

Linguistics of the Tibeto-Burman Area
Volume 9.2 Fall 1985

An anthropologist/psychiatrist looks at Southeast Asian
 (including Japanese) linguistics¹

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I must begin this talk with both a confession and an announcement. Two years ago I accepted a royalty payment and this past year I accepted another. The amount involved was not large - my wife recently totaled it up on her calculator and informed me that I am undoubtedly the lowest paid worker in the world, including even Haiti, Swaziland and other Third World countries. The fact remains, however, that I did accept the payments and am now a professional.

No one was more surprised by this turn of events than I, with the possible exception of the publisher. It means, of course, that I can no longer take refuge under the cloak of amateurism. I used to wonder why so many of my published papers went unnoticed. I finally came to realize that my colleagues were reluctant to criticize me because of my amateur status in the field, figuring that it would not be fair. Not all of them, of course - I have occasionally run into an unkind critic but by and large I've led a charmed life. This has had a most unfortunate effect upon my writing, however, since it has allowed me to become more and more cryptic, even unreadable at times. I am pretty disorganized around my study and tend to lose track of notes on this and that. One day a great light dawned on me: why not put all these notes in articles and get them published - that way they can never get lost! This is not altogether true since sometimes I even lose the published articles themselves but it is a vast improvement over my earlier situation. It does mean, of course, that I've been turning out articles mainly for my own convenience, the idea being that (a) probably none will be read very carefully and (b) even if by chance one does get read I probably won't be criticized.

So much for the confession part. Some of you may even have thought that I can't write any clearer than that! Well, I can, and I hereby announce that from now on anything that I turn out will be readable. I've been practicing lately on my Japanese/Austro-Tai book, which I'm plugging here just as any of you professionals might. The book is simply loaded with tables, references, explanatory notes and all sorts of help for the readers. After the Austro-Tai book came out some 10 years ago an eminent linguist informed me that I had made the readers do far too much work - 'and they'll never do it'. He was right, of course, and in fact he is one of the few linguists in the field who appears actually to have read the book. I have learned my lesson now; the reader of my new book will do absolutely no work at all and, without even having to think, will put down the book with the firm knowledge that Japanese is simply another branch of the great Austro-Tai stock.

This talk has been advertised as a 'leap in the level of generality' and I shall attempt to make it just that. I'll be discussing several matters that have been of interest to me over the years: how did we all get to be linguists and why did we choose the specific field or fields in which we are working; how do we perceive what we are doing and it is possible that this perception plays a role in the results that we attain; how do doctrines become established in the field and how do they influence our findings; how to interpret the

¹ Keynote address delivered at Sino-Tibetan Conference, Sept. 8, 1984.

prominent role played by anthropologists in Southeast Asian linguistics; finally, how to explain what appear to be certain improbable happenings or 'mysteries' in the field.

I was a bookworm of sorts as a child and was even known to carry a dictionary about with me at times, all of which I did my best to hide from my friends. It was confidently predicted that I would grow up to be a professor or, perhaps, a writer. I originally thought that other linguists had similar backgrounds but was disabused of this notion years ago when traveling in the Far East. An English woman introduced herself to me as Miss Wolfenden and I made a remark to the effect that I was familiar with the work of a distinguished scholar of the same surname: Stuart N. Wolfenden. On hearing this she burst out, evidently in great surprise:

"Good old Stu! He's my cousin, you know. And the family never thought he'd amount to anything".

Miss Wolfenden then proceeded to question me at some length, as if to assure herself that 'good old Stu' had really made his mark in the world. For my own part, I found it difficult to believe that a great linguist like Wolfenden, after whom a society has been named, could somehow have remained so utterly unimpressive in his formative years. I've read how Einstein once got a C or D in chemistry or something but this was ridiculous. Or was it? The 'good old Stu' syndrome has haunted me over the years. Was he one of a kind, really? But how would one find out? Wouldn't most scholars with the syndrome not realize that they had it?

As you can see, I haven't got very far with that problem nor, for that matter, with a somewhat related problem: what intrinsic factors underlie our choice of this or that special area of linguistics? In medicine there are some fairly clearcut differences in the specialties, e.g. as an intern and later as a resident I discovered that if one wanted to engage in an intellectual discussion he'd better seek out a pathologist or a psychiatrist and, if he wanted it to be a rational discussion, he'd better stick to the pathologist. How about linguists - why does one become a comparativist, especially of the armchair variety, or a decipherer of oracle bones or specialist in Archaic Chinese reconstruction, or a grammarian or an analyst of 'deep structures'?

I spent some of my residency years in a research institute and became interested in the general field of intelligence and special abilities, which I pursued later in my career as a psychiatrist. I've always favored a radical position, which sees intelligence as a mosaic of pluses and minuses, so to speak, each to some degree independent of the others, as opposed to the prevailing view that so-called 'test scattering' is the result of psychological blocking in this or that area. This view came very naturally to me since I am grossly defective in the area of small mechanical manipulation and I refuse to believe that I am undergoing a psychological block every time I try to change a razor blade or open a small package of something in a restaurant. In fact, I have long argued that neuroticism is, to a considerable degree, the result of defective social intelligence: the neurotic suffers because he misinterprets the social world around him just as I and others of my tribe suffer because we fail to understand how these bastardly little objects work.

To return now to linguistics, one would have to suppose that we all have 'verbal ability' but surely this in itself must be a complex mosaic. Howard Gardiner at Harvard and other cognitive psychologists now talk about 'multiple intelligences' but they are using the word in a much broader sense, e.g. one of several such 'intelligences' is described as musical ability. I'm not at all sure that musical ability has any genetically programmed relationship with verbal ability, e.g. I've known linguists to show good ability in recording, in general, but poor ability in handling tones. The old view of a 'little g'

Intelligence Factor is still applicable, I believe, but it is very much influenced by input: in dealing with small mechanical objects it is my input that is at fault; once I see how the little sons-of-bitches work I'm pretty good at playing around with them, as in improving them for the benefit of defectives like myself.

How does all this relate to our choice of this or that special field in linguistics? My impression is that the principal differentiating factors are connected with input rather than with 'little g', with comparativists more under the influence of visual than of auditory factors. There is also an intriguing possibility that more than one 'little g' is involved, e.g. one concerned with synchronic and another with diachronic parameters. This would nicely explain, of course, why otherwise brilliant students of languages have on occasion produced idiotic papers in the field of historical linguistics. *And, for all I know, perhaps the other way around.*

In addition to the above there is also the matter of special abilities, which may even cut across other relationships. Some of these are likely to remain unknown except under very special circumstances, as I can testify to from my own experience.

Until some 10 years ago I had always questioned my own language ability, believing that my linguistic results had been achieved largely through 'little g' factors plus a lifetime of absorbing linguistic data of various kinds. I have often complained about having a terrible handicap as a linguist: poor language ability. And I haven't ben kidding - I am abysmally poor in handling spoken languages and mediocre, at best, in dealing with written languages, e.g. I have been working on Chinese for almost 50 years and I still find myself baffled by it much of the time. I have it, on good authority, that the following kind of criticism has been leveled against me: Benedict can't be any good because he doesn't know any languages. True enough - but maybe I've been using a 'secret weapon' all these years.

About 10 years ago I attended a hospital seminar on hearing disabilities at which a special record was played to illustrate the problems faced by patients with hearing disorders in which various frequencies had been disrupted. In the introductory material on the record it is stated that the material being read will not be understandable because of the distortion produced by the dropped frequencies. I found, to my surprise, that I had no difficulty with it and, in fact, I was even thinking that I'd still be able to understand it with far more distortion. After the record stopped, I remarked to the lecturer that the company should be notified of the mistake in the introduction. The audience of some 40 to 50 staff members, looked at me in amazement and I suddenly realized that I had been the only one to have had this experience. I did my best to explain in terms of my being a linguist but later I discovered that the others felt I had only been kidding. Even as in the past, when I had told stories about languages called 'Miao-Yao'.

Ever since that day I've wondered whether I've been making use of some special ability without realizing it, running disconnected bits of linguistic data by an analyzer of some sort and asking it to fill in the gaps. Could it be that I've even arrived at certain reconstructions in this manner? If so, I must admit that my analyzer has made some terrible mistakes! But it also appears to be working even when I'm not aware of it, hence my experience at times of having solutions to problems pop up in my mind when I'm engaged in an altogether different thought process, e.g. discussing a psychiatric patient. I wonder how many of you are aware of similar abilities of one kind or another and, if so, whether you see any connection with your work.

And then there is the matter of how we perceive what we are doing and the possible influence upon our linguistic results. Here again I can only talk

about my own perceptions by way of illustration.

A lifelong fantasy of mine has been prospecting for treasures of one kind or another, perhaps diamonds or the like - 'goodies'. I've always regarded prospecting as an ideal occupation, particularly in unknown territories. I believe that this has led me into all sorts of strange places in Southeast Asia - linguistically, that is - turning out papers on a variety of subjects, to a degree not otherwise readily explainable. It may even have influenced my findings at times by enhancing the quality of a discovery - and I use that last word advisedly. So please keep this in mind when you read some of my publications. But it has been fun!

At a somewhat deeper level, it is undoubtedly significant that my father was an architect and that my mother wrote poetry, so that linguistics was something of a compromise for me. I am a 'natural' as an architect and have actually designed and built houses. My linguistic reconstructions are literally 'constructions' and I constantly find myself writing about 'frameworks' and the like. This probably explains in large measure my use of what I've called 'teleo-reconstruction' since this procedure affords an over-all framework within which one can operate at will, modifying here and there as one might move a wall in a building under construction. This underlying need to fit everything, almost from the very beginning, into a large framework probably also explains certain aspects of my approach to the reconstruction of Archaic Chinese. By way of illustration, the medial -r-line, which has just been presented to you in some detail, will not do for me since it cannot be fitted into Sino-Tibetan, in my opinion, e.g. the basic root for 'kill' simply does not have a medial *-r- in Tibeto-Burman. A leading exponent of the -r- system once explained this to me along the lines of 'you can't win them all'; this is rather better than the method employed by other exponents of simply excluding the root, yet I can't have 'losers' like that in my edifice; it will all tumble down!

In addition, this facet of my background explains in large measure my opposition to what I call 'formulaic linguistics', as practiced notably by Dyen and his students in the field of Austronesian studies. The position taken by these practitioners is that proto-forms are essentially unknowable and that one can only tag sets of reflexes with whatever symbols are convenient, and little matter that on occasion one can end up with several /s/'s and as many as five /w/'s. I find it difficult to build linguistic edifices with material of that kind, however, hence my architectural bias leads me to try to resolve all such reconstruction problems. This does make comparative linguistics hard work, to be sure, but I believe that it enables one to arrive at reconstructions otherwise unattainable, e.g. in the case of Austro-Tai, the 'formulaic' approach failed to uncover a separate Proto-Austronesian alveolo-palatal series, as shown by distinct reflexes in Japanese.

The perception of my linguistic work as an art form is a far more subtle thing. I do find myself thinking of a given solution as 'beautiful' and saying to myself at times things like: if this is not right it ought to be right. It is quite possible that on occasion a 'beautiful' but incorrect solution has found its way into some publication of mine. I recall only one definite case of this type, however, and I'm glad to say that esthetics eventually triumphed here. I once outlined a 'beautiful' etymology for Polynesian tabu, based on the *t- prefixed *a(m)pu kinship term for 'grandparents' ~ 'uncles/aunts', etc. even though I knew about the apparently related Cham word: tabuŋ; it is now clear that final *-ŋ is widespread in Western Malayo-Polynesian as a kinship term suffix, thus restoring the viability of my original etymology for tabu. It is always a pleasure to see art triumph!

Up to now I've been talking largely about Unknowns and even Unknowables; I'd now like to take up a subject that is more readily accessible: the role of doctrine in Southeast Asian linguistics. And I'll be talking only about historical doctrine here.

When I first got into the Southeast Asian field at Harvard in the middle and late 30's, first in anthropology and later in Oriental studies, I encountered a very well established body of ideas about the linguistic and cultural history of the region. It was taught at Harvard, along with other eternal verities, in both departments and I don't recall having heard anyone express serious doubts about it. The prestige language was Chinese, seen as the star member of the Sino-Tibetan or Indo-Chinese family of monosyllabic tonal languages, which included Tai and Miao-Yao as well as Tibeto-Burman and even Vietnamese, which the great Maspero himself had linked with Tai. For cultural history an equally simple system prevailed - one which I have labeled the 'China Doctrine' - whereby all the manifestations of higher culture in the region were in one way or another of Chinese origin. This view had been vigorously promulgated by the great trio of French sinologists: Chavannes, Pelliot and Maspero, who were ranked at Harvard only slightly below the Trinity. Looking back upon this now I find it difficult to believe that I ever fell for this simplistic nonsense. But I did - and I took this mindset with me to Berkeley, along with the manuscript of my first paper in the field, rejected by a Harvard editor because of a mild criticism of Pelliot in one footnote.

When I arrived in Berkeley to take over from Shafer as the director of the Sino-Tibetan Philology project I encountered the same doctrine, backed by something that can only be described as a 'Chinese lobby'. I soon came to realize the strength of this 'lobby' when I decided that all the Chinese work on the project had to be junked because of hopelessly poor quality. When Shafer heard about this I was accused of some nefarious plot to destroy the project itself and Kroeber, the sponsor of the project, had to shield me from the wrath of the Berkeley Orientalist community at large. I got away with it, however, largely because of the support from Kroeber, who indicated to me that he'd be just as happy if I had no further surprises up my sleeve.

Everything ran smoothly for a few months while I tried to sort out the Tibeto-Burman materials collected on the project. It then came time, however, for me to prepare an introductory volume for the project and I began to examine in some detail both the Tai and Miao-Yao materials. I soon realized, with something of a shock, that a very important piece of the picture was lacking: there was no core vocabulary! Not even all the numerals had made it to Miao-Yao, to start with, and beyond them there extended a vast lexical wasteland, represented by extensive white spaces in Shafer's tables, with here and there a lone entry such as 'tea'. Years later a scholarly attempt was made to fill this space but the two entries selected for that purpose are hardly of the kind to create much confidence: 'iron' and 'needle'.

The day finally came for me to march into Kroeber's office to inform him of my two new surprises: all the Tai and Miao-Yao material had to join the Chinese in the project junk pile! By a stroke of luck I had seen the draft of a new article by Kroeber on diffusion, in which he had set up a contrast between 'direct' and 'stimulus' diffusion. I was able to break the news gently, so to speak, by first talking about the fantastic example provided by Southeast Asian languages of the diffusion of tones and probably of monosyllabic structures. Before getting around, as casually as possible, to a remark such as, "Of course, all the Tai and Miao-Yao materials will have to go". This approach must have worked since I don't recall any outburst of

cursing on Kroeber's part. I was also fortunate, of course, in the fact that I was dealing with one of the great minds of our times.

I had no difficulty in explaining to Kroeber the reasons for my decision and he later got the revised plan through his committee, so that the Sino-Tibetan Philology project ended up reduced to only Tibeto-Burman. Shafer never forgave me for this and I think that he convinced the Orientalists around Berkeley that I was crazy and/or wicked. In case any of you are wondering how Kroeber was able to convince people that his graduate student director was right and scholars like Maspero were wrong, you must realize that the Harvard Trinity in this case was up against the Berkeley God: A. L. Kroeber.

To continue with this case history, the detachment of Tai and Miao-Yao from Sino-Tibetan set up the need, which was not previously felt, for a 'parking space' for these two language families and eventually to the setting up of the Austro-Tai language stock. The point here is that the very existence of a dominant doctrine or theme tends to inhibit the development of ideas that might lead in other directions. In this case, the trail ultimately led to the overthrow of the China Doctrine itself, with archeological findings supporting the linguistic evidence as to the direction of early cultural movements in Southeast Asia from south to north. Much earlier Vavilov had found Southeast Asia to be a great early center of plant domestication but this view had been largely repressed as in conflict with the China Doctrine, e.g. at Harvard graduate students were taught that Vavilov had made a great contribution in the field but was wrong - 'of course' - about Southeast Asia. Everyone at Harvard - and the rest of the civilized world - 'knew' that this region was a miserable cultural backwater and that its inhabitants were quite incapable of creating any higher culture on their own. Strangely enough, even the Southeast Asians themselves were convinced of this! In view of all this, it is little wonder that when I first saw the connection between Tai and Austronesian I kept looking for roots for 'bow' and 'arrow' and the like. Similarly, I kept looking for ways to derive all the Tai/Chinese 'culture words' from Archaic Chinese forms, hardly suspecting that the direction of the loan could ever have been in the opposite direction! In a sense, one can say that eventually I was literally 'forced' to give up that doctrine, just as I had earlier been 'forced' to abandon the idea of Tai and Miao-Yao as components of Sino-Tibetan.

One can gain important insights from this case history: doctrines can be universal in their effect - and they die hard! This one, in particular, is dying a lingering death. Years ago, after my first Austro-Tai articles appeared, I was persuaded by a colleague to talk on this subject to a class of his graduate students. After the talk two of the students, both of Thai nationality, made a comment to my colleague along the following lines: "Dr. Benedict is a nice man and we know he's a very good scholar but, after all, everybody knows that we're really related to the Chinese".

This amused me at the time but was also a little shocking, coming as it did some 25 years after my original paper on Tai, Kadai and Indonesian. Since that time the doctrine has been severely battered, especially by Solheim and other archeologists and prehistorians, but has hardly yet been laid to rest. This is especially true of the field most affected by it, that of Tai studies, and it is a striking fact that up to the present day neither the Thai people themselves nor the specialists in Tai studies have shown any interest in breaking out of the 'China Doctrine' mold and away from the mainland. I sometimes even feel that if Maspero were around today, almost anywhere other than Briarcliff Manor, he'd feel right at home!

Another doctrinal matter concerns the traditional division in Southeast Asia between mainland and islands, which I have likened to the Monroe Doctrine.

I believe that this has operated at a deeper, largely unconscious, level as compared with the China Doctrine. The fact that both the Malay and the Cham and related peoples came ashore two or three millenia ago has had little effect here; in fact, Schmidt classified Cham as a 'mixed language' largely, it would appear, because it had broken the rules by moving to the mainland.

Last year we heard a most eloquent description of a curtain flung across Southeast Asia, a mysterious and essentially impenetrable barrier before which one stood in awe. Years before there had been political talk of a Bamboo Curtain in the region, but this sounded more like the Maginot Line. It was, of course, simply another way of looking at our Monroe Doctrine, which works here in both directions. A distinguished Austronesianist once confided in me that the Li and Austronesian numerals appear to be related but he was still offshore (on Hainan) and refused to step ashore so as to include the clearly cognate numerals of Laqua and other Kadai languages. Maspero had made the same observation at the turn of the century and he also would not step on the mainland. It has all been very much as if someone had planted a sign on Hainan: GOING TO THE MAINLAND IS FORBIDDEN.

Austro-Tai runs counter to this doctrine, of course, and this fact has apparently played a role in its neglect, for the most part, at the hands both of Austronesianists and of specialists in the mainland languages. It could be argued, I suppose, that anything that 'far out' or megalolinguistic is bound to be neglected in one way or another. I admit to a certain bias in any discussion of this subject but I do strongly feel that it is not that 'difficult' and that much of the skepticism expressed about the relationship reflects an underlying 'Monroe Doctrine'.

Let me cite one illustration. A noted scholar writes an article on possible affiliations of Miao-Yao but does not stray from the mainland in his speculations, giving not the slightest hint that any wider relationship might be possible. Even more to the point, two key basic roots, for 'die/kill' and 'bird', which not only connect Miao-Yao with Tai but also connect both of these families with Austronesian, are somehow relegated to mere footnotes, which completely disappear in the official abstract of the paper. Coincidental? I hardly think so. Intentional? Probably not, in view of the largely unconscious nature of this particular doctrine.

A third matter relating to doctrine is far more obscure and must be considered speculative. You've all probably been wondering just how I was going to get Japanese into this talk - and this is how. As probably most of you know by now, I have lately come to realize that Japanese is simply another component of Austro-Tai, with the evidence to date all pointing to a direct relationship with Austronesian. This is hardly a novel idea; it was presented at length by Labberton over 50 years ago and in recent years has been advocated by Kawamoto and others. The problem here is of a very special kind. In the case of the traditional Chinese-Tai hypothesis the main problem, as I've remarked in the past, is why anyone ever believed it. Here we have the converse problem: why has anyone not believed it. I'll admit that the hypothesis has been presented in an inadequate fashion but the cognate sets are so numerous and the comparative material in general is so rich that I find it difficult to see how the relationship could have been neglected these many years. This is particularly true of the last decade or two, as comparative work on the Formosan languages has so enriched our knowledge of the Austronesian family.

Can it be that some underlying doctrine is involved here as well? This does indeed appear to be the case and here I'll cite two illustrations. A noted Japanologist contributes a supposedly authoritative piece on Japanese and

Malayo-Polynesian. The remarkable thing about this contribution is that it seems to have been designed to prove that Japanese is not related to Malayo-Polynesian! And, believe me, one has to work real hard to do that! Or be incredibly inept as a linguist. Another illustration involves an excellent linguist with a far better knowledge of Japanese than mine (that could be almost anyone), who is analyzing one of the key language groups within Austronesian. He turns out a classic in the field, which minutely dissects out morphemes and lexical items of all sorts - with one glaring exception: the bound form of one of the numerals, a very obvious feature of the language that is bound to attract the attention of even the casual reader of this classic. It so happens that this bound form and the Japanese numeral are almost identical. Coincidental? Again, I think not. And certainly not intentional. Rather, it would appear, this fine linguist was being whispered to at some unconscious level: let's leave this one out - it looks so much like Japanese that even dummies will catch on and - let's face it - everyone knows that Japanese is not related to these languages.

I've described the underlying doctrine here as being obscure, perhaps only another way of saying that it lies at an unusually deep level. One can truly speak of a 'mindset' here but how is one to define the doctrine itself? Can it be that a special kind of 'Monroe Doctrine', involving only Japan and the Japanese, is involved here? Can it be connected with the old idea, fostered by Japanese and Western scholars alike, that Japanese is 'one of a kind' and therefore not to be compared with other languages? If so, we may have a clue here to the prominence of the Japanese/Korean hypothesis, which seems hardly justified on the basis of evidence assembled to date. One has to wonder about the large investment of time and funds in so unpromising a line of research, with the subsequent neglect of the much more obvious - and substantial - Japanese/Austronesian line. Unless, that is, this was the intent of the manœuvre, always at the unconscious level that we have in mind here.

I must admit that much of the foregoing discussion of doctrines, mindsets and such is highly speculative and perhaps will be dismissed by many as psychiatric fantasizing on my part. And some mean critic is certain to point out that if I'm wrong about Austro-Tai and Japanese and the rest of it, then the need to talk about doctrines will evaporate; that I have in effect been running around Southeast Asia making imaginary discoveries, then inventing imaginary doctrines to explain these pseudo-discoveries. As I've indicated, only a really mean person would talk like this; in practice, I can only have faith in my own findings, which do very definitely point up the existence of these underlying themes in Southeast Asian linguistics as a whole.

I'd like now to turn to a less speculative topic: the role that anthropology has played in Southeast Asian linguistics. I came into the field as a graduate student in anthropology, one of the last two turned out at Harvard under a program that covered all aspects of the subject: primate and hominid evolution; physical anthropology; prehistory and archaeology; ethnography and ethnology; linguistics and anthropological theory as well as all areas of the world. Anthropology was taken very literally to mean, in the words of a famous definition by Malinowski, 'the science of man, embracing woman'. If they had had Women's Studies at Harvard in those days it would have come under anthropology. As you can see, linguistics was simply one of the fields of anthropology.

This was the tradition of Boas and was carried on at Berkeley by his outstanding student, A. L. Kroeber. It was Kroeber, not the linguists or Orientalists at Berkeley, who had the foresight and courage - as it turned out! - to sponsor the Sino-Tibetan Philology project. It was Kroeber who had

written about diffusion and thus was in a position to understand my reasoning for eliminating Tai and Miao-Yao from the project. As for myself, my views at the time had been largely shaped by anthropologists and by anthropologically oriented linguists like Swadesh, who pioneered in developing the concept of 'core' vocabulary. My first paper in the Austro-Tai field, on Tai, Kadai and Indonesian, was published in the American Anthropologist even though it was almost exclusively linguistic in content. I recall Kroeber's remark at the time, when he suggested that I send the article to that journal, that my best chance for publication lay there - 'because only anthropologists understand those things' - or words to that effect - thus by inference demeaning both linguists and Orientalists.

When I later returned to the field I discovered that the main support for Tai/Kadai/Indonesian had come from Greenberg, an anthropologically oriented linguist, with the conventional linguists having spent most of their time guarding their several territories, so to speak, with a spectacular avoidance of the basic questions involved, always under the baneful influence of the 'Monroe Doctrine' of Southeast Asia. The Austro-Tai book was published by an anthropological organization, the HRAF Press, and the Foreword to the book was contributed by Ward Goodenough, an anthropologist/linguist. The earlier Austro-Tai articles were accepted for publication by Frank LeBar, an anthropologist, in the exclusively anthropological Behavior Science Notes, in view of what he saw as the importance of the work. To this very day only Haudricourt has attempted to make any serious contribution to the Austro-Tai field and God knows that whatever he is - among other things, one of the world's great botanists - he is not a conventional linguist.²

I could go on and on like this. The first substantial presentation of the Japanese/Malayo-Polynesian hypothesis in the 20's was by an ethnologist/linguist: Labberton. The famous work on Austroasiatic, from a still earlier period, was by Schmidt, who was primarily an anthropologist/ethnologist. It was also Schmidt who introduced the 'Austrie' concept, proving that even anthropologists can make mistakes on occasion. Skipping now to the present, we can rejoice in the fact that Bob Blust, perhaps now the leading figure in the Austronesian field, was trained as an anthropologist - and that he gives evidence of this in the breadth of his studies.

What have the linguists been doing all this time? I wouldn't know! That's not so, really, since I have made good use of the many excellent studies that they have turned out. But I submit that there has been a remarkable lack of work at the broader or megalolinguistic levels in Southeast Asia on the part of conventional linguists. In view of all this I have wondered if it might not be wise to make anthropological training a part of linguistic programs. But I'm hopelessly biased here, of course, and I'm delighted when a Blust comes along and attacks some of the broader problems in the field. I'd be even more delighted, I must say, if he were to accept Austro-Tai. He has only a Master's in anthropology, however, and that might not be quite enough.

How is one to capture the essence of the anthropological vs. the conventional linguistic approach? I'm not sure but one of the distinctions lies in the fact that the former often involves worries unknown to the latter. One example in the Southeast Asian field furnishes a beautiful illustration here. Blust turned up an apparent root for 'iron' at an early Proto-Austronesian level and I turned up apparent cognates for the same root at an even earlier Proto-Austro-Tai level and both of us expressed the same concern: no such root should exist! At roughly the same time a conventional linguist

² Haudricourt (p.c.) has modestly denied that he is a 'great botanist' but, significantly, does not lay claim to being a 'conventional linguist'.

made use of the same root for 'iron', along with one for 'needle', as primary evidence for reviving a long-dormant Sino-Tibetan-Tai-Miao-Yao stock - and he didn't express any reservations about either root. Nor even any amazement that both roots had survived from the proto-period at the expense of a few other lexical items, e.g. all the roots for body parts! Granted that this is an extreme case, the fact remains that anthropological training has the effect of placing lexical items in a broad cultural perspective, with effects more often seen at deeper, megalolinguistic levels. I rest my case.

My last topic concerns certain 'mysteries' of Southeast Asian linguistics. These are of two kinds, really, and the first is readily disposed of. These are the problems for which solutions are not only lacking but appear to be impossible. The most notable, perhaps, is the problem of tone assignment in Kadai and Miao-Yao, although recently there is a glimmer of a possible solution here, with the Japanese pitch-accents supplying a clue to an underlying Proto-Austro-Tai accentual system that gave rise to this feature as well as to the mainland tonal systems and the several stress or length systems of Austronesian.

At times as I have made my way through the vast maze of Austro-Tai I have experienced the uncanny feeling that I am working out something that has been programmed for struggling linguists like myself. It is all very much as if I am following a trail of planted clues, arranged in such a fashion as to let one know that he is on the right trail without making things look too easy for him. I have had similar experiences on occasion also in working with Sino-Tibetan but not nearly on the same scale, perhaps because the Austro-Tai relationships have unfolded so gradually over a period of many years. But let me illustrate what I mean with several cognate sets (out of deference to Bill Gedney, I might add that the Tai forms are all on tone *A).

FIRE The earliest available clues were Proto-Malayo-Polynesian *ʔapuy and the Proto-Tai doublet: *vay^A ~ *vii^A. Fortunately I followed these clues and set the pair up as a cognate set, even without being able to explain the Proto-Tai forms very well, but just think how easier it would have been if I had known about the Kam cognate pwi^A, also on tone *A; a young colleague of mine once confessed that he never really believed in Austro-Tai until he ran across this Kam form. As for the /a/ vocalism of Proto-Tai *vay^A, only years later was I able to figure out that it is part of the most diabolical conspiracy against linguists that has ever been devised - a manoeuvre that I have called 'vocalic transfer', which operates in Southeast Asia to disguise clue after clue. I have encountered this time after time - key cognates stuck away in less accessible languages - and I'm not sure that it's altogether fair. But I must say that in this case the situation in Japanese more than made up for it. Eighth century Japanese had an 8-vowel system that was on the verge of replacement by a 5-vowel system; the Japanese cognate for this root: hi represents an earlier *pui. It so happens that Japanese was first recorded in any detail at this period; if this had occurred a century or so later these critical vocalic distinctions would have been lost forever and the task of demonstrating the Japanese/Austro-Tai relationship would have been nearly impossible. Did I say, 'It so happens.'?

GOD/SPIRIT The basic clues here were not too difficult: Formosan has /pili/ 'shadow' (with velar ɭ) and Proto-Tai has *phrii^A 'god', with English 'shade' offering a semantic parallel. Japanese has /hi/ 'sun', from an earlier *pi (contrasting with the above form for 'fire'), and it turns out that Japanese resembles Miao-Yao, and contrasts with Kadai, in making regular use of canonical reduction-on-the-right, so that *pi can well represent an earlier *pili or the like. The linguist obviously needs here, however, to connect the

form with the Formosan/Tai root hence a few clues were sprinkled about, e.g. compound forms such as /hiko/ 'prince; male god', not 'sun-child', as it appears, but 'god-child'; /hijiri/ 'saint', not 'sun-knower', as it appears, but 'god-knower'; also, even better, the Old Japanese form: Fi has the additional gloss of 'spirit' and appears in the names of gods in the Kojiki. But how about the final *-li element, which should yield /ri/ in Japanese? Might not some doubting Thomas - or William - dismiss it all as one of those remarkable coincidences that occur so often in Austro-Tai? Clearly another clue was indicated, so why not provide a 'split allofam' - those are always good fun. Sure enough, Japanese has the compound form: inari 'god of harvest', with ina- clearly standing for 'rice' and -ri as one of those elements so felicitously named 'morphans' by Jim Matisoff. Even the indication of sun worship on the mainland at a very early period can be fitted into a larger cultural framework since the adjoining Sino-Tibetan peoples probably followed the same practice, as witness the likely etymology of the Sino-Tibetan root for 'sun' from a core root for 'being', as already indicated in Matisoff's linguistic/theological tract on God, etc.

DIE/KILL The first clues here were real easy: Proto-Malayo-Polynesian *ma-tay 'die' and *pa-tay 'kill'; Proto-Tai *t(h)aay^A 'die', with unexplained initial aspiration in Central Tai. A perfect parallel, also from the 'core' vocabulary, was provided: Proto-Malayo-Polynesian *mata 'eye'; Proto-Tai *t(h)aa^A, also with initial aspiration in Central Tai. These clues obviously were designed to induce someone to enter the field - and they worked - but they were far too good to be true. I should have suspected that something more complicated was lurking below the surface; easy clues like that always bear watching. Soon further clues came in from Northern Tai, eventually including the totally unexpected Saek forms: praay^A 'die' and pra^A 'eye', and things looked bad for a while. But lo and behold! all was not lost - the Formosan clues finally came into the picture and they showed that a special Proto-Austronesian reflex: capital *C, had to be reconstructed in both roots, nicely confirming the mainland evidence for consonant clusters. As for the Miao-Yao clues, they were found to involve a bold attempt at arrant deception - another bit of unfairness, I felt. The dental reflexes were only to be expected, of course, but Proto-Miao-Yao *day^C 'die' and *tay^C 'kill' (both on tone *C) happen to fit in the most precise way the Tibeto-Burman verbal paradigm of intransitive/stative with voiced stop initial vs. transitive/causative with unvoiced stop initial. I saw through this, of course, though it did take a bit of doing to explain the initial *d- of 'die' (from the unprefix root, with regular secondary voicing). The Japanese clue was not too difficult in this root: /hate/ 'end'; /hate-ri/ 'to end; die', from *pate, incorporating the prefixed *pa- and with regular final -e from *-ay. The lapsing of function presented no real problem in view of its frequency in Austronesian, but just to allay any possible doubts a perfect Malayo-Polynesian parallel was thoughtfully provided: Toba-Batak mate 'dead'; pate 'come to an end'. I really appreciated this but I did harbor some resentment over the attempt at deception in Miao-Yao.

DOOR In this final root I have no complaints, for a change. The original clues were straightforward: Proto-Malayo-Polynesian *pintu and Proto-Tai *tuu^A. The later Formosan clue, however, showed unexpected vocalism: Thao pitaw (with the /n/ = nasal increment). Dyen has suggested that Proto-Austronesian had the /ə/ = shwa vowel before final *-w and *-y glides, but strangely enough, in all the vast amount of Austronesian comparative material, this appears to be the only root in which the shwa vowel is unmistakably indicated: Proto-Austronesian *pi(n)taw. It turns out that the same final is also indicated for Proto-Kadai on the basis of the Proto-Kam-Sui form: *too^A,

on the same *A tone as the Tai root. Now 'door' is not the most 'basic' of 'basic' lexical items, e.g. it is lacking as a simple root in Sino-Tibetan, yet Japanese has the perfect cognate here: to, with regular final -o from *-gw as contrasted with final -u from *-u. The shwa reconstruction is of basic importance in Austro-Tai and it does seem that this one root, with its unique representation in Austronesian, was supplied to insure that the linguist not go astray here.

I cannot close this talk with any of you wondering whether Benedict has flipped his lid - or should I say, has really flipped his lid? Everyone knows, of course, that none of this can really be so. I have to go along with that, although rather reluctantly, I must admit - but it does seem so. It'd be nice if we could make this all very scientific by citing odds on this and that but my mathematician friends have always thrown up their hands in despair when handed questions of this kind. Which is very good since it allows us all to continue being unscientific. And at times I must think it is so - or why otherwise have I found myself cursing the powers that be after finding a key cognate lacking in some language, muttering things like, 'How can they do this to me?'. And why at other times have I felt like blessing the source of some goodie that I've encountered in my work. Bill Gedney has written about my 'affection' for Saek final -l but this scarcely expresses the depth of my feelings; in fact, I love Saek final -l and have for many years now been carrying on a love affair with the whole Saek language. And I adore whatever preternatural power that arranged for it all - and for having a great and generous linguist like Bill Gedney record the language. But I return to the basic question here: is reality really what it seems to be? Can it be that we are only looking at it from the wrong perspectives? I close with the following quote from a cognitive psychologist:³

All that is granted to us are surface appearances behind which ultimate truth remains forever hidden. Surely that is the human condition.

³ George A. Miller, book review of Sir John Eccles and Daniel N. Robinson: The Wonder of Being Human: Our Brain and our Mind, New York Times Book Review, Aug. 26, 1984.