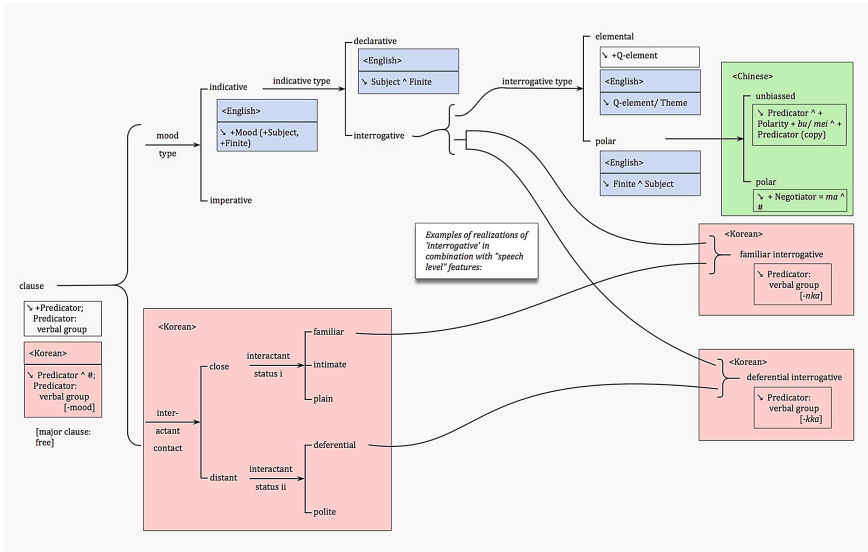
Fig. 7.4 Multilingual system network of MOOD in Chinese, English and Korean

7.4 Relating Language Typology with Its Neighboring Areas

Isaac Mwinlaaru: Now going back to our previous point, in the paper you published with Kazuhiro Teruya and Wu Canzhong (Matthiessen, Teruya & Wu 2008), you use the term "multilingual studies" to locate language typology, translation studies, language comparison and language description, and just now you have made this point here. Can you shed more insights on the motivation for putting language typology and translation studies together as one field of research?

Christian Matthiessen: They are all connected, which I came to realize through my own personal history, being exposed to different facets of "**multilingual studies**" on different occasions. I had of course the experience over a number of years as an L2 learner, and starting during my undergraduate studies, I had learnt about Contrastive Analysis (e.g., reading Lado 1957). I also learnt about the role of the comparative method in historical linguistics, and we have already discussed my engagement with language typology. In addition, I had early exposure to translation as a phenomenon, and then later to the study and teaching of translation.

   Part of my exposure to translation was the family experience with my half-brother (Tryggve Emond), half sister-in-law (Ingrid Emond) and half niece (Vibeke Emond). They were all doing translation professionally. My half-brother and half sister-in-law translated from French, English and Italian into Swedish, and my half niece from Japanese and Italian into Swedish. So, it was part of my thinking of what translation

phenomena of experience

process unfolding through time

... construed on the model of

location

bounding

construal in grammar — systems

construed as tense:
location: unfolding of process located relative to 'now' of speaking (or other reference time)

tense → past / present / future

construed as aspect:
bounding: unfolding of process as bounded / unbounded in time

aspect → perfective / imperfective

(Modern Standard) Arabic

aspect → perfective / imperfective

tense → past / present / future

realizational resources in grammar

verb form: perfective / imperfective
auxiliary: *ka:na*
particle (affix): *sawfa (sa-)*; *qad*

Fig. 7.5 A schematic representation of the complementarity of the tense and aspect models in English, Chinese and Modern Standard Arabic

means; we would on occasion spend a good deal of time discussing points of translation, and Tryggve would give me books he had translated, like Joseph Conrad's *The End of the Tether* — providing me with interesting material for translation study.

Also, there has been a long tradition in SFL of engagement with translation. A particular kick start for me came with the colloquium that Erich Steiner and his long-distance colleague Colin Yallop organized at ISFC (International Systemic Functional Congress) in Cardiff in 1998. They invited me to make a contribution, and the results of that was a paper called "The Environments of Translation" (Matthiessen 2001). What I wanted to do was to build on Ian Catford's (1965) monograph, which was couched in terms of scale-&-category theory (Halliday 1961) and bring it up to date in terms of SFL. I was saying that we can now say a great deal more with the further developments in systemic functional theory.

Then I got involved in contributing to an MA programme in translation and interpreting at Macquarie University both on the campus of Macquarie and over a period of several years, also twice a year delivering this course in intensive mode over two

weeks at Korea University in Seoul. That was an additional welcome and very productive opportunity to think about translation. Since Korean was one of the languages involved, I read up on descriptions of Korean, and constructed for myself a systemic functional sketch of Korean so that I could draw on it in my teaching at Korea University; for example, see the sketchy outline of the metafunctional organization of the Korean clause in Fig. 7.6. I also had some Ph.D. students working on translation even back at Macquarie University, in particular Mira Kim. She had been instrumental
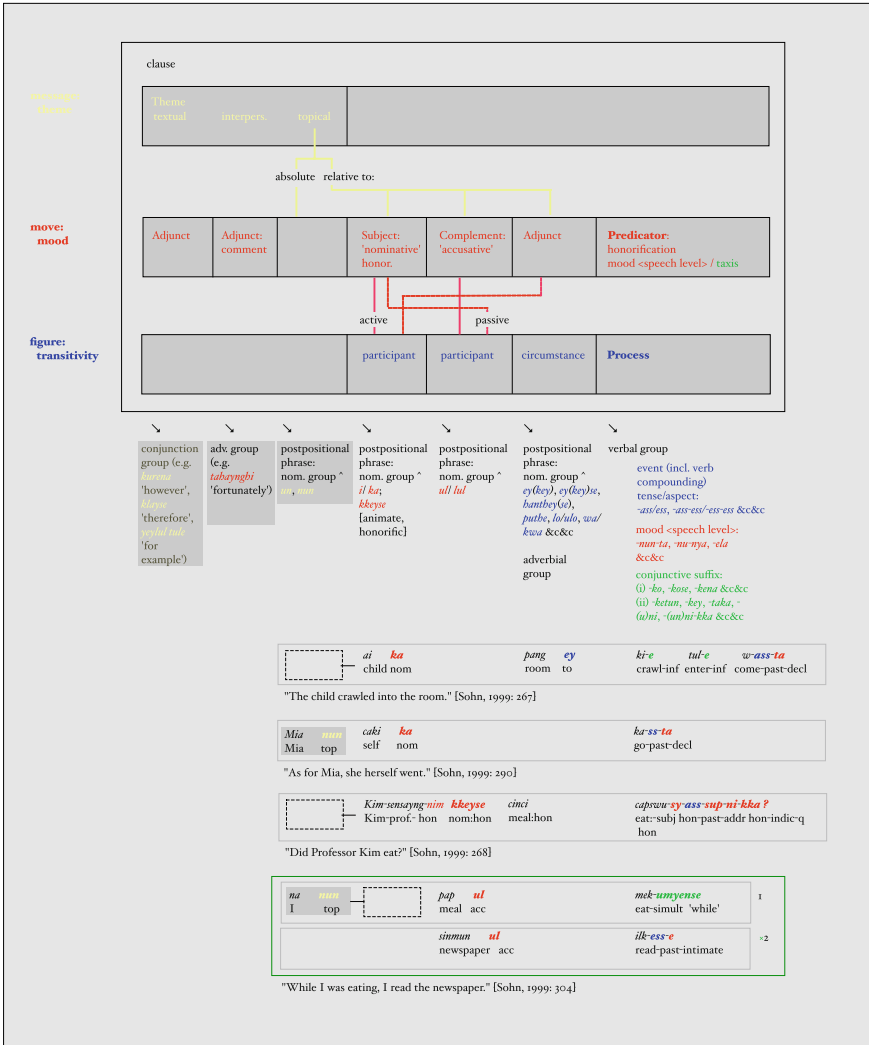
Fig. 7.6 Simplified outline of metafunctional structure of the Korean clause as message (THEME), move (MOOD) and figure (TRANSITIVITY), with examples of realizations

in the development of systemic functional translation investigations of translation between English and Korean, and, not surprisingly, she found that in order to do her research into translation, she needed to develop a systemic functional description of the textual grammar of the Korean clause. Thus her Ph.D. thesis, Kim (2007), includes both a descriptive foundation and a text-based translation study. This is a powerful illustration of the need for the conception of multilingual studies.

As typically happens in academia, over time, realms of related phenomena become separated institutionally in the study of these phenomena; this process has been characterized by David Bohm (1979) as the **fragmentation of knowledge**. So instead of a **unified field of multilingual studies** — unified in terms of the realm of multilingual phenomena, there emerged distinct communities of people who did not talk with one another and most probably did not know each other's work. Also, there were people engaging with the practical aspects of teaching and learning foreign languages. (Naturally, there were particular conditions that contributed to the separation of different areas of multilingual studies; for example, the "translation method" was largely abandoned in L2 education and as scholars transformed translation studies into distinct disciplines, the focus often moved away from linguistics. Similarly, even empirical language typology tended to be system-oriented rather than text-oriented, creating a significant distance to the focus on instances in translation studies.)

As noted above, I was fortunate enough to have had opportunities to be involved in one way or another in these different activities and academic communities, and I saw that there were crucial connections. I felt that we needed a conception of multilingual studies and it would be helpful if one could point out how they were related to one another (see Fig. 7.3), and I realized that the dimensions of systemic functional theory could be used to locate them in relation to one another. So, if there are contributions coming from translation studies, there are potential implications for typology and the other way round. That was behind the attempt to say that there could be a lot of benefit from bringing these distinct communities to dialogue with each other.

7.5   Description of Akan Lexicogrammar and Phonology

Isaac Mwinlaaru: Two of your earliest works on language typology and description of languages other than English were papers on Akan lexicogrammar and phonology (Matthiessen 1987a; b). Why were you interested in describing Akan?

Christian Matthiessen: Again, I was very lucky that there was a focus on African languages in Linguistics at UCLA with experts on different languages, as I mentioned earlier. I took advantage of this even in my first year, 1979–1980, and tried to learn Zulu, taking a course with a faculty member who was a poet rather than a linguist. The focus on African languages was part of the descriptive and typological orientation of the department. These people both did work on the languages and taught the languages, which provided a very rich and interesting experience. We also had people

like Peter Ladefoged (e.g., 1964), who was very prominent in phonetics and worked on phonetics of languages spoken in Africa. We had Paul Schachter, who had worked on languages from West Africa, and the Philippines like Tagalog (e.g., Schachter & Otanes 1972).

When I had the time to do the field methods course, which ran over a full academic year, the language offered in 1985 was Akan. It could have easily been an Austronesian language or a native American language from the Mohawk family because that was what Pamela Munro (e.g., Hinton & Munro 1998; Munro 2016) was specialized in. But it turned out to be Akan, which was good for me since I had already tried to learn another Niger-Congo language, Zulu, in 1979–1980. The language consultant we had was a very nice fellow. He was doing his Ph.D. in geology at UCLA, and he was very engaging. At the end of the year, he said to me: "Could I have a copy of what you've written up to read? Unlike what the others are doing, I can understand what you are doing". The others were all asking questions derived from the current version of generative grammar, essentially Government and Binding (Chomsky 1981).[6] At the time, it was fashionable to explore long-distance movements, so they were torturing him in the sessions, trying to painfully extract examples of long-distance movements from him. In contrast, what I did was to try and establish paradigms and to get something out of the texts that he produced either for me in our weekly one-on-one sessions or in class with everybody. So, it was a very different kind of exercise.

I would have liked to keep up my research into the systemic functional description of Akan, but I was pulled in other directions by my work as a research linguist on the "Penman project". However, it did give me a first opportunity to get more deeply into a language from a non-Indo-European family. Of course when I started studying Arabic in the mid-1970s, I tried to sketch aspects of the language by myself, but it was not that kind of sustained effort, and it was not with field methods. So, I thoroughly enjoyed my close encounter with Akan. I was lucky that it was Akan, because while the phonology did present challenges, it was perhaps more transparent than that of a Mohawk language or some other North American indigenous language.

I was quite surprised at how far I could take systemic functional theory of grammar — or perhaps I should say, how far it could take me, as I developed my description of Akan lexicogrammar, drawing on descriptions of other languages that I had seen. For phonology, I had not seen any example of systemic functional phonology at all. I had seen Firthian phonology (e.g., Firth 1948; Palmer 1970), but while this phonology was prosodic and polysystemic, it was not that systemic in the sense of systemic phonology (cf. Matthiessen 2021). And while Michael Halliday's (e.g., 1963, 1967, 1970) work on intonation was already around and included system networks as part of his description of intonation, I didn't find any models for dealing with non-prosodic articulatory phonology (I had not seen two potentially relevant unpublished Ph.D. theses, couched largely in terms of scale-&-category theory, viz. Barnwell 1969

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[6] A couple of years before the field methods I course, Paul Schachter gave a course going through Chomsky's GB lectures, which I audited — fairly heavy going for him and us students. At the end of the course, I wondered aloud if the effort had been worth it.

and Mock 1969, nor had I seen Bamgboṣe 1966). I had to work out not only Akan phonology myself but also the general conception of systemic functional phonology. What systems and system networks would there be? What would the terms (features) in phonological systems be (where would I take them from)? What could you do with the system network?

One of the striking things was that so many rule systems had been introduced in generative phonology, with Chomsky and Halle (1968) being a beautifully Baroque example of how far the rule metaphor could be taken, recapturing the history of English in the rule system. But many of these rules had to do with correcting initial over-specifications, under-specifications and so on in the structure.[7] What I found was that you could do away with this. The system network will allow you to treat phonology in this way. It will allow you to specify just as much as you need at a certain point in delicacy so that you would never need to over-specify it. You also have the notion of the rank scale; hence, you do not need to spread properties syntagmatically from one phoneme to another, but you can specify patterns at a higher rank. You can analyse and describe a phonological domain other than that of the phoneme — that of a syllable or a foot or something higher. That was part of the Firthian inheritance; while there was no rank scale, there was the notion of prosody. That was then rediscovered in autosegmental phonology within the generative tradition (e.g., Goldsmith 1979) — for a Firthian perspective on these developments, see Henderson (1987).

One huge difference between systemic phonology and generative phonology was the systemic functional conception of phonology as the sounding resources of a language — the sounding potential, what speakers "can sound". In generative phonology, you took the phonological representation of an item from a dictionary, and then you had rule systems that work on these. In contrast, if you conceptualize the phonology of a language as a **sounding potential**, you should be able to generate possible sound patterns in the language, even if they had not been lexicalized. I did not treat phonology as fragments of the phonological structure that had to pass through rules, but I really tried to interpret phonology as the sounding resources of the language, representing it as a potential in its own right.

I still think that makes a great deal of sense. There naturally remains much work to be done, including thinking about the syllabic tones, how you interpret phenomena that phonologists had done with more generative approaches representing downstep, and how you specify that in terms of successive differentiations of high tones or low tones. It would certainly help if I had known the language as a speaker.

Isaac Mwinlaaru: But it is still very insightful. I drew a lot of inspiration from it in my own work on Dagaare (Mwinlaaru 2017).

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[7] Quite a few of the findings in generative phonology at the time had to do with rule ordering, like the distinction between feeding and bleeding rules. But such findings had to do with the representational system — the rule system, and anyone familiar with computer programming would already have known about such ordering. See also Sect. 5.1. This is one of the many reasons why it is fundamentally important to distinguish between the level of theory and the level of representation in our metalanguages.

Christian Matthiessen: Then it has certainly served one of its key purposes.

Isaac Mwinlaaru: I read a paper by McGregor (1992) on systemic phonology in the contributions in Tench (1992). He has the systemic representation of phoneme in Gooniyandi, which I find insightful. Does this have any connection with the overall resource of phonological systems you just mentioned?

Christian Matthiessen: McGregor (1992), who had done his Ph.D. with Michael Walsh in Halliday's Department of Linguistics, sketches the phonological rank scale of Gooniyandi (tone group > word > syllable > phoneme, reserving judgement as to whether the rhythmic unit of the foot also needs to be posited), and he then focusses on the phonological system network of the phoneme. While he discusses issues of phonotactics and prosodies as well, the focus is on the phoneme. Consequently, he has to posit phonotactic adjustments, like rules of neutralization. This suggests a need to shift more of the descriptive burden to the systemic description of the syllable. In my own work on Akan, I found it absolutely essential to develop the systemic description of the syllable alongside the description of the phoneme. Various phenomena that have often been treated phonemically are in fact more insightfully interpreted in terms of syllables in the first instance. The syllable serves as the gateway between the prosodic and articulatory domains of phonology (cf. Matthiessen 2021), and articulatorily it is a gesture that reflected in phonotactic patterns (and auditorily it is a wave of sonority). Similarly, to me, the descriptions of English phonology in that book (Tench 1992) were too phoneme-oriented (e.g., Young 1992). The truly amazing chapter in the book was Michael Halliday's (1992a) on the Peking syllable — an account that had been in preparation since the late 1940s, a real *tour de force.*

Michael Halliday said to me that he had asked Paul Tench: "Why don't you include Christian's work on Akan?" Paul Tench said: "Oh, I had approached him, and he didn't respond". That was a misunderstanding because there was a very bright honours student in linguistics at Sydney University who had done his honours thesis on systemic phonology. He was the one that Paul Tench had written to, not me. Obviously, he had not responded at all. If Paul had written to me, I would have sent him something on Akan phonology, which might have been included in that book. It would have been a unique contribution, e.g., shedding light on the division of descriptive labour between the syllable and the phoneme and illustrating the use of phonological features (systemic terms) derived from the work by Catford (1977).

Isaac Mwinlaaru: We still expect the works on Akan to be published (Matthiessen 1987a, b).

Christian Matthiessen: They will be included in one of the eight volumes of my collected works. Since I produced those descriptive sketches of Akan, much more has been published on Akan phonology (not only by Nick Clements but also by Florence Abena Dolphyne, a scholar who came to talk to us in the mid-1980s), and also on lexicogrammar; and of course I have read and learnt more. So there is the temptation to revise and update the manuscripts, but I think it is better to follow Michael Halliday's approach to his collected works. He resisted updating or correcting the papers being collected because that would have interfered with the

sense of the stages of the development of theory and description. It is better to add some commentary, and say that these are the shortcomings or claims, that the work was written 35 years ago.

In collaboration with you, I look forward to doing much more work on Akan, including work grounded in the manual analysis of texts from key registers and computational analysis of corpora of Akan — and of course of other languages of the area: Mwinlaaru, Matthiessen and Akerejola (2018) is an early installment in this long-term research programme.

When I started working on Akan, it was interesting to develop an understanding of the complementarity of primary and secondary sources. In terms of secondary sources, I found it useful to go back to Christaller's (1875, 1881) work from the nineteenth century; he was one of the missionary linguists of that era and he was unusually good as a linguist. So, I had access to his grammar and his dictionary and drew on them. It was fascinating how he had approached various regions of the grammar — perhaps in particular, the catenations of verbs, now generally called "serial verb constructions". Understandably, he was restricted by the traditional framework rather than empowered by it; and one might argue that he did not crack the code of "serial verb constructions". However, in his work on Ewe, Westermann (1907) got further, and his account can perhaps be seen as the beginning of descriptions of "serial verb constructions" qua "serial verb constructions" in languages spoken in West Africa (cf. Stewart 1963). Around the mid-1980s, linguists still tended to approach such constructions through constituency analysis (although new insights were coming in, as in the work by Carol Lord — cf. the later publication by her, Lord 1993); but I was fortunate enough to have seen Michael Halliday's work on verbal group complexing (and on clause complexing) and to have discussed it extensively with him. So I interpreted "serial verb constructions" simply in terms of verbal group complexing ("serial verb complexes"), exploring the range of logico-semantic types in terms of which one verbal group can be linked to another. This is, of course, related to another project you and I are engaged in, together with Pattama Patpong, involving the description and comparison of "serial verb constructions" in West Africa and East and South-East Asia. As always in Systemic Functional Linguistics, this means not just inventorying and characterizing structural patterns, but rather exploring the lexicogrammatical resources for construing our experience of the flow of events (since this is where serial verb complexes are deployed in lexicogrammars of different languages in the first instance).

7.6   Interpreting Greenberg's Work on Word Order in Systemic Functional Terms

Isaac Mwinlaaru: In "Descriptive Motifs and Generalizations" (Matthiessen 2004), you related to Joseph Greenberg's (e.g., 1978, 2005) work on typology by managing the metafunctional complexity of languages, by looking around different languages

and by looking at the functional pressure on languages that simultaneously realized different meanings. How would you position this particular orientation towards word order to Greenberg's work in the 1960s?

Christian Matthiessen: Greenberg was certainly inspired by the Prague School. His work was important both in providing a kind of framework for stating "universals" and also in the empirical approach, which was taken further in the Stanford project — the four volumes that he co-edited with Ferguson and Moravcsik (Greenberg, Ferguson & Moravcsik 1978). (It was one of my first purchases after arriving at UCLA. I still have the series of four volumes, and they are pretty battered by now.)

Part of my thinking was concerned with how to translate his way of stating "universals" into systemic functional terms. One answer was: there were classic universals that were really universals about elaboration of systems; that is, even if they are stated in structural terms, the principles behind them are systemic. Thus: if a language has a certain systemic distinction, it also has another systemic distinction. You could take examples from a number of systems. I have given one simplified example in Fig. 7.7 based on Comrie & Keenan's "Noun Phrase Accessibility Hierarchy" (Keenan & Comrie 1977; Comrie & Keenan 1979). So, I felt that it was clarifying to state this in terms of systems. Some of these are conditional universals, and that carries forward to the use of all the information in *WALS*.

One area where there has been a great deal of typological work is "word order", which really means the sequence of the elements of a unit — a clause, a clause nexus, a group, a phrase or a word. Greenberg (1966) had a sample of 30 languages. Since his pioneering study, one of the linguists who has extended this work considerably is Matthew Dryer (e.g., 1980, 2011, 2013), whose findings based on large samples are also represented in *WALS*. Some of the patterns that Greenberg found have been confirmed, and others have been disconfirmed (Givón 1979b). Interestingly, one of the patterns that has been disconfirmed is the correlation between the sequence of elements in the clause (e.g., whether it is VO or OV) and that of the nominal group (AN, NA) (see e.g., Givón 1979a). In larger samples, it turned out that there was no correlation with sequence of elements in the nominal group. This finding makes excellent sense in systemic functional terms because clauses and groups manifest different kinds of structure. In the earlier terms from the American descriptive tradition, due to Bloomfield (1933), the nominal group is endocentric and the clause is exocentric. In contrast, if you have phrases, you would expect a correlation between VO and pre-positional phrases, and OV and post-positional phrases, as long as the
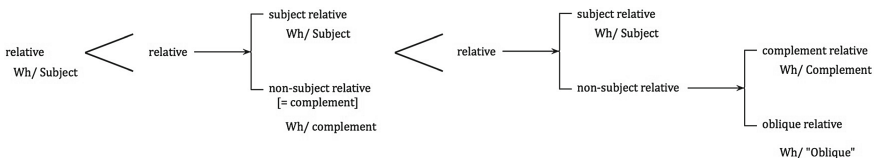
Fig. 7.7 The "Noun Phrase Accessibility Hierarchy" for relative clauses re-interpreted systemically as an ordering of the elaboration of systems across languages (simplified version)

pre-position and post-position is verbal in nature (as it is in Chinese) as opposed to being nominal in nature (as it is in Arabic).

As we know and as noted above, there is now a vast literature on "word order typology", including both typological overviews and explanations like Jack Hawkins's (e.g., 1980, 1983) processing model. So this is certainly an area where continuing contributions in the form of systematic reviews and meta-analysis will be very helpful. But then you have to take a step back, problematize the accounts and say: "What is the S in this SVO or SOV?" The S is problematic, but I suspect that the V and the O may be more robust; in any case, we must continue to problematize the S, drawing on contributions made already in the 1970s (see e.g., Keenan 1976a, b and other contributions in Li 1976). So, the S may not be comparable across languages. Importantly, Paul Schachter (1976, 1977, 1996) explored the status of "Subject" in Philippine languages back in the 1970s (cf. also Schachter & Otanes 1972). He suggested that there are actually different clusters of properties rather than a unified notion of "subject" of the kind we inherited from traditional grammar (but of course traditional grammarians always had difficulties with the notion of "subject" ever since it had been introduced by the Modistae in the late Middle Ages). This diversification of the notion of "subject" is of course precisely what we would expect based on Halliday's theory of metafunction originally articulated in the 1960s. Schachter's clusters of properties can be related to the textual and experiential metafunctions; but this still leaves the interpersonal contribution largely out of the picture. This has been addressed head-on by Jim Martin in his work on Tagalog (e.g., Martin 1990, 2004).

When you begin to do more text-based work, as Talmy Givón (e.g., 1983, 1984) did, you will see a more nuanced picture emerge, but that is what systemic functional (metafunctional) theory would invite you to do in the first place. So, I think there is a great deal of work to be done based on text in context. One way to manage the complexity of the task is to do it register by register, which also has the advantage that different registers will put different patterns of meaning and of wording at risk. For a long time, narratives were quite fashionable; they provided a good source of information in the study of tense-aspect systems (e.g., Hopper 1922). It was not surprising because narratives were easy to collect if you do field work, and they were comparable across cultures and languages (cf. Rose 2005) — a way of making sense of our experience of the world, as also emphasized by Susan Greenfield, the neuroscientist. Later, as the focus of investigation shifted, casual conversation became fashionable (e.g., Ochs, Schegloff & Thompson 1996).

However, in doing comparative and typological research grounded in text in contexts — texts selected from particular registers, you cannot make it really comprehensive and exhaustive simply because of time constraints, but you can get an interesting registerial range — beyond narrative and conversation, where you can reason about the different ways in which the grammatical resources have been deployed. The discursive "pie of fortune" with the fields of activity that work towards some sense of characterizing context would be useful for language description (see Fig. 7.8; see also Matthiessen 2015b). Then you could say: "I collect this kind of text because it is likely to shed light on certain features of the grammar." A few of such features are indicated in Fig. 7.9. I realized the significance of taking account of register in language descrip-

Fig. 7.8   The eight primary fields of activity and their sub-types

tion and typology a long time ago, one important source of insight was Benveniste's (1966) description of the two tense systems of French — which in a sense turned out to be the tip of the temporal-registerial iceberg, as shown by Caffarel (1992) in an article she wrote based on a BA Honours thesis she did with me. Somewhat later I realized that this applies more generally, crucially to the complementarity of transitive and ergative transitivity models in different languages, illustrated for English in Matthiessen (1995) and then also in the third and fourth editions of *IFG* (Sect. 5.8).

To make further progress, typologically oriented studies of "word order" will need to expand the database of texts sampled from different registers[8] and the analysis will have to differentiate textual, interpersonal, experiential and logical factors influencing recurrent patterns. I thought it important to sort out what generalizations were textual

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[8] The registerial composition of the database is important. For example, while Modern Standard Arabic is usually characterized as "VSO" (or "VO", with the S structurally implicit) and this certainly applies to narrative text within contexts of recreating. In Fig. 7.9, taxonomic texts within contexts of expounding knowledge about classes of phenomena are likely to have "SVO" as the unmarked pattern for good thematic reasons.
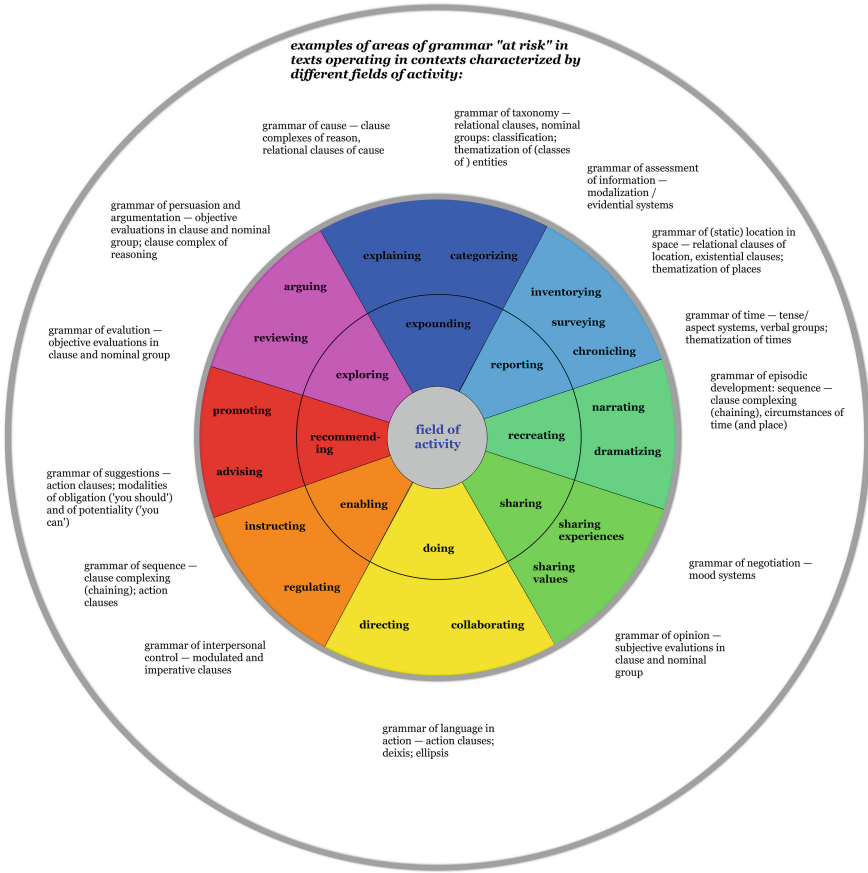
Fig. 7.9 Example of areas of grammar "at risk" in texts operating in contexts characterized by different fields of activity

in nature and which ones were interpersonal in nature. I arrived at this sense from an interpersonal point of view by considering the clause as a move (an interactive event). In addition to textual significance, it was reasonable to expect variations across languages at the beginning of the clause and at the end of the clause, which had interpersonal significance for the clause as an interactive move in dialogue — where interactants start the move, the overture to the clause, and where they are about to hand over, the finale of the clause, i.e., the turn transitional points in the terminology of conversation analysis.

Once we have distinguished textual and interpersonal factors, we can consider how languages vary in the ways they map the metafunctions onto one another. For example, English has a very clear mapping in terms of syntagmatic realization between the interpersonal and the textual at the beginning of a clause. In a language like Japanese, there is very clear mapping between the interpersonal and the logical

at the end of a clause through clause-final markers (which may move over time from one metafunction to another). You can then take a step back and relate that to the so-called word order typology. What attracts the verb towards the end of the clause? What are the reasons? What attracts the verb towards the beginning of the clause? What are the reasons for a verb to be placed medially in the clause, splitting participants into two syntagmatic domains? (If the verb is in initial or final position, it will not divide participants into pre-verbal and post-verbal zones.) Then you can begin to ask: Does this correlate with ways of marking participants, like case marking systems? By asking various questions, you begin to see the systemic network of connections, i.e., what Michael Halliday (e.g., 1992b, 1996) calls "a syndrome of features". What I have just sketched here is a partial version of what I present in Matthiessen (2004), which is already a very condensed account. These observations will be fleshed out in the multilingual version of *IFG* I mentioned above.

7.7 Some Background of "Descriptive Motifs and Generalizations"

Isaac Mwinlaaru: That is insightful. How long did it take you to write "Descriptive Motifs and Generalizations" (Matthiessen 2004)? The depth, insights and comprehensiveness in the chapter are fascinating.

Christian Matthiessen: I am glad if it has had an impact and can be used in that way. I have been working on the multilingual version of *IFG* on and off for a long time (see Sect. 4.4), where I will provide more details and explore the issues with more text-based exemplification, and in writing "Descriptive Motifs and Generalizations", I drew on my findings from that long-term project. But I don't know how long it took me to write up the chapter itself. As always, the work was interleaved with other activities, including teaching — and preparing the whole book manuscript for submission to the publisher, which took me a very long time because the contributions included in the volume were not homogeneous at all in terms of document styles.

Isaac Mwinlaaru: It might have been some years then.

Christian Matthiessen: Yes. There was a workshop on language typology held at the University of Sydney in 1996 (see Caffarel, Martin & Matthiessen 2004a). Originally, the work on Chinese was presented by Zeng Licheng — a Ph.D. student of mine and a native speaker of Mandarin who was extremely good. He was more into computational linguistics (Zeng 1996), and we worked on multilingual text generation (e.g., Bateman et al. 1991; Bateman, Matthiessen & Zeng 1999). He had thought about the description of Chinese, but once he graduated, he went into the commercial world. That chapter on Chinese was then written up by Michael

Halliday, and edited with Edward McDonald (Halliday & McDonald 2004), who had also worked on Mandarin for his Ph.D. with me (McDonald 1998) and among other contributions added text examples to the chapter. In that workshop, Michael Halliday presented on the typology of prosody in languages, and he had worked out a cline from pure tone languages to pure intonation languages — related to the degree to which tone is deployed locally with the syllable as the domain or over a more extensive prosodic domain, i.e., the tone group (or intonation unit). His contribution was very interesting and foundational for future research, but it was not included as a separate chapter in the book since we organized it in terms of languages except for the intro and outro chapters (Caffarel, Martin & Matthiessen 2004b).

7.8   Computational Tools for Research on Language Typology

Isaac Mwinlaaru: In one of the end notes in Matthiessen (2004: 658), you indicated that you have developed a typological database for storing information about a range of languages. What is the nature of this database?

Christian Matthiessen: It was something I created in FileMaker Pro. I was trying to add information that I could extract from a wide range of sources. When I began this development, *WALS* was not even on the horizon (at least not on my horizon at that time). The typologically relevant information can be added from publications, like the four volumes by Greenberg (Greenberg, Ferguson & Moravcsik 1978), anything that has been discussed systemic functionally, information you could get from the Ethnologue and genetic information about the language family. Because I felt that I needed a way of being able to search for different patterns, my vision was of a typological database. It was like *WALS*, but I wanted to include texts, both "raw" and annotated. With *WALS*, you can download their different parameters with the values, which is type-formatted in Excel or other software. I actually got the point of importing them into FileMaker Pro database, so it is certainly something to return to.

Isaac Mwinlaaru: What computational tools do you find useful in building data or developing models for research in language typology?

Christian Matthiessen: My reasons for developing the FileMaker Pro database were as follows: on the one hand, there was so much information available in the literature, but it was dispersed throughout many sources and needed to be compiled into a unified resource; on the other hand, while many people would turn to some form of spreadsheet, I always felt that a relational database was more powerful, so I tried in a very amateurish way to develop a variety of databases with FileMaker Pro. When I was at Macquarie, it was great to work with Wu Canzhong, who had more computational expertise and knew how to write scripts in FileMaker Pro. We

did not work on the typological database together, but we worked on SysAm, which includes the SysFan database for text analysis (Wu 2000, 2009).

7.9 Challenges for Systemic Functional Typology

Isaac Mwinlaaru: What challenges confront systemic functional typology?

Christian Matthiessen: There are a number of challenges relating to different aspects of doing systemic functional typology, including both **researchers** and **resources**. One challenge is to get students and scholars involved in the task of developing **comprehensive language descriptions**. Such descriptions are of tremendous value in their own right, particularly since systemic functional descriptions are designed to be appliable — to be resources for the community. But at the same time they are, of course, absolutely essential resources for the development of systemic functional typology. Such comprehensive systemic functional descriptions — text-based and meaning-oriented accounts complement narrower accounts focussed on some particular domain such as causation, tense-&-aspect, number, evidentiality — accounts that may not be part of comprehensive descriptions. Comprehensive descriptions allow us to reason about particular domains systemically, engaging in systems thinking (cf. Sect. 4.3).

Getting people involved has to do with both the tenor and field parameters of the context of research and scholarship. (1) In terms of **tenor**, they have to be invited to see the value of committing to the development of comprehensive language descriptions (cf. the discussion of "commitments" in Sect. 6.3). The increasing awareness of "language documentation" and of what I call "semodiversity" (and what has been called "glottodiversity") should help: systemic functional linguists have an amazingly high awareness of and commitment to what Halliday called "social accountability" (see Halliday 1984). Their sense of social accountability is strongly reflected in the contributions to e.g., educational linguistics, healthcare communication studies, forensic linguistics, ecolinguistics and critical linguistics (and the later separate development of Critical Discourse Analysis). Here there is likely to be a short to median term pay-off in terms of visible impact, either in terms of positive interventions and applications or in terms of debate within the communities and societies that the scholars are active in. But appliable language documentation is likely to necessitate a longer-term perspective and an orientation towards communities that are less immediately visible in terms of the current metrics attached to considerations of impact.

(2) Now, even if students and scholars get highly motivated in terms of tenor considerations, they face another obstacle as they try to become involved and make substantial contributions. How do they gain access to expertise in the **fields of activity** that underpin the development of comprehensive language descriptions? There are now a great many programmes around the world that are informed by SFL

— many of which, perhaps the majority of which, are at masters level. However, these programmes are geared towards fostering the skills needed for discourse analysis rather than the skills needed in language description. More generally, I would say that within the SFL community around the world, we know how to train discourse analysts — there are many supportive resources; but we have much less overall experience with training systemic functional language descriptivists. A number of us have, of course, learnt how to proceed with highly motivated and capable doctoral students; but here the model remains more of one-to-one apprenticeship and what I have in mind are teaching programmes that will enable students to become systemic functional descriptivists. I'm convinced such programmes can be developed, but we have to recognize that language description is a much harder task than discourse analysis (cf. Fig. 7.10).

Another challenge has to do with the data — more specifically, the sample size. Greenberg (1966: 74) had thirty-plus languages, and some of the contributions to Greenberg, Ferguson & Moravcsik (1978) are based on considerably larger samples. If you fast forward about a quarter of a century and look at *WALS*, the largest sample would be between 1,200 and 1,300 at least for grammatical "features". If you try to create a representative sample, it is not that easy. What do you take as consideration? What is the largest family in a language phylum that you can find? Do you go further down? What about areal considerations? There are lots of considerations, and linguists have suggested the number could be between 300 and 400. I mention this because the number of languages that have been described in systemic functional terms is much smaller, although the spread of language families is increasing. There
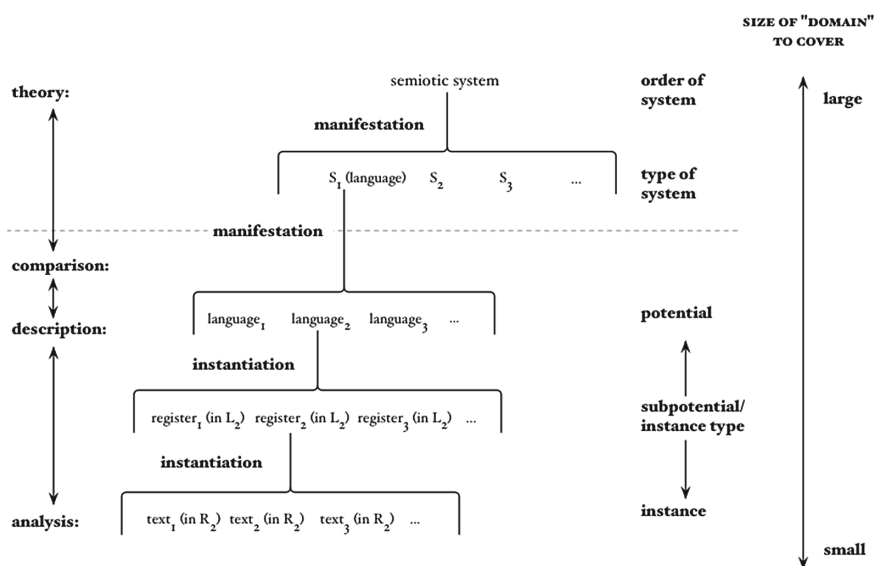
Fig. 7.10 Hierarchy of research activities in linguistics indicating how each activity feed into the other

are also gaps in that no indigenous language from the Americas have been described in systemic functional terms. I have met systemic functional linguists at Latin American conferences who have worked on some of these languages, but I am not aware of any comprehensive accounts that have been published. My approach has been to work with descriptions that are close enough to Systemic Functional Linguistics to include at least some systemically and functionally significant information. There are non-SFL descriptions that use texts and are in some sense functionally-oriented; hence, we can make some connections.[9] To develop such connections, we need to adopt a multi-pronged approach. Even if you look across different linguistic frameworks, the truly comprehensive descriptions are not even in the hundreds. While systemic functional descriptions have typically been developed to be comprehensive and rich enough to support extensive text analysis, this key linguistic task is in fact often not on the agenda of grammar writers outside SFL, even if they aim to produce reference grammars. Typologists have to make the most of whatever descriptive sources that are available to them; they will work with anything from reference grammars to field notes. Thus from the point of view of SFL, there are vast descriptive gaps, and the descriptive work ahead of us is both essential and exciting. But I have to come back to the challenge of getting systemic functionalists interested and actively involved.

Issues of this kind were part of my thinking as I proposed the hierarchy of challenges in linguistic activities (see Matthiessen 2013: 141), reproduced here as Fig. 7.10. Text analysis is at one end, and then there is language description of individual languages, language comparison and contrast, language typology, and theory on another end. It is not the only way of thinking about it, but one has to be realistic about the amount of work needed. It is also a way of pleading with people who have focussed only on text analysis (often with some kind of a critical stance) up to now to turn also to the task of language description because one cannot do language typology unless we seriously increase the descriptive database. I want to get these people to engage with the much harder task of actually describing languages that are in urgent need of description. A great many people from different backgrounds adopt critical stances, but only linguists with scientific expertise and technical training can resource communities of speakers by contributing appliable descriptions of their languages, thereby empowering both individual speakers and collective speech fellowships.

7.10　Ways Forward for Systemic Functional Typology

Isaac Mwinlaaru: Given these two challenges: (1) getting a critical mass of scholars and students to get involved in the description of languages in systemic functional terms and (2) the difficulty of students getting access to the expertise of systemic

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[9] To give just a couple of examples: the descriptions of Quechua by Weber (1989), of Mapuche by Smeets (2008) and of Tariana by Aikhenvald (2003) are reasonably comprehensive and have enabled me to interpret the grammars of these languages in systemic functional terms — up to a point since the descriptions are not in fact oriented towards the grammars of these languages as resources for making meaning, and are thus not paradigmatically oriented.

functional language description and typology, what are the ways forward for systemic functional typology? What advice will you give to young scholars working on or intending to work on systemic functional typology?

Christian Matthiessen: Going back to what you said before, I still think that it is important to pursue the long-term project of developing a typological database (which would include texts from different registers as well as systemic descriptions) that is informed by SFL and has information that can be used by systemic functional typologists and other scholars and practitioners involved in one way or another in what we call "multilingual studies". That will give us the systemic guidance when we approach the task of describing a "new" language.

Another related dream of mine is to have a kind of linguist's workbench — implemented computationally, providing linguists with both resources and tools. You may work on translation, language description or text analysis, but you will have this workbench where you can pull in the information already available. You will have, among other things, some analysis tools and description tools. A typological database would be part of such a workbench. Figure 7.11 shows the interface that Wu Canzhong and I have created to enable us to access a number of databases for "doing linguistics". (This does not include a database I have designed for supporting language typology.)

One of the challenges in doing language description and typology has to do with expanding the linguist's imagination, inviting her or him to go beyond what they have experienced or found documented in previous accounts. Arguably, the pursuit of language universals is all about constraining imagination, although some colleagues might argue that this is not a fair characterization. What we need are many more comprehensive, detailed and rich descriptions of a wide range of different languages. How could communities of users of a language evolve ways of achieving semiotic tasks of different kinds in different ways? How are they related to one another? A typological database should help with that.

If you look at *WALS* (though we should value it as a really amazing contribution), what they have not done is similar to what corpus linguists have done with tagging corpora, annotating them for systems or features. We should look at this relative to what would be included in a comprehensive metafunctional text-based description of a language. There is a fairly recent textbook on corpus linguistics by Tony McEnery and Andrew Hardie (2012). In this book, the authors say that it would be helpful to turn the features that Douglas Biber (e.g., 1995) chose to work with into some kind of a classification that is like a tree of features. This is actually remarkable because that conception of feature organization has been around since the 1960s in SFL. It is unfortunate that they make this suggestion without apparently being aware of the vast amount of work done in this area.

In a way, it is the same for typology. A major challenge is to get a clearer picture of what has been covered, what could be covered and what needs to be covered. I often feel that typologists do not think along such lines. Thus while *WALS* is a great contribution, it is important to recognize that only "features" that are relatively accessible in the documentation of fairly large numbers of languages are included,

Workbench environment for doing linguistics

Linguistics

SysFigures New

Linguists   SysGloss   SysBiblio   SysFigures

Projects

SysProject

Project: verbal

Project: projection

Project: RST realization

Systems

SysRef   SysVerb

SysAm

SysFan   Analysis Figures   Text Analysis

SysConc   Praat

Languages New

Languages   LanguageItems   LanguageExx   SysRef Ex.

TextArchive   TextTypology   Text (SysFan)   Scenes from films

Texts

Fig. 7.11 Prototype of a workbench for doing SFL

Table 7.2   Generalized function-rank matrix (focus on clause)

	ideational		interpersonal	textual
	logical	experiential		
clause	COMPLEXING	TRANSITIVITY	MOOD & POLARITY	THEME (& INFORMATION)
clause/group	PHASE & C TENSE	⇑ PHASE; TEMPORALITY: TENSE/ASPECT SPATIALITY [DIRECTIONAL & OTHER LOCATIVES] ⇓	⇑ MODAL ASSESSMENT: MODALITY [MODALIZATION] & MODALITY [MODALIZATION] / EVIDENTIALITY / PREDICTION/ATTITUDE; MODAL DISTANCE: HONORIFICATION, POLITENESS ⇓	⇑ VOICE; SWITCH REFERENCE ⇓
group (verbal)	(Realization within the domain of the verbal group in terms of auxiliaries, semi-auxiliaries, clitics, and verbal affixes.)			

leaving many areas unaccounted for (just as tools used in the analysis of corpora can only access relatively low-level patterns in text). The grammatical features that are included are all essentially seen "from below" in terms of Halliday's trinocular vision. For example, information about temporal systems is largely based on word grammar, as is information about evidentiality. A typologically informed generalized function-rank matrix would tell you what to probe (see Table 7.2),[10] e.g., the temporal systems of the language, the modelling of time in the process, tense system, aspect system, some kind of synthesis of the two or none of the above. Making that accessible in the way of going beyond the printed books would be tremendous.

The matrix involves two semiotic dimensions within the system of lexicogrammar, but, still staying within lexicogrammar, we can add one more dimension, the cline of delicacy extended from the grammatical zone of lexicogrammar to the lexical one. This will give us a three-dimensional space in terms of the common patterns of variation across languages, as shown diagrammatically in Fig. 7.12. Thus we can begin to identify recurrent patterns of variation as we examine comprehensive descriptions of different languages:

- **variation in terms of rank**: variation along the rank scale in the location of comparable systems and/or their structural realizations. This was of course first highlighted in the word-based typologies that emerged from the nineteenth century, but

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[10] The matrix in Table 7.2 is only an illustration. In a talk given at a typology symposium at Sydney University in 2018, *The shape of grammar(s)*, I discussed the project of producing such overviews that can serve as guides for both descriptivists and typologists, noting previous efforts such as Whorf's (1956: 126) "Language: plan and conception of arrangement". I analysed it systemic functionally and suggested how it can be revised and expanded.
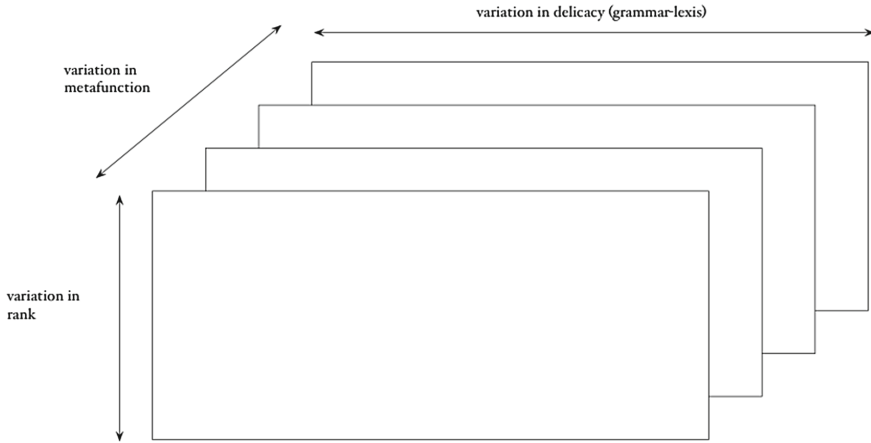
Fig. 7.12 Typological variation internal to lexicogrammar viewed in terms of three semiotic dimensions

this kind of variation is more general — it concerned the division of lexicogrammatical labour across the ranks of the overall system (see e.g., Matthiessen 2004: Table 10.6 on p. 556, Table 10.8 on p. 565). For example, while the system of MOOD tends to be a clause rank system, realizations of terms of system ('declarative' / 'interrogative' / 'imperative' and so on) vary in location across languages from clause rank (e.g., interpersonal particles, as in many languages e.g., [clause initial] Irish, Arabic, Zulu, Chamorro, Hopi and [clause final] Kannada, Thai, Cantonese, Japanese, Choctaw; intonation contours, as in Spanish, Tuareg, Pashto, Paiwan, Nahuatl or other prosodic features) via group rank (e.g., interpersonal clitics) to word rank (affixes, e.g., verbal affixes in Abkhaz, Chuvash, Korean, Wappo, Greenlandic).

- **variation in terms of metafunction**: variation along the spectrum of metafunctions, perhaps in particular logical / experiential and logical / textual. The primary sources of variation are most likely between the logical and experiential modes of construing experience: languages vary considerably in how they rely and combine the logical modes and experiential mode in construing various domains of experience, e.g., time (cf. Fig. 7.5), motion and more generally the flow of events, the properties of entities. In the logical mode, a certain domain of experience is modelled as a chain, but in the experiential mode, it is modelled as a configuration. Consequently, the logical mode will involve paratactic and hypotactic complexes of units, but the experiential mode will tend towards delicate taxonomies: see e.g., Halliday & Matthiessen (1999/2006: Chap. 7), Matthiessen (2004, 2015c). This difference in division of labour between the logical and experiential modes of construal may also be related to the size of inventories of lexical verbs, ranging from around one hundred to thousands of verbs.
- **variation in terms of delicacy**: variation along the cline of delicacy from grammatical to lexical construction of meaning as wording. Looking across languages,

we find considerable variation in how they lexicogrammaticalize various areas of experiences and of interaction. For example, experience of sequence in time is highly grammaticalized in English by means of the system of tense, but not in Chinese, which operates with an aspect mode concerned with the boundedness of events in time and instead may lexicalize sequential relations. For this and other examples of differences between English and Chinese, see Halliday & Matthiessen (1999/2006: Chap. 7). Certain domains of experience have been studied cross-linguistically in considerable detail, our experience of motion through space being a prominent example. Differences have typically been discussed in terms of lexical differences, going back to Talmy's (1985) influential typological study of "lexicalization patterns", but they are in fact part of lexicogrammatical differences among languages and tend to apply more generally than only to the domain of motion (cf. Beavers, Levin & Thao 2010). This is easy to conceptualize and model if grammar and lexis are construed as a continuum of patterns of wording extended along the cline of delicacy. Grammars provide templates for different patterns of lexicalization across languages.

Variation within the three-dimensional space of lexicogrammatical resources shown in Fig. 7.12 also show up in processes of **lexicogrammaticalization**. They involve movement within the space, the dominant trends being from lexis towards grammar along the cline of delicacy, from clause (complex) towards word along the rank scale and from the experiential metafunction (logical and experiential) to the interpersonal and textual ones (see Matthiessen 1995). Since lexicogrammar is related directly to both semantics and phonology, there are considerations external to the space of variation shown in Fig. 7.12. For example, the move down the rank scale tends to be realized naturally by phonological reduction, and the move along the cline of delicacy from more to less delicate involves semantic generalization or "bleaching". If we also take the global semiotic dimension of instantiation into consideration, we can add quantitative observations and take register variation into consideration, noting that lexicogrammatical changes may be spearheaded by certain registers — like casual conversation (e.g., Halliday 2002) or scientific discourse (e.g., Halliday 1988). In short, while I focussed on the multidimensional space internal to lexicogrammar in Fig. 7.12, all semiotic dimensions in the overall "architecture" of language are relevant to considerations of typological variation across and within languages over time.

We urgently need typologically descriptive generalizations to support researchers developing language descriptions, which have been reinforced by presentations at recent conferences, including the International Conference on Functional Language Typology in Shanghai in December 2015 and the 43rd International Systemic Functional Congress (ISFC) in Indonesia in July 2016. Some people have examined the descriptive tradition relating to the language of their choice (if that language has any descriptions), and they have looked at examples from English. But it is also important that they become more aware of the typological literature. One way of doing that is to interpret it in terms of SFL, carefully noting what areas have been covered against the background of a multilingually comprehensive map. I think that a multilingual

version of *IFG* of the kind I am working on would be a contribution, one that distills key points from many descriptions; but of course one has to go back to the original work as well.

Let me add a footnote drawn from my personal history (see also Matthiessen, Wang & Ma 2019). In one of my conversations with Michael Halliday, I learned that he had accepted a position at the University of California, Santa Cruz. This was the time when he was a linguist nomad, moving around the world in the first half of the 1970s. He was all set to work on an American Indian language spoken in California, but then he reconsidered based on what he learned about the local academic interpersonal situation. In the end, he went to Sydney. But what would the history of language description and language typology have been if he had worked in the 1970s on a language like an American Indian language, producing a rich comprehensive meaning-oriented and text-based description of the language as a resource? That could have been quite extraordinary and amazing.

Isaac Mwinlaaru: But he wrote some of his most important contributions in the 1980s in Australia, especially those on linguistics in general.

Christian Matthiessen: Yes. Of course there were various positive reasons for these pioneering contributions, in particular in the area that has come to be known as educational linguistics. In a way, there was no space for SFL in Australia in the areas of language description and language typology, because they were already in the institutional domain established by Dixon and others, starting when Dixon was at Australian National University (ANU) (e.g., Dixon 1972, 1977, 1980).

Still, linguists drawing on SFL did make important contributions, at the early stage, in particular Bill McGregor (e.g., 1990, 1992), who drew on SFL and did his Ph.D. on Gooniyandi. He was supervised by Michael Walsh, who came out of Dixon's ANU tradition; but Michael was very open and receptive to ideas from SFL — as was Bill, of course. A bit over a decade later, David Rose produced a very insightful systemic functional description of Western Desert, under Jim Martin's guidance (e.g., Rose 2001). In general, it has not been easy for systemic functional linguists producing superb descriptions of an increasingly wide range of languages to get published, to get recognized, and even to get referred to. It has sometimes struck me as curious that linguists who are genuinely committed to valuing and celebrating **linguistic diversity** do not take the next step of valuing and celebrating **metalinguistic diversity**. But, to round off on a positive note, conditions are changing. There is much more awareness now of the language description and typology programme informed by SFL. More and more people are interested in it, perhaps first in the meaning-oriented text-based part but then hopefully also in the systemic part. The systemic part is, of course, also inherent in the orientation to meaning; but linguists brought up with a focus on structure seem to find it hard to find their way into the more implicate order of language.

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Chapter 8
Translation as a Linguistic Process

Abstract In this chapter, we first elaborate on the significance of applying Systemic Functional Linguistics (SFL) to translation studies. We also discuss various topics on SFL and translation, including the linguistic turn in translation studies, differences between prescriptive and descriptive studies in translation and Matthiessen's own works on translation, i.e. the environments of translation and metafunctional translation shift. Finally, some directions for future research are suggested.

8.1   Introduction

Translation studies can be approached as one academic area where Systemic Functional Linguistics has been used both as a framework for research and as a pedagogical resource. In this interview, Christian Matthiessen discusses translation as a linguistic process, emphasizing the need for a powerful linguistic theory to be able to account for the translation process. He tells us about the significance of doing translation studies from the point of view of SFL. We examine different environments of translation using the theoretical dimensions of SFL: instantiation, metafunction, stratification, axis, delicacy and rank. We also engage him on some of his publications on translation studies, identifying important concepts that are useful to scholars, professionals and students interested in translation as a practice and as a field of study.

8.2 Significance of Studying Translation from the Perspective of Systemic Functional Linguistics

Bo Wang: What insights can be derived from studying translation in the perspective of Systemic Functional Linguistics (SFL)? What is the significance of these studies?

Christian Matthiessen: It is about how you conceive of translation. No matter how you look at it, translation is in the first instance a **linguistic process**. Given that it is a linguistic process (you might also say it is a metalinguistic process), you would certainly want to illuminate it as a linguistic process.[1] So, you look around for a theory of language that has something to say about language as potential, language as process and language as instance. Language, very broadly conceived, is **language in context**; so the theory needs to include not only language but also context. What you want is a holistic theory to engage with language, with comprehensive descriptions that will allow you to reason about both the source language and the target language.

That is a very general characterization of what kind of theory we want to bring to the linguistic study of translation. SFL is a holistic theory of language in context, and it has certain key features. One is that it is **meaning-oriented**, which immediately implicates text since text is a unit of meaning functioning in context. Descriptions of particular languages have been **text-based** from the beginning. Meaning-oriented means paying attention to different kinds of meaning in terms of how they are unified and balanced in text.

Also importantly, SFL is oriented towards the paradigmatic axis. SFL conceives of language as **a resource organized as choices in meaning** available to speakers and listeners (or writers and readers). These are the key elements you need in reasoning about translation and understanding translation in terms of the meaning potential of the source language, the meaning potential of the target language, recreating meanings in the course of translation and recreating meanings in context.

Even if nothing had been done in terms of an SFL perspective on translation, these features of SFL would present it as an interesting candidate — a candidate with a powerful potential for engaging with translation. But in fact, significant parts of this potential have been actualized: there is a long history going back to the 1950s first on machine translation and then on human translation. Halliday (1956) worked on the notion of a mechanical thesaurus as an alternative to the traditional conception of a dictionary in the context of the machine translation project he was part of in the 1950s, which was directed by Margaret Masterman (see also Sect. 5.1). The notion of mechanical thesaurus means thinking of language as a resource organized paradigmatically, not as a list of entries, but rather as a network of alternatives in lexical meaning.

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[1] Cf. our discussion in Chap. 6 of the interpretation of 4th-order phenomena in either semiotic or cognitive terms. Here the semiotic interpretation is foregrounded: translation is a semiotic process, more specifically a linguistic one (although we allow for translation involving semiotic systems other than language).

In the 1960s, there were relevant early SFL discussions of translation as a general phenomenon, e.g., Halliday (1962), Halliday, McIntosh & Strevens (1964) and, importantly, Catford (1965); see also Ellis (1966). In the last twenty-five years, there have been more scholars doing research by using SFL in translator training in different parts of the world and constantly with different language pairs. That has been successful.

Apart from SFL, you can look around for other linguistic theories of language that have the potential to shed light on translation and whether some work has actually been done. In this vein, it is fair to say that the only major theory of language where translation has been taken seriously from the start is SFL. This is important in seeing translation as part of what a linguistic theory has to account for; as far as systemic functional theory is concerned, translation is not an optional extra but rather central to the potential for multilinguality in language and central as a fundamental linguistic process. Your question was: "What insights can be derived from studying translation in the perspective of Systemic Functional Linguistics (SFL)?" In a way, I've answered the question of how SFL can shed light on translation. Now, to return to your question, I would like to re-iterate that translation is a linguistic phenomenon, so all linguistic theories should really take translation — and interpreting — as phenomena that they must engage with and account for if they want to be taken seriously as linguistic theories. As a number of linguists have pointed out, including Michael Halliday (e.g., 1974) and Nick Evans (Evans 2010), **multilingualism** can be taken as the unmarked condition of human societies — for most of our history, our ancestors must have been multilingual, so it stands to reason that interpreting would also have been part of the human condition. Consequently, the engagement with multilinguality should really be central to all linguistic theories, not an optional extra or afterthought.

Rounding off this first exchange, I would in response to your first question, "What insights can be derived from studying translation in the perspective of Systemic Functional Linguistics (SFL)?", that one absolutely fundamental insight is this: translation can be viewed in relation to other multilingual phenomena, including code switching and code-mixing (cf. the increasing attention paid to translanguaging in the last couple of decades), and multilingual studies, including language comparison and contrastive analysis and language typology (cf. Matthiessen, Teruya & Wu 2008). While for practical purposes scholars find it useful to refer to Systemic Functional Translation Studies, or SFTS (SFLTS), which is understandable, I try to avoid it — certainly as an institutionalized abbreviation — because I think that by now we have actually had enough of dedicated translation studies in the sense of isolating and insulating both the multilingual phenomena and the studies of these phenomena from other closely related areas.

8.3   A Linguistic Turn in Translation Studies

Bo Wang: In the field of translation studies, there are many "turns", such as the cultural turn in the 1980s (Bassnett & Lefevere 1990) and the sociological turn in

the late 1990s (e.g., Simeoni 1998; Gouanvic 2005; Wolf & Fukari 2007). Has there ever been a linguistic turn?

Christian Matthiessen: Yes, I think so; and there is arguably an emergent linguistic turn now, perhaps suggesting a helical movement rather than simply turns. Such turns are sometimes, or often, a matter of attention rather than original contribution in the sense of first mention. For example, I would have pointed to **Malinowski** as a pioneer in drawing attention to **culture** (cf. Steiner 2005, 2015) — and so laying the foundation of, or at least anticipating, the cultural turn, but remarkably Bassnett & Lefevere (1990) don't refer to his work or even mention him.[2] Firth (1957b: 106) summarizes Malinowski's approach to the interpretation of Kiriwinan text through translation as follows:

> The main features of his textual method can be summarized as follows: having placed the text functionally, from the sociological point of view, let us say, as a particular kind of spell tabulated in his systematic magic, linguistic statements of 'meaning' are to be made — first, by an interlinear word-for-word translation, sometimes described as a 'literal' or 'verbal' translation, 'each expression and formative affix being rendered by its English equivalent', secondly a free translation in what might be described as 'running English', thirdly by the collation of the interlinear and free translations, leading, fourthly, to the detailed commentary, or 'the contextual specification of meaning'.

This then in a sense takes us from low-ranking grammar to context — the context of situation of a particular text and the context of culture in which it operates.

Both **Eugene Nida** (e.g., 1964, 2001) and **Ian Catford** (1965) took steps that could be seen to initiate a linguistic turn in different ways in the 1960s.[3] Nida was, of course, particularly active in the context of Bible translation. Catford, more generally, conceived of linguistic translation studies as one aspect of typological linguistics, comparative linguistics and general linguistics, and wrote about that in his monograph in 1965. From the 1960s onwards, other people also did, and conceived of translation in the linguistic context (e.g., Ellis 1966). Later, there was a reaction against Catford in certain quarters as a linguistics imperialist. But he has a place for context; for example, Catford (1965: 49) writes "The SL [source language] and TL [target language] items rarely have 'the same meaning' in the linguistic sense; but they can function in the same situation. In total translation, SL and TL texts or items are translation equivalents when they are *interchangeable in a given situation*".

Looking back on this as an outsider to translation studies institutionalized as a distinct separate discipline, it struck me over the years what extraordinary efforts scholars would go to so as to avoid actually engaging with language in translation

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[2] However, they draw attention to later scholars (Bassnett & Lefevere 1990: 301): "In translation studies, the culturalist phase would describe the work of Nida and probably also of Peter Newmark, as well as the work of scholars such as Catford or Georges Mounin. The value of their attempts to think culturally, to explore the problem of how to define equivalence, to wrestle with notions of linguistic versus cultural untranslatability is undeniable."

[3] And there were certainly other contributions as well; cf. the brief mention of the work by Russian scholars in Sect. 9.2.

studies,[4] instead focussing on other aspects of translation like the contexts of culture of translation, to put this in Malinowski's terms — terms developed in SFL, and translator competence. They are, of course, part of a holistic engagement with translation as a multilingual phenomenon, and I am absolutely not saying they were not useful, but they did not actually engage with the primary phenomenon and the most difficult phenomenon, i.e., **language**, the multilingual meaning potential and the multilingual processing. People find all sorts of ways of avoiding engaging with language. That is a shame and is even bordering on a tragedy, because there is so much that still needs to be explored as far as the linguistic conceptualization of translation is concerned.

Also, various aspects that have been discussed under the headings of culture and translation competence and so on are, in fact, simply part of a theory empowered (and not driven) by the conception of language in context. I found it much more productive to try to ground the exploration of translation, even if one is concerned with translator competence or culture, within a general theory of language in context. If you slide up and down **the cline of instantiation** (e.g., Halliday 1991, 1992), all the phases relevant to a broad and deep conception of translation are actually in place, ready to illuminate our understanding of translation. Translation is a complex process unfolding through time, starting with the instance pole of the cline of instantiation, the source text in its context of situation, moving towards the system pole of the cline of instantiation, recreating the meanings in the source language and in the target language, and then moving down the cline of instantiation again, ending with the instance pole of the cline of instantiation, i.e., the translated text in its context of situation. The concern with culture just means that we move up the cline of instantiation all the way to the contexts of culture. It is all there on the map. When we model the process of translation, we will of course take into account all the relevant semiotic dimensions, and the process will involve interleaving phases along the dimensions and involve parallel processing.

When we consider interpreters, in particular simultaneous interpreters, topics like translator memory and memory capacity are especially crucial. But that is also there in the general theory of language as a fourth order system — a semiotic system that is enacted socially and embodied biologically. It is part of the conception of language as a fourth order system that manifests socially, biologically and ultimately physically (Halliday & Matthiessen 1999/2006) (see Fig. 8.1).

Based on **the ordered typology of systems** represented schematically in Fig. 8.1 (see also Matthiessen 2021), we can characterize translation and translator in a number of systemic steps, as indicated in Table 8.1. This ordered typology of systems enables us to locate translation issues very precisely. For example, the central property of translation as choice among options in meaning and thus of the recreation of meaning is a 4th-order property in the first instance; the role of the translator in teams involving other translators, editors and other professionals as well as clients is

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[4] The situation has of course been completely different in machine translation (MT) since MT researchers have actually had to develop detailed explicit models in order to build automatic translation systems. As linguists, we may not agree with a given MT model, but it will necessarily involve an account of the linguistic resources and processes involved in translation.
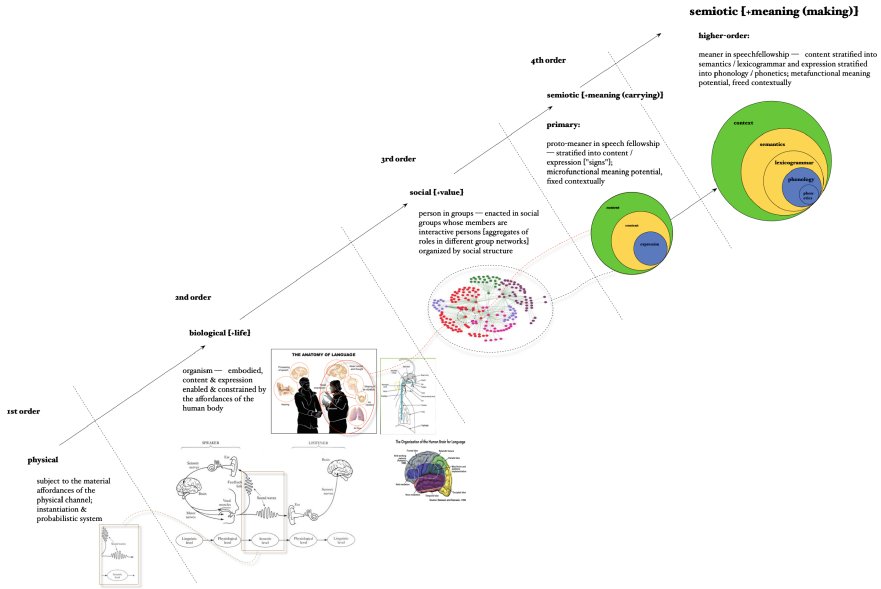
Fig. 8.1 The ordered typology of systems operating in different phenomenal realms

a 3rd-order property in the first instance and the nature of the translator's memory is a 2nd-order property in the first instance. One of the interesting new developments in translation research that is coming into focus is the correlation of observations across systemic orders, perhaps in particular relating biological observations to semiotic ones.

There is every reason to be optimistic about the possibilities of relating **text/discourse analytic work** that has been done in SFL and studies of **translation process** that record keystrokes and eye movement (see also Sect. 8.6). This is work that has been collaboratively done between **Fabio Alves** and his group in Belo Horizonte, Federal University of Minas Gerais (Universidade Federal de Minas Gerais, UFMG) (e.g., Alves 2003; Alves & Campos 2009; Alves & Gonçalves 2013; Alves et al. 2016) and **Erich Steiner** and his group at the Universität des Saarlandes (e.g., Hansen-Schirra, Neumann & Steiner 2012). This kind of work links these two ways of understanding translation, and is very interesting (e.g., Alves et al. 2010; Steiner et al. 2018). Here the **neurolinguistic research** done by **Adolfo García** and his group is also highly relevant and promising. Muñoz, Calvo & García (2018) provide a survey of neuroscientific research into translation and interpreting over a period of almost a century, and in his recent book, García (2019) provides a context for such research and a framework (including a "methodological toolkit") for moving forward:

> *The Neurocognition of Translation and Interpreting* seeks to achieve five general, interrelated aims, namely: (a) introducing neuro-cognitive research on IR [Interlingual Reformulation, CMIMM] vis-à-vis other cognitive approaches; (b) describing the methodological toolkit

Table 8.1 The activity of translation and translators as semiotic phenomena, enacted socially and embodied biologically

Systemic order	Research methods	Translation (field of activity)	Translator (tenor)
4th order: semiotic [+meaning]	Multilingual description, multilingual analysis (including computational corpus tools)	Translation as the recreation of meaning in context from one language to another	Translator as meaner operating with a multilingual meaning potential playing the semiotic role of recreator of meaning in context (a semiotic go-between)
3rd order: social [+value]	Ethnographic observation and analysis, questionnaires, interviews, focus groups	Translation as professional activity within translation as an institution, producing a (typically) commercial commodity	Translator as person playing the social role of a professional in translation as an institution, engaging with clients and other professionals such as editors in various social role networks
2nd order: biological [+life]	Brain imaging, eye tracking, key-stroke logging	Translation as bodily process, crucially involving neural activity within the multilingual brain	Translator as organism embodying multilingual meaning potential as a multilingual brain, subject to biological affordances and constraints (such as memory constraints)

employed so far in the literature; (c) presenting key notions of neurology, the neural basis of language, and the neurocognitive particularities of bilingualism as fundamental constraints to examine findings about IR proper; (d) compiling, organizing, and interpreting neuropsychological, neuroscientific, and behavioral evidence on highly prominent topics for TIS [Translation and Interpreting Studies, CMIMM]; and (e) discussing the present and future of the field, with emphasis on its accomplishments, strengths, weaknesses, and requirements.

Here it is important to note their engagement with SFL in other cutting-edge contributions, e.g., García & Ibáñez (2017) and Trevisan & García (2019). These contributions provide us with a bridgehead in future work linking neuroscientific research into translation and interpreting to the conception of them in SFL.

8.4   Prescriptive and Descriptive Studies in Translation

Bo Wang: Many studies on translation are prescriptive rather than descriptive. Are SFL-based studies descriptive or prescriptive?

Christian Matthiessen: In all fields SFL has been engaged with, the starting point is descriptive, not prescriptive.[5] If you look at the general conception of theory in science, in the sense of scientific theory, then prescription has no business being part of it. Prescription is something that comes out of scientific theory-based studies, drawing on findings to intervene where necessary to improve practices, as in the context of education or healthcare — and also in the context of translation. It is more helpful to think of this in terms of **guidance** rather than prescription: based on the results of research into translation, we may be in a good position to highlight what approaches are effective, to identify best practices, to determine how variant translations are valued and assessed in (sub-)cultures and to translate the results into programmes for educating translators — as in our work teaching students text analysis as part of their training to become translators.

Halliday (2001) talks about the conceptions of theory in the context of translation in a chapter in the volume edited by Erich Steiner and Colin Yallop (2001), where he compares this linguistic descriptive notion of theory in translation with the prescriptive one that translators may often want. But I think the problem is: if you start with the prescriptive focus, then it will essentially be folk notions or practitioner's notions that are not really grounded in empirical research.[6] When you do empirical research, you will find what the issues are, then you will be in an informed, evidence-based position to intervene in translator training with guidelines, protocols and the like. One example could be the development of manuals helping writers produce texts that are easier to translate — the use of controlled language (e.g., Hartley & Paris, 1997).

One can draw a parallel with **communication** and **healthcare** (e.g., Matthiessen 2013; Slade et al. 2015). We know that in training healthcare practitioners, it would be very beneficial to have courses in effective communication. But to be able to develop such courses, you need to actually research actual healthcare communication first. So, you need to understand what is going on and where problems arise — which may vary across cultures. For example, in societies that are still quite hierarchical in organization, it may be necessary to train junior healthcare practitioners to speak up in groups with senior colleagues in such a way that they are listened and still heard as polite. Once you know this, you can begin to move towards effective intervention.

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[5] Mauranen (2008: 43) writes: "Since the emergence of Toury's Descriptive Translation Studies (Toury 1980, 1995), translation research has tried to shed its strongly normative traditions; the tradition nevertheless lingers on, in part perhaps because translations need to maintain a certain quality in order to constitute acceptable texts in the target culture". But, importantly, the "normative traditions" were never part of the tradition of SFL and its precursors (Firth, Malinowski). If you locate translation studies within multilingual studies, as we did in Matthiessen, Teruya & Wu (2008), the problem disappears or can at least be seen in a different light: nobody ever talks about "normative typological studies", "normative contrastive analysis". The "normative" aspect will emerge as advice about effective translation in context, as in translator programmes, based on extensive descriptive research. Of course, translation norms in communities where translation is practised may themselves be the focus of descriptive research.

[6] Wang Dongfeng, who works on translation studies in China, was strongly against the significance of descriptive work in Wang and Ma (2020) by arguing that if a study cannot inform its readers about which translation is good, the study is then a failure.

The same applies to translation. You cannot actually develop effective guidelines until you know a great deal about authentic naturally occurring translation both as product (as source text and target text) and as process. This issue came up when I was involved in the MA programme in translation at Macquarie University. Students wanted something prescriptive, but I think you are doing them a much better service if you are giving them the tools to actually examine and diagnose issues in translation themselves. That is why we had a course on text analysis for translators. You give them the tools to examine the source text and the target text. Once you examine the text, you will see what is going on; you can possibly begin to evaluate it; you can compare translations and so on. I think that is the way to go. I remember once in a discussion of our T-&-I programme people said to me that training in text analysis for translation students was all very well, but interpreters would not have the time to engage in text analysis. I disagreed, saying that one can train oneself to perform real-time analysis alongside interpreting. This skill can actually be very empowering, raising practitioners' awareness and ability to monitor themselves in real-time.

8.5 The Environments of Translation

Bo Wang: In 2001, you published a book chapter titled "The Environments of Translation" (Matthiessen 2001). What are the environments of translation?

Christian Matthiessen: It started with a colloquium or workshop that Erich Steiner and Colin Yallop put together for ISFC at Cardiff University in 1998. They invited a number of us, and we had a very productive time together with contributions covering different facets of translation. Based on the contributions to this event, they edited a book on translation (Steiner & Yallop 2001).

In developing my contribution, I drew both on personal experiences and on SFL as a framework for engaging with multilingual phenomena, including translation. Translation had not been a dedicated research area of mine, but I had various insights into it because I came from a family background involving different languages and I had been involved in multilingual text generation (e.g., Bateman et al. 1991; Matthiessen et al. 1998). For example, sight translation was part of my childhood experience. And I had a half-niece-in-law, Vibeke Emond, who was a professional translator from Japanese into Swedish and Italian into Swedish. Also, my half-brother, Tryggve Emond, and half sister-in-law, Ingrid Emond, both did translation work as outsiders' activity.[7] It was very much part of the family experience. I also have a multilingual family on my mother's side. When I began to learn foreign languages — English, German, French and Arabic, I was lucky enough that the "translation method" still played a significant part in the approach.[8] And of course along the way, I had read

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[7] See https://sv.wikipedia.org/wiki/Tryggve_Emond and https://sv.wikipedia.org/wiki/Ingrid_Emond.

[8] I still remember an in-class translation exercise in French. One of the texts was a tango lesson in French, and the teacher asked a friend of mine to translate it into Swedish. He clearly hadn't done his

accounts of translation — I was familiar with Nida's work and had attended a talk he gave at one of the LSA (Linguistic Society of America) meetings in the 1980s, and, as noted above, our exploration of multilingual text generation was an interesting complement to machine translation (e.g., Bateman, Matthiessen & Zeng 1999). I had also initiated an informal translation project that turned out to be very informative — the translation of Michael Halliday's (2002a) *Computing meanings* into Chinese by Wu Canzhong (Halliday 2002b) and Japanese by Kazuhiro Teruya (Halliday 2002c), and the four of us would meet regularly to discuss issues that arose in this translation project. I remember Michael and Canzhong energetically discussing alternative translations into Chinese.

So, when I was invited by Erich and Colin in 1998, I felt prepared for the task, and thought part of my contribution could be to update Ian Catford's (1965) monograph, which was based on pre-Systemic Functional Linguistics — **scale-&-category theory** (Halliday 1961). Catford (1965) had demonstrated very clearly how the theory at the time could serve as a resource for illuminating translation as a linguistic phenomenon. But naturally it did not include later key theoretical developments, in particular it did not cover the theory of metafunction, the theory of axiality giving primacy to the paradigmatic axis, represented by system networks, and it did not operate with the more developed version of the theory of the stratification of language in context. So, there are a number of very interesting and rich developments since Catford (1965) that I felt would feed into thinking about translation from a standpoint of linguistic theory, specifically SFL theory.

I do not quite remember how I arrived at this, but what a general theory of language in context gives you is a very clear way of interpreting the different **environments** in which translation is located as a multilingual phenomenon, from the broadest environment to the narrowest. The traditional notions, like literal translation versus free translation, could be replaced, or at least be illuminated by actually locating them in terms of the environments in which they operated. That was inspired by illustrations from the 1960s by Michael Halliday's (1966) examples of translation between languages at different ranks (see Fig. 8.2).

My point is: the different environments provided by rank scale would apply to all other semiotic dimensions in the architecture of language in context, all the way to the most inclusive environment along any of the dimensions. That is, the amount of information available when you make choices in translation depends on the **environments** you have access to. In so-called **literal translation**, you will have access to fairly narrow grammatical environment. In so-called **free translation**, in principle, you will have access to the environment of the text or the text in this context of

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homework learning the French names of body parts, so he kept guessing and by the time he translated "and the man puts his right leg around his partner's neck", the whole class had dissolved into laughter. The translation method was, of course, used in the teaching of Latin (and Ancient Greek), and there is a "nice" example of a Latin class in the 1944 film *Hets* (English title: "Torment"), directed by Alf Sjöberg from Ingmar Bergman's screenplay — two giants of Swedish cinema (https://www.imdb.com/title/tt0036914/?ref_=nm_flmg_dr_15). The Latin teacher, nicknamed Caligula, takes obvious pleasure in tormenting a student with a translation exercise.

|||	la	jeune	fille	avait	+	+	raison	|| je	vais	+	lui	demand + er	pardon	|||
M	X	young	daughter	have	X	X	reason	X	go	X	X	ask for	X	pardon
W	the	young	daughter	had			reason	I	am going	him		to ask for		pardon
G	the girl			had			reason	I am going to ask him for						pardon
C	the girl was right							I am going to apologize to him						

S   The girl was right; I am going to apologize to her.

X = grammatical morpheme
+ = fused morpheme (e.g. *avait* consists of three fused morphemes)
M = morpheme equivalents
W = word equivalents
G = group equivalents
C = clause equivalents
S = sentence equivalent

Fig. 8.2 Multilingual correspondence and rank (adapted from Halliday 1966: 31)

situation. That was the notion of the environment, i.e., what you have access to that informs choices in translation (see Fig. 8.3).

Halliday (2009: 17–18) has described the environments of translation as follows:

[The environments of translation] are, or rather are defined by, the various dimensions along which language is organized: stratification, instantiation, rank, metafunction, delicacy and axis. Taken together, these are what give a language its inexhaustible power of making meaning, opening up all the different vectors — of abstraction, of combination, of depth in detail, of functional specialization and so on. […]
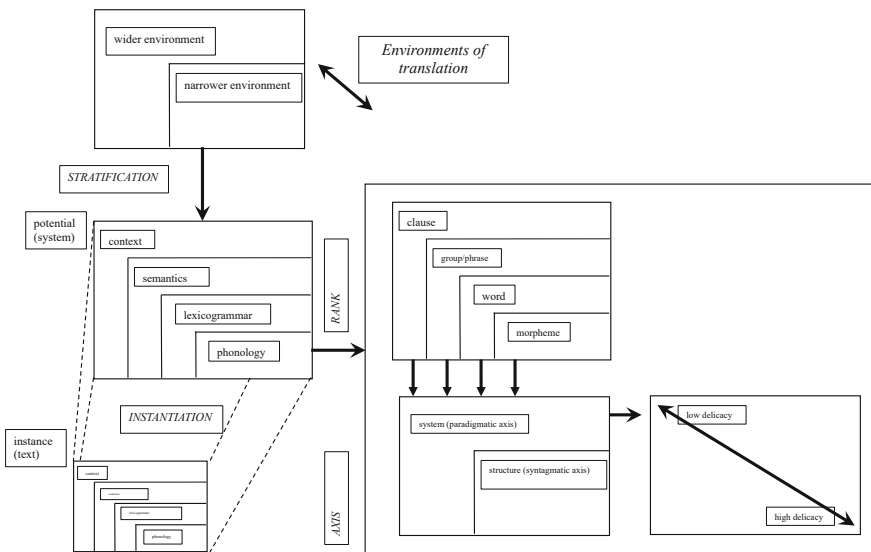
Fig. 8.3 The environments of translation

> These six dimensions […] are critical to any comparison of two or more different languages; and hence to the process of translation, because they are the parameters that define equivalence (and therefore also non-equivalence, or shift).

Bo Wang: Among all the six dimensions you have mentioned in your paper (stratification, instantiation, rank, metafunction, delicacy and axis), can we measure equivalents and shifts along all these dimensions?

Christian Matthiessen: Yes, I agree. That was the notion. Instead of having a kind of monolithic notion of **shift** or **equivalence**, you deconstruct it into the different dimensions that the theory of language gives you so that you can reason about it in a more balanced way.

So, the cline of instantiation gives you a way of reasoning about it. We translate texts at the instance pole of the cline, texts in context; but how far up towards the potential pole do we move to consult the systems that constitute the environments behind these texts? Is the environment just a mid-region along the cline — i.e., that of the particular register within which the translated text is located, or "sub-language" (to use the term that machine translation people arrive at, e.g., Kitteridge & Lehrberger 1982; Kittredge 1987)? Or you slide all the way up to the potential pole of the cline of instantiation, and consult the overall meaning potential in context of culture. This is part of the general architecture of language in context, not a special purpose account for translation. You can be very explicit about how you decide where you locate yourself.

8.6   Translation Shifts from the Metafunctional Perspective

Bo Wang: In 2014, you published another book chapter – "Choice in Translation: Metafunctional Considerations" (Matthiessen 2014), which discusses translation shifts from the perspective of metafunction. Why do we need to study translation from the point of view of metafunction instead of the other environments or dimensions?

Christian Matthiessen: I see that paper as a continuation of the concern from Matthiessen (2001). Various aspects of my explorations of translation, e.g., in preparing for teaching, have still not been written up, but I thought that one of the recurrent themes in translation is what translators attempt to do and what they do not attempt to do. You see again and again that from the translators' point of view (even from the professional translators' point of view), the **metafunctions** are not treated equally because the translators are not aware of the metafunctions.

In Mira Kim's studies (e.g., Kim 2007; Kim & Huang 2012), she examined what professional translators do, going between Korean and English. She looked at the translated text, and analysed both the source text and the translated text from a textual point of view. She found that there were shifts in the textual choices of Theme. The questions then are: Were these shifts motivated or not? Were they necessary? In one experiment, she took the translated text, revised it, focussing on textual choices.

Then she presented the text to her students, who did not know which was the original translation and which was the version edited by her. The interesting finding was: it was the text she had revised based on her insight from the textual thematic analysis that they preferred.

Thus the professional translator had been so focussed on ideational meaning so that he/she had overlooked the textual meaning. The point of problematizing choices in the different metafunctions is to ascertain how they work together. Sometimes they may compete because, among other things, translation is a process of optimizing, weighing competing motivations against one another. Sometimes perhaps, you have to give up on certain recreations of meaning in order to achieve others. But what you want to ensure is that the choices are as informed as possible, and that the translator is in fact aware of them — that they do not just happen by chance. If you have translators who do not actually know much theoretically and professionally about language, chances are this would not be something that they have thought about. (I have similar examples going from Danish to English. I have identified various examples where the translation shifts seem completely uncalled for, not motivated in any obvious way. It seemed that the translator was just unaware of the shifts).

Translation is probably one of the most challenging human tasks, certainly one of the most challenging semiotic tasks. It is a very difficult operation. Translation, whether it is done in the community traditionally by people who meet from different tribes to interpret and translate or done by professional translators, is quite challenging in terms of what you have to be able to do and what you have to be able to control to be an effective translator.

Bo Wang: In Matthiessen (2014 see also Matthiessen 2021), there is a matrix of "the metafunctional translation shifts" (see Fig. 8.4). What do metafunctional translation shifts mean? Why are some boxes in the matrix left empty?

Christian Matthiessen: They are empty simply because of the way I designed the matrix. As we explore the shifts empirically by analysing texts, we allow for all possible shifts. You can shift from textual to experiential, for example; looking at

		From source text			
		textual	ideational: logical	ideational: experiential	interpersonal
To target text	**textual**	textual > textual: e.g. thematic shift	logical > textual: e.g. complex to cohesive sequence		
	ideational: logical	textual > logical: e.g. cohesive sequence to complex	logical > logical: e.g. tactic shift		interpersonal > logical: e.g. mood or modality represented by verbal or mental clause in clause complex of projection
	ideational: experiential		logical > experiential: e.g. clause (in complex) > phrase (as circumstance)	experiential > experiential: e.g. process type shift	
	interpersonal				interpersonal > interpersonal: e.g. mood type shift

Fig. 8.4 Matrix of metafunctional translation shifts

the matrix, we locate this in the "textual" column at the "ideational: experiential" row, and you can also move from "ideational: experiential" (column) to "textual" (row). So in principle, all cells of the matrix could be filled. However, some shifts are more likely and occur more often, like shift between the textual and the logical, which tells us something about those two metafunctional modes of meaning. Others are less likely, such as shift from textual to experiential, but it does not mean that they would not happen.[9]

What I would love to see is a long-term project of analysing lots of source and target texts from different registers in terms of metafunctional translation shifts. When we progress in this way, we will learn more about which boxes would ultimately remain empty and which ones would be filled. For example, if you translate advanced academic discourse from some languages to English, you might find more shifts towards the experiential because of the prevalence of grammatical metaphor. This relates to Elke Teich's (2003) study of translation in the area of academic discourse between English and German. She also focusses on nominalization as one aspect of grammatical metaphor, what kinds of metaphor are possible in one language, and what kinds are possible in another (cf. also Steiner & Teich 2004). Perhaps, there are areas where English has gone further than German in metaphorizing patterns of meaning (possibly not in all areas, but in certain ones). This is really an open and empirical question, one that can only be answered by extensive text-based research involving both automated analysis of the whole corpus of texts in terms of "low-level" patterns and manual analysis of selected samples in terms of "high-level" patterns (for the design of multilingual corpora, see Hansen-Schirra , Neumann & Steiner 2012).

See Fig. 8.5 for illustration of choices in different metafunctions.

8.7 Directions for Future Research

Bo Wang: In which directions do you think we should move forward?

Christian Matthiessen: One direction is simply to conduct more **empirical research** that involves **text analysis** as a central component, not only as part of translation studies, but also as part of interpreting studies — and even studies of "translanguaging" in general, and across many different registers and involving different kinds of translators. (Sometimes it is useful to have translation as a cover term for translating and interpreting.[10]) There is a great deal that we need to find out, and to examine contexts of target texts and of source texts that are likely to encourage or require different kinds of translation. If you translate in a written expounding context (see Fig. 7.5), such as a textbook in medicine, the primary focus is on ideational

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[9] While I won't explore the point here, it is interesting to compare the possible and common shifts with variation across languages within the lexicogrammatical space defined by metafunction, rank and delicacy, as discussed in Chap. 7(see Fig. 7.12).

[10] Muñoz, Calvo & García (2018) use the cover term "interlingual reformulation".
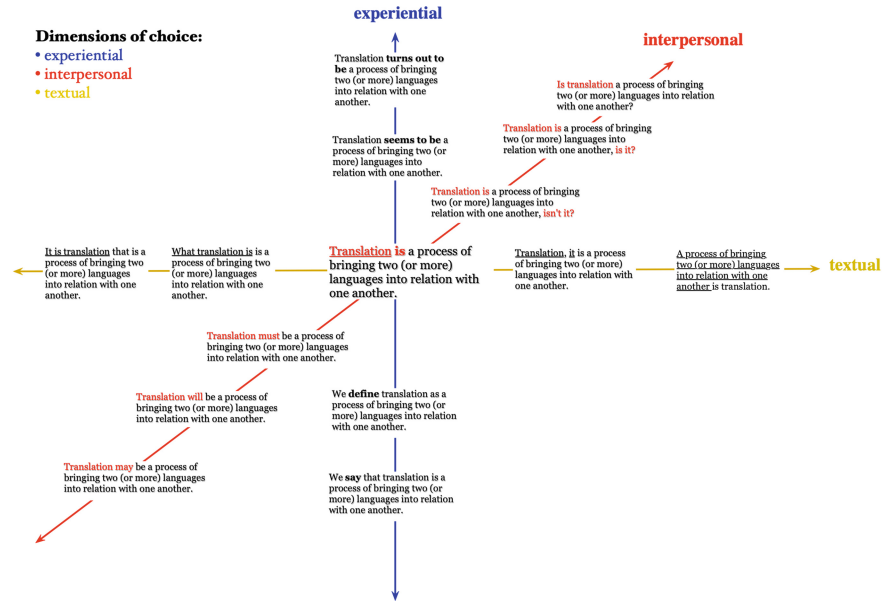
Fig. 8.5 Metafunctional dimensions of choice in clause — experiential, interpersonal and textual patterns of agnation

meanings in relation to the field of medical expertise because you are constructing medical knowledge, which will ultimately be an issue of life and death. In some sense, ideational accuracy and faithfulness are relative to the source text as a source of expert knowledge. Contrast that with a context of recommending: promoting, such as commercials and advertisements in relation to tenor settings within context. Here ideational meanings will really be in the service of interpersonal considerations. What you want is interpersonal success, assigning value to commodities to persuade readers or viewers to buy them or at least to regard them highly. There are interesting studies by Erich Steiner (e.g., 1997, 1998, 2004) on the translation of advertisements of Rolex watches. You might make different translation choices depending on what has value in terms of tenor in the target culture — its value system, which may differ from the source culture (as studied in axiology). What you really want is an advertisement that is an effective and persuasive text in the target culture. The ideational meaning does not matter a great deal unless the advertisement is about some piece of machinery such as a computer equipment or fancy automotive machinery that people are interested in terms of how they work. To impress the customers is part of the successful way of selling the product. What you are really interested in is persuading people to buy something that they do not necessarily need, parting with the money they do not necessarily have. So, the considerations are very different. I would like to see this highlighted in a much more general way. Scholars have commented on the translation of advertisements, pointing out that the translator serves also as a mediator.

The translation of verbal art — literature as art made out of language — is also an area that has attracted a great deal of attention. In your recent excellent book (Ma & Wang 2021), you have surveyed the literature concerned with the translation of poetry as one kind of verbal art, and presented a systemic functional framework for the translation of poetry, and applied it to the translation of Tagore's *Stray Birds* into Chinese. And in Wang & Ma (2020), you do the same for the translation of drama, applying the systemic functional framework to the English translations of Lao She's *Teahouse*. I would love to see many more such systematic studies of a wider range of texts belonging to registers operating in different cultural domains, a programme that is related to my notion of registerial cartography (e.g., Matthiessen 2015a). Again, it comes down to really engaging with the key phenomenon in translation, i.e., language.

I would also love to see continued research into the **translation process** and the **interpreting process**, and finding ways of observing this in a way that does not disturb the process. For a while, think-aloud protocols were popular. They are interesting, but the problem with them is the observer's paradox. They actually disturb the phenomenon they are trying to observe because if they ask a translator to think aloud as he/she is doing translation, they are actually changing the phenomenon quite considerably. If there are ways of observing the translation process without actually disturbing it, like keystroke logging and eye tracking, this will be very interesting (Alves 2003, and a growing number of publications concerned with such TPR [translation process research] — Jakobsen 2014). Nowadays, one can do this quite subtly. I would love to see more of this kind of work. At some point, we will be able to observe and scan the brain in the process of translating and interpreting (e.g., García 2019; Muñoz, Calvo & García 2018). That will be fascinating, and will feed into a thickened description of translation — thickened here in relation to the ordered typology of systems (see Fig. 8.1). So, that is what I will think is one key aspect, just in a sense of scaling up the empirical efforts.

Some scholars have discussed the existence of **translation universals** (e.g., Blum-Kulka 1986; Baker 1993; Mauranen & Kujamäki 2004; Mauranen 2008; cf. House 2008). If you look at the history of the engagement with language, you will find that people are always over-eager to talk about universals. Think about the number of languages still remaining with us (e.g., Dixon 2010). Then think about how many of these have been examined in translation studies or in studies that are concerned with translation. How many language pairs have been studied (cf. Muñoz, Calvo & García 2018)? Such considerations should be an immediate invitation to humility about making any claims about translation universals (also in view of the "myth" of language universals — Evans & Levinson 2009, and the "universalist fallacy" — Firth 1957a).[11] Here, I would recommend the work that I have cited in different contexts by Andrew Pawley (1987), which is very illuminating, revealing differences

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[11] This of course also relates to our discussion of language universals in Chap. 7. The area is obviously very complex and even contentious, spanning debates from say empiricism vs. rationalism to identity politics. While I can't go into it here, I would like to emphasize that this is yet another area of contestation where the theory of the ordered typology of systems can help us sort out a number of properties that have been interpreted in universalist or relativist terms.

not normally found in studies involving standard varieties of "major languages". He cites George Grace (e.g., 1981, 1981–1983) by saying that the languages that are typically studied in translation research are actually not that far apart. If we take English and Chinese, they are not that far apart on a global scale. They both have very long histories as written standard languages, and have evolved the register ranges of the modern nation state. In contrast, there are languages that operate under very different cultural conditions. Pawley's (1987) example is English and Kalam, a Trans-New Guinea language spoken in the highlands of New Guinea. For instance, he compares the construal of our experience of hunting in English and in Kalam, setting out an account by a Kalam speaker, Saem (p. 340): "Saem describes the circumstance as a sequence of five or six events:

GO / KILL GAME / CARRY AND COME / BAKE / EAT (GAME)".

After analysing this sequence, which we can interpret as a clause complex, he comments on a possible English translation (p. 342):

The single English verb 'hunted' is an idiomatic translation of the whole sequence. It is noteworthy that in contrast to their extraordinary explicitness in specifying the component actions in the hunting sequence, Kalam narrators are very sparing in their mention of other elements — places, instruments, objects affected, etc. The audience is left to infer these from his knowledge of customary usages. It is not simply a function of the sequence of verbs being lexicalised, or as denoting a familiar series of events. ... Kalam speakers follow the same conventions when talking about unfamiliar events — detailing the component actions (according to certain conventions) while omitting reference to many of the other elements that an English speaker feels it necessary to mention.

Thus the degree of "explicitation" and "implicitation" in the translation depends on the affordances of the ideational systems for construing experience in Kalam and in English. Kalam tends towards the logical mode, modelling experience serially or as chains of events, while English tends towards the experiential mode, modelling experience configurationally with considerably taxonomic delicacy in the construal of processes (cf. Sect. 7.9). Pawley gives many more examples of the differences between Kalam and English, concluding the comparison by saying (p. 351):

We may conclude from the foregoing that there is no universal set of episodic conceptual events. Indeed, it seems that languages may vary enormously in the kinds of recourses they have for the characterization of episodes and other complex events.

We can also take other language pairs where there will be very significant semiotic distances; we could again refer to Malinowski's findings as he tried to translate Kiriwinan texts into English. These need to be systematically studied as well, and the functional-typological literature now provides many studies showing how languages may differ in how they construe our experience of the flow of events under headings such as "event encoding", the construal of motion being an area that has attracted considerable attention (cf. Matthiessen 2015b). We can also consider the interpersonal and textual modes of meaning. For example, a study of translation of texts in a language that obligatorily assesses the evidential status of information (e.g., Quechua) into texts in a language that doesn't (e.g., English, Chinese) would be very interesting from the point of view of "explicitation"/"implicitation".

If you want to pretend to talk about translation universals, then you need this kind of really extensive empirical database. That is one reason why we were very keen to promote the notion of **multilingual studies** (Matthiessen, Teruya & Wu 2008). You take different strands of engaging with multilingual phenomena that have tended to be drifted apart as areas of study, and ascertain whether there are any connections among them so that investigations into translation inform work on language comparison and typology and also the other way round. It is a total intellectual tragedy that translation studies have to a large extent become separated from language comparison and from typology; whereas if you really take a step back and look at the overall scene, the two domains of multilingual studies could really inform each other (see also Sect. 7.3).

By the same token, insights in translation can be very helpful in thinking about second/foreign language education when you push towards more advanced mastery of a second foreign language (see Steiner et al. 2018). We all know that the traditional approach to second/foreign language teaching had a syllabus based on grammar and translation as a method, which was thrown out of the classroom partly because it was copied from the attempt to work with reading classical languages, Ancient Greek and Latin. But if you think about what would constitute a rich conception of a curriculum for inviting people to go into further advanced mastery of their L2, that is an environment where it makes very good sense to bring translation back. Why? Coming back to what I said above — that translation is probably one of the most challenging processes that we can undertake linguistically, it seems very clear that if you move in the direction of translation, you are really pushing the envelope of the target language and the source language when you are translating from a second language or foreign language into the mother tongue. Also, you will find that it really pushes translators in training, who are also the L2 learners; and at the same time, they are also invited to expand the meaning potential of their mother tongue. So, if you are looking for new opportunities and challenges for truly advanced learners, you are trying to invite learners not to plateau, but to continue to grow their meaning potentials. Then translation and interpreting are very interesting additional semiotic processes to master.

I would also like to see more work that takes insights from translation and from editing original texts, and much more work on editing from an informed linguistic point of view, to compare notes with what is going on in translation, successive translations of the same source text and approximation of translations (cf. Alves & Vale 2011; Alves et al. 2016). There are many other aspects, such as the theoretical developments that are needed. We can save that for another time (see Sect. 10.3). On the whole, linguistics has been reasonably good at theorizing the potential of language and also instances in the form of texts. But linguistic theories need to engage much more head-on with linguistic processes, and this applies to the process of generation, the process of analysis and interpreting and the process of translation. So I would love to see this being developed further as part of the general theory of language in context.

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Chapter 9
Theoretical Contributions to Translation Studies

Abstract This chapter, as a sequel to the first part of the interview on translation in Chap. 8, further examines issues related to Systemic Functional Linguistics (SFL) and translation studies. We discuss the acceptance of the term "systemic functional translation studies" in academia. Then, we comment on the contributions made by various scholars, including J. R. Firth, Erich Steiner and his group, Mona Baker, Juliane House, and J. R. Martin. Finally, we explain the relationship between SFL and some other translation theories, including skopos theory, polysystem theory and descriptive translation studies.

9.1   Introduction

In the preceding chapter, we examined translation as a linguistic process, mostly using the dimensions of the systemic functional linguistic architecture of language. In this chapter, we proceed to engage Christian Matthiessen on various theoretical contributions to translation studies. We problematize the term "Systemic Functional Translation Studies" and consider the contributions by key scholars (both within and outside the SFL tradition) to translation studies, notably J.R. Firth, Erich Steiner, Mona Baker, Juliane House, J.R. Martin, Gideon Toury among others. The chapter thus complements our discussion of Christian Matthiessen's own theoretical contributions in Chap. 8. The present discussion goes beyond SFL to engage with insights from other theories and models of translation and how these interact with SFL. Some of the models considered include Juliane House's model of translation quality assessment, the Skopos Theory, polysystem and Descriptive Translation Studies.

9.2 Rehabilitation of the Term "Systemic Functional Translation Studies"

Bo Wang: I have observed that some scholars, especially those who work in translation studies, are not comfortable with the term "systemic functional translation studies", and they repeatedly remind me not to use it. I first saw this term in "Ideas and New Directions" (Matthiessen 2009a), which outlines the different areas of research that SFL can shed light on. The term can also be found in works such as Vasconcellos (2009) from UFSC (Universidade Federal de Santa Catarina) in Brazil, Elaine Espindola (2010) and Wang and Ma (2021). I am curious why this term seems to be a taboo to some people.

Christian Matthiessen: That is interesting. I cannot recall any instances of people being critical of it, but maybe it is more implicit than explicit. What would be an example of somebody being critical?

Bo Wang: I think it is because of their bias. They said: "Oh, I have never read about this term before. It is totally new. You sound like a layman when using this term".

Christian Matthiessen: In 1905, when Albert Einstein introduced "relativity", he complained that people did not like the term "relativity theory" because they had not come across it before. If the novelty of the term is the basis for people's objection, then that is so comical that it is difficult to take it seriously and comment on. It is interesting how eager scholars can be to serve as gatekeepers, guarding some academic territory. The history of science is full of examples of such inertia in science as an institution. Of course, things develop, new ideas are sometimes labelled by new terms.

There is an interesting issue about the institutional construction of research in **translation studies**. You could say translation studies simply ought to mean any research concerned with translation as a phenomenon, whether you view it as a process or as snapshots of the process, including the end result, the final translated target text or successive editions and revisions of it. But "translation studies" has come to take on an institutional sense as a separate distinct discipline. Consequently, you get issues that are not necessarily associated with the field of research, but have more to do with the tenor roles of the construction of this institutionally, the guarding of the territory and so on. It happens, and I think this is due to Holmes' (1988) charting out of different approaches to the study of translation (see Fig. 9.1). He died in 1986, so he did not have the opportunity to develop the map or translation studies further, taking in and contributing to new developments. I do not know if he ever intended the field to take the shape it now has, but certainly other people have run with his conceptual map (e.g., Malmkjær 2005, 2013; Toury 1995).[1] I would draw the map

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[1] From the point of view of our notion of multilingual studies, it is noteworthy that the map separates "translation studies" from other multilingual phenomena (contrast e.g., Catford, 1965; Ellis, 1966), including the closely related phenomena of code switching and code mixing — closely related at least in the sense that they also presuppose a mastery by speakers of a multilingual meaning potential.
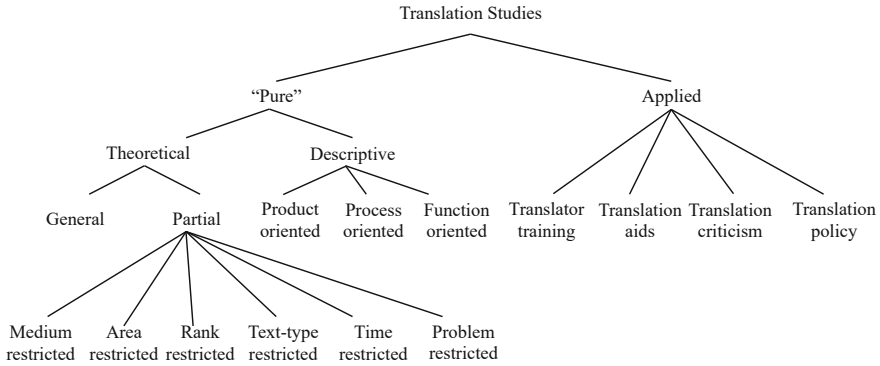
Fig. 9.1 Holmes's map of translation studies (adapted from Toury 1995: 10)

differently, one aspect of which is indicated by Fig. 7.3 (see Chap. 7), where I have intersected with the cline of instantiation with the number of languages in focus. Here the focus of "translation studies" is located at the intersection of "instance (text in context of situation)" and a two or a few more languages (as opposed to many, sampled according to explicit criteria in language typology). However, just as we need different projections and themes for maps (physical maps), we do so for conceptual maps as well. In Fig. 9.2, I have drawn a simple display showing that the phenomenon of translation-&-interpreting can be related to other linguistic phenomena that we study along two dimensions, viz. other multilingual phenomena and other metalingual phenomena. We can gain considerable insight into translation-&-interpreting by considering this phenomenon in the study of these other related phenomena. As far as multilingual phenomena are concerned, we can also note the significance of the conceptualization of **translanguaging** (e.g., Mazzaferro 2018).
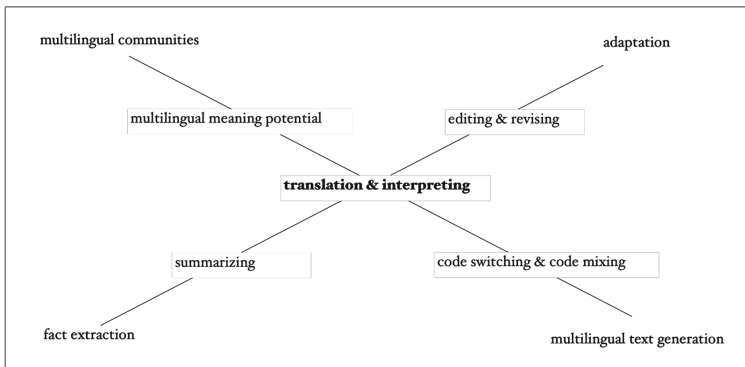
Fig. 9.2 Translating-&-interpreting related along two dimensions to other multilingual phenomena and to other metalingual phenomena

By **systemic functional translation studies**, I do not mean it in that institutional sense. What I mean is research into issues of translation empowered by, informed by and guided by SFL and feeding back into the development of SFL. And like systemic functional studies in general, systemic functional translation studies is **appliable** in nature, transcending the dichotomy between theory and application, or between "pure" and "applied". Halliday's notion of appliable linguistics is of great significance, helping us to conceive of theoretical and applied activities simply as phases of a general process rather than as institutionally distinct spheres; see e.g., Halliday (2002, 2008), Matthiessen (2014a). So it would make sense to recognize the value of **appliable translation studies**, where theory & description and application are phases in dialogue in a continuous process.

Another point is: unlike many theories of language, Systemic Functional Linguistics takes translation seriously as a linguistic phenomenon. That would be a very general point about the scope of engagement — the theoretical and scientific engagement with language. Different linguistic theories and traditions draw the boundaries in different ways; but in SFL, translation has been on the agenda from the start (e.g., Halliday 1956a, 1962; Halliday et al. 1964; Catford, 1965). One thread that Steiner (2005, 2015, 2019) has illuminated goes back all the way from the influence of Malinowski (e.g., 1923, 1935). His contextual theory and engagement had to face the issues of translation with language and culture pairs that were very different from the kind of language-culture pairs that European scholars focussed on (cf., also Firth on Malinowski's textual approach, referred to in Chap. 8).

This is my reflection on the issue you raise, but I cannot explain why people would consider the term "systemic functional translation studies" as a taboo, unless as I said, you cross some kind of institutional boundaries. But then to me, this is a tragedy of creating academic ghettos such as translation studies and separating translation studies from other forms of theoretical and scientific engagement with language. That is part of the theme of the chapter on **multilingual studies** that Wu Canzhong, Kazuhiro Teruya and I wrote over ten years ago (Matthiessen, Teruya & Wu 2008).[2] I feel that many issues that come up in the works by different groups of scholars are concerned with multilingual phenomena in one way or another, whether it is at the system end or at the instance end, whether it is comparison and contrast of a small number of languages or typology of a large number of languages, whether it is to feed in linguistic theory (what language typology is often concerned with) or educational applications and so on. There are so many reasons to bring multilingual studies

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[2] As I noted in Sect. 8.1, I tend to avoid the term "Systemic Functional Translation Studies", or "SFTS", especially if there is a risk of an institutionalized interpretation in order to ensure that investigations of translation are located within the realm of multilingual and metalingual phenomena charted in Fig. 9.2. Regarding "SFTS", I tend to avoid acronyms in general unless they are widely known in the community. While acronyms make good Zipfian sense, they can exclude students and scholars who are not part of the community of users; they must look them up when they come across them. So since readers use Systemic Functional Linguistics in so many different areas of activity, there are always some likely to be puzzled by SFTS, GM, IR and other acronyms coined by users in one particular area. But there are of course always competing motivations in naming choices (as in other choices).

together instead of having different communities of scholars concerned with one area or the other. Again, it is unfortunate if translation studies is hived off from other aspects of language studies, which often means that the approach taken comes with a very weak theory of language. Engagement with translation could be informed by very rich and powerful theories of language, with very comprehensive descriptions of the languages involved. But if translation studies focusses on issues around language, rather than taking language as central, and builds models of translator competence, then it is likely to lead in different directions.

9.3   J.R. Firth and Translation Studies

Bo Wang: J.R. Firth is one of the linguists who has greatly influenced SFL. Has he played any role in translation studies?

Christian Matthiessen: Yes. Firth was one of these generation of linguists who faced and had to sort out approaches to a number of fundamental issues. He was a generalist, and his works ranged over a large number of manifestations of language phenomena. In terms of linguistic theory, he tended to engage with the outer strata — phonetics, phonology and context, not so much on lexicogrammar (lexis of course, with works on collocation and its grammatical analogue, colligation). That explains why when Michael Halliday (e.g., 1956b, 1957, 1985; Halliday & Matthiessen 2014) began to develop what was to become SFL in the 1950s, he focussed on the inner strata (see Fig. 6.8).

Firth (1956a, 1956c) was also very interested in translation. He took over the notion of **context** from Malinowski (e.g., 1923, 1935), and contributed to thinking about translation, like his notion of "**restricted languages**" (Firth 1952), which then led to the **register theory**, as developed by Halliday and his colleagues in the 1960s (see Halliday et al. 1964). Also, his works on **person and personality** (Firth 1950, 1952) can be helpful to research in translation studies. Further, his concern with **collocation** (Firth 1956d) is very important because these are patterns where languages may have interesting differences; a situation that can pose practical challenges to translators (e.g., Bernardini 2007; and to L2 learners, as pointed out by Halliday 1966).

Outside the Firth-Halliday tradition, scholars have also brought attention to **collocation** and other lexical patterns that can be difficult to translate or at least require special attention (and resources such as bilingual collocational dictionaries). One is the Russian group of scholars such as Apresjan, Žolkovskij and Mel'čuk (e.g., Apresjan et al. 1992; Apresjan et al. 2007; Mel'čuk & Žolkovskij 1970; Mel'čuk & Wanner 2008), who developed the notion of **lexical functions** in the 1960s and observed that such functions are important when you move across languages. One of the functions is MAGN — magnify the meaning of whatever it is applied to. One can use a classic example by Michael Halliday (1966) of how we indicate the magnitude of something. He cites collocations like "heavy traffic", "strong tea", "powerful

argument" and so on, which have different manifestations of the gradation of "up". That is something that translators face, and getting the collocational patterns right can be a challenge, even in cases where it is possible to relate collocational patterns to systems of lexical metaphor as suggested in Matthiessen (2009b). Then over time, that led to collocational dictionaries (Sinclair 1990). But you need the tools that came in later — the corpus in the 1960s. There is a whole pattern of contributions that Firth has made that feed into thinking about translation.

Interestingly, Firth was not a universalist, but a **particularist**. This contrast can be interpreted as a manifestation of the common move between thesis and antithesis. Much thinking about language had been universalist in orientation by treating universals as realities, and the universalist orientation was part of the later Greenbergian empirical approach to language typology and universals (cf., Greenberg 1966, Greenberg, Ferguson & Moravcsik 1978). Firth (1956b) reacted against this tradition, which had emerged in the nineteenth century from earlier roots, and argued for the importance of treating each language in its own right based on empirical evidence. This emphasis on bringing out the characteristics of particular languages was taken up and further developed by Halliday (e.g., 1957, 1959–60), who emphasized the value of comprehensive descriptions. This particularist orientation is important when you approach translation. You do not treat two pairs of languages as merely a variance of some universalist conception of language (cf., the discussion of language universals in Chap. 7, Sect. 7.2).

9.4 Erich Steiner and Translation Studies

Bo Wang: In your opinion, what contributions have Erich Steiner and his group made to SFL and translation (cf., Steiner et al. 2019)?

Christian Matthiessen: Focussing first on **Erich Steiner** himself, there are different phases of his foundational contributions. An early phase was in the 1980s, when he got involved in one of the grand European projects — **the EUROTRA project** (e.g., Steiner et al. 1988), which involved a number of scholars and researchers from different traditions and various European countries and languages to work towards machine translation system (the EUROTRA system). He made significant contributions to that project, including thinking about transitivity (Steiner 1985).

More recently, after he moved quite a few years ago to the Universität des Saarlandes in Saarbrücken in Germany, his brief was to develop a framework for doing translation studies. Besides systemic functional theory, he was concerned with incorporating **corpus methodology**, which had of course been important to both Michael Halliday and John Sinclair in the 1960s, and has been part of the SFL methodology since then. Halliday's Ph.D. on Chinese was a corpus-based study even from the mid-1950s (Halliday 1959). His early description of intonation in English (Halliday 1967, 1970) was also corpus-based, as was Afaf Elmenoufy's Ph.D. thesis on intonation, supervised by him (Elmenoufy 1969). So, to Halliday, the corpus was a natural part of

the methodology. When Halliday directed a project on scientific English (Huddleston et al. 1968; cf., also Halliday 1988, 1992), the researchers compiled and investigated a corpus.

In the 1960s, it was very difficult to get corpus-based studies published because publishers were so much oriented towards the Chomskyan tradition, where the notion of corpus was not taken seriously; so the Hallidayan orientation to corpus-based research may be overlooked simply because publishers did not publish manuscripts submitted to them. Then **Sinclair** (e.g., 1987, 1990) continued the corpus-based research in Birmingham, and built up the COBUILD corpus work with Collins publishers, resulting in a string of very valuable and influential contributions. Then this influence came into translation studies through Baker (e.g., 1993, 1995, 1996). The connection came through Birmingham to Manchester, and gave this corpus-based translation studies quite a push.

What Erich Steiner and his group did in Germany was to develop the corpus-based approach to translation studies using a theoretically informed framework for thinking about how to design a multilingual corpus, and how to design a set of interrelated corpora that will allow you to look into translation issues and do this with a recognition of **register**. To Erich Steiner, register was important. In some of his earlier contributions with manual analysis of translated advertisements into different languages (e.g., Steiner 1997, 1998), translating advertisement was very much target-culture-oriented, including an interesting example of advertisements of Rolex watches. These articles were collected in Steiner (2004). The projects he developed with the team of scholars had to do with the design of multilingual corpora, and they reported this in a book (Hansen-Schirra et al. 2012). Works have continued and various scholars have been involved in such research either as junior professors or as professors. This has been continued especially in the German context.

Elke Teich has made very important and pioneering contributions as well in this area, after her Ph.D.[3] Her Habilitationsschrift (the second Ph.D. for scholars in Germany, undertaken to qualify for a professorial position) was on comparative translation studies between English and German through corpus methodology (Teich 2003). She addressed the questions that scholars like Mona Baker have talked about, such as source language shining through register norms in the target language.

I think Erich Steiner has been very important institutionally in training junior scholars, and also as a person who has written about the development of ideas about translation in SFL going back to Malinowski (Steiner 2005, 2015, 2019).

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[3] Her Ph.D. (Teich 1995) was on the computational linguistic modelling of Systemic Functional Grammar, which considered how representational issues could be addressed, informed by other frameworks within the unification-based family of approaches.

9.5   Mona Baker and Systemic Functional Linguistics

Bo Wang: You have just talked about Mona Baker. She related SFL to translation in her textbook *In Other Words: A Coursebook on Translation* (Baker 1992).

Christian Matthiessen: Yes, it was a very helpful contribution. We used it ourselves as a textbook at Macquarie for our MA programme in translation studies, and we referred to it when we developed the course on text analysis for translators and translator training. In a sense, this is **Mona Baker** before she very energetically turned to corpus-based research in translation, drawing on the work by John Sinclair and his group at Birmingham University (e.g., Baker 1993, 1995, 1996), but it is a classic, and is now in its third edition.

You can see the influence of SFL in her book most clearly in the chapter on the **textual metafunction** (i.e., Chap. 5 Textual Equivalence: Thematic and Information Structures). From my point of view, it is the most successful chapter. It is also the one that is most influenced by SFL, which deals with issues of the recreation of thematic choices in the process of translation (cf., Kim & Matthiessen 2017). That is very interesting, and it has been quite influential in that respect.

9.6   Juliane House and Her Model of Translation Quality Assessment

Bo Wang: Juliane House is another famous scholar, and her functional-pragmatic model of translation quality assessment has been very influential (e.g., House 1976, 1977, 1997, 2015). What do you think of her model?

Christian Matthiessen: Again, it is a very interesting and important contribution. Unlike Mona Baker, whom I have unfortunately not yet met, I know **Juliane House** both as a long-distance colleague and as a friend. The first time I met her was in 2004, when she had invited me to spend one month as a visiting professor at the Research Centre for Multilinguality (Sonderforschungsbereich für Mehrsprachigkeit) affiliated with the University of Hamburg. It was fascinating to see the projects she was working on and directed with Ph.D. students at the time.

Her **model of translation quality assessment** has been very helpful. Interestingly, a great deal of work has been done in machine translation on quality assessment because while if you are dealing with human translation, there are of course the issues of translation quality, but in machine translation, the need has been absolutely urgent. Instead of dealing with the output by human translators, you are dealing with the challenge of assessing the output by machine translation systems. I remember talking to Professor Yu Shiwen from the Institute of Computer Science and Technology at Peking University, who was working on this in the early 1990s. He was in computer

science, involved in one of the early efforts to deal with the assessment of translation quality of machine translation.

Both of you (i.e., Wang Bo and Ma Yuanyi) have come right up against the considerable variation in translation quality in looking at translations of drama (Wang & Ma 2020; Wang 2017) and poetry (Ma & Wang 2021; Ma 2018). When you do it with human translation, in some sense, you can pull away from primarily worrying about the local details of wordings because you assume the competent translator will probably be able to deal with them (though not always); so it makes sense instead to focus on meaning in context, which is precisely what Juliane House did. She grounded her framework in context and then moved from context into language. When you think about assessing translation quality, context is very important, whether it is the context of the source text, the context of the target text, the context of moving between the two, or the context of translation itself by using field, tenor and mode with different values. She related these contextual parameters to certain areas of lexicogrammar in order to investigate the quality in terms of text analysis focussed on these areas.

This is a steppingstone towards a full-fledged comprehensive model where you look at everything to assess the quality of translation. This is an important ideal, partly because translation is often an issue of **trade-offs** (cf., Matthiessen 2014b): what you achieve in one area may only be possible by giving up something in another area, or you simply have to prioritize. Ideally, you have a full-fledged description of the context in the source language culture for the source text, and similarly for the target language culture and the intended target text, and also comprehensive descriptions of the languages. You run with this when you assess the quality of the translation. But on the one hand, this can become fairly unwieldy. On the other hand, it means that you have to have full linguistic expertise to support the analysis. Significantly, with training, translators, editors or other evaluators can operate Juliane House's model, even if they do not have a full-fledged background in linguistics, or even in discourse analysis. That is at the metalevel because of the decisions that you have to take. How complex do you make the model? How workable is it if you want to work with a large number of people who are in the position of evaluating the quality of translation? Of course, Juliane House has continued to develop this model.

Bo Wang: Yes, she updated her model several times. The model is based on her Ph.D. thesis in 1976. The first version was published in 1977 (House 1977). In 1997 and 2015, she updated the model (House 1997, 2015).

Christian Matthiessen: Juliane House and her colleagues also started the research centre for multilinguality in Germany, which ran for a long time, and she was responsible for various projects within that. She was very interested in the English patterns of meaning shining through, the influence of English as a lingua franca (e.g., House 2003, 2009, 2010) and implicit translation (e.g., House 2004). So, her long-term research programme addressing different facets of translation has been very productive.

9.7   J.R. Martin and Translation Studies

Bo Wang: I have found that Jim Martin seldom deals with translation.

Christian Matthiessen: Right. I think there are various reasons. One is simply because since the 1970s, he focussed more on developing educational linguistics, which has been a huge incredibly productive effort, involving both research and application (e.g., Martin & Rothery 1986; Martin 1984; Rose & Martin 2012), but in more recent years, he has turned to translation among many other topics of research and application. At the Martin Centre at Shanghai Jiao Tong University in July, 2017, there was the International Conference on Appliable Linguistics and Translation Studies. I was not able to go because I was in Europe for conferences at that time.

Jim Martin worked with Ladjane de Souza — a translation scholar from Brazil who used to be his research student. She presented her work influenced by Jim's work in Pontifical Catholic University of São Paulo (PUC) over a decade ago at an event organized by Leila Barbara; a group of us from PolyU took part in it, including Elaine Espindola Baldissera, who has since returned from Hong Kong via Chile to Brazil. There were other people who also contributed to the investigation of translation along systemic functional lines, like Figueredo (2011), who also did his Ph.D. in Brazil, at UFMG. As part of his studies, he spent a year-long sandwich programme with us, starting with Kazuhiro Teruya in Sydney and then moving to Hong Kong when Kazuhiro Teruya came here. So happily, Giacomo was able to take part in ISFC in Beijing in 2009.

But parallel with this, we continued to develop the research into translation which we had initiated at Macquarie University (e.g., Bardi 2002; Kim 2007; Burns, Kim & Matthiessen 2009; Matthiessen, Teruya & Wu 2008). In the Department of English here at the Hong Kong Polytechnic University, there was no research into translator training because the educational work was done in our sister department — Department of Chinese and Bilingual Studies. But in terms of personal research or doctoral projects, that had been quite active here in the Faculty of Humanities and Department of English.

One of the notions of Jim Martin that influenced translation is **re-instantiation** in translation (cf., Martin 2009). I characterized translation two decades ago as the **recreation** of meaning in context (Matthiessen 2001, 2014b, 2021) partly because I was concerned to get away from the "message passing" metaphor and anything to do with transfer, which I think was a figure of speech that was quite misleading in how people think about translation. So, I was keen to come up with a characterization that emphasized the creative nature of translation.[4] Recreation obviously involves re-instantiation. You have to re-instantiate, but recreation of meaning also means re-construal, re-enactment and re-presentation of meaning across the metafunctional spectrum and up and down the hierarchy of stratification. And since meaning is theorized in SFL as meaning in relation to context, with semantics as an interlevel

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[4] In this respect, I departed from Catford (1965), who defined translation (p. 20) as "the replacement of textual material in one language (SL) by equivalent textual material in another language (TL)".

(e.g., Halliday 1973), **remeaning** also involves recontextualization, with context interpreted as a higher order of meaning (e.g., Halliday 1978; Martin 1992). While we can characterize translation as rewriting following Lefevere (e.g., 1992), who drew out the cultural implications, the key property of language and other semiotic systems is meaning, so **remeaning** is more to the point — the recreation of meaning in context, and it covers both translation and interpreting.[5] So, I would have said that re-instantiation is part of what recreation of meaning means when you do translation. But there are other specific issues in terms of Jim Martin's contribution.

Bo Wang: Yes, re-instantiation is in the title of de Souza's (2010) Ph.D. thesis supervised by Jim Martin.

Christian Matthiessen: Yes, she did a joint degree between Brazil and Sydney University. I am sure she was not aware of our work, so she did not refer to it in her presentation at PUC.

9.8   Function in the Skopos Theory of Translation

Bo Wang: Skopos Theory is also referred to as functional or functionalistic (e.g., Reiss 1971; Reiss & Vermeer 1984; Nord 1997), but text does not play a role in their theory, and the scholars are not trying to study how the global Skopos of a text is realized linguistically.

Christian Matthiessen: Right, and Skopos Theory has attracted a great deal of interest since the 1970s. It is a European phenomenon. One can trace it back to the kind of issues that **Prague School linguists** and **Roman Jakobson** were concerned with in terms of translation. Roman Jakobson was not in Prague after the early 1940s, and there were Prague School contributions on translation even during the war years (e.g., Procházka, 1942). That was a very **functional** way of thinking about translation (Jakobson's 1959 contribution is often cited; I should have cited it myself in Matthiessen 2001).

   In any case, there is this kind of functionalist thinking in and around translation. One can say that Skopos Theory falls within this broad functional tradition (e.g., Nord 2018). If you approach translation with a holistic and powerful theory of language with comprehensive and rich descriptions, then you can accommodate, redirect a reference to the text, to the system behind the text, to any notion of purpose (i.e., Skopos). Then you can relate that to different kinds of registers. There are lots of scopes for calibrating the purpose of translation or function of translation, if you approach translation with a general linguistic theory.

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[5] It is also, of course, relevant to other metalinguistic processes involving **resemioticization**, notably editing, revision, summarization — in principle, any recreation of meaning involving new versions of a text relative to the meaning potential that lies behind it (cf., Fig. 9.2).

Bo Wang: Their function comes from Bühler's (1933, 1934), who was concerned with the meaning of signs.

Christian Matthiessen: Bühler's work emerged in the European tradition, while he was still in Vienna. He proposed his famous **organon model** in his *Sprachtheorie* (Bühler 1934), which Jakobson (1960) later elaborated on. Bühler had three functions. As Michael Halliday (see e.g., Halliday & Hasan 1985) pointed out, these functions were derived from the person system as described by traditional grammarians: the function oriented to me (**expressive**), the function oriented to you (**appellative**), and the function oriented to "reality" (**representational**), i.e., first person, second person, and third person. That was not a theory based on a deep engagement with the *internal* organization of language — the **intrinsic** metafunctions (Martin 1991). It was a more philosophical and psychological theory inspired by the person system.

Jakobson (1960) ultimately added another three functions. But as Halliday (see Halliday & Hasan 1985) has pointed out, these were not functions inherent in the organization of language, but rather uses of language. We can now theorize the uses of language with reference to context. I have tried to do this from the point of view of field — the critical socialization of context in the uses of language — to see how they emerge and develop into other fields of activity (e.g., Matthiessen 2015a, 2015b): see Fig. 9.3. But the point is: they are not inherent organizing principles of language. This contrast is further illuminated by Martin (1991) in his discussion of the contrast between intrinsic functionality and extrinsic functionality. Translation scholars have tended to go for extrinsic functionality, referencing the Bühler and Jakobson tradition. However, as far as extrinsic functionality is concerned, I would say that the systemic functional account of context, including the contextual parameters of field, tenor and mode, is more informative — especially since the extrinsic functions of language identified within that framework, the uses of language, have been shown to resonate with the intrinsic metafunctional organization of language in very interesting ways, ways that are central in translation as a choice in the recreation of meaning.

Interestingly but not surprisingly, since the **textual metafunction** is an intrinsic function, Bühler did not include it in his model, but it was foregrounded by Mathesius (1975) and was the focus of the **Functional Sentence Perspective** (FSP) of the Prague School (e.g., Mathesius 1928; Daneš 1974; Firbas 1992). However, the Prague School scholars never put this together into a unified theory of the intrinsic functionality of language, instead in a sense conflating stratification and functional organization (but leaving out the interpersonal metafunction), as in Daneš (1964), which is echoed in other non-SFL functional frameworks like Dik's (1978) original formulation of Functional Grammar. Halliday (e.g., 1978, 1985) was really the first and the only scholar to do this.

9.9 Polysystem and Descriptive Translation Studies

Bo Wang: According to Even-Zohar's (1978) Polysystem and Gideon Toury's (1995) Descriptive Translation Studies, translation is studied inside the system of
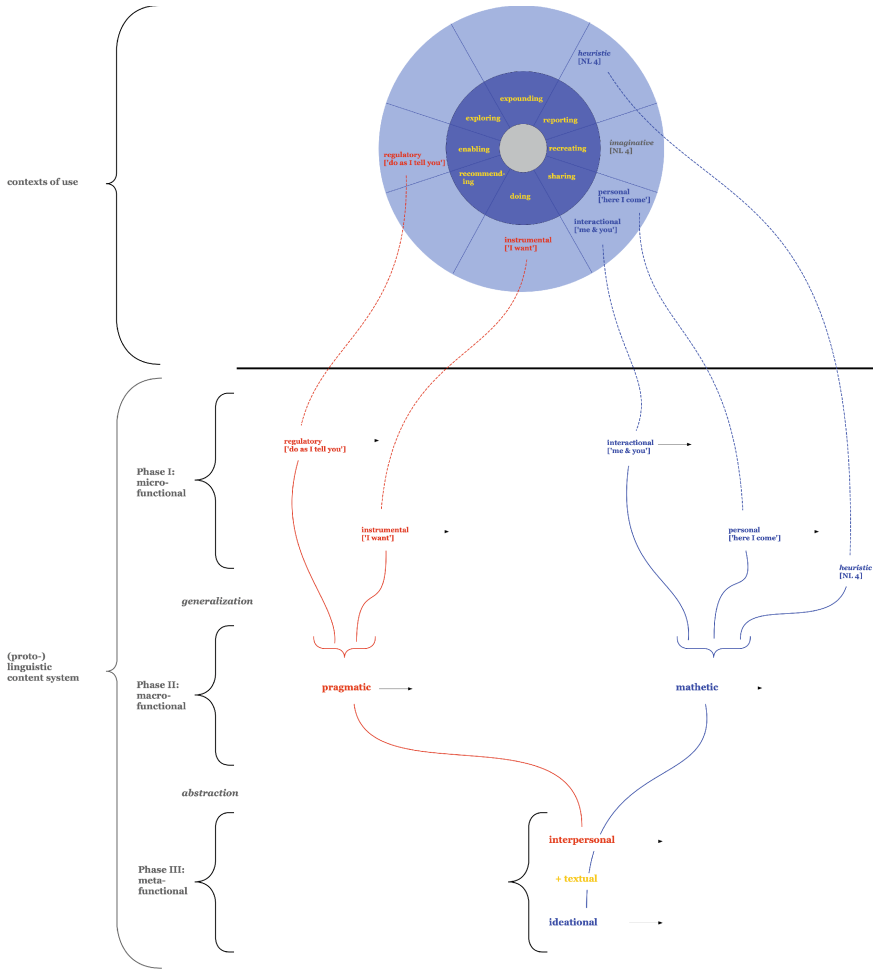
Fig. 9.3 The relation between critical contexts of socialization, characterized in terms of field of activity (see Fig. 7.8), and the ontogenesis of the functional organization of language

the receiving culture. What are they trying to do? Are their theories complementary to the SFL approach?

Christian Matthiessen: The notion of **polysystem** is interesting. That was something that Firth (1957a, 1957b) emphasized in a different context, in his conception of language in context as a system of systems. Even-Zohar (e.g., 1978, 1979) developed it when thinking about literary translation. When you translate literature, whether it is prose, drama or poetry, you will face the negotiation between cultures and the role translation itself plays as an activity or a phenomenon in the target culture, as you and Helen Ma have brought out in your books on the translation of *Teahouse* (Wang &

Ma 2020) and *Stray Birds* (Ma & Wang 2021), as have Zhang (2020) and Wang (2020) — also exciting new contributions based on doctoral theses I supervised.

If you look at European history in the last thousand years tracing the location of the centres for developing new ideas in Europe, you will find the centres moving around continental Europe and Britain. A famous example is the Renaissance, which started in Italy for various reasons but then spread gradually to other centres in Europe. Other movements have started in other areas: the centre for the movement around the printing press was Germany and the Reformation. That has been an important aspect of the European history (cf., Diamond 1997 on the affordances provided by the European landscape). During that process, translation was important partly because Latin as the language of scholarship gave way to the vernaculars of Europe; and it had of course also been crucially important during earlier phases, as when many texts were translated into Arabic. And if we look at the cultural periphery of Europe such as Sweden, we also see that translation has long played a central role in importing new ideas. More generally, we can think of this in terms of the multilingual aspects of semiotic history, involving multilingual speakers and communities, translation, the development of registers such as discourses of the law and of science as part of the evolution of standard languages. It would be very interesting to look at this in terms of translation in general, not just of literature, but also translation across different registers and the role that translation played in the target culture because Sweden was a very different place of importing and assimilating ideas. It depends on where you are at in cultural developments, whether at the centre or at one of the peripheries.

In terms of complementarity, people tend to adopt either of two approaches to combining insights coming from different sources. (1) One approach is that of the **eclecticism**, according to which you take theories and methodologies from different places and try to assemble them into a heterogenous framework, a kind of academic quilt. For example, researchers may take Conversation Analysis from sociology or Speech Act Theory from philosophy and combine them with some form of linguistic analysis, e.g., appraisal analysis or transitivity analysis. You could say this is inter-disciplinarity in dialogues across disciplines, and eclecticism is often held up as a positive feature of a given framework. (2) My own approach has been to translate insights from various sources outside SFL into the homogeneous framework of SFL. This is **metatranslation**: you translate them into whatever your own metalanguage is, in my case SFL. This raises issues of adaptation or reinterpretation, but they are interesting and productive issues leading to new insights. One reason for my preferred method is this: I can understand the totality, and I can reason about the global and local semiotic dimensions so that I will know how different insights fit. In contrast, if the approach is just eclectic, it runs the risk of becoming a Frankenstein's monster. Things do not really fit together, and it is very difficult to reason about how they are related to one and another.

For example, if you want to develop the account of the tenor parameter within context, you will find some of the most interesting contributions in social psychology (e.g., Argyle et al. 1981) and in developmental studies (Smidt 2017; Trevarthen & Aitken 2001; Trevarthen 1979, 1987). Their insights can then be translated into the

systemic functional account of context as organized in terms of the parameters of tenor, field and mode, and once interpreted in terms of tenor, the insights can be related to variation in the use of the interpersonal resources of language, based on the resonance between tenor and the interpersonal metafunction.

To approach the interesting works by the Israeli group of scholars (e.g., Even-Zohar 1978, 1979, 2005; Sela-Sheffy 2005; Toury 1985, 1995), I would go back to Malinowski (1923, 1935), and draw on his ethnographic tradition, as it has been incorporated within SFL, and of course to the emphasis on language in context as a system in the sense of the Firth-Halliday tradition; whereas others would say: let me do a separate polysystem theory analysis.

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Chapter 10
Applying Systemic Functional Linguistics to Translation Studies Around the World

Abstract This chapter is the final part of the interview on Systemic Functional Linguistics (SFL) and translation. We first comment on some works by various scholars, including Roger Bell, Mona Baker as well as Basil Hatim and Ian Mason. Then, we suggest some theoretical developments needed, highlight the developments in different parts of the world, and point out the challenges and oppositions. Finally, we examine the relationship between literary and non-literary translation in translation studies.

10.1 Introduction

In this chapter, we continue to discuss translation studies in the context of Systemic Functional Linguistics (SFL). We focus here on the applications of SFL to translation studies around the world, in terms of textbook and material development, translator pedagogy, translation process and empirical research. Christian Matthiessen explains some pertinent notions that are useful in translation studies, translation pedagogy and translation practice, namely, the notion of multilingual meaning potential, the role of context in translation and the importance of taking register into consideration. On this latter issue, he also examines the need to consider both literary and non-literary texts as equally crucial in giving us a holistic understanding of the registerial parameters that are involved in translation across different languages.

10.2   Complementarity of Bell (1991) and Baker (1992)

Bo Wang: Our first question is about a monograph written by Roger Bell (1991). In that book, he illustrates how SFL can be applied to translation studies. He approaches translation as a linguist, orients towards translation practice and tries to demonstrate how helpful a linguistic theory of translation can be. What do you think of his book?

Christian Matthiessen: It is an interesting book. It appeared around the same time with Baker's (1992) *In Other Words*, which has been used quite widely as a textbook, but I would be curious to know the extent to which Bell's (1991) book is used as a textbook. The two books are interestingly different in design as potential textbooks. You could say they are complementary because Baker's (1992) is more focussed on observing features in language in translation, and she included a textual chapter on theme in translation (see Sect. 9.4). One of the strengths of her work is the selection of languages as illustrations. Arguably, it works less well for this part of the world (i.e., East and Southeast Asia), based on the experience we had in Australia, because we had the languages that were in pair with English, Chinese, Korean and Japanese. Baker's (1992) selection is in a sense more European, though she includes Arabic.

The two books are **complementary**. What Bell (1991) tried to do was to develop a model of translation, specifying the stages of the process of translation and the resources they access, with a bit of a psychological orientation (e.g., his diagram of "A model of the translation process" on p. 59, which can be compared to architectures of machine translation systems). Among other things, he was concerned with theorizing translation. As I said, we referred to Baker's (1992) book at least when we did text analysis for translator training, and it was also used in other courses taken by the students, but I have not used Bell's (1991) book as a textbook.

My sense in terms of developing **translation theory** is: it should not be a thing in itself separated from other aspects of linguistic theory, but a part of a general theory of language. At the same time, it is essential for linguistic theories to treat translation and interpreting (and different variants, like sight translation and subtitling) as part of what linguistic theories should cover. That has certainly been the case in Systemic Functional Linguistics all along (e.g., Steiner 2005a, 2015, 2019; Wang & Ma 2021), but that is not true of all theories, remarkably—though not surprisingly, in view of the fact that modern linguistic theories have tended to be developed in major languages serving as the standard languages of nation states. For myself, what I am looking for is not a theory of what translation per se is as a separate notion, but a theory as part of the general phenomenon of language and the particular properties of multilinguality, including translation.

As an aside, Michael Halliday has pointed out that the **monolingual state of affairs** is fairly recent in human history (e.g., Halliday 1972); it accompanies the creation of modern nation states and their standard languages (although the suppression of languages of conquered communities goes further back in time, of course; see e.g., Ostler 2005). In human history, the default condition has been multilinguality (see also Sect. 8.1 in relation to multilinguality being inherent in deep-time human

history). It is a point that Evans (2010) makes in his book *Dying Words*. He takes Mornington Island in Australia, where he has done field work, as an example of this kind of situation, and makes the fundamental point that it is very common for speakers to know several languages. He says that this has most likely been the default condition for most of the human history, and it is in the very recent history of the last five hundred years or so that monolingual societies seem to have appeared. He also makes an important point that these observations should be taken into account by linguistic theories. A great deal of linguistic theorizing has been based on monolingualism. I fully agree, and that is very much the position of SFL—hence our work on developing a theory and representation of **multilingual meaning potentials** (e.g., Bateman et al. 1991; Bateman, Matthiessen & Zeng 1999; Matthiessen 2004; Teruya et al. 2007; Matthiessen 2018) and outline of **multilingual studies** (Matthiessen, Teruya & Wu 2008). So we need to ask: What does it mean to theorize translation (including interpreting[1])—as an aspect of multilinguality, viewed as both system and process, just like code-switching and code-mixing? It is part of theorizing language in general. In addition, translating and interpreting also need to be viewed together with other metalinguistic processes—drafting, editing, revising, rehearsing, etc. (see Fig. 9.2), all of which should be taken in account and supported by a general theory of language.

10.3 Hatim and Mason and Systemic Functional Linguistics

Bo Wang: In *Discourse and the Translator*, Hatim and Mason (1990) try to demonstrate how linguistic tools can be helpful in analysis. Pragmatics is also there in the book.

Christian Matthiessen: Yes, they take a fairly **eclectic** approach to discourse and discourse analysis as applied to translation; in SFL, what is covered under the heading of pragmatics is of course part of a unified but metafunctionally diversified account of semantics (cf. Sect. 9.8, Chap. 9).

Bo Wang: Are there any SFL components in their book?

Christian Matthiessen: Yes. Mason (2003) also contributed an interesting paper on the translation of European documents for Venuti's (2004) *The Translation Studies Reader*, where he showed the value of doing an SFL study by means of a transitivity analysis.

Hatim and Mason's (1990) book is fairly eclectic in terms of its coverage. It was published in the early 1990s—just before Baker's (1992) and Bell's (1991)

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[1] When there is no risk of ambiguity, I tend to use "translation" to cover both translation and interpreting. It would be helpful to have a cover term in English. Reiß & Vermeer (1991: 6) write: "Als Oberbegriff für 'Übersetzen' und 'Dolmetschen' wird der Terminus *Translation* von der Leipziger Schule übernommen" (As cover term for "Übersetzen" [translation] and "Dolmetschen" [interpreting] the term "Translation" is taken over from the Leipzig School.)

contribution. I myself still think there would be value in a book based on SFL that is about the analysis of text in context for translators or translator training. In fact, we have been working on this, and I have drafted some chapters, and it is still on my agenda to write this book because it seems to me there is value in having one book that is homogeneous from the point of view of the theory of language and the kind of analysis that supports it.

In this context, I am keen on not to insulate discourse analysis from (i) the engagement with the description of the system that one needs for the analysis, and (ii) the comparison of systems in the languages that would come into contact in translation. It is important not to lose sight of the value in comparing systems, and that is partly related to my notion of the **multilingual meaning potential** (Matthiessen 2018). The multilingual meaning potential is an account of a meaning potential of the kind that a translator, an interpreter, a bilingual or multilingual speaker would have to operate with in order to speak different languages on different occasions, to translate and interpret and to switch between or mix languages, and it is also something that a language learner builds up when he or she is learning how to mean in a new language. So, that is what I would see as my contribution to a book on text analysis for translators and interpreters.

10.4   Theoretical Developments for Future Research

Bo Wang: In our previous interview (see Sect. 8.6, Chap. 8), we talked about some possibilities of future research in applying SFL to translation, and you promised that we would talk about the theoretical developments needed.

Christian Matthiessen: Yes. One is the notion of a **multilingual meaning potential**. It is also a theoretical construct and something that is underpinned with a form of representation that allows you to develop descriptions of meaning potentials of different languages in such a way that they have their own integrity relative to the particular language—**multilingual system networks** (e.g., Bateman et al. 1991; Bateman, Matthiessen & Zeng 1999; Matthiessen 2018). You can unify the languages, integrate them, and represent the resources in all the languages. That would explain what a bilingual or multilingual speaker, translator or interpreter and language learner can do, as they build up the meaning potential in a new language. That would also explain phenomena like code-switching and code-mixing (e.g., Muysken 2000)—or "translanguaging" (e.g., Mazzaferro 2018). There are a number of linguistic phenomena where we need this notion of multilingual meaning potential.

There is also the interesting issue of how this meaning potential gets instantiated and activated. Systemic functional linguists are typically good at representing the potential in some way or another directly as a systemic potential. Other theories have tended to be syntagmatically oriented, often involving some rule system that specifies and generates syntagms or structures. In addition, linguistics has been reasonably good at representing the analysis of instances (texts in their contexts of situation).

There has been a considerable amount of work on that under headings like text analysis and discourse analysis. But there has been rather less work on **linguistic processes**, and all linguistic processes are in some sense processes of instantiation—centrally, generating texts, analysing text or a mixture of the two (but also other processes such as processes of categorizing and inferencing). Processes include not only processes of translation and interpreting, but also processes of editing and revising. In general, these processes have been given less attention in terms of the development of linguistic theory. I am not talking about transformations in traditional or transformational grammar, because transformations tend to represent potential rather than the process of instantiation—or, seen in a different light, they mix up declarative and procedural representation. This is something that has to be addressed in developing a linguistic theory to support translation and interpreting under the roles of different conditions.

Linguistic processes have been handled in **computational modelling**, as in our own research since the beginning of the 1980s (e.g., Mann 1982; Matthiessen 1989; Matthiessen & Bateman 1991), including modelling the process of instantiation involved in generating texts (see Chap. 5). Such computational models do not necessarily have to be semiotically or psychologically real (cf. the discussions of different kinds of commitment in Sect. 6.2), but in trying to model linguistic processes in a theoretically informed way, we have learned a great deal about them. For example, as part of the work on the Penman text generation system, we investigated the possibility of **parallel processing** in generation with the help of a colleague from another project who specialized in parallel processing, Tung Yu-Wen (see Tung, Matthiessen & Sondheimer 1988). What we found was that thanks to the representation of system networks, a systemic representation of the resources enabled massive parallel processing. That is, system networks do not impose artificial "rule" ordering; and they constitute a purely declarative form of representation (an exception being the "recursive" system used in descriptions of logical systems, as noted in Sect. 2.2). If you say the metafunctions are largely processed in parallel, you begin to think about what that means in terms of generation and analysis, which will surely be part of translation and interpreting. There are a great many interesting issues here, fundamental to a deep understanding of language as system-&-process. This may not be apparent to many linguists if they are not committed to explicit modelling and have no experience with it, thus they do not see the value of it. But if you are committed to explicit modelling, you need this kind of dialogue between ways of representing the resources and the processes, and theorizing what this means.[2]

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[2] Again, it is interesting to look at what has happened in machine translation. On the one hand, the communities of people working on machine translation and human translation have virtually not intersected at all. There is very little cross fertilization. On the other hand, many people in machine translation had a lot of linguistics in their works. The same is true for people in translation studies. But in machine translation, a number of systems, like SYSTRAN, are legacy systems of simply patching translation rules. It is interesting to think about what it means in terms of coming to a theoretical understanding not only of the declarative representation of linguistic resources, but also of processes. One has to be a little bit wary of talking about instantiation without actually having explicit models to take the account further. That is a very exciting part of the agenda of moving

Linguistic processes are, of course, also being investigated in "TPR", **translation process research** (e.g., Alves 2003; Jakobsen 2014, 2017), and this line of research has been combined with SFL, as in the collaboration between the group headed by Fabio Alves in Belo Horizonte, where they have extensive experience studying the translation process using keystroke logging and eye tracking, and Erich Steiner's group in Germany, where they have extensive expertise in the analysis of source and translated texts, including corpus-based investigations (e.g., Hansen-Schirra et al. 2012; Kunz et al. 2021; Steiner 2004; Teich 2003). In the collaboration, they have combined the two views on translation, e.g., trying to correlate the translator's extended or repeated gaze at certain passages of text with their linguistic features (e.g., Alves et al. 2010). The possibility of investigating the process of translation in terms of the multilingual meaning potential involved is a very promising input into future developments. As noted in Sect. 8.6, I see the same potential for the combination of neuroscience and SFL in the study of translation and interpreting (e.g., García 2019).

10.5 Researchers in Different Parts of the World

Bo Wang: Many of the researchers we have talked about are European or at least based in Europe, such J.R. Firth, Ian Catford, Erich Steiner and Juliane House. Are there scholars from other parts of the world that apply SFL to translation?

Christian Matthiessen: Yes, there are scholars and students around the world doing research into translation and training translation students in different countries in Asia, in Australia, in Latin America as well as in Europe, as illustrated in Table 10.1 (for a research synthesis of systemic functional contributions to translation studies, see Chen, Xuan & Yu forthcoming). (Although contributions on text-based language comparison and work on multilingual text generation are actually quite relevant to translation studies, I have not included them in the table.)

Translation studies informed by SFL are certainly increasing in a number of places around the world and also spreading to new places. In terms of networking and international collaboration, there are, naturally, the usual challenges, and some are perhaps particularly keenly felt in this field.

(1)   Part of the challenge is because they are **spread around the world**, it is not easy to bring them together. We hosted the 1st PolySystemic Symposium on Translation, Interpreting and Text Analysis that Elaine Espindola organized nine years ago at the Hong Kong Polytechnic University (on April 26–27, 2012). We were able to get scholars from Europe, including Chris Taylor and Carol Taylor Torsello, because Europe is not too far away (well, at least there are direct flights to Hong Kong from many cities in Europe). But to have an event

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forward with translation studies underpinned by linguistic theories. The need to be underpinned by linguistic theories does not only apply to translation studies, but also to other areas.

Table 10.1 Systemic functional contributions to translation studies in different regions

Region		Language pairs	Examples of publications
Europe	Germany	various	House (1977, 2015), Steiner (1992, 2004), Baumgarten (2003), Thoma (2004)
	Norway	English-Norwegian	Hasselgård (1998)
	Italy	Italian-English	Taylor (1998, 2003, 2013), Taylor & Baldry (2001), Manfredi (2011)
	The UK	various, e.g., English-French, English–Spanish	Halliday (1956, 1962), Catford (1965), Ellis (1966), Ure, Roger & Ellis (1969), Costa (1992), Hartley & Paris (1997), Mason (2003), Munday (2000), Calzada-Pérez (2007)
Africa		English-Arabic	Al-Kenani & Banda (2018), Sellami-Baklouti (2018)
Asia	India		Prakasam (1999: Chap. 13)
	Iran	English-Arabic	Pakravan (2004), Sichuani & Hadian (2015), Dorri (2020)
	Indonesia	English-Bahasa Indonesia	Setyaji et al. (2019)
	Malaysia	English-Arabic	Kadhim & Kader (2011)
	Thailand	English-Thai	Chueasuai (2010, 2013, 2017), Phanthaphoommee (2019)
	Vietnam	English-Vietnamese	Hoang (2006, 2019)
	China (HK)	various, e.g., English-Mandarin Chinese, English-Cantonese, English-Korean	Espindola & Wang (2015), Wang (2015, 2020), Wang & Ma (2020, 2021), Ma & Wang (2021), Wang & Tan (2014), Macdonald (2019)
		English-Mandarin Chinese	Huang (2006a, 2006b), Wang (2004), Zheng & Miao (2005)
	Japan	English-Japanese	Sasaki (1995)
	South Korea	English-Korean	Sung (2016), Mah (2017, 2018), Lee (2020)
Australia		various, e.g., English-Arabic, English-Mandarin Chinese, English-Korean	Tebble (1999), Bardi (2002), Fang (2005), Halliday (2009), Kim (2007a, 2007b, 2007c), Choi (2013), Kim & Matthiessen (2017), Wu & Yu (2019)
Latin America	Brazil	Portuguese-English	Pagano (2005), Espindola & Vasconcellos (2006), Vasconcellos (2004, 2009), Figueredo (2011), Pagano et al. (2014), Espindola (2016)

where you bring people from all these different places would be fantastic. Part of the challenge is just logistics of long-distance flights and so on—although during the COVID-19 pandemic, we have become more accustomed to online conferences, and appreciative of their ability to bring students and scholars

together from around the world who would probably not be able to afford to travel to a particular place for a traditional face-to-face conference.[3]

(2) Another interesting challenge is the **mixture of languages**. I have been to all these different communities around the world, so I would know if I go to Indonesia, the languages most likely under investigation would, naturally enough, be Bahasa Indonesia and English. If I go across the border between Hong Kong and Mainland China, the languages would be Mandarin and English. If I go to South Korea, the languages would be Korean and English. If I go to Brazil, the languages would be Brazilian Portuguese and English. How would you create possible conditions for these scholars investigating different language pairs to come together to compare notes and to share findings?[4] So, there are also challenges of the different languages spoken and challenges of finding a way for people to share experiences of working with different language pairs that might not include English. There are significant theoretical and practical challenges in bringing these communities together. But I think the potential is there.

One way forward would, of course, be to obtain the resources to investigate the translation of one or a few source texts into multiple languages, either by getting the funding to commission new translations of an interesting text or set of texts or by turning to a text that has been translated into multiple languages, such as *Le Petit Prince* or, say, *The Universal Declaration of Human Rights* or other texts that have become part of our international or global canon. The second option is more feasible, but then one would be restricted to texts that have been highly valued enough to be translated into multiple languages (for discussion in relation to language typology, see e.g., Stolz 2007—this kind of methodology being yet another reason for taking the conception of multilingual studies seriously).

I do hope we can follow up with more translation symposia around the world, including of course in Hong Kong and the Greater Bay Area more generally. We have enough critical mass—"critical" in the sense of quantum physics (not in the sense of critical discourse analysis) for very interesting and explosive developments. The engagement with SFL and translation issues is evolving. Maybe, there will be ways of intervening with some kind of design that would help researchers from different research communities bring their findings together. One can think of different ways of doing it. One is to look at registers that people have engaged within different language pairs. There are other ways as well, including inviting scholars to ensure that their examples are accessible to other people who do not speak one or other of

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[3] One such initiative bringing students and scholars together from SFL and a range of other disciplines has been taken by Hedra: https://hedra.eu.

[4] On the whole, this is working in language comparison and language typology. There are obviously various reasons for this, but a central one is that scholars working in these areas have learned how to make languages not necessarily known to all scholars accessible to them. Curiously, in translation studies, the assumption often seems to be that readers of papers should know the languages involved, so no attempt is made to make the languages accessible to readers who do not know them—critically, interlinear glossing is often not supplied, nor even transliteration where it is needed.

the languages under discussion, and there is great potential for greater collaboration. The question is: How can we develop this potential to turn exciting challenges into opportunities?

10.6 Challenges and Oppositions from Other Areas

Yuanyi Ma: While we are developing our theory, do you think there will be opposition from the community of translation studies and from people who are not acquainted with SFL?

Christian Matthiessen: No doubt. We spoke about the taboo treatment of SFL before (see Sect. 9.1). One has to be realistic and diplomatic. The worst thing one can do is to go in and say "You are all misguided. Now learn about the right way of doing translation studies". That would be completely misguided and counter-productive. We have seen such challenges in education. These challenges are interesting, and depend on the period of time as well as the particular trends (discussions, debates and battles) in an area.

In **education** in the 1980s, one of the biggest challenges was the fact that progressive writing was fashionable (as an aspect of "progressive education"). That seemed very difficult to combine with the notion of conventionalized registers and genres, so the progressivists were in opposition for a long time. Now I would say the issues have been settled: the "**genre pedagogy**" prevailed for the simple but powerful reason that it delivered very positive results (cf. Rose & Martin 2012), and mastery of the collective semiotic resources of a community is in fact a prerequisite for individual creativity. (As a relevant aside, we need more studies of creativity interpreted in terms of semogenesis; a recent brilliant example informed by SFL and Carter 2004 is Law 2017.)

The issues in areas other than education are different. If you go into the area of **computational modelling**, the **technicality** of SFL will not be a problem at all even though it has been seen as one in educational contexts. They would be very happy to pick up and try to model the grammar computationally. When you come to semantics (including of course Martin's [1992] "discourse semantics"), then the accounts are less explicit than those provided for grammar. So, it is harder to develop these accounts in the context of computer science. In addition, since semantics is at one remove from the core of the strata of "form", the strata linguists have tended to be left alone to take account of, you will find that researchers in AI and computational linguistics have already developed their own models because they did not find anything on offer in the kind of linguistics they engaged with.

But if you go into **translation studies** as an institutionalized discipline, then technicality could be a big issue in engaging with language. In different border territories, you will have different issues. One solution is to talk to people in **machine translation** because they will not have a problem with technicality, and they realize that it is needed. In some sense, that goes back to what we discussed with respect to

Erich Steiner (see Sect. 9.3), and his contribution to the EUROTRA project in the 1980s. There were also other people, including John Bateman (e.g., Bateman 1997; Bateman, Matthiessen, Nanri & Zeng 1991; Bateman, Matthiessen & Zeng 1999), who tried to take our experience with **multilingual text generation** and relate it to challenges in machine translation. That may be one way of doing it when people in human translation studies get interested in machine translation or take the results from this area of research seriously.

One common theme is: the extent to which they are prepared to take **language** seriously. If you take language seriously as a general human system, you have to take on the awesome complexity of language. As Halliday (e.g., 1966, 2008) has said, if you pretend that language is not complex or that you can have a simple account of this ferociously complex system, nothing good will come out of it. The complexity of language is mirrored and reflected and theorized in SFL, so people may balk at the complexity of SFL—but the source of it is language, the phenomenon being investigated. What you do to address the difficulties in engaging with researchers depends on the interface. If you are in the enterprise of developing medical expert systems, which became a task in artificial intelligence in the 1970s, of course, researchers would not say: "Let's not talk to doctors because their knowledge on medicine is too complex. Let's go to folk theories of illness". Instead, the AI researchers building medical expert systems realized that the onus was on them to understand and then model the expert knowledge doctors provided them with—obviously, this was not even an issue. But when it comes to language, some scholars would seem to think it is reasonable and ethical to say: "SFL is too complex. Let's turn to something simpler." That is not very rational; if such views seem outlandish, I'd invite inspection of one or two quotes from "specialists" in de Bot's (2015) excellent history of applied linguistics. Such views can presumably only be given by scholars who are secure in terms of privilege and power (within the tenor parameter of academia).

The way of countering this common attitude is this: go to a classroom of primary kids, and you will quickly discover that they love the technicality empowering them to engage with language and have no problem with it (e.g., French 2010, 2013; Williams 2005). It is funny in that it is perfectly okay for a textbook on human anatomy to be technical and full of Greek or Roman technical terms. But if you talk about language, suddenly it is not all right. In the educational context, people may say: "Traditional grammar is simpler". My response is: read a real traditional grammar (not a watered-down school grammar), and you will find it is quite complex—even though it is analogous to pre-Newtonian physics (so quite limited in terms of its insights, essentially reflecting the understanding before the era of modern science). Naturally, for scientific accounts of any phenomenal realms, we need "translations" for readers and listeners who have not been initiated into specialist circles: the ideational construal of "knowledge" in different domains is always adapted according to tenor considerations (cf. Fuller 1995).

10.7   **Literary Versus Non-literary Texts in Translation**

Bo Wang: Literary translation has played a dominant role in translation studies for a long time (cf. Tymoczko 2014), but it is only a small area in the whole map. How can we take other text types or registers into consideration?

Christian Matthiessen: To begin with, I think Tymoczko (2014) is right to point out that the translation of literature has played an important role in the development of translation studies, including contributions coming from linguistics. But I think she is wrong in her characterizations of literature (p. 15):

> A general characteristic of literature in human cultures is that literary language is language writ large. It is characteristically rich and complex, exemplifying the full range of the linguistic phenomena and capacities of a particular human language. No other corpus of texts can make this claim.

"Literary language" is simply one of many registers, or functional varieties, of any given language; it is not "language writ large". While the register of "literary language" is certainly very extensive and varied and fascinating and often privileged in high culture and academia, it can be characterized in very general terms as "verbal art" and broadly differentiated from other such macro-registers like scientific language or casual conversation. But many other registers are "characteristically rich and complex"—just in different ways; casual conversation tends to be much more complex than literature in the sense of **grammatical intricacy** (although it is more likely to be "interpreted" than "translated"), and scientific language is much richer in its deployment of **ideational grammatical metaphor**, as well as lexical metaphor, to construe domains of knowledge. The way forward is definitely through **register theory**—the theory of the functional variation of language according to the context of use. The basis for theorizing translation should most decidedly not be "literary language" but it should be multi-registerial and include register variation as a central consideration (cf. Steiner 2004, 2005b).

The need to treat register variation as central in accounts of translation was something I suggested based on our experience in Sydney at Macquarie University (and it has of course been foregrounded by various translation scholars, e.g., Snell-Hornby 1995). It was very similar to what happened in second foreign language learning. The curriculum should be staged according to registers or genres, in the way that Heidi Byrnes and her colleagues did for the German undergraduate curriculum at Georgetown University (e.g., Byrnes et al. 2006; Byrnes, Maxim & Norris 2010). That meant students were exposed to the task of translating different registers in a progressive way, and the curriculum was designed to expose students to different degrees of difficulty through different registers.

Here is an anecdotal experience. The first time I went to Korea University to deliver our course on text analysis for translators (in intensive mode of over two weeks) as part of our programme at Macquarie University, I asked the very nice excellent teachers there who dealt with translation practice if they could give me a sample of the texts they used in class. They were very helpful. But it turned out that

virtually all the texts were media texts. Why would that be? Because such texts were very easy to access. I thought this was not a good reason. A person's multilingual meaning potential is distilled from an exposure of a wide range of registers, being built up successively from engagement with texts from different registers, and we need to build this into the curriculum of translator training. Learning to translate means pushing the frontier of your mastery in the second/foreign language or the third language. In that sense, translation is one of the hardest and the most challenging semiotic tasks you can undertake, and it would invite you to continue to progress in the second/foreign language and actually also in the mother tongue. In order to recreate the meanings, you are pushed to go into the meanings as deeply as you possibly can. This is also something to be taken into account as you try to develop a curriculum for a translation programme. As far as I know, there have not yet been attempts to develop a curriculum built around a registerial map (cf. Matthiessen 2015). That would be a very productive practical possibility.

What happens is this: if you are talking about registers, you are always talking about both the source and the target contexts. In a context like the European one, there are lots of targets. In machine translation, there was the EUROTRA project (e.g., Steiner 1986; Steiner et al. 1988). This project again reminds us that translation is **a many-splendored thing**.[5] Translation is not just one task or one process. It includes many different ones according to the context of the source text, the context of the target text and the context of translators undertaking translation. That is why there is value in trying to model the context of translation as well. For instance, a number of efforts in information retrieval were central in the development of machine translation in the 1950s and the 1960s. In the U.S., people were aware that there was a great deal of technical and scientific information in Russian documents, so the task was not to create beautiful readable translations, but instead to retrieve information.

If you have a large volume of texts, it is a good task for a machine translation system just to extract information; and of course nowadays, machine translation has reached the stage where we can routinely view and read translated web pages (or even engage in real-time dialogue supported by translation systems). That is very different from translating poetry, textbooks in neuroanatomy or advertisements. If you look at the contexts of the source and the target text in terms of field, tenor and mode, you can begin to recognize and characterize various kinds of translation. I often suggest to translators to do interlinear glossing—a very specialized kind of translation. Many professional translators cannot do this because they have not been given the linguistic training needed for this highly analytical technical task, but they will learn a great deal by doing it. Then, in addition to the contexts of the source text and the translated text, there is also **the context of doing translation**—the **meta-context** if you will: see Fig. 10.1. If you consider the translation tasks extracting information from the source text versus producing effects through the translated text in the target culture, there are issues related to how you see yourself in the context

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[5] This is an allusion to a film set in Hong Kong from the mid-1950s called *Love Is a Many-Splendored Thing*. In the same way, translation is a many-splendored thing.
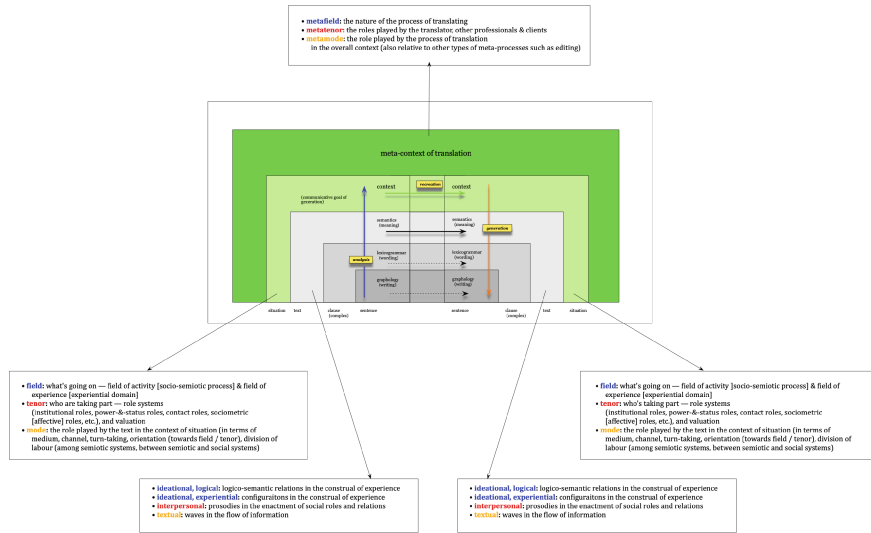
Fig. 10.1   The contexts of translation—the context of the source text, the context of the target text and the context of the process of translation

of translation. This is, again, related to the register you translate, like the role of translators of advertisement as mediators.

There are other issues like whether the translator works on his/her own or not, whether s/he works in a team or not, whether s/he works with editors or not, whether the editor produces the final result or not, etc. One project we had in the early 1990s, led by Gunther Plum, was concerned with the quality of Fujitsu manuals translated from Japanese into English, which were then sold together with the Fujitsu products in Australia, where customers found again and again that while they liked the products, the translated manuals were not good. There were a number of reasons, but one of them was that the manuals were written in Japanese by the technicians in Japan, who were not necessarily tech-writers. Then, the manuals were translated by professional translators, and then edited in Australia by professional editors. But these people did not work as part of a collaborative team. The editors produced the end result, but they probably had no expert knowledge about the technology. That is why we need to think about the translation process in terms of the whole team involved.

Yuanyi Ma: I agree that context is very important. In the translation exercise in the textbook I used (Miao 2017), there is a dialogue on the departure of business partners. The company staff who sees the partner off says: "I hate leaving you". It should not be said in that context, and that sounds very weird. It is an interesting example that tells us the importance of context.

Christian Matthiessen: Yes, context is very important. "I hate leaving you" perhaps evokes a more romantic tenor relationship (I can imagine this being part of an alternative version of the ending of *Casablanca*, spoken by either Ingrid Bergman's

character or Humphrey Bogart's); but if we imagine that the business partners are departing from their hosts, leaving them to deal with unsolved problems, then "I hate leaving you", with the implication "to deal with the unsolved problems on your own", could make good sense and not sound weird. Context is multidimensional and richly variable, and the wide range of possible settings may be overlooked in the philosopher's armchair where examples are invented. Let me generalize. When translating documents that are highly valued in a community, we must take into account that these documents may be regarded as sacred. It could be the Bible, the Qur'an, the Bhagavad-Gita, or any comparable text. But you have special challenges there, and Nida has written interestingly about this (e.g., Nida & Taber 1969; Nida 1964, 2001). The same thing will arise when you are translating across centuries when the source culture no longer exists. So, there are interesting challenges.

I remember one talk at a conference on computational linguistics in Japan in the early 1990s (Anwyl et al. 1991). There was a team from one of the laboratories in Japan working on machine translation. They had come to the realization that even for machine translation, they still had to distinguish between different translation tasks based on the context of translation. One major dichotomy they came up with was: Is the translation source language driven? Or is it oriented towards communication in the target culture? If it is oriented towards communication in the target culture, the source text may actually serve as only one source, to be complemented by other sources. So, you are actually operating, in some context, in the target culture (cf. again Steiner's 2004 study of the translation of advertisements). You use the text from another language in its context, but you may use other sources as well. This further illuminates the many-splendored nature of translation, and it takes us away from anything to do with transfer or message passing or anything that is a unified single-type conception of what translation is. So, there are a great many possibilities.

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Epilogue

Christian M. I. M. Matthiessen

There are many ways to write an article or a book. Sampling writers whose main output was verbal art—different literary registers, Stodola (2015) examines their writing processes and working habits, and mode of living more generally. She classifies the writers under nine headings: "nine-to-fivers" (e.g., Franz Kafka), "productive procrastinators" (e.g., David Foster Wallace), "autodidacts" (e.g., Edith Warton, George Orwell), "plotting ahead" (e.g., Virginia Woolf, Vladimir Nabokov), "winging it" (e.g., Salman Rushdie, Joan Didion), "the author as protagonist" (e.g., Jack Kerouac, Ernest Hemmingway), "slow and steady" (e.g., James Joyce), "the social butterfly and the lone wolf" (e.g., F. Scott Fitzgerald, Philip Roth), "two takes on the digital age" (e.g., Margaret Atwood).

As a team, Helen, Wang Bo, Ike and I probably have a number of these traits—with the exception of lone wolves and only a small flutter of social butterfly, quite possibly alternating over time. By design, this book has a fair dash of "the author as protagonist"; from the start, it was based on "plotting ahead"—sometimes the process has been "slow and steady" (plodding ahead), sometimes we have been "productive procrastinators"—mostly because nine-to-five academic tasks have intervened, although procrastination becomes productive when it leads to a fresh return to a manuscript. Significantly, Stodola chose to focus on "great authors"—and we are in a different class (and unlike Churchill or Russell, not the class remotely likely to be given the Nobel Prize for literature).

One great author whose approach to writing seems very appealing to me is not included in Stodola's book: Somerset Maugham. He would spend the first half of the day writing, and then he would have lunch and spend the rest of the day in ways that would be stimulating and energize continued semiosis—an approach that would be very good for the creativity of academic brains in dialogue as well (the social butterfly effect). And of course, he trotted around the globe, experiencing many different places, engaging with many different groups of people and listening to people and re-semioticizing stories they told him. (His short stories based on his

travels in our part of the world—the part where we started our dialogues—have led to a characterization of him as a chronicler of a late phase of the British Empire, producing vignettes that are often far from flattering.) Somerset Maugham soberly considered himself a B tier writer; but this view can certainly be disputed—see e.g., Blackburn and Arsov (2016).

My reason for this short excursion into the realm of recreation—the realm populated by writers of imagined worlds, writers of fiction, is that I think it may be helpful to explore different approaches to writing—not only of verbal art, but also of academic discourse. As far as academic writing is concerned, the variation in approaches touched on above are highly relevant; one should experiment, and try to find one's own semiotic groove—including different modes of co-authorship.

In *Zen and the Writing Life*, a distant relative of mine (we both hail from the first Matthiessen on the Isle of Föhr—Matthias der Glückliche), Peter Matthiessen, provides us with many insights and pieces of practical advice that are quite relevant to linguists trying to produce academic discourse—one key strategy is to be aware, to observe and to keep a good notebook of observations and ideas. His multi-registerial output can also serve as a source of stimulation—short stories, novels, verse narratives, nature and travel texts (interleaved with passages of spiritual insights, like *The Snow Leopard*) and ecological writing, tracts in defence of the rights of indigenous members of our human family, and arguments against US warmongering. His work underlines the point that writers can pursue a sense of social accountability from different vantage points and through different registers; thus Peter Matthiessen and Michael Halliday are complementary, providing models of social accountability and approaches to ecological awareness deriving from different sources and pursued through different ranges of repertoires.

As a book growing out of interviews conducted by my three co-authors, this volume represents a collective effort. Through their questions, Wang Bo, Ike and Helen have provided a kind of semiotic scaffolding. This is perhaps more clearly noticeable in the recorded interviews and the transcripts. With successive revisions, we have drifted further away from the initial interviews; but this is both a natural and desirable aspect of the semogenesis in this book. Hopefully the result is readable and useful even though it has a hybrid quality of a transcript of exchanges in interviews over a reasonably long period of time and written book with long monological passages. It is still the case that the overall organization of the book, as well as of each chapter, was determined by my co-authors.

In my own experience, interviews with scholars can be very helpful in large part because they include questions that might not otherwise have been addressed. This first occurred to me when I read Herman Parret's (1974) interviews with leading scholars of language a few years after it had appeared; for example, through his questions, he invited Michael Halliday to speculate on the relational organization of semantics and to comment on generative semantics in contrast with Chomsky's generative linguistics. These were topics he had not really elaborated on in his "monologic" writings at that point. I hope that there will be topics—preferable interesting ones—of this kind in our book.

Naturally, I worry that there are passages that are too centred on my own personal experiences ("the author as protagonist"), and thus of little or no general interest. So this has been one question I have asked my co-authors; and in responding, they have been very kind, considerate or polite—very positive values in terms of dimensions within the tenor parameter of context. Still, if our dialogues about my own background can stimulate ideas about how to generate interest in language as a central human resource, I would be delighted. One of my modest dreams is that linguistics will be introduced as a subject in secondary school curricula around the world. I know that there have been considerable successes with grammatics (i.e., the study of or the science of grammar), even in primary school, which is very encouraging; but I mean engaging with language as a key human system in general—in many if not all its manifestations. But it obviously needs to be done in such a way that students are fully engaged—captivated by new insights into language, and into the languages they speak or are trying to learn. Captivated, so energetically engaged, and then also empowered. This book will clearly not reach many of the people who could make this happen, but it can hopefully serve as a kind of witness account of what makes the study of language intriguing to an adolescent and to a young adult. This book is certainly not a linguist's autobiography—that would be another book—it could perhaps make a minor placeholder addition to autobiographic books by Otto Jespersen (1938), David Crystal (2009) and Charles Li (2009)—and shorter accounts by many linguists including British linguists in Brown and Law (2002).

As I noted above, the interview topics were chosen by my interviewers, and while I may have meandered at times, I have followed their chosen routes through the territory. There are plans for another volume or more based on subsequent interviews, so the coverage in this volume will be extended to other areas of activity. But while the coverage may seem somewhat eclectic, the common theme for me has always been Systemic Functional Linguistics (SFL). It has become my native metalanguage, my mother-meta-tongue (MMT). This has, in a sense, been an ongoing choice because I have kept moving from one academic environment to another where other traditions in linguistics—approaches to language—flourished or even dominated. At times, SFL has been an endangered metalanguage, but right now it is being taken up by new users in an increasing range of places around the world—always in interaction with users of other metalanguages. My own approach has always been to translate insights from other scholarly communities into SFL, since this has been a way for me to integrate such insights with existing ones in SFL and to reason about them holistically—and to expand the resources of SFL along the way.

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