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FORMAL RULES, COGNITIVE REPRESENTATIONS, AND LEARNING IN LANGUAGE AND OTHER
CULTURAL SYSTEMS

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ABSTRACT

This article focuses on the relationship between a formal description of some cognitively driven behavioral regularity (such as language/speech or culture/action) and the cognitive basis of that regular behavior--as seen in Chomskyeen grammar, Lounsburean kinship terminology analysis, and circles in Euclidian analytic geometry. It considers the nature of people's production of the regularity, including their use of "rule of thumb", their learning of underlying regularities, and the increasing abstraction of their knowledge. In this context the role of formal descriptions, including both their attractions and their limitations is discussed--as is the repeated attempts by anthropologists to copy or adapt linguistic formalisms.

KEYWORDS: Chomsky, A. N.; Grammar; Kinshipo terminologies; Lounsbury, F. G.

MAIN TEXT

The central issue of this paper concerns the relationship that exists--or, more particularly, that need not exist--between a successful formal description of behavioral regularities (e.g., in speech/language, culture, or other behavioral/cognitive activities) and the cognitive processes that produce the regularities.

The issue is alive today, and important to various varieties of contemporary linguistics. But I will be anchoring my discussion in older formulations of Chomsky's. This is not only because he is the figure most strongly associated with one view of this relationship, but because, in articulating his view, he has managed to define the subsequent field of debate (the nature of contrasting possibilities or alternatives)--and managed to define it in a way that entails a fundamentally wrong presupposition. Chomsky's claim is that the success of his formal analysis of grammatical relations necessarily entails that something very much like the specific analytic machinery of his theory must exist in the minds of the speakers who produce the grammatical regularities in question. Since this analytic machinery is very complex and appears to be very language-specific--while describing behavior that all children learn fairly quickly and easily--Chomsky infers that the major pieces of this specific machinery must already be in the learners' minds at birth. Advocates of comparable formal analyses in linguistics or in other areas of behavior (e.g., within anthropology, in aspects of kinship or ethnotaxonomy) get tempted to make similar claims, while, on the other hand, those who do not accept the view of mind or psychology presented by Chomsky often feel obligated to reject the very idea of formal analytic descriptions of whichever behavior is in question. That is, Chomsky's way of dichotomizing the possibilities is presumed not just by those who share some aspect of his views but also by those who disagree with any basic aspect of his claims.

It is this dichotomization itself that I want to query. In my discussion I will be assuming, for purposes of discussion, the descriptive adequacy of Chomsky's grammatical accounts; this assumption does not mean that I

necessarily actually accept his accounts (or, even, that I am unaware of the variation that exists among his various accounts). I do not want to argue the truth value of his accounts, but only to examine the implications his accounts would have, if true, regarding cognition, mind, and innate mental structures. In the course of this discussion I will offer some suggestions regarding the kinds of cognitive processes which I think produce the behaviors in question, though these suggestions will not constitute any formal account or theory.

I will first lay out three examples of formal descriptions of regularities produced by human behavior. I will then discuss the relevance of each formal description to the cognitive processes which produced the behavior which, in turn, produced the regularities. I will conclude with a consideration of some of the larger issues which these examples highlight.

Chomsky, starting with *Syntactic Structures* (1957), has developed a powerful family of formal descriptions of aspects of English syntax--descriptions whose formal apparatus seems, to varying degrees, to apply to other, perhaps all other, natural human languages as well. Chomsky (e.g., in Language and Mind [1968] and Reflections on Language [1975]) has asserted that the complexities of human language are too structured and too complex to be learnable from scratch in the time frame within which human children learn language--and at the level of cognitive development which these children are at when they enter the process. He infers, then, that the basic machinery of natural language must be already innately present in children's minds as they begin the learning process. In some general sense such has to be true tautologically--that is, whatever machinery is in fact needed must perforce be present, since we all learn the languages that we speak, and in contrast to other apparently "smart" animals, we learn it automatically, without much special effort or prodding. The issue, thus, is not whether or not there is any genetic component at all to human language (I certainly assume that there has to be some) but instead has to do with the nature of such a component. The problem is that Chomsky, more specifically, defines the structure and complexity that must be present as the analytic machinery (specific categories and operations) of Generative/Transformational Grammar; this is the part I wish to argue with.

I want, for the moment at least, to posit that Chomsky's syntactic descriptions do accurately and powerfully capture important basic structures of English, and that the analytic apparatus serves equally well for other languages. I am aware that there is much debate on these very issues; I myself pretty much, with caveats, agree about English, but have no good basis for assessing the wider claim. Later in this paper I will discuss an analytic perspective--the point of this paper--which might reconcile the claims of many who take issue with Chomsky's formalism and syntactic descriptions with my claims. However, for the moment I am not concerned with the actual truth itself of Chomsky's claims, but with consequences which would and would not follow if they be true--which is why I only "posit" their accuracy and power. In particular I want to separate out Chomsky's claims about the regularities of English (or language) from the inferences he goes on to draw (from the 'success' of his descriptions) regarding the cognitive structures whereby speakers produce such regularities, the 'innate' mental structures that might account for such cognitive regularities, and the learning process (or its lack) whereby new speakers learn the regularities of one language vs. another, or of human language itself. In a major sense, then, I want to address Chomsky's claims regarding the human "language acquisition device" (LAD) and the implications of his descriptive theory (if posited as 'correct') re such a device; but I do want to note that my remarks pertain not only to implications of the descriptive theory for "acquisition" but also for our ongoing, day to day, production of sentences (words, discourse, or whatever).

The basic, underlying claim is that the complexities and interdependencies captured by the descriptive theory are so complex as to be un-reproducible by any device that did not directly embody the rules and operations of the descriptive theory (or their close analogs), and that these rules and operations are too complex for humans to learn from scratch within the time and maturational constraints of the period which humans learn language--implying that the basic framework of operations and potential rules must already be present (i.e., be "innate"). I do want to note that the logical or physiological nature of this "innate" presence is left unspecified--as a "mystery" for psychologists to solve. I want also to note that the learning possibility against which this "innateness" alternative is evaluated is the naive Skinnerian/Behaviorist one of direct inductive generalization based directly and simply only on reinforcement; such evaluations have not, to my knowledge, been made (or even attempted) re more modern constructivist models of the sort pioneered by Piaget, and variously developed in a variety of forms by many

different cognitive psychologists. The differences in language learning facility between humans and other primates, contrasted with the greater similarities in many kinds of non-linguistic learning and reasoning are sufficient to suggest that there clearly exist at least some species-specific, innately driven aspects of our language use and learning; the question concerns first whether the package which produces language is a single indissoluble whole vs. some mix of different kinds of components, and, second, if a package, which components are specific to humans (vs. general across all mammalian learning, or general to learning in some more general sense) and which are specific to language (vs. general across some wider class of cognitive activity, or across more general mental/neurological functioning). These are issues that I have previously addressed elsewhere (Kronenfeld 1979); for present purposes I do not intend to repeat that argument. Instead I propose to utilize two narrower and more specific analogous examples over which we have much better experimental control to illustrate my point and what is at issue in it. I will then suggest that these other cases represent at least a kind of hypothesis regarding the complex language example--and I will suggest that analytic claims and approaches in anthropology and elsewhere based on any tight adherence to the Chomskyeian analytic model are, by this demonstration, clearly at risk.

For my second example of a successful formal description whose cognitive implications need to be examined, I want to take up Lounsbury's account of Crow- and Omaha-type kinship terminological systems (Lounsbury 1964b, 1965). Again, as with the Chomsky example, I want to note the existence of debate concerning the accuracy and correctness of Lounsbury's analysis (a debate which, in this case, I have myself participated [see Kronenfeld 2001a for an overview]), but want for present purposes to posit Lounsbury's accuracy and correctness. Additionally, I want to separate the notational scheme via which Lounsbury expresses his kinship regularities (structures, rules, etc.) from the regularities themselves. Finally I want to acknowledge that for Lounsbury's analysis there exists noteworthy debate concerning the logical or mathematical completeness or rigor of the analytic game itself; in this area there have been serious (and to my naive mind) reasonably successful attempts to construct genuine algebraic formulations (which, as I understand it, there have not been for the larger and messier natural language syntax problem)--Read's (1974, 1984, 2001 and Read and Behrens 1990) and Gould's (2000) are two that I am particularly aware of, also see Lehman's work (1993, 2001, and Lehman and Witz 1974, 1979).

Lounsbury has shown how a very small set of rewrite rules, some of which require a small set of alternative (for different empirical terminologies) context constraints, can logically account for the assignment of kintypes (abstract genealogical positions) to kinterm categories for a number of anthropologically important classes of kinterminologies. These classes particularly include Crow-type and Omaha-type systems, but also, by a simple extension from his work, include also Dravidian-type systems--that is, his rules cover the bulk of what have sometimes been called "Classificatory" systems.

For my third example of a successful formal description of a cognitively structured, behaviorally produced regularity, I would like to take the description of a circle in analytic geometry. This one really is mathematical, and is embedded in a much more general mathematical system.

The general description of a circle (any and all circles) in this formal system is " $x^2 + y^2 = a^2$ ", where "x" and "y" are the values taken by points on the circle respectively on the X- and Y-axes of a graph, where the origin of the graph is taken as the center of the circle, and where a, then, is the radius of the circle; the circle is the complete set of (real number) points on the graph which satisfy the equation.

It might be argued that this example is unlike the others, since circles are an abstraction, which are only approximated by our attempts to draw them. However, it also seems that circles are a conceptual category that we have abstracted out of our experience, and it seems that the abstraction (in our understanding, if not in some pure Platonic world) comes out of our experience with the ones that we have tried to produce and/or make use of.¹

The first thing I want to note about our cognitive processing of circles is that we rarely, maybe never, make them in any way like the production sequence entailed by the formula--that is by plotting successive values of x and y in a coordinate system, where the successive values are sufficiently close together to represent a continuous line. What we do is, in a formal mathematical sense, largely equivalent to the formal representation (equivalent to the degree that our drawn circle is an accurate circle), but the details (actual axioms regarding entities and operations, if

¹ These last aspects, to my mind, are sufficient to make the proposed parallel of circles to kinship structures and grammatical structures reasonable.

you will) are quite different. a) Commonly we move our hand--the one holding the pen or pencil--around in what feels like a constant distance from some hypothetical center point. The "feeling" which guides this hand/arm movement is not mathematically calculated, and is not tightly based on any continuous calculating or monitoring of the distance from the center (see below for why this matters); instead the feeling is a subconsciously structured pattern of movements based on past iterations of a process of drawing a circle, noting its deviations from the intended ideal, and trying to modulate the next production to correct such deviations. (This process has extended over each of our individual lifetimes--but, in this particular case of simple circles, maybe particularly going back to kindergarten and primary school !) This amounts to a process of successive approximations, where the revised approximations are based on a combination of observation of the previous result, proprioceptive feedback about smooth and continuous muscular movement, and some underlying algebraic group-like understanding of the effects of actions within the presumed Euclidian space of our immediate world². b) On other occasions we pick a template (e.g. a coin) and draw a line around it. c) Sometimes we connect our pencil or pin via a compass or a taut string to a point stuck on the origin, and move the pencil everywhere it will go with that linkage. d) Some other variants also exist.

Thus, for one of the most elementary mathematical objects, our normal cognitively structured means of producing the object has no direct and immediate relationship to the formal mathematical description of the object.

However, my point is not limited to--or even primarily aimed at--the accidents of how we happen to approximate some production, important though these may be. There exist alternative formal representations of the object in question. In a radial analytic geometry, the formula for a circle becomes " $r = a$ ", where "r" represents the distance of the given point from the origin (which, again, has been taken as the center of the circle) and "a" is the radius of the circle; the circle, again, is the complete collection of points which satisfy the equation. Thus, there exist very different formal mathematical accounts of the same object. Obviously the alternative formalizations have to be mathematically equivalent. For the logician or mathematician such great differences in axiomatic entities and

² See Flavell 1963:135-142, 327-341 for an outline of Piaget's work and views concerning group structures and our understanding of space.

relations pose no problem--as long as the final, formal mathematical equivalence can be shown. But the differences should give great pause to any cognitive scientist who is tempted to claim that the specific entities of some formalization directly represent elements in the cognitive (or mental or neurological) make-up of the creature producing instances of the formally described object, or who is tempted to claim that the specific structure of the formalization can be taken as directly implying the cognitive (or mental or neurological) structuring utilized by the creature producing the object; that is, alternative formal representations, even if formally equivalent, would imply radically different (and, at this cognitive level, often contradictory) cognitive structures under any strong theory concerning the specific equivalence between a formal description of an entity and the cognitive production of that entity.

I claim that this insight applies not only to circles or to geometry, but also to the kinds of formal accounts of kinship terminologies and natural language syntax that we earlier looked at.

Let us return now to the kinship example. There exist a number of serious alternatives to Lounsbury's rewrite approach, all of which are equivalent in the sense that each produces the correct assignment of the relevant range of kintypes to kinterms. They sometimes differ widely, however, in the specific nature of their basic entities and operations--and in the analytic questions that they best answer. Such analytic questions or goals, for example, can include the best representation of native speaker definitions, the best framework for comparing the logic or operations of different systems, the best framework for comparing terminological categories with relevant aspects of social organization, economic structure, or cultural content, the best framework for describing the cognitive entities and properties of underlying mental representations of native speakers, and so forth. Such questions, as Hugh Gladwin and I long ago noted in the unpublished introduction to an unpublished Mathematical Social Science Board conference on the formal analysis of kinterminologies, can be quite varied, and can be quite differently addressed by alternative kinds of formal analysis.

Gould (2000, and see Kronenfeld 2001b), for example, is the latest in a long line of analysts (going back at least to Greenberg 1949) who have constructed an algebraic representation of the concatenation of parent-child and

spouse or sibling relations which structure genealogical space and then examined the key equations which generate the membership in different genealogical categories in different kinterminologies. I have assembled and edited the volume in which Gould (a mathematician, now deceased) describes his system and applies it to a wide range of "classificatory" systems; I single out Gould's system because it seems to me to be the clearest, simplest to understand and use, most concise in its entities and operations, and most extensively applied to actual systems.

Read (1974, 1984, 2001--also Read and Behrens 1990), on the other hand, offers an algebraic group system based on relative product relations among categories (the categories of the system being analyzed) and generating equations--which involves no obvious or direct representation of genealogical space. Read's system is particularly attractive to me because it closely parallels (though in a much more mathematically elegant and complete way) the kind of system I came up with when I attempted to produce a formalized version of the kinds of statements which my Fanti informants used to calculate, explain, and justify their own assignment of relatives or kintypes to kinterms.

In a previous article (Kronenfeld 1980) I compared a reasonably³ straightforward Lounsburian-type analysis (involving componential feature contrasts--cf. Lounsbury 1964a--among categories and their kernel kintypes and rewrite/extension accounts of the extended membership of categories) of my Fanti data with my formalization of the Fanti manner of calculation. I started the comparison with a view toward deciding which form was superior, but quickly tripped over the fact that they addressed very different tasks--and thus were each better or worse depending on the analytic question being asked. Of the two (i.e., leaving Gould, Read, and other approaches out), the Fanti formalization--obviously--provided a much better representation of Fanti calculations regarding genealogical kin; it furthermore accounted fairly straightforwardly for the application of kinterms to kinsfolk for whom the speaker did not know the precise genealogical linkage. The Lounsburian analysis, on the other hand--obviously--provided a better vehicle for comparing the Fanti system with other systems, and provided a better vehicle for comparing the structure of terminological categories with the structure of the categories implicit in other kinds of behavior among kinsmen. Furthermore, and perhaps more surprisingly, the Lounsburian analysis enabled a

³ The hedge means that while Lounsbury himself accepted my analysis as a "Lounsburian" one, Scheffler took issue with it as not using the canonically correct entities and operation forms.

better explanation of how kinterm labels were applied to non-kinsfolk (as they commonly were) and a clearer account of the conjunctively defined superordinate categories (equivalent to "parent" in English) which Fanti speakers seemed clearly to utilize in their thought (as expressed in conversation) but which appeared not to be labeled in Fanti.

We are now in a position to return to our consideration of Chomsky's descriptive theory. We are in a position to see what I mean when I suggest that Chomsky could well be right about his formal description of English syntax (or, even, about general properties of the syntax of all languages) and still be deeply wrong about the psychological--and hence, neurological, biological, and evolutionary--implications of that description--including claims about the specific nature or shape of either the LAD or any other supposedly innate structures or knowledge. By "right" I mean that he may well have correctly, insightfully, and powerfully described deep regularities of English syntax, as felt by native speakers. I clearly do not mean that his being "right" need automatically make others wrong; alternative accounts may accurately portray other regularities or serve other purposes.

Let me reiterate, though, I have not shown (nor tried to show) Chomsky's descriptive theory to be right, and I have not demonstrated his account of the psychological implications of his descriptive theory to be wrong. What I have tried to do is open up or broaden our consideration of these issues, and I have tried to warn anthropologists who might be tempted to leap too quickly to emulate some piece of Chomsky's reasoning in regard to their own account of one or another system of cultural knowledge of some of the pitfalls they risk.

My own sense, for the record, is that a producer-oriented model of syntax--analogous to my formalization of the Fanti system for kinterm calculations--will focus more on speakers' recognition of patterns and their attempts to reproduce and extend them in a Piagetian-like equilibration process with past patterns being embedded, in the manner of Miller's chunking (Miller 1956) in newer patterns; the embeddings would be subconscious and automatic, but capable of being raised to consciousness. I think this pattern-extension process, at the individual level, is not restricted to current syntactic practice but works on any patterns--as we perhaps see in people's attempts, say, to

make productive use of English strong verb inflectional patterns when the phonetic shape of some otherwise inappropriate verb seems to invite it. See Kronenfeld 1979 and 1996:138-141 for a fuller discussion of some of these issues--and see below.

In part I am merely quarreling with the connotations of the word "rule"--as used by Chomsky and others to capture the regularities they find in language; I would prefer to see such regularities as patterns which we recognize in speech around us, and then use in our own constructions of speech acts. Such patterns would be of structural elements (i.e., here, perceived/guessed at categories of words and smaller patterns). We are not 'forced' to follow such rules/patterns, but use them as best accords with our communicative aims--important among which, however, will of course be some component of comprehension. Our LAD, then, would not be quite so unique to grammar as Chomsky would have it; it would, by this view, consist of a drive to find such patterns and a drive to systematize their interrelations--including contrastive relations, embedding, relative focus, etc. By "pattern" here I definitely do not mean any kind of statistical regularities of behavioral patterning. It seems to me, in simple terms, we keep looking for logical/structural patterns that we can generalize from and that we can play variations and extensions on. My model for such learning is the kind of interpreted Piagetian view that I sketch in Kronenfeld 1996:237-242.

I think my position winds up being a variant on the idea of "functional grammar", with the differences that I attribute much more importance to structure and that I buy much more of Chomsky's specific descriptions of syntactic relations that do participants in the Functional Grammar approach--but, I hasten to confess/warn, I am not sufficiently up on Functional Grammar to know for sure whether I am right regarding either my agreement or my disagreements. Some of these issues are addressed, a little, in my book chapter on Miller and chunking (Kronenfeld 1996:114-143. Also raised by these issues are the reasons I prefer (my version of) Saussure's *Lingue/Parole* contrast over Chomsky's competence/performance one.

Larger Issues

The mathematics-like formality of linguistic descriptions has always appealed to anthropologists as a model for how to do social science. The attraction came from the explicitness, succinctness, and power that successive

linguistic models have offered; embedded within such models has always been some kind of simplicity criterion, wherein the preferable solution was the one which used the least and/or simplest machinery to accomplish the most.

This formalism has almost always--so far--been mathematics-like, rather than truly mathematical (in the sense of an mathematically well-formed theory--see Kronenfeld 2001:151-154), in part because of the situation that I first encountered in a mathematical linguistics course that I took in graduate school. The course covered what was mathematically understood about language like systems and what mathematical devices were used in linguistic description (then, early transformational). The problem was that there was no overlap between the two parts--what was mathematically understood and what was linguistically useful--nor, even, any significant connection between them. Subsequently, work on kinship terminological structures such as that of Lehman, Read, and Gould that I have already cited has closed the gap--at least for that much simpler domain.

This paper is primarily about the attractions and limitations of linguistic formalism as a model for anthropology, but also in some ways for linguistics itself.

Linguistics has consistently provided a model for social science--at least as far back as the 19th Century, when linguistic science consisted of Neogrammarian "laws" about regular sound change. Linguistics offered the best example (or case) in human behavior of science-like regularities susceptible to precise, succinct, and powerful mathematical-like characterization.

As anthropologists we seem never to have quite been able to decide whether the privileged position of linguistics came from the fact that linguists did it better--implying that we anthropologists should copy their methods--or from the fact that linguistic systems leant themselves to the enterprise much more readily than did culture, the other system or systems⁴ delineated by anthropologists--implying perhaps some further search for theoretical insight prior to serious attempts at formalism.

⁴ depending on how one views culture !

That is, what repeated attempts to treat culture like language--by applying linguistics-like formal analyses--have shown is that culture as a whole is nowhere near as coherently and completely structured as is language--even given the partial successes produced by each generation of linguistic formalism and the cultural or social regularities that each highlighted.

At the same time, more local and specific formal (linguistics related) descriptions have proved quite useful (worked quite well) within anthropology (cf. kinship, folk taxonomies, ollas and drinking vessels, etc.). This has occurred, I will argue, in areas where there exist both substantive reasons for such complexity within in the phenomenon itself and significant cognitive support for such complexity in the manner and quantity of usage. "Substantive reasons... within the phenomenon" refers to the presence in the given domain of a significant number of culturally and linguistically recognized entities that relate systematically to one another in structured ways. "Cognitive support...usage" refers to the presence of sufficient conversational usage of various alternative analytic categorization of the entities for children (new speakers) to recognize and learn the features that structure the domain. I discuss these considerations in more detail in my Plastic Glasses and Church Fathers (Kronenfeld 1996:134-137).

Computer programming has more recently provided another kind of model for potential anthropological formalism which has enjoyed some success (cf. the decision theory work of the Gladwins (C. Gladwin 1975, 1980; Gladwin and Butler 1984; H. Gladwin and C. Gladwin 1971) , Hutchins' 1980 work on Trobriand land litigation, Schank and Abelson's 1977 restaurant work, etc.); Colby's work on folk tales (1973, 1975) and divination (Colby and Colby1981) represents a joining of computer approaches with insights from Propp and Halliday. These computer related formal approaches will not be further pursued in this paper.

The role of formal accounts (whether linguistics-like, or more mathematical or computational) is itself, then, raised. A given formal treatment can be a good and powerful way of describing regularities found within a system. By showing such regularities and by laying bare the constraints they represent, a successful formal account (and the formal game the account is based on) will have powerful implications regarding the cognitive processes that

produce the behavior in question; but that formalism will not necessarily be equivalent to those cognitive processes. In fact, I want to suggest that, for the examples we have so far considered, whether in anthropology, in linguistics, and (even !) in mathematics itself, the formal descriptions of regularities in behavior (or in the products of such behavior) are assuredly not directly equivalent to the cognitive processes that produced them--and for a good and general reason.

Insofar as the cognitive processes are aimed at reproducing some perceived regularity in the experienced world--that is, a regularity exterior to the cognizing critter which is seen, felt, heard or otherwise experienced by that critter--the critter will work things through in a necessarily somewhat ad hoc manner (after all, it does not completely know where the process is headed) in its attempt to produce the desired pattern. As experience requires, and as time permits, the critter will repeat the process, taking account of how recent previous attempts worked out, and iteratively approach the goal.

I have in mind here, in a general way, a Piaget-like process of equilibration involving successive approximations--and a related presupposition that relatively subtle features will only be noticed and dealt with after relatively gross ones (within the given universe or domain) have been mastered. In this interactive cognitive process the kind of understanding that is necessary for a sophisticated, axiomatic solution only comes, if at all, at the end of the process (and, presumably, not for all tasks and not for all those attempting to master any given task)⁵. In speaking of "successive approximations" I mean explicitly to assert that this is not a strictly inductive, logical process--but, instead, that it is a process of refining hypotheses via insight, guesses and leaps with a view toward reducing the gap that remains between reach and grasp (cf. Newell and Simon's old General Problem Solver work).

One of the things we do is try to perceive patterns in the world around us--and then generalize our expectations regarding these patterns and what they represent (what the patterns can guide us to look for, how to use them in accomplishing various tasks, etc.). 'Things in the world around us' would seem to include the actions

⁵ This distinction is an important part of what is at issue in Piaget's distinction between "concrete operations" and "formal operations", and why the order in which they are learned is what it is. See Flavel 1963:19-25, 164-236 for an overview of the Piagetian concepts.

and products of other people as much as of the non-human world. As observers we may well perceive patterns in the speech of those around us that the producers are themselves unaware of. We then can try to reproduce these in our own version of the behavior. This is another way of thinking about the process that produces linguistic drift and that I discussed in *Plastic Glasses...* (Kronenfeld 1996)--though, there, more in regards to semantic change.

The regularities can get into our behavior (in this case, we as the unknowing perceived sources of the regularity in question) in the first place because all of our cognitive activity seems to represent some process of adapting some existing skills, knowledge, structures, ... to new tasks; for all novel tasks we first look in our kit-bag of skills, etc. to see what might give us a starting point. That is, we are doing some new task as we do some other, old, task--and, for cases where the repetition is successful enough to get further repeated, those observing us and learning from our behavior can pick up the new pattern and build it into their representation of the activity at hand (whether language, culture, or ...).

I want to note that, at least at this abstract level, there is no issue (pro or con) regarding 'innate' knowledge, rules, etc.--at least regarding syntax, semantics, and culture. That is, any innate dispositions could easily affect the specific actual content of what we perceived and how we tried to represent such perceived patterns--without affecting the iterative homing-in equilibration process that I have alluded to.

Insofar as a pattern of behavior or knowledge seems productively repeated in our experience, we can strive for an ever purer and cleaner representation of it--of the pattern's "Platonic essence", if you will. This means that we can have "Platonic" English sentences or "Platonic" kin categories as easily as "Platonic" circles--even if the circles seem universal in a way that the sentences or kin categories surely do not (and even if there may well turn out to exist some universal [whether within the human mind or within the logical world] set of grammatical operations like the apparently universal set of geometric figures [c.f. what we inscribe on space probes--for alien consumption]). It is these "Platonic" patterns that our cognitive equilibration process homes in on, and it is these which, if I am right, our theories of syntax and of kinship describe. These patterns are "Platonic" because they are rarely realized in our

actual productions; but note also that they can serve as targets of equilibration, and as targets of formal theoretical representation without actually existing in empirical sense; such a pattern can be something to which a trend appears to be heading--even if the trend never gets there.

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