CLARIFICATION
THIS IS A CONTINUATION OF CHOMSKY'S RECENT ARTICLE, WHICH WILL APPEAR IN MARTIN ET AL. (LASNIK'S FESTSCRIFT). AS I SAID BEFORE, THE PAPER IS GLOSSED WITH MY OWN THOUGHTS (IN CAPITALS), AND IT KEEPS FOOTNOTES AS THEY APPEAR IN THE ELECTRONIC MANUSCRIPT. FINALLY, I MUST INSIST ON THE FACT THAT, EVEN IF WE HAVE CHOMSKY'S PERMISSION TO DISCUSS HIS WORK IN THIS FORMAT, THIS IS NOT INTENDED FOR DISTRIBUTION AS SUCH.

<4. The Strong Thesis>

Let us now consider the strongest minimalist thesis (2) and ask where it fails.

PERHAPS IT SHOULD BE EMPHASIZED THAT ONE COULD BE DOING MINIMALISM WITHOUT TAKING THIS STRONG THESIS. FOR EXAMPLE, THE METHODOLOGICAL THESIS THAT HORNSTEIN HAS FORCEFULLY ADVOCATED IS GENUINELY MINIMALISTIC, YET NOT (AT LEAST NOT NECESSARILY) IN THE STRONGEST SENSE THAT WE ARE NOW GOING TO EXPLORE.

The research strategy is to seek "imperfections" of language, properties that language should not have, assuming (2). Apparent imperfections come in several varieties. Given some apparent property P of language, we may find:

(29)
(i) P is real, and an imperfection
(ii) P is not real, contrary to what had been supposed
(iii) P is real, but not an imperfection; it is part of a best way to meet design specifications

The most likely -- hence least interesting -- outcome is (i). Conclusion (ii) is less likely, therefore more interesting; efforts within the minimalist program have sought to establish (ii) in particular cases.

WE WILL REVISIT SOME.

Conclusion (iii) is the most interesting possibility.

WHY THIS IS MORE INTERESTING THAN (II) IS NOT COMPLETELY CLEAR TO ME--THEY BOTH SEEM EQUALLY INTERESTING!

The question is novel, perhaps premature; or unreasonable, because the strongest minimalist thesis (2), or even weaker versions, is not correct, as one might expect for some complex biological system.

AGAIN, THIS IS THE IMPORTANT THEME ABOUT BIOLOGICAL MESSINESS THAT WE SAW AT THE BEGINNING.

Adopting (2) as a point of departure, assume that FL provides no machinery beyond what is needed to satisfy minimal requirements of legibility and that it functions in as simple a way as possible. We would like to establish such conclusions as (A)-(D):

{These are conceptual "good design" conditions, but fairly trivial ones, based on the assumption that less machinery is better than more.}

(30)
(A) The only linguistically significant levels are the interface levels

OF COURSE, THE PROBLEM HERE IS TO KNOW WHAT ARE REALLY THE INTERFACE POINTS, SOMETHING WHICH IS NOT USUALLY DISCUSSED. (SOUND AND MEANING IS *OBVIOUSLY* TOO SIMPLISTIC.)

(B) The <interpretability> condition: LIs have no features other than those interpreted at the interface, properties of sound and meaning

SAME ISSUE--WHAT ARE REALLY THE INTERFACES?

(C) The <inclusiveness> condition: No new features are introduced by C<HL>

THIS IS WHAT I CALLED BEFORE THE CONSERVATION LAW. OF COURSE, THE CONSERVATION LAW INCLUDES *BOTH* THE INCLUSIVENESS CONDITION AND A CONDITION PREVENTING UNRECOVERABLE DELETIONS. NUNES (1995) CLAIMS THAT THE CLASSICAL RECOVERABILITY CONDITION FOLLOWS FROM ECONOMY, UNDER THE ASSUMPTION THAT RECOVERABILITY RULES ONLY OVER DELETION, AND IF YOU CAN YOU SHOULD TRY TO AVOID DELETION (IN OTHER WORDS, IF YOU DO NOT DELETE, YOU DO *LESS* OPERATIONS THAN IF YOU DO DELETE). THAT SEEMS TO ME TO MISS THE POINT. IF I WERE TO BE ALLOWED TO DELETE THE ENTIRE SET OF WORDS IN THE NUMERATION (SAY, AFTER SELECTING THEM AND EITHER BEFORE OR AFTER MERGING THEM) I'D OBVIOUSLY SIMPLIFY THE SYSTEM: I'D DO NOTHING BEYOND THAT POINT, NO MOVE, NO SPELL-OUT, NO ATTRACT, NOTHING. DELETION WOULD BE EXTREMELY OPTIMAL--IT WOULD REDUCE THE DERIVATION TO SELECT, PERIOD. OBVIOUSLY THAT'S ABSURD, AND WHAT PREVENTS IT IS THE TACIT ASSUMPTION THAT IF John IS IN THE NUMERATION IT SHOULDN'T BE DELETED, IT HAS TO MAKE IT TO LF. THAT'S RECOVERABILITY. WHY THE SYSTEM SHOULD HAVE RECOVERABILITY, SINCE AS WE SAW DOESN'T FOLLOW FROM ECONOMY, IS THEN INTERESTING--AS IS WHY THE SYSTEM SHOULD HAVE INCLUSIVENESS. MY SUGGESTION IS THAT WE THINK OF THOSE TWO TOGETHER AS A CONSERVATION LAW. CONSERVATION LAWS ARE THE MOST FUNDAMENTAL IN THE NATURAL WORLD. IN PHYSICS THEY STARTED WITH THE LAW OF CONSERVATION OF MATTER, THEN ENERGY, THEN MATTER-ENERGY, AND THEY NOW EXTEND TO SOME SEVEN OR MORE CONSERVATION LAWS IN ALL REALMS OF REALITY (E.G. CONSERVATION OF A PARTICLE SPIN, AT THE BASIS OF FIELDS, AND SO ON). EVEN IN OTHER FIELDS

YOU COULD SPEAK OF CONSERVATIONS; FOR INSTANCE, THE INSTINCT TO SURVIVE, WHATEVER THAT IS, IS THE ENGINE THAT DRIVES EVOLUTION--WITHOUT IT, THE WHOLE LOGIC OF THE SYSTEM COLLAPSES. THE FACT THAT THESE CONSERVATIONS ARE CALLED LAWS CLEARLY TELLS YOU THAT NOBODY REALLY KNOWS WHERE THEY COME FROM, OR HOW TO REDUCE THEM TO SOMETHING MORE BASIC. IN ANY CASE, BEHIND THE CONSERVATION OF INFORMATION THAT I'M TALKING ABOUT HERE IS THE IDEA THAT PATTERNS OF FORM ONLY EMERGE IN THE DYNAMIC INTERACTIONS OF MORE ELEMENTARY FORMS. TAKE FOR INSTANCE AN EDDIE THAT FORMS IN A CURRENT: WE KNOW IT TO BE THE RESULT OF THE MACROSCOPIC INTERACTION OF FLUID MOLECULES THAT FEED-BACK INTO THE SYSTEM AFTER SOME OBSTACLE. NOTHING IN THE SYSTEM IS *ADDED* SO THAT YOU CAN GET THE EDDIES FORM; THEY ARISE OUT OF THEIR OWN BUMPS AND MOVES, NOTHING ELSE; LIKEWISE, YOU CANNOT CHOSE NOT TO HAVE THE EDDIES IF THE RIGHT OBSTACLES AND FLUID VISCOSITY AND VELOCITY ARE IN PLACE--THAT INFORMATION SIMPLY DOESN'T GO AWAY. YOU CAN SPEAK OF CONSERVATION PROPERTIES HERE BECAUSE THE RESULTING PATTERN (WHAT COMES OUT) IS A CONSEQUENCE OF WHAT CAME IN. I PROPOSE THAT WE EXTEND THE ANALOGY TO THE INCLUSIVENESS/RECOVERABILITY PROPERTIES OF DERIVATIONS, WHICH WOULD THEN FIT NICELY WITH OTHER SYSTEMS WHERE FORM EMERGES OUT THERE. AND INCIDENTALLY, IF YOU'RE TEMPTED TO THINK THAT PHRASES OR CHAINS ARE CREATED OUT OF NOTHING IN THE SYSTEM, THINK AGAIN: THEY'RE CREATED ALRIGHT, BUT NOT OUT OF NOTHING; THEY ARE PRECISELY LIKE THE EDDIES: CREATED OUT OF MACROSCOPIC INTERACTIONS OF PARTICLES. CHAINS, SAY, ARE CREATED OUT OF INTERACTIONS OF PHRASES, WHICH ARE CREATED OUT OF INTERACTIONS OF WORDS. THE KEY IS NOT TO ADD ANYTHING TO THAT--THAT'S A CONSERVATIVE SYSTEM, VERY MUCH IN LINE WITH THE STRONG MINIMALIST THESIS.

> (D) Relations that enter into C<HL> either (i) are imposed by legibility conditions, or (ii) fall out in some natural way from the computational process

THAT SECOND ONE IS WHAT I WAS JUST TALKING ABOUT. NOTE, INCIDENTALLY, THAT (i) AND (ii) ARE, IN A SENSE, REPRESENTATIONAL (i) AND DERIVATIONAL (ii). IS THERE ANYTHING IN THE NATURAL WORLD CORRESPONDING TO (i)? THAT'S THE TOUGH QUESTION WE MENTIONED BEFORE, CONCERNING THE NATURE OF A REPRESENTATIONAL SYSTEM. FOR WHAT IT'S WORTH, I WANT TO POINT OUT THAT THERE IS A CHARACTERISTIC 'STATIC' ASPECT TO (i), WHICH MIGHT ALLOW US TO SPEAK OF IT WITHOUT HAVING TO INVOKE TRICKY ISSUES ABOUT REPRESENTATIONALITY. THAT IS, (ii) IS CLEARLY DYNAMIC, IN THE CYCLIC SENSE WE'VE SEEN BEFORE, AND IN MORE WAYS THAT I'LL RETURN TO; IN CONTRAST, THE DEMANDS THAT INTERFACE SYSTEMS IMPOSE ON (i) MAKE THE RELEVANT RELATIONS RATHER FIXED. SIMILAR ISSUES ARISE IN ORGANISMS, WHICH DEBATE THEMSELVES BETWEEN STATIC AND DYNAMIC FORCES. WITHOUT DYNAMIC FORCES THERE WOULDN'T BE ANY METABOLISM, ANY REPRODUCTION, ANY DECAY; AT THE SAME TIME, WITHOUT STATIC POINTS

OF STABILITY THERE WOULDN'T BE ANY ORGANS, OR MORE BASICALLY SKELETONS, AXES OF VARIOUS SORTS, MEMBRANES... THERE ARE SOME SERIOUS CONJECTURES IN THE SYSTEMS LITERATURE AS TO HOW ONE MIGHT WANT TO CHARACTERIZE THIS DIALOGUE BETWEEN DYNAMIC AND STATIC PULSIONS. FOR INSTANCE, PEOPLE SPEAK OF DISSIPATIVE PHASE TRANSITIONS (E.G. GLOBAL CHANGES IN AN OPEN SYSTEM THAT, FOR INSTANCE, GOES INTO TURBULENCE) VS. CONSERVATIVE PHRASE TRANSTIONS (E.G. GLOBAL CHANGES IN A QUASI-CLOSED SYSTEM, E.G. CRYSTALLIZATION). MY POINT IS THIS: WE MAY FIND GOOD METAPHORS AND EVEN WELL-STUDIED SYSTEMS IN THIS VIEW, WHICH ABSTRACTLY FITS RATHER NICELY WITH THE TWO PROPERTIES OF A GRAMMAR: THE STABILITY POINTS (STARTING WITH LEXICON ITEMS AND FINISHING WITH CHAINS) VS. THE DYNAMIC INTERACTIONS (MOVE, AGREE, AND SO FORTH). NOTICE THAT THE WORD 'SYMBOLIC REPRESENTATION' DOESN'T HAVE TO APPEAR HERE--WHETHER YOU NEED IT BEYOND THIS MACHINERY IS OF COURSE POSSIBLE.

{The significance of (ii) was brought out in Epstein's (1994) derivational analysis of c-command, which underlies much important work since, including Epstein et al. (1998).}<EPS = footnote>

Condition (A) requires that there is no Deep or Surface structure, or other levels that have been proposed.

WELL, I DON'T SEE THAT. I SEE THAT FOR SURFACE STRUCTURE, IN THAT IT IS AN *INTERNAL* SYSTEM--BUT D-STRUCTURE COULD BE AN INTERFACE WITH SOME OTHER SYSTEM. WE MIGHT NOT WANT THIS ON EMPIRICAL GROUNDS (KEARNEY'S ARGUMENT), BUT THAT'S A DIFFERENT STORY. IT DOESN'T *FOLLOW* FROM THE LOGIC OF THE PROPOSAL THAT D-STRUCTURE SHOULDN'T EXIST UNLESS WE PROVE THAT IT COULDN'T BE INTERFACING SOME REASONABLE SYSTEM, WHATEVER IT MIGHT BE. I'LL SAY MORE: THE WAY WE'VE LAYED THINGS OUT (WITH A NUMERATION, CONDITION (6) ON MERGE, THE INCLUSIVENESS/RECOVERABILITY CONDITION) IT WOULDN'T EVEN BE UNREASONABLE TO INTERPRET ALL THAT AS RESULTING FROM AN INTERFACE CONDITION WITH SOME SYSTEM--SAY HALE/KEYSER'S CONCEPTUAL STRUCTURE. AGAIN, THAT NEED NOT BE A *LEVEL OF REPRESENTATION* INTERFACE (AND COULD NOT BE ONE, IF KEARNEY IS RIGHT); BUT THERE COULD STILL BE A DYNAMIC CONNECTION WITH THAT CONCEPTUAL SYSTEM. IF SO, WHETHER WE CALL IT D-STRUCTURE OR WHETHER WE LIST ITS PROPERTIES SEPARATELY ((6), NUMERATION, ETC.) IS JUST A MATTER OF TERMINOLOGY. I SHOULD ADD HERE, INCIDENTALLY, THAT MANY WANT TO GET RID OF THE NUMERATION AND (6) FOR PRECISELY THAT REASON--SO THAT NO D-STRUCTURE RESIDUE REMAINS IN THE SYSTEM. THAT'S A NOBLE CAUSE, BUT ONE THAT, IT SEEMS, MISINTERPRETS THE STRONG THESIS. THE STRONG THESIS, TO REPEAT, JUST ASKS YOU TO OPTIMIZE THE INTERFACE CONNECTIONS. IF THE INTERFACES HAPPEN TO BE SIX HUNDRED AND SIXTY SIX, YOUR TASK IS NOT TO SEE HOW TO GET RID OF SIX HUNDRED AND SIXTY FOUR LEVELS, BUT RATHER TO FIGURE OUT A WAY OF OPTIMALLY CONNECTING WHAT YOU HAVE.

OF COURSE, IT'S EASIER TO CONNECT TWO THINGS THAN TO CONNECT THREE OR MORE, AS NEWTON SHOWED. BUT THAT'S A DIFFERENT POINT. NOW, YOU MIGHT WANT TO STOMP YOUR FOOT INSISTING ON THE EXISTENCE OF ONLY TWO INTERFACES--THE INEFFABLE SOUND AND MEANING. THAT, HOWEVER, SEEMS AN A PRIORI, THUS UNSCIENTIFICLY DOGMATIC POSTURE. FOR ALL I KNOW OR CARE, THERE MAY BE INTERFACES WITH THE MUSICAL SYSTEM. SURELY, A *WORKING* HYPOTHESIS COULD BE TO GO WITH TRADITION, AND SAY 'WELL, ALRIGHT, BUT SINCE WE DON'T KNOW HOW TO TEASE THE DIFFERENT ASPECTS OF MEANING, WE'LL LUMP THEM ALL TOGETHER INTO AN INTERFACE.' THAT'S FINE, BUT THEN YOU HAVE TO BE PREPARED TO SEE THE VARIOUS POSSIBILITIES, IF THEY MAKE SENSE, AND YOU SHOULDN'T REALLY BANK ALL YOUR MONEY ON GETTING RID OF A WELL-ESTABLISHED COMPONENT OF THE SYSTEM (E.G. (6)) JUST BECAUSE IT WOULD BE MORE MINIMALIST OTHERWISE. IT ISN'T. THE LOGIC OF 'LESS LEVELS IS BETTER THAN MORE LEVELS' ONLY WORKS *UP TO FACTUALITY*. TRIVIALLY, THUS, FOR INSTANCE, A SINGLE LEVEL OF, SAY, LF WOULD BE EVEN BETTER, RIGHT? EXCEPT WE WOULDN'T HAVE HUMAN LANGUAGE, WE'D HAVE TELEPATHY. THIS IS RATHER SERIOUS, AND YOU SHOULD BE AWARE OF WHAT YOU'RE SAYING WHEN YOU INSIST ON ONLY LF AND PF *BECAUSE IT'S SIMPLER* THAN THAT PLUS D-STRUCTURE. YOU SHOULD REALLY ASK YOURSELF WETHER IT GIVES YOU BOTH 'CONCEPTUAL NECESSITY' AND, OF COURSE, THE FACTS--FOR INSTANCE THE FACTS THAT (6) IS MEANT TO COVER. WE'LL RETURN TO ALL THIS LATER ONE IN THE COURSE.

It holds that everything accounted for in these terms has been misdescribed and is better understood in terms of legibility conditions at the interface: that includes the Projection Principle, Binding Theory, Case theory, the Chain Condition, etc.

Condition (B) is transparently false.

RIGHT, ALTHOUGH WE'LL HAVE TO ASK SOME TOUGH QUESTIONS HERE, LIKE WHETHER THE FEATURES THAT DO NOT FIT THE INTERPRETABILITY DESIGN ARE REALLY PART OF THE CORE LEXICAL ITEM, OR THEY ARE BORN AT THE NUMERATION (THAT WOULD MAKE ALL THE MORE SENSE IF IT IS THERE THAT A CONNECTION WITH THE CONCEPTUAL SYSTEM IS CHECKED).

Condition (C) permits rearrangement of lexical items LI and of elements constructed in the course of derivation, and deletion of features of LI. But optimally, nothing more.

AND EVEN THAT MIGHT NEED TO BE STREAMLINED...

Condition (D) has to be spelled out. Properties induced by legibility conditions might include adjacency, argument structure, scope, etc. Those of category (ii) should include at least (perhaps at most) the relations provided directly by the indispensable computational operation Merge. But there should be no government, no stipulated properties of chains, no binding relations internal to language, no interactions of other kinds.

THIS, OF COURSE, IS THE CENTRAL PHILOSOPHY OF THE PROGRAM. THE ONLY NOTE OF CAUTION I'D ADD IS THE SAME ONE I'VE BEEN PRESSING. IT'S AN EMPIRICAL ISSUE WHETHER, SAY, CHAINS HAVE A SEPARATE LEVEL OF INTERFACE. I DOUBT THAT'S THE CASE, BUT MY REASONS ARE EMPIRICAL. OR PUT DIFFERENTLY, YOU COULD IMAGINE MARTIANS HAVING FL LIKE WE DO AND A SYSTEMATIC INTERFACE WITH A CHAIN SYSTEM, IN WHICH CASE IT WOULD BE JUSTIFIED TO HAVE A CHAIN COMPONENT WHERE YOU WOULD HAVE OPERATIONAL PROPERTIES OF CHAINS IN SOME FORM; IF HOWEVER THIS IS NOT FL, THEN THE PROPERTIES OF CHAINS IN QUESTION (UNIFORMITY, COMMAND, COLLAPSE AT SOME POSITION, AND SO ON) SHOULD FOLLOW FROM SYSTEMIC DYNAMICS. THAT'S WHERE THE GAME BECOMES REALLY INTERESTING.

It is hardly necessary to observe that all of this is highly unlikely.

HERE I FEEL CHOMSKY IS BEING BOTH TOO CAUTIOUS AND TOO MODEST. THESE ARE SURELY UNLIKELY SCENARIOS, BUT SO IS EVERYTHING ELSE IN MIND AND, INDEED, EVEN IN LIFE. HEY, LIFE IS AN UNLIKELY EVENT, IT SEEMS! NONETHELESS, IF YOU'RE INTERESTED IN THE STUDY OF THE EMERGENCE OF FORM IN THIS UNIVERSE (THE BASIS FOR LIFE, MIND, AND OTHER SYSTEMS), THE LOGIC OF CHOMSKY'S PROGRAM IS NOT ONLY HIGHLY LIKELY, BUT INDEED THE ONLY GAME IN TOWN (WHICH OF COURSE DOESN'T MEAN IS RIGHT).

There is substantial empirical evidence supporting the opposite conclusion at every point.

THAT *IS* A FAIR POINT. PARTICULARLY BECAUSE:

Furthermore, a basic assumption of the work in the P&P framework, with its impressive achievements, is that everything just suggested is false: that language is highly "imperfect" in these respects.

HOWEVER, IT MIGHT BE TIME TO REFLECT ON WHY THAT WAS THE P&P LOGIC. (HERE I'LL BE DOING HISTORIC RECONSTRUCTION, HENCE I'LL BE EVEN MORE PERSONAL THAN BEFORE, SO TAKE EVERYTHING I SAY WITH A TON OF SALT.) I SEE TWO LINES TO EXPLORE, FOR THE 'HISTORIAN OF MODERN LINGUISTICS'... ONE: CHOMSKY CAME INTO THE ARENA OF MIND STUDIES WITH A BOMB FROM THE PAST: AS SOME NATURAL PHILOSOPHERS HAD BELIEVED, FUNDAMENTAL ASPECTS OF LANGUAGE ARE INNATE. FROM THE MID FIFTIES UP TO THE MID EIGHTIES THIS ONLY HAD ONE INTERPRETATION WITHIN MAIN-STREAM BIOLOGY, PSYCHOLOGY, AND SO ON: WHAT PINKER CALLS "THE LANGUAGE INSTINCT". ACTUALLY, THERE WERE TWO SUB-INTERPRETATIONS. ONE, ADVOCATED BY PINKER, BICKERTON, JACKENDOFF, NEWMEYER, AND OTHERS, IS ESSENTIALLY ADAPTATIONIST. LANGUAGE FORMS EMERGED BECAUSE OF SOME FUNCTIONAL NEED. ANOTHER ONE, ADVOCATED BY FODOR, BERWICK, OR LIGHTFOOT IS WHAT YOU MAY CALL, FOLLOWING GOULD, 'EXAPTATIONIST'.

THAT IS, LANGUAGE FORMS DID NOT EMERGE BECAUSE OF SOME FUNCTIONAL NEED, BUT RATHER AS THE BY-PRODUCT OF SOME OTHER FORM, WHOSE ORIGIN MAY OR MAY NOT, ITSELF, BE AS AN ADAPTATION (E.G. THE BRAIN GREW LARGER FOR SOME REASON, AND THAT LIBERATED ENOUGH 'MIND SPACE' TO ALLOW FOR LINGUISTIC STRUCTURES). THE LATTER POSITION IS THE ONE THAT, SO IT HAS ALWAYS SEEMED, ACCORDS BEST WITH THE FACTS OF LANGUAGE (THE KEY ONE BEING THE AUTONOMY OF SYNTAX). BUT NOTE AN IMMEDIATE CONSEQUENCE IT HAS: SINCE YOU'RE TRYING TO SEE THE LANDMARKS OF A KIND OF INNATE INSTICT, ONE FURTHERMORE ARISING FROM AN EXAPTATION (IN THE 'RIGHT' VIEW), YOU WELCOME ANY SUB-OPTIMAL QUIRK OF LINGUISTIC FUNCTION THAT YOU ENCOUNTER. FOR INSTANCE, YOU LOOK AT 'CENTER EMBEDDING' AND YOU SAY: 'SEE? THAT STUPID, INNATE LINGUISTIC FORM LEADS TO SOMETHING THAT IS UTTERLY UNCOMMUNICABLE.' APART FROM BEING A CHALLENGE TO THE ADAPTATIONIST VIEW, THAT WAS BIG-TIME ANTI BEHAVIORIST AND SO ON, FOR HOW COULD YOU EXPLAIN THE ACQUISITION FROM THE ENVIRONMENT OF A KIND OF FORM THAT DOESN'T MAKE ANY GOOD SENSE? NOW, CHOMSKY HAS ALWAYS BEEN A BIT DISTANT ON THIS DEBATE, MAKING CRYPTIC REMARKS HERE AND THERE ABOUT LANGUAGE BEING 'LIKE A CRYSTAL' OR REMISCENT OF 'FIBONACCI PATTERNS', USUALLY NOT IN PRINT. ONE INTERPRETATION OF THIS DISTANCE IS THAT CHOMSKY WAS ACTUALLY BEHIND THE 'RIGHT' VIEW, ALBEIT WITH A TWIST: LANGUAGE ISN'T STRUCTURALLY SUB-OPTIMAL. NOTICE, BY THE WAY, THAT THIS DOESN'T AFFECT AT ALL THE LOGIC OF THE 'RIGHT' VIEW, WHICH ONLY DEMANDS SUB-OPTIMALITY *OF FUNCTION* AND HAS ABSOLUTELY NO STAKE ON WHETHER THE FORMS HAPPEN TO BE OPTIMAL OR NOT. THESE TWO, HOWEVER, AREN'T ALWAYS EASY TO SEPARATE, FOR SOMETIMES THE OPTIONAL FORM DOES HAVE AN ASSOCIATED OPTIMALITY OF FUNCTION (E.G. IF DERIVATIONS ARE COMPUTATIONALLY OPTIMAL, THAT WILL NO DOUBT AID THE PARSING OF CORRESPONDING REPRESENTATIONS). CONSEQUENTLY, SOMETIMES LACK OF OPTIMALITY IN STRUCTURE WAS SEEN AS A WELCOME TURN OF EVENTS IN THE DEBATE I'VE JUST MENTIONED (FOR THE 'RIGHT' VIEW ANYWAY), SINCE IT WAS MORE KINDLING TO THE NATIVIST ANTI-ADAPTATIONIST FIRE.

BUT THERE IS A SECOND INTELLECTUAL LINE WHOSE PROPERTIES HAVE, I THINK, AFFECTED CHOMSKY'S GEARING OF THE P&P SYSTEM IN A DIFFERENT DIRECTION. IN RETROSPECT (AND ASIDE FROM THE TREMENDOUS WEALTH OF DATA THAT THE MODEL HAS UNEARTHED), THERE WERE TWO ARCHITECTURAL LINES THAT HAVE POWERED P&P RESEARCH IN THE EIGHTIES, AFFECTING MUCH OF CHOMSKY'S OWN RESEARCH. THESE ARE THE EXTENDED PROJECTS OF KAYNE AND LASNIK; BY THAT I MEAN NOT JUST THEIR WORK WITH THEIR VARIOUS ASSOCIATES, BUT ALSO THE REACTIONS FROM MANY OTHER COLLEAGUES. KAYNE'S PROJECT WAS FOUNDED ON THE MATHEMATICAL PROPERTIES OF TREES AND THE VARIOUS INTERACTIONS THEY DO AND DO NOT ALLOW. LASNIK'S, ON THE COMPUTATIONAL DYNAMICS OF THE TRANSFORMATIONAL COMPONENT AND ITS VARIOUS RESULTS. ALL THE CONNECTEDNESS AND LCA LITERATURE IS BASED ON THE FIRST LINE OF

RESEARCH, WHEREAS ALL THE EXTRACTION AND BINDING LITERATURE IS BASED ON THE SECOND. ASIDE FROM ALL THE EMPIRICAL RESULTS, TWO PROPERTIES IMMEDIATELY JUMP OUT OF THESE LINES: THEY ENRICHED THE SYSTEM WITH NEW NOTATIONS (PATHS, SUBSTANTIVE PROJECTION LEVELS, ABSTRACT FEATURES, INDICES) AND THEY ALLOWED FOR VERY RICH INTERACTIONS, BE THEY AT THE TREE LEVEL (CONNECTEDNESS) OR AT THE DERIVATIONAL LEVEL (AFFECT @). IT IS IMPORTANT TO SEE THAT THESE SYSTEMS WERE CONCEPTUALLY SPOTLESS, THE LOGICAL EXTENSIONS OF (AND TO SOME EXTENT BASIS FOR) THE GOVERNMENT AND BINDING THEORY OF LGB. THE QUESTION, THOUGH, WAS THIS: DO WE WANT A SYSTEM WITH SUCH NOTATIONAL AND OPERATIONAL RICHES? MY SENSE IS THAT CHOMSKY ATTEMPTED THE MINIMALIST MOVE IN THE LATE EIGHTIES WITHIN THE CONTEXT OF ADDRESSING THAT INTERNAL QUESTION.

IT IS CURIOUS, HOWEVER, THAT THESE TWO LINES CONVERGE, UNDER CERTAIN ASSUMPTIONS. OF COURSE, WE ALL STILL WANT TO BE NATIVIST AND MANY OF US STILL HAVE NO REASON TO GIVE UP THE 'EXAPTATIONIST' LINE (WHERE FORM DOESN'T GENERALLY FOLLOW FROM FUNCTION). BUT DO WE HAVE ANYTHING ABOUT THE EMERGENCE OF LINGUISTIC FORM OTHER THAN ITS NOT BEING AN ADAPTATION? HERE CHOMSKY'S OLD METAPHOR OF THE CRYSTAL AND THE FIBONACCI PATTERNS MAY WELL FIND A CURRENT INSTANTIATION IN A SYSTEM THAT AVOIDS THE OPERATIONAL AND NOTATIONAL EXCESSES OF THE PAST, HENCE (ARGUABLY) REALISTICALLY IN TERMS OF PRESENT-DAY MIND STUDIES THAT MAY ASSUME D'ARCY THOMPSON'S GENERAL QUESTION FOR BODY PLANS.

It would be no small task, then, to show that this apparatus is a kind of descriptive technology, and that if we abandon it, we can maintain or even extend descriptive and explanatory force.

I READ THIS SENTENCE IN THE TERMS EXPRESSED IMMEDIATELY ABOVE, AS I DO THE NEXT. THERE'S A SENSE OF EXCITEMENT IN THE NEXT SENTENCE, ACTUALLY, WHICH I FEEL PERTAINS NOT JUST TO THE PROGRESS WE'VE WITNESSED IN LINGUISTICS, BUT MORE GENERALLY IT SEEMS TO ME ALLUDES, ALSO, TO PROGRESS IN OTHER (MORE OR LESS RELATED) FIELDS. IN MY VIEW, THOUGH STILL CONTROVERSIAL IN MANY CAMPS, IT IS NO LONGER WEIRD TO ARGUE FOR THE INNATENESS OF LANGUAGE. THE ISSUES NOW ARE MORE SUBTLE, PERTAINING TO PROFOUND QUESTIONS ABOUT FORM WHICH ARE, NO DOUBT, STILL HIGHLY NON-STANDARD. BUT (PERHAPS OPTIMISTICALLY) I FEEL AS IF WE'VE MOVED ONE NOTCH FORWARD IN STUDIES OF COMPLEX SYSTEMS, AND (BY FAR) WE DON'T KNOW ANY COMPLEX SYSTEM AS WELL AS WE DO THE LINGUISTIC ONE.

Nevertheless, recent work suggests that such conclusions, which seemed out of the question a few years ago, are at least plausible, possibly correct in nontrivial respects.

Much of <MP> is devoted to establishing parts of (30) in terms of bare phrase structure, with a

cyclic notion of generalized transformations and reinterpretations of Binding and Case theory. I will assume that these are the right directions, though many serious problems remain.

Consider Condition (C). It requires that there be no phrasal categories or bar levels, hence no X-bar theory or other theory of phrase structure, apart from bare phrase structure, which will be simplified further below. It also rules out introduction of traces, indices, lambda operators, and other new elements in the course of operation of C<HL>. Recourse to such devices could be innocuous (e.g., if used for convenience to annotate properties that can be determined by inspection at LF), but questions arise if they enter into interpretation and function significantly within the computation -- for example, percolation of indices, operations that apply specifically to trace, etc.

THIS IS ALL STRAIGHTFORWARD, AND IS WHAT THE CONSERVATION LAW IS SUPPOSED TO PREVENT. INCIDENTALLY, YOU CAN THINK OF THESE SORTS OF QUESTIONS IN OTHER DOMAINS AS WELL. SO IMAGINE YOU WERE STUDYING PROTEIN FOLDING AND YOU NEEDED INDICES TO CODE A DEPENDENCY BETWEEN TWO LONG-DISTANCE CHUNKS OF PROTEIN. WOULD YOU TAKE THE INDICES TO BE REAL? PERHAPS, BUT NOT WITHOUT CONSEQUENCE, SURELY. ALTERNATIVELY, THE INDICES ARE JUST MACHINERY WITHOUT A BIO-CHEMICAL BASE. MUTATIS MUTANDIS, WE MAY BE RIGHT OR WRONG ABOUT THE EXISTENCE OF INDICES, BAR LEVELS, OR EVEN SYMBOLS THEMSELVES. BUT THE STRONG THESIS FORCES YOU TO CONSIDER THE CONSEQUENCES OF ANY OF THE DECISIONS YOU MAKE HERE.

In <MP>, indices are introduced, but not within C<HL> itself; rather, in extending lexical arrays to numerations (see note <NUMER>). This narrow departure from Condition (C) could be eliminated by weakening the requirement that an item of a lexical array be removed when accessed in computation, leaving this as an option.

IF SO, THE NUMERATION WOULD IN ESSENCE NOT BE A SET OF TOKENS, BUT A SET OF TYPES, AS FAR AS I CAN SEE. THE ISSUE IS CONFOUNDED BY A RELATED QUESTION: IS THE LEXICON A SET OF TYPES OR A SET OF TOKENS? THIS SEEMS LIKE A MORE TRIVIAL QUESTION THAN IT IS. IF YOU'RE TEMPTED TO THINKING THAT OF COURSE THE LEXICON IS A SET OF TYPES, TELL ME WHAT'S YOUR ARGUMENT. I ONLY KNOW OF ONE, WHICH TRIES TO REDUCE THE ALTERNATIVE VIEW TO THE ABSURD--AND IT PROBABLY BEGS THE QUESTION. IF YOU'RE GOING TO SAY THAT THE LEXICON IS A SET OF TOKENS, YOU'LL NEED A COPYING MECHANISM, SO THAT EVERY TIME YOU ACCESS THE LEXICON YOU ACTUALLY COPY SOME TOKEN, AND USE IT IN A SENTENCE. IS THIS COPYING MECHANISM CONCEPTUALLY NECESSARY? WELL, ONE THING IS ARGUABLE: IT INTUITIVELY ADDS INFORMATION: PRIOR TO THE COPY YOU HAD ONE TOKEN, AFTER THE COPY YOU HAVE ANOTHER ONE (OR PERHAPS A TOKEN SEPARATED FROM THE LEXICON, IN THE SPACE OF A DERIVATION). THEN AGAIN, THE CONSERVATION LAW THE WAY IT IS USUALLY UNDERSTOOD RESTRICTS THE INPUT-OUTPUT RELATION IN THE DERIVATION, THAT IS, BETWEEN THE LEXICAL ARRAY AND THE LEVELS OF REPRESENTATION. HERE, THOUGH, WE'RE NOT

TALKING ABOUT DERIVATIONS, BUT ABOUT WHAT GOES ON *PRIOR* TO DERIVATIONS. CHOMSKY HAS OPENED THAT CAN OF WORMS WHEN TALKING ABOUT COMPLEXITY IN A DERIVATIONAL SIZE, HIS SOLUTION BEING IN TERMS OF REDUCING LEXICAL ACCESS. THE QUESTION IS WHETHER THAT PRE-DERIVATIONAL MAPPING (FROM THE LEXICON TO THE LEXICAL ARRAY) OBEYS SOME CONSERVATION LAW, AND IF SO WHAT FORMAT IT HAS. IF IT DOES, AND THE MAPPING CANNOT CREATE STRUCTURE, THEN YOU WON'T BE ABLE TO JUST HAVE TOKENS IN THE LEXICON, YOU'LL NEED TYPES TO INSTANTIATE. BUT THIS IS NOT MUCH BETTER, TO TELL YOU THE TRUTH, SINCE NOW YOU MUST REALISTICALLY ASK YOURSELF 'WHAT'S A TYPE?' REMEMBER, THE NOTION 'TYPE' WAS INVENTED BY ARISTOTLE AND WORKED OUT BY RUSSELL, BUT NOTHING IN ITS PHILOSOPHICAL HISTORY FORCES YOU TO ASSUME IT, OR WORSE STILL, TO TAKE IT FOR GRANTED. BLUNTLY PUT, IF TYPES EXIST IN MIND, YOU HAVE TO SEE WHAT THAT MEANS, AND WHAT IT MEANS FOR THEM TO DIFFER FROM THEIR INSTANTIATIONS INTO TOKENS. FOR INSTANCE, ARE TYPES PARADIGMATIC AND, IN SOME SENSE, IN LONG-TERM MEMORY, WHEREAS TOKENS OF THOSE TYPES ARE SYNTAGMATIC AND, AGAIN IN SOME SENSE, IN SHORT-TERM MEMORY? PERHAPS--WHO KNOWS. BUT THAT SORT OF THING IS WHAT'S AT STAKE, AT THE VERY LEAST.

Whether that is the correct move depends on the consequences. A minor matter is that it would require modification of interpretive procedures at the LF interface.

THAT IS, IF THE INTERFACE DOESN'T DEAL WITH IDENTIFYING TOKENS FROM THE NUMERATION, YOU'LL HAVE TO TELL THE INTERFACE WHAT PROCEDURE IT MUST FOLLOW IN ORDER TO GET 'ANOTHER ONE OF THOSE'. NOTHING TRIVIAL, FOR SURE. ONE MIGHT EVEN WONDER WHETHER SUCH THINGS AS THETA-ROLES OR CASE FEATURES (OR OTHER THINGS) AREN'T TELLING LF PRECISELY THAT. NOTE, INCIDENTALLY, THAT THAT SPECULATION WOULD ADDRESS ONE OF MY WORRIES FOR THE VERSION OF THE SYSTEM WHERE LF IS THE SINGLE 'SEMANTIC INTERFACE'. MY CONCERN WAS HOW TO TAKE THAT SERIOUSLY, WHEN LF IS A NON-CONFIGURATIONAL SYSTEM (A COLLECTION OF CHAINS) WHILE THETA STUFF IS CONFIGURATIONAL. BUT YOU MIGHT RESPOND TO ME THUS: WITHOUT THETA STUFF YOU WOULDN'T EVEN BE ABLE TO TELL APART THE DIFFERENT TOKENS THAT ENTER LF, IF YOU DON'T MARK THEM IN THE NUMERATION. THAT'S CONCEIVABLE, AND WE SHOULD RETURN TO IT LATER ON.

More important, it would require a new notion of <chain>. That seems to be the only significant consequence, and it merits some thought.

THAT'S PRECISELY THE POINT I WAS TRYING TO MAKE. I'LL POSTPONE FURTHER DISCUSSION OF THESE ISSUES UNTIL LATER, THOUGH.

INCIDENTALLY, ONE OTHER THING. THE INDICES IN ITEMS IN THE NUMERATION WERE NEVER REALLY NEEDED--AND THAT'S NOT THE PROBLEM. THAT IS, YOU

COULD SAY THE NUMERATION IS A SET OF TOKENS, AND CLEARLY A TOKEN OF A MAN IS NOT THE SAME AS ANOTHER TOKEN OF A MAN, OR THERE WOULDN'T BE YOU AND ME. BUT OF COURSE, WHETHER THE NUMERATION IS A SET OF TOKENS IS DEEP, AND WHAT WE'RE DISCUSSING. (IF IT IS, IS IT BECAUSE WE'RE COPYING TOKENS FROM AN ALL TOKEN LEXICON--WHICH I DOUBT--OR BECAUSE WE'RE SOMEHOW INSTANCIATING THOSE TOKENS FROM THE LEXICON INTO A SHORT-TERM-MEMORY TRACK--WHICH I SUSPECT--AND IF THE LATTER WHAT DOES IT MEAN TO INSTANCIATE SOMETHING? ALL FAMILIAR, TOUGH STUFF.)

If _ in the syntactic object SO is merged somewhere else (by the operation Move) to form SO', then the two occurrences of _ constitute a chain, the original occurrence called the "trace" or "copy" of the new one.

THIS IS THE TRADITIONAL TAKE ON CHAINS, OFTEN MISUNDERSTOOD.

The terminology is misleading, for several reasons. First, each of the elements is a "copy" of the other;

OF COURSE, MATTERS HERE GET EVEN MORE MESSY, SINCE THESE COPIES ARE INTERNAL TO THE SYSTEM, THUS NOT REALLY COPIES OF TOKENS, BUT (AS USED TO BE CALLED IN LSLT) OCCURRENCES OF THOSE TOKENS. NOW YOU HAVE TO WORRY ABOUT SOMETHING ELSE, AGAIN. YOU HAD TYPES AND TOKENS AND NOW YOU HAVE TOKENS AND OCCURRENCES. ARE THE RELATIONS AMONG EACH OF THOSE THE SAME? MATHEMATICALLY THAT COULD BE POSSIBLE, WITH OCCURRENCES BEING TOKENS OF TOKENS UNDERSTOOD AS TYPES. MY HUNCH, FOR WHAT IS WORTH, IS THAT REALISTICALLY THIS IS NOT WHAT'S HAPPENING AT ALL, AND IF WE'RE TO JUDGE FROM THE LONG-TERM/SHORT-TERM DISTINCTION IN TERMS OF ACTIVATION OR THE PARADIGMATIC/SYNTAGMATIC CHARACTERIZATION IN TERMS OF THEIR FORM, OCCURRENCES ARE NOT TOKENS AT ALL; IN FACT, THEY ARE NOT EVEN 'MATERIAL' IN A SENSE THAT I RETURN TO--THEY ARE MERE OPERATIONAL CONSTRUCTS THAT SURVIVE ONLY WHILE THE DERIVATION IS ACTIVE. MY STRONGEST ARGUMENT FOR THIS IS IN TERMS OF THE PARADIGMATIC/SYNTAGMATIC CUTS. THAT IS, THE LEXICON IS REAL IN A PARADIGMATIC SENSE. WE WITNESS LEXICAL RULES, FOR INSTANCE, IN TERMS OF THE PANINIAN ELSEWHERE CONDITION, OR CANONICITY RESTRICTIONS, WHICH HOLD OF PARADIGMS. THERE IS NO SUCH THING INSIDE THE SYNTACTIC ENGINE, JUST AS--I'LL TRY TO ARGUE, CONTRA GENERATIVE SEMANTICISTS--THERE IS NO SYSTEMATICITY, TRANSPARENCY, AND REGULARITY IN THE LEXICON, ALL SYNTAGMATIC PROPERTIES. IF YOU USE THIS AS YOUR WEDGE TO SEPARATE TYPES FROM TOKENS, THEN IT IS PLAINLY THE CASE THAT THERE ARE NO PARADIGMATIC PROPERTIES THAT HOLD INTERNAL TO THE MEMBERS OF A CHAIN, HENCE NO JUSTIFICATION IN THINKING IN THEM AS TYPES OF ANYTHING BY MY CRITERIA. INDEED, I SUSPECT THAT *NO* PROPERTIES HOLD INTERNAL TO THE MEMBERS OF A CHAIN, WHICH IS WHY I THINK IT IS A

MISTAKE TO THINK OF THEM AS WHAT I'M CALLING 'MATERIAL'.

second, "copy theory" is the simplest version of transformational grammar, making use only of Merge, not Merge followed by an operation that deletes the original; and under trace theory, a further operation that creates a new kind of element, trace, a serious violation of inclusiveness. These are "imperfections," to be avoided unless shown to be necessary. I will continue to use the terminology, but only for expository convenience, adopting the "copy theory" as the null hypothesis.

THESE ARE ALL OPERATIONAL POINTS, BUT FAIRLY REASONABLE ONES.

{That the "copy theory" is the simplest version is clear in the earliest formulation, in Chomsky (1955-6). The more complex operation Merge-Delete was adopted there on the assumption that T-markers are mapped to PF. When that picture was modified under EST, Delete was abandoned in favor of trace theory, trace being a new kind of element. That was an error (mine), and the "copy theory," which restored the simplest case of the original approach, was mistakenly regarded as a further innovation. The divergence of history and logic has caused much confusion.}

THIS IS A VERY INTERESTING FOOTNOTE, HISTORICALLY. IT IS WORTH KEEPING IN MIND, BUT IT IS ALSO IMPORTANT TO UNDERSTAND THAT WHETHER COPY THEORY IS SIMPLER OR NOT THAN MOVE--AND I DO THINK IT IS--DEPENDS ON ADDRESSING THE COMPLEX MORASS OF QUESTIONS JUST POSED.

If LAs are extended to numerations NUM, a chain can be defined as a sequence of identical _'s; more accurately, a sequence of <occurrences> of a single _.

THIS IS WHAT I WAS SAYING. THESE ARE NOT TOKENS OF _, REMEMBER. IF YOU HAVE A HARD TIME UNDERSTANDING HOW A COPY OF _ IS NOT A SEPARATE TOKEN, YOU'RE PERHAPS BEING A BIT TOO LITERAL WITH HOW THE MATERIAL UNIVERSE OPERATES. TAKE THE FAMOUS 'TRANSPORTER' IN STAR TREK. IT'S SUPPOSED TO WORK BY MEANS OF 'TELEPORTATION' (SOMETHING WHICH, INCIDENTALLY, HAS BEEN RECENTLY ACHIEVED FOR A LASER BEAM, I BELIEVE). IT GOES LIKE THIS: YOU'RE SOME SET OF PARTICLES, AND THE TRICK IS TO FIND (BY SOME MEANS) AN IDENTICAL SET OF PARTICLES IN ANOTHER REGION OF THE UNIVERSE. BY THE WAY, YOU'RE PROBABLY ALREADY TROUBLED: HOW CAN THE TWO SETS BE IDENTICAL IF THEY ARE IN DIFFERENT REGIONS? WELL, CHECK YOUR INTUITIONS ABOUT IDENTITY: HOW DO YOU KNOW THAT TWO ELECTRONS ARE *NOT* IDENTICAL? WHEN THINGS GET AT THAT LEVEL, THEY END UP BEING SO SIMILAR THAT, EXCEPT FOR TIME/SPACE (OR SIMILAR DIMENSIONS, OFTEN MORE ARCANE ONES) THEY ARE QUITE LITERALLY THE SAME BY MOST CRITERIA. CERTAINLY, YOUR PARTICLES ARE CHANGING CONSTANTLY, AND THE FACT THAT THEY ARE DIFFERENT (BY SOME CRITERION) WHEN YOU'RE BORN AND WHEN YOU DIE DOESN'T MEAN THERE ARE TWO DIFFERENT YOU'S OUT THERE. SO THEN THE TRICK IS THAT, FOR ALL THAT COUNTS (E.G. CONSCIOUSNESS AND MEMORIES IN STAR TREK...), THE TWO

SETS OF PARTICLES (YOURS AND THE OTHER) ARE INDEED IDENTICAL. AT THAT POINT, SO THE STORY GOES, YOU'VE BEEN TELEPORTED. OF COURSE, THE TRICK IS TO FIND OUT THE EXACT SAME PARTICLES AND TO ARRANGE THEM APPROPRIATELY; HOWEVER, THIS IS TRICKY ONLY IN THEORY: YOU'RE VERY COMPLEX. IT APPARENTLY IS NOT SO TRICKY ANYMORE FOR WELL BEHAVED CHUNKS, LIKE THE BEAM I'VE JUST MENTIONED. A SECOND PROBLEM IS ALSO OBVIOUS: WHY DOESN'T THE PROCEDURE CREATE TWO YOU'S? AS IT TURNS OUT, FOR PROPERTIES OF QUANTUM REALITY THAT I DON'T CLAIM TO UNDERSTAND, YOU CANNOT MAKE ONE OF THESE 'COPIES' AND KEEP THE ORIGINAL--SOMETHING IN THE EQUATIONS FAILS IF YOU DO THAT. THE WAY I VISUALIZE IT IS THUS: YOU STRETCH AND BOND AND KNOT A GIVEN PART OF THE UNIVERSE TO GET YOUR PATTERN OVER THERE, AND AS A CONSEQUENCE OF ALL THAT TWISTING AND SHOUTING ANOTHER PART OF THE UNIVERSE (THE PRESENT YOU) DISENTANGLES. ANYWAY, THAT'S TELEPORTATION, AND REMEMBER, THE ISSUE WAS: DID WE CREATE A SEPARATE TOKEN OF YOU WHEN WE TELEPORTED YOU? WELL, NOT COMPLETELY OBVIOUS, AT LEAST IN STAR TREK (ALTHOUGH A GLORIOUS EPISODE PLAYS WITH PRECISELY THIS ISSUE, HAVING RYKER DUPLICATED AND APPEARING LATER ON TO CONFRONT THE 'REAL' RYKER--SOMETHING WHICH, AS I SAY, IS APPARENTLY PHYSICALLY IMPOSSIBLE). MORE TO THE POINT: CONCEPTUALIZE THE MOMENT IN WHICH WE ACTUALLY ARE REPRODUCING YOU OUT THERE, SO THAT WE SORT OF SIMULTANEOUSLY (ALTHOUGH THAT'S REALLY NOT THE RIGHT NOTION) HAVE YOU HERE AND THERE. ARE YOU THEN TWO TOKENS OF YOU? OR IS THAT A MERE ARTIFACT ABOUT HOW THE SYSTEM WORKS? TRICKY ISSUES--EVEN IN PHYSICS! AT ANY RATE, I SUGGEST THAT WHEN YOU EXPLORE THESE MATTERS PERTAINING TO COPIES, CHAINS, AND SO ON, YOU GO WITH WHAT WORKS, ON A MATHEMATICAL SENSE, FORGETTING ABOUT MORE OR LESS NAIVE INTUITIONS ABOUT COPYING A MATERIAL THING, OBJECTS HAVING TO BE IN A SINGLE POSITION, AND OTHERWISE TAKING THE COPIED OBJECTS, AS IT WERE, TOO SERIOUSLY. I MEAN, TAKE THEM SERIOUSLY AS FORMAL OBJECTS, BUT DON'T CONFUSE THE METAPHOR OF AN OPERATIONAL COPY WITH THE CREATION OF AN OBJECT IN THE SYSTEM.

That seems to be the simplest way to characterize chains, hence to be adopted unless shown to be empirically inadequate. Thus in (12iii), repeated as (31), there are two occurrences of <I> and two occurrences of <a proof> (where the terms are spelled out and at the trace):

THAT IS, 'TERMS' IN THE SENSE OF THE {a, {a, b}} NOTATION; IN OTHER WORDS, A CHAIN HAS, BY DEFINITION, MORE THAN ONE TERM (WE'LL RETURN TO THIS).

(31) I<{i}> T [<t><{i}> expect [[a proof]<{j}> to be discovered <t><{j}>]]

Chains so-defined can be formed only by movement, given LA = NUM.

THE CONSERVATION LAW AGAIN. OF COURSE, THERE IS AN EMPIRICAL ISSUE HERE; WHETHER, FOR INSTANCE, ELLIPSIS SHOULD BE CONSIDERED A CHAIN

PHENOMENON (EVEN IF IT OBVIOUSLY DOESN'T INVOLVE MOVEMENT, ETC.). WE'RE SETTING THAT ASIDE NOW, ASSUMING THE ANSWER IS NO.

Basic properties of chains should then follow from elementary derivational principles. That would be a good result, eliminating stipulated properties of chains and explaining why these hold.

IN FACT, IT WOULD BE MORE THAN JUST A GOOD RESULT; IT'S A NECESSARY RESULT IF THE SYSTEM IS DESIGNED THE WAY WE HAVE DESIGNED IT, WITHOUT AN INTERFACE LEVEL SPECIFICALLY FOR CHAIN PROPERTIES.

If achievable, it would also bear on broader questions that arose in sec. 3, providing support for the derivational interpretation of the recursive function C<HL>, hence for the surprising conclusion that there is even an empirical issue concerning alternative interpretations.

THIS TOO IS VERY IMPORTANT (ALTHOUGH IT IS CONCEIVABLE, THOUGH I HAVEN'T SEEN THIS PROVEN) THAT NATURAL CHAIN PROPERTIES COULD ALSO BE OBTAINED IN A REPRESENTATIONAL FORMAL SYSTEM, AS A CONSEQUENCE OF MERE FORMAL INTERACTIONS.

What exactly do we mean by an "occurrence of _"? To ensure that occurrences in the intended sense are distinguished, we can take an occurrence of _ in K to be the full context of _ in K.

NOTE, THIS ISN'T JUST A METHODOLOGICAL ISSUE (HOW TO REFER TO THE DIFFERENT COPIES). GO BACK TO THE TELEPORTATION METAPHOR. IT WAS IMPORTANT TO TELEPORT YOU BOTH TO TRACK YOU DOWN (WHAT IS CALLED IN STAR TREK 'LOCKING INTO YOUR COORDINATES'...) AND TO TRACK DOWN THE REGION OF THE UNIVERSE WHERE YOU ARE TO REAPPEAR, INCLUDING THE GENEROUS PARTICLES OVER THERE THAT WILL RECONSTITUTE YOU (FORTUNATELY, THE UNIVERSE HAS A GOOD SUPPLY OF THE PARTICLES THAT MAKE YOU). WITHOUT KNOWING THE CONTEXT WHERE YOU ARE AND WHERE YOU WILL BE, THERE SIMPLY WON'T BE ANY TELEPORTATION FOR YOU, SINCE IN A SENSE THE TRICK IS TO TURN THE CONTEXT INTO YOU, WHILE UMPLUGGING YOU IN THE CONTEXT YOU INITIALLY OCCUPY (LETTING YOUR INITIAL PARTICLES GO FREE). IN OUR CHAINS TOO, YOU MUST KNOW THE REACH OF THE CHAIN, THE CONTEXT WHERE IT STARTS AND THE CONTEXT WHERE IT REAPPEARS. INDEED, IT IS PERHAPS BEST TO THINK OF THE CHAIN AS LINKING THOSE TWO CONTEXTS, JUST AS YOU CAN BE THOUGHT, UPON A TELEPORTATION, AS LINKING TWO GIVEN PHYSICAL CONTEXTS. IN A VERY LITERAL SENSE, THEN, A CHAIN IS A SORT OF STRETCHING OF A PHRASAL CONTEXT BEYOND THE NORMAL LIMITS IMPOSED BY THE MERGE OPERATION.

{We could, for example, identify this as K' = K with the occurrence of _ in question replaced by some designated element OCC distinct from anything in K. In Chomsky (1955-6), "occurrence" is defined in terms of linear order, adapting a device from Quine's <Mathematical Logic>; but that mechanism is not available here.}

THIS IS A FORMAL FOOTNOTE TO TELL YOU HOW OCCURRENCES WERE CODED INITIALLY. FOR WHAT IT'S WORTH, I WOULDN'T BE TOO CAVALIER ABOUT DISMISSING QUINE'S NOTATION AS MERELY FORMAL. IT IS NOT ALTOGETHER CLEAR THAT MANY OF QUINE'S ARTIFACTS CAN BE REPLACED BY ANYTHING WITHIN THE CONFINES OF SET THEORY. FOR INSTANCE, TAKE HIS DEFINITION OF ORDERED PAIR {{a}, {a, b}}. WHAT ELSE CAN YOU DO? NOT MUCH. YOU COULD TRY {a, {a, b}}, IF YOU DON'T CARE ABOUT NOT ASSUMING THE FOUNDATION AXIOM (BUT THERE ARE REASONS FOR ASSUMING IT). YOU CANNOT JUST SAY 'I'LL PAINT ONE ELEMENT BLUE', SINCE THAT'S NOT A PREDICATE THAT SET THEORY ALLOWS WITHOUT APPROPRIATELY ENRICHING IT. SO IT MAY TURN OUT THAT YOU'RE STUCK WITH QUINE'S NOTATION FOR GOOD FORMAL REASONS. IF SO, THIS IS AS CONCEPTUALLY NECESSARY AS A SIMILAR POINT ABOUT DERIVATIONS. THE POINT IS, IN ORDER TO ARGUE FOR THE DERIVATIONAL SYSTEM YOU MUST BE FAIR WITH THE REPRESENTATIONAL ONE: THAT SYSTEM TOO MAY HAVE SOUND FORMAL LIMITATIONS THAT YOU'RE FORCED INTO FOR NON-TRIVIAL REASONS.

In <MP>, a simpler notion is proposed: an occurrence of _ is a sister of _.

THIS IS SIMPLE, BUT IT MAKES A FANTASTICALLY INTERESTING CLAIM, ALONG THE LINES OF WHAT I WAS SUGGESTING ABOVE FOR QUANTUM REALITY. NOTE, A SISTER OF _ IS QUITE LITERALLY _'S CONTEXT (EXPLICITLY SO IN MP). SO TRANSLATING THE STATEMENT ABOVE, AN OCCURRENCE OF _ IS _'S CONTEXT. AS WE SAW, THAT'S A WAY OF DESCRIBING WHAT GOES ON IN YOUR TELEPORTATION, AND IT WOULD BE APPROPRIATE TO DEFINE EACH OF YOUR OCCURRENCES AS YOUR CONTEXT--INDEED, THAT'S THE *ONLY* WAY OF DEFINING YOUR OCCURRENCES (THE STARTREK COORDINATES).

Then in (31) the occurrence of matrix subject <I> is I<1> = "T...discovered <t><{j}>" and the occurrence of its embedded copy is I<2> = "expect...discovered <t><{j}>" (actually, the syntactic objects corresponding to them).

THAT IS, OBJECTS DEFINED AS {T, {T,...}} AND {EXPECT, {EXPECT,...}}

The occurrences of <a proof> and its trace are P<1> = "to be discovered <t><{j}>" and P<2> = "discovered," respectively.

WE SHOULD EMPHASIZE THIS. "discovered" IS ACTUALLY, AFTER MOVEMENT, AN OCCURRENCE OF THE CHAIN OF a proof!

In (31) two chains are defined: C<I> = <I<1>, I<2>> and C<P> = <P<1>, P<2>>.

THIS IS CLASSICAL NOTATION, NOW SUBSUMED UNDER THE MORE INTERESTING IDEA ABOVE WHICH DOESN'T USE INDICES.

In informal description C<I> is the chain <<I>, <t><{i}>> and C<P> the chain <<a proof>, <t><{j}>>, where <I> and <a proof> are called the "heads" of the respective chains.

IT IS WORTH ASKING WHETHER THIS TRADITIONAL NAME MAKES ANY SENSE. IN PF IT DOES, TO THE EXTENT THAT THE HEAD OF THE CHAIN IS WHAT GETS LEXICALIZED. IS IT A DEEP FACT ABOUT CHAIN CONSTRUCTION? PERHAPS HEADS OF CHAINS LEXICALIZE IN THE SAME WAY THAT HEADS OF PROJECTIONS (LEXICAL ITEMS) DO. PERHAPS CHAINS ARE, IN SOME SENSE, SUPER-PROJECTIONS WHICH TAKE AS OPERATIONAL TERMS NOT THE CONSTITUENTS OF MERGE, BUT RATHER MORE ELABORATE CONSTRUCTS THAT, AS IT WERE, UNDERGO A KIND OF SUPER-MERGE. THAT IS, NOTE THAT THE HEAD OF A CHAIN (E.G. a proof) SERVES AS A KIND OF LABEL TO RELATE TWO DISTANT (NON SISTER) TERMS, E.G. "discovered" AND "to be discovered t". LIKE MERGE, THE RELEVANT RELATION IS ASYMMETRIC, NOT ONLY IN THAT AT MOST ONE 'LINK' OF THE CHAIN FORCES THE PF COLLAPSE IN THAT POSITION, BUT ALSO IN THE UNSPOKEN ASSUMPTION THAT INTERPRETATION OF THE CHAIN-HEAD PARTS IS IN A SINGLE PLACE FOR LF REASONS, EVEN IF SCATTERED (AS IN OPERATOR-VARIABLE CONDITIONS). I WILL NOT PURSUE ANY OF THIS YET (SEE THE END OF CHAPTER FIVE OF RHYME AND REASON--HENCEFORTH R&R--IF YOU CARE), BUT I WANT TO NOTE THAT IT IS CONSISTENT WITH THE FORMALISM AND THE GENERAL OUTLINE SO FAR.

In (31), occurrences are properly identified if taken to be sisters, but that might not always be the case.

{Suppose, for example, that the operation Move could raise the object of V to become the object of V', forming [V' OB]. Then if V = V' (by virtue of V-raising), the sisters of OB would be identical.}

THIS EXAMPLE IS NOT COMPLETELY STRAIGHTFORWARD, FOR EVEN WHEN V RAISES TO V' IT IS NOT OBVIOUS THAT, THEN, V = V'--BUT THE MATTER OF PRINCIPLE REMAINS.

The simplified definition in terms of sisterhood is based on the assumption that other properties of C<HL> guarantee that no problems arise. I will assume that to be true.

We can simplify chains from sequences to sets, relying on the fact that a "higher" occurrence of _ properly contains lower ones. Thus in C<I> of (31), I<1> properly contains I<2> (<I> c-commands its trace).

THE DEFINITION IN CHAPTER 4 RELIED ON ORDERED PAIR NOTATION. AS IT TURNS OUT, THAT IS GENERALLY NOT NEEDED, FOR PRECISELY THE REASON CHOMSKY JUST MENTIONS (IT IS GENERALLY POSSIBLE TO DISTINGUISH THE TWO CHAIN LINKS WITHOUT MENTIONING LINEAR ORDER). THAT'S NICE, FOR OTHERWISE ONE WOULD HAVE TO MOTIVATE WHY THE LINKS ARE ORDERED, HOW THAT'S CAPTURED, AND SO ON--WHEREAS THE SIMPLEST NOTATION

MAKES FEWER ASSUMPTIONS.

Chains formed by successive-cyclic movement fall into place directly.

THAT IS, ASSUMING THEY ARE REAL, WHICH IS NOT COMPLETELY OBVIOUS IN THE CASE OF A-MOVEMENT (PARTICULARLY IF, AS CHOMSKY CLAIMS, THERE IS NO RECONSTRUCTION INTO A-POSITIONS), ALTHOUGH LOTS OF EVIDENCE EXIST FOR A'-MOVEMENT.

Consider (32):

(32)
(i) a proof is likely [<_> <t> to be [discovered <t>]]

(ii) who did you say [<t> has [<t>' discovered the proof]]

In (i), each <t> = <a proof>, and the chain headed by the matrix subject is {P<1>, P<2>, P<3>}, the three occurrences of <a proof> (P<1> = "is likely _," etc.). In (ii), on the assumptions of <MP>, the external system at the LF interface requires an operator-argument analysis, with <who> "reconstructed" to an operator binding the (also "reconstructed") A-chain argument headed by <t>.

WE WILL TALK ABOUT THIS LATER ON IN THE SEMESTER, AND NOTE ALSO:

{See <MP>, chap. 3.5, and for improved versions with many consequences, Fox (1998), Sauerland (1998b). A question that might be raised is where the "reconstruction" operation takes place: within narrow syntax or on the other side of the interface, along with Binding Theory (I am assuming, following <MP>) and other interpretive systems.

WE WILL TRY TO DEAL WITH RECONSTRUCTION AND BINDING IN THESE TERMS, AS POST-LF PROCESSES.

Similar questions arise about other covert operations; see note <TYP>.}<RECON = footnote>

THIS IS A REFERENCE TO FOOTNOTE 45, ON STYLISTIC PROCESSES AND OTHER POST-CYCLE STUFF. IN A SENSE, THIS PAPER IS TAKING THE CYCLE AS A CRITERION FOR NARROW SYNTAX.

A straightforward implementation can be based on analysis into two chains: the A'-chain (<who>, <t>), and the A-chain (<t>, <t>').

WELL, LET'S SEE HOW STRAIGHTFORWARD THE IMPLEMENTATION IS. TO BEGIN WITH, WE MUST WRESTLE WITH CONSERVATION MATTERS. HOW CAN WE GET TWO CHAINS OUT OF A SINGLE OCCURRENCE OF who?

TWO POSSIBILITIES. FIRST, who IS MORE COMPLEX THAN IT LOOKS. GOING BACK

TO IDEAS BY KLIMA AND KURODA, WE MAY THINK OF THIS AND SIMILAR ELEMENTS AS INVOLVING, IN EFFECT, TWO FORMATIVES: AN OPERATOR AND A PREDICATE/VARIABLE. THIS IS MORE APPARENT IN which one, AND SIMILAR ELEMENTS. IF THAT'S THE WAY TO GO, ALTHOUGH OVERTLY WE HAVE THE SINGLE LEXICAL ITEM who, IN SOME SENSE (PERHAPS TRHOUGH LEXICAL FEATURES), WE NONETHELESS HAVE ENOUGH MATERIAL TO COME UP WITH TWO CHAINS, SINCE IN EFFECT WE'RE NOT GAINING INFORMATION, BUT SIMPLY SHIFTING IT AROUND. FOR CONCRETENESS, LET'S SAY THAT IN THE NUMERATION WE HAVE (THE OPTION OF) TWO FEATURES THAT NEED NOT BE SEEN AS PERTAINING TO A SINGLE LEXICAL ITEM, AND EVENTUALLY LF RECOVERS EACH FEATURE AS CORRESPONDING TO A SEPARATE CHAIN. NOTE, INCIDENTALLY, THAT THE CHAINS CANNOT BE TOTALLY DISTINCT, FOR AFTER ALL ONE STARTS WHERE THE OTHER ONE ENDS. WHETHER THAT'S DEEP DEPENDS ON WHAT ONE DOES WITH RESUMPTIVE PRONOUNS, WHERE CLEARLY THE TWO CHAINS ARE LEXICALLY SEPARATE (i.e. Who do you wonder whether Mary likes him?). THERE, TOO, YOU WANT THE TWO CHAINS TO BE CONNECTED, BUT THIS TIME THE DERIVATION IS NOT GOING TO HELP YOU, SO PERHAPS YOU NEED SOMETHING LIKE HIGGINBOTHAM'S ANTECEDENCE.

A SECOND POSSIBILITY IS A BIT MORE RADICAL. IT COULD BE THAT EXTRA CHAINS CAN EMERGE IN THE COURSE OF THE DERIVATION UNDER CERTAIN (HOPEFULLY LIMITED) CONDITIONS. IN THAT CASE YOU WOULDN'T TRY TO GET TWO CHAINS OUT OF TWO SEPARATE NUMERATION ITEMS, BUT RATHER YOU'D ADMIT THE POSSIBILITY THAT N ITEMS IN THE NUMERATION PRODUCE N+M CHAINS. WE WILL RETURN TO THIS ISSUE WHEN WE ANALYZE NUNES'S TREATMENT OF PARASITIC GAPS, PARTICULARLY IN THE TERMS DISCUSSED IN NUNES & URIAGEREKA (1999)--HENCEFORTH N&U.

THAT'S WITH REGARDS TO CONSERVATION, BUT THERE'S MORE ABOUT CHOMSKY'S IMPLEMENTATION:

The three occurrences of <who> are analyzed into the chains {Q<1>, Q<2>} and {R<1>, R<2>}, where Q<1> = "did you say....the proof," Q<2> = R<1> = "has...the proof," and R<2> = "discovered the proof." This is one of the permissible analyses into chains; it need only be permitted, not forced. In (i) there is a three-membered chain, in the (interpretable) analysis of (ii), there are two two-membered chains, sharing one occurrence of <who>.

THIS IS WHAT WE MUST STUDY--WHY IN SOME INSTANCES YOU GET A SINGLE CHAIN (A-MOVEMENT, PERHAPS SUCCESSIVE CYCLIC, THOUGH NOT OBVIOUS) WHEREAS IN OTHERS YOU GET AT LEAST TWO (PERHAPS AT MOST TWO) CHAINS (A'-MOVEMENT AND A-MOVEMENT). AT ISSUE, IN PART, IS WHETHER INTERMEDIATE RECONSTRUCTION SITES EXIST (FAIRLY OBVIOUS FOR A'-MOVEMENT, LESS CLEAR FOR A-MOVEMENT).

{Attempts in <MP> to account for successive-cyclic movement in terms of linked chains and other devices can be eliminated; they were based on failure to take seriously enough the actual

notion of <chain>.

I DON'T THINK I UNDERSTAND THIS, AND THE COMMENT IS TOO CRYPTIC TO ASCERTAIN. CLEARLY THE MP SYSTEM WAS RATHER CUMBERSOME, BUT IT IS NOT OBVIOUS TO ME WHAT IN THE PRECISE NOTION OF CHAIN MAKES IT SO EASY TO COLLAPSE THE CHAIN HERE OR THERE. AGAIN, WE WILL RETURN TO THIS ISSUE LATER ON.

They were also in error, in not recognizing the role of intermediate traces in interpretation and computation.

THAT'S CERTAINLY TRUE, AT LEAST FOR A'-MOVEMENT. NONETHELESS, WE'LL HAVE TO SEE WHETHER THIS ERROR IS AMENDED IN THE PRESENT SYSTEM.

The conclusions drawn there about trace invisibility no longer hold, though interesting aspects of the question remain.}

AGAIN, TOO CRYPTIC A COMMENT TO INTERPRET--SO LET'S POSTPONE IT.

A chain, then, is a set of occurrences of an object _ in a constructed syntactic object K. It would make sense to rethink all notions involving chains in these terms, including their interpretation at the interface. If we do, some apparent problems disappear. Suppose that raising of DP to SPEC-T checks and deletes its uninterpretable Case feature. We want DP and its trace <t> to be identical, so the feature must also delete in the trace. But what guarantees that the feature is deleted throughout the chain? The question does not arise if we think of a chain, more properly, as a set of occurrences of _ in K: the feature is deleted in the single element _, unproblematically.

AN ANALOGY MIGHT HELP. SUPPOSE YOU'RE TELEPORTED FROM YOUR HOME TO THE ANDROMEDA GALAXY, AND UPON ARRIVAL SOME CREATURE SNAPS OFF YOUR MEMORY FOR YOUR GRANDMOTHER'S FACE. SAY YOU GO ABOUT YOUR BUSINESS WITHOUT KNOWING THIS, AND THEN YOU'RE TELEPORTED BACK. THEN YOU SEE GRANNY AT HOME; DO YOU REMEMBER HER FACE? WELL, IF THE TRICK IS PLAYED THE WAY I SUGGESTED BEFORE, THE ANSWER WOULD BE NO, EVEN IF IT WASN'T AT HOME THAT THE MEMORY GOT SNAPPED. (INCIDENTALLY, YOU COULD HAVE USED GORIER SCENARIOS, LIKE SNAPPING AN EAR, SAY.) ANYWAY, I DON'T THINK ANY OF THIS IS AS 'UNPROBLEMATIC' AS CHOMKSY SUGGESTS, FOR IT FORCES YOU INTO A FASCINATING ONTOLOGY. (NOT THAT I HAVE PROBLEMS WITH THAT ONTOLOGY, BUT A RATHER SERIOUS SUSPENSIONOF DISBELIEF IS NECESSARY HERE.)

Should the notions of occurrence and chain be extended to proper subparts of LI that are not elements of the lexical array: features and sets of features? Let's put the question aside for the moment,

YOU WILL SEE THAT THIS ONE IS IMPORTANT IN THIS PAPER.

and turn to the relations permissible under Condition (Dii) of (30). Consider the operation Merge (indispensable in some form).

MIND YOU, FOR FACTUAL REASONS--NOT FOR OBVIOUS REASONS HAVING TO DO WITH THE STRONG THESIS. NEED FOR MERGE PRESUPPOSES EITHER THAT INTERPRETATION IS NECESSARILY FREGEAN (SAY) OR ELSE THAT A COMPUTATIONAL SYSTEM WHICH IS MORE COMPLEX THAN A MARKOVIAN SYSTEM IS FOR SOME REASON NECESSARY WHEN ONE INVESTIGAGES THIS SORT OF STRUCTURE. IN BOTANY A SIMILAR QUESTION ARISES, INCIDENTALLY, SINCE ALL VEGETABLE GROWTH PRESUPPOSES FRACTAL STRUCTURES, WHICH ARE NOT GENERABLE BY STANDARD MARKOVIAN SYSTEMS. PERHAPS WHATEVER UNDERLIES THAT (WHICH IS EXTENSIBLE, IT SEEMS, TO THE INNER PLANS OF ALL MULTICELULAR ORGANISMS) MIGHT BE RELATED TO WHATEVER IS NECESSARY FOR MERGE--AT LEAST IT IS WORTH ASKING WHAT THAT WOULD MEAN.

Merge takes two objects _ and ß and forms a new object K(_,ß). The operation provides two relations directly: <sisterhood>, which holds of (_,ß), and <immediately contain>, which holds of (K,_), (K,ß), and (K,K) (taking it to be reflexive).

THIS IS ALL TRUE, BUT NOT GIVEN. THOSE DEFINITIONS HAVE THE STATUS OF PRIMITIVES IN THE THEORY, HENCE WE MUST ASK THE USUAL QUESTIONS ABOUT THEM (WHY THOSE, ETC.).

Suppose we permit ourselves the elementary operation of composition of relations.

DITTO--NOTHING TRIVIAL, PERHAPS RELATED TO THE ISSUES MENTIONED BEFORE.

Applying it in all possible ways, we derive three new relations: (i) the transitive closure <contain> of <immediately contain>; (ii) identity (= (sister(sister)),

THIS IS IDENTITY ONLY ASSUMING THE BINARITY OF MERGE, WHICH IS ALSO MERELY ASSUMED--AND THIS ONE IS CERTAINLY NOT TRUE OF, SAY, FRACTAL SYSTEMS. KAYNE HAS A WAY OF DEDUCING BINARITY, BUT CHOMSKY SEEMS SKEPTICAL ABOUT KAYNE'S ARGUMENT ANYWAY, SO HE HAS TO STICK TO A COMPLETE STIPULATION. WITHOUT BINARITY, A SISTER OF A SISTER NEED NOT BE ITSELF (NOT THAT MUCH RELIES ON THIS, THOUGH).

and (iii) c-command (= sister(contain)).

THERE ARE A COUPLE OF TRICKS HERE. FIRST, THAT IS NOT WHAT WE GET BY APPLYING THE COMPOSITION OF ELEMENTARY RELATIONS. RATHER, WHAT WE GET IS (sister (immediately-contain)), WHICH DOESN'T HAVE THE LONG-DISTANCE CHARACTER OF COMMAND. SECOND, BY THE SAME REASONING WE GET (contain (sister)), WHICH DEFINES THE RELATION BETWEEN, SAY, A MAXIMAL PROJECTION

AND THE SISTER OF ITS HEAD. PERHAPS THAT'S THE WAY TO DEFINE THE ELEMENTS IN THE MINIMAL DOMAIN OF A GIVEN HEAD, BUT CHOMSKY DOESN'T EXPLORE THIS. I SHOULD SAY THAT, IN PART, ALL OF THIS IS AN ATTEMPT AT GETTING COMMAND AS A NATURAL RELATION WITHOUT INVOKING EPSTEIN'S DEDUCTION BASED ON KAYNE'S LCA. AS YOU'LL SEE LATER ON, THE LCA DOESN'T SQUARE WELL WITH SOME OF CHOMSKY'S EMPIRICAL ASSUMPTIONS, HENCE THE NEED TO DO THINGS WITHOUT IT. WE WILL RETURN TO CHOMSKY'S SKEPTICISM, TO SEE WHETHER WE CAN ADDRESS IT. PRESENTLY, I MUST SAY WE HAVE STIPULATED COMMAND, NOT THAT WE HAVE DERIVED IT FROM ANYTHING SERIOUS, AS IN EPSTEIN'S DEDUCTION (WHICH IS NOT WITHOUT ITS TROUBLES, BUT IS GENERALLY WELL TAKEN).

Thus K contains _ if K immediately contains _, or immediately contains L which contains _; conversely, _ is a <term> of K if K contains
_.
THIS POSES SOME PROBLEMS FOR ADJUNCTION, BUT I WILL TRY TO STAY AWAY FROM THEM. IF YOU NEED A MORE FORMAL PRESENTATION OF THESE SORTS OF ISSUES, SEE THE APPENDIX TO RHYME AND REASON BY NUNES AND THOMPSON.

And _ c-commands ß if _ is the sister of K, which contains ß.

{The compositional definition of c-command is suggested by Epstein's derivational approach (see note <EPS>): _ c-commands ß if _ is merged with K containing ß. There are empirical differences between the two approaches, to which we return.

THAT'S WHAT I MENTIONED--THEY REVOLVE AROUND THE LCA.

The derivational definition also raises some questions: in particular, why does "containment" enter? (i.e., why does X merged with Y c-command terms of Y?).

THIS IS THE DIFFICULTY I HAD IN MIND WHEN MENTIONING EPSTEIN'S SYSTEM. SEE DRURY (1999) FOR A FULLER DISCUSSION.

The matter is addressed in Epstein et al. (1998, ch. 6), but inconclusively, as far as I can see. An argument for asymmetry of c-command is also presented there; I am assuming that there is no asymmetry, its effects being derived in other ways.}<DER = footnote>

THAT IS TO SAY, THERE IS NOTHING PRIVILEGED, IN CHOMSKY'S SYSTEM, ABOUT THE C-COMMANDING ELEMENT.

The relation of c-command is available, and expected, on very weak assumptions.

I MUST SAY THIS IS NOT OBVIOUS TO ME.

The relation has played a large role in syntactic theory, though it may be that it does not function within narrow syntax but only in interpretation of the information it provides -- that is, in

mapping it to syntactic objects that belong to mental systems external to the language faculty itself (see note <TYP>). That might be expected if external systems access representations (PF and LF) to which the notions <sister> and <contain> apply.

I WOULD FIND IT SURPRISING IF EXTERNAL SYSTEMS *BOTH* USE COMMAND (AS THEY APPEAR TO), PARTICULARLY SINCE THE ASSUMPTIONS WE'VE GIVEN FOR COMMAND ARE NOT WEAK, REALLY. THAT COMPOSITION WITH THE TRANSITIVE CLOSURE OF ONE OF THE BASIC RELATIONS THAT MERGE ALLOWS IN TERMS OF THE OTHER BASIC RELATION SHOULD HAPPEN TO BE THE WAY THAT DIFFERENT INTERFACE SYSTEMS WORK IS, TO SAY THE LEAST, A REMARKABLE EMPIRICAL FINDING. EPSTEIN'S APPROACH WAS CONCEPTUALLY MORE PLEASING: THE DERIVATION ITSELF GIVES YOU COMMAND, HENCE YOU EXPECT TO SEE IT IN BOTH SIDES OF THE GRAMMAR. OF COURSE, IT'S NOT COMPLETELY CLEAR THAT THE DERIVATION *SHOULD* GIVE YOU COMMAND, SINCE OTHER COMPUTATIONAL SYSTEMS WITH ROUGHLY THE SAME POWER INVOLVED IN MERGE CLEARLY DO NOT GIVE YOU COMMAND (E.G. YOU DON'T HAVE IT BOTANY OR GENETICS). IT IS A DEEPLY SURPRISING PROPERTY OF THE MERGE SYSTEM THAT GUYS THAT MERGE SHOULD CARE ABOUT THE DESCENDENTS OF THEIR SISTERS.

FOR WHAT IT'S WORTH, HERE'S A DIFFERENT STAB AT COMMAND. WHEN YOU MERGE THINGS, THE DERIVATION ITSELF DICTATES ONE OF TWO MODES: EITHER YOU ARE WITHIN THE SAME DERIVATIONAL WORKSPACE, OR ELSE YOU NEED TO SEPARATE WORKSPACES. FOR INSTANCE, IN he saw a woman YOU ONLY NEED ONE DERIVATIONAL WORKSPACE, WHEREAS IN [the man] saw a woman YOU CLEARLY NEED TWO: ONE TO ASSEMBLE the man AND ONE TO ASSEMBLE saw a woman, TO WHICH YOU MERGE the man IN DUE TIME. IT IS A SIMPLE FACT ABOUT DERIVATIONS THAT ELEMENTS THAT ARE ACTIVE WITHIN THE SAME DERIVATIONAL WORKSPACE STAND IN A COMMAND RELATION. IF THIS WERE THE WAY TO GO, WE WOULDN'T BE MAKING COMMITMENTS TO ANYTHING HAVING TO DO WITH LCA (ALTHOUGH THERE SURELY WOULD BE CONSEQUENCES FOR THE LCA IF THIS WERE TO BE ASSUMED), AND IT WOULDN'T BE SUCH A BIG SURPRISE THAT PF AND LF SHOULD CARE ABOUT COMMAND UNITS, SINCE THESE WOULD BE CARVED UP BY THE DERIVATION ITSELF. IN ESSENCE, THAT'S THE MSO SYSTEM IN THE NARROW SENSE.

The sisterhood relation is significant primarily (perhaps only) for heads, that is, LIs and M(odified)LIs formed from them. Furthermore, sisterhood relations presumably remain if LI is modified to MLI: if VP is sister of T, for example, it should remain so even if uninterpretable features are deleted from T. More generally, LI and its modifications are not distinguished with regard to the fundamental relations defined in terms of Merge.

BASICALLY THIS TREATS ADJUNCTION TO HEADS AS HAPPENING ON A DIFFERENT PLANE OF ITS OWN, SINCE IT DOESN'T AFFECT ANYTHING IN THE PHRASE-MARKER. (UNLESS CHOMSKY ISN'T TALKING ABOUT HEAD ADJUNCTION AT ALL, GIVEN HIS NEW NOTION OF 'AGREE'; THAT IS, IN MP, THE WAY TO CHECK

A FEATURE IN A HEAD H WAS TO ADJOIN ITS CHECKER TO H, BUT NOW WE WILL DO THE TRICK LONG-DISTANCE, DIRECTLY ELIMINATING THE RELEVANT FEATURE WITHIN H; IF SO, MLI MIGHT JUST BE THE MODIFIED HEAD, WITH NO ADJUNCTS--THEN AGAIN ONE HAS TO WORRY ABOUT SPELLING OUT THE DETAILS OF WHAT LOOKED LIKE GENUINE ADJUNCTION, E.G. ON HEAD MOVEMENT, WHICH WE RETURN TO BELOW.)

For _ an LI, then, we extend the relations defined for (_,ß) to (MLI(_),ß), MLI constructed from _ (typically -- perhaps only -- by deletion of uninterpretable features). We extend the same convention to a feature F(_) of a head _. Thus if LI _ is the sister of ß or c-commands ß, then MLI(_) and F(_) do as well.

I AM NOT HAPPY ABOUT SIMPLY 'EXTENDING CONVENTIONS', IF THESE WILL HAVE EMPIRICAL CONSEQUENCE--THE MINIMALIST MUST JUSTIFY THE EXTENSION. IN ANY CASE, IT SEEMS AS IF THIS IS REALLY SAYING THAT CONFIGURATIONAL RELATIONS ESTABLISHED UPON MERGE ARE KEPT EVEN AFTER THE TRANSFORMATIONAL COMPONENT OPERATES. IF SO, THIS IS INTERESTING, FOR THE OBVIOUS QUESTION IS WHY SHOULD MERGE RELATIONS PERSIST THAT WAY? COULD IT BE THAT THIS IS ANOTHER CONSERVATION? IF SO, NOT ONLY WOULD WE HAVE THE LEXICAL CONSERVATION LAW THAT WE SAW BEFORE, BUT ALSO A PHRASAL CONSERVATION LAW. IN TURN, THIS WOULD ENTAIL THAT PHRASES DEFINE THEIR OWN SEPARATE NATURAL CLASS, JUST AS WORDS DO. EMPIRICALLY THIS SEEMS SOUND, AND IT SHOULD BE POINTED OUT THAT IT HAS THE EFFECT OF CREATING A WORD-LEVEL COMPONENT (WHERE LEXICAL CONSERVATION HOLDS AS AN INPUT-OUTPUT CONDITION ON DERIVATIONS) AND A PHRASAL-LEVEL COMPONENT (WHERE PHRASAL CONSERVATION AGAIN HOLDS AS A FURTHER INPUT-OUTPUT CONDITION ON DERIVATIONS). THE LATTER, OF COURSE, IS VERY REMINISCENT OF THE TRADTIONAL MAPPING BETWEEN D-STRUCTURE AND LF, BUT THAT ONE WAS DONE IN TERMS OF LEVELS OF REPRESENTATION (UNIFIED OBJECTS), WHEREAS THE PRESENT CONDITION IS ONE THAT HOLDS OF CHUNKS OF DERIVATIONS, NOT UNIFIED LEVELS. I FIND THIS IMPORTANT IN ESTABLISHING THE CLAIM THAT UNDERLIES MUCH OF MY COMMENTARY TO THIS PAPER: THAT WHEN YOU LOOK CAREFULLY, DISECTING THE ASSUMPTIONS, CHOMSKY IS MAKING THE SUBSTANTIVE CLAIM THAT SOMETHING LIKE A D-STRUCTURE COMPONENT (CLEARLY, NOT A LEVEL) HOLDS IN THE SYSTEM. AT THIS POINT, THE ISSUE IS PRESERVING THE PHRASAL INTEGRITY OF THE SYSTEM. IT SHOULD BE CLEAR, I HOPE, THAT A TRANSFORMATIONAL SYSTEM COULD OCCUR WHICH LITERALLY DESTROYS PHRASAL INTEGRITY. INDEED, NATURAL SYSTEMS OUT THERE PROBABLY EXIST WITH THOSE PROPERTIES, WITH RADICAL TRANSFORMATIONS DESTROYING, IN THE PROCESS OF APPLYING, WHAT WENT INTO THEM. BUT LINGUISTIC TRANSFORMATIONS, AT LEAST CORE ONES, ARE STRUCTURE PRESERVING--PERHAPS THE MAJOR RESULT OF JOE EMMONDS'S WORK.

Questions have arisen about the interpretation of these notions for adjunction, particularly

head-adjunction of H' to H.

THAT'S WHAT I WAS MENTIONING ABOVE--SO CHOMSKY DOES AT LEAST ACKNOWLEDGE THE ISSUE.

With no further elaboration, H and H' would be sisters and neither would c-command outside.

THAT'S THE CASE IN THE TRADITIONAL SYSTEM.

Whether this matters is unclear. Failure of H' to c-command its trace seems to have no significance in the present framework;

THAT'S THE CASE, IN LARGE PART, BECAUSE THE PRESENT FRAMEWORK HAS BASICALLY NOTHING TO SAY ABOUT INSTANCES WHERE C-COMMANDING A TRACE SEEMED TO BE IMPORTANT. WE WILL RETURN TO SOME OF THESE.

failure of H to c-command into its former c-command domain would be problematic only if H functions after adjunction in implementing agreement and movement, but that does not seem necessary.

THAT'S AN INTERESTING QUESTION. TAKE INFL AFTER A VERB HAS MOVED TO IT. DOES INFL (NOT INFL+VERB) FUNCTION IN ANY RELATION OF GRAMMAR? PERHAPS CLITICIZATION IN ROMANCE IS OF THIS SORT, SINCE CLITICS SEEM TO GO TO AN INFL-LIKE ELEMENT REGARDLESS OF WHETHER V HAS MOVED. THE CASE IS TOO MURKY TO GO INTO NOW, BUT I'LL RETURN TO EXAMPLES OF THIS SORT.

Pending some good reason to sharpen the sisterhood relation for this case, I will leave it as is.

{Sisterhood relations would be carried over under head-adjunction if the result were taken to be an extended MLI.

WHICH IS THE OTHER ISSUE I WAS RAISING ABOVE. PERHAPS SOME INSTANCES ARE OF THIS SORT (WHAT SOME OF US CALLED 'MORPHOLOGICAL INCORPORATIONS') WHEREAS OTHERS ARE NOT ('FUNCTIONAL INCORPORATIONS')

Many questions dissolve if head-adjunction is part of the phonological component. There are, I think, independent reasons to suspect that this may be true, at least over an interesting range, but I will defer the question.}<HADJ = footnote>

THIS IS WHAT I MENTIONED ABOUT V-2, ALTHOUGH IT IS MUCH LESS CLEAR FOR, SAY, V TO I.

Though varieties of government would be "imperfections," to be avoided if possible, the closer-to-primitive notion of L-marking should pass muster, hence also notions of barrier that are

based on nothing more than L-marking.

WELL, THIS IS A BIT FAST, PARTICULARLY IF YOU RECALL THE DEFINITIONS OF L-MARKING OF TEN YEARS AGO. R&U HAD ONE DEFINITION THAT MIGHT INDEED (WITH SOME MODIFICATIONS TO UPDATE IT) HAVE THE RELEVANT EFFECTS, BUT I WON'T GO INTO THIS NOW.

Here numerous questions arise about island conditions within a minimalist framework, about which I have nothing useful to say.{See Kitihara (1998), Lasnik (1995a,b), Uriagereka (forthcoming a).}<URIA = footnote>

WE WILL TRY TO RETURN TO THIS, SINCE THE HIGHLY DERIVATIONAL SYSTEM IS A RATHER NATURAL CANDIDATE FOR EXPLAINING EXTRACTION DOMAINS.

<5. Imperfections>

There are some respects in which the strong thesis seems untenable, and we find what appear to be "design flaws" that are not necessary for language-like systems. The most obvious involve the phonological component, which takes syntactic objects constructed by the computational operations C<HL> and converts them to representations at the PF interface. Here there are radical violations of the interpretability and inclusiveness conditions ((B) and (C) of (30)). Condition (C) is violated by operations that introduce such new elements as prosodic structure and narrow phonetics. Condition (B) is violated by the discrepancy between the phonological properties of lexical items LI ("morphophonemes," "phonological units," etc., within various frameworks and terminologies) and the narrow phonetic instantiations of combinations of such elements. It may be that phonological features of LIs do not even appear at the level PF, that the "input" and "output" of the phonological component are in different "vocabularies." In that case the interpretability condition would be maximally violated by the operations of the phonological component, while the inclusiveness condition is clearly inoperative.

IN OTHER WORDS, THIS COMPONENT WOULD NOT BE SUBJECT TO A STRONG CONSERVATION LAW OR THE GENERAL DESIGN INTUITION THAT FL HAS PROPERTIES THAT ARISE ONLY AT *MERE* INTERFACES--ALTERNATIVELY:

We therefore turn to the alternatives of (29). Option (ii) seems implausible; the properties appear to be real, whatever their proper expression. Of the two remaining possibilities, the most interesting by far is (iii): the properties are real, but not imperfections: the super-engineer, called upon to map independently-motivated syntactic objects to PF, would hit upon the phonological component as an optimal solution. I have no idea whether this can be formulated as a sensible research task.

DITTO...

As for option (i), it might be argued that departures from "good design" are not surprising in this

domain. Direct evidence about sound systems is available for language acquisition, permitting a degree of complexity and variation. And the subsystem reflects special properties of the sensorimotor systems, which are in a certain sense "extraneous" to language, relating to externalization by systems with non-linguistic properties, and capable of much variation while FL remains fixed, as in sign languages. Symbolic systems designed for special purposes (metamathematics, computers, etc.) dispense entirely with a phonological component, not facing the need to meet the legibility conditions for human language at the sensorimotor interface.

IT IS WORTH POINTING OUT, PERHAPS, THAT SYMBOLIC SYSTEMS ARE TYPICALLY EXPRESSED IN WRITING, WHICH IMPOSES SOME LIMITATIONS WHICH ARE COMPLETELY EXTRANEOUS TO THE SYSTEM. FOR INSTANCE, YOU NEED SUFFICIENTLY DISTINCT SYMBOLIC REPRESENTATIONS, VISUALLY SPEAKING, SO THAT YOU DON'T CONFUSE "PLUS" OR "MINUS" AND SO FORTH, AND THESE ARE COMPLETELY ARBITRARY AND PRESENT OBVIOUS VARIATION ACROSS CODING SYSTEMS. THERE IS A SENSE IN WHICH THOSE PROPERTIES OF THE SYSTEM ARE COMPLETELY IRRELEVANT TO ITS INTERNAL WORKINGS, AND MUCH AS I HATE TO SAY THIS CLEARLY, IT COULD WELL BE THAT SO TOO ARE PHONETIC AND EVEN SOME PHONOLOGICAL PROPERTIES OF THE LANGUAGE FACULTY; I DON'T MEAN THIS IN A DIMINING WAY, THOUGH, IN TWO SENSES. FIRST OF ALL, THE SYSTEM IS WHATEVER IT IS, AND IF IT HAS PROPERTIES THAT ARE OUTSIDE WHAT WE CALL "FL" IN A TECHNICAL SENSE, WELL--SO BE IT. BUT MOST IMPORTANTLY FROM A SYNTACTIC POINT OF VIEW, THERE PROBABLY ARE SOME PROPERTIES OF THE PHONOLOGICAL SYSTEMS THAT AFFECT SYNTACTIC COMPUTATION. FOR INSTANCE, AS WE WILL SEE AT LENGHT LATER ON IN THE COURSE, LINEARIZATION REQUIREMENTS (HIGGINBOTHAM'S 1983 POINT) AND LIMITATIONS IMPOSED BY THE MORPHOLOGICAL SYSTEM (IN MARANTZ'S OR EVEN MORE RADICALLY IN ANDERSON'S SENSE). TO GO ON WITH THE ANALOGY WITH SYMBOLIC SYSTEMS, SUCH LIMITATIONS EXIST THERE AS WELL, FOR INSTANCE THE CONVENTION IN FORMAL LANGUAGES THAT ELEMENTS THAT ENTER INTO A RELATION ARE WRITTEN CONTIGUOUSLY, OR WITH A MARKER FOR THE RELATION IN BETWEEN THEM--NOT LONG DISTANCE. IT'S NOT AS IF, MATHEMATICALLY, YOU COULDN'T EXPRESS A MORE LONG-DISTANCE RELATION (FOR INSTANCE IN A CONVOLUTED GRAPH, OR A QUANTUM ONE--AND GET READY FOR THAT IN QUANTUM COMPUTATION!) BUT NO FORMAL SYSTEM WORKS LIKE THAT (I DON'T CLAIM TO KNOW WHY, ALTHOUGH THIS IS PROBABLY A REFLEX OF THE WRITTEN FORMAT, PERHAPS PIGGY-BACKING ON VISUAL AND EVEN PSEUDO-LINGUISTIC DEMANDS). MY POINT IS: SOME ASPECTS OF THE "PUBLIC SUPPORT" (IN THE SENSE OF WHAT MAKES THE SYSTEM OF KNOWLEDGE COMMUNICABLE) COULD WELL LIMIT THE FORMAT OF THE SYSTEM. NOW THAT'S INTERESTING. CURIOUSLY, THOUGH, IT IS MUCH LESS CLEAR, WITH REGARDS TO THOSE, SHALL WE CALL THEM, "DEEP" PROPERTIES OF THE PHONOLOGICAL SYSTEM THAT THEY DO VIOLATE INCLUSIVENESS AND INTERPRETABILITY. IN ESSENCE, THAT'S THE QUESTION OF "FAITHFULNESS" IN OT, ON THE ONE HAND, AND AGAIN IN OT, OF THE NATURE OF PHONOLOGICAL CONSTRAINTS. ABOUT THE LATTER, LITTLE IS KNOWN, SO IT'S HARD TO SPECULATE. BUT THE ISSUE WOULD BE WHETHER ANY OF THE

"DEEP" (IN THE SENSE OF FL-RELATED) CONDITIONS OF THE PHONOLOGICAL COMPONENT ARE OR ARE NOT THERE FOR INTERPRETIVE REASONS. IN ANY CASE, ANSWERING THAT DOESN'T SEEM MORE OR LESS CLEAR THAN ANSWERING A SIMILAR QUESTION, SAY ABOUT CHAINS, IN THE LF SIDE.

FOR INSTANCE, STRESS ASSIGNMENT, WHICH WE KNOW IS ALTERED BY SYNTACTIC CONSIDERATIONS. IS THAT PHENOMENON THERE FOR INTERPRETIVE REASONS? PERHAPS, IT DEPENDS ON WHETHER YOU BELIEVE THE OLD IDEA THAT STRESS AIDS OR EVEN DETERMINES WORD IDENTIFICATION. SUPPOSE IT DOES; THEN YOU CAN THINK OF IT AS A WAY TO MATCH PF OUTPUT TO THE LEXICAL ARRAY YOU START WITH. THAT, NOTE, WOULD INDICATE THAT SUCH A MAPPING SATISFIES SOME FORM OF INCLUSIVENESS, THE OTHER ISSUE THAT CONCERNS US. AND HOW ABOUT STRESS ALTERATIONS, IMPOSED BY RHYTHMIC CONSIDERATIONS? AGAIN, IT DEPENDS ON WHAT RHYTHM IS DOING IN THE SYSTEM, AND HOW MUCH THOSE ALTERATIONS AFFECT *PRIMARY* STRESS, WHICH SEEMS MUCH MORE CONSERVATIVE THAN SECONDARY (AND TERTIARY, ETC.) STRESS. PERHAPS RHYTHM IS A PURE INTERPRETIVE IMPOSITION OF THE PF COMPONENT, RELATED TO BREATHING, DISCERNABILITY, AND/OR WHAT NOT (I DON'T THINK THESE QUESTIONS HAVE BEEN ASKED IN THIS WAY, SO I DON'T REALLY KNOW). IF SO, IT MAY PERHAPS BE LEGITIMATE FOR RHYTHM TO ALTER STRESS ASSIGNMENT, ALTHOUGH THEN PERHAPS ONLY PRIMARY STRESS OBEYS THE CONSERVATION LAW (AND INCIDENTALLY, ONLY PRIMARY STRESS WOULD SEEM TO RELATE TO TOKENS IN THE NUMERATION; SECONDARY ONE IS CLEARLY A PURELY RHYTHMIC DEVICE, SO PERHAPS THE ISSUE OF CONSERVING IT SIMPLY DOESN'T ARISE). MY POINT: DEEP PHONOLOGICAL PROPERTIES MIGHT WELL BE MUCH MORE ELEGANT THAN IT SEEMS.

OTHER PHONOLOGICAL PROPERTIES ARE PERHAPS "INELEGANT", IN THE TECHNICAL SENSE, BUT SO WHAT? "INELEGANCE" IS WELCOME IN AN UNDERSPECIFIED SYSTEM--INDEED, IT IS OFTEN WHAT MAKES IT VIABLE IN OUR OTHERWISE TOO REGULAR UNIVERSE. WITHOUT BUMPS IN THE EXPANSION OF THE UNIVERSE EVERYTHING WOULD BE HARMONICALLY NOT HERE!

The strongest sustainable inclusiveness/interpretability requirement, then, is (33):

> (33) Inclusiveness holds of narrow syntax, and each feature is interpreted at the level LF or associated with phonetic features by the phonological component

PERSONALLY, I FIND THIS A BIT TOO WEAK. OF COURSE, SINCE CHOMSKY WILL BE GIVING UP THE LCA (WHICH FALLS INTO WHAT I WAS SAYING ABOVE), AND DOESN'T SEEM TO CARE TOO MUCH ABOUT MORPHOLOGICAL DETAIL, (33) IS EXACTLY WHAT HE WANTS. BUT WE'LL BE SEEING, FOR INSTANCE, CLITICIZATION EFFECTS OF THE SORT RAPOSO HAS BEEN RECENTLY STUDYING, MUCH IN THE SPIRIT OF WAKERNAGEL REVISITED BY ANDERSON. THAT SYSTEM, FOR INSTANCE, SEEMS EXTREMELY ELEGANT AND WELL BEHAVED IN ITS CORE

ASPECTS, AND (33) WOULD RENDER IT IRRELEVANT.

The phonological component is generally assumed to be isolated in even stronger respects: there are <true> phonological features that are visible only to the phonological component and form a separate subsystem of FL, with its own special properties. Assume this to be true.

SURE, BUT THOSE PROPERTIES MIGHT BE WHAT I WAS CALLING SURFACE ONES. STRESS, INTONATION PHRASING, MORPHOLOGICAL UNIFICATIONS, AND OTHERS, DON'T SEEM TO BE SO ISOLATED.

Then in the course of construction of LF, an operation Spell-Out delivers the structure already formed to the phonological component, which converts it to PF.

RIGHT, WHICH IN AND OF ITSELF MEANS NOTHING, REALLY. IT IS ONLY A MATTER OF PERSPECTIVE TO TAKE SPELL-OUT AS SHIPPING STUFF OUT OF THE NARROW SYNTAX PATH. WE COULD ALSO THINK OF SPELL-OUT AS SHIPPING STUFF OUT OF THE SURFACE PATH, INTO THE LF BOX, OR (MORE PLAUSIBLY, GIVEN THE FACTS) AS SPELL-OUT LITERALLY *SPLITTING* A UNIFIED PATH IN TWO. I MEAN, THE FACT THAT THERE ARE AFTER-LF PROPERTIES, AND THUS ISOLATED ONES (SAY, CONDITIONS ON CONTEXT CONFINEMENT, TO BE CONCRETE) DOESN'T IN ITSELF ENTAIL THAT THE LF COMPONENT IS ISOLATED; MAYBE IT IS (WE THINK NOT), MAYBE IT ISN'T (WE THINK SO). ASSUMPTIONS HERE ARE OF LITTLE HELP, PARTICULARLY SINCE WE'RE MODIFYING THEM AS WE GO ALONG; THUS, FOR INSTANCE, MATTERS OF ANTECEDENCE WERE ONCE TAKEN TO BE PURELY SYNTACTIC, ALTHOUGH NOW THEY SEEM MORE POST-SYNTACTIC--WHY? BECAUSE ASSUMING THEY ARE SYNTACTIC MAKES OUR LIFE HARD FOR THE CONSERVATION AND INTERPRETABILITY ASSUMPTIONS. WITH THE SAME KIND OF REASONING WE COULD TECHNICALLY DECIDE THAT WHATEVER IS CONSERVATIVE AND OBEYS INTERPRETABILITY IN THE PF SIDE IS COMPLETELY WITHIN THE SYSTEM. IT'S EITHER THAT OR ELSE ABANDONING THE HOPE OF ACCOUNTING FOR, SAY, CLITIC PLACEMENT IN MINIMALIST TERMS.

If lexical items LI express Saussurean arbitrariness in the conventional way, then Spell-Out "strips away" the true phonological features, so that the derivation can converge at LF; it will crash if later operations introduce LIs with phonological features.

THAT'S ALL WELL, BUT EQUALLY LOADED. NOTE ONE MORE THING, INCIDENTALLY. IF YOU DON'T SAY SOMETHING ABOUT CONSERVATION IN THE PF SIDE, THEN IT'S WITTGENSTEINIAN BEDLAM ALLOVER AGAIN. THAT IS, NOTHING SHOULD PREVENT, IN PRINCIPLE, A SYSTEM THAT ALLOWS YOU TO START IN A NUMERATION WITH TEN ITEMS AND BY THE END OF THE DAY YOU PRONOUNCE THAT "ba". THE LINE I WAS SKETCHING ABOVE WAS VERY DIFFERENT. PRIMARY STRESS, FOR INSTANCE, IS IN THE SYSTEM TO ALLOW YOU TO IDENTIFY NUMERATION TOKENS (ASSUMING A SUBSTANTIVE NUMERATION). SIMILAR STORIES COULD BE TOLD ABOUT PHONOLOGICAL PHRASING, PERHAPS

A REFLEX OF NUMERATION-PHASES (AND AS SUCH AGAIN A WAY OF CODING THE INPUT-OUTPUT MAPPING BETWEEN THE NUMERATION AND THE PART OF THE SIGNAL YOU ACTUALLY GET AT PF). NEEDLESS TO SAY, WE KNOW THERE ARE LOTS OF READJUSTMENTS--INDEED, WE HOPE THAT THESE TO SOME EXTENT GEAR CLITICIZATION, IN ANDERSON'S SPIRIT. BUT THOSE READJUSTMENTS ARE PERFECTLY CONSERVATIVE ASWELL, HAPPENING AT THE EDGES AND THROUGH SIMILAR RECOVERABLE DEVICES. IN A WORD, IT SEEMS AS IF THE DEEP PHONOLOGICAL SYSTEM IS WELL DESIGNED TO ALLOW YOU TO GRASP NOT JUST NUMERATION TOKENS, BUT EVEN NUMERATION CHUNKS THAT ARE RELEVANT TO SYNTACTIC COMPUTATION (IN A SENSE TO BE DISCUSSED BELOW IN THE PAPER). AND HOW COULD IT BE OTHERWISE, FROM THE PERSPECTIVE OF ACQUISITION? CHOMSKY WILL INSIST ON VARIOUS PLACES ON THE IDEA THAT LINGUISTIC PROPERTIES ARE NOT IMPOSED BY LEARNABILITY (OR OTHER) CONSIDERATIONS. BUT IF THIS IS SO, THEN, THE PROPERTY I'M ALLUDING TO HAS TO BE A DESIGN PROPERTY OF THE MAPPING TO THE PF COMPONENT. AT LEAST, I DON'T SEE ANY OTHER ALTERNATIVE (OF COURSE, YOU COULD SHRUG YOUR SHOULDERS AND SAY THAT THE FACT THAT STRESS COINCIDES WITH NUMERATION TOKENS IS A MERE COINCIDENCE; BUT THEN AGAIN YOU COULD SHRUG YOUR SHOULDERS ABOUT LESS OBVIOUS THINGS: CHAINS, FOR INSTANCE).

On the assumptions of Distributed Morphology, the phonological features are introduced after Spell-Out by phonological operations applying to LIs lacking them.

THAT'S VERY PLAUSIBLE, AND INDEED PERHAPS THE WAY TO SEPARATE DEEP PROPERTIES OF PF FROM SHALLOW ONES. FOR INSTANCE, I'M PERFECTLY HAPPY WITH SAYING SOMETHING LIKE THIS: THE CONSERVATION OF NUMERATION-TO-PF INFORMATION DEMANDS INTONATION STRESS AND INTONATION PHRASING CORRESPONDING TO PHASES, BUT NEITHER OF THESE IS "ASSIGNED" TO WORDS. RATHER, YOU STRESS AN ABSTRACT WORD SHELL AND BRACKET AN ABSTRACT PHASE. LATER ON, DISTRIBUTED MORPHOLOGY DOES ITS REAL JOB, ASSIGNING ABSTRACT STRESS WHERE IT CORRESPONDS, READJUSTING FOR RHYTHM WITHIN A PHASE, AND SO ON (POSSIBLY MORE RADICALLY IN THE CASE OF EDGE-ORIENTED "STYLISTIC" DISPLACEMENTS).

I will assume some instantiation of this array of options to be correct.

Narrow syntax

BOTTOM LINE: THIS IS WHAT CHOMSKY IS INTERESTED IN HERE

also involves devices that are imperfections unless shown to be unreal (option (ii) of (29), which again seems implausible) or to be motivated by design specifications (option (iii) of (29), the most interesting possibility). Consider two striking examples:

(34)

(I) Uninterpretable features of lexical items
(II) The "dislocation" property

Under (I), we find features that receive no interpretation at LF and need receive none at PF, hence violating any reasonable version of the interpretability condition (B). The example that has played the most important role ever since Jean-Roger Vergnaud's famous unpublished letter twenty years ago is structural Case. The picture is more complex for agreement features: semantically interpretable for nouns, but not for verbs/adjectives, and phonetically optional throughout.

{There are important distinctions among these features, which I will largely ignore here. Problems of interpretation are also not trivial, as in:

(i) animal languages (is, are) their main research interest

(ii) three books (is, are) too much to read in a week

(iii) we expected animal languages to be their main research interest

(iv) we found three books too much to read in a week

(v) animal languages raise(s) serious issues, seem(s) to be their main research interest)

In (i),(ii), the difference in meaning appears superficially to lie in the verbal inflection,

THAT IS, IN (i) FOR INSTANCE, THE PLURAL READING INVOKES REFERENCE TO VARIOUS LANGUAGES, WHEREAS THE SINGULAR ONE INVOKES REFERENCE TO A TOPIC.

but it carries over to forms lacking that inflection, as in (iii),(iv). And though agreement properties of the copula (with surface subject or complement) are a factor, the matter is more convoluted ((iv),(v)).

IT IS, ALTHOUGH A TREATMENT IS POSSIBLE WHERE THE COMPLEXITY OF THE SUBJECT IS WHAT TRIGGERS THE AGREEMENT WHEN AGREEMENT OBTAINS. WE WILL SEE A SYSTEM OF THIS SORT IN CASTILLO'S WORK, TO BE DISCUSSED LATER ON IN THE SEMESTER. IN ANY CASE, CHOMSKY'S IS A GENERAL POINT, WHICH ALL OTHER THINGS BEING EQUAL CERTAINLY HOLDS (THAT IS, DO WE EVER HAVE NON-INTERPRETABLE AGREEMENT FEATURES, AND IF SO WHAT DOES THAT MEAN?)

See Reid (1991) for discussion from a "functionalist" perspective.}

External manifestation of inflectional features appears to be the locus of much of the variety of languages, a topic that has gained prominence within the P&P framework.

THIS, OF COURSE, IS CRUCIAL, AND IN A SENSE A THIRD IMPERFECTION TO BE ADDED TO (34), PARTICULARLY IF, AS SEEMS PLAUSIBLE, THIS IS THE ONLY SOURCE OF CORE PARAMETRIC VARIATION. THE QUESTION THEN IS WHETHER ALL THE PROPERTIES IN (34), INCLUDING VARIATION--ALL OF THEM NON-OPTIMAL--COULD BE ACCOUNTED FOR IN SIMILAR OR IDENTICAL TERMS.

These observations presuppose that occurrences of features can be distinguished, a question raised earlier but put off: occurrences of agreement features are distinguished by category, some interpretable, some not. That falls well short of truly identifying occurrences. To do so would be necessary if feature chains exist.

THIS IS AN IMPORTANT ISSUE, AND WILL INTRODUCE AN CHANGE FROM CHAPTER 4. THE QUESTION IS AS SIMPLE AS IT IS DEEP. YOU HAVE SOME FEATURE F MANIFESTING ITSELF IN SOME CATEGORY K, AND A DIFFERENT FEATURE F' MANIFESTING ITSELF IN K'. ARE THEY OCCURRENCES OF THE SAME NUMERATION TOKEN, OR ARE THEY DIFFERENT TOKENS, OR A DIFFERENT POSSIBILITY EVEN, ARE THEY PROPERTIES THAT MANIFEST THEMSELVES IN CATEGORIES, EACH A DIFFERENT NUMERATION TOKEN? (INCIDENTALLY, YOU COULD ASK SIMILAR TOUGH QUESTIONS ABOUT CATEGORIES THEMSELVES, BUT THERE AT LEAST YOU HAVE THE PF COMPONENT--PERHAPS--TO MAP OCCURRENCES TO TOKENS; THERE ARE SOME COMPLICATIONS HERE AS WELL, BUT UNLESS SOMEBODY POINTS THEM OUT, I'LL SIDESTEP THEM FOR NOW). WHERE THE QUESTION MATTERS HERE IS IN TERMS OF WHETHER THERE ARE TRUE FEATURE CHAINS, A CENTRAL PART OF CHAPTER 4. OF COURSE, THE FACT THAT YOU POSSIBLY PRONOUNCE SOME FEATURES IN MORE THAN ONE PLACE ALREADY RAISES A WARNING FLAG, FOR AFTER ALL STANDARD CHAINS INVOLVE PRONOUNCIATION IN A *SINGLE* SPOT (I'M THINKING, FOR INSTANCE, OF SO CALLED 'PARTIAL' WH-MOVEMENT).

THAT WORRY, THOUGH, IS NOT TOTALLY CONCLUSIVE, IF YOU BELIEVE NUNES'S LINE THAT THE REASON WHY YOU PRONOUNCE ONLY THE HEAD OF A CHAIN HAS TO DO WITH THE FACT THAT DIFFERENT COPIES (OCCURRENCES) OF THE SAME ITEM COULD NOT BE LINEARIZED, SINCE THEY WOULD COMMAND AND BE COMMANDED BY THE SAME ITEMS, THUS NOT YIELDING A LINEAR ORDERING IN KAYNE'S TERMS--SO YOU HAVE TO DELETE EVERY OCCURRENCE BUT ONE. (CHOMSKY WILL CHALLENGE THE LCA, SO HE WON'T BE MOVED BY THIS, BUT LET'S PUT THAT ASIDE NOW.) HOWEVER, NUNES ARGUES THAT WHICHEVER COPY HAPPENS TO MANAGE ITS WAY INTO THE MORPHOLOGICAL DOMAIN OF SOME HEAD X WILL NOT FACE ANY LINEARIZATION PROBLEMS, ASSUMING THAT WORD-LEVEL (MORPHOLOGICAL) RELATIONS ARE NOT DETERMINED BY THE LCA. IF YOU FOLLOW ME ALL THE WAY HERE, THEN YOU SHOULD BE ABLE TO ACCEPT THAT PRECISELY FEATURE CHAINS COULD HAVE SEVERAL PLACES OF PRONOUNCIATION. BUT THERE ARE TWO CRUCIAL

ASPECTS TO NUNES'S REASONING, ONE OF THEM ARGUABLY A FORMAL TRICK. ONE IS THIS BUSINESS ABOUT MORPHOLOGICALLY ORDERED ITEMS NOT MATTERING, ACTUALLY CHOMSKY'S IDEA. THE REASON THIS IS PLAUSIBLE IS THAT, AFTER ALL, WORD-LEVEL RULES SEEM DIFFERENT; HOWEVER, IF WE GO BY LEVELS, IT IS TRICKY FROM NUNES'S PERSPECTIVE THAT LINEARIZATION CONCERNS OF THE PHASE-SYNTAX ALSO AFFECT HIGHER ORDERS, AS WE WILL SEE WHEN WE DISCUSS PARASITIC GAPS. PERHAPS THESE HIGHER ORDERS ARE ALSO SYNTACTIC, BUT THERE IS AN OBVIOUS WORRY HERE: WHY IS (ONLY) THE SYNTACTIC OBJECT (AT HIGHER ORDER THAN NUMERATION PHASES) SENSITIVE TO COMMAND? BUT THIS IS NOT WHAT CONCERNS ME NOW; MORE PROBLEMATIC IS THIS: WHAT DOES IT MEAN TO SAY THAT TWO COPIES ARE THE SAME? WE KNOW WHAT THIS MEANS FOR CHOMSKY: IN ESSENCE, CHOMSKY IS USING A QUANTUM OBJECT, WHICH APPEARS IN SEVERAL PLACES AT THE SAME TIME, BUT IS REALLY A SINGLE OBJECT. BUT THIS IS NOT WHAT NUNES IS SAYING AT ALL. FOR HIM EVERY COPY IS AS REAL AS IT GETS, AND IN FACT THIS IS SO MUCH SO THAT IN PARASITIC GAP CONSTRUCTIONS DIFFERENT COPIES OF THE SAME ITEM, WHICH HAPPEN NOT TO FALL UNDER A COMMAND PATH, MANAGE TO OBTAIN DIFFERENT REFERENTS AT LF. IN OTHER WORDS, FOR NUNES COPIES END UP BEING OCCURRENCES OR TOKENS DEPENDING ON WHETHER THEY FALL INTO A COMMAND PATH. OF COURSE, THIS IS FASCINATING, BUT WE SHOULD WORRY ABOUT WHETHER IT IS JUSTIFIED. THE KEY QUESTION IS THIS: TO SAY THAT SOMETHING IS THE SAME AS A COPY OF ITSELF JUST BECAUSE IT IS WITHIN THE SAME COMMAND PATH IS NECESSARY? THAT IS, COULDN'T IT BE OTHERWISE? WHAT IF THEY TOLD US THAT EXATLY THOSE COPIES THAT ENTER INTO A COMMAND PATH ARE *NOT* OCCURRENCES OF THE SAME TOKEN? WOULD THAT BE INCOHERENT? I MUST CONFESS THAT I DO NOT REALLY SEE WHY THAT STATE OF AFFAIRS WOULD BE INCOHERENT, AND IF I'M RIGHT ABOUT THIS WORRY, THEN IT DOESN'T FOLLOW THAT THIS IS A FORMAL PROPERTY OF THE SYSTEM, AND THEN IT IS A SUBSTANTIVE PROPERTY THAT IS NOT JUSTIFIED WITHIN MINIMALISM, SINCE IT IS NOT SOMETHING WE CAN BLAME ON INTERFACE CONDITIONS--AT LEAST I DON'T SEE HOW WE CAN. IN ANY CASE, THESE ISSUES WILL BE AGAIN DISCUSSED WHEN WE RETURN TO PARASITIC GAPS, WHICH NUNES HAS A GREAT LINE FOR BUT WHICH WOULD SEEM TO FACE THIS CONCEPTUAL DIFFICULTY. FOR NOW ALL I HAVE TO SAY IS THAT, IF NUNES'S GENERAL REASONING FAILS, THEN IT IS SURPRISING THAT DIFFERENT FEATURES SHOULD BE PRONOUNCED IN DIFFERENT PLACES AT PF--SUGGESTING THE RELEVANT FEATURES SIMPLY DO NOT FORM A CHAIN.

{A possible case is control contingent on long-distance agreement.

THAT IS, WE'RE DEALING WITH INSTANCES LIKE there arrived two men without PRO identifying themselves, AND SIMILAR EXAMPLES INVOLVING PRO IN THE ADJUNCT, AND THEN A REFLEXIVE PRESUMABLY BOUND TO PRO.

See <MP>, chap. 4.5 (examples (40)-(43)), reviewing work of Anna Cardinaletti and Michal

Starke. It was assumed there that both binding and control are subject to these long-distance effects, but closer examination of Italian by Carl Cecchetto (pc) indicates that the effects are limited to control alone. That would explain the failure of binding in such examples as "there seem to each other [to be many men in the room],"

THIS IS AN OBVIOUS INSTANCE, ALTHOUGH IT INVOLVES MORE COMPLEX ISSUES (THE BENEFACTIVE CONFOUNDS THEM). THERE WAS AN INTERESTING WEB DISCUSSION ABOUT THIS A COUPLE OF YEARS AGO, MOSTLY BY PESETSKY AND LASNIK, WHICH ACTUALLY STARTED WITH A LITTLE EXAMPLE IN MY THESIS: there arrived two knights on each other's horses. AT THE TIME (MORE THAN A DECADE AGO, REALLY), I WAS TRYING TO ESTABLISH THAT BINDING THEORY AS UNDERSTOOD THEN WAS ESSENTIALLY A HAPHAZARD COLLECTION OF STATEMENTS. THIS EXAMPLE WAS TRYING TO SHOW THAT CONDITION A HOLDS AFTER EXPLETIVE REPLACEMENT, SINCE BY THE ASSUMPTIONS I WAS MAKING THE ANTECEDENT DID NOT COMMAND THE ANAPHOR PRIOR TO THAT.

WITH CURRENT ASSUMPTIONS, LASNIK ACTUALLY MANAGED TO DEMOLISH MY ARGUMENT IN THAT WEB DISCUSSION, BY OBSERVING THAT THIS IS BAD: *The DA proved [there to have been two men at the scene] during each other's trials. NOTE THAT, GIVEN THAT two men SHOULD RAISE NOT TO THE EXPLETIVE HERE, BUT HIGHER UP, TO AgrO OR v, TO CHECK ITS CASE, IT SHOULD BE ABLE TO BIND THE ANAPHOR FROM UP THERE. (YOU COULD TRY SOMETHING HERE, BUT GOOD LUCK: PERHAPS THE NEED TO MOVE BOTH THE EXPLETIVE AND each other CREATES A BINDING CONFLICT, WITHOUT COMMAND OBTAINING BETWEEN THE MOVED two men, THE MOVED there, AND THE MOVED ANAPHOR.) ANYWAY, THERE IS AN INTERESTING, RECENT LI SQUIB BY CARDINALETTI DISCUSSING THESE SORTS OF EXAMPLES, AND THE BOTTOM LINE IS THIS: IT APPEARS CLEAR THAT (OBLIGATORY) CONTROL, FOR SOME REASON, CAN BE SATISIFED IN A BROADER SET OF CASES THAN (STANDARD ANAPHORIC) BINDING CAN. I HAD INSTANCES IN R&R OF THE FOLLOWING SORT: There are two heaviweights in tonight's bout because of each other's challenge, WHERE THERE OBVIOUSLY IS NO CONTROL ISSUE. HOWEVER, I HAVEN'T BEEN ABLE TO PERSUADE ANYONE--EVEN MYSELF--THAT THESE ARE GOOD (ACTUALLY, AS OPPOSED TO SOMETHING LIKE there's two heaviweights in tonight's bout because of each other's challenge, WITHOUT AGREEMENT AND THUS, PLAUSIBLY ANYWAY (PARTICULARLY IF CARDINALETTI WAS GENERALLY RIGHT) WITHOUT EXPLETIVE REPLACEMENT BY two heaviweights.

and fits with other evidence that binding requires an "overt" (possibly PRO, trace, or pro) antecedent, not just an implicit argument; see Rizzi (1986).

BUT WHAT DOES IT MEAN FOR A SYNTACTIC PHENOMENON TO HAVE TO BE OVERT? ACTUALLY, I THINK I KNOW ANOTHER ONE JUST LIKE THAT: TOPICALIZATION. THERE IS OVERT FOCUS AND (PLAUSIBLY) LF FOCUS, AND OVERT WH-MOVEMENT AND (PLAUSIBLY) LF WH-MOVEMENT. BUT I HAVEN'T SEEN A SINGLE CONVINCING CASE OF COVERT TOPICALIZATION. (I WAS CRAZY ENOUGH IN THE LAST CHAPTER OF MY THESIS--CHAPTER 5 OF THE PUBLISHED

VERSION--TO TRY AND ARGUE FOR THIS, CLAIMING THAT A DE-RE INTERPRETATION OF A LOWER, SAY, NAME, HAD TO INVOLVE SCOPING OUT TO A MATRIX POSITION, WHICH WAS TAKEN TO BE LF TOPICALIZATION; I DON'T THINK I CONVINCED ANYONE, AGAIN INCLUDING MYSELF.) SO IF TOPICALIZATION--THE CREATION OF AN ANTECEDENT--AND BINDING--THE RELATION TO AN ANTECEDENT--MAY NEED TO BE DONE OVERTLY, OR MORE PRECISELY NOT BY MOVEMENT, WE'RE MISSING SOMETHING BIG TIME ABOUT ANTECEDENCE. PERHAPS THE RELATION INVOLVES A COMPONENT WHICH RELIES ON INFORMATION-THEORETIC CONSIDERATIONS, WHICH MAKES IT, FOR REASONS OF SALIENCE (WHATEVER THAT MEANS) NECESSARILY OVERT.

There are additional complications. The phenomena seem less clear for passives than unaccusatives (possibly because of interference from an implicit subject), and become blurred or disappear in "long-distance" cases; see Hornstein (1996), Lasnik (1997).}

REMEMBER, THIS WAS ALL TRYING TO ESTABLISH FEATURE CHAINS PROPER. THE LOGIC WAS THIS: IF YOU MANAGE TO SAY THAT A FEATURE IS AT LF IN A POSITION DIFFERENT FROM WHERE IT IS AT SPELL-OUT, THEN THAT'S A CHAIN, BY ANY REASONABLE MEASURE. THAT'S WHY THE BINDING FACTS WOULD HAVE BEEN IMPORTANT. OF COURSE, WHETHER THE CONTROL FACTS HOLD MAY ALSO BE IMPORTANT, BUT THAT DEPENDS ON A MORASS OF ISSUES THAT I DON'T WANT TO GO INTO NOW. SO LET'S ASSUME THE GENERAL IDEA AND DENY THE EXISTENCE OF FEATURE CHAINS--IN EFFECT DENYING ATTRACT--AND SEE WHAT HAPPENS. EXPLICITLY:

In the absence of clear evidence to the contrary, I will assume that feature chains do not exist, hence that features cannot move or be attracted.

{Contrary to <MP>. The extension of these notions to features raises difficulties, not insuperable but better avoided, as seems possible.}

I SUPPOSE these notions REFERS TO MOVE AND ATTRACT. IT WAS ALWAYS UNCLEAR WHY, FOR INSTANCE, FEATURES COULD BE ATTRACTED IN MP, BUT THEY COULD NOT PROJECT THEIR OWN STRUCTURE, ASIDE FROM THE OTHER DIFFICULTIES MENTIONED ABOVE.

The dislocation property (II) is another apparent imperfection. In (35), for example, the phrase "an unpopular candidate" is in the natural position of interpretation as object of "elect" in (i)-(ii), but not in (iii)-(iv), though the interpretation is in relevant respects the same:

(35)
(i) they [elected an unpopular candidate]
(ii) there was [elected an unpopular candidate]
(iii) an unpopular candidate was elected
(iv) there was an unpopular candidate elected

AS I THINK I SAID BEFORE, THIS IS AN IMPORTANT PARADIGM THAT I WANT TO RETURN TO LATER ON IN THE SEMESTER (IN PARTICULAR THE CONTRAST BETWEEN (ii) AND (iv).)

In (iii)-(iv), the surface phonetic relations are dissociated from the semantic ones.

{We continue to disregard (iv), abstracting to the expected form (ii); see note <PROB>.}

THIS IS WHAT WE *WON'T* DISREGARD; THE REFERENCE IS TO FOOTNOTE 40, WHICH I THINK I ALREADY COMMENTED ON. MORE LATER ON.

Such phenomena are pervasive. They have to be accommodated by some device in any adequate theory of language, whether it is called "transformational" or something else.

Dislocation of _ yields a chain (_, <t>) -- more accurately, a chain {X,Y}, where X and Y are occurrences of _. The raised element typically c-commands its trace in the original position, but where true, that follows from independent properties of C<HL>. Further operations might lead to violation of c-command and of locality relations between the two positions, as in multiple head-raising or independent XP-dislocation:

(36)
(i) [[V<{j}>-T]<{i}>-C] [...<t><{i}>...[<VP>...<t><{j}>...]]

(ii) [written <t><{j}> for children]<{i}>, [those books]<{j}> couldn't possibly be <t><{i}>]

NOTE THAT WHETHER OR NOT COMMAND OBTAINS IN (i) DEPENDS ON TRICKY ISSUES CONCERNING ADJUNCTION.

In (36), the indices are redundant, the chains determined by constitution of the trace. That need not be so, however, as in (37):

(37) whom<{i}> did everyone talk to whom<{j}> about whom<{k}>

THIS IS A VERY CONFUSING EXAMPLE. OF COURSE, (37) IS NOT A PF. THE POINT HERE IS THAT YOU CAN FORM CHAINS FROM EITHER THE to PHRASE OR THE about PHRASE POSITIONS, AND MERE COMPUTATIONAL CONDITIONS DO NOT DICTATE THE CHOICE.

Chains can be formed with <i> = <j> or <i> = <k>, both consistent with locality conditions; and the full range of interpretations seems to be available, either way.

In the approach we are pursuing here, the chains at LF are determined by identity throughout, with the ambiguity of (37) resolved by the derivation, given the initial numeration.

THAT IS, THE AMBIGUITY DOESN'T REALLY ARISE IN THE DERIVATIONAL

SYSTEM. YOU HAVE TWO who'S IN THE NUMERATION AND ONE MOVES CREATING A CHAIN, AND UNDER IDENTITY OF OCCURRENCES YOU IDENTIFY THAT CHAIN, THUS RECOVERING THE NUMERATION TOKEN. BUT REMEMBER, THIS IS A WAY OF EXECUTING THE PROGRAM, NOT A NECESSARY ONE. IF YOU'RE A REPRESENTATIONALIST, YOU WANT TO FIND WAYS OF FORCING THE RELEVANT PF AND LF INTERPRETATIONS OF (37), WITHOUT RESORTING TO NUMERATIONS AND SO ON. AND IF YOU DON'T WANT THE NUMERATION, THEN:

Other means would be required if we were to eliminate this device along lines discussed earlier. In a strict derivational approach semantic interpretation is cyclic and the problem of interpreting (36), (37) arises in a somewhat different form. In a representational approaches chains are determined by an algorithm A operating in a "search space" of possible LFs; the burden of accounting for locality and other conditions on chains would then rest on A. Here we return to issues of general architecture raised in sec. 3.

SEE FOR INSTANCE BRODY'S MONOGRAPH FOR THIS SORT OF APPROACH, OR MUCH OF KOSTER'S WORK (INCLUDING EARLIER ONE) AND PART OF RIZZI'S.

However these matters are resolved, we have two "imperfections" to consider: uninterpretable features and the dislocation property.

AGAIN, UNDERSTOOD IN TERMS OF ALGORITHM A IF YOU CHOOSE NOT TO DO THINGS DERIVATIONALLY. THE PROBLEM DOESN'T GO AWAY, THAT IS.

These properties (in fact, morphology altogether) are never built into special-purpose symbolic systems. We might suspect, then, that they have to do with externally-imposed legibility conditions.

I UNDERSTAND THE TWO CLAIMS, BUT I DON'T REALLY SEE HOW THE then RELATES THEM. THAT IS, I DON'T SEE HOW ANYTHING WE SAY ABOUT SYMBOLIC SYSTEMS REALLY BEARS ON THESE ISSUES. IN ANY CASE, LET'S GO WITH THE GENERAL IDEA, WHICH SEEMS IF NOT PLAUSIBLE (WHO KNOWS) AT LEAST INTERESTING.

With regard to dislocation, that has been suggested from the earliest days of modern generative grammar, with speculations about facilitation of processing (on the sound side) and the dissociation of "deep" and "surface" interpretive principles (on the meaning side). The boundaries are not clear,

{They have also shifted as inquiry proceeded: thus from the 1950s through the mid-'70s, such matters as quantifier scope were commonly taken to be prototypical examples of "surface" interpretation, while more recent work generally takes them to be prototypical properties at LF.}

nor the mechanisms to express them. One approach to the array of problems was to distinguish the role of deep and surface (D- and S-) structure in semantic interpetation: the former enters into determining quasi-logical properties such as entailment and theta structure; the latter such

properties such as topic-comment, presupposition, focus, specificity, new/old information, agentive force, and others that are often considered more discourse-oriented, and appear to involve the "edge" of constructions.

OF COURSE, THIS PARTICULAR CUT WAS BEFORE LF WAS SYSTEMATICALLY STUDIED. I DON'T THINK IT IS REALLY EASY TO ARGUE THAT SURFACE SYNTAX COVERS ALL MATTERS PERTAINING TO SPECIFICITY, FOCUS, AND THE LIKE, UNLESS ONE IS READY TO CLAIM THAT ALL SUCH MATTERS INVOLVE, SAY, THE DISPLACEMENT ONE SEES IN HUNGARIAN OR IN GERMAN *AT SURFACE SYNTAX*. OF COURSE, THAT'S PRECISELY WHAT KAYNE HAS BEEN ARGUING RECENTLY, A MATTER THAT I PLAN TO RETURN TO AFTER WE INTRODUCE MANY MORE ASSUMPTIONS IN THE COURSE. BUT IF YOU SET THAT ASIDE, THERE JUST SEEMS TO BE THE CASE THAT IN LANGUAGES LIKE ENGLISH YOU DO NOT OVERTLY FOCALIZE AND YOUR SPECIFIC EXPRESSIONS CAN BE LEFT IN SITU, AND NEED NOT BE OVERTLY SCRAMBLED. AT ANY RATE, I DO THINK THE WORLD WOULD BE A NICER PLACE IF IT WERE TRUE THAT SURFACE SYNTAX DOES DO ALL OF THIS, HAVING TO DO WITH INFORMATION STRUCTURE, AND NONE OF THESE ISSUES HAPPEN AT LF. BUT THAT POSES A FURTHER QUESTION: WHY? ARE THERE, THEN, ANY LF OPERATIONS? ONE VIEW, NOTABLY EXPLORED BY BEGHELLI, FOX, REINHART, AND OTHERS, WOULD ARGUE THERE ARE (WE SHOULD SEE IF THEY HAVE CONVINCING CASES); AS I SAID KAYNE ARGUES THERE AREN'T, RADICALLY SO EVEN FOR TRADITIONAL QUANTIFIER RAISING INSTANCES. THAT COULD BE, BUT POSES TWO NEW QUESTIONS: (I) ARE THERE ANY IN-PRINCIPLE DIFFERENCES BETWEEN INFORMATION-RELATED RELATIONS (SPECIFICITY AND SO ON) AND INTENTIONALITY-RELATED RELATIONS BEARING ON TRUTH CONDITIONS? (II) MORE GENERALLY, IS THE LF COMPONENT ACTIVE AT ALL, OR DOES IT SIMPLY PIGGY-BACK ON WHATEVER HAPPENS FIRST, ALONG THE LINES OF KITAHARA/HORNSTEIN? NEEDLESS TO SAY, THESE ARE ALL DEEP ISSUES.

AND HERE'S THE FINAL ONE: SHOULD WE CONFLATE THOSE BASIC RELATIONS (SUCH AS THETA-STRUCTURE AND LEXICAL ENTAILMENT) THAT USED TO BE THOUGHT OF AS THE DOMAIN OF D-STRUCTURE WITH RELATIONS THAT TAKE PLACE AT LF? THAT INFERENCE IS INVITED BY GETTING RID OF D-STRUCTURE, BUT IN THE LIMIT IT WILL JUSTIFY GETTING RID OF THOSE NOTIONS ALTOGETHER (THETA STRUCTURE, LEXICAL ENTAILMENTS)--HOWEVER, IS THAT A GOOD IDEA? A RADICAL WAY OF PUTTING THIS, WITHIN THE PRESENT CONTEXT ASKING ABOUT DISPLACEMENT, IS THIS: IF IT'S ALL BUNDLED UP AT LF, WHY DON'T WE HAVE LEXICAL CHOICES FOR SUCH THINGS AS DIFFERENT SCOPAL ORDERS OR DIFFERENT FOCAL PROPERTIES? IN OTHER WORDS, WHY ISN'T THERE A LANGUAGE THAT ALLOWS YOU TO SAY someone glibs everyone TO MEAN THAT EVERYONE IS SUCH THAT HE OR SHE LOVES SOMEONE, AND someone bligs everyone TO MEAN THAT SOMEONE IS SUCH THAT EVERYONE LOVES HIM OR HER? OR WHY CAN'T WE FIND A LANGUAGE THAT ALLOWS YOU TO SAY John babum Mary TO MEAN THAT JOHN LOVES *MARY* AND John badabum Mary TO MEAN THAT *JOHN* LOVES MARY. THOSE LANGUAGES WOULD BUILD WORDS

NOT AROUND THETA STRUCTURE AND ENTAILMENT RELATIONS, LIKE OURS DO, BUT IN TERMS OF INTENTIONAL AND INFORMATION-DRIVEN RELATIONS (OR AT ANY RATE, BOTH). IN AS MUCH AS NONE OF THAT EXISTS--ALTHOUGH IT IS PERFECTLY IMAGINABLE AND SENSIBLE AS A FORMAL SYSTEM--THERE PLAINLY IS A MISMATCH BETWEEN THE DIFFERENT KINDS OF RELATIONS (SAY, BETWEEN CONCEPTUAL STRUCTURE AND INTENTIONAL STRUCTURE). THAT'S THE MYSTERY.

Theories of LF and other approaches sought to capture the distinctions in other ways. The "deep" (LF) properties are of the general kind found in language-like systems; the "surface" properties appear to be specific to human language.

MORE PRECISELY, THE "DEEP" PROPERTIES WERE INTENTIONAL (ULTIMATELY TRUTH-FUNCTIONAL, WHICH NEED NOT BE, INCIDENTALLY, TRUTH CONDITIONAL OR EVEN TRUTH DESAMBIGUATING--SEE MAY (1985)), WHEREAS THE "SURFACE" STRUCTURE WERE NOT (E.G. THE RELATIVE HEIGHT OF SPECIFIC ELEMENTS DOESN'T CHANGE TRUTH CONDITIONS). WHETHER BETWEEN NON-INTENTIONAL PROPERTIES YOU MAY ALSO SEPARATE A LAYER OF CONCEPTUAL PROPERTIES DEPENDS ON SUCH ISSUES AS WHETHER THERE IS SPECIFIC THETA INFORMATION (HIERARCHIES, THETA CONDITIONS OF THE DOWTIAN SORT, PERHAPS BAKER'S UTAH, ETC.).

If the distinction is real, we would expect to find that language design marks it in some systematic way -- perhaps by the dislocation property, at least in part.

THAT'S A BIT OF WISHFUL THINKING. THE REAL DISTINCTION CAN BE MARKED IN A ZILLION WAYS, AND MORE IMPORTANTLY, IT NEED NOT BE MARKED AT ALL... ON THE OTHER HAND, IF MOVEMENT WERE AN IMPERFECTION (I.E. YOU BUY THE BULLET), THEN YOU COULD RUN THE ARGUMENT THAT THE HUMAN MIND EXAPTED IT FOR "MARKING" (I.E., IN CHOMSKY'S SENSE, PRESUMABY FOR PURPOSES OF SALIENCE IN THE MAPPING WE'RE NOW CONSIDERING), JUST AS IT PRESUMABLY EXAPTED A WEIRD PROPERTY OF LARYNXES FOR CODING NUMERATION TOKENS. SOMETIMES ACCIDENTS ARE NICE, AND OUR UNIVERSE IS THERE TO PROVE IT! JUST LOOK AT HOW USEFUL YOUR THUMB HAS TURNED OUT TO BE... THE POINT IS, THE STRONG MINIMALIST THESIS, AS FAR AS I CAN SEE, WOULDN'T SUFFER IF IT IS TRUE ONLY TO THE EXTENT THAT IT IS, AND SOME ACCIDENTAL PROPERTIES OF PF/MORPHOLOGY LEAD TO CURIOUS RESULTS, LIKE WORD TOKENS OR CHAINS. OF COURSE, WITHOUT THE STRONG MACHINERY BEHIND IT, ALL OF THAT WOULD LEAD, AT BEST, TO A BIRD-SONG! BUT THE FACT THAT YOU'RE ARGUING FOR A WONDERFULLY PRETTY MACHINE DOESN'T MEAN THAT IT CANNOT HAVE PATCHWORK PARTS, EVEN PATCHWORK PARTS THAT MAKE IT USABLE. AFTER ALL, NOTHING IN THE PRETTY DESIGN HAD ANYTHING TO DO WITH USABILITY, LEARNABILITY, AND ALL THE REST! (AND INSPITE OF THE METAPHORS, THERE *WASN'T* AN ACTUAL ENGINEER OPERATING HERE). ANYWAY, NOT TO BLAME CHOMSKY OF THIS GOULDIAN RETREAT (?), CALL THIS URIAGEREKA'S "MIXED" STRONG THESIS. (YOU'LL SEE

THAT, IN SOME RESPECTS, I'LL PRESENT A THESIS THAT IT'S EVEN STRONGER THAN CHOMSKY'S, BUT I HAVE NO SCRUPLES IN MAKING IT DIRTY AS WELL, PRECISELY BECAUSE WE'RE DEALING WITH BIOLOGICAL, DIRTY BEINGS.)

To the extent that such ideas can be given substance, it would follow that the dislocation property is required; it falls within the design specifications given to the super-engineer seeking an optimal solution to conditions imposed by the external systems.

OF COURSE, WE CAN GO ALONG AND BUY THIS JUST TO SEE WHERE IT LEADS, BUT I MUST SAY I DON'T FOLLOW THE PUNCH-LINE. WE MAY AS WELL CALL THIS THE "PRINCIPLE OF HOPE"... (GOOD ONE TO HAVE, THOUGH.)

This line of argument might provide motivation for the dislocation property, but it would remain to find the mechanisms employed to implement it. The distinction is familiar. We may say that the function of the eye is to see, but it remains to determine the implementation; a particular protein in the lens that refracts light, etc.

ACTUALLY, THAT'S THE KIND OF EXAMPLE THAT ADDS KINDLE TO MY FIRE ABOVE. WHAT DOES IT MEAN TO SAY THAT THE EYE'S *FUNCTION* IS TO SEE? THAT SEEMS LIKE AN A POSTERIORI COMMENT. MORE LIKELY, EYES ARE SO COMMON (THEY INDEPENDENTLY EVOLVED DOZENS OF TIMES, THOUGH MORE ON THIS SHORTLY) BECAUSE OF A COUPLE OF FACTORS. FIRST, BECAUSE LIGHT SENSITIVE CELLS (WITH THE RELEVANT PROTEIN, ETC.) DON'T SEEM PARTICULARLY HARD TO COME BY, PERHAPS NOT SURPRISINGLY IN A WORLD LIKE OURS THAT GETS LOADS OF LIGHT, APPROPRIATELY POLARIZED (OTHERWISE IT MIGHT BE TOO STRONG TO USE FOR FORMING IMAGES). SECOND, BECAUSE THE SORT OF INVAGINATION YOU NEED TO CAPTURE IMAGE-FORMING EYES IS SOMETHING THAT MANY COMPLEX ORGANISMS NEED ANYWAY. TO BEGIN WITH, WE START AS AN INVAGINATED TUBE IN THE PRIMORDIAL AXIS, AND FURTHERMORE MOST FEMALES (THOUGH NOT ALL, CURIOUSLY) INVOLVE SOME SORT OF INVAGINATION FOR REPRODUCTION. AS IT TURNS OUT, THE GENES RESPONSIBLE FOR VULVA-FORMATION IN MANY SPECIES ARE ALSO RESPONSIBLE FOR CORE EYE-FORMATION. THAT, I'D SAY, IS A PRETTY LUCKY ACCIDENT; IF WE HADN'T FOUND MULTICELLULAR ORGANISMS OR EVEN SEX, WE WOULDN'T HAVE FOUND EYES. PUT DIFFERENTLY, IN THE WORLD OF BACTERIA (WHICH IS MOST OF THIS PLANET, NOW AND ALWAYS) THERE ARE NO EYES, ALTHOUGH THERE PROBABLY WOULD BE A PRETTY NICE FUNCTION FOR EYES. (IN THE WORLD OF EUKARIOTIC BEINGS, INCLUDING UNICELLULAR EYES, APPARENTLY THERE ARE ALREADY SOME LIGHT SENSITIVE DEVICES IN THE CYTOSKELETON, BUT IT'S HARD TO CALL THAT AN EYE--CERTAINLY, IT ISN'T IMAGE-FORMING, LET ALONE IMAGE-INTERPRETING!) BUT THERE ARE PROBABLY EVEN MORE ACCIDENTS BEHIND THE FORMATION OF IMAGE-FORMING EYES. AS IT TURNS OUT, THE GENE RESPONSIBLE FOR DIRECTING THE DESIGN OF SUCH EYES (APARENTLY DICTATING SUCH THINGS AS THE SPOT OF AN ALREADY POSSIBLE INVAGINATION, BUT WITH LIGHT SENSITIVE CELLS AND A CERTAIN CHUNK OF TRANSPARENT CELLS TO FOCUS THE IMAGE--WITHOUT

WHICH YOU'D ONLY HAVE A WIDE ANGLE PINHOLE-STYLE CAMEREA OBSCURA) IS PRESENT IN AN ANCESTOR COMMON TO MICE, FLIES, SQUID, AND GOD KNOWS WHAT ELSE--THE SO-CALLED EYELESS GENE. THAT'S PRETTY FAR BACK, AND PRESUMABLY ANOTHER ACCIDENT (IN OTHER WORDS, PART OF THE SURPRISING CONVERGENCE BETWEEN, SAY, A SQUID EYE AND AN INDEPENDENTLY EVOLVED FLY EYE IS NOT SO SURPRISING). THE POINT IS, THERE IS NO FUNCTION TO ANY OF THIS UNTIL THE STRUCTURES ARE IN PLACE, PARTLY BECAUSE OF ACCIDENTS AND PARTLY BECAUSE OF DEEP STRUCTURAL PROPERTIES OF ORGANISMS, SUCH AS THEIR HAVING TRANSLUCID CHUNKS OR LIGHT-SENSITIVE CHUNKS. THAT'S HOW FORMS EMERGE; WHETHER THEY *STAY* OF COURSE IS A DIFFERENT MATTER: THEY DON'T IF THEY'RE HARMFUL (MEANING THE ORGANISM DIES BEFORE REPRODUCTION), THEY DO IF THEY'RE NOT, AND THEY THRIVE IF THEY'RE ADAPTIVE, LEADING TO MUCH SEX AND MANY CHANGES AND SO ON. SO ANYWAY, IF THE EYE IS ANY INDICATION, MY MIXED STRONG THESIS CLEARLY APPLIES TO IT, IF YOU'RE GENEROUS WITH INTERPRETATION. SO THEN THE QUESTION IS WHETHER YOU WANT TO SAY THAT THE LINGUISTIC SYSTEM IS REALLY VERY DIFFERENT. OF COURSE, WHO KNOWS--HERE WE HAVE ONLY ONE, AND WE CAN'T REALLY TORTURE OURSELVES TO FIND OUT ALL THE THINGS WE'VE FOUND ABOUT EYES... BUT ANYWAY, PROFOUNDLY, THAT'S PARTLY WHAT'S AT ISSUE--AND WHY I'M NOT TOO BOTHERED WITH HAVING SOME GLITCHES IN THE SYSTEM THAT WE CAN USE, IN FACT EVEN MORE PRODUCTIVELY THAN THE GRAND STRUCTURAL DESIGN. INDEED, I'LL GO AS FAR AS TO CLAIMING THAT WITHOUT GLITCHES YOU WOULDN'T HAVE ANYTHING, SINCE IT IS GLITCHES THAT DRIVE SYSTEMS INTO EXISTENCE TO BEGIN WITH. PERFECT FORM IS DEAD; WORSE, IT DOESN'T EXIST (DEATH ALREADY APPLIES TO AN IMPERFECTION: LIFE). I KNOW I'M GETTING PHILOSOPHICAL, SO I'LL STOP SAYING THIS: THE ISSUE IS NOT PERFECTION, BUT QUASI PERFECTION. THERE'S THE RUB: ALMOST PERFECT, WHICH MEANS YOU NEED TO UNDERSTAND THE PERFECTION AND THEN SEE WHAT SETS IT IN MOTION.

Similarly, certain semantic properties may involve dislocated structures, but we want to discover the mechanisms that force dislocation. Minimalist intuitions lead us to look at the other major imperfection, the uninterpretable inflectional features. Perhaps these devices are used to yield the dislocation property.

I THINK THIS IS BACKWARDS. I'D TRY TO RUN THE ARGUMENT THE OTHER WAY AROUND: YOU HAVE THOSE STUPID DEVICES FOR NO GOOD REASON (FOSSILIZED MORPHOLOGY, SAY), BUT HEY: THEY SET THE SYSTEM IN MOTION--LITERALLY, A CATASTROPHE--SO THAT IT CAN GET RID OF THEM. ONCE THE SYSTEM IS IN MOTION THAT WAY, YOU CAN USE THOSE DYNAMICS FOR OTHER PURPOSES--YOU COOPT THEM. I TRY TO RUN THAT LINE IN CHAPTER 6 OF R&R, OF COURSE IN THE VOICE OF THE OTHER... AT ANY RATE, FOR MOST OF THE ANALYSIS BELOW, WHETHER YOU THINK IT IS THE CHICKEN (MOVEMENT) OR THE EGG (FEATURES), IT DOESN'T MATTER.

If so, then the two imperfections might reduce to one, the dislocation property. But the latter might itself be required by design specifications. That would be an optimal conclusion, falling under option (iii) of (29).

SO TO SUM UP, I AGREE WITH THE CONCLUSION THAT THE IMPERFECTION IS ONLY ONE, BUT DISAGREE WITH THE CONCLUSION, WHICH ONLY THE PRINCIPLE OF HOPE YIELDS, THAT THIS HAS TO DO WITH DESIGN SPECIFICATIONS. AND I CLAIM THAT THIS MAKES NO DIFFERENCE SO FAR, PARTICULARLY BECAUSE WE DON'T KNOW HOW THE PRINCIPLE OF HOPE WORKS.

To establish any such conclusion is no simple matter.

NOTE, THE MIXED STRONG THESIS HAS TO ESTABLISH THIS CONCLUSION TOO.

We are entering terrain that is mostly unexplored. One approach is suggested by the observation that for convergence, uninterpretable features must be deleted in the course of computation of LF. Consider the dislocated example (35iii), repeated here:

THE TONE OF THESE SENTENCES IS WORTH EMPHASIZING. THERE IS NOTHING OBVIOUS OR OBVIOUSLY RIGHT ABOUT THE OBSERVATION (NOTE, A MERE OBSERVATION) THAT YOU MUST GET RID OF CERTAIN FEATURES. THIS IS ALL TENTATIVE, MOSTLY UNEXPLORED STUFF...

(38) an unpopular candidate T-was elected <t>

There are three kinds of uninterpretable features in this structure:

THESE ARE THE *RELEVANT* FEATURES--THERE'S OTHERWISE MANY MORE...

(i) the agreement features of T (taking them as a unit, the set of _-features), (ii) the EPP feature of T that requires "second Merge," and (iii) the structural Case feature of "an unpopular candidate." The _-set (i) identifies T as a target of dislocation;

CAREFUL WITH THIS, WHICH WILL SOON MOVE FROM AN OBSERVATION TO A FUNDAMENTAL PROPERTY OF THE NOTION 'AGREE'.

the EPP-feature (ii) requires that something be merged in this position; the Case feature (iii) identifies "an unpopular candidate" as a candidate for such merger (hence dislocation).

AND HERE TOO BE CAREFUL; THE IDENTIFICATION IN QUESTION--LESS OF AN OBVIOUS, OR MERE OBSERVATION IN THIS INSTANCE, PARTICULARLY SINCE CASE DOESN'T ALWAYS IDENTIFY A WHOLE RELEVANT PHRASE ACROSS LANGUAGES--WILL TURN INTO A CENTRAL PROPERTY OF 'AGREE'.

Successful implementation of the operation erases all of the uninterpretable features, forming MLIs with a reduced set of features.

THAT IS, BY ASSUMPTION.

The approach is optimal, if indeed uninterpretable features are the mechanism for dislocation.

THE LOGIC IS: YOU NEED ALL OF THOSE THREE COMPONENTS TO MAKE MOVEMENT WORK (WHAT TO DO, WHERE TO GO, WHERE TO COME FROM, WHICH RECALLS THAT OLD PHILOSOPHICAL TRINITY OF WHO WE ARE, WHERE DO WE COME FROM, AND WHERE ARE WE GOING!) AND LO AND BEHOLD, YOU HAVE THEM ALL. THE TRUTH IS, THOUGH, THAT THE INVERTED LOGIC ALSO WORKS. THAT IS, SUPPOSE YOU HAD UNINTERPRETABLE FEATURES FOR SOME REASON, SAY A RESIDUE OF INCORPORATED MORPHOLOGY (IN ESSENCE, WHAT THEY USED TO CALL 'GRAMMATICALIZATION'). THE SYSTEM IDENTIFIES THIS 'VIRUS' AND HAS TO GET RID OF IT, FAST; FOR THAT, IT NEEDS A COMPATIBLE ELEMENT TO GO OVER WHERE THE VIRUS IS AND KILL IT--A SORT OF ANTIBODY. SAY YOU IDENTIFY THE ANTIBODY IN TERMS OF CASE AND THE VIRUS IS THE NASTY UNINTERPRETABLE FEATURE. AS FOR EPP, YOU ADD IT TO THE PROCESS JUST AS YOU DO IN CHOMSKY'S REASONING: BECAUSE IT'S THERE. INDEED, FROM THE PERSPECTIVE OF THE MIXED THESIS YOU'D SAY: ONCE THE ANTIBODY GOES TO THE VIRUS, THE IDENTIFIED (CASE MARKED) PHRASE IS IN AN EDGE SITE (THE EPP DOMAIN), WHERE IT HAD TO BE ANYWAY--FOR SOME REASON. WE WILL RETURN LATER ON TO WHAT THAT REASON COULD BE, BUT IT WORKS THE SAME ON BOTH TREATMENTS, THE ONE HOLDING MOVE AS MOTIVATING FEATURES AND THE ONE HOLDING FEATURES AS MOTIVATING MOVEMENT.

{The approach renders superfluous the intuitive motivation for pied-piping proposed in <MP>, in terms of PF crash.

SPECIFICALLY IN TERMS OF PF CRASH, WHICH DOESN'T MEAN THE GENERAL PIED-PIPING LINE COULDN'T WORK FOR OTHER REASONS, INCLUDING AT LF, AS IN FOX'S DISSERTATION. AND ALSO:

See Uriagereka (forthcoming a) for argument that some such device nevertheless operates in languages with rich morphology, with consequences for null subject, barriers, and Spell-Out.

WE'LL SEE THIS PAPER LATER ON IN THE SEMESTER--THE BASQUE ONE. IN ANY CASE, THE ISSUE IN LANGUAGES LIKE BASQUE IS WHETHER RICH (OR WHAT I CALLED 'HEAVY') MORPHOLOGY INVOLVES NOT JUST PIED-PIPING FOR MORPHOLOGICAL REASONS, BUT FURTHERMORE THE EMERGENCE OF A KIND OF BARRIER IN THE SYSTEM. LET'S SET THIS ASIDE FOR NOW.

For an approach somewhat similar to what is outlined below, from a partially different perspective, see Frampton and Guttman (1998).}

TECHNICALLY, THE FRAMPTON AND GUTTMAN PERSPECTIVE IS RATHER

DIFFERENT, BUT THE GENERAL LINE OF REASONING, INCLUDING VARIOUS SPECIFICS ABOUT COMPUTATIONAL COMPLEXITY, IS EXACTLY THE SAME. BUT AGAIN, LET'S PUT THIS TO THE SIDE.

Assuming so, let us look more closely. Suppose that the derivation has constructed the syntactic object (39), having merged T with the copula-headed phrase:

(39) T be elected an unpopular candidate

The new element T has uninterpretable features of two types: its _-set and its selectional feature EPP. Like other selectional features, EPP seeks an XP to merge with the category it heads.

THIS IS RATHER MISLEADING TERMINOLOGY AND PERHAPS EVEN IMPLEMENTATION. OF COURSE, WE CAN CALL 'SELECTION' WHATEVER WE WANT, BUT USUALLY THIS IS A TERM USED FOR THETA-THEORY, AND THUS WE SAY, FOR INSTANCE, THAT A TRANSITIVE VERB SELECTS FOR A DIRECT OBJECT, OR A DITRANSITIVE FOR WHATEVER YOU THINK IT DOES. BUT IT IS REALLY NOT VERY PROFOUND TO SAY THAT A DETERMINER SELECTS FOR AN NP OR A TENSE FOR A VP; IN A SENSE, OF COURSE THEY DO. BUT THEY ALWAYS DO! DETERMINERS ARE NOT CLASSIFIED IN TERMS OF WHETHER THEY START THEIR DERIVATIONAL LIFE SELECTING ONE OR TWO NOUN PHRASES, FOR INSTANCE. THERE'S NO SELECTION ISSUE THERE--IT'S JUST THE GARDEN VARIETY COMBINATORIAL PROPERTY OF THESE ELEMENTS. I AT LEAST THINK OF SELECTION AS SOMETHING MORE SUBSTANTIVE, IN PART BECAUSE I TAKE THETA-THEORY RATHER SERIOUSLY, AND I DON'T SEE HOW DETERMINERS OR TENSES HAVE SERIOUS THEMATIC PROPERTIES. ANYWAY, WORSE STILL PERHAPS, WHAT T IS SUPPOSED TO SELECT IN ITS SPECIFIER IS SOMETHING THAT CAN GO THERE EITHER BY MERGE OR BY MOVE. AGAIN, CALL IT WHAT YOU WILL, BUT I THINK IT'S GOOD TO KEEP THE TERM SELECT FOR SOMETHING YOU DO UNDER MERGE, AGAIN AS IN THETA THEORY. OR IF YOU CALL SELECT THE MORE GENERAL THING, THEN WHATEVER HAPPENS UNDER MERGE WITH THETA ROLES AND SO ON IS NOT SELECT, BUT SOMETHING NARROWER. THE POINT IS: CALLING THE EPP PROBLEM A SELECTION FEATURE WON'T MAKE IT GO AWAY. WE REALLY HAVE NO CLUE AS TO WHAT'S GOING ON IN EPP SITES. THIS PAPER IS TRYING TO SAY SOMETHING MEANINGFUL ABOUT IT--AND I THINK IT DOES--BUT I WOULDN'T CALL IT A SELECTION ISSUE, SINCE IT CLOUDS THE REAL DIFFICULTY.

The _-set we can think of as a <probe> that seeks a <goal>, namely, "matching" features that establish agreement. The relation of the probe of T to its goal is the T-<associate> relation.

OKAY, SO THIS IS NOW ALL NEW TERMINOLOGY, THOUGH INTUITIVE. THE IDEA IS THAT THE PROBE IS WHAT WE USED TO CALL IN THE OLD DAYS (LITERALLY IN THE SAME CONTEXTS IN THE R&U FRAMEWORK) THE 'GOVERNOR' AND OF COURSE THE 'GOAL' IS WHAT WE USED TO CALL THE 'GOVERNEE', AND EXACTLY WHERE WE USED TO SAY AGREEMENT CAN HOLD BY LICENSING OF THE

FEATURES OF THE GOVERNEE, NOW WE SAY THAT THERE IS A MATCHING OF FEATURES THAT APPEAR ON BOTH PROBE AND GOAL. I HOPE YOU SEE THIS IS TECHNICAL IMPLEMENTATION, STILL CODING A LONG-DISTANCE RELATION IN PRETTY MUCH THE SAME WAY IT USED TO BE CODED. INDEED, I SHOULD SAY THAT BY THE END OF THE PAPER THE RESEMBLANCE OF THE SYSTEM TO THE R&U PAPER IS EVEN MORE DRASTIC, SINCE IN THAT PAPER WE HAD LONG-DISTANCE GOVERNMENT APPLY OVERTLY PRECISELY TO DISTINGUISH IT FROM WHAT AT THE TIME WERE THOUGHT TO BE CATEGORIAL DISPLACEMENTS AT LF (IN THOSE DAYS THE ENTIRE ASSOCIATE, NOT JUST ITS FEATURES, WAS TAKEN TO MOVE TO THE TARGET), WHICH WE TRIED TO SHOW WERE NOT OPERATIVE IN THE EXAMPLES WE WERE ANALYZING. SO R&U WAS A THEORY OF AGREEMENT UNDER GOVERNMENT IN THE OVERT SYNTAX, WHERE GOVERNMENT WAS DEFINED IN TERMS OF A DYNAMIC NOTION THAT WAS PRACTICALLY IDENTICAL TO THE EFFECTS OF THE MINIMAL LINK CONDITION (INDEED, IN SOME LATER WORK WE CALLED THE NOTION 'LOCALITY' INSTEAD OF 'GOVERNMENT', ALTHOUGH THAT'S JUST TERMINOLOGY). AT THIS POINT IN THIS PAPER, IT SEEMS AS IF THE NOTION OF AGREEMENT BEING DISCUSSED IS NOT SOMETHING THAT TAKES PLACE IN OVERT SYNTAX, THUS IS DIFFERENT FROM WHAT WE HAD. BUT BY THE END OF THE PAPER, ONLY OVERT SYNTAX REMAINS! SO AGAIN, AS FAR AS I CAN SEE THE NOTIONS ARE FOR ALL INTENTS AND PURPOSES FORMALLY THE SAME.

For the _-set of T in (39), there is only one choice of matching features: the _-set of "candidate." Locating this goal, the probe erases under matching. Taking structural Case to be a reflex of an uninterpretable _-set,

THE NOTION OF ERASURE UNDER MATCHING IS WHAT SOMETIMES GETS REFERRED TO AS 'SUICIDAL GREED'. THAT IS, THE PROBE F SEEKS AN IDENTICAL FEATURE F' IN THE GOAL, AND WHEN F FINDS F', F GOES, LITERALLY ERASES--KAPUT BIG TIME, NOT JUST SOME CODING SAYING 'DON'T INTERPRET ME AT THE INTERFACE,' AS IN ELLIPSIS SITES, SAY.

{If a reflex of an interpretable _-set, it would be erased in situ by the _-set of <candidate> itself.}

I MUST CONFESS I DON'T GET THIS FOOTNOTE. IS CHOMSKY TALKING ABOUT INHERENT CASE, WHICH COULD REASONABLY BE SAID TO BE THE REFLEX OF SOMETHING INTERPRETABLE? OR IS HE TALKING ABOUT SOME OTHER FORM OF CASE? PART OF THE REASON I DON'T JUMP INTO CONCLUSIONS HERE IS BECAUSE OF THE PHENOMENON OF CASE SPREAD IN LANGUAGES LIKE RUSSIAN, WHERE IT IS NOT ENTIRELY OBVIOUS TO ME A) WHY CASE APPEARS IN SEVERAL PLACES WITHIN THE NOMINAL EXPRESSION, AND B) HOW DO YOU GET RID OF ALL OF THOSE?--OR CAN YOU SAY THAT THOSE ARE INTERPRETABLE?

it too erases under matching with the probe. The erasure of uninterpretable features of probe and goal is the operation we called <Agree>.

BY DEFINITION, AND NOTE: THE ERASURE IS THE AGREEMENT, NOT THE MATCHING. INDEED, YOU CAN THINK OF THE MATCHING AS THE ERASURE ITSELF. THIS IMMEDIATELY MEANS YOU MUST SEPARATE AGREE IN THIS TECHNICAL SENSE FROM THE OLD CONCORD, OR OTHER FORMS OF LOOSE AGREEMENT (FOR INSTANCE OF THE SORT SEEN IN ANTECEDENCE), WHERE IT IS JUST NOT TRUE THAT MATCHING IS ERASURE, AND OF COURSE NO MOVEMENT IS INVOLVED IN ANY SENSE.

But EPP of T must also be satisfied;

OKAY, SO HERE'S WHAT WILL BE BEHIND MOVEMENT IN THE INSTANCES WHERE IT HAPPENS. NOTE, UP TO THIS POINT YOU JUST HAD AGREEMENT, THAT IS MATCHING. SO WHY IS THERE DISPLACEMENT?

in this case, by "pied-piping" of a phrase P(G) determined by the goal of T's probe, which merges with (39), becoming SPEC-T.

SO AGAIN, PIED-PIPING IS STILL ALIVE--IT'S JUST NOT 'FOR PF REASONS', AS IN CHAPTER 4. INDEED ALL THAT DISCUSSION IN CHAPTER 4 ABOUT WHAT DETERMINES THE MINIMAL PHRASE THAT PIED-PIPES WITH THE GOAL IS, SO FAR AS I UNDERSTAND, COMPLETELY RELEVANT (WHICH MEANS THE ACCOUNT IS REALLY NOT THAT DIFFERENT FROM THE ONE INVOLVING ATTRACTED FEATURES, AFTER ALL).

The combination of selection of P(G), Merge of P(G), and feature-deletion under match (Agree) is the composite operation Move, which dislocates "an unpopular candidate," eliminating all uninterpretable features.

RIGHT, JUST AS IN CHAPTER 4 IT WAS ATTRACTING OF F WITHIN SOME ELEMENT (HERE P(G)), ALSO MERGE OF P(G) (WHERE P(G) IS DETERMINED SOMEHOW OPTIMALLY), AND THE CHECKING OF AN ATTRACTING FEATURE IN THE TARGET OF MOVEMENT. DON'T GET ME WRONG: THERE ARE DIFFERENCES. FOR INSTANCE, CHAPTER 4 HAD A FEATURE CHAIN, WHEREAS THIS ARTICLE DOES NOT, PROBABLY WITH GOOD CAUSE, SINCE CERTAINLY WE HAVE INSTANCES WHERE WE DO NOT PRONOUNCE THE UPPER FEATURE (COMPLETELY CONTRARY TO THE CHAIN ASSUMPTION, SAY IN NUNES'S TERMS) AND POSSIBLY ALSO INSTANCES WHERE WE PRONOUNCE BOTH (WHICH I DISCUSSED ABOVE), BUT NOT, AT LEAST NOT THAT I KNOW, INSTANCES WHERE YOU PRONOUNCE THE UPPER FEATURE BUT NOT THE LOWER ONE, WHICH IS WHAT YOU WOULD EXPECT IF STANDARD CHAINS WERE INVOLVED.

Matching is a relation that holds of a probe P and a goal G. Not every matching pair induces Agree. To do so, G must (at least) be in the <domain> D(P) of P and satisfy locality conditions.

REMEMBER, THE NOTION OF 'DOMAIN OF P' IS WHAT USED TO BE THE 'COMPLEMENT DOMAIN' IN CHAPTER 3. LOCALITY CONDITIONS ARE WHATEVER

THOSE ARE, SAY MLC AND PERHAPS OTHERS.

The simplest assumptions for the probe-goal system are (40):

(40)
(I) matching is feature identity
(II) D(P) is the sister of P
(III) locality reduces to "closest c-command"

I SEE THAT THESE ARE THE ASSUMPTIONS THAT *WORK*, BUT ARE THEY ALSO THE SIMPLEST, TO SATISFY MINIMALIST SCRUPLES? (I) IS ARGUABLY SIMPLEST, AS IS (II). (III) IS TRICKY, THOUGH: THE C-COMMAND DOMAIN ALSO INCLUDES SITUATIONS WHERE THE GOAL IS INSIDE, SAY, A SUBJECT OR AN ADJUNCT TO SOMETHING IN D(P)--BUT THOSE NEVER WORK (CED EFFECTS). I ACTUALLY THINK THERE IS A MORE NATURAL NOTION THAN C-COMMAND: NAMELY 'SAME DERIVATIONAL WORKSPACE'. WE SAID THIS BEFORE: ADJUNCTS AND SUBJECTS INVOLVE SEPARATE DERIVATIONAL WORKSPACES; IF SO, IT WOULD NOT BE UNTHINKABLE THAT THE SYSTEM SHOULD BE SENSITIVE TO CLOSENESS WITHIN THE SAME DERIVATIONAL SPACE, WHICH IS LIKE SAYING THAT TWO RACERS COMPETE IF THEY ARE RACING IN THE SAME STADIUM, BUT NOT IF THEY ARE IN DIFFERENT STADIUMS--EVEN IF THOSE STADIUMS END UP JOINING THEIR TRACKS CLOSE TO THE FINISH LINE, SAY. AT ANY RATE, THE GENERAL REASONING CHOMSKY IS TRYING TO MAKE WORKS, I THINK, IF WE NARROW THINGS DOWN FROM THE UNMOTIVATED C-COMMAND (WE SAW BEFORE THAT THE COMPOSITE OF SISTER AND THE TRANSITIVE CLOSURE OF CONTAIN IS NOT A PARTICULARLY ELEGANT NOTION) TO THE MORE NATURAL 'SAME DERIVATIONAL SPACE.' I'LL USE THE TERM 'DERIVATIONAL CASCADE' TO REFER TO WHAT GOES ON WITHIN A GIVEN DERIVATIONAL WORKSPACE, PARTICULARLY AS IT BLEEDS INTO THE INTERPRETIVE COMPONENTS.

Thus D(P) is the c-command domain of P, and a matching feature G is <closest to> P if there is no G' in D(P) matching P s.t. G is in D(G').

THE SAME HOLDS FOR THE NARROWER, MORE MOTIVATED VARIANT I'VE SUGGESTED.

In the absence of evidence to the contrary, we adopt (40), with a qualification taken over from earlier work:

{See <MP>, Ura (1996). The condition stated there refers only to SPECs of the same head. Whether the generalization (41) is appropriate depends on answers to questions about the structure of more complex constructions (double-object verbs, etc.). These and other questions, including parametrization, are put aside here. See Boskovic (1997), McGinnis (1998), among others.

ANOTHER PAPER TO SEE IN THIS RESPECT IS ORMAZABAL'S (1999).

The notion "feature occurrence" used implicitly here for expository convenience can be eliminated by restatement in terms of the heads to which features belong.}

TRUE, BUT A DEEP ISSUE REMAINS: ARE THERE ANY OCCURRENCES OF FEATURES, AND IF NOT WHY NOT? ANOTHER WAY TO SAY THIS IS WHY ARE FEATURES DIFFERENT FROM CATEGORIES, THE LATTER CLEARLY ENTERING INTO VARIOUS OCCURRENCES AS THE DERIVATIONS PROCEDE? YOU CAN OF COURSE STIPULATE THAT DERIVATIONS ONLY MANIPULATE CATEGORIES, BUT YOU WANT TO UNDERSTAND WHY THINGS ARE THAT WAY, WHAT IS NECESSARY ABOUT THAT. FOR INSTANCE, COULD IT BE THAT THE INTENTIONAL SYSTEM CAN ONLY MANIPULATE CATEGORIES, NOT MERE FEATURES FROM THE CONCEPTUAL SYSTEM? I'M NOT SAYING THAT'S THE ANSWER, BUT RATHER THAT YOU'D NEED AN ANSWER OF THAT SORT.

(41) Terms of the same minimal domain are "equidistant" to probes

NOTICE, ALSO, THAT JUST AS WE HAD NO REASON FOR THIS IN CHAPTER 3, WE STILL DON'T HAVE IT IN THIS ARTICLE. THIS IS A GENERALIZATION THAT MUST BE UNDERSTOOD TO FOLLOW FROM SOMETHING ELSE, NOT ANYTHING OBVIOUS. IT SEEMS AS IF FOR THE PURPOSES OF THE DERIVATION DISTANCE COLLAPSES WITHIN A GIVEN CHUNK, AS IF THE INNER STRUCTURE OF THIS CHUNK WAS IRRELEVANT. OBVIOUSLY, THAT'S ANOTHER POCKET OF REGULARITY WHERE YOU EXPECT CONSERVATIONS TO TAKE PLACE (IN THIS INSTANCE, PERHAPS L-RELATEDNESS).

The minimal domain of a head H is the set of terms immediately contained in projections of H.

AS BEFORE, THIS IS A SET-THEORETIC OBJECT, WHICH CONFIRMS THE IDEA THAT THIS IS A NEW DOMAIN OF CONSERVATION, SEPARATE FROM CONFIGURATIONAL MATTERS--THAT IS, A HIGHER ORDER OBJECT BUILT FROM A PHRASAL OBJECT. SO FAR, WE HAVE SEEN THE FOLLOWING DOMAINS OF CONSERVATION: WORDS, PHRASES, MINIMAL DOMAINS, AND POSSIBLY ALSO NUMERATIONS (MORE ON THIS BELOW, WHERE THE ISSUE BECOMES CLEARER). TO SERIOUSLY IMPLEMENT THE IDEA THAT CHAINS ARE OBJECTS IN DIFFERENT POSITIONS AT THE SAME TIME, THEN YOU ALSO NEED A FIFHT DOMAIN OF CONSERVATION, A CHAIN LEVEL ONE. I SHOULD EMPHASIZE THAT I'M NOT ADDING ANYTHING HERE, I'M JUST TRYING TO CODE IN REGULAR WAYS THE ASSUMPTIONS THAT CHOMSKY HAS BEEN MAKING. THAT IS, YOU CANNOT JUST ASSUME (40) AND GO TO THE NEXT PAGE; IN EFFECT, WHAT YOU'VE ASSUMED IS THE EXISTENCE OF A KIND OF ENTITY WHERE INNER PHRASAL STRUCTURE IS IRRELEVANT. SIMILARLY WITH ALL THE OTHER DOMAINS WE'VE BEEN ASSUMING, WHERE CERTAIN INTEGRITY HOLDS; FOR INSTANCE, WITHIN WORDS YOU DON'T CARE ABOUT LCA, SAY, AND WITHIN CHAINS YOU MUST MAKE SURE THAT ONLY ONE OF THE CHAIN SITES GETS INTERPRETED, AND SO ON. YOU CAN EITHER DO THIS MECHANICALLY WITHOUT OBSERVING WHAT'S COMMON TO

ALL THESE INSTANCES, OR YOU CAN CHOOSE TO REFLECT ON THE COMMONALITIES. THE REASON I USE THE CONSERVATION METAPHOR IS TWOFOLD. FIRST, IN PHYSICS A SIMILAR METHODOLOGY WAS USEFUL WHEN PEOPLE WERE TRYING TO SORT OUT THE BEHAVIOR OF NEW PARTICLES AND THEIR INTERACTIONS, THE RESULT OF WHICH WAS GELMAN'S QUARK MODEL. SECOND, AND MOST IMPORTANTLY, IT SEEMS AS IF WE DO WANT TO CONSERVE CERTAIN PATTERNS IN THE DOMAINS I'M ASSUMING, EVEN IF WE DO IT TACITLY. WE'VE SEEN THIS FOR THE TOKEN-TO-CHAIN MAPPING, AND IF, FOR INSTANCE, L-RELATEDNESS IS WHAT CHARACTERIZES THE MINIMAL DOMAIN ESSENCE, THEN THE FACT THAT WE PRESERVE L-RELATEDNESS THRHOUGHOUT THE DERIVATION WOULD BE THE OBVIOUS RAISON D'ETRE OF MINIMAL DOMAINS--AND WITH IT WOULD POSSIBLY COME YOUR DEFINITION OF A VS. A' MOVEMENT, ANOTHER MANIFESTATION OF THE CONSERVATION OF L-RELATEDNESS. IT IS WORTH POINTING OUT ALSO THAT THIS ARCHITECTURE IS PARADIGMATIC, IN THE SENSE THAT IT ESTABLISHES VERTICAL CUTS ON THE SYSTEM, EACH OF WHICH IS PROGRESSIVELY DEFINED ON THE PREVIOUS. FOR INSTANCE, CHAINS ARE NECESSARILY DEFINED IN TERMS OF PHRASES, BUT NOT THE OTHER WAY AROUND, AND PHRASES IN TERMS OF WORDS, AND NOT VICE-VERSA. THIS DIMENSIONAL TAKE ON THE LINGUISTIC ARCHITECTURE (WHICH IS OF COURSE FULLY COMPATIBLE WITH THE HORIZONTAL, OR SINTAGMATIC CUT THAT WE USUALLY TAKE) WILL BE OF CERTAIN IMPORTANCE LATER ON IN THE SEMESTER.

With matching restricted to identity, Case and lexical category cannot enter into Agree or Move, since the probes do not manifest these features.

THAT IS, YOU DO NOT FIND NOMINATIVE LOOKING FEATURES, SAY, IN TENSE; WHAT YOU TYPICALLY FIND INSTEAD IS THE ABILITY TO RELATE TO NOMINATIVE FEATURES IN A NOMINAL. AND THE SAME WITH PREPOSITIONS AND, AS FAR AS I KNOW, THE REST--FOR CASE. SIMILAR CONSIDERATIONS OBTAIN FOR CATEGORIAL FEATURES. THEN IT FOLLOWS THAT WHAT GEARS MOVEMENT/AGREE IS NOT CASE OR CATEGORIAL DEPENDENCY, BUT JUST AGREEMENT. THE SAME IS TRUE FOR OBJECTS:

And Object Shift must be for (here invisible) object agreement, with ancillary Case checking.

If uninterpretable features serve to implement operations, we expect that it is structural Case that enables the closest goal G to select P(G) to satisfy EPP by Merge.

THAT IS, SOME GOAL G ENTERS INTO AGREEMENT WITH SOME PROBE P IN A CATEGORY K (WHEREBY P(=G) ERASES); THEN THE IDEA IS THAT ALL CASE DOES IS SELECT OR (SINCE, AGAIN, I WOULDN'T USE THAT TERM, I'LL SAY:) PICK OUT A PHRASE P(G) TO SATISFY EPP IN THE DOMAIN OF K BY COPY+MERGE. CASE IS JUST THE FRAME THAT TELLS YOU WHAT WILL SATISFY EPP IN THIS LANGUAGE--HOW, WE DON'T KNOW YET.

Thus, if structural Case has already been checked (deleted), the phrase P(G) is "frozen in place," unable to move further to satisfy EPP in a higher position.

MECHANICALLY, THIS WILL BE IMPORTANT, PREDICTING A NUMBER OF INSTANCES WHERE NPS ARE STUCK AFTER THEY CHECK CASE, ERASING THE CORRESPONDING CASE FEATURE (I WOULD HAVE USED THE WORD 'ERASE' HERE TOO, INSTEAD OF THE CONFUSING TERM 'DELETE'). HERE'S ALSO A MECHANICALLY CONFUSING ISSUE. WHEN P AND G ENTER INTO A CHECKING RELATION, WHERE P=G, IT IS P THAT ERASES (SUICIDAL GREED). WHAT ABOUT CASE THEN? WELL, CASE IS A FEATURE OF P(G), A PHRASE CONTAINING G--AND IT TOO ERASES. THE ONLY FEATURE THAT REMAINS IS G. YOU CAN THINK OF THE FREEZING EFFECT AS FOLLOWS: SINCE CASE IS GONE FROM P(G), IF I TRIED TO MATCH G (WHICH IS NOT ERASED, REMEMBER) TO SOME HIGHER P' LATER ON, I WOULDN'T BE ABLE TO SATISFY EPP IN TERMS OF MOVING THE PHRASE P(G), SINCE I HAVE NO CASE FEATURE TO MAKE IT ACCESSIBLE TO THE SYSTEM.

More generally, uninterpretable features render the goal <active>, able to implement an operation: to select a phrase for Merge (pied-piping) or to delete the probe.

THIS IS GOING TO BE THE GENERAL LINE FOR *ALL* UNINTERPRETABLE FEATURES (FOR INSTANCE, A SIMILAR MECHANISM WILL BE POSTULATED FOR WH-MOVEMENT). AN ACTIVE GOAL IS, FIRST, ONE THAT LETS YOU PICK OUT A PHRASE FOR EPP. BUT CHOMSKY SEEMS TO BE SUGGESTING HERE, ALSO, THAT IT MAY BE ALLOWING YOU TO DELETE THE PROBE AS WELL; THAT CLEARLY WOULD NOT BE THE CASE OF CASE, SINCE YOU'LL NEVER FIND AN IDENTICAL CASE FEATURE UPSTAIRS--BUT COULD IT BE THE CASE OF A WH-FEATURE? PERHAPS, IT DEPENDS ON HOW YOU INTERPRET WH-FEATURES, AS INTERPRETABLE (AS IN CHAPTER 4) OR UNINTERPRETABLE (AS HERE).

The operations Agree and Move require a goal that is both local and active.

LOCALITY WE'VE ASSUMED ALL ALONG, BUT NOW WE'RE ADDING ACTIVITY. I SHOULD SAY, PERHAPS, THAT WHILE THIS INTERPRETATION OF CASE IS ENTIRELY PLAUSIBLE, IT IS BY NO MEANS OBVIOUSLY NECESSARY, UNLESS WE MAKE SOME NON-TRIVIAL ASSUMPTIONS ABOUT PHRASES. LET'S THINK ABOUT THIS FOR A SECOND. PHRASE-MARKERS, YOU RECALL, ARE OBJECTS OF THE FORM {A, {B, C}}, WHERE TERMS B AND C CAN BE ARBITRARILY COMPLEX, AND LABEL A CAN BE THOUGHT OF AS A GENERALIZATION, NOTHING OBVIOUSLY REAL TO THE SYSTEM--CERTAINLY NOT ANYTHING ACCESSIBLE TO IT. IF SO, IMAGINE A PHRASE-MARKER WHERE C CONTAINS A COMPLEX SET OF TERMS. HOW DOES THE SYSTEM KNOW THAT IT IS SUPPOSED TO WORK WITH A CERTAIN CRUCIAL TERM P(G)? FOR THE PROBE-GOAL RELATION THE ISSUE IS NOT SO TROUBLING IF P JUST SEEKS SOMETHING LIKE IT ITSELF. BUT WHAT IS SOUGHT FOR EPP FEATURES? DOES THE SYSTEM HAVE THE ABILITY TO LOOK FOR SOMETHING LIKE 'THE MINIMAL VALID PHRASE THAT CONTAINS G?' PERHAPS IT

DOES, ALTHOUGH THAT'S A HIGHLY ARTICULATED REASONING. PERHAPS, INSTEAD, THE SYSTEM JUST SEEKS 'A STUPID UNINTERPRETABLE CODING'. THAT WOULD BE CASE. SO IT ALL DEPENDS ON WHAT KIND OF INTELLIGENCE YOU'RE WILLING TO GIVE THE SYSTEM. (AS USUAL, AND AS IT SHOULD BE FOR THIS KIND OF NATURAL SYSTEM, WE'RE GOING WITH THE DUMBEST PROCESS THAT DOES THE TRICK.)

{Among the problems that arise is the status of scrambling. The logic would suggest that for at least some cases, a scrambling feature induces pied-piping even after Case assignment, with the pied-piped element "attracted" by a higher probe,

THAT IS, GIVEN THAT SOME INSTANCES OF SCRAMBLING SEEM TO INVOLVE MOVEMENT AFTER CASE HAS ALREADY BEEN CHECKED, IF THEY ARE TO REDUCE TO THE PRESENT SYSTEM, ONE WOULD HAVE TO POSTULATE SOME KIND OF SCRAMBLING FEATURE THAT DOES THE JOB OF CASE. THIS IS SOMEWHAT DUBIOUS, IN THAT, SO FAR AS I KNOW, THE RELEVANT SCRAMBLING FEATURE HAS NEVER BEEN ATTESTED--AND IS PROPOSED HERE ONLY IN TERMS OF THE LOGIC.

while other cases fall into a category distinct from feature-driven movement.

THAT IS INDEED MORE PLAUSIBLE, SUGGESTING SCRAMBLING FALLS TOGETHER WITH OTHER INFORMATION-DRIVEN, NON-INTENTIONAL OPERATIONS.

For exploration of alternatives in a comparative study, see Sauerland (1998a).}

We therefore have the possibility of <defective intervention constraints> in a structure (42), where > is c-command, ß and _ match the probe _, but ß is inactive so that the effects of matching are blocked:

(42) _ > ß > _

THAT IS, THE FACT THAT ß IS INACTIVE BECAUSE ITS CASE HAS BEEN CHECKED RAISES THE QUESTION: DO WE THEN HAVE A RELATION BETWEEN _ AND _ WHICH WOULD OTHERWISE BE BLOCKED? CHOMSKY THINKS WE DO. A RELATED EXAMPLE TO THINK ABOUT WOULD BE THIS: IMAGINE ß PROBES INTO _, AND FINDS IT AS ITS GOAL, WITH ß THEN DELETING AND P(_) DOING ITS BUSINESS EPP-WISE, THUS ELIMINATING THE CORRESPONDING CASE FEATURE IN _; DOES THAT MEAN, THEN, THAT _ COULD NOT BE A VALID GOAL FOR A HIGHER _, EVEN IF _ SATISFIES ITS EPP FEATURE DIFFERENTLY? IF I READ HIM CORRECTLY, CHOMSKY THINKS NOT. IF SO, THIS WOULD BE RATHER STRONG EVIDENCE FOR THE IDEA THAT ACTIVITY IS AN ISSUE IN ALLOWING LONG-DISTANCE OPERATIONS. THAT *WOULD* BE RATHER DIFFERENT FROM THE R&U THEORY, SINCE THERE CASE WAS NOT WHAT SANCTIONED THE AGREEMENT RELATION, BUT RATHER SOMETHING THAT WAS ASSIGNED IN THE RELEVANT CONFIGURATIONS. IN ANY CASE:

We return to some illustrations.{For a similar configuration in phonology, with > = linear order, see the discussion in Halle (1995) of coronal assimilation in Sanskrit, barred by intervening (non-assimilating) coronal.}

In <MP>, Agree is analyzed in terms of feature-movement (Attract) and a concept of matching that is left unclear. Here we take matching to be identity and dispense with Attract, with complications it induces about extended MLIs, feature chains, and other matters. Checking reduces to deletion under matching with an active local goal and ancillary deletion of the uninterpretable feature that rendered the goal active. I will use the terms "checking" and "attract" only for convenience.

THIS IS PRETTY MUCH A SUMMARY OF WHAT WE'VE DONE SO FAR, AND ONE OF THE MAIN CHANGES FROM CHAPTER 4.

Suppose that the EPP feature of T could be satisfied more simply than by the full operation Move.

HERE WE GO INTO EXPLETIVES, AND NOTE THE FAST, THOUGH ACCURATE ASSUMPTION: MERGE IS SIMPLER THAN MOVE, SO IF YOU COULD SATISFY EPP BY MERGE, THAT DERIVATION WINS OVER ITS ALTERNATIVE, WHICH HAS TO INVOLVE MERGE PLUS COPY (AND POSSIBLY DELETION, DEPENDING ON HOW YOU SEE CHAINS).

That is the case in (35ii), repeated here:

(43) there [<_> T-was elected an unpopular candidate]

Here the lexical array includes the expletive <there>.

THAT IS A CRUCIAL ASSUMPTION, OR ELSE YOU WOULD NEVER GET an unpopular candidate was elected, WHICH IS POSSIBLE IN SPITE OF INVOLVING MOVEMENT BECAUSE IT DOES NOT COMPETE WITH A DERIVATION INVOLVING AN EXPLETIVE.

At stage _ of the derivation (= (39)), the independent operations Agree and pure Merge suffice: Agree deletes the _-set of T and the structural Case of <candidate>, and Merge (of <there>) satisfies the EPP feature of T. The more complex operation Move is preempted; dislocation does not take place, though we have long-distance agreement of T and its goal (its associate).

{On the assumptions of <MP> 4.10, multiple-subject (including transitive expletive) options are parametrized in terms of deletion of EPP-features. See particularly Ura (1996), and for skepticism about the option, Zwart (1997).

IN ANY CASE, THE ISSUE IS REAL, HOWEVER YOU CODE IT. EXPLETIVES IN ENGLISH APPEAR ONLY (BASICALLY) WITH UNACCUSATIVES AND PASSIVES,

WHEREAS THEY GO WITH TRANSITIVES IN MANY OTHER LANGUAGES. URA'S POINT WAS THAT THIS CORRELATES WITH MULTIPLE SUBJECTS OF THE SORT SEEN IN EAST ASIAN LANGUAGES. THE EXTENSION TO GERMANIC LANGUAGES, HOWEVER, IS FAR FROM OBVIOUS (IT DOESN'T SEEM AS IF THESE HAVE BONA FIDE MULTIPLE SUBJECTS, PUTTING ASIDE EXPLETIVES).

Agreement in the sense discussed here is to be distinguished from concord, with different properties.}

WE ALREADY SAID THIS BEFORE. CONCORD, FOR INSTANCE, IS WHAT GOES ON BETWEEN A NOUN AND ITS ADJECTIVES--THOUGH THE ISSUES ARE VERY MURKY.

Manifestation of structural Case depends on interpretable features of the probe: finite T (nominative), <v> (accusative), control T (null), on our earlier assumptions.

AGAIN, THIS SEEMS TO BE TRUE, BUT WOULDN'T FOLLOW FROM ANYTHING SAID SO FAR. THAT IS, THERE IS NOTHING LOGICALLY INCONCEIVABLE ABOUT FL MARKING ALL GOALS IN TERMS OF A SINGLE, DESIGNATED CASE.

We may therefore regard structural Case as a single undifferentiated feature.

INDEED, THIS WILL BE CRUCIAL ON EMPIRICAL GROUNDS, GIVEN THAT WE WANT DIFFERENT CASES TO COUNT FOR INTERVENTION PURPOSES.

The same would be expected for the uninterpretable _-set of the probe. Its manifestation depends on interpretable features (namely, _-features) of the goal, so that it too can be taken to be undifferentiated as to the value of the individual features of the _-set ([+/-plural], etc.).

UNFORTUNATELY, THE COMPARISON IS NOT TOTALLY FAIR. THAT IS, THE FACT THAT SOMETHING MANIFESTS ITSELF AS PLURAL OR MASCULINE DOES SEEM TO DEPEND ON GENUINE INTERPRETIVE FEATURES OF THE CATEGORY THAT BEARS THOSE FEATURES, AS MANIFESTED IN THE SEMANTICS. CASE IS VERY DIFFERENT. THERE IS NO SUCH THING AS A SEMANTIC NEED FOR T TO BEAR NOMINATIVE CASE. SO A MYSTERY REMAINS: WHY WE HAVE DIFFERENT CASE *VALUES*, EVEN IF WE ACCEPT THE NEED FOR CASE FEATURES IN TERMS OF ACTIVITY.

For both probe and goal, the form of the uninterpretable features is determined by Agree. To rephrase in traditional terms, verbs agree with nouns, not conversely, and Case is assigned.

THE TRADITIONAL OBSERVATION SEEMS CORRECT, AND PRESENT CONJECTURES CERTAINLY ARE ON TRACK WITH REGARDS TO AGREEMENT, BUT THE FACT THAT 'CASE IS ASSIGNED', WHILE TRUE DOESN'T SEEM TO FOLLOW.

We therefore understand "feature identity" in (I) of (40) to be identity of the choice of feature,

not of value.

OF COURSE, THAT'S CRUCIAL, AND WHAT WE'RE SAYING. ANOTHER WAY OF PUTTING THIS IS AS FOLLOWS: THERE IS NO SUCH THING AS AN ACCUSATIVE FEATURE, ANYMORE THAN THERE IS A PLUS FEATURE. ACCUSATIVE IS A VALUE OF A CASE FEATURE, JUST LIKE PLUS IS A VALUE OF A NUMBER FEATURE. FEATURE IDENTITY, THUS, IS NOT SENSITIVE TO HOW THE FEATURE MANIFESTS ITSELF. THIS IS LIKE SAYING THAT THE COMPUTATIONAL SYSTEM DOESN'T MANIPULATE SUBSTANTIVE INSTANCIATIONS OF THE CONCEPTUAL SYSTEM, BUT MERELY ITS GENERAL PARAMETERS. THIS IS VERY PLAUSIBLE. JUST AS THE GRAMMAR DOESN'T MANIPULATE VALUES OF SYNTACTIC PARAMETERS (THERE IS NO PRINCIPLE OF GRAMMAR THAT DEPENDS ON WHETHER YOU ARE OR ARE NOT PRO-DROP), SO TOO THE GRAMMAR DOESN'T MANIPULATE VALUES OF CONCEPTUAL PARAMETERS INVOLVED IN LEXICAL CHARACTERIZATION. SYNTAX IS PURELY FORMAL, AND HAS NO WAY OF KNOWING, FOR INSTANCE, WHETHER IT IS OPERATING WITH he OR she IN she loves peanuts (OR FOR THAT MATTER, WITH loves AS OPPOSED TO hates, AND SO ON). IF THIS IS GENERALLY THE CASE, WE MIGHT HAVE A HINT HERE AS TO WHY WE HAVE CASE *VALUES* TO BEGIN WITH, FOR IN A SENTENCE LIKE he loves her THE GRAMMAR JUST SEES SOMETHING ROUGHLY LIKE [D [v [V D]]]. INDEED, IN THE NUMERATION YOU'D HAVE TWO D TOKENS, BUT HOW DO YOU CODE THAT? PERHAPS CASE VALUATION IS NOTHING BUT A WAY OF CODING THE D TOKENS, SO THAT IN THE NUMERATION YOU HAVE {[D-nom], [D-acc], V, v,...}, AND THEN IN THE SYNTAX YOU OPERATE WITH [D-nom [v [V D-acc]]]. THAT WOULD JUSTIFY WHY YOU HAVE DIFFERENT VALUES. OF COURSE, THAT STILL DOESN'T JUSTIFY WHY YOU HAVE THE ACCUSATIVE VALUE ASSOCIATED TO v AND THE NOMINATIVE VALUE ASSOCIATED TO T, AND SO ON. YOU MIGHT THINK THIS IS JUST NOMENCLATURE, BUT IT ISN'T. LANGUAGES DIFFER IN WHICH OF THESE THEY TAKE TO BE THE DEFAULT VALUE (E.G. IN TERMS OF CITATION FORMS OR TOPIC/DEFAULT CASE), AND ALSO IN HOW THEY DISTRIBUTE THESE CASE VALUES TO DIFFERENT ARGUMENTS. WE SHOULD RETURN TO THESE ISSUES.

More important, defective intervention effects are induced whether or not ß and _ of (42) are identical in _-feature value (singular blocks plural agreement, etc.).

THIS IS WHAT WE SAID ABOVE, AND ONE OF THE MAIN EMPIRICAL ARGUMENTS NOT TO CONSIDER A VALUED FEATURE WHAT IS RELEVANT IN SYNTAX; MORE GENERALLY (A POINT THAT GOES BACK TO BRESNAN), VALUED FEATURES NEVER DO ANYTHING FOR YOU: NO TRANSFORMATION OR ANY OTHER RULE IS GEARED TOWARDS VALUED FEATURES.

This lends theory-internal support to the earlier observation that _-features are interpretable only for N; their value is specified only in this case.

THIS IS A VERY INDIRECT ARGUMENT, SINCE UP TO NOW WE WERE TALKING ABOUT THE VISIBILITY OF FEATURE VALUES FOR THE SYNTAX IN GENERAL. THE

POINT IS: IN VERBS, YOU DON'T CARE ABOUT AGREEMENT VALUES, SINCE YOU GET INTERVENTION EFFECTS REGARDLESS OF VALUES. THEREFORE (?) INTERPRETABILITY OF VALUES IS ONLY IN N. THE THEREFORE, HOWEVER, DOESN'T LOGICALLY FOLLOW (EVEN IF IT SEEMS TRUE).

Notice also that only the most underspecified element, PRO, can have null Case,

THIS SOUNDS PROFOUND, BUT IT ISN'T. IT IS ARGUABLE THAT PRO IS THE MOST UNDERSPECIFIED ELEMENT, IF IT INDEED EXISTS, BUT IT ISN'T OBVIOUS EITHER THAT NULL CASE IS IN ANY SENSE UNDERSPECIFIED (IT'S JUST NOT PRONOUNCED, IF IT INDEED EXISTS), OR THAT THERE SHOULD BE A CORRELATION IN SPECIFICATION HERE. IF THAT CORRELATION HOLDS, IT HAS TO BE ASSUMED FROM SOMETHING ELSE, NOT SAID EXPLICITLY HERE. WE SHOULD RETURN TO THIS TOO.

so raising of _ ÿ PRO to SPEC-T crashes the derivation when T is a control infinitival.

OF COURSE, THAT'S WHAT WE WANT TO ACHIEVE, BUT LET'S NOT CONFUSE THE DESIRE WITH AN EXPLANATION.

We take deletion to be a "one fell swoop" operation, dealing with
the _-set as a unit.

AGAIN, THIS DOESN'T FOLLOW FROM ANYTHING OBVIOUS, ALTHOUGH IT SEEMS TRUE. YOU CAN BUILD THE GRAMMAR THAT WAY, OF COURSE, BUT YOU HAVE TO WONDER WHETHER IT IS NECESSARY.

Its features cannot selectively delete: either all or none. The _-features of T do not agree with different NPs, for example.

LET'S SEE IF WE CAN ACTUALLY BUILD A RELEVANT EXAMPLE. IMAGINE A LANGUAGE THAT HAS SUBJECT AND OBJECT AGREEMENT, WHERE YOU CAN ACTUALLY TEST THESE ISSUES. AND IMAGINE A TENSE THAT HAS AGREEMENT IN PERSON WITH THE SUBJECT AND AGREEMENT IN NUMBER WITH THE OBJECT. FOR EXAMPLE, IN BASQUE YOU CAN SAY Nik Miren maite dut, LITERALLY I-subject Miren-object love I/SG Aux III/SG. NOW IMAGINE THE FOLLOWING SENTENCE: Nik Miren maite naiz, THAT IS, LITERALLY AGAIN I-subject Miren-object love I/SG Aux. WHY COULDN'T I USE THIS FIRST PERSON SINGULAR AGREEMENT TO AGREE IN FIRST PERSON WITH THE SUBJECT AND IN NUMBER WITH THE OBJECT? IF I COULD DO THAT, I'D PROCEED TO CHECK THE SUBJECT AND OBJECT CASE FEATURES WITHOUT ANY PROBLEM. BUT THE SENTENCE IS CRASHINGLY UNGRAMMATICAL. IF NONE OF YOU FINDS A LOOPHOLE WITH MY ARGUMENT (AND NOTE THAT PRESUMABLY I AND Miren ARE BOTH WITHIN THE SAME MINIMAL DOMAIN), THEN THIS IS THE SORT OF EXAMPLE THAT PROVES CHOMSKY'S POINT.

In the same spirit, we assume that only a probe with a full complement of _-features is capable of deleting the feature that activates the matched goal.

THIS ONE IS EVEN MORE STIPULATION THAN THE PREVIOUS, EVEN IF RIGHT. WE NEED TO SAY IT, IN ORDER NOT TO ELIMINATE CASE FEATURES IN PLACES WHERE WE WANT THEM TO REMAIN ACTIVE; FOR INSTANCE:

Suppose that the probe for participial (like adjectival) _ is a _-set lacking the feature [person], and that G is the closest matching goal in its search space: P(G) = DP may be attracted to SPEC-_ deleting the probe of _ (participial agreement), but the operation will not delete structural Case in DP, which can move on to SPEC-T, deleting the probe of T and Case of DP (subject agreement).

LET'S ILLUSTRATE. TAKE THE ROMANCE ellos han sido arrestados, LITERALLY they have-III/pl. been arrested-pl. YOU WANT TO BE ABLE TO SAY THAT they MOVES TO A SITE WHERE IT CHECKS NUMBER AGREEMENT WITH THE PARTICIPIAL (WHICH THEN ERASES), BUT STILL CAN MOVE ON TO T IN ORDER TO CHECK THE FULL SET OF _-FEATURES (ALSO INCLUDING PERSON). THE LOGIC THEN DICTATES THAT THE CASE FEATURE IS STILL ACTIVATING THE SUBJECT, OR IT WOULDN'T MOVE ANY FURTHER.

<v> and nondefective T, with a full complement of _-features, delete the uninterpretable feature that activates the matched goal (raised or not).

THAT'S THE BOTTOM LINE, BUT AGAIN NOTHING PROFOUND SEEMS TO GRANT THIS. IT IS CERTAINLY THE CASE THAT, EMPIRICALLY, THOSE TWO (AND ALSO DATIVE SITES, WHICH I'M SURE WE COULD ADAPT TO THE PRESENT SYSTEM) IS WHERE PERSON FEATURES ARE ENCODED. IT THUS SEEMS AS IF THE PERSON-ENCODING SITES ARE CENTRAL IN ELIMINATING THE CASE FEATURES (INDEED, ORMAZABAL (1999) ARGUES THAT WHAT MATTERS IN ALL THESE SORTS OF INSTANCES IS NOT CASE, BUT PERSON INSTEAD, WHICH SEEMS LIKE A GOOD WAY OF BITING THE BULLET--EVEN IF THE MYSTERY IS STILL THERE: WHY DO THOSE SITES CODE PERSON AND WHY DO WE BOTHER TO CODE PERSON DISTINCTIONS?). IT IS PERHAPS WORTH MENTIONING, ALSO, THAT THE PERSON FEATURE SEEMS CRUCIAL IN 'LICENING' pro, AS ARGUED SOME TIME AGO BY GALVES, FOR INSTANCE. AS YOU PROBABLY KNOW, pro TYPICALLY MANIFESTS ITSELF IN CASE SITES, WHICH SUGGESTS A TIGHT CORRELATION PERSON/CASE/pro. WHETHER IT IS IN TERMS OF FULL SETS OF _-FEATURES, AS IN CHOMSKY'S SPECULATION, I DON'T KNOW.

{The analysis of structural Case is along the lines of George and Kornfilt (1981). As they observe, structural Case linked to _-features may be dissociated from finiteness.

A SIMILAR POINT WAS RAISED IN RAPOSO'S THESIS CONSIDERABLY BEFORE THAT (1973), AND AGAIN IN HIS LI ARTICLE ON INFLECTED INFINITIVALS.

Matters become more complex when we consider ergative/absolutive and mixed systems, and

languages in which _-features without finiteness do not suffice for nominative Case assignment (see Iatridou 1993).}
How would non-control infinitivals (T<def>) and weak expletives EXPL of the <there>-type fit into this picture? The former category falls into place if T always has at least a minimal feature complement, perhaps only [person] for T<def>. If so, Move of _ to SPEC-T<def> will delete the _-set of T (= uninterpretable [person]) but not the structural Case feature of _, so that _ can undergo further movement and agreement.

GIVEN WHAT I JUST SAID ABOUT PERSON, I FIND IT HARD TO BELIEVE THAT DEFECTIVE T HAS PRECISELY THE FEATURE THAT SEEMS SO INVOLVED IN THE CASE SYSTEM. INDEED, IN LANGUAGES WITH INFLECTED INFINITIVALS, SUCH AS PORTUGUESE, WHAT YOU SEEM TO BE ADDING TO THESE INSTANCES WITH DEFECTIVE T IS PRECISELY A PERSON FEATURE, AND THEN ALL OF A SUDDEN CASE LICENSING BECOMES POSSIBLE AND FURTHER RAISING IS IMPOSSIBLE. THIS IS JUST AS CHOMSKY WOULD WANT IT, BUT ONLY IF HE DROPS THE IN-PASSING ASSUMPTION THAT THE DEFECTIVE FEATURE IN QUESTION IS PERSON. OF COURSE, HE NEEDS A FEATURE IF HE WANTS TO MOTIVATE SUCCESSIVE-CYCLIC MOVEMENT, AND GIVEN THE LOGIC OF WHAT HE'S SAID SO FAR, THE FEATURE IN DEFECTIVE T CANNOT BE, OBVIOUSLY, A CASE FEATURE OR (LESS OBVIOUSLY, BUT WITH THE SAME REASONING) A CATEGORIAL FEATURE OF THE SORT HE INVOLVED IN CHAPTER 4. THERE IS NOT MUCH MORE LEFT: NUMBER AND GENDER FEATURES DON'T SEEM TO BE INVOLVED HERE, CONTRARY TO WHAT HAPPENS IN PARTICIPIALS, WHERE YOU FIND PRECISELY THOSE. SO EITHER THERE IS A NEW, MYSTERIOUS FEATURE IN THESE INSTANCES, OR THERE ISN'T SUCCESSIVE CYCLICITY HERE, OR ELSE SUCCESSIVE CYCLICITY IS AN ENTIRELY DIFFERENT PROCESS. WE'LL RETURN TO ALL OF THIS.

The phase heads <v>/C have no counterpart to T<def> with a reduced _-set, and therefore do not provide an "escape hatch" for successive-cyclic A-movement.

THE ASSUMPTIONS HERE ARE SEVERAL. FIRST THAT SUCCESSIVE CYCLIC MOVEMENT IS DONE THROUGH THE SPEC OF v, C, OR T (THIS MIGHT SEEM OBVIOUS, BUT AGAIN IS NOT NECESSARY). SECOND, THAT ONLY DEFECTIVE T HAS A REDUCED _-SET (THAT, WE CAN GRANT). THIRD, THAT MOVEMENT OF TYPE X ACROSS A REDUCED X-SET DOESN'T RESULT IN THE DE-ACTIVATION OF THE RELEVANT PHRASE (THIS IS THE NEW IDEA, AN INTERESTING ONE, BUT ONE THAT HAS TO BE DERIVED WITHIN THE SYSTEM). IF YOU MAKE THESE ASSUMPTIONS, THE NORMAL ESCAPE HATCH FOR A-MOVEMENT WILL BE THE DEFECTIVE T (ON SIMILAR GROUNDS, v AND C WILL BE THE ESCAPE HATCHES FOR A' MOVEMENT, AT LEAST THOSE v'S AND C'S THAT HAVE DEFECTIVE A'-FEATURES, WHATEVER THOSE ARE).

Weak EXPL shares the basic movement/attraction properties of nominals. That is expected if EXPL has an uninterpretable feature F that activates it until erased and a _-set G that matches a probe in T. But G is uninterpretable for EXPL, so a distinct F is unnecessary, obviating the need for structural Case in EXPL.

SO BY THE FACT THAT THE EXPLETIVE IS AN EXPLETIVE, IT DOESN'T NEED CASE--THIS IS CLEVER, OF COURSE.

The composition of G is determined by two conditions: (A) EXPL can raise to SPEC-T<def>; (B) EXPL cannot delete the probe of nondefective T.

THESE ARE EMPIRICAL CONDITIONS. A) IS THE STANDARD ASSUMPTION, AND B IS NEEDED, I BELIEVE, IN ORDER TO INSTANTIATE THE EXPLETIVE/ASSOCIATE RELATION.

(A) requires that G contain a feature to match the probe of T<def> ([person], if what precedes is correct).

OR WHATEVER, IF WHAT PRECEDES IS NOT. IN FACT, IT SEEMS BIZARRE TO CLAIM THAT AN EXPLETIVE HAS A PERSON FEATURE.

From (B) it follows that G must be less than a full _-set, hence optimally just [person]. That (B) holds is shown by long distance agreement structures such as (44ii-iv), (ii) surfacing commonly as (iii), or in English more naturally as (iv), as noted:

(44)
(i) they declared [three men guilty]
(ii) there were declared [three men guilty]
(iii) there were declared guilty three men
(iv) there were three men declared guilty

If the matching feature of the probe were deleted by the operation, it would not be available for associate matching and the nominative Case of the associate would remain unchecked because of the lack of a full complement of features in T (compare participial agreement).

THIS IS WHAT I WAS SAYING--SO (B) IS EMPIRICAL.

The problem does not arise if (B) holds and uninterpretable features delete in an "all or none" fashion, not selectively. In (44ii-iv), the full complement of _-features of T deletes the uninterpretable feature G of <there>, barring further raising. When EXPL raises to SPEC-T<def>, the probe (a single feature) deletes under matching as before, but G does not, because deletion requires matching with a full complement of _-features of the probe.

THIS IS THE KEY, OF COURSE.

Therefore successive-cyclic raising through SPEC-T<def> is possible.

{For a different approach, on the assumption that <there> has structural Case, see Lasnik (1995a). For a different perspective on a wide range of related issues, see Moro (1997).}

Reinterpretation of Attract in terms of Agree eliminates the need to introduce "checking domains." That is a step forward.

FINE, BUT REMEMBER WE'RE STILL USING THE NOTION MINIMAL DOMAIN TO DETERMINE EQUIDISTANCE. MINIMAL DOMAINS, OF COURSE, WHERE THE UNION OF CHECKING DOMAINS AND INTERNAL DOMAINS; INDEED, AT THE TIME WE DEFINED THEM, WE THOUGHT OF CHECKING DOMAINS AS 'THE REST', THEREFORE A SORT OF EMERGENT DOMAIN WHICH YOU HAVE GIVEN THE NATURAL NOTION 'MINIMAL DOMAIN' (UNDERSTOOD IN THE SENSE OF A POCKET OF L-RELATEDNESS REGULARITY WITHIN THE PROJECTION OF A CATEGORY) AND THE EQUALLY NATURAL NOTION 'INTERNAL DOMAIN' (UNDERSTOOD IN THE SENSE OF A POCKET OF THETA REGULARITY IN THE HEAD-COMPLEMENT CONFIGURATION).

The notion is complex, and furthermore unnatural in minimalist terms;

AS I SAY, THAT DEPENDS ON WHAT YOU MAKE OF MY PREVIOUS PARAGRAPH.

feature-checking should involve features, nothing more, and there is no simpler relation than identity. More important, the notion is irrelevant for the core cases: elements merge in checking domains for reasons independent of feature checking; and feature-checking takes place without dislocation to a checking domain.

THAT IS A WELL-TAKEN POINT, AND THE MAIN REASON WHY IT IS WORTH PURSUING THESE MATTERS.

As discussed, both properties are illustrated in expletive constructions. Much work on the topic has taken long-distance effects to be a property of these constructions, hence of an expletive-associate relation; various ideas have been explored as to how that relation is established. In chap. 4 of <MP>, a different approach is suggested: the long-distance effects are attributed to an T-associate relation that involves features only and is independent of the expletive. The reasons were theory-internal, but a broader range of cases adds empirical support. Long-distance effects are found without expletives in such constructions as (15i),

THOSE WERE INSTANCES OF QUIRKY DATIVE RAISING, SAY IN PSYCH CONSTRUCTIONS IN THE LANGUAGES THAT ALLOW THEM.

{Similar conclusions are supported by locative and quotative inversion (see Collins 1997), though with restrictions and complications, and similarities to other poorly understood constructions (e.g., "still unclear remain (are, seem to be) the answers to those questions").}

THESE ARE TRULY FASCINATING, SINCE OBVIOUSLY THEY DON'T INVOLVE AN EXPLETIVE, AND YET THEY PRESENT AGREEMENT-ASSOCIATE RELATIONS.

with EPP satisfied by raising of quirky Case; and expletive subjects are found without T-associate agreement when there is no accessible nominative.

THAT'S THE OTHER HALF OF THE COIN, SHOWING THAT EXPLETIVE/ASSOCIATE IS ONE THING, AND AGREEMENT/ASSOCIATE IS A DIFFERENT THING.

We return to some illustrations. The general conclusions are (45):

(45)(i) Long-distance agreement is a T-associate (probe-goal) relation

(ii) EPP can be satisfied by:
(a) Merge of expletive
(b) Merge of associate
(c) Merge of _ closer to T than the associate

Case (a) is illustrated by T-associate agreement, with the definiteness effect.

YOU CAN IN FACT USE THAT EFFECT TO TEST WHETHER YOU HAVE THIS KIND OF RELATION, PARTICULARLY IF THE LANGUAGE DOESN'T HAVE OVERT EXPLETIVES. WE HAVEN'T SAID MUCH OF WHAT PREDICTS THE DEFINITENESS EFFECT, BUT WE'LL RETURN TO THAT.

Case (b) exhibits agreement of SPEC-T and T, but that is ancillary to the T-associate relation.

THAT IS, THE ASSOCIATE OF T HAS MOVED THERE FORE EPP REASONS.

In case (c) there is no definiteness effect and long distance T-associate agreement holds with embedded accessible nominative; or, if such an associate is lacking, T is default.

WE HAVEN'T SEEN THIS SORT OF EXAMPLE, I DON'T THINK, BUT IT WOULD BE ONE WHERE T MANAGES TO AGREE DOWN WITH SOME NOMINATIVE ELEEMENT, AND A CLOSER ELEMENT HIGHER UP IN THE TREE, OR PERHAPS MORE STUFF IN THE NUMERATION, MERGES AT EPP LEVEL; THEN YOU WOULDN'T HAVE A DEFINITENESS EFFECT SINCE THERE IS NO EXPLETIVE. I'D SAY THAT LOCATIVE INVERSION IN ENGLISH, OR THOSE INSTANCE WE JUST SAW INVOLVING A PREPOSITIONAL PHRASE, MIGHT BE OF THE RELEVANT SORT (SO all of a sudden came Johnny with his gun). YOU MIGHT ALSO GET DEFAULT AGREEMENT, ALTHOUGH I'M NOT SURE HOW YOU GET RID OF THAT IF IT IS UNINTERPRETABLE.

More generally, we should not expect SPEC-head relations to have any special status. Within bare phrase structure, we cannot, for example, take the result of first Merge to _ to be sometimes a specifier and sometimes a complement, as in an X-bar-theoretic analysis that takes the object of _ to be its complement ("see John," "proud [of John]") but the subject of objectless _ to be its specifier (base forms of "John eat," "John proud").

WELL, OF COURSE, BUT THAT WAS NEVER REALLY TAKEN TO BE AN ISSUE BEFORE, ASSUMING THE HALE/KAYSER LINE FOR UNERGATIVES. I'M NOT SURE, EITHER, THAT THE ISSUE WAS EVER, FOR CHECKING DOMAINS, HEAD/SPECS, BUT RATHER 'WHAT REMAINS FROM MINIMAL DOMAINS ONCE YOU TAKE AWAY INTERNAL DOMAINS', SO A TOTALLY DERIVATIVE NOTION. THAT WAY WHAT YOU COULDN'T DISTINGUISH WAS HEAD ADJUNCTION FROM SPECS (UNIFORMITY AND SIMILAR CONSIDERATIONS DICTATING WHETHER SOMETHING HAD TO BE A HEAD OR NOT), BUT THAT WAS TAKEN TO BE A VIRTUE OF THE PREVIOUS SYSTEM.

The restriction to single specifier is also questionable: rather, we would expect first Merge, second Merge, etc., with no stipulated limit.

TRUE, BUT THAT TOO SEEMS ORTHOGONAL. WHETHER A SINGLE SPECIFIER EXISTS IS SOMETHING AKIN TO SAYING WHETHER THE SYSTEM SHOULD RECOGNICE last Merge, AND IN ANY CASE IS AN EMPIRICAL ISSUE. WHICH IS ALL TO SAY, CONCEPTUALLY WE'VE SIMPLIFIED SOME THINGS, BUT IT'S NOT OBVIOUS THAT THE SYSTEM IS REMARKABLY BETTER. EMPIRICALLY, HOWEVER, WE HAVE IMPROVED IT IN QUITE INTERESTING WAYS, I BELIEVE.