

CRITIQUE OF LINGUISTIC REASON, I:
PRAGMATIC PREREQUISITES TO UNIVERSAL
GRAMMATICAL ANALYSIS

by
Wataru Koyama

In this paper, I shall argue that (1) there are enough points of intersection between not only 'natural language' and 'linguistic theory', but also between what native language users do and what linguists do, to warrant the inclusive treatment of these two analytically distinct domains; (2) synchronic 'symbolic' systems, such as linguistic structures and linguistic theories, are too interdependent with their synchronic pragmatic contexts to warrant the non-critical acceptance of universal claims of rationalist linguistics and pragmatics; and (3) not only synchronic pragmatic contexts, but also synchronic symbolic systems cannot be identified without investigating the diachronic processes that have created their historical conditions, which always partially determine them; thus, it is unwarranted to generate achronic universals without considering the historical dimension.

1. *Why cognitive linguistics needs the sociologist*

About forty years ago, in his critical review of Joos' 1957 anthology of neo-Bloomfieldian papers, *Readings in linguistics*, Carl Voegelin wrote:

It would be possible to list an impressively high number of interests in linguistics which lie outside of the selection range of the *Readings*... The selection of the majority of papers... seems confined to exemplars of the aim *toward a final linguistic analysis* (b), without ever bothering about (a), *techniques for a preliminary analysis*, despite the fact that (a) is prerequisite to (b). However, since every (b) is preceded by an application of (a), it might be argued that every (b) is a restatement of results obtained from (a). The minimum of descriptive linguistics is surely a consideration of both (a) and (b). (Voegelin 1958:86; emphases original)

Here, Voegelin pointedly accused the editor, and in effect the authors, of *neglecting* the question of 'discovery procedure', with which they would be accused of having been unnecessarily *preoccupied* to the detriment of the 'properly scientific' question of 'evaluation procedure'. And even a cursory look at Joos (1957) confirms

Voegelin's observation, as we find in this anthology of the allegedly *empiricist* program, no systematic discussion of 'field technique', 'elicitation procedure', 'sampling', and similar notions which typically appear in handbooks of more empirically-oriented disciplines, e.g., sociolinguistics, anthropological linguistics, and pragmatics.

A moment's historical reflection, however, suffices to solve this little puzzle. After all, neo-Bloomfieldians were, just like their generativist successors, preoccupied with less empirical problems of 'structural restatements' (cf. Voegelin and Voegelin 1963), and not involved in field work as intensively as Boas, Sapir, Bloomfield, and those who have kept working in the 'American ethnographic tradition', both practically and *theoretically*. Thus, the two problematics to which the 'founding fathers' of 20th century American linguistics struggled to respond, namely, (1) the question of how linguists, who necessarily live in some *socio-historical contexts*, do and should abstract 'data' for their strictly structural analyses from the cultural contexts in which such data are, necessarily, partly found and partly constructed, and (2) the problematic of coherently accounting for both synchronic and *historical* linguistics (cf. Boas 1911; Sapir 1921; Bloomfield 1933) – these problematics of relating *indexicality* with symbolicity without treating the first as a residual problem of the second were quietly replaced with the *achronic, non-indexical* problematic of explicit formalization of *symbolic* systems by the neo-Bloomfieldians. Subsequently, this 'purely' symbolic problematic had become so presupposable that it was taken to be *the* natural theme of 'linguistic theory' by the Chomskyans, who thought they had a better answer to the *same kind of question*, witness the historical fact that they were able to formulate their contentions in terms of *technical* issues of structural analysis, as opposed to *general* linguistic issues of the relationships between 'language' and 'culture', synchrony and diachrony, and the grammarian and her 'language and culture', or the status of 'linguistics' as *Natur- vs. Kulturwissenschaft* (see below; also cf. Hymes and Fought 1975; Silverstein 1986; Stocking 1996; Koyama 1997c, 2000).

No one is better suited to appreciate this real locus of 'paradigm shift' in 20th century Anglo-American linguistics than we pragmatists, who know how fundamental the indexical-symbolic distinction is, and how imperative and difficult it is to bring together irreducibly indexical facts of 'culture' and 'history', on the one hand, and

similarly irreducibly symbolic facts of 'linguistic structure', on the other (cf. Silverstein 1976a). And no one is better suited than we pragmatists to appreciate the relevance of Voegelin's observation to today's linguistics, and show that it is not justifiable to interpret the difficulty of constructing 'discovery procedure' as licensing the construction of linguistic structural theories without any general pragmatic theory of data construction. Indeed, I shall argue that the rigid separation of 'discovery' and 'evaluation procedure' is spurious, as the *evaluation* of universal validity of theoretical claims about linguistic structure crucially involves the theoretical consideration of *pragmatic contexts* of not only languages, but also of linguistic theories and, ultimately, of linguists themselves.

Thus, the aim of this paper is to show that, unless we systematically investigate the socio-cultural and historical, that is, pragmatic contexts of not only language, but also of linguistic analysis, we remain deprived of any theoretical means to discern whether universal claims of linguistic analysis are *epistemologically* valid. In other words, putative universals formulated by particular linguistic theories which do not possess analytical tools to systematically investigate pragmatic contexts of languages and linguistic theories need to be 'tested' by systematically investigating such pragmatic contexts, lest we remain epistemologically uncertain whether these putative universals are 'real' universals, which obtain despite the necessarily limited, and often deliberately narrowed, perspectives and historical conditions of these theories.

Let us start with articulating a few epistemological issues implicitly suggested in this thesis. First of all, at least conceptually, the two parts of the thesis, the one concerning 'object language' (natural language) and the other its '*metalanguage*' (linguistic analysis), are independent of each other. Thus, on this conceptual ground, one might object to the inclusive treatment of the two analytically independent problems, which might be more 'fruitfully' dealt with if analyzed separately, 'fruitfulness' measured in terms of such evaluative criteria as 'clarity' and 'precision'. Let us scrutinize this objection, as it seems to lead to several important methodological issues.

To begin with, we must note that these evaluative criteria, clarity and precision, are *themselves* quite vague and imprecise, at least without further specification¹. Yet, we should *not* dismiss the objection to the inclusive treatment on the grounds that it is self-

defeating, since it becomes so only when one starts applying these criteria to 'meta'-evaluations of evaluative criteria themselves, at the 'second-order' of evaluative hierarchy. (To be precise, if one applies these criteria to meta-evaluations of some evaluative criteria only, say 'cross-disciplinary coherence' and 'practical importance', but not clarity and precision themselves, it does not become self-defeating; but then it becomes arbitrary, and justifiable only tautologically; cf. Putnam 1992). Thus, *in itself*, the vagueness of the notions of 'clarity' and 'precision' does not seem to undermine the validity of the objection to the inclusive treatment, although it is important to see the limitations on the domain to which the criteria can be applied without causing methodological difficulty (see below).

Yet, even at the *first-order* of evaluative hierarchy, these evaluative criteria allow a multitude of interpretations when it comes to evaluating various specific features of theories. Think of the familiar case of the 'simplicity' ('economy') criterion in the early generativist literature on phonological description, a problem of which we still have no clear conceptual grasp, let alone a solution². That is, given a multitude of intricately interrelated phonological notions and levels of analysis (e.g., 'underlying' and 'surface representations', 'distinctive features', 'phonemic inventory', various kinds of 'rules' and 'filters', various kinds of interacting patterns of 'rules'), where 'simplicity' for one analytic unit usually means 'complexity' for another, it is not easy to formulate a justifiable system that can tell us how to maximize the simplicity of a phonological analysis of even a single language. Considering that 'simplicity' is much easier to translate into specifics of grammatical analysis than 'clarity' and 'precision', we might conclude that these notions are quite 'vague' in terms of their interpretability even at the first-order level of analysis.

Moreover, thirdly, subcategorical variants of each of these evaluative criteria often have contrary values; and fourthly, the two evaluative criteria may behave differently from each other, at the first-order level. Recall, for instance, that *formal* precision (or 'clarity' in some sense) is often accompanied by the lack of *conceptual* clarity. For reasons of familiarity and perspicuity, I again refer to the generativist program, in which truly unprecedented sophistication in formal precision has not resulted in much conceptual clarification in such matters as abstractness *vs.* naturalness, formal *vs.* functional explanation, structural *vs.* surface representational description of phonological regularities (interaction of rules *vs.* phonotactic filters),

and the phonemic *vs.* phonetic basis of distinctive features. To be sure, these obviously important issues have been talked about vigorously in the research program, but usually, such clearly interrelated issues are treated separately, only one topic being discussed at a time, at relatively low levels of methodological abstraction, where 'bits and pieces' of empirical evidence are used as illustrative examples to argue for some theoretical interpretations of the topic and against others³. It is fairly easy to see that the implicit primary criteria which justify this practice are '*topical* clarity' and '*empirical* accountability', two notions which are basically *pragmatic* in character, as distinct from 'conceptual, *semantic* clarity'. Thus, generativism has been circling around one important conceptual 'core' of these interrelated notions, since all these dichotomies are ultimately related to the evaluative criteria of (1) symbolic system-internal regularity *vs.* (2) correspondence to indexical phenomena which obtain fairly independently of such symbolic systems⁴, where the 'system' may be that of the 'object language' or 'metalanguage' (phonological theory). (See below; cf. also Silverstein 1976a, 1987; and Koyama 1997a, b, 1999).

Thus, there are several kinds of vagueness of 'clarity' and 'precision' even at the first-order of evaluative hierarchy. These kinds of vagueness, however, also fail to provide a convincing reason for dismissing the objection to the inclusive treatment of the two problems, to the extent that this vagueness is involved in *all* evaluative criteria. Thus, we must duly recognize the validity of the objection, insofar as we share the assumption that clarity and precision are desirable, even though we must explicitly note that the validity of the objection depends on whether or not one espouses these evaluative criteria (see below). Accordingly, I shall separately treat the two problems when that is possible, or rather, deemed appropriate, given the following. That is, I take strong objection to the assumption that these are the only criteria which are relevant, or at least the most important ones. At the very least, the fact that it is possible to 'refer to' these two problems in one coherent sentence, as I have done above, suggests that they have many points of intersection; hence the criterion of 'comprehensiveness' is relevant. In other words, it suggests that the two problems must also be investigated together (aggregately) or as a whole (collectively) in a comprehensive framework, without which there is no clear way of systematically analyzing their interrelationship and, indeed, the problems

themselves, to the extent that each problem is partially constituted by such interrelationships⁵. Note that the objection to the espousal of the criterion of comprehensiveness on the grounds that the concept is not 'clear' or 'precise' is not tenable as it stands, as we have seen above.

Let us, then, assume that we must approach the two problems comprehensively. Here, one may start with elaborating a comprehensive framework and then apply its analytical tools to the two problems, 'from the top down', as it were; *or*, in theory, one may proceed 'from the bottom up', first primarily dealing with each of the problems as constitutive part of the totality (i.e., without 'losing the big picture'), and then trying to identify their interacting patterns. While the latter procedure satisfies the criterion of comprehensiveness, its methodological false cognate, which deals with only one problem 'autonomously', or 'independently' (Jakobson 1990), without articulating its relation to the other or the whole, does not (see Newmeyer 1986; cf. Koyama 1997b).

In practice, however, even this procedure, called 'analytic(al)' in the epistemological literature, almost always results in fragmentation in both theoretical and institutional terms, as pointed out by Newman (1951:185) *vis-à-vis* the 'narrowing' of Sapir's *general* linguistics into what we now call 'autonomous linguistics'. Since this is an empirical observation concerning scientific inquiries in our times in general, and this directly leads to central themes of this paper such as 'universality' and 'socio-historical contextualization of knowledge', we must now move to explore the issue of a 'comprehensive' *vs.* 'atomizing' approach in a broader matrix of socio-historical conditions of scientific inquiries in our culture. For expository purposes, I shall proceed to deal with the following topics in the order named: (1) general characteristics of scientific specialization; (2) historical relativity of the contemporary stereotypes about the nature of 'science' which appear to justify such specialization; (3) dependency of the universality of scientific claims upon the critical analysis of historical conditions of the sciences; (4) explanation for the atomization of the sciences; and (5) desirability of the comprehensive approach, where (1)-(4) collectively support (5).

First, to approach (1), we must take heed of sociological disciplines which have, for more than a century, investigated modern conditions of scientific knowledge formation⁶, and which strongly support the observation that the analytic procedure, *as a matter of social fact*⁷,

does lead to a proliferation of progressively specialized sciences. Typically, these sciences (a) claim 'disciplinary autonomy'; (b) demand from their in-group members technical expertise, but not the ability to formulate a comprehensive view of phenomena separately covered by various sciences ('*Bildung*'), an ability often stigmatized as 'amateurism' or 'dilettantism'; (c) articulate a 'web' of *decisively* theory-dependent, 'incommensurable' or 'untranslatable' concepts; (d) devote themselves to solving 'puzzles' which cannot be characterized but theory-internally; (e) measure 'progress' in terms of their 'puzzle-solving' capacity⁸ (cf. Weber 1989 [1919]; Kuhn 1970; Feyerabend 1975; Kuklick 1977; Murray 1983; Danziger 1990; Ringer 1990).

Here, let me underline that sociologists have identified this as a macro-social *fact*, a *positive* state of affairs which is 'objectively' *given to us*, prior to our agentive understandings or reactions. In other words, as agents living in a universe where this state of affairs validly obtains, fairly independently of our agentive consciousness and intentional actions, we cannot but presuppose, consciously or not, this condition as a context of our epistemically-focused acts (cf. Koyama 1997a). In this positive sense, 'specialized science' is what 'science' *is* for us, and thus constitutes the prototype of our 'cultural stereotypes' about 'science' (cf. Putnam 1975), *whatever its epistemological justifiability might be*, which is a very different question. Accordingly, we have even formulated various perspectivalist, pluralist, or pragmatist epistemological theories which fairly accurately *describe* the relativistic and instrumentalistic conditions of today's sciences, and which can be easily interpreted as *justifying*, and are often used to justify tautologically, the *status quo*: 'positivity' (see the references cited above).

But, we must keep proceeding carefully. We must ask if we are justified to interpret the italicized copula *is*, formally [-past], of 'specialized science is what 'science' *is* for us', not as the present tense (un-past), but as generic atemporality or nomic truth (non-past). As a matter of *historical fact*, it is clear that this condition *has become* what 'science' *is*, and there is every reason to assume that it will *not* be the prototype of 'science' in the future, even though it now *appears* to possess the intensional property of 'scientific-ness' as its inherent essence, seen from our *agentive* perspectives located *hic et nunc*. Hence, if our sciences aspire to claim 'genuine' universality, the validity of which is not restricted to what we have here and now, we must first understand their 'historical relativity' and then try to figure out which universal claims of our sciences are likely to be spurious

products of the epistemological condition, the validity of which is limited to the present. To use the standard terminology of post-Kantian critical philosophy and sociology, we must use not only our instrumental rationality towards what we purport to study at the first-order, but also our 'critical rationality' towards the actual condition of sciences at the second-order. Otherwise, our universals would remain a hodgepodge of genuine and spurious universals, the latter being no more than 'ethnocentric' pseudo-universals: e.g., the 'evolutionary universals' of 19th century positivist social anthropology (cf. Stocking 1982) and possibly, some of our 20th century 'linguistic universals', as suggested below (cf. Hacking 1979; Koyama 1997a).

Thus, *a fortiori*, we should squarely face negative consequences of progressive scientific specialization. Along with all these desiderata like formal precision, conclusive solutions to very specific (and theory-dependent) questions, technical sophistication, aesthetic satisfaction (cf. 'elegance'), prestige of 'science', and more mundane objects of sociological analysis (cf. Danziger 1990), sociologists have also identified its negative consequences, somewhat abstractly called the 'eclipses' of 'critical rationality' and 'totality' in the Continental jargon (cf. Horkheimer and Adorno 1994 [1944]; Jay 1984). Translated into more familiar terms, 'totality' includes general epistemology and comprehensive perspectives⁹, which have come to be perceived increasingly obscurely, and increasingly as obscure *in themselves*, and even irrelevant. This obtains, I think, precisely as a functional consequence of advances made *within* specialized theories or sciences, the dynamic of which is motivated by the criteria by which such advances are measured: i.e., precision, clarity, and others (such as empirical adequacy and simplicity), but not comprehensiveness, in any *real* sense.

Let me briefly explicate this psycho-social process, social and phenomenological in character, distinctly different from phenomena investigated by individualist 'cognitive psychology', which may *appear* more 'scientific' to us (cf. Putnam 1973; Gigerenzer *et al.* 1989:203-34; Danziger 1990; see below). First, at the level of social fact, it is obvious that specializing sciences, which justify their perspectival narrowing on the grounds of the criteria of clarity, precision, and the like, have 'naturally' realized an indisputable increase in the degree of clarity and precision of scientific analysis in some limited senses of these terms (see above).

Second, because 'clarity' and 'obscurity' are notions relative to each other, it is similarly natural that this increase is perceived as equivalently meaning the marked lack of clarity and precision in comprehensive theories, *seen from the agentive, 'phenomenological' perspectives* of specializing scientists who do not take a critical, 'objective' stance towards this social dynamic. This perception of obscurity (if not an ascription of obscurantism) is then corroborated by these specialized agents' unfamiliarity with comprehensive theories, which are often extremely, if not excessively, rigorous and precise in their theoretical formulations (e.g., Peirce 1932; Derrida 1967). Thirdly, on the basis of this agentive, phenomenologically 'real' and even 'correct' apperception, these agents naturally attribute the *contextual* characteristic of 'obscurity', which obtains, as we have seen, *relationally between* the perceiving agents and the perceived objects, to the objects (comprehensive theories) *themselves*, which are thus construed to have 'obscurity' as their 'essential', intensional property¹⁰. Then, on the basis of this *agentive, perspectival* (mis)recognition and essentializing reification of the *social relation*, the agents seem to develop their aversion to comprehensive theories, here identified as 'the obscure', or in more cross-culturally generalizable terms, the Durkheimian 'profane'. Thus, they strengthen their group-identities and the autonomy of their group-activities 'negatively' on the basis of their shared avoidance of it (a 'tabu'), and 'positively' on the basis of their shared loyalty to the symbolic norms, or Durkheimian 'totems', of 'clarity' and 'precision'. Obeying the Lévi-Straussian 'primitive logic' of categorical 'binary opposition', consequently, those scientists who 'worship' the criteria of 'clarity' and 'precision' seem to forget that their theories and practices include much obscurity in various aspects, and that these are not the only criteria which are relevant to evaluations of sciences (see above). Thus, we now at least have a general and tentative, yet *cross-culturally, universally projectible* account of how the analytic procedure degenerates, *as a matter of social fact*, into the ritual avoidance of comprehensive theories and the observance of theoretical and institutional autonomization (cf. Durkheim 1965 [1915]; Lévi-Strauss 1963, 1967; Douglas 1966).

Of course, the cogency, if not intelligibility, of this account depends on one's familiarity with cultural anthropology and comparative sociology. In fact, some might question the 'scientific' validity of the notion of 'essentialization', or identify its use in this

account as *ad hoc*. Hence, we are obliged to note that the notion of 'essentialization' is required, independent of its use in this account, for the explication of such *linguistic* phenomena as 'honorific registers', where language users behave as if certain linguistic types had 'deferential illocutionary forces' *in themselves*, even though 'deference' obtains *relationally in indexical contexts*. And similarly, *mutatis mutandis*, for other registers (cf. Silverstein 1992b:316; Koyama 1997c). Simply, the phenomenon appears so widely that we must infer that the interpretation of its use in the account as *ad hoc* is cogent only for those who have not investigated it at all: i.e., the interpretation is viciously circular.

Indeed, the cross-cultural validity of this notion, and hence its 'scientific' validity in a strong sense, have been variously remarked upon, under such names as 'reification', 'hypostatization', 'objectification', 'naturalization', 'totemic emblemization', 'fetishization', by a truly impressive variety of philosophical and sociological theories which have tried to identify cross-culturally observable, universal psychological phenomena, without ethnocentrically relying on our 'folk' notions of *individualist* psychology. This psychology is known to have risen, along with individualist sociology (in the spirit of Hobbes, in particular; cf. Sahlins 1976), in the 17th century mercantile, proto-industrial capitalist societies which started to espouse and socially implement – on a universal scale, as it turned out – the doctrines of *individual* rights and properties. It would require almost complete historiographical and sociological naïveté to interpret this co-occurrence as merely fortuitous and deprived of causal interaction. Indeed, the epistemological validity of claims of individualist psychology beyond the 'modern culture'¹¹ appears even more dubious, in view of the historical fact that these psychological, legal, and political-economic individualisms diachronically showed up, and synchronically cohere, with other prototypical features of modern societies and sciences, such as (1) religious individualism and liberalism (cf. Protestantism, latitudinarianism, 'ethical' relativism); (2) anti-theocentric (anthropocentric), anti-Scholastic, and anti-deterministic epistemological probabilism, scepticism, and relativism; and (3) instrumental rationality, utilitarianism, and proto-Benthamite experimental 'techno-science', as most prominently illustrated in the works of Bacon, Descartes, and Locke (cf. Weber 1992 [1904-5]; Blumenberg 1987 [1975]; Taylor 1989; Danziger 1990:186 *et passim*). Naturally, in critical cross-cultural theories,

'essentialization', or its meta-theoretical equivalent, is considered much more robustly universal than those putative psychological universals which conceptually depend on the theoretical objectification of 'the mind' as an aggregate of neuro-physiological or 'functional states' in the individual's brain, despite the latter's compelling phenomenological reality to us as agents living in the societies where psychological individualism has become naturalized, legally institutionalized, and factually presupposable (cf. also Putnam 1975, 1988).

Of course, as *agents* deeply engaged in specialized sciences, we are inclined to believe that the anthropological notions utilized above are applicable only to those 'primitive societies' for the analysis of which comparative sociologists have found these notions valid or useful, but not to our own 'developed' cultures, *inter alia* the professional cultures of specialized sciences. Notice, however, that these 'primitive societies' have sciences of their own, called 'ethno-sciences' in the literature, whose claim to rationality and universality is, theoretically speaking, on a par with that of our own 'sciences' (i.e., our 'ethnoscience'). To be sure, the 'laws' these sciences have formulated may not seem to us as 'universal' as our laws (cf. Lévi-Strauss 1967), but there is no epistemological guarantee that this 'universality' *itself* is not our own ethnoscientific notion; hence our 'universals' might turn out to be ethnocentric universals, the validity of which is restricted to the particular ways we interpret 'universality' and, more generally, to the particular cultures which we have *hic et nunc* (see below). Obviously, to claim that our sciences have 'genuine' universality on the basis of their ability to formulate laws which are 'universal' according to *our* notion of 'universality' would be a mere tautology (*petitio principii*), and it is only by critically examining which of our scientific theses are transparently reducible to the socio-historical, pragmatic contexts which are obviously not universal from cross-cultural perspectives, that we can differentiate, if at all, 'genuine' and 'spurious' universals.

Having examined the problematics raised by the objection to the inclusive treatment of the two problems in some detail, I believe it should be obvious that, independently of specialized theories dealing with only one of the two problems addressed by the thesis of this paper, we need a sufficiently articulated theory comprehensively covering both of them. Thus, we now move to consider another objection to my main thesis. That is, one might find trivial the

problematic of the thesis, especially the part which concerns the dependency of the validity of universal claims of linguistic analysis on its pragmatic context. Certainly, it is merely a truism that any linguistic analysis takes place within a 'research program', 'disciplinary matrix', etc., which is 'embedded' in a larger 'culture', and thus there is a possibility, or even a high likelihood, of investigable interrelationships, analytic or causal, between these three sociological levels, even though we should not assume, from the outset, any particular directionality of such interrelationships, let alone the reducibility of linguistic analysis to characteristics of general culture. However, to the extent that we can articulate, in theoretical terms, that (1) there is a sense in which the very notion of 'universality' calls for cross-disciplinary and -cultural comparisons, as I have hinted at above, and (2) there is convincing evidence to indicate that not only linguistic structure and language use, but also linguistic analysis itself interacts with the general cultural context in which it takes place, in such a way that cultural variables systematically affect the linguistic analyses of grammarians who have no theoretical means to control the cultural variables of which they are theoretically unaware; and to that extent, we can conceptually and empirically substantiate the truism. For expository purposes, I shall first deal with (1) the conceptual, general epistemological issue, and then with (2) the empirical, specifically 'linguistic' issue, concerning both the theoretical objects of the linguistic sciences and such sciences themselves.

2. *Universality vs. ethnocentricity: Language, culture, and their disciplinary matrices*

Since I have already explicated, albeit in general terms, in what sense the very notion of 'universality' calls for cross-disciplinary and -cultural comparisons, I start this section with some observations that might help to bring this issue closer to 'linguistics', as we know it. To begin with, in late 20th century Anglo-American linguistics, 'universality' seems to be usually understood against the background of Boas', the Boasians', Bloomfield's, and the neo-Bloomfieldians' anti-rationalist injunction that grammarians should not suppress unique, particular features of 'exotic' languages they analyze, by uncritically imposing their pre-conceived ideas about what 'language'

should look like. Clearly, rationalists would object to my rendering of this injunction. Let us, then, scrutinize it.

First, Joos (1957:96), who is often cited in this context, used a much stronger phrase: 'languages could differ from each other without limit and in unpredictable ways'; the phrase obviously carried a 'rhetorical' purpose: e.g., the discursive construction of uniformitarian group-identity of, and the essentializing ascription of the intensional doctrine to, 'American linguistics' all the way from Boas up to and including the neo-Bloomfieldians, as opposed to 'European linguistics' (cf. the Labovian 'stereotype' and Lévi-Strauss's 'binary opposition'). Obviously, however, this view could not be ascribed at least to Boas (1911), who constructed a more or less delimited universal inventory of grammatical categories which could be variously encoded, morphologically or syntactically, and obligatorily or facultatively, in various languages, such variation in formal encoding being one of his primary objects of research (cf. Silverstein 1979; Koyama ms). Similarly, it is very difficult not to interpret Sapir (1921) and Whorf (1945) as explorations of the Boasian 'plan' to construct a universal, non-ethnocentric grammar on the basis of patterns of formal encoding of grammatical categories in world languages (cf. Koyama 1997b, 1999, 2000). As Hymes and Fought (1975:1077) put it, '[i]t is always dangerous to take one figure's comment as representative of a group'.

Notwithstanding, there is no way of denying the historical fact that Joos (1957) succeeded in inserting this phrase into his anthology of neo-Bloomfieldian papers, even though not only the Boasians, but also many neo-Bloomfieldians would have rejected his characterization (cf. Hymes and Fought *op. cit.*) Hence, I must qualify my statement by specifically noting that it is concerned more with what we might call 'latent doctrine' than with 'manifest doctrine', two terms borrowed from sociology to differentiate pragmatic, operational principles, or *modi operandi*, from explicitly articulated, ideational, or 'ideological' norms ('dogmas'), which are usually distinct from each other, as anyone familiar with the 'variationist' literature should be aware of (cf. also Silverstein 1979; Koyama 1997c). As is well known among historiographers of linguistics, the neo-Bloomfieldians, despite their general espousal of naïve inductivism as a manifest ideology, never purported to formulate a theoretical model which would accurately *describe* how scientific linguists constructed structural statements of languages. Nor did

they (with the exception of the Pike group) formulate a theoretical model which more or less explicitly articulated how scientific linguists *ought to* construct structural statements from available data (cf. Voegelin 1958:86, quoted above). Rather, they formulated, consciously or otherwise, theoretical models which explicitly formalized how scientific linguists ought to *reconstruct* the procedures by which they should have arrived at the (re)statements which they had constructed as (scientific) *bricolage* (cf. Lévi-Strauss 1967), and which intuitively looked satisfying and defensible, given the *bricoleurs'* explicit or implicit assumptions about 'language', 'languages', and 'linguistics as a science', inductivism being only one among these (cf. Hymes and Fought 1975:1078-88). With the possible exception of the reconstructive part¹², this is, *mutatis mutandis*, what all language users do, including the linguists themselves (of whom, to be sure, explicitness is more rigorously demanded), as argued by the Boasians, the Peirceans, and, in part, the Chomskyans, who nonetheless often fail to attribute this putatively *universal*, exceptionless capacity of 'hypothetical' or Peircean 'abductive' reasoning to the neo-Bloomfieldians. Certainly, this failure is rhetorically motivated; but more importantly, it must be also due to the Chomskyans' implicit, quasi-Russellian type-theoretic assumption that linguists themselves are not to be included among language users or cognitive subjects *qua* objects of their theoretical accounts¹³. It is easy to see that this assumption – a mere postulate which is justifiable only if one assumes that 'linguistics' is a 'natural science', which does not apply its theoretical tools to itself (see below) – forecloses the possibility of a 'critical linguistics'.

Note that our account of neo-Bloomfieldian linguistics in terms of its latent doctrines explains various historical facts that would appear puzzling in the Chomskyan account, which focuses on its manifest doctrines: compare, e.g., the fact that 'zero morphs', *deductively* obtained from *structural* considerations, so conspicuously figured in many allegedly empiricist analyses (cf. Hymes and Fought 1975:963); or the fact that those who hypothesized semantic, *intensional* 'meaning' as more than a mere heuristic device, e.g., Hockett, were not excluded from the group, a remarkable circumstance, given what happened to 'generative semanticists' later in the century; or the fact that generativism did not have much theoretical or technical difficulty incorporating 'immediate-constituent' ('IC') *structural*, formal-distributional analysis (at the

'surface structure'), and phonemic *structural* analysis (at the level of 'redundancy-free surface representation') from neo-Bloomfieldian thinking. Moreover, we can explain why the Chomskyans have failed to pay sufficient attention to the neo-Bloomfieldians' implicit, latent doctrines, given the former's rationalistic preoccupation with explicitness (cf. the often invoked Cartesian 'clear and distinct ideas'). Here is, then, at least one 'clear' instance of ideologically-motivated, ethnocentric (mis)recognition of important historical facts on the part of the Chomskyan rationalists. (See Hymes and Fought (1975), Silverstein (1986), and Murray (1989) for many, many more instances of the same.)

Returning now to my rendering of the anti-rationalist injunction, another likely objection would be that not only 'American structuralists', but also the Chomskyans would not oppose the injunction, which, so formulated, is a mere 'platitude'. But, as we have seen, unlike the Boasians, most Chomskyans seem to espouse a postulate that forecloses the linguistic-theoretical possibility of critical investigations into socio-historically conditioned, ethnocentric, non-universal prejudices of linguists themselves; accordingly, they are generally unfamiliar with even the fundamentals of comparative sociology, cultural anthropology, and historiography, witness Newmeyer (1986; cf. Murray 1989). Moreover, as we have seen above, they have no theoretical means, and perhaps no 'pre-theoretical' inclination, to control their rationalistic bias, even with regard to linguists who live in the 'same' culture without sharing their rationalist ideologies. The point is not whether they perfunctorily accept the truism, but whether they have *theoretically* integrated the truism into their linguistic theories and *pragmatically* implemented it, which they have not.

Thus, for instance, despite the fact that any beginning philosophy student knows, or ought to know, that empiricism and rationalism do not exhaust the entire field of epistemology, and much of post-Kantian philosophy has been concerned with formulating non-empiricist and non-rationalist 'critical theories' which systematically examine the socio-historical, pragmatic conditions of possibility, plausibility, and 'verity' of (symbolic) ideas and theories through which we perceive (at least partially) extensional, pragmatic phenomena (cf. Koyama 1997a, c, 2000) – despite this, I know of no 'Chomskyans' who have clearly differentiated empiricist and critical theories, perhaps because, from their agentive perspectives,

everything which is not rationalist looks, in essence, the same (again, see Lévi-Strauss (1967) on the 'primitive' logic of binary opposition, seemingly at work here).¹⁴ Their uncritical acceptance of rationalist, anti-empiricist dogmas has prevented the Chomskyans from distinguishing between empiricist theories, which see facts as mere givens, and critical theories, which see facts as socio-historically constructed; partially because of this, they have failed to note that 'linguistic data' for structural analysis are social facts constructed not only by native users, but also by linguists, and that, therefore, a scientific analysis of the pragmatic conditions of data construction is, as Voegelin (1958) contended, a prerequisite to any efforts at universal grammatical analysis. Thus, many universal claims of rationalist theories may turn out to be merely spurious universals, created by uncritically imposing, across the board and on a universal scale, the theorists' own ethnocentric (i.e., non-universal) ideas and symbolic formulae upon empirical phenomena, without investigating whether these ideas and formulae receive their plausibility from the particular, non-universal pragmatic contexts in which they are constructed along with their data (see below).

Having identified 'genuine' universality with non-ethnocentricity, I now turn to elaborate on 'ethnocentricity', a central notion in comparative sociology, anthropology, and historiography. Naturally, I shall start with explicating the notion in relation to these socio-historical disciplines and the idea of 'inter-disciplinary analysis', and then move on to contrast 'linguistic anthropology'¹⁵ with 'rationalist linguistics' with regard to this notion. First of all, in view of Newmeyer (1986), it must be explicitly stressed that cross-disciplinary and -theoretical consideration is itself a *theoretical* activity that comparatively deals with more than one disciplinary matrix or theory. In principle, such matrices and theories have different concepts and evaluative criteria, even if these concepts purport to describe extensionally more or less identical phenomena, and even if scientists in a disciplinary matrix which adopts some of the criteria might interpret them as universally applicable. Hence, when engaged in cross-disciplinary or -theoretical analysis, we must try our best to suspend our pre-conceived ideas about theoretical objects (e.g., 'language') and, more importantly, about the criteria with which we evaluate the merits and demerits of theoretical investigations (e.g., 'formalizability', 'simplicity', 'delimitation of scope'). These criteria are constitutive, indeed integral, parts of theories, and cross-

theoretical evaluations should not be conducted on the basis of a set of criteria which is internally constitutive of, and thus favors, certain theoretical orientations. This is *the* first principle of cross-disciplinary or -theoretical investigation, duly recognized in the disciplines which scientifically investigate epistemically distinct 'systems': 'theories', 'historical periods', and 'cultures'. The importance of explicitly recognizing this principle cannot be overemphasized, since it is in these 'cross-epistemic' domains that we are particularly exposed to the danger of unscientific 'ethnocentrism' and Whiggish ('presentist') historicization, in which an epistemically distal system is evaluated on the basis of the conceptual schemes of an epistemically proximal system, the cross-systemic applicability and epistemic superiority of the latter being simply assumed without any cross-epistemic justification (cf. Stocking 1982; Rorty, Schneewind and Skinner 1984; Koyama 2000).

Of course, it is *phenomenologically correct* for working scientists to assume that the criteria which they espouse are universally valid and should be applied to all other theories which investigate the theoretical objects they study. Yet, such a natural, pre-critical attitude of practicing scientists as overtly interested agents has been shown to be unscientific, indeed unreasonable, *from the sociocentric and critical perspectives* of systematic comparative studies of different epistemic systems. Such studies have established that theories must be evaluated in terms of (1) how successful they are in terms of their own criteria of evaluation (see above); (2) how justifiable these criteria are in view of our general and vague, but deeply entrenched understandings of human actions (see above), understandings which cannot be adequately formalized because of their transcendental character relative to the activity of formalization itself (cf. Putnam 1988:118 *et passim*, 1992); and (3) how critical our theories can be towards our understanding of human actions, the validity of which might be culture-restricted, despite our pre-critical perception of it as universally valid (cf. Geuss 1981; Calhoun 1995; Koyama 1997a). The first point is required in order to systematically avoid ethnocentric obstacles to more universal, cross-systemically valid epistemology; the second to avoid epistemological relativism, scepticism, and nihilism, not constrained by the first; and the third for theoretically guaranteeing self-reflexive criticisms which prevent the closure of an epistemological tradition as a whole, and which

pave the way for a further approximation to a universal, non-ethnocentric epistemology as a normative ideal (cf. Koyama 1997a).

Assuming that the notions of 'ethnocentricity' and 'universality' have been explicated sufficiently, however succinctly, we now try to bring them closer to the question of 'linguistic analysis', by describing the primary characteristics of the post-Boasian discipline of 'linguistic anthropology', which has 'non-ethnocentric universality' as one of its main concerns. As the disciplinary label indicates, one of the criteria by which 'linguistic anthropology' evaluates the success or failure of its own research is how consistently and coherently it can bring together 'linguistic' and 'cultural' investigations, or 'linguistics' and 'cultural anthropology', two disciplines that have distinct methodological orientations: i.e., an 'analytic' and generalizing *vs.* an 'hermeneutic' and individualizing one, as will be explicated shortly (cf. Boas 1887; Silverstein 1986; Stocking 1996; Koyama 1997c). This criterion, of course, is in itself internal to the discipline, just like the criteria of any other disciplines. What is distinctive about this criterion, however, is that it is not 'arbitrarily' posited as a 'convention', like 'simplicity',¹⁶ but directly motivated by desiderata at the cross-disciplinary level of general epistemology: i.e., (1) *unity* of the human sciences¹⁷ and (2) validation, or 'testing', of their claims to *universality*, which might obtain despite the *divergence* of particular human sciences, such as 'linguistics' and 'cultural anthropology', with their distinctly different theoretical perspectives, and the historical *contingency* of their disciplinary boundaries (cf. Andresen 1990; Ricken 1994; Gal and Irvine 1995).

Here, it may be helpful to recall that the distinction between 'linguistics' and 'cultural anthropology', or more generally, the 'analytic' and 'hermeneutic' sciences (*Natur-* and *Geisteswissenschaften*), is not naturally given, but emerged in a particular period of a particular culture, to wit, 19th century 'Central' and 'Western Europe', where Idealism, Romanticism and Positivism became the dominant ideologies (cf. Beiser 1987; Ringer 1990). Hence, the scientific, universal validity of this distinction must be rigorously investigated rather than simply assumed, lest we uncritically accept universal claims made by the particular sciences which operate within just one of these general epistemological matrices and thus (at least implicitly) assume this distinction for their autonomous status. Fortunately, ever since the academic institutionalization that assumes the validity of this demarcation started to take place in the

mid-19th century, it has been rigorously examined by epistemologists, most notably the neo-Kantians (cf. Ringer 1990), and more recently the neo-pragmatists (cf. Rorty 1991). One important feature which has emerged from these sustained inquiries is that the 'analytic' and 'hermeneutic' sciences *both* aspire to be 'universal'; yet, the former locate universality in their ability to formulate the regularities of events which can be shown to obtain independently of the particular contexts in which such regularities are tokened, while the latter locate universality in their ability to formulate the particularities of events which can be shown to obtain in spite of the particularities of the contexts of investigation, as distinctly different from the particularities of the investigated events (cf. Koyama 1997c, 2000). Clearly, the notion of 'universality', so central in our construal of 'science', is only partially interpreted by each approach, the one focusing on regularities and the other on particularities (i.e., 'singularities', or Peircean 'Sinsigns'; cf. Koyama 1997a).

This illustrates, I believe, the ethnocentric limitations of 'universal' claims made by sciences operating within just one of these epistemological matrices, and the need to achieve their synthesis, so as to raise the level of universality. At least, we should realize the problematic nature of the radical separation of the 'analytical' and 'hermeneutic' sciences in recent decades, and of the interpretation of this separation as 'progress' without any cross-disciplinary justification or even awareness. We should be deeply concerned about the use of such disciplinary boundary-setting terms as 'the linguistic turn' and 'the cognitive revolution' by some analytic philosophers, linguists, and cognitive psychologists (cf. Rorty 1967; Newmeyer 1986), and 'the hermeneutic turn' and 'the historical turn' by some Continental philosophers, cultural anthropologists, sociologists, historians, and literary critics (cf. Rabinow and Sullivan 1987). Both are blissfully unaware of the nihilistic nature of this kind of discourse: nihilistic, as the discursive 'logic' of 'revolution' calls for newer 'revolutionaries' who would wipe *them* out, and the process repeats itself in principle *ad infinitum*, without any inter-systemic, epistemological guarantee that the 'pre-' and 'post-revolutionary' systems of knowledge can be 'translated' into each other across the epistemic ruptures. This is not to deny the 'paradigm'-internal validity of 'progress', but we must see how invalid it is to generalize such progress beyond the disciplinary matrix relative to the criteria of which the progress is made and measured, and how desirable it is to

establish a cross-disciplinary matrix of epistemological inquiries, which transcends the disciplinary boundaries, in particular, the historically conditioned split between 'nomothetic' (or 'analytic') and 'ideographic' (or 'hermeneutic') sciences, as it has arisen from the general antagonism between the 'Enlightenment' and the 'Romantic' reaction to it.

Thus, we must construct a theoretical framework that subsumes the epistemological matrices of both 'analytic' and 'hermeneutic' sciences: one which can account for (1) *both* regularities and singularities without reducing the latter to mere tokens of the former (cf. Hacking 1990; Koyama 1997a), (2) *both* context-independency and -dependency, and (3) the interrelationship between *investigating* and *investigated* events. Here, linguistic anthropology and other 'semiotic' sciences have found invaluable the epistemological framework formulated by Peirce (1932), to whom we owe the distinctions between the 'symbolic' (context-independent) and the 'indexical' (context-dependent), between regularities and singularities ('Legisigns' and 'Sinsigns'), and, thus, between symbolic regularities (e.g., linguistic structure), indexical regularities (e.g., speech act types, indexical elements of shifters), and indexical singularities (e.g., individual speech acts) (cf. Coseriu's 'system' /'norm'/'parole'). Importantly, this framework was introduced into linguistics *and* anthropology by Jakobson (1957), who related the trichotomy, shifters in particular, to the notions of 'speech event' and 'narrated event', or, in more general terms, *signifying* and *signified* events (cf. Silverstein 1976a, 1992a, 1993; Koyama 1997a). Thus, *on the grounds of these intra- and cross-disciplinary epistemological principles*, linguistic anthropology, along with other 'genuinely universalist' programs, has adopted the semiotic framework as the matrix of linguistic *and* cultural inquiries.

Having illustrated the conceptual distinctions between the two kinds of universality, genuine and spurious, the former of which has, as its theoretical prerequisite, cross-disciplinary, -cultural and -historical comparisons, and between two kinds of linguistics, critical and pre-critical, only the former of which has the theoretical means to differentiate between genuine and spurious universals, we now move to suggest that there is empirical evidence to indicate that not only linguistic structure and language use, but also linguistic analysis itself interacts with the general cultural context in which it takes place, in such a way that cultural variables systematically affect

linguistic analyses of grammarians who have no theoretical means to control the cultural variables of which they are theoretically unaware. As noted, this may give empirical substance to the thesis of this paper. And, to reveal the ethnocentricity of rationalist linguistic 'universals', we shall use empirical findings of linguistic anthropology, which must be, as a rule, less ethnocentric than those of rationalist linguistics, insofar as the former has, theoretically and empirically, focused on the question of ethnocentricity, while the latter has not.

3. Ethnocentric limits of rationalist universals: Critique of linguistic reason

It is a social fact that linguistic phenomena are more individualized and specifically characterizable than are cultural phenomena in general. Unlike the dominant branch of modern linguistics, however, linguistic anthropology does not interpret this as indicating that problematics of 'culture' are inherently more vague than those of 'language', an essentialist interpretation that seems to be used to warrant the methodological autonomy of 'linguistics'. Instead, linguistic anthropology conceptualizes 'language' as an integral and methodically central part of 'culture', and thus investigates 'culture' by methodically focusing on 'language' (cf. Koyama 1997c).¹⁸ In other words, the discipline does not make a strong, implausible assumption that one can scientifically deal with language without rigorously theorizing 'culture', and thus without knowing how to systematically control (non-linguistic) cultural or historical variables which may interact with linguistic ones, given that there are such interactions aplenty.

Let us cite a few important cases. Some linguistic structural variables, such as forms which encode grammatical categories of 'shifters' (e.g., 'person', 'evidential'; cf. Jakobson 1957), require socio-cultural indexical categories such as '(addresser/addressee) role', 'sortal (qualitative) or mensural (quantitative) epistemic relations between the speech and narrated events' (e.g., evidentials and epistemic modals), etc., for their denotational descriptions, while other linguistic structural variables do not – or at least, not as clearly (cf. 'first person pronoun' *vs.* 'abstract noun'). The former, then, constitute a first point of systematic interrelation between linguistic-

structural and socio-cultural variables (cf. Silverstein 1976a). Now, to my knowledge, there have been several distinct approaches to this important social fact, and I shall contrastively deal with three of them. The first approach, called 'semiotic' (cf. Silverstein 1976b, 1987; Nichols 1984, 1992), has tried to show how cross-linguistically observable patterns of formal encoding of grammatical categories of NPs, case-relations, etc., might be related to the shifter *vs.* non-shifter (symbolic-indexical *vs.* symbolic) distinction, and what this might indicate about the interrelationship between the indexical and the symbolic, as will be explicated below.

The second approach, that of the 'autonomous formalists', has, generally speaking, taken up the problematic of indexicality only insofar as it directly pertains to constituent-structurally encoded regularities, *inter alia* those of 'reference maintenance mechanisms' (RMMs) that involve '(zero) anaphors', without developing a comprehensive perspective on RMMs as a whole, inasmuch as these consist of morphological, syntactic, semantic, *and* pragmatic mechanisms of co-reference. As has been conclusively shown by linguists working in the Praguean 'Functional Sentence Perspective' tradition (cf. Firbas 1964; Daneš 1970; Halliday and Hasan 1976; Silverstein 1976b, 1987; Koyama 1999), RMMs are primarily discursive, pragmatic phenomena that operate across sentence-boundaries; only a small portion of them is encoded in formal constituency. Deliberately depriving themselves of such a comprehensive perspective, autonomous formalists do not have the theoretical tools to control the variables which are known to interact with syntactic-structurally encoded regularities of RMMs: (1) not *syntactic*-structurally, but morphologically encoded RMMs: e.g., noun classes¹⁹ and many topic-marking devices (cf. Heath 1975; Foley and Van Valin 1984; Koyama 1997b, 1999), (2) inter-clausal linkages (cf. Bolinger 1979:298-305; Silverstein *op. cit.*; Koyama *op. cit.*), (3) cultural-stereotypical semantic categories (see note 22 below; cf. Bolinger 1960:377; Putnam 1975; Halliday and Hasan 1976; Silverstein 1987), (4) pragmatic elements, such as 'presupposed contexts', and 'perspectives' from which referents are described (cf. Bolinger 1979:307-8). Hence, when autonomous formalists try to abstract their data for properly structural analysis, i.e., sentences as symbolic types, from the token-sentences which pragmatically occur in their introspective acts, they typically fail to control both those structural elements which do not belong to what they purport to

analyze (1 & 2 above), and non-structural and/or pragmatic elements (3 & 4 above). Consequently, they often construct obviously invalid structural analyses of syntactic-structurally encoded RMMs, as demonstrated by Bolinger (1979) *vis-à-vis* what would emerge as the 'Government and Binding' program.²⁰ This is, then, one instance where pragmatic and other non-syntactic variables of RMMs systematically affect grammatical analyses of those who do not know how to control them. And note that the pragmatic, socio-historical context of scientific specialization and autonomization, of which 'formalism' in linguistics is a mere symptom, must be at least partially responsible for this blatant neglect of one of the most basic prerequisites of any scientific treatment: i.e., the controlling of interacting variables.

Let us now turn to the third approach, that of 'universalist pragmatians' (cf. Levinson 1988), which has analyzed indexical characteristics of (first and second) person shifters by using a method of componential analysis which is known to have serious problems even at the level of 'lexical semantics', let alone *pragmatics* (cf. Bolinger 1965). Briefly, Levinson (1988) purported to develop Goffman's (1979) subcategorization of the 'speaker'-role into 'animator', 'principal', 'figure', 'author', and of the 'addressee'-role into '(un)addressed recipient', 'overhearer', 'eavesdropper', and constructed an expanded, yet delimited and allegedly universal set of such 'participant roles' (PRs), 17 in number, which are differentiated by specified combinations of 7 'distinctive features' such as 'Motive', 'Form', 'Recipient', 'Channel-link', etc.²¹ In so doing, he failed to note a subtle, yet crucial distinction between (1) the 'speaker' and 'addressee' roles and (2) the subcategorial PRs. While (1) the former are *indexical* categories transparently encoded in, and obligatorily indexed by, *symbolic-indexical* grammatical categories of 'person' (cf. Jakobson 1957; Benveniste 1966), which are marked across languages in various nominal or verbal-affixal *symbolic* ('arbitrary') forms, and thus constitute a central part of 'universal grammar' that can be used to describe how various *symbolic* structures may be mapped onto the 'same' (universal) *indexical* range (cf. Silverstein 1976b, 1987; Koyama 1997b, 1999), (2) the latter are no more than indexical categories which *discursive incumbents* (i.e., *referents*) of the 'speaker-' or 'addressee-role' can be said to inhabit (i.e., they are *not* grammatical-categorially encoded; see below). To be sure, one might claim, with Levinson (1988), that even subcategorial PRs (e.g., 'animator') are

sometimes formally 'encoded' (e.g., 'the evidential form *X* is used when the speaker (animator) is not committed to *p* (non-principal)'; see Levinson (1988:181-92) for details); however, this kind of descriptions of lexical forms is not linguistic-structural, but cultural-stereotypical.²² That is, the subcategorical PRs appear only in *cultural stereotypical, non-structural, and essentializing* descriptions of typical uses of these lexical forms. In *structural* descriptions, the lexical forms in question encode *grammatical categories* of evidentiality that correspond to *non-PR* indexical categories: i.e., sortal epistemic relations between the speech and narrated events, such as (a) 'absolute' or 'relative' categorial identity, or *iconicity, qua* linguistic event between the speech and narrated events ('quotative', 'reportive'), (b) sensorial-categorial *indexical* nexus between the two events ('hearsay', '(in)visibility', etc.), and (c) 'absolute' categorial mismatch between the *indexical* universe of the speech event and the *symbolic* realm of myths, laws, ideals, or other-worldly events, where the narrated event is said to belong (grammatically categorized as 'nomic'; cf. Jakobson 1957; Silverstein 1993; see below for a theoretical justification of this structural analysis). Language users recognize uses of these grammatical categories from their agentive, essentializing perspectives, and try to capture them with their cultural-stereotypical descriptions. For example, the use of 'hearsay', which structurally signals a linguistically mediated nexus between the speech and narrated events, is typically re-interpreted by users – and even by linguists (e.g., Levinson 1988:184-6) – as signaling the speaker's non-committal agentive stance to *p*.

Note here how the *social relation* between the two events is essentialized as the *intentional state of the individual agent* (e.g., 'personal commitment'). It cannot be by chance, I believe, that an ethnoscience of our *individualistic* culture – namely, our 'Standard Average Anglo-American Pragmatics' (see Levinson 1983, 1988; Brown and Levinson 1987; cf. Koyama 1997a, c) – has failed to notice this process of essentialization, and indeed, tried to construct theories of 'evidentials' and 'speech acts' *on the basis of* such essentialization. That is, I conjecture, the ethnoscience does not see the process because the discipline itself is embedded in, and saturated by, a culture where psychological individualism has become naturalized, legally institutionalized, and implicitly and even 'unconsciously' presupposable. The process works at the level of the cultural unconscious, and remains covert, or 'hidden' from the ethno-

scientists' explicit, theoretical consciousness, which 'scientifically' rationalizes ('reifies') the overt, epiphenomenal product of essentialization, and naturalizes ('universalizes') it in accordance with the cultural scheme of psychological individualism (cf. Boas 1911; Sapir 1921; Sahlin 1976; Silverstein 1979, 1986; Stocking 1982, 1996; see above).

But, of course, this conjecture assumes that Levinson (1988: 184-6), in particular, did embrace this essentialization, a hypothesis which assumes, in its turn, that Levinson's (1988) descriptions of evidential forms in terms of subcategorical PRs are not structural, but cultural-stereotypical. We must, therefore, seek structural evidence for this. Here, we may note that, unlike the speaker- and addressee-roles (see below), (1) only some of subcategorical PRs could be said to be 'encoded' in structural forms; (2) further, only some languages 'encode' them, and often merely optionally at that; and (3) there appears no cross-linguistic 'significant generalization' as to which of them may be so 'encoded', if we mean by this term something which is not a 'pure' artifact of the way the categories are set up, and which is categorically or probabilistically expressible in implicational terms, such as a 'hierarchy of features' (cf. Levinson 1988:172-3; see 'the NP hierarchy', explicated below). These facts indicate that subcategorical PRs are indexical categories which, like *verba dicendi*, happen to be (cultural-stereotypically) 'encoded' in *lexical* forms of particular languages which are used in cultures where such indexical categories are saliently recognized, while the speaker/addressee-roles are indexical categories which are, like moods, encoded in *grammatical categories* that are a *structurally* central part of universal grammar (see below).

There is not only structural, but also pragmatic evidence against Levinson (1988). Note that, unlike symbolic or symbolic-indexical categories, indexical categories can be generated, as ethnomethodologists say, in an *ad hoc* manner in micro-social discursive interactions. For example, one can perform a highly contextually specified action, and in a later phase of the 'same' discourse, point to that action as the prototype of what one is doing *hic et nunc*, thereby generating a highly contextually specified indexical type. Thus, in her critical discussion of Levinson (1988), Irvine (1996:134) came up with a variety of PRs which cannot be sufficiently characterized by Levinson's (1988) 'universal' set, e.g., 'the person quoted against his or her will'; 'the role in a stage play, as opposed to the actor playing it';

'the person a child is named after, who may (if living) then have certain specified responsibilities towards the child', etc., noting that such indexical categories can be multiplied, *in principle, ad infinitum*. This shows, once again, that subcategorical PRs are 'mere', or 'genuine', indexical categories that must be clearly distinguished from the PRs which are indexical categories encoded grammatically and universally (see below).

Further, let us note that Irvine (1996) was able to find the essential inadequacy in Levinson's (1988) methodology, most probably because her fieldwork had taught her about the highly contingent character of micro-sociological indexical categories that people create in actual ethnographic contexts. In contrast, Levinson (1988) seems to have failed to understand the 'genuinely' pragmatic, indexical character of what he purported to analyze, uncritically imposing his habitual 'logico-analytical' method of componential analysis upon a domain that should not be analyzed non-contextually, and constructing a spurious universal (see Brown and Levinson 1987; Levinson 1983; cf. Geertz 1973; Koyama 1997a). In other words, Levinson (1988) confused the indexical with the symbolic, and methodically assimilated the former to the latter, while Irvine (1996) did not. Here, we might be justified to attribute this contrast partially to these authors' differing theoretical understanding of 'linguistic data' as 'input for analytic generalizations' *vs.* 'products of discursive processes unfolding in specific cultural contexts', two construals which are, in our professional cultures, institutionalized in 'universalist' and 'interpretive' linguistics, respectively.²³ If this is the case, then we have an instance where cultural variables systematically affect the linguistic analysis of a *pragmatician* who has not paid sufficient attention either to pragmatic contexts of language, or to his own socio-historical contexts (cf. Levinson's (1983:371) interpretation of scientific specialization as epistemological evolution).

Pursuing the issue of indexicality-symbolicity interaction, we note that, as is widely recognized in the 'functionalist' and 'linguistic anthropological' literature since Silverstein (1976b), (1) the pragmatically motivated distinction between nominal shifters and non-shifters is merely one division, albeit an important one, among an ordered array of fine *grammatical categorial* differentiations of NPs on the basis of the degrees of epistemic presupposability of their various denotata in the speech event; and (2) this grammatical

categorial ordering of NPs is systematically interrelated with so central a linguistic structural variable as case-marking (nominative-accusative *vs.* ergative-absolutive systems, to be precise). Thus, *ceteris paribus*, the nominative-accusative case-marking *universally* 'starts' from the NPs whose denotata are most presupposable in the speech event (e.g., 'personal pronouns'), while the ergative-absolutive case-marking starts from the NPs whose denotata are least presupposable (e.g., 'abstract nouns'); particular structures may select certain NP grammatical categories that fall in between, such as 'proper name' and 'animate term' – serially arranged on the basis of the degrees of epistemic presupposability of their denotata in the speech event – , as 'endpoints' of such case-marking systems (see Silverstein (1976b, 1987), Dixon (1979), Koyama (1999) for more exact statements). Note that this kind of universal is independent of regional and genetic variables (cf. Nichols 1992), and that it is much less theory-dependent than those universals which cannot be stated without using a host of theoretically (over)specified technical notions and formalisms. Clearly, then, the morphosyntactic regularity of case-marking cannot be adequately and sufficiently captured or explained without a systematic description of the socio-cultural variable of the epistemic presupposability of denotata, which are differentially interrelated with various NP types (which, needless to say, function as semantic arguments having case-relations with predicates). Notwithstanding, most 'formalists' do not seem aware of this cultural prerequisite to grammatical analysis, perhaps because they misinterpret the irreducibility of linguistic-structural characteristics to contextual ones as somehow guaranteeing the methodological autonomy of a purely formal-distributional analysis, an interpretive leap of faith the plausibility and naturalness of which seem to owe much to the historical context of scientific specialization.

Second, note that it is the structural correlates of the 'speaker-' and 'addressee-roles' – not the subcategorical PRs such as 'animator' and 'principal' – that are central elements of the NP grammatical-categorial hierarchy, the 'reality' of which we must recognize, as (1) cross-linguistically attested patterns of case-marking would remain inexplicable without it, and (2) it is based on a unitary pragmatic principle – the presupposability of denotata in the speech event – which is logically independent of case-marking. In contrast, there are no structural, grammatical-categorial (as opposed to 'lexical' and cultural-stereotypical) correlates of the subcategorical PRs. Along with

the fact that language users can create subcategorical PR *types* (not just tokens), *in principle*, infinitely (see above), this strongly indicates that subcategorical PRs are 'genuinely' indexical categories, while the 'speaker-' and 'addressee-roles' are indexical categories that are symbolically encoded across languages as grammatical categories.

Further, recall that my account of evidential grammatical categories, unlike Levinson's (1988) lexically-based account, is based on the three primary types of relations between the speech and narrated events, namely, (a) 'iconic' or 'reflexive' (e.g., 'quotative' utterances, where the speech event and the narrated event (i.e., the quoted/reported speech event) count as 'the same'), (b) 'indexical' (based on a 'sensorial nexus', such as 'hearsay'), and (c) 'symbolic' ('nomic'; see above). The *theoretical* cogency of this account should be clearer now, as we can see that the 'semiotic' account of NP grammatical categories, which has been strongly confirmed by structural evidence, is based on the degree of indexical presupposability of denotata in the speech event, from the maximum ('iconically indexical', or 'reflexive'; cf. Récanati 1979), through the medium ('indexical nexus'), to the minimum ('symbolic', or 'least indexical') degree. That is, my account can be seen as based on the three primary types of relations between the speech event and the universe of denotata (i.e., the narrated event), (a) 'reflexive' or 'self-referential' (e.g., personal pronouns, which are felicitously used when the speech event and the narrated event are 'the same', i.e., coincide, insofar as the denotata of personal pronouns are concerned), (b) 'indexical nexus' (e.g., proper names, as in 'the causal theory of reference'; cf. Kripke 1972; Putnam 1975), and (c) 'symbolic' (e.g., abstract nouns and 'dummy NPs'; cf. Silverstein 1987, 1993). In other words, my 'semiotic' account of evidential grammatical categories is 'formally' analogous with the structurally-confirmed account of NP grammatical categories, even though the integration of evidential grammatical categories into the central components of structure is yet to be fully investigated (but cf. Koyama (1999), which presents a universal-grammatical analysis of Japanese in which it is shown that the quotative/nomic predicational distinction is systematically interrelated with the structurally central variables of (1) predicate types and (2) the degrees of tightness of interclausal linkage (parataxis/hypotaxis), and more indirectly, (3) reference entailment and maintenance, and (4) case-relations).

Thus, unless one can provide structural evidence indicating (a) 'positively', that the subcategorical PRs are *structurally* encoded just like the 'speaker-' and 'addressee-roles', or (b) 'negatively', that my 'semiotic' account of evidentials is untenable, all available evidence and (meta)theoretical considerations indicate that (1) Levinson's (1988:184-6) descriptions of evidentials in terms of subcategorical PRs are not structural, but cultural-stereotypical; thus, Levinson (1988) (2a) most probably committed the error of cultural-stereotypical essentialization, re-interpreting the social relations between the speech and narrated events structurally signaled by evidentials, as the intentional states of the individual agents (e.g., 'personal commitment'), and (2b) presented an account of evidentials on the basis of such essentialization, of which he was (at least theoretically) unaware; and (3) if this is the case, a sociological reason for his consistent 'misrecognition' of the linguistic phenomena may be found in the cultural context of his linguistic analysis, namely, the institutionally entrenched psychology of individualist essentialization in the modern culture, *inter alia*, our 'Standard Average Anglo-American Culture'.

4. 'Closing Statements'; or 'Linguistics and Poetics'²⁴

In this paper, I have tried to articulate a (meta)theoretical position from which it can be seen that (1) the linguistic rationalists' construal of 'universal' itself might be historically bound and ethnocentric (hence, not 'genuinely' universal), and moreover, (2) some major problems in the linguistic analyses they have produced may be transparently reducible to the socio-historical condition of scientific specialization, the modern epistemological division of labor, and the cultural ideology of psychological individualism. Hopefully, I have given some support to Voegelin's observation that the minimum of (universal) linguistics is a consideration of structure *and* pragmatics, and of 'structuring' *and* its pragmatic context, in their synchronic *and* historical dimensions. Some day, I hope, we will all come to realize that 'pure autonomy', structural or disciplinary, is a spurious and pre-critical illusion of an atomistic age – as Roman Jakobson, Jurij Tynjanov, Petr Bogatyrev, and their 'wasted' poets, had tried to teach a generation of 'linguists', before it squandered its own. And

now, as the poets said, it matters little whether 'I or he killed them'. We all did, and with the poet, we repeat 'Nevermore' (see note 24).

3-323-104 Ryôgae-machi, Fushimi Ward
Kyôto, 612-8082, Japan

Notes

1. I shall explain why such specification, e.g., in terms of 'formal explicitness', is not desirable for *meta-theoretical*, general evaluative criteria.
2. Note that one of original attractions of Chomskyan linguistics to *both* older and younger formalists, i.e., neo-Bloomfieldians and generativists, was its promise to give a final solution, by means of an explicitly formalized 'evaluation procedure' formulated on the criterial notion of 'simplicity', to the long-standing problem of 'non-uniqueness of structural analysis' (Chao 1934), exacerbated by the emergence of the age of 'structural restatements' (Voegelin and Voegelin 1963) around 1940 (cf. Hymes and Fought 1975). This is important when we evaluate the theoretical significance of the historical fact that, after several decades of intensive research, this promise is yet to be delivered and, to a significant extent, it has been forgotten.
3. This scientific discourse genre acquired its currency among neo-Bloomfieldians, and is still extremely fashionable among generativists and others. This indicates, again, that a 'paradigm shift' occurred between Boasians and neo-Bloomfieldians, *not* between the latter and generativists (cf. Hymes and Fought 1975:1025-6, 1028; Silverstein 1986; Koyama 1997c, 2000).
4. Cf. the 'coherence' *vs.* 'correspondence' theory of truth in the epistemological literature (cf. Haack 1993; Koyama 1997a).
5. Should this statement appear unwarranted, one might consider, e.g., a morphological theory which does not specify how morphology is interrelated with phonology and syntax, or a 'free-floating' syntactic theory not specifying how syntax is interrelated with semantics and pragmatics (cf. Koyama 1997a, b, c, 1999; see below).
6. The fact that these disciplines are themselves part of those socio-historical conditions which they study should not mislead us to confuse this methodological self-reflexivity, painstakingly articulated by Max Weber, with Russell's semantic paradox or Apel's 'performative contradictions'. Clearly, unless one assumes the reductionistic social determination of ideas, ideologies, theories or rationality *à la* vulgar Marxism and other

- sociocentric cognitive theories, there is a theoretical possibility of such self-reflexive critical sciences. This possibility, in fact, is guaranteed if we have critical rationality transcending the bounds of empirical and conceptual realms (cf. Koyama 1997a, 2000).
7. I use this technical term of post-Durkheimian sociology without committing myself to the functionalist empiricism of British social anthropology, and indeed try to show that it is not necessary to interpret this notion as pre-critically assuming our epistemological access to 'positive' facts which exist wholly independent of our historically situated 'scientific' activities. Also note that the term may refer to (1) *macro*-social facts, which obtain independently of social individuals' agentive awareness of them, *or* (2) *micro*-social facts, which are more closely integrated with agents' models of their own actions. A general idea of this standard sociological distinction may be obtained from the comparison of the Labovian *macro*-social analysis of social stratification with the Gumperzian *micro*-social analysis of metaphorical code-switching or the Gricean *agentive* theory of 'speech acts' (cf. Silverstein 1992a; Koyama 1997a).
 8. In post-Weberian sociology, this is identified as a kind of 'instrumental rationality', a goal-oriented (purposive), strategic capacity, which is *not* the only kind of rationality, as elaborated below.
 9. More specifically, the 'eclipse of totality' includes disciplinary ethnocentrism (cf. 'idols of the cave'), incoherence among axioms and findings of specialized sciences, nihilistic relativism about 'reality' separately investigated by highly specialized and 'incommensurable' sciences, and so on (cf. Koyama 2000).
 10. To see how common such essentialization is, recall 'trait psychology' in Anglo-America and 'psychology of expression' in Germany, where contextual relations involving 'subjects' are essentialized as persisting personality characteristics of the individual subjects themselves (cf. Danziger 1990).
 11. The 'modern culture' includes any context to which the epistemic and methodological apparatuses of the modern culture have been uncritically applied, whether it is inside or outside the societies in which this cultural order dominates (cf. a certain type of 'cognitive linguistics').
 12. But cf. 'folk etymology' and other phenomena of native 'linguistic rationalization' (cf. Silverstein 1979).
 13. One might wonder how this assumption can be consistently maintained, given the Chomskyan identification of (first-order) 'natural language' with (second-order) 'linguistic theory'. The whole program might be inconsistent.

14. Note what Chomsky says of this work:

[Lévi-Strauss (1967)] is a serious and thoughtful attempt to come to grips with this problem [i.e., structure of language-like systems, e.g., kinship, *à la* Pike]. Nevertheless, I do not see what conclusions can be reached from a study of his materials beyond the fact that the savage mind attempts to impose some organization on the physical world – that humans *classify*, if they perform any mental acts at all. Specifically, Lévi-Strauss' well-known critique of totemism seems to reduce to little more than this conclusion. (Chomsky 1972: 74; my emphasis and brackets)

The two paragraphs which immediately follow the quoted sentences (pp. 74-75) clearly show that Chomsky (1972) has 'read' Lévi-Strauss (1967) only in terms of his own, pre-conceived totemic scheme of binary opposition, opposing 'mere classification' (them) *vs.* 'infinite generative capacity' (us); everything which did not fit his *a priori* conception simply escaped his rationalistic interpretation: e.g., ironically, essentialization and the critique of the 'primitive logic' of binary opposition. This might be, then, yet another instance of rationalist ethnocentrism, and we cannot help but discern a *consistent pattern* of ideologically-motivated *apperceptions* of extensional phenomena given a free rein by pre-critical rationalism (cf. Koyama 2000).

15. Since the term "linguistic anthropology" is associated with various 'cultural stereotypes' and 'prototypical exemplars' (cf. Putnam 1975), I must explicitly note that my usage is restrictive, as it utilizes not only the criterion of 'topic' (i.e., 'language and culture'), but also those of 'methodology' and 'theorizing', as elaborated below. As prototypical exemplars, it may include (1) Boas and his students, especially (2) Sapir, and his students ('the First Yale School'), as well as (3) the Prague and Indiana Schools of semiotics, and others critical of *both* positivist ('analytical', 'objectivist', 'ahistorical', 'universalist') *and* anti-positivist ('hermeneutic', 'constructionist', 'historicist', 'particularist') approaches to 'language and culture' (see below; cf. Boas 1887; Silverstein 1986; Stocking 1996; Koyama 1997a, c, 2000).
16. 'Simplicity' (cf. 'Ockham's razor') is usually taken to be a conventional maxim by philosophers of sciences who are too 'mature' to be unaware of the danger of rationalist Platonism, or naturalization and reification of symbolic forms and ideas.
17. Note that the ideal of 'unified science' is detachable from various doctrines of logical positivism that have been discredited, such as (1) 'scientism', which holds that the human sciences require no methods other than those of

the natural sciences (see below) and (2) 'evolutionary positivism', which holds that the sciences which we now have are those which have survived 'epistemic struggles', and which are *ipso facto* better than the sciences of yore (see above).

18. To use the Praguean terminology, 'language' is theorized as the *plus*-marked sub-category privatively opposed to the *zero*-marked super-category of 'culture'. Note that, as can be seen in the *he-or-she* controversy, language users typically re-interpret the *semantically privative* opposition in terms of its implicatural, *pragmatic* implementation, thus obtaining an effectively *equipollent* opposition. Thus, we have a linguistic-theoretic account partially explaining why some language users have essentialized the 'language'-culture' opposition as semantically, theoretically equipollent, and accordingly proceeded to construct 'non-cultural' linguistic theories since around 1940 in America north of Mexico.
19. Heath (1975) has shown that, very roughly, if a language has an elaborate system of noun class distinctions, it does not have an 'elaborate' (strict, extensive) system of syntactic-structural marking of co-reference (cf. 'Equi NP Deletion'), and conversely, since they serve the same *pragmatic* function of reference maintenance (cf. 'functional unity').
20. For reasons of space restrictions, let me cite just one illustrative case: Bolinger (1979:290) pointed out that Lasnik wrongly attributed the 'ungrammaticality' of **It surprises him_i that John_i is so well liked* to the command relationship, though the sentence is, in truth, just pragmatically 'abnormal': it suggests that the 'speaker' can read John's mind (cf. the 'normalcy' of *It surprised him_i that John_i was so well liked; It obviously surprises him_i that John_i is so well liked; It surprises me that I am so well liked; etc.*) See Kuno (1987), Kuno and Takami (1993), and Koyama (1999), not to mention the whole literature of 'Generative Semantics' (cf. McCawley 1979:217-22, 234-46), for a plethora of examples of this sort.
21. For instance, one such 'PR', 'author' (e.g., 'ordinary speaker'), is defined as [+Participant], [+Transmitter], [+Motive], and [+Form], and differentiated from 'spokesman' (e.g., 'barrister[-at-law]'), defined as [+P, +T, -M, +F]; 'deviser' (e.g., 'statement maker'), defined as [+P, -T, +M, +F]; etc. (Levinson 1988:172). Since our discussion is primarily concerned with his methodology and theoretical orientation, let us simply note the apparent impression of suspiciously *ad hoc* and contingent quality, exhibited by those allegedly 'universal' categories: 'spokesman', 'deviser', 'relayer', 'sponsor', 'ghostor', 'ghostee', 'ultimate source', 'formulator' (Levinson 1988: 172-3).
22. An example of the distinction between (1) cultural-stereotypically and (2) linguistic-structurally semantic descriptions of lexical forms is: 'water' = (1)

'H₂O', 'liquid', 'transparent', or other properties attributed to the entities to which speakers typically refer when using the term *water*, vs. (2) properties such as 'substantive', 'non-human', etc., which are structurally encoded as 'nominal', 'neuter', etc. (cf. Putnam 1975; Koyama 1999).

23. Levinson (1988:192) characteristically argues that the identification of categories is methodologically prior to the description of the discursive processes in which they are 'used'. This position appears cogent only because we tend to interpret 'used' as 'presupposed', not 'created'. That is, if categories are not only presupposed, but also created in discursive processes, they cannot be identified without describing the discursive processes in which they are created (cf. the later Wittgenstein; Koyama 1997a).
24. The title of this section, which is named after Jakobson's (1960) famous article, may appear (con)textually incoherent, and certainly too ambitious. Notwithstanding, I find its use (or 'mention') in this (con)text fairly appropriate, since (1) it clearly signals that the spirit of the entire article is thoroughly Jakobsonian (cf. 'interdependence' of the human sciences; *langage* as 'a system of systems'; the motto '*Linguista sum; linguistici nihil a me alienum puto*'; structural and historical 'relativity' of human concepts and actions; the synthesis of the universal and the particular; Jakobson (1973, 1990)), and (2) the reader will find me giving myself a 'poetic license' at the end of this section, where various Jakobsonian sub-texts are implicitly referred to: cf. especially Jakobson (1987 [1928]:48) for the statement, co-authored with Tynjanov, 'Pure synchrony now proves to be an illusion'; Jakobson (1987 [1931]:273-300) for 'a revolutionary generation that squandered its poets' (such as Gumilev, Blok, Xlebnikov, Esenin, and most importantly, Majakovskij, and perhaps, the Futurist Aljagrov, i.e., Jakobson's poetic *alter ego*), as well as the quoted epigraph 'Killed; – / Little matter / Whether I or he / Killed them'; and Jakobson (1987 [1964]:50-61) for Poe's *Raven*, with its fateful, 'horrifyingly regular', quotative repetitions of 'Nevermore'.

Lest my point in the final passage (as well as in the entire article) be missed by the reader, let me state it explicitly: The dictum '*Das Wahre ist das Ganze*' contains an important truth, as Hegel and, following him, Benveniste insisted (cf. Jakobson 1973:15); yet, in the technically specialized, epistemically myopic, and psychologically individualistic culture that is ours (see above), 'the whole', or genuine universality, is not – and perhaps, cannot be – realized (cf. Jay 1984:241-75). At best, it can be merely pointed at by 'poets' as a counterfactual ideal, against the background of which they cannot but see the spuriousness of those '[s]pecialists without spirit, sensualists without heart' who imagine and proclaim that they have 'attained

a level of civilization never before achieved' (Weber 1992 [1904-5]:182; the phrase was inspired by Nietzsche; cf. Sapir 1924; see above). And moreover, these poets are lavishly squandered, generation after generation, by our age; ours is an age of *byt*, 'a stagnating slime, which stifles life in its tight, hard mold' (Jakobson 1987:277), or, in more sociological terms, of lethargic inertia operating in the iron cage of instrumental rationalization, specialization, and 'formalization' (in the Weberian sense; see above). This is, I believe, the sad truth of our age, squarely faced and tragically lived not only by the Russian poets, but also by Nietzsche, Weber, Adorno, Sapir, Radin, Whorf, Stanley Newman, Charles Voegelin, and other 'poets' who have come to see how spurious our culture, especially our professional culture, is (cf. Weber 1989 [1919]; Sapir 1924; Silverstein 1986; Koyama 1997a, b, c, 2000; see above). Thus, as a macro-sociologist, I should not, and cannot, underestimate the seemingly inexorable force of these social processes in our culture; yet, as a social agent, I am convinced that we must at least seriously try to change it, and make it genuine. And that is why I have written this article, hoping for your cooperation, heartily, in the linguistic and poetic spirit of Jakobson.

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