From All Possible Worlds to Small Worlds: A Story of How We Started and Where We Will Go Doing Semantics

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Abstract

This is a short story of how we have evolved over the last 40 years, doing semantics. It could partially overlap with a history of PACLIC which is commemorating the 25th year or a quarter of a century of its founding. The story tells how we semanticists of natural language moved from all possible worlds to small worlds, now living in and with a tiny mobile world.

1 Introduction

The story goes back to the early 1970s with generative semantics and the dawning of Montague semantics. The beginning was concerned with big open worlds, all possible worlds, for truth meant, in the eyes of philosophers, being true in all possible worlds. And linguists inherited their notion of truth in constructing a formal theory of natural language semantics. In the 1980s, however, the focus of linguistic semantics changed from necessary or possible truth to something more contingent or informative, namely various sorts of information obtainable from small worlds, called *situations*. A new trend developed in the 1990s towards the computational modeling of semantic theories, based on so-called *real* language data or large corpora such as BNC (the British National Corpus). This required various situations of language use to be constrained with an idealized set of conditions. Then around the turn of the second millennium, semanticists have followed a data-driven approach to the construction of their model-theoretic semantics, which requires a large amount of language resources or raw corpora tagged with a variety of information, both morpho-syntactic and semantic. As a result, some semanticists including myself have proposed doing semantics using annotated language resources, which was known as *annotation-based semantics*.

My story will narrate how our colleagues have reacted to all these changes. Not being an historian, however, the speaker dares not guarantee his view to be fair and objective. Instead, it will be very subjective and introspective. Hence, it will simply be head-driven without being data-driven. I, as an old member of the PACLIC community, justify this narrowly defined role of an invited speaker because I trust that other PACLIC founding members, Benjamin T'sou and Akira Ikeya, will balance whatever might be one-sided in my talk.

2 The 1970s: Truth and All Possible Worlds

Every decade has its own exciting moments. To me, the 1970s must have been the most exciting decade in my life. In the summer of 1971, I went as a Fulbright student to the University of Texas at Austin, hoping to specialize in machine translation and the theory of translation. When I got there, there was no trace of such a thing as machine translation with all the projects and the people gone away and with nothing left of MT. Professor Winfred P. Lehmann, an outstanding historical linguist and a pioneer in machine translation, who had initiated the Texas MT project, was still at Austin, running the Department of Linguistics and making the Department rank Number 2 nationwide or globally in the area of linguistics along with UCLA after MIT around 1970.

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So I ended up the Department of Linguistics, writing a doctoral thesis in an area that had just begun, known as *Montague grammar*, much later *Montague semantics*.

Till early 1970s, semantics has failed to be recognized as a proper part of linguistics arguably because it could not be evaluated quantitatively or its data was not measurable or simply because it could not be a subject matter of empirical science. At Austin, Texas, however, we had a wonderful group of linguists who would become forerunners of formal semantics: Emmon Bach, Stanley Peters, Bob Wall, Lauri Karttunen, and David Dowty, but none of them offered any course in semantics, when I arrived there. Emmon taught syntax and I loved his way of raising issues and urging his students to think, although most of my classmates didn't agree with my pleasure of sitting in his class. Stanley taught mathematical linguistics, while Bob was gone on his sabbatical. Lauri was supposed to create a course in semantics or pragmatics, but he hadn't had any students till or just before the summer of 1973 when Texas would be hosting the first Performadillo conference with its theme on pragmatic presupposition and *implicature*, a term coined by H.P. Grice (1967). David Dowty was in the last years of his graduate study, finishing up his dissertation (Dowty, 1972) on aspectual features (e.g., progressive) of predicates based on generative semantics, if I remember correctly.

One day in 1972 or so, Stanley Peters came back from a conference on the West Coast, USA, with a thick typescript written by Barbara Partee. This was Barbara's first introduction to Montague grammar with its focus on the categorial grammar-based syntax, for her first effort was to synthesize Montague and generative grammars. As his research assistant, my sole assignment was to read that typescript. I was, however, more interested in the formalization of semantics with a type-theoretic lambda calculus and so-called *model-theoretic semantics* mainly because they sounded more challenging or because these were the things that I had not known about. At that time, however, there was no one around who could help me understand all this stuff. I exchanged a couple of letters with Barbara Partee and her replies were great. Bob Wall, who was my dissertation supervisor, had set up a course in intensional logic specifically to help me with a student named Tom Hester, who had studied philosophy. Stanley Peters gave me two tutorial courses, one of which was modal logic, using Hughes and Cresswell's (1968) wonderful book on modal logic. Those courses were tutorial because no one else wanted to study such a thing as modal logic or anything that had to do with mathematics or logic. What I have learned from these courses became a basis for me to go through Montague's PTQ (Montague, 1973), EFL (Montague, 1970a), and UG (Montague, 1970b) almost by myself.

My paper, entitled "Negation in Montague Grammar", was accepted for presentation at the Tenth Meeting of the Chicago Linguistic Society in 1974. My presentation was a disaster, I think, because it consumed most of the allocated time explaining one single translation:

(1) **everyone** $\Rightarrow \lambda P \forall x[human(x) \rightarrow P(x)]$.

The formula looked worse because it still had the capped $\hat{}u$ for the (individual) concept or intension of an individual variable u or the de-capped $\check{}x$ of an individual concept variable. So I didn't have enough time to explain how to treat quantified sentences like:

(2) a. Everyone didn't come
    b. Not everyone knows everything.

in Montague's PTQ or any other more interesting issues involving negation. Nevertheless, Bob Wall as my supervisor devoted himself and his whole summer to help me to finish my doctoral thesis and receive a Ph.D. within three years after coming to Texas. I was just in time to get back to my university in Korea to resume my professorial responsibilities, for I had only three years' leave of absence from my university in Korea. When my family and I arrived in Tokyo on our way home to Korea, a telegram had been waiting, telling me to get back right away.

Here I should mention Roland Hausser, for he and I have been working together all our life since our Texas days. He and I have helped each other to finish our doctoral theses and we are still proud of ourselves being two of the three first ones, including Michael Bennett, to write a doctoral thesis on Montague grammar. That was August, the

hottest summer month, of 1974 in Texas, while Michael was enjoying his cool summer in California. Roland discussed quantification in the framework of Montague grammar, whereas I explicated PTQ and treated some English constructions as a non-native speaker. Unfortunately none of us had an opportunity to join the inner group of Montague grammarians, for both Roland and I had to leave the United States and found it hard to travel back to the States, while Michael passed away early in his career because of his ill health.

Happily back in Korea, I found a nice group of excellent linguists trained in the United States. Among them were three semanticists: Suk-Jin Chang from Illinois at Urbana-Champaign, In-Seok Yang from Hawaii, and Chungmin Lee from Indiana. They had received their Ph.D.'s in 1973 or a year earlier, but that was too early for them to have enough time to work on Montague grammar. Instead, they followed the group of generative semanticists such as Jim McCawley, George Lakoff, Paul Postal, and Haj Ross. Nevertheless, they have been the most influential persons in Korea to persuade me and, five years later, Ik-Hwan Lee, who also received his Ph.D. from Texas in 1979, to propagate Montague grammar in Korea.

In the winter of 1975, In-Seok Yang organized a small workshop supported by the Fulbright Commission in Korea and invited me to conduct a one-week or ten-day seminar on Montague grammar, using my dissertation as a textbook. For that workshop, a dozen of us stayed at a Fulbright hermitage at the Academy House in the north-eastern mountain ranges of Seoul. With a larger group of linguists, this seminar was resumed six months later again with its focus on Montague.

After these seminars, Professor Yang proposed the founding of the Linguistic Society of Korea. With his nomination, Professor Suk-Jin Chang was then elected its first president. Meanwhile, Korea invited Emmon Bach, Barbara Partee, and David Dowty to give lectures on Montague grammar. At that time Barbara was fully occupied with importing Chomsky's transformations into Montague grammar, as exemplified by two of her papers, "Some extensions of transformational extensions of Montague grammar" (1973) and "Montague grammar and transformational grammar" (1975). When I was asked to comment on her lecture given at Seoul National University, I had to state regretfully that my post-lecture comments were to be replaced with a series of questions that I had already asked during her lecture. I was afraid to keep asking questions or making negative comments because I personally preferred to do Montague grammar or any other grammar without any transformations.

Montague grammar was not a fully developed grammar of natural language, for it did not have any phonological component nor a lexical component. It did, however, contain a small list of interpretation rules, called *meaning postulates*, the original idea of which had been proposed by Rudolf Carnap (1952). Montague (1973) introduced them as constraints on a set of possible worlds or models that delineate so-called *admissible* worlds. Natural language semanticists such as David Dowty caught on this notion of meaning postulate and developed it to a full set of lexical decompositions, as had been discussed in generative semantics with examples like the verb **kill** being decomposed into a logical form

(3) $\exists\{M, x, y\}[M(x)\, cause'[become'[\neg alive'(y)]]$.

These endeavors were well represented by Dowty's (1979) *Word Meaning and Montague Grammar* or his earlier work (1976) "Montague grammar and the lexical decomposition of causative verbs". In contrast, Partee's (1976) *Montague Grammar* represented other efforts to extend Montague grammar. Michael Bennett's "A variation and extension of a Montague fragment of English" and Rich Thomason's "Some extensions of Montague grammar" both of which are included in Partee (1976), are good examples of how Montague grammar was explicated and extended.

The decade of 1970s was dominated by Chomsky's transformational grammar. Partee's (1973) "Some transformational extensions of Montague grammar" or Bach's (1979) "Montague grammar and classical transformational grammar" were typical examples of how Montague grammarians responded to Chomskyan linguistics. In our PACLIC group, however, we were freer to accept non-transformational approaches to syntax or grammar in general. GPSG, HPSG, and LFG were well accepted both in Japan and Korea. As I mentioned earlier, Takao Gunji produced JPSG for Japanese.

Byung-Soo Park started to turn his Kyunghee University into the oriental mecca of HPSG, while Soo-Song Shin of the German Department of Seoul National University was a strong believer in LFG.

Before moving over to the next decade, I should explain why I have called the 1970s as the decade of truth and all possible worlds. When we inherited truth-conditional model-theoretic semantics from philosophers and logicians, we also inherited their concerns: the notions of truth and possible worlds or models. For them, meaning meant truth or truth with respect to some model or a possible world in a model, while validity meant truth in all possible worlds in a model or in all models. The interpretation of negation, disjunction, quantification, and modality all involve truth and possible worlds or circumstances. Montague's PTQ itself hardly talks about truth or possible worlds. In other papers, however, Montague claims that the aim of semantics is to formulate truth conditions and entailment relations.

Most linguists of natural language semantics are fully aware of what Montague or formal semantics should be concerned with. In those early years, however, we had not been exposed to many of the important works by analytic philosophers or philosophers of ordinary language. We knew almost nothing about Alonzo Church's lambda calculus nor of his type theory, and very little about Alfred Tarski's truth-conditional semantics or Rudolph Carnap's meaning and necessity. We read little about David Lewis and Gilbert Harman, one or both of whom said that the construction of logical forms or semantic representations, as done by generative semanticists, was not doing real semantics, but playing with Markerese or some artificial language, while generative semanticists were trying to apply or enrich first-order quantificational logic to represent ambiguity or inferences in natural language. McCawley (1981)'s famous book *Everything that Linguists Have Always Wanted to Know about Logic (but were Ashamed to Ask)*, however, was a result of such efforts to help linguists to learn logic and do semantics. Montague (1970a) himself stated that natural language semantics could be developed without going through the process of translating natural language to some formal language, an intermediate language, as shown in EFL (Montague, 1970a). Nevertheless, to do semantics or formal semantics, linguists had to learn all sorts of logics, higher-order logics and modal logics for both epistemic and deontic modalities.

While trying to cohabit with philosophers in the universe of all possible worlds, formal semanticists of natural language or Montagovian semanticists were mostly occupied with the translation of some fragments of English or some other languages into intensional logic with a type-theoretic lambda calculus. One minor, but most important revision of intensional logic was to get rid of the type of *individual concepts*, as illustrated by Montague's example (a):[1]

(4) a. The temperature is 30, but it's rising.
b. My son tries to go up the tree [literal], while my blood pressure is going up [metaphoric].

Here, *the temperature* was treated in PTQ as denoting an extensional entity of type *individual*, tagged `<e>`, while the *it* was treated as denoting an intensional entity of type called *individual concept*, tagged `<s,e>`. K. Lee (1981) tried to save the notion of individual concepts unsuccessfully, for the inclusion of individual concepts simply complicated the representation of semantic content in general. While the notion of intension or the distinction between extension and intension, the ambiguity between *de re* and *de dicto*(opaque) readings played no central role in the analysis of natural language, the λ-operator with the β reduction has become a powerful descriptive tool and remains as such to this day. This little tool helps to treat such linguistic phenomena as:

(5) a. Deletion: `John tried PRO to fly.`
$\lambda PP(j)(\lambda x[x\, tried\, \lambda y[y\, to\, fly]])$

b. Coordination:
`John`$_i$ `sings and x`$_i$ `dances well.`
$\lambda PP(j)(\lambda x[x\, sings\, and\, x\, dances\, well])$

c. *wh*-constructions:
`Who do you think t loves Mary?`
$\lambda x[do\, you\, think\, x\, loves\, Mary]$

d. Quantification:
`John and every student of his`

[1] Comparing (a) with (b), we could have treated Montague's example (a) much differently.

wanted PRO to run a marathon.
$\lambda P \exists x[P(j) \wedge x = j \wedge \forall y[student(y, x) \rightarrow P(y)]]$
$(\lambda z[want(z, \lambda w[run\_marathon(z)])])$

e. Coreference:
John$_i$ loves his$_i$ mother.
$\lambda P P(j)$
$(\lambda x \exists y[loves(x, y) \wedge mother\_of(y, x)])$

and many other interesting phenomena in language. Knowingly or unknowingly, the little lambda operation (λ) allowed those abstract entities, called PRO and *trace*, to be introduced into syntax or the extended version of Chomsky's generative transformational grammar.

While finding it difficult to construct a model-theoretic semantics of fragments of natural language, we linguists have found it easier to accommodate Frege's notion of compositionality. This was so, especially because we have known about recursivity in generative syntax, introduced by Noam Chomsky, or when we were playing with the BASIC programming language, as in the following:[2]

(6) a. PS rules
S $\rightarrow$ NP VP
NP $\rightarrow$ NP S

b. Home Rules
#1. Wife, the Boss.
#2. Go to #1.

and also the notion of projection rules for semantic combination, introduced by Katz and Fodor (1963). We were fascinated with the so-called *homomorphism*, structural resemblance or one-to-one correspondence between the syntactic rules and their corresponding interpretation (semantic) rules or the rules of translating a natural language to a formal language such as intensional logic. We thus extended Montague's PTQ to other fragments of English or other languages. I myself tried to construct something called AMG, Augmented Montague Grammar, to accommodate case marking phenomena in Korean. Many of my colleagues were more ambitious and successful to extend categorial grammar as an alternative to Chomsky's generative grammar, then based on his *Aspects* theory called the *Standard Theory* or later called the *Extended Standard Theory*. Montague grammarians could not follow Chomsky, when his theory became *Revised Extended Standard Theory* with an acronym *REST*. Our late Professor In-Seok Yang jokingly predicted that time had come for Chomsky to rest with his 1982's *Government and Binding* theory that might apply to the conditions and rules of dictatorial regimes as well as of linguistic theories. I should, however, note that our European colleagues around Amsterdam were more successful in constructing model-theoretic semantics or doing real semantics for natural language. One prominent contribution was made by Daniel Gallin's work (1975) *Intensional and Higher-order Modal Logic with Applications to Montague Semantics*. Most of their efforts, however, were known later, in the 1980s and 1990s. Theo Janssen's work on Montague grammar, for instance, was published in 1983. I should also mention Harry Bunt's work (1985), *Mass Terms and Model-Theoretic Semantics* that discussed the distributivity of quantified events with examples such as:

(7) The two old men swallowed a beer and lifted the piano upstairs.

This and other similar examples are still discussed among semanticists.

3 The 1980s: Situations and Small Worlds

Again I will begin to talk about the 1980s by narrating what started to happen around me in Korea. In mid-summer 1981, the First Seoul International Conference on Linguistics (SICOL-1981) was held in Seoul. It was organized by Professor In-Seok Yang, the third president of the Linguistic Society of Korea. He was that very person who set up the first workshop on Montague Grammar in Korea and probably was the most energetic administrator who turned into a brilliant linguist with a lot of humor that was often misunderstood. When she was visiting Korea, he embarrassed Barbara Partee, asking her if she could remember him sitting in her class packed with a large audience in an LSA institute, held in LA ten years before. Susumo Kuno could

[2]The second example is taken from a plaque hanging on the wall of a country house belonging to a colleague of mine. He said that he bought it at a souvenir shop somewhere in New England.

not pardon his joke on his non-Oxonian Cambridge accent during his lecture at Seoul National University.

To this first SICOL, several world-known or aspiring linguists were invited. Among them were George Lakoff, Haj Ross, and Gerald Gazdar. By then George Lakoff had given up anything formal, including generative semantics. Instead, he talked about metaphors and also about *Women, Fire, and Dangerous Things*. Before coming to Seoul, John R. Ross, more often called *Haj*, had produced a landmark work in syntax, an MIT dissertation, entitled *Constraints in Variables in Syntax*. When I attended the LSA Linguistic Institute held at the University of North Carolina, Chapel Hill, in the summer of 1972, he taught Ivan Sag, me, and others Squish Grammar, a non-discrete grammar, with the fuzzy notion of nouniness. The title of his talk at SICOL-1981 was "Human Linguistics", but against our expectation it was focused on complicated and very sad human relations among the MIT linguists headed by Noam Chomsky. These relations were sad and bad, for eventually Haj had to pack up and leave MIT. Our small group in Korea was prepared to listen to Gazdar (1979) talking about his new book on formal pragmatics, but he talked about something else, which turned out to be the beginning of GPSG. We also had the honor of meeting the two most important persons from Japan: Professors Kazuko Inoue and Akira Ikeya. Both of them were much impressed by the organization of SICOL and also by linguistic activities in Korea especially because Professor Inoue was in charge of hosting the International Congress of Linguists in the ensuing year in Japan, while Professor Ikeya was much more interested in importing or inviting Korean linguists to Japan.

With the support of Professor Inoue, Ikeya sensei immediately proposed to start a series of binational joint working group meetings focusing on formal linguistic theories and other related issues. As a result, Korea agreed to host its first Korea-Japan joint workshop, entitled *The First Seoul Workshop on Formal Grammar Theory*, in January 1982. Ik-Hwan Lee, the first Secretary of our Korean group, which later became KSLI (the Korea Society for Language and Information), organized this first meeting at International House, Ewha Womans University. Roland Hausser was invited from the University of Munich, Germany, to give the first keynote lecture at this first meeting.

For these pre-PACLIC meetings, I remember going to Kyoto University (February 1983), Matsuyama University (December 1984), and Sophia University in Tokyo in those early years. I missed the meeting that was held in Japan in December 1989, for I just had a major medical operation at that time. We had a real symposium over soju or sake, while discussing Montague, categorial grammar or lambda calculus. In Kyoto, we had the honor of meeting Prof. Makoto Nagao at his Kyoto University Lab and listened to him perhaps with the demonstration of his famous example-based machine translation (EBMT). EBMT was publicly opened to the world in 1984. In Matsuyama, Geoff Pullum was invited, who was one of the authors of Generalized Phrase Structure Grammar. Byung-Soo Park and Hwan-Mook Lee attended that meeting, each presenting a paper. I was also there too. Before or around that time, Byung-Soo and I promised to coauthor a book on GPSG and I wrote a few chapters, but we never managed to publish a book, for HPSG moved in too fast. That book could have been the first KPSG, corresponding to JPSG proposed earlier by Takao Gunji. I forgot the names of all those wonderful people, whom I met in Kyoto and Matsuyama and would like to thank again, but I still remember the young lady then from Hiroshima, named Mizuho Hasegawa, who later became a dean (of academic affairs) at a women's university in Tokyo or its vicinity.

Going back to earlier years, Ikeya sensei with the support of Professor Arata Ishimoto, organized the Second Colloquium on Montague Grammar and Related Topics in March 1982.[3] At this workshop, Takao Gunji (1982) presented a paper, entitled "Dynamic Universe of Discourse and Implicatures", analyzing the semantics of donkey-sentences. I don't remember exactly when, but Professor Ikeya introduced me to Professor Arata Ishimoto, the first president of the Logico-Linguistic Society of Japan. He then invited me to come to Japan and stay at the guest house of his Science University of Tokyo to work together for over a week. We worked on

[3] See Ishimoto (1982).

the law of identity and the copular verb "be" in the framework of Montague Grammar, but unfortunately didn't manage to produce a joint paper.

By this time, a couple of things have changed particularly in the field of formal semantics. Montague grammar began to be called *Montague semantics*. It wasn't a grammar in a real sense, for it lacked both phonology and morphology. It also had very little to say about the lexicon. Furthermore, the categorial grammar that was adopted in Montague's PTQ wasn't Montague's invention. Instead, it had a long Polish tradition in mathematical logic, especially attributed to Kazimierz Ajdukiewicz at Adam Mickiewicz University in Poznań (See Ajdukiewicz (1935) and other contributions by Bar-Hillel (1953), and Lambek (1958). Through Hwan-Mook Lee, who was teaching at the University of Warsaw, I had the honor of visiting this university and sitting on a leather-made worn-out, but glorious chair of Ajdukiewicz in his old office. There I was invited by Professor Jacek Fisiak to give a talk at his School of English. Although it wasn't his invention, Montague made linguists like me work on categorial grammar. His real contribution was, however, most recognized in the area of making formal semantics applicable to the semantics of natural language. Dowty, Wall, and Peters's (1981) great book that introduced Montague's work was thus entitled *Introdction to Montague Semantics*.

Besides its emphasis on Frege's principle, called the *principle of compositionality*, Montague semantics helped understand the three basic characteristics of formal semantics: it should be characterized as a (1) truth-conditional, (2) model-theoretic, and (3) possible worlds semantics. What is true or false has become the core of meaning or the starting point of discussing what is meant by a sentence. This feature was understood as part of the correspondence theory of meaning that relates language to the world. That a sentence is true means that there is a world or situation in which what is meant or described by that sentence holds. A model theory allows the construction of some situations in which such a sentence holds to be true or false. Then a possible worlds semantics is needed to treat the meaning of sentences involving modality or factuality. Consider worn-out archaic sentences like:

(8) a. `If I were a bird, then I could fly.`

b. `I wish I were a millionaire.`

c. `I believe that the earth is a square.`

or more mundane sentences from E.L. James #1 *New York Times* bestseller, *Fifty Shades of Grey* like:

(9) a. `If this guy is over thirty, then I'm a monkey's uncle.`

b. `Just because you can doesn't mean that you should.`

To interpret sentences like these, we have to go beyond the actual world where we live and think of some other possible worlds in which I could be a bird and fly or be a millionaire or a monkey's uncle and in which the earth could be a square. We should also be thinking of what we can do and what we should or must do. As attested by Partee (2004)'s book *Compositionality in Formal Semantics*, many of the great semantics works have been following all these principles of semantics, making great contributions in the area of natural language semantics, based on formal semantics in the short history of Montague semantics.

As we began to understand what Montague semantics was, we also began to understand its limitations. First, higher-order intensional logic was not of much help, for it failed to properly interpret propositional attitudes involving verbs like *believe*, *assert*, *know*, and *wish* and so-called *propositions* expressed by them. In Montague Semantics, a proposition is defined to be a function from worlds or indexes to truth values and a valid proposition maps every world or index to a truth value. Hence all of the valid propositions such as:

(10) a. (p=Law of Identity) `If John is an idiot, then he is.`

b. (q=Law of Excluded Middle) `Either Mary is a genius or she is not.`

denote one identical function, at least in a bivalent logic. As a result, statements like:

(11) a. `Mia believes that p.`

b. `Mia believes that q.`

where p and q are understood to be valid propositions, are understood to convey the identical beliefs. Ordinary linguists, however, know that they are about two different persons and that they carry different information.

Second, consisting of a single non-empty set of individuals as its domain of discourse, classical model-theoretic semantics does not help to resolve paradoxes such as the Liar's paradox or a restricted quantification. Epimenides, a Cretan, supposedly said:

(12) All Cretans are liars.

and also we often say, even if there are many people around:

(13) No one is here.

while a model theory fails to exclude the speaker from the domain of the discourse or that non-empty set of individuals in a model.

Third, the universe of possible worlds is too big to comprehend. David Lodge's *Small World* is, on the other hand, quite interesting, for humans can talk about and do a lot of things in a small world. The basic difference between the possible worlds view and the small world view is like seeing the whole universe from the top down with the eyes of God or a tiny part of the world from the bottom up with the near-sighted eyes of the created beings. This difference can be easily understood if you accept the action theory of language use. All these issues came up to the surface when Jon Barwise and John Perry of Stanford University published a book, entitled *Situations and Attitudes* in 1983 or much earlier with some other people. Their subsequent work was known to be *Situation Theory* and *Situation Semantics*, an application of situation theory to the semantics of natural language, to be acronymed *STASS*.

By mid-80s, CSLI (Center for the Study of Language and Information) was founded by these philosophers and others in linguistics, psychology, and computer science at Stanford University and at the research centers surrounding the university, namely Xerox PARC and SRI International. It soon became the center of formal semantics as well as formal and computational works in language and the mind, attracting a lot of scholars home and abroad. Accelerated by the fame of the Silicon Valley, the place was occupied with computer scientists, computational linguists, psychologists, and all the people who were called *cognitive scientists* or scientists of *symbolic systems*. When I got there as a one-year visiting scholar in December 1986, I found such renowned persons as well as friends such as Martin Kay, David Israel, Jerry Hobbs, Terry Winograd, Stanley Peters, Lauri Karttunen, Joan Bresnan, Ron Kaplan, Ivan Sag, Roland Hausser, Kris Halvorsen, Peter Sells, Craig Roberts, Mary Dalrymple, Carl Pollard, Dan Flickinger, and others from Hewlett-Packard, Zerox PARC, and SRI International as well as various departments at the university. There was Syun Tutiya, a young philosopher, from Tokyo and later a large group of computer scientists from Japan including Hideyuki Nakashima, Yasuhiro Katagiri and Koiti Hasida. At its peak, CSLI reports and other publications were more in demand than those publications by MIT, Academic Press, or Springer. CSLI also had the strong funding and other support from the Systems Development Foundation and the Fifth Generation enterprises in Japan to host workshops and also to build its own beautiful mission-style building near the medical center of the university.

Perhaps the first large-scale workshop was held in Half-Moon Bay not far from Santa Cruz along California Highway 1 soon after the Christmas holidays in 1986 or in January 1987. I was there to witness how the STASS activities would start developing in the following decade and how the STASS meetings would continue to be held in Asilomar, California, (March 1989) and also in Loch Rannoch, Scotland, (September 1990), till Jon Barwise left for Bloomington, Indiana, and sadly died of cancer to our great loss. Being a mathematical logician, Jon was most interested in constructing his own unique theory of situations, so he has been working with mathematical logicians such as Peter Aczel, Gordon Plotkin, and Keith Devlin. At the same time, possibly persuaded by linguists such as Robin Cooper, he was also interested in representation issues in Situation Theory and Situation Semantics. They jointly published two articles enti-

tled "Simple Situation Theory and its graphical representation" (1991) and "Extended Kamp Notation" (1993). One of my books, written in Korean and published in Korea, was a collection of my papers based on Situation Semantics, entitled *Situation Semantics*, that partially tells what Situation Semantics is. Unfortunately these days no one talks about Situation Theory or Situation Semantics. Nevertheless I believe it has had a great impact on the development of formal semantics of natural language, especially dealing with some dynamic or computational aspects or pragmatic-oriented issues that arise in the ordinary use of language.

The STASS group was interested in Kamp's (1981) DRT (Discourse Representation Theory) not simply because of its representation scheme. It found in DRT a way of constructing an interpretation model bottom-up, without bringing in all of the imaginable possible worlds most of which are found irrelevant for the interpretation of a fragment of language under analysis. Even the interpretation of the notorious *donkey sentences* can be fully represented in a small box with some linkings with a small set of entities referred to, called *discourse referents*. The combination of STASS representation of various types of situations such as *described*, *discourse*, *resource*, and *background* situations with DRT boxes was shown to work beautifully for the treatment of many complicated sentences. Cooper and Kamp (1991) thus managed to coauthor a paper entitled "Negation in Situation Semantics and Discourse Representation Theory", showing how they can implement each other or benefit from each other.

Apparently the first joint efforts between Barwise and Kamp failed to produce anything significant mainly because neither the earlier theory of Situation Theory nor the 1981 DRT was adequately developed to be able to deal with issues involving negation or some other issues. Kamp (1981) treated implication, but not negation. By late 1980s, Situation Theory was able to deal with two types of negation, one of which can be interpreted as *denial*, for a proposition as a truth-value carrier could be considered as consisting of a situation s, an infon i, often called *soas* (state of affairs), and a *support relation* $\models$ that links them. This was then represented as below:

(14) Proposition p: $(s \models i)$,
   such that p is true if there is a situation s which supports the infon i that carries a basic unit of information, but otherwise it is false.

The type of negation, which can be interpreted as a denial, is then represent as:

(15) Denial or Negative Proposition:$(s \not\models i)$

The infon also carried information on its polarity, either positive and negative.[4] So we may have a negative infon as represented as below:

(16) Negative Inform: $\ll bald, Socrates, 0 \gg$,
   which carries the information about Socrates not being bald.

This information could have been correct of Socrates when he was still a young man.

Such a treatment of negation in Situation Theory could have been amalgamated into the new version of DRT, presented in Kamp and Reyle (1994), which was forthcoming at the time when Cooper and Kamp (1991) jointly worked on negation. This paper, however, dealt with negative infons only with an example:

(17) John doesn't own a car.

This does not entail that there is no situation whatsoever in which John owns a car because the existence of a car is restricted to a particular set of cars, say Hyundai-made Korean cars, by a resource situation, as proposed in Situation Theory. The same interpretation can be uphold in Kamp and Reyle (1994). Details of this amalgamation work should be left for discussion in some other occasion in the future.

During all these fast-evolving years, I found myself stuck with a pre-terminal stage cancer. I could participate in most of the STASS activities, but to my regret failed to submit any papers and have them included in the STASS proceedings and to make my name known forever. At any rate, my colleagues in Korea thought that I would die soon and they decided to lengthen my life by electing me president of the Linguistic Society of Korea and also of the Korean Society for Cognitive Science. They then persuaded me to organize the 1991 Seoul International

[4] 0 stands for the negative polarity.

Conference on Linguistics and also to organize the first international conference on cognitive science in Seoul. Hans Kamp was invited to SICOL-1991, but I am afraid he didn't say a word on DRT. This announced the end of the 1980s and the beginning of the 1990s.

4 The 1990s: Data-driven, Statistics-bound, and Computational

Despite my ill-health and poor publication records, I managed to get invited to travel and give talks here and there. Professor Arnim von Stechow invited me to come to Tübingen to give a talk. I was reluctant to accept the invitation and make such a long trip to Europe, but the Korean students, Jung-Goo Kang and Byongrae Ryu, there in Tübingen, Germany, and Professor Roland Hausser in Erlangen, Germany, persuaded me to accept his invitation and come to Germany. Roland also invited me to his university in Erlangen. Since then I frequented Germany, visiting Saarbrücken and Erlangen. In Saarbrücken, I met Hans Uszkoreit briefly and then Manfred Pinkal for lunch. Manfred wanted to hear about Situation Semantics from me, almost thinking that I had been its originator, and we had a wonderful discussion. He then suddenly remembered his afternoon class and left me alone in the faculty dining room.

While traveling here and there in Germany, Great Britain, Japan, Columbus, Ohio, and also making regular visits to Palo Alto, California, I began to realize that the focus of formal semantics was changing from strictly mathematico-logical issues to something more computational. In the early 1990s, Kris (Per-Kristian) Halvoresen, then of Xerox PARC, for instance, gave a tutorial on Computational Semantics during the LSA Linguistic Institute, held in Santa Cruz in the summer of 1991. And fortunately around that time I was able to work with Ron Kaplan at Xerox PARC and began to do something that you may call computational, for I was trying to implement Korean on his LFG workbench and hoped that I could use it to test my toy programs for Computational Semantics. There were, however, a couple of practical problems that hindered the continuation of any serious work in Computational Semantics. One simple, but serious problem had to do with the importing of Hangul characters and fonts into the system, for Korea was still arguing which coding system it should adopt beyond industrial applications and no full-fledged Unicode had been developed by then. Another practical problem was that the LISP-based system required too much memory for ordinary workstations, not to mention personal desktops or laptops, to run anything really significant. Ron tried to install a new version of the LFG Workbench on my newly-purchased expensive workbench remotely from Palo Alto, but every endeavor just ended in frustration only. The Internet was also too slow then in both U.S.A. and Korea. Remember that this was twenty some years ago and I was still a young man reaching to be sixty.

In the summer of the same year, namely 1991, I also attended an ACL conference held at UC Berkeley. There everybody saw that a number of accepted papers in the area of corpus work rapidly increased, in contrast to the predominance of accepted papers in the the area of logical programming or AI-oriented researches in the previous years. Till then few accepted papers had dealt with corpora and any statical findings from data in corpora, for the so-called stochastic approach was not welcome on the American scene of linguistics. At least twice I was invited to review NSF grant applications in the 90s, but nothing theoretical or formal was successful in securing any grant in those years. Every national grant had to account for its technological or social applicability and usefulness for the nation or its communities that paid taxes. This trend was more so in the area of communications using spoken data. Unlike written texts, spoken data was more manageable to the statistical approach, for humans seem to discern sound differences in a more probabilistic way. Years later, namely during the 2004 LREC in Lisbon, where he was giving an acceptance speech for the Antonio Zampolli award, I remember Fredrick Jelinek saying "Every time I fire a linguist the performance of the speech recognizer goes up" and he indeed fired linguists at his IMB Research Center.

In December 1995, almost 13 years after the first Korea-Japan joint workshop, our PACLIC was born in Hong Kong at the hands of our venerable Benjamin T'sou. The conference was officially named *The Tenth Pacific Asia Conference on Language, Information and Computation*. According to the Call

for Abstracts for this conference, organizers of two conferences, the Asian Conference on Language, Information and Computation (ACLIC) and the Pacific Asia Conference on Formal and Computational Linguistics (PACFoCoL), agreed to merge their conferences to PACLIC and number this merged conference the 10th. Strictly speaking or if you prefer PACLIC to be recognized to be older, we can rightly claim that PACLIC dates back to the winter of 1982 when Ikeya sensei and Ik-Hwan Lee, secretary of the Korean group, organized the first J-K joint workshop in Seoul that I just mentioned. Hence, we should be celebrating not the 25th anniversary, but the 30th anniversary of PACLIC this year. Remember that in our Asian society the older the more respected, wrongly believing that the older are the wiser.

From the beginning, the scope of this group, which I mean to be the whole PACLIC group, has gone beyond language and information, comprising the area of computation in general and NLP in particular. We have thus invited computer scientists to form the core of our group. From Korea, we have always had Key-Sun Choi of KAIST, who once organized a J-K joint workshop in Wonju as Secretary of the Korean hosting group. We also had Hyuk-Chul Kwon, now full professor of computer science at Pusan National University, always occupying a seat on the second row right after professors in our tutorial classes as a graduate student of SNU. Professors Kilnam Chon, who was known to be the father of Internet in Korea, of CS Department, KAIST, and Yungtaek Kim, who was the godfather of NLP and MT in Korea, have been the strong supporters of our KSLI, the Korean Society of Language and Information. I expect Professor Ikeya, Benjamin, and my good old friend Chu-Ren to make a long list of their colleagues working for the organization of PACLICs in the past and the present.

By the end of the decade of 1990s, computation definitely got into the core of linguistics. ACL and COLING got flourishing, gathering up a huge crowd for each of their conferences. Publishers were looking for books prefixed with the magic word *computational*. Oxford University Press published a book, entitled *Computational Approaches to the Lexicon*, edited by B.T.S. Aktkins and A. Zampolli, in 1994. A year later, namely in 1995, Cambridge University Press published *Computational Phonology: A Constraint-based Approach* by Steven Bird. Books on computational morphology came out much earlier: in 1991, the MIT Press published *Computational Morphology* by G.D. Richie et al. and, in 1992, *Morphology and Computation* by R.W. Sproat. Kiyong Lee couldn't wait too long to publish his own, so he published a prize-winning book, entitled *Computational Morphology*, but written in Korean. Patrick Blackburn and Johan Bos published two books on computational semantics: *Representation and Inference for Natural Language: A First Course in Computational Semantics* (1995) and *Working with Discourse Representation Theory: An Advanced Course in Computational Semantics* (1996), both of which were published by CSLI Publications, Stanford.

My ambition has been to write a book on Computational Semantics and then to end my life. This was so because I published three books on semantics in 1988: *Language and the World: Formal Semantics*, *Tense and Modality: Possible Worlds Semantics*, and *Situation and Information: Situation Semantics*, all of which were again written in Korean and also prize-winning. I had thought this could be a pioneering work at least in Korea, but then learned that the term *Computational Semantics* appeared far back in the mid-1970s: Eugene Charniak and Yorick Wilks edited a book, entitled *Computational Semantics: An Introduction to Artificial Intelligence and Natural Language Comprehension*, in 1976 and a course on Computational Semantics was offered at the Institute for Semantic and Cognitive Studies in Switzerland in 1975, while I was still working on Categorial Grammar and Lambda Calculus.

In the late 1990s, computational stuff started popping up in PACLICs, too. Most of the papers in PACLIC 10 (1995) were computational. Chungmin Lee presented a wonderful paper on polarity phenomena and Ik-Hwan Lee another great situation-theoretic paper on generic expressions with examples such as *The dog barks* and *Dogs bark*, but I am afraid papers on pure linguistic theories were attracting less attention than in the earlier decades. On the other hand, papers like "HMM Parmeter Learning for Japanese Morphological Analyzer" (Koichi Takeuchi and Yuji Matsumoto), "Using Brackets to Improve Search for Statistical Machine Translation"(Dekai Wu and Cindy Ng), and "Predication

of Meaning of Bisyllabic Chinese Compound Words Using Back Propagation Neural Network"(Lua Kim Teng) attracted the audience. Kiyong Lee also presented a computational paper "Recursion Problems in Concatenation: A Case of Korean Morphology", but to his great disappointment he had almost no audience, for it had no statistical formulas or tables, thus being understood as one of the classical morphology papers.

Papers related to Computational Semantics or Lexical Semantics also began to appear in PACLICs. PACLIC 14, held in 2000 at Waseda University, Tokyo, for instance, had papers like: Chu-Ren Huang and Kathleen Ahrens, "The Module-Attribute Representation of Verbal Semantics", Samuel W.K. Chan, Benjamin K. T'sou, and C.F. Choy, "Textual Information Segmentation by Cohesive Ties", and to your disappointment, my own "Developing Database Semantics as a Computational Model". In contrast, we had a keynote lecture by Masayoshi Shibatani gave a keynote lecture, which was far from being computational. The title of his lecture was "Language Typology and the Comparison of Languages" (abstract) and I was asked to introduce him and chair his lecture. He emphasized that his work was real linguistics because it was totally data-driven and that I agreed. Pleased with my chairing and support, Professor Shibatani invited me and a few others to an expensive udong house near Waseda University. He then invited my wife and me to his castle-like two-story mansion, located somewhere deep in the valleys of the Okayama Prefecture, where Momotaro-san, born of a big peach floating on the river, conquered Oni, or Japanese devils, with his faithful company of a pheasant, a monkey, and a dog. Shibatani sensei and I promised to meet again when I would develop a computational semantics based on his Mindanao dialects of the Philippines.

5 The 2000s: Linked with Bits of Information, Distributed Partial Information

As the second millennium reached, too many things were happening all over. Here were a few things that happened around me. In the summer of 2002, I retired from my university and began to build a house in the country where I could retire and be a farmer. When I retired, my colleagues were, I thought, really happy to see me go, but kept me teaching for two more years in their Department of Linguistics, which I could no longer claim to be ours or mine, although I helped found it. My good old friends Ik-Hwan Lee and Minhaeng Lee at Yonsei University invited me to their university to teach Computational Semantics, Computational Morphology or something like that. Key-Sun Choi of KAIST also put me to work for ISO. Alex Fang of City University of Hong Kong invited me to do writing at his university as a visiting professor three times and I still owe him a monograph on ISO annotation schemes to finish. So I have had very little time to take care of my country house and the two doggies from one of my neighbors, whom I seldom see around, but all the trees there have grown up for themselves, while all the books, the papers, the diskettes, and the notes were piled up unsorted. Thanks to you the PACLIC Steering Committee members and the PACLIC 26 organizers in Bali, I managed to recollect myself and revive my short memory of the past, the past 40 years. Having said enough to bore you with my private chatting, I just like to end my talk by telling you a bit about a kind of Computational Semantics, called *Annotation-based Semantics*.

Annotation-based semantics was initiated by several people. Among them are Ian Pratt-Harttman, Harry Bunt, Graham Katz, James Pustejovsky, and Kiyong Lee myself. We all agree that such a semantics guarantees a *robust* system. It should not fail to operate when applied to the processing of natural language texts, although they usually contain a large number of syntactically ill-formed strings of words and indexical or other expressions that are interpretable only contextually. Ordinary linguistic semantics fails to process information from materials presented in a tabular form, maps, and pictures. Annotation-based semantics, however, continues to work successfully, that is, in a robust way, because all of the appropriate pieces of information taken from those media are annotated and represented in a machine-readable format before they are formally interpreted.

Annotation-based semantics can also control the flow of information. Sometimes we get too much or too little information to take an appropriate action.

To inform the local organizers of PACLIC 26 of my flight schedule, I wrote to Ruli the following email, asking if I should book a hotel myself. Part of the email relevant for the flight schedule can be annotated in XML, a machine-readable language, using two ISO-supported annotation schemes, ISO-Space (2012) and ISO-TimeML (2012), as follows:

(18) a. Email Text:

Dear Ruli,
I'll be **arriving at Bali/Denpasar by KE629 at 00:05 Thursday 1 November** and leaving by KE634 at 02:05 Monday 12 November. Should I book a hotel myself? Best, Kiyong

b. Annotation:

```
<isoSpace xml:id="a1">
<PLACE xml:id="pl1"
type="PROVINCE" country="ID"
form="NAM"/>
<PLACE xml:id="pl2" type="PPLC"
cvt="CITY" province="#pl1"
country="ID" form="NAM"
latLong="8°39'S 115°13'E"/>
<QSLINK xml:id="qsl1"
figure="#pl2" ground="#pl1"
relation="IN"/>
<ADJUNCT xml:id="a1"
type="flight" value="KE629"/>
<MOTION xml:id="m1"
motion_class="PATH/MANNER"
motion_type="REACH"/>
<MOVELINK xml:id="mvl1" source=""
goal="#pl2" goal_reached="YES"
means="KE629"/>
</isoSpace>
<isoTimeML xml:id="a2">
<TIMEX3 xml:id="t1" type="TIME"
value="2012-11-01T00:05"/>
<TIMEX3 xml:id="t2" type="DATE"
value="2012-11-W4T00:05"
corres="#t1"/>
<TLINK xml:id="tl1"
eventID="#m1" relatedToTime="#t1"
relType="IS_INCLUDED"/>
</isoTimeML>
```

c. Interpretation:

$\exists\{e, x, y, z, w\}[move(e) \wedge named(x, Seoul) \wedge named(y, Denpasar) \wedge named(z, Bali) \wedge IN(y, z) \wedge named(w, KE629) \wedge source(x, e) \wedge goal(y, e) \wedge means(w, e) \wedge [\tau(e) \subseteq t] \wedge calYear(t) = 2012\ calMonth(t) = November\ calDay(t) = 01\ dateTime(t) = 00{:}05\ weekDay(t) = Thursday]$

You may say that annotation and interpretation make things more complicated. The fact is, however, that what we seem to know very little contains a very long list of complicated pieces of information. Till I analyzed this tiny fragment of a text, for instance, I had thought that Bali was a tiny island somewhere in the Indian Ocean. I didn't know at all that Bali was a province of Indonesia and that Denpasar was its capital city. The first three lines of the annotation contain this information. Nevertheless, the interpretation here conveys only part of the information conveyed by the two sets of annotations above: one set contains spatial information, whereas the other set contains temporal information.

Information may be provided in a tabular form. Here is a daily bus schedule, presented in a table format. Some relevant part of the information can also be annotated and represented in XML, followed by its interpretation.

(19) a. Daily Bus Schedule:

Bus#1048 05:30am, Bus#950 05:45,
Bus#055 06:00, Air-Bus#10 06:15,
..., Bus#1049 23:45.

b. Annotation:

```
<isoTimeML xml:id="a3">
(1) <TIMEX3 xml:id="t1"
type="SET" value="PT15M"
quant="EVERY" scopes="#e1"/>
(2) <TIMEX3 xml:id="t2"
type="PERIOD" value="DAY"
beginPoint="XXXX-XX-XXT05:30"
endPoint="XXXX-XX-XXT23:45"
qaunt="EVERY" scopes="#t1"/>
(3) <EVENT xml:id="e1"
type="TRANSITION" pred="DEPART"/>
(4) <TLINK xml:id="tl1"
eventID="#e1" relatedToTime="#t1"
relType="IS_INCLUDED"/>
(5) <TLINK xml:id="tl2"
timeID="#t1" relatedToTime="#t2"
relType="IS_INCLUDED"/>
</isoTimeML>
```

c. Interpretation:
σ_{a3} :=
$\forall t_2[[day(t_2) \rightarrow \exists t_3[interval(t_3)=[T05{:}30, T23{:}45] \wedge t_3 \subseteq t_2]] \rightarrow \forall t_1[length(t_1)=(15, minute) \rightarrow [depart(e) \wedge bus(x) \wedge Arg(1, x, e) \wedge (t_1 \subseteq t_3) \wedge (\tau(e) \subseteq t_1)]]]$

This says that a bus leaves every 15 minutes from 5:30 in the morning to 23:45 in the evening everyday.

What is being said in a plain language is much easier for us to understand, for this is the basic linguistic ability of humans. But to represent in a formal language gets complicated. To show how we derive such a complex piece of information in a compositional manner requires a much more complex process of combining its component pieces of information, each of which is represented by each XML element. Computational semanticists, however, attempt to formulate each step of such processes so that the computer can be trained to perform the process of annotating and interpreting various pieces of information conveyed by various types of media, for instance, not only still photos, but moving pictures. Inderjeet Mani and James Pustejovsky's most recent book, *Interpreting Motion: Grounded Representations for Spatial Language*, clearly shows what we, linguists and computer scientists, should be doing to develop the semantics of motion and space in general and the specification of semantic annotation and interpretation in particular. Harry Bunt's lecture, "The Semantics of Semantic Annotation", which was presented in PACLIC 21 (2007), Seoul, is an excellent example of showing how to interpret semantic annotations.

6 Concluding Remarks

C.S.Lewis is quoted, supposedly saying that he was told not to trust Catholics, as he began his early life, nor to trust linguists, as he began his career of teaching English at Oxford. Two of his best friends among his informal literary discussion group *Inklings* were, however, Catholic linguists: Hugo (H.V.D.) Dyson and J.R.R. Tolkien, the author of the novel *the Lord of the Rings*. They were philologists and made things easy to understand. Present-day linguists, on the other hand, are proud of writing or talking like Noam Chomsky, who wrote *Syntactic Structures* in terse English and made us memorize each page of it, or like Richard Montague, who published "Universal Grammar" and developed higher-order logics for natural language semantics. If linguists or semanticists keep talking or writing like them, then they may not have any followers who trust them. I once presented a paper, entitled "A Simple Syntax for Complex Semantics", which was supposedly a keynote speech for PACLIC 16 in February 2002. I concluded that talk by saying that a syntax must be kept simple for complex semantics, for the complexity of syntax is a theory and that of semantics, a reality. Fortunately, generative syntax took its path to minimalism (See Chomsky (1993).), while we have also seen a semantics like Copestake at al. (2005)'s Minimal Recursion Semantics (MRS).

I was surprised to learn that *small world* is a mathematical notion. It forms a network with nodes most of which are not directly connected, but connected with some distances. We thus get information about ourselves or our surrounding environment not from our neighbors right next door, but from those third persons at a distance who are situated in better perspective. It is still a robust structure with distributed bits of information, for the whole structure is preserved even when some of its parts collapse, thus providing objective validity. This picture seems to well represent the current situation of the world in which we live by exchanging information in the most efficient way with a tiny mobile gadget. Seen as a theory of action, the meaning of semantics can be understood with respect to such a small or tiny world rather than with respect to all possible worlds that are inconceivable or keep asking for the proof of their logical consistency or mathematical completeness. We need new semantics that can interpret all those signals that are sent out from those small worlds and also translate those interpretations in our metalanguage that is still bound to be a system consisting of sequences of discrete linearized symbols.

Reflecting on the past quarter of a century of the PACLIC meetings, I hope that our PACLIC will remain as a small world and that all its members would be closely connected with one another. I have attended most of the biennial conferences of LREC (Language Resources and Evaluation Conferences), namely those meetings in Las Palmas (2002), Lis-

bon (2004), Marakech (2008), Malta (2010), and Istanbul (2012). I also attended several meetings of ACL or LSA. I am afraid that these meetings have kept growing with so many plenary and parallel sessions, poster sessions, and satellite workshops and also with so many participants. This year's LREC, held in Istanbul, for instance, had over 1,300 participants. One big problem is that one gets either totally lost in the crowd or completely exhausted with so many contacts. As the cost for organizing such big conferences rises, participants have to pay a larger amount of the registration fee, sometimes reaching one thousand dollars. This was the case with an IEEE-sponsored workshop that I attended to read a paper a year ago. With so many official events and personal appointments, some papers are presented with almost no audience and some posters are just standing there. As a result, I predict that all these big conferences will eventually break up into smaller groups and that these small groups will grow up to become big organizations, with a cycle of growth and breaking up necessarily repeating. I thus repeat my hope that PACLIC will remain a small world so that all of us can enjoy close comradeship in pursuing our academic work and exchanging every bit of our knowledge or doubt with each other as we may be doing at a meeting like this wonderful conference in Bali.

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