version of (3b), without the contextual restriction (adjacent to a subject NP).

A transformational movement rule consists of an elementary operation (substitution or adjunction) that can be further specified in terms of a structural description and a structural change. The structural description indicates the special properties of a phrase-marker to which the elementary operation applies, and the structural change indicates how the elementary operation modifies the phrase-marker. The elementary operation is part of universal grammar (UG). Any language whose grammar involves movement operations involves one of these elementary operations. The structural description of a transformation is a language-specific condition on the application of the elementary. Current syntactic theory has dropped the statement of these language-specific structural descriptions from the formulation of transformational rules because it has been shown that the behavior of transformations can be accounted for in terms of more general conditions on the output and application of elementary operations – conditions that are also part of UG.[2] From this perspective, rule (4) is just an instance of the general rule (5):

(5) Move α,

where α stands for any syntactic category (e.g., tensed copula) and "move" is an abbreviation for the elementary operations of substitution and adjunction. More precisely, substitute α for β and adjoin α to β, where α and β are syntactic categories.

In a theory in which rule (5) accounts for question formation in English, a structure-dependence constraint on the form of transformations does not explain the deviance of example (2c). Rather, there is a general principle of UG that accounts for the deviance of such constructions. Example (2c) violates the principle known as the *Head Movement Constraint* (Travis 1984), which restricts the movement of a lexical head to a head position that governs the phrasal projection of the head. In the structure underlying (2b, c), the head position to which the tensed copula moves governs the main clause copula but not the relative clause copula. The Head Movement Constraint also accounts for the deviance of questions in which an auxiliary verb has been moved into sentence-initial position across another auxiliary, as in (6a), in contrast to the well-formed question.

(6) a. **Have* John should _ left you a copy?
b. *Should* John _ have left you a copy?

Since this analysis is posited at the level of UG, it is preferable to one that involves a language-specific rule like (3b), all other considerations being equal.

Note that the issue of structure-dependence is still relevant, since it is important to distinguish between rule (5) and structure-independent rules like (3a) that do not occur in the grammars of human languages as far as we know. Given our characterization of the structure-independent rules above, we can eliminate the possibility of such rules by a constraint on the form of transformations that prohibits the counting property. This is achieved by restricting the structural analyses that define transformations to Boolean conditions on analyzability, a constraint that was proposed in some of the earliest work on transformational grammar.[3] This restriction excludes quantificational statements that are needed to instantiate the counting property. Thus, we eliminate structure-independent rules by prohibiting quantificational statements in the formulation of transformational rules. In this way the structure-dependence of rules follows from a more specific constraint on the form of rules.[4]

The analysis of structure-dependence given above should be instructive concerning the relation between linguistic theory and psycholinguistic studies of language acquisition. It demonstrates why it is crucial for psycholinguistic research to be informed by current developments in linguistic theory.[5] It also shows that the kinds of studies reported in Crain's target article do not tell us much about syntactic theory. For example, they do not distinguish between the analysis of question formation as given in rule (3b) as opposed to rule (5) plus the Head Movement Constraint. The relationship between linguistic theory and the kinds of language acquisition studies discussed by Crain therefore seems to be one-sided, in answer to the question raised at the outset. What these studies do show, however, is that quite young children demonstrate the kind of linguistic behavior we would expect if our theory of the language faculty (i.e., an innate UG with very specific properties) is on the right track. In this way they contribute to our understanding of human language and for this reason they merit our careful attention.

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NOTES

1. Obviously the rule of question formation applies more generally to tensed auxiliary verbs. How to specify the full range of elements that are moved need not concern us here.
2. For a further discussion, see Freidin (1978, pp. 541f.).
3. See Chomsky (1965) for discussion. See also Lasnik and Kupin (1976) for a discussion of an even more restrictive theory of transformations that eliminates Boolean conditions.
4. Note further that structure-dependence is also relevant to the formulation of general principles of UG. We do not expect to find principles of grammar that depend on counting elements in a linear string. Thus, principles at the level of UG must also be structure-dependent in the relevant sense. Because this constraint defines the general character of the language faculty, it should presumably be construed as a fact of nature rather than as an axiom of the theory.
5. This is not meant as a general criticism of Crain's work, which is clearly informed by current work in syntactic theory.

Is "innate" another name for "developmentally resilient"?

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In studies with children, there is always the danger that the child's inability to perform a task reflects the experimenter's lack of skill in asking the questions rather than the child's lack of skill in answering them. Crain's very cleverly designed studies indeed suggest that children have complex structural knowledge about their language at much younger ages than previous experiments have shown. He argues that because of the complexity of the linguistic knowledge and the fact that the environment does not provide the child with sufficient information for that knowledge, children must come to the learning situation *constrained* to interpret the language data they receive in certain ways and not in others. Constraints are assumed by those who use the term (e.g., Gelman 1990b) to be *internal* to the child at the moment when a particular skill is acquired.

The fact that a constraint is internal to the child on the momentary time scale does not necessarily mean, however, that the development of that constraint is internally controlled on the ontogenetic time scale (cf. Keil 1990). Crain argues that the constraints explored in his target articles, which are said to appear universally in all languages, are not learned directly from experience. He then concludes that these constraints are innate. Although this seems a small step beyond the data, one might ask what is gained by calling these constraints "innate" past the point that Crain makes explicitly in his article – that language-learning proceeds in the face of inadequate input (inadequate in the sense that it underdetermines the output). The problem of innateness has been addressed repeatedly and

elegantly in other disciplines, especially ethology (cf. Lehrman 1970; Mayr 1974; McClintock 1980), and as many as 17 definitions of innateness have been proposed in the literature (Wimsatt 1986). I focus here on two of those definitions: genetic encoding and developmental resilience.

A genetic program is one obvious criterion for innateness that Crain might intend in calling the constraints "innate." Although he never invokes the term "genetic" directly, Crain does refer to the "human biological blueprint" that makes one (or at least me) think of genes. Is anything gained by saying the constraints are genetically encoded? One might answer "yes" if it were conceivable that a discovery in genetics could influence linguistic theory in any way or, conversely, if the results of linguistic analysis could be expected to influence the course of genetic research. Cross-pollination of this sort seems unlikely, however, because there are too many levels between genes and language behavior. For example, if one were to claim that these constraints are genetically encoded, would that bear on how variation across individuals is interpreted? Would we want to claim that the one child who failed to observe the constraint on *wanna contraction* is genetically distinct from the others, and propose studies of adoption and twins to resolve the question? Probably not. Indeed, it would seem to be more profitable to explore the patterns of variability within and across children staying within the level of language itself; for example, are the children who fail to observe the constraint on backward anaphora (a constraint that involves Principle C) the same children who fail to observe the constraint on strong crossover (which also involves Principle C)? Is there any (linguistic) reason to expect a child who fails to observe these constraints to perform in a particular way on, say, *wanna contraction*?

The constraints that Crain proposes serve to narrow the range of possible outcomes in language learning simply because they guide the child's search through the environment for relevant data. Although this sort of narrowing, or canalization, is often attributed to genetic causes (cf. Waddington 1957), there is evidence that canalization can be caused by the environment as well (Gottlieb 1991a). For example, Gottlieb (1991b) has shown that exposure to a particular stimulus at one point in development not only makes the organism particularly susceptible to that stimulus later on, but it also makes the organism *less* susceptible to other stimuli; that is, it buffers the organism against other stimuli. This way it narrows the range of possibilities open to the organism. Two points about the canalizing role of the environment are worth noting. First, when the environment plays a canalizing role, that role is often not easily categorizable as "learning" in the strict sense that Crain has used the term. Second, for acquisition to be universal when the environment is playing a canalizing role, the relevant aspect of the environment must be reliably present in the world of each member of the species. In a sense, the environment must be considered as much a part of the species as its genes.

A second definition of the term "innate" that seems more in line with the types of data Crain presents is "developmentally resilient" or "developmentally buffered against certain kinds of experience" (cf. Alcock 1988; Goldin-Meadow 1982). Under this definition, one of the goals of the enterprise is to specify the range of environments in which language learning can take place. Crain provides the evidence that just by living for two years in a world in which a linguistic system is used, children learn not only which sentences are allowable in their language but also which are not allowable – a particularly difficult task, given that lots of sentences are *not* said and the child has to figure out which are not said for good (linguistic) reasons. Crain's data make it clear therefore that children routinely go beyond the sentences they hear, although not beyond the linguistic system to which they are exposed; that is, children do not invent the system de novo, they just induce it from data that do not appear to be sufficient to justify (let alone compel) that induction.

In fact, it turns out that even if children are exposed to sentences from a conventional language that do not form a coherent linguistic system, they are capable of going beyond their input to construct such a system (Singleton 1987; 1989; Singleton & Newport 1987). As a more extreme example, children who are not exposed to usable input from any established language are able to invent a system that has many of the properties of language. Deaf children, whose hearing losses are so severe that they cannot naturally acquire oral language and whose hearing parents have not exposed them to a conventional sign language, develop gestural communication systems that have many of the properties of linguistic systems (Goldin-Meadow & Mylander 1984; 1990a; 1990b). These children receive as their input the gestures their hearing parents produce along with speech – gestures that have been shown to be global and synthetic (McNeill 1987). The children produce as their output gestures that are linear and segmented, however, with both inter- and intra-gesture structure characteristic of language. Not surprisingly, the deaf children do not invent all of the properties found in conventional languages. Indeed, the absence of a particular linguistic property or constraint in the deaf child's gesture system could be taken as (indirect) evidence that exposure to a conventional linguistic system is necessary for the development of that property or constraint (cf. Goldin-Meadow 1987).

Finally, it is important to note that language learning is resilient in the face of variation caused not only by environmental factors but also by genetic factors. For example, retarded individuals (except for the most severely handicapped) follow paths of language development similar to those of normal children, but at a slower rate (Morehead & Ingram 1976). Although the process of language development requires an organism with a human genotype, it is not clear that the process can be localized to one, or even to a set of particular genes. It is clear, however, that the process is developmentally resilient.

Language development: Relatives to the rescue!

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In his target article, Crain takes the position that children's internal, innately given knowledge of possible language systems compensates for the impoverished nature of their actual linguistic experience, such that "the acquisition of syntax will . . . be nearly instantaneous, once the relevant lexical items and sentence structures are in place." The plausibility of this position is enhanced by experimental demonstrations of sophisticated syntactic knowledge in children, such as several of the studies that Crain describes. I am wholly sympathetic to this approach and have spent some energy collecting data to support it. I also agree with Crain that recent "parameter-setting" ideas with respect to language acquisition do not logically entail gradual accretion of knowledge by children. I think I differ from Crain, however, as to whether some of the empirical evidence to date supports the very early development of an adult-like system. There are several areas where I believe the evidence points in the direction of a period of development that extends into the school years, and for which development may not be reduced to lexical learning or the acquisition of sentence structures, at least in any simple manner. The acquisition of the grammar of relativisation, the acquisition of control rules for some constructions, and the acquisition of movement in some constructions are among the areas of grammar for which full adult knowledge may take some years to develop. Because