

Generative complexity and psycholinguistics: divorce American style¹

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ABSTRACT

In the American scientific context of the 1950s, the confluence of information theory and behavioristic views seemed to hold the promise of a truly interdisciplinary psycholinguistics. However, the prospects opened up by this confluence were soon ruined by the advent of transformational grammar (TG). For reasons detailed in this paper, such was the attraction of TG that it became the nearly exclusive source of psychological hypotheses on linguistic processing. Correlating transformational complexity with measures of performance, such as response times, set the new methodological trend. Problems quickly crept in: psychologists could not solely rely on linguistic theory to account for their data and had to make room for heuristics and biases. Evidence for transformational theory was difficult to come by and TG was evolving at a pace psychologists had difficulty sustaining. On their part, linguists were reluctant to submit their constructs to psychological testing, so that the relationship of psychology to linguistics justifiably seemed to be one-sided. In all likelihood, countering the threat posed by psycholinguistics and defending the autonomy of linguistics underpinned Chomsky's affirmation that linguistic theory, with its own methods, lays a claim to psychological reality and does not need an auxiliary science.

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Divorce was inevitable, but this short-lived episode of interdisciplinary research had significant consequences both in linguistics and in psychology. These consequences are dealt with in the last part of this paper.

KEYWORDS

history of linguistics, history of psychology, behaviorism, psycholinguistics, generative grammar, transformational grammar

RÉSUMÉ

Dans le contexte scientifique américain des années 1950, la confluence de la théorie de l'information et de conceptions behavioristes semblait promettre le développement d'une psycholinguistique véritablement interdisciplinaire. Toutefois, les perspectives ouvertes par cette confluence furent bientôt compromises par l'avènement de la grammaire transformationnelle (GT). Pour des raisons examinées dans cet article, l'attrait exercé par la GT fut tel qu'elle devint la source quasi exclusive d'hypothèses psychologiques sur le traitement du langage. La nouvelle méthodologie en vogue consista à corrélérer la complexité transformationnelle avec des mesures de la performance, comme par exemple les temps de réponse. Des problèmes surgirent rapidement : les psychologues ne pouvaient rendre compte de leurs données uniquement en se fondant sur la théorie linguistique et devaient prendre en considération des heuristiques ou des biais de traitement. Il se révélait difficile de fournir des preuves de la GT, laquelle évoluait à un rythme que les psychologues avaient du mal à suivre. De leur côté, les linguistes rechignaient à soumettre leurs constructions théoriques à l'épreuve de la psychologie de sorte que la relation entre les disciplines apparaissait déséquilibrée. Il est vraisemblable que la menace représentée par la psycholinguistique et le désir de préserver l'autonomie de la linguistique ont poussé Chomsky à soutenir que la linguistique peut prétendre établir la réalité psychologique de ses hypothèses par ses propres moyens et n'a pas besoin d'une science auxiliaire. Le divorce était inévitable, mais ce bref épisode de recherche interdisciplinaire ne fut pas sans conséquences, à la fois en linguistique et en psychologie. Ces conséquences sont l'objet de la dernière partie de cet article.

MOTS-CLÉS

histoire de la linguistique, histoire de la psychologie, behaviorisme, psycholinguistique, grammaire générative, grammaire transformationnelle

1. Introduction

It was not long after the term *psycholinguistics* came into wide use that the field experienced its first upheaval in the U.S. With the advent of transformational grammar (TG), says Reber (1987: 327), “the Chomskyan orientation captured the discipline to such an extent over the next two decades that the very name psycholinguistics became synonymous with the set of ideas that emanated on a regular basis from MIT”. In practice, this meant disrupting the “new interdisciplinary field” (Osgood & Sebeok 1965: xi), whose *modus videndi* rested on the association of behavioristic learning theories and information theory. This interdisciplinary framework may well have acted as a counter-model for generative grammar, especially when associated with an epistemology and a new psychology (cf. Fodor, Bever & Garrett 1974).

In this paper, we shall first examine the course of events leading to this upheaval in psycholinguistics and the appropriation of TG by psycholinguists. It will be seen that, once this appropriation was realized, a large part of psycholinguistic research in syntactic processing crucially involved measuring subjects’ performances against the amount of structural complexity posited by TG. The way this division of labor was conceived and the problems posed in particular by the evolution of TG will be examined.

Making psychological experiments the touchstone of linguistic theory was a potential threat to TG and to the autonomy of linguistics. Experimental testing, based in particular on transformational complexity, could disconfirm what was most distinctive about TG, i.e. transformations. How did linguists, and notably Chomsky, react to this threat and the encroachment of psychology? In tackling this question, we shall be drawn into a discussion of the complex of arguments marshalled by Chomsky to maintain the autonomy of linguistic theorizing. Or, to put it differently, it will be contended that some aspects of Chomsky’s metalinguistic reflection might be better understood as a self-protection device than as a coherent epistemology. Lastly, a few words will be said about the consequences of transformational psycholinguistics for the integration of processing

considerations into linguistic theorizing, and the shift of perspective this integration facilitated.

2. The historical context: the first happy marriage (or its prospect)

More than forty years separate Blumenthal's thin volume on the history of language psychology (Blumenthal 1980 [1970]) and the hefty compendium of Levelt (2013) on the same subject. Yet, if we believe Levelt, it would seem that the historical awareness of practitioners made little progress during these four decades. The notion that psycholinguistics emerged in the 1950 is still, says Levelt, "a widely shared opinion" (2013: 3), as it was when Blumenthal lamented the ignorance, on the part of his American colleagues, of an "illustrious earlier history of psycholinguistics" (1980 [1970]: viii).

Is this eclipsing due to a new experimental stance promoted from the 1950s on? This is quite doubtful: Osgood, who sanctioned the idea that the field was "new" (see the citation in our introduction) was well aware of the experimental past of behaviorist studies of language, including Soviet research (e.g. Osgood 1952). It is more likely that, for practitioners, the novelty lay in what they perceived as a genuine interdisciplinary collaboration between linguists and psychologists, a collaboration which was further legitimated by a new overarching technoscience, that is, information theory (IT).

At a time approximately coinciding with Osgood and Sebeok's *Psycholinguistics*, Hockett devoted a lengthy review to Shannon and Weaver's pioneering essay on information theory (Shannon & Weaver 1949). Hockett's text was actually more than a review: it was a discussion extrapolating the benefits linguistics could draw from looking at language in information-theoretic terms. Viewed from an engineering perspective, some specificities of natural languages could be brought out in a starker light, especially their massive redundancy on all levels and their corollary robustness. The identification of immediate constituents, a central issue after Bloomfield (cf. Wells 1947), could be reframed in terms of greater levels of indeterminacy ("entropy", in IT terms) at "cuts" between units, although

this criterion was quite probably not fool-proof, as Hockett himself remarked (1953: 87-8).

Rather surprisingly, Hockett makes no mention of Miller's *Language and Communication* (1963 [1951]), in which language design was also envisaged as an engineering question². In particular, Miller gave quantitative estimations of the massive downsizing of theoretically possible discriminations in the acoustic channel, thus providing an assessment of the robustness of language which was much more detailed than the one given by Hockett. Miller also evoked transition probabilities between words as furnishing a realistic view of what speakers were doing when stringing forms together, and by so doing he showed how far he was from taking sufficient account of the hierarchical nature of linguistic structuring. By contrast, Hockett, as we just saw, was preoccupied with locating cuts in order to circumscribe units.

Hockett's tone in his review was also rather programmatic, while Miller's embrace of information theory and statistical reasoning evinced a good degree of confidence. For example, right after having mentioned his technique of probabilistic approximation to English strings, Miller stated that "the variety of sentence forms that a talker uses is not great, and probably the lengths of the patterns that are fitted together into these forms seldom exceed 10 words. The process of forging sentences is not inexplicable, and a clear formulation of what we need to know should lead to better observations and, eventually, better explanations of our verbal habits" (1963 [1951]: 192). What Chomsky, and Miller in his wake, would later declare as completely misguided was therefore deemed manageable at this point.

Whatever the discrepancies between Miller and Hockett, that a study as relevant as Miller's was not mentioned by Hockett may simply betray the fact that information theory percolated into psychology at a quite early date and before linguists took an interest in it. Miller had worked on signal intelligibility in a psycho-acoustic

2. This was not, however, the only perspective adopted by Miller. The book also contains chapters on personal styles, language acquisition, word-association protocols, the use of language in problem-solving and social patterns of communication. For a brief description of Miller's intellectual background, see Boden (2006: 286 ff.).

laboratory during the war, he was definitely attracted to mathematics and statistics, but he had strong reservations about the way some neo-behaviorists, like Hull and Spence, attempted to quantify behavioral variables (cf. Baars 1986: 303 ff. for a self-portrait of Miller). Obviously, this profile made him receptive to IT and ready to dissociate himself from neo-behaviorism, of which the Hullian brand was perceived as the most advanced until the early 1950s (Baars 1986: 60) and was still abundantly referred to into the 1960s (Leahey 1994: 207).

This bifurcation between linguistics and psychology in the reception of IT was not to last. The collective undertaking embodied in Osgood and Sebeok's *Psycholinguistics* could appear as foundational of a new interdisciplinarity, perhaps especially so in the eyes of Osgood, who bore the largest responsibility for the book's theoretical orientation (cf. Levelt 2013: 3-11 for a contextualization). We may observe, for instance, that the following introductory text to a chapter entitled "sequential psycholinguistics" is much in line with Osgood's later attempt to conciliate linguistic structure and probabilistic transitions between linguistic units (cf. Osgood 1963):

Study of the sequential or transitional structure of language behavior provides a meeting ground for linguists, information theorists, and learning theorists. The linguist, applying his own methods of analysis, discovers hierarchies of more and more inclusive units; the information theorist, usually starting with lower-level units such as letters or words, finds evidence for rather regular oscillations in transitional uncertainty in message sequences, the points of highest uncertainty often corresponding to unit boundaries as linguistically determined; and the learning theorist, working with notions like the habit-family hierarchy, finds it possible to make predictions about sequential psycholinguistic phenomena that can be tested with information theory techniques. (Osgood & Sebeok 1965: 93)³

The "meeting ground" referred to in this text rests on a consonance between the three disciplines in question. This kind of statement

3. The text is not signed, and it is unclear who penned the introduction to the chapter "sequential psycholinguistics", of which this text is excerpted. The rest of the chapter is due to several authors, including Floyd Lounsbury, a rather unexpected authorship since Lounsbury is better known for his involvement with structuralist ethnolinguistics.

(cited in Fodor, Bever & Garrett 1974: 53) would invite pro-generativist psychologists and linguists to lump together a bankrupt “taxonomic” linguistics with an outdated neo-behavioristic psychology (including a learning theory) and a lowbrow inductivism, using each to discredit the other. It is tempting to speculate that this antagonism helped them define a global endeavor whose ambition mirrors that of their opponents.

3. Situating Chomsky’s attack on behaviorism

Boden (2006: 285) rightly observes that “at base, Shannon’s theory—like behaviorism—was a beads-on-strings affair, for it concerned sequences of events conceptualized as Markov processes”, and this consonance implicitly exposed behaviorism to the attack launched by Chomsky (1957a) on associated explanatory initiatives, including probabilistic and Markovian accounts. However, the testimonies of Margaret Boden and George Mandler (Baars 1986: 263) agree on the fact that Chomsky’s critique of behaviorism really came to psychologists’ attention thanks to his arch-famous vitriolic review of Skinner’s *Verbal Behavior* (1959). It is not our purpose here to discuss the factors which led to the demise of behaviorism⁴. In this paper, our first concern lies in the role Chomsky and TG assumed in the evolution of psycholinguistics. However, the general context is certainly relevant for understanding the impact of Chomsky’s review, hence his status in the eyes of psychologists.

Testimonies (e.g. in Baars 1986) show that, in the 1950s, the Hullian behavioristic theory of learning was perceived as an empirical failure. Behaviorism at large was contested from several sides. In the United States, humanistic psychology was one of the paths taken by this critique of behaviorism’s dry operationism. Perhaps a sign of the theoretical “disorientation” of psychology diagnosed by

4. “Demise” may not be the best term. At any rate, it should not convey the idea that behaviorism has disappeared. Behaviorist protocols live on in cognitive psychology, which motivates Leahey’s assignment of methodological behaviorism and cognitive psychology to a super-category of “behavioralist” psychology (1994: 138-9). Behaviorist results (for example on the functioning of reinforcement) have been or are exploited in some areas of dynamic psychology, and today, in the algorithms of digital games and social networks.

Sigmund Koch as early as 1951 (Baars 1986: 67-68) was Skinner's perspective, which rejected speculation on inner variables and defended descriptivism, thus accomplishing a retreat to safe and arid empiricism. Other objections to behaviorism involving the role played by innate proclivities (or "instinct") and cerebral organization would soon surge, and some of these studies would later be marshalled by Chomsky in support of his nativism⁵.

Now, Skinner's fortes, experimental protocols and descriptions of stimulus-reinforcer-response contingencies, were missing from *Verbal Behavior*, although Chomsky's review made frequent mentions of animal conditioning and lever-pressing rats to ridicule the idea of making human speech a stimulus-dependent activity (for a discussion see Joseph 2002). To the eyes of a psychologist, the objection did not strike at the heart of Skinner's behaviorism, but could only concern a speculative book written in behaviorese, such as Weiss' rhapsody on behaviorist reductionism, which had so impressed Bloomfield and was non-experimental as well (Weiss 1929 [1925]). And in fact, Skinner's opus apparently fell on deaf ears: it seems that it got only one other review, globally negative, by a Soviet behaviorist in *Word* (Andresen 1991: 58). With hindsight, however, lumping together *Verbal Behavior* and animal learning theory appears to strike at the whole edifice. That is, Chomsky's critique targeted a book toward which most were indifferent, but its destructive power would soon be enhanced by the global obsolescence of behaviorism.

Chomsky's review left unscathed the synthesis between IT, learning theory and linguistics envisaged by Hockett and later promoted by Osgood and Sebeok's *Psycholinguistics*. Yet, this synthesis was linguistically better articulated than Skinner's account, and favorably considered by some linguists. However, he had already criticized this approach in his review of Hockett's *Manual of Phonology* (Chomsky 1957b), a text which may provide the missing link between *Syntactic Structures* and its attack on probabilistic processes, and the all-out

5. To the first category belongs the work of the Brelands on the "misbehavior of animals", that is, on responses that would be unexplainable if an organism were a *tabula rasa* and instinct not factored in (Breland & Breland 1961). To the second class belong studies of Hubel & Wiesel's famous studies (1963, 1965) on the innate organization of the visual cortex.

assault on the psychological branch of the “beads-on-strings” theory delivered in the review of Skinner’s *Verbal Behavior* (written shortly later, in 1958)⁶. It might be, as argued by Radick (2016), that the new mentalist, rationalist, nativist and anti-probabilist synthesis erected around TG took shape, in Chomsky’s case, in opposition to Hockett (or to the kind of unifying theory promoted by Osgood and Sebeok); to be sure it emerged progressively⁷. Whatever the case, the new synthesis invited psychologists to take a stance on linguistic theory and question their empiricism.

Finally, it should be noted that Chomsky’s opposition to behaviorism did not deter all behaviorists from seeking a compromise. Neobehaviorists like Osgood and Jenkins, who postulated internal processes mediating overt behavior, had already moved these mediational states from a peripheral level (as in Hull’s theory) to a more central, “cognitive” level. It would turn out that from their side, no strong objection to TG would be looming. In fact, both Jenkins and Osgood would show their readiness to endorse Chomsky’s criticism of Markov processes and to accept TG’s global framework, though criticizing linguists’ reluctance, including on the part of transformationalists, to delve into semantics (Jenkins & Palermo 1964; Osgood 1963). In spite of this good will, Chomsky seemed to ignore conciliatory solutions: they did not fit in with his definition of behaviorism (according to which this approach would not envisage systems “underlying behavior”, 1965: 193), and they were fundamentally “descriptivist”, in the sense of *Aspects*, i.e. based on the extraction of patterns and speech-habits (1965: 15). Indeed, in Palermo and Jenkins (1964), bona fide “empiricist” principles relying on the functional equivalence of stimuli and responses obtain general lexical classes and syntactic categories (such as SN and SV). This concern with a procedure used in constructing lexical and syntactic categories is in

6. A few years later, Fodor (1965) would mount an attack on neobehaviorist mediational accounts of meaning, specifically against the notion of mediating response, a central tenet of Osgood’s theory.

7. See Seuren (1998: 233-252), Koerner (2004: 40-41) and Radick (2016: 57), who points out that “there is not a whiff of antipathy to behaviorism” in the *Logical Structure of Linguistic Theory* nor in *Syntactic Structures*.

fact more in line with distributional accounts (e.g. Fries 1952) than with TG, in which it is swept under the rug.

4. Miller and the introduction of TG into psychology

Miller was completely won over by the prospects opened up by the early Chomsky (cf. his testimony in Baars 1986: 207) and set out to acquaint psychologists with the theory, even if he was aware of the existence of alternative models which he described as equivalent (Miller 1962). Whatever the ultimate ground for Miller's choice of TG, his bet reflects TG's rise to prominence and victory over rival theories, for reasons well described in Nielsen (2010).

His introduction of TG into psychology, however, took an unexpected form, more comprehensible if we try to put ourselves in the shoes of a psychologist of this period. Firstly, TG was introduced in a collaborative book centering on goal-driven behavior (Miller, Galanter & Pribram 1960), not specifically on language⁸. This perspective reflected a long-standing concern of American psychologists working in one behavioristic perspective or the other: how can we account for apparently teleological behavior without resorting to mentalistic notions? (cf. e.g. Holt 1915; Tolman 1932). Teleological self-adjusting mechanisms (i.e. feedback mechanisms) had been on the market for some time, for example steam-engines regulators, but 'goal-directed' was associated with 'idea', or 'will' and 'will' with a ghostly faculty. When Bloomfield repudiated the mentalistic outlook, he scorned in particular its postulate of a "non-physical factor" called *will* (1933: 32)⁹. But in the 1950s, feedback mechanisms, like

8. On the background to this collaboration and the book that it spawned, see Boden (2006: 337). On the latter, a mischievous George Mandler commented that "Miller wrote it, Galanter takes credit in it, and Pribram believes it" (Boden, *ibid.*).

9. In *Language*, will and "mentalism" are opposed to "mechanism". A "mechanical" account makes no appeal to a "mind". Prima facie, mechanism fits in well with Bloomfield's conception of phonetic change, which he (like Neo-grammarians) regards as driven by absolute regularities and independent of functional factors (for example, the fact for a phoneme, to bear a grammatical function). Yet, such regularities are not presented as "physical" (unlike in

anti-aircraft gun-control servomechanisms, were far more visible, and the reduction of purposive behavior to feedback mechanisms had already been proposed by Rosenblueth, Wiener, and Bigelow (1943).

Secondly, goal-directed behavior, envisaged as a feedback mechanism, could be connected with the concept of reflex and given a very general import through the idea, defended by Dewey (1896), that perception was fully integrated with sensori-motor coordination. Or to put it differently, the feedback loop involved in this integration could be envisaged as the basic behavioral unit (Miller, Galanter & Pribram 1960: 26-31). In its general pattern, this unit is very simple: the organism tests an input against some criteria, responds until there remains an incongruity between the result of the test and the criteria, and exits the loop when congruity obtains. The acronym *TOTE* (Test-Operate-Test-Exit) is the name chosen to designate this basic pattern (*ibid.*).

The behavior of an organism as simple as a tick cannot be modeled with a single TOTE unit. That is, TOTE units need to be sequentially and hierarchically arranged. Such an organization is called a *plan* and, most importantly, the call of subroutines by superior routines is assimilated to symbolic processes in a computation. On this account, the inner manipulation of symbols, including the high-level one traditionally designated as "inner speech", is the process whereby plans may be organized and reorganized. This is the important contribution that Miller *et al.* bring to the construction of cognitive psychology: several American psychologists had grappled with the inflexibility of stimulus-response chains, notably Tolman (1932), but symbolic computation based on TOTE units had not yet served to capture the fact

Neo-grammarians) but as a good heuristics (Bloomfield 1933: 357-358). In short, mechanism is not a plea for physicalist reductionism. In fact, behaviorism may appear as a scientifically acceptable alternative to such a reductionism, since, in the version of Weiss, it makes room for a "biosocial" level of analysis (in spite of Bloomfield's claim of a shared "physicalism" between Weiss and the Vienna School logical empiricists in Bloomfield 1936). The point is that Bloomfield's behaviorism and Chomsky's cognitivism may have something in common: finding a framework that does not entail an adoption of a lower-level reductionism (mechanism for Bloomfield, behaviorism for Chomsky) and safeguards the scientific credentials of linguistics.

that organisms may display flexibility, i.e. find a route alternative to the S-R chains they have been conditioned to.

The computer analogy, the existence of processing languages simulating hierarchical reasoning, and the distinction made between the abstract machine and its implementations entailed that complex hierarchical procedures could be posited without worrying too much about their physical realization. This legitimated the claim that TG deals with psychologically real processes, but need not concern itself with neurophysiology¹⁰. The coming of a new mentalism was heralded, and set in opposition to “Bloomfieldian” or “taxonomic” grammar (Katz 1964; Fodor, Bever & Garrett 1974). Yet, as we shall see, objections to taxonomic grammar on account of its association with an outdated (behaviorist) psychology will prove somewhat ironic from linguists who will end up insulating themselves from current psychology.

In the all-encompassing book of Miller *et al.*, and as befits a general psychological essay on behavior, language is introduced very late, after basic abilities and organic parameters (drives, needs, instincts, motor skills, memory) have been discussed. The confidence in IT expressed in Miller’s 1951 book has now evaporated and the rejection of the old synthesis is fully in line with Chomsky: Markov-type processes are rejected as unrealistic generators in view of the infinite linguistic capacity of human speakers. This capacity is distinguished from processing bottlenecks (such as memory limitations), following Yngve (and anticipating Chomsky; Miller, Galanter & Pribram 1960: 148). As for the reasons motivating the adoption of transformations, they conform to arguments presented in *Syntactic Structures*, in par-

10. “...it is clear that the linguist, though he claims that his theory describes a neurological mechanism, cannot immediately translate the theory into neurological terms, i.e. into talk about synapses, nerve fibers, and such. But – and this is the crucial point in showing that the mentalist is not a psychophysical dualist – this failure to have a ready neurological translation means only that he cannot yet specify what kind of physical realization of his theoretical description is inside the speaker’s head” (Katz 1964: 129). It is only required of a theory that it be “consistent” with psychology and neurophysiology (*ibid.*: 133). In this occurrence, Katz makes explicit reference to Putnam’s famous analogy between mental states and the logical states of a Turing machine (Putnam 1960), that is, to the view today identified as “(machine state) functionalism.”

ticular, regarding the economy of description¹¹. But their appeal also lies in their being comparable to these symbolic processes which are hypothesized to underlie the reorganization of behavior and which Miller *et al.* associate with the rejection of S-R chains.

5. The appeal of a transformationalist psycholinguistics

An incentive for interpreting TG in a psychological way resided in Chomsky's referring to his theory as "mentalistic", i.e. "concerned with discovering a mental reality underlying actual behavior" (*Aspects*: 4). It should be noted too that Chomsky's proclaimed reliance on the native speaker's intuition could in itself be perceived as mentalistic, and as such, as a dubious introspective exercise¹². This fact alone made him side with non-behaviorists. In addition, on Chomsky's part, there was apparently no reluctance to engage in an interdisciplinary enterprise: according to George Miller's testimony, Chomsky was far from averse to a psycholinguistic investigation of TG, at least initially (cf. Baars 1986: 243). In a chapter written in collaboration with Miller, the sketch of a processing model is drawn in broad strokes (Miller & Chomsky 1963). The model includes a "deep" processing stage whose output is the structural description of a sentence, complete with its transformational history. A major justification is that grammatical relations can only be recovered when this output is delivered (*ibid.*: 480).

On psychologists' side, generation and transformation were congenial to experimenters who thought in terms of dynamic processes, and had to measure response times (Greene 1972). Importantly, TG seemed to propose a division of labor between linguists and psychologists along the distinction between an account of "actual behav-

11. Miller *et al.* contend that greater economy is needed for learning a language in a "finite childhood" (Miller *et al.* 1960: 151). The non-commensurability between finite exposure and infinite creativity is the first feature to be integrated into Chomsky's ramified and complex argument in favor of nativism (cf. Thomas 2002 for a history).

12. For a telling rejoinder of Hill to Chomsky's use of introspective data, cf. Harris (1993: 54).

ior” and the “internalized grammar” which projects an infinite set of sentences (in the terms of Chomsky 1959), and, from 1963 on, between the elaboration of a grammar reflecting the “competence” of a speaker and a theory of “performance” (Léon & Riemer 2015: 131-7). On the other hand, it should be noted that Chomsky moved from a conception in which a grammar generates sentences and assigns degrees of grammaticalness to them, to a conception in which a grammar generates grammatical sentences only, and from there to a conception in which the set of grammatical sentences is obtained by factoring out all parameters related to performance (such as the limitation of memory span)¹³. Surely, psychologists (and, indeed, some linguists) may be inclined to resist the idea that memory span or various heuristics cannot enter into the very design of human language. We shall come back to this issue. And more to the present point, this increasingly abstract notion of grammaticalness may pull grammar out of the reach of psychologists, who would be entrusted with the task of studying its implementation in the mind/brain. The following passage suggests this interpretation:

When we speak of a grammar as generating a sentence with a certain structural description, we mean simply that the *grammar* assigns this structural description to the sentence. When we say that a sentence has a certain derivation with respect to a particular generative grammar, we say nothing about how the speaker or hearer might proceed, in some practical or efficient way, to construct such a derivation. These questions belong to the theory of language use – the *theory of performance*. (Chomsky 1965: 9, my italics)

To compound matters, this division of labor may not coincide fully with disciplinary lines. In the following pages, Chomsky proceeds to demonstrate how judgments of acceptability – for the average psychologist, a dubious introspective exercise – may serve to draw conclusions about the “perceptual device” used in parsing sentences (1965: 14-15)¹⁴. In this circumstance, the linguist is encroaching on

13. Chomsky used the expression “degrees of grammaticalness” before the distinction between acceptability and grammaticalness got established (cf. Léon & Riemer 2015).

14. A classification of sentences into “degrees of grammaticalness” was present as early as 1955 in *The Logical Structure of Linguistic Theory* (1975 [1955]), where

what would seem to be the psychologist's bailiwick, the theory of performance. All the same, this was also an encouragement to conduct psychological research in an interdisciplinary spirit but with psychology as *ancilla linguisticae*. As for the study of language use by itself, without the guidance of hypotheses on the underlying system, it was not deemed worthy of a "serious discipline" (1965: 4). In short, Chomsky made a distinction between a respectable theory of performance and the frivolous study of language use, the latter being probably a matter of determining why an utterance occurs in a given context (and encompassing, therefore, probabilistic accounts). The theory of performance can be identified with TG-inspired psycholinguistics and all conclusions derivable from judgments of acceptability.

6. Testing the psychological reality of transformations

Though TG-inspired psycholinguistics does not reduce to experiments specifically bearing on transformations, these experiments are crucial: transformations are a construct proper to TG (unlike constituent structure). Ultimately, as we shall see, results delivered by the relevant psychological variables (items recalled and response time) would not be found to be straightforwardly related to transformational complexity.

At first, however, there was hope that TG could provide a plausible blueprint for psychological processes. An early experiment by Miller & McKean (1964) seemed to show that response times were proportional to transformational complexity. Further, the results exhibited a neat additive pattern: a transformation was found to take an approximately equal time in different contexts, and performing two transformations yielded a response time close to the sum of the transformations when these were processed singly. Thus, transforming an affirmative active sentence into its passive counterpart, and an active negative one into its passive negative counterpart took approximately equal time. On the other hand, performing a negative

intuition is opposed to probability of occurrence, with Miller and Hockett in view (cf. Léon & Riemer 2015).

and a passive transformation on the same sentence yielded a time approximating the sum of the average response times obtained for each kind of transformation. For reasons we shall not go into here, other psychologists believed that transformational complexity would be better assessed by measuring the memory capacity taken up by their storage. This criterion was used by Savin & Perchonock (1965), who demonstrated that the more transformationally complex the sentence, the more it depleted the storage capacity of their subjects.

Several kinds of consequences arose when other parameters were found to interact with transformational complexity. First, some results could be interpreted as contradicting the additive hypothesis, or, alternatively, as entailing that subjects could bypass the transformational stage. For example, in an experiment conducted by Slobin (1966), subjects responded as quickly to passive sentences as to their active counterparts when the scene depicted in the passive sentence conformed to a familiar strong asymmetry between the participants (so called “non-reversible sentences”, e.g. *the flowers are being watered by the girl*, vs “reversible sentences”, e.g. *the boy was hit by the girl*). More striking even was Fodor & Garrett’s observation (1967) that transformationally more complex sentences could be decoded more easily than simpler ones. Thus, *The first shot the tired soldier the mosquito bit fired missed* was actually easier to understand than the same sentence without the adjectives (i.e. a less complex sentence), presumably due to the presence of semantic cues.

The central role of processing complexity casts doubt on the psychological relevance of one of the original motivations of transformation, namely that they provide an economical way of generating related sentences. This can be exemplified by Dryer’s typological study on the position of sentential NPs within their matrix clause (1980), in which Dryer takes stock of previous studies published in the 1970s and submits his own account in terms of positional tendencies. If, for reasons we shall not go into here, *That Fred loves Mary is obvious* is more difficult than its variant with an extraposed sentential NP (*It is obvious that Fred loves Mary*), why would subjects bother to generate the latter utterance from a more difficult structure corresponding to the former sentence? If economy of linguistic description *must* have psychological relevance, as argued by Chomsky, it

cannot but conflict with ease of processing in cases like these. Either economy of description supersedes considerations of processing costs, and all psychological and typological results are of secondary importance, or economy of description simply is a meta-theoretical criterion which has nothing to do with “psychological reality”. But in the eyes of psychological realists like Chomsky, the latter conclusion is ruled out.

As we have seen, data were often too messy to be accommodated by a simple mapping from transformational complexity to processing time. In some cases, this gave rise to a second kind of consequences: psychologists felt compelled to supplement transformations with the decision procedures needed to account for their results. Negative sentences proved to be a very delicate matter: interpreting the pattern of results entailed that the truth-value of a negative sentence had to be factored in¹⁵. Typically, response times to true negative sentences were longer than to false negative ones (for a review, see Greene 1972). The decision procedures designed to capture the patterns of results took the form of flow charts in which what remained of TG was the assumption that kernel sentences furnished the canonical representation of sentences as bearers of truth-value, with deviations requiring additional processing steps (Trabasso *et al.* 1971; Chase & Clark 1972). In addition, a natural motivation for the advantage of false negatives over true negatives presented itself: on the assumption that negations are most naturally used for denying a state of affairs held as true, false negatives are closer to “naturalistic” contexts (cf. Greene 1972; Wason 1972). The functional motivation invoked in this occurrence by psychologists like Wason was quite extraneous to TG’s concerns. Further, a connection was thereby established between linguistic comprehension and “natural”, not necessarily normatively logical, reasoning. Deductive biases in natural reasoning were not a new concern in themselves, since philosophy was born partly out of a reaction to sophistry. The novelty lay

15. Negative sentences are transformationally more complex than kernel sentences in the framework of *Syntactic Structures*. This is not so in *Aspects*. The problem remains, however: for psychologists, the interaction between truth-value and negation still has to be accounted for, peripheral as it might be for linguists.

in the systematic experimental investigation of hypothesis formulation and logical deduction (one of Wason's pet subjects). This issue, then, takes us to a different continent. Within generative grammar, "natural" reasoning would be broached by supporters of generative semantics only (cf. Lakoff 1970).

The varying status of negative sentences from *Syntactic Structures* to *Aspects* illustrates yet another kind of consequence brought about by the wedding of psycholinguistics to TG: the interpretation of performance data hinged on the current state of the theory. Thus, it was countered that in Miller & McKean's experiment cited above, transformations had not been counted properly: negation also implied the *do*-support transformation for non-auxiliary verbs¹⁶. The results, therefore, were not valid. But in the model of *Aspects*, Miller and McKean's results were defensible again: the *do*-support transformation was post-deep structure, and Miller & McKean's results could be taken to measure the relative complexity of deep structures (DS), not of transformations (cf. Fodor, Bever & Garrett 1974: 235-241). Likewise, experiments on the role of deep-structure frequency in recall depended, for their interpretation, on the hypothesis that pronouns are inserted post-DS, that is, that they replace full nominal phrases present at DS, but this was controversial (Fodor, Bever & Garrett, *ibid.*: 259-263). Not only did psychologists have to look to linguistics for their hypotheses, but the interpretation of their results depended on the current state of generative theory, and this was rapidly changing.

To sum up, elaborating a psychologically plausible theory of performance required making room for semantic factors, decision procedures and functional considerations. Further, it was not always clear how to reconcile psychological processing factors with an a priori criterion such as economy of description. More circumstantially, for psychologists to keep up with the pace of theoretical change in linguistics, experiments had to be framed in the terms of the latest version of TG, and results interpreted or reinterpreted in the light of this version. This put a double strain on psychologists: that of separating the grammatical core from factors linguistic theory regarded

16. This issue and others related to the proper way of counting transformations are raised by Fodor, Bever & Garrett (1974: 231-234).

as extraneous, and of updating their hypotheses in conformity with theoretical changes made by linguists.

7. Some pills psychologists found hard to swallow

Ideally, the division of labor between linguistics and psychology should yield results that are complementary and cross-fertilize both disciplines. However, this division also entails a methodological parting of the ways: linguists who make a distinction between ‘grammatical’ and ‘acceptable’ filter out some acceptable sentences, and accept as grammatical unacceptable ones, on the ground of their conformity to generalizations they believe to have gained. For psychologists, actual data (abstracting away from statistical corrections, aberrant responses etc.) are all *bona fide*, and the identification of performance factors is a complex empirical matter. On the other hand, unattested or very unlikely sentences which linguists regard as grammatical can hardly figure in psychologists’ empirical evidence.

Chomsky would come to depict the generativist attitude as the sound scientific practice of a “Galilean” science, a quest for principles idealizing away from theoretically irrelevant aspects of natural phenomena (for a critique, see Riemer 2009). Further, the formulation of principles being the linguist’s bailiwick, nothing warrants that the data and processing hypotheses submitted by experimenters are relevant to linguistic theorizing. This situation was apt to make linguistics impervious to psycholinguistics’ meddling in its affairs, and could be perceived as leading to a form of linguistic isolationism. The risk was clearly articulated by Greene: “the danger is that any data fitting in with a linguist’s preconceptions about specifically linguistic universals will be counted as evidence for the basic structure of language, while any awkward data can be put down to the influence of functional ‘performance’ factors” (Greene 1972: 104)¹⁷.

17. A suspicion that is not unwarranted when we read: “A grammar is simply an axiomatic representation of an infinite set of structural descriptions, and the internal evidence in favor of the structural descriptions modern gram-

The term “preconceptions” seems to reflect the perspective of an agnostic observer, in a time when linguistic theories had not only failed to reach any consensus (and have not to this day) but, in the generative camp, were involved in a bitter strife pitting the syntax-centred interpretive stream against the generative semantics movement (Newmeyer 1986; Harris 1993; Huck & Goldsmith 1995; Fortis 2015). As a matter of fact, the peak of “generative” psycholinguistics corresponds approximately to the period during which generative semantics is in full swing, roughly between 1965 and 1975¹⁸. The discord could not but shake psychologists’ confidence in the bet they had placed on TG. Fillenbaum (1971) gives voice to this disorientation: even leaving aside Yngve’s phrase structure model and Lamb’s stratificational theory, he asks, should psycholinguistics remain faithful to standard deep structures, or should it espouse generative semantics? And what should psychologists make of the new relevance attributed to surface structures for semantic interpretation, and wielded by Jackendoff (1969) and Chomsky (1972a) against generative semantics? Whereas the most convincing experimental results had been secured for (deep) structures, not for processes (i.e. transformations, cf. Fodor, Bever & Garrett 1974), controversy over the very nature of deep structures was now rife and the alternative views put forth by generative semantics were not without appeal. Albeit generative semantics largely handled semantics with syntactic tools, their theory was no longer syntax-centred but made proposals on the ways semantic representations, or ‘thoughts’ (the term of Lakoff 1976 [1963]), could be mapped to forms, and thus promised a more integrated blueprint of the speaker’s articulation of content. Chomsky’s rebuttal (1972a: 69 ff) that the Standard Theory is not only interpretive but, taken in the opposite direction, is also a the-

mars generate is so strong, that it is difficult to imagine their succumbing to any purely experimental disconfirmation” (Fodor & Garrett 1966: 152).

18. Reber (1987: 328) provides the *Science Citation Index* for “Chomsky” from 1964 through 1983 (I assume the discipline concerned is psychology but Reber is unclear on that point). A sharp decline is observable after 1976. This decline occurs in the context of a burgeoning of publications in AI, cognitive psychology, neurosciences etc. In this context, and apart from the demise of generative psycholinguistics, we may surmise that a number of psycholinguists have turned to other frameworks (see below, section 10).

ory of production, or that Chafe's and Fillmore's semantics-centred accounts are notational variants of his own, were hardly more than a sleight of hand and could scarcely mask the lack of consensus which prevailed in linguistics. As appealing as their views might have been, however, generative semanticists were not offering a unified theory, but rather scattered descriptions tackling all kinds of fields newly absorbed into linguistics: quantification, focus, belief-contexts, decompositional semantics and adverbial scope, possible worlds and presuppositions... The over-ambitious scope of their inquiries and a hectic pace of theoretical evolution obviously did not facilitate the elaboration of a unified account and, although a textbook was announced, it failed to materialize (Harris 1993: 219). On the other hand, textbooks of standard TG were being churned out (Bach 1964; Roberts 1964; Thomas 1965; Jacobs & Rosenbaum 1968; the list is not exhaustive), and the institutional clout of interpretive generativists was stronger (Harris 1993).

Eventually, the domains covered by generative semantics were dispersed among specialized disciplines, sometimes incorporated into mainstream generative grammar, and partly reelaborated into strands of cognitive linguistics. For example, problems related to quantification and which had proved to be thorny for Standard Theory were reformulated in formal brands of semantics. From her own testimony, we learn that the problems attending quantifiers were the main reason why Barbara Partee turned away from TG and to Montague's theory (Partee 2011). The use of variables and formulas akin to predicate calculus introduced by generative semantics was imported into generative grammar (in the module of "Logical Form"). Questions involving belief-contexts, possible worlds and presuppositions, then much debated in philosophy and linguistics, would be appropriated in particular by a theory affiliated to cognitive linguistics, the mental spaces approach proposed by Fauconnier, starting in 1978 (see Fortis & Col 2018). More globally, generative semanticists mutated into cognitive linguists (approximately after 1975) and redirected their semantic inquiries within this new theoretical network (Fortis 2015).

The state of dissent among linguists was thus accompanied by a fragmentation of semantics into distinct approaches. For some psy-

chologists and linguists, this situation was compounded by doubts about the possibility of identifying psychologically real semantic representations with componential structures assumed to underlie generation. For example, Fodor et al. (1975), partly from experimental evidence, argued against the psychological reality of definitions assumed by generative semanticists to underlie some lexical items, a classic example of which was the analysis of *kill* as derived from 'cause to become not alive' via a lexical transformation (Predicate Raising, cf. McCawley 1968, 1971). This, again, militated against correlating complexity of linguistic constructs with complexity of processing. Meanwhile, psychologists were drifting to non-behaviorist, constructivist approaches which emphasized that perception, memory, categorization and semantic processing involve recoding, reanalyzing and transforming the sensory input subjects are exposed to (Neisser 1967). Bransford and his associates insisted that the comprehension of sentences could not be based solely on a linguistic analysis treating them as self-contained objects; that is, semantic representations rest on knowledge of the world and the abstraction of "wholistic ideas" summing up various formulations of the same situation (Bransford & Franks 1971; Bransford & Johnson 1973). The constructivist perspective thus tended to divorce semantic representations from any kind of deep structure specifically directed at generating surface sentences.

To sum up this discussion, in addition to the experimental difficulties outlined above, a number of circumstances negatively affected the collaboration of psycholinguistics with generative grammar: the one-sidedness of psychology's relationship with linguistics, the inner strife shaking TG and the ensuing theoretical changes, the fragmentation of semantics into sub-fields taken over by various disciplines, and the new constructivist perspective on semantic representations in psychology.

8. "Psychological reality" and the autonomy of linguistics

Given circumstances adverse to a balanced collaboration between psychology and linguistics, how should we interpret the statement

made by Chomsky in *Language and Mind* (1972b: 1), and repeated afterwards, that linguistics “is a branch of cognitive psychology”? In *Rules and Representations* (1980), written after the divorce between transformational psycholinguistics and GG, the answer is quite plain: linguistics, by its own methods and criteria of validation, is a psychological discipline, more precisely a branch of theoretical psychology “primarily concerned with the genetically determined program that specifies the range of possible grammars for human languages and the particular realizations of this schematism that arise under given conditions” (*ibid.*: 202).

What Chomsky rejects is the view that “certain types of evidence are held to relate to psychological reality, specifically, evidence deriving from studies of reaction time, recognition, recall etc.,” while theoretical explanations advanced to account for evidence based on informant judgments would have no claim to “psychological reality” (1980: 192). There is no such epistemological divide, he says:

...any theory of language, grammar, or whatever carries a truth claim if it is serious, though the supporting argument is, and must be, inconclusive. We will always search for more evidence and for deeper understanding of given evidence which also may lead to change of theory. What the best evidence is depends on the state of the field. The best evidence may be provided by as yet unexplained facts drawn from the language being studied, or from similar facts about other languages, or from psycholinguistic experiments, or from clinical studies of language disability, or from neurology, or from innumerable other sources. [...] But there is no distinction of epistemological category. In each case, we have evidence – good or bad, convincing or not – as to the truth of the theories we are constructing, or if one prefers, as to their “psychological reality”, though this term is best abandoned, as seriously misleading. (Chomsky 1980: 109)

This is a strange argument. What counts as legitimate “evidence” each time hinges on the criteria and on the methods which prevail within a given discipline. Even if we assume that the evidential base of GG is completely sound (but see Riemer 2009), a well-supported generative description simply does not comply with the requirements of experimental psychology. In the eyes of a psychologist, such description must await confirmation and cannot be regarded as valid simply because it is impeccably argued for from a linguistic point

of view. By the same token, and to reiterate a point already made above, “evidence” is shaped by the methods employed to obtain it: reaction times in a psychological experiment are part of what counts as evidence, hence it is requested from the theory that it account for the pattern of reaction times. In practice, this means that parameters extraneous to linguistic forms and their well-formedness must be factored in, for example the truth-value of negative transforms (see above, section 6). This may cause (and has caused) psychological models to diverge from linguistic ones, since psychological models must be specifically designed to fit behavioral data.

A possible way to understand Chomsky’s argument, I suggest, is to consider it in its historical context, as a strategy put in place after the demise of TG and elaborated with the goal of protecting the autonomy of linguistics. In effect, the argument keeps psycholinguists from meddling in the linguist’s affairs, but does not in the least relinquish linguistics’ claim to psychological reality.

What we interpret here as a disguised plea for autonomy is well illustrated in the text in which it is put forth. In the chapter of *Rules and Representations* (1980) entitled “On the biological basis of language capacities”, Chomsky pays lip service to the sum of neurophysiological knowledge in the book which has inspired his own chapter title, Lenneberg’s *Biological Foundations of Language* (1967). Besides a nod of approval to Lenneberg’s argument for innate mechanisms or predispositions, no attempt is made at taking stock of the “evidence” in neurophysiology, aphasiology, language acquisition, etc. Instead, Chomsky devotes a few pages to demonstrating the generalization of the *wh*-island constraint to sentences of the type *I found a violin to play sonatas on*, a generalization he sees as a ground for asserting the soundness of the *wh*-island constraint, and, as a consequence, its “psychological reality” (1980: 193-197)¹⁹.

While the point made by the generalization of the *wh*-constraint concerns the *description* of linguistic structures, the autonomy of linguistic argumentation seems more difficult to maintain when we are

19. The *wh*-island constraint, originally put forth by Ross, prohibits *wh*-movement from a *wh*-clause. For example, *Which sonatas did you find a violin to play on is ruled out by assuming an underlying *wh*-clause of the form *I found* [_{NP} a violin [_S which for PRO to play sonatas on t]].

dealing with language *acquisition*. Yet, the arguments marshalled by Chomsky in favor of his nativist stance essentially rest on invented examples, not, for instance, on a corpus of children's utterances. His objection to analogy as a source of linguistic creativity, for instance, merely rests on the possibility of false analogies between a couple of sentences created for the purpose of giving an illustration of a misleading formal parallelism²⁰. The objection, however, is weakened by real-life observations, which tend to show that children learn constructions in a piecemeal fashion, and extract patterns very progressively, as has been shown by Tomasello (2003)²¹. False analogies of the kind mentioned by Chomsky involve a generalization over constructions of quite different types, a step that is in need of evidential support and would be *prima facie* unlikely if we extrapolate from Tomasello's observations. It is beyond the scope of this paper to examine Chomsky's arguments for his brand of nativism. Suffice it to note here that they are by and large independent from acquisition studies, hence fall in line with the autonomy proclaimed in *Rules and Representations*, and that they form a mixed bag of a priori reasoning (e.g. on constraints on possible hypotheses made by children) and statements in need of empirical support (as in the just mentioned example). In other words, the autonomy of linguistics may verge on speculative thinking in matters regarded elsewhere (in the psychology of acquisition) as empirical.

20. Cf. *Knowledge of Language* (Chomsky 1986: 8, 105). Suppose analogy were the parsing procedure used in analyzing sentences. Then (a') and (b'), which stand in analogous relation to (a) and (b), would be parsed identically:

(a) John ate an apple.

(a') John ate.

(b) John is too stubborn to talk to Bill.

(b') John is too stubborn to talk to.

But they are not parsed identically. *Ergo*, analogy is not the procedure used.

21. An objection already raised by Prideaux (1975) when discussing the transformational account of children's acquisition of interrogative sentences put forward by Klima & Bellugi (1966).

9. The linguistic aftermath

The brief marriage of transformational psycholinguistics and generative grammar brought into relief processing factors involved in the parsing of structures. This offered a favorable context for an inversion of perspective, the idea that processing demands and heuristics play a role in shaping linguistic structures themselves. In this new perspective, complexity of processing is no longer a property of the mechanism operating on independently motivated linguistic structures; rather, linguistic structures partly reflect strategies aimed at reducing processing complexity. This inversion seems to have occurred in several steps, and through different ways. The studies mentioned below are meant to illustrate this evolution, but others might have been cited.

An early step in this change of perspective is the study of Fodor & Garrett (1967). Their reasoning is worth retracing. We have reasons to believe, they point out, that understanding a sentence is not straightforwardly related to transformational complexity (see section 6). Processing rests on cues of various kinds which together help subjects home in on the right analysis. An example of a cue which helps reduce parsing uncertainty is furnished in the initial string of (1). Uncertainty is relatively high in (1), since it could be followed by a conjunct or the predicate of a relative clause or a relative clause, etc. The addition of *whom* in (2) rules out one possibility, that is, the conjunct, thus reducing processing complexity.

(1) *The boy the girl...* [*The boy the girl...and the man left...*, *The boy the girl...met.....*]

(2) *The boy whom the girl...*

Note that *wanted* in (3) would augment uncertainty with respect to *met*:

(3) *The boy whom the girl wanted...* [or wanted to meet..., or wanted the man to meet...] vs *The boy whom the girl met*.

Uncertainty rises with *wanted*, because of its having alternate constructional possibilities. In this framework, then, subjects do not undo transformations. They use whatever cues are available along the

incoming string that is being processed, be they semantic, syntactic, lexical (e.g. the subcategorization frame of a verb).

A further step is taken by Bever in his paper on “the cognitive basis of linguistic structures” (1970), in which it is contended that processing heuristics (in his terms, “perceptual strategies”) reflecting cognitive biases (“perceptual constraints”) may *determine* the grammaticality of sentences. In his own words, Bever suggests “that certain aspects of sentence structure reflect the perceptual constraints placed on it by the child as he learns the structure and by the adult as he uses the structure” (1970: 313). One of the cognitive biases identified by Bever would be the default segmentation of strings into ‘actor action object...modifier’ roles, used by children acquiring a first language. This early bias would underlie a default perceptual strategy favoring in adults as well the analysis of an initial string (in English, a NP VP string, for example) as a main clause. A consequence of this perceptual strategy would be the introduction of a marker of subordination to prevent an initial NP VP structure from being interpreted as a main clause²²:

(4) *The fact that / That / The fact the door was discovered to be unlocked amazed the tenants.*

But not

(5) **The door was discovered to be unlocked amazed the tenants.*

In Bever’s paper, other “aspects of sentence structure” for which “perceptual strategies” determine grammaticality involve pronominal coreference and constraints on the deletion of relative pronouns,

22. This strategy would be strong for English, but presumably weaker or non-existent in languages (like Japanese) which allow initial subordinate clauses with no initial marker of subordination. Note that the Main Clause strategy, if extended to all languages, would make processing left-branching structures (as, again, in Japanese) inherently more complex: whether a clause is a matrix clause or a subordinate one is undetermined until a complementizer or the clause second boundary is received (see the discussion in Dryer 1980). The “actor action object” bias in the linguistic construal of states of affairs and events has a long history (see e.g. Knobloch 1988: 357). The relative novelty of Bever’s account lies in the ontogenetic (rather than phylogenetic) role of this heuristic, and its operating as a default parsing strategy.

that is, issues that generative grammar handled in formal, configurational terms.

Among the attempts to derive structural facts from processing considerations, one particularly stands out for its typological scope and its ambition to explain observational generalizations, and that is the line of research concerned with word order universals. Perhaps the most remarkable efforts in this direction are due to Hawkins, who himself based much of his early theory (in Hawkins 1983) on Greenberg's essentially inductive universals (1966), and on Vennemann's (notably 1974a, 1974b) and Keenan's (e.g. 1973, 1978, 1979) tentative accounts to formulate very general principles constraining the alignment of syntactic order, for example head-modifier vs modifier-head, across different syntactic categories. In Hawkins' own account, one of the most important explanatory principles, the "Heaviness Hierarchy", ranks modifiers in their potential to move to the right of the modified element, eventually in violation of the dominant order of a given language. On Hawkins' view, "heaviness" acts as a psychological parameter: the term sums up a number of processing constraints which directly condition the syntactic order of constituents. With a typological scope, hence a cross-linguistic generality which far exceeds Bever (1970), processing costs are no longer seen as a filter on structures compatible with competence, but as "one of the causal factors explaining why languages employ the rules and structures that they do" (Hawkins 1983: 104).

That processing factors may account for facts of linguistic structure is also a point made repeatedly by Givón in his first book, *On Understanding Grammar* (1979). Givón's undertaking, however, goes beyond this. It illustrates the broader view that linguistic structure is functionally motivated, the reduction of processing costs being but one of the functions performed by grammar. A clear connection is thereby established between "psycholinguistic" considerations, and a broader functional perspective for which processing factors are one of the motivations accounting for grammatical structure, understood as a "solidification" of discourse structure, world categorization, cognitive and ontogenetic constraints, peppered with vestiges of diachronic evolution (1979: 3-4). Givón's attitude evinces a frustration we find expressed in the work of other scholars: generative grammar

merely stipulates rules which are descriptive of grammatical facts but which are not explanatory. The same quest for explanations external to formal generation characterizes one of Langacker's first "pre-cognitive" attempts. In his 1974 paper on raising rules, their very existence is justified by the higher "degree of prominence" which they assign to the raised constituent. Here, "prominence" appears to be a precursor of Langacker's future cognitive and monological conception of Gestalt-like saliency; it is not intended as a pragmatic notion. On the other hand, pragmatic considerations are at the forefront in Hooper [= Joan Bybee] & Thompson's study on "root transformations" (1973)²³. Just like for Bolinger (1977) and others, for Hooper & Thompson the actual non-synonymy of transforms points to the semantic laxity of transformationalists. It is also an invitation to seek non-syntactic parameters explaining limitations on the productivity of transformations. In the case at hand, they hypothesize the motivation of root transformations resides in the speaker's intention to "emphasize some particular element", with the corollary that this element should not be presupposed in the discourse situation. Taking into account pragmatic intention introduces another layer of complexity to constraints on transformations, potentially interacting with processing factors. For example, the difference between (5) and (6) could be claimed to rest on the fact that the transformation Comp Preposing is ruled out when the main clause is asserted (as in example 5); in (6), *he said* is not the focus of the assertion but is claimed to be merely parenthetical.

(5) **It's just started to rain, he didn't say.* [Comp Preposing]

(6) *It's just started to rain, he said* [parenthetical].²⁴

23. Roughly, transformations in which the moved constituent is immediately dominated by S. A simple example is VP-preposing, illustrated for instance by *Mary plans for John to marry her, and marry her he will.*

24. In the first version of this paper, the examples taken from Hooper & Thompson concerned Negative Constituent Preposing. They were:

(5') **That never in his life has he had to borrow money is true.*

(6') *It's true* [= parenthetical] *that never in his life has he had to borrow money.*

Both reviewers found (5') perfectly acceptable. John Joseph (p. c.) remarks that a number of sentences marked as ungrammatical by Hooper and Thompson are quite acceptable to him. This raises again the issue of the soundness of data

In these instances, issues mapped out by generativists and first tackled as a matter of formal grammar (in linguistics) and various heuristics (in psycholinguistics) usher into broader functionalist views. On the side of early functionalists and cognitive linguists, normal description is no longer deemed as explanatory and it is seriously doubted whether sentences related by transformations are cognitively, semantically or pragmatically equivalent.

10. By way of conclusion: some notes on the subsequent evolution of psycholinguistics

As we have just seen, the failed partnership of TG and psycholinguistics proved indirectly fertile by directing the attention to parameters involving functional motivation, cognitive, semantic and pragmatic constraints. The divorce, therefore, should not be evaluated too negatively. In the short-term, however, psycholinguists were left without a general blueprint for the syntactic processing of surface structures. As Frazier notes (1988: 15): “Considerable psychological evidence showed that the structures the grammar attributes to sentences are psychologically real, even if the transformational rules of the 1960-style grammar were not. Since the only known characterization of surface structures was the one provided by transformational grammar, this state of affairs presented an apparent contradiction. In response to this situation, several models of comprehension were developed in which grammatical information was either not used, used haphazardly, along with sundry heuristics and heavy reliance on lexical and world knowledge...” Clark & Clark’s psycholinguistics textbook (1977) is a good illustration: constituency (“the structures the grammar attributes to sentences”, in Frazier’s words) remains the firmest gain of experimental research and parsing has dissolved into a set of “strategies”, often amounting to paraphrasing grammatical and pragmatic facts as a set of instructions (“when you have a determiner, open an NP and look for a closing N”, “look for given information to precede new information”). This bottom-up, category-based tendency also manifests itself in their nod

based on the linguist’s intuition, a problem that seems to be particularly acute in generative grammar (Riems 2009).

of approval to lexically- and valency-based accounts (invoking for example Schank 1972). In a framework such as this one, complexity of processing can be calculated by the degree of deviation from the set of heuristics brought to bear on an utterance. However, since strategies encapsulate all sorts of structural facts, as well as semantic and pragmatic biases, we are no longer on the safe ground of a self-contained syntax, and the possibility of giving an operational definition of complexity, one amenable to experimental testing, appears as an unrealistic hope.

The divorce that took place in the 1970s is associated with the redirection of psycholinguistics toward low-level processes (e.g. word recognition, especially after Morton 1969), and toward the study of meaning (Tanenhaus 1988). While semantics was the subject of a growing interest on the part of American linguists, phonological and morpho-syntactic matters remained the center of gravity of linguistic theorizing. As far as linguistic semantics was concerned, and although the psychological reality of semantic constructs seemed to be the concern of every linguist, including supporters of formal models (e.g. Partee 1979), this preoccupation found very little echo in psychology. If we except a few scattered attempts (e.g. Fodor *et al.* 1975 on the psychological reality of definitions), the psycholinguistics of meaning and modelling systems with a psychological ambition do not serve as an ancilla linguisticae, as is attested, for example, by the flourishing of semantic descriptions of lexical items, texts and real-world knowledge in the form of predicate calculus and propositional systems (e.g. Kintsch 1974, Miller & Johnson-Laird 1976), and semantic networks (especially after Quillian 1966; for a critical overview, see Rastier 2010 [1991]). To a certain extent, the tables are turned: the redirection of psycholinguistics toward semantic questions opens up new niches of investigation for linguists. As argued by Honeck (1980), for example, Lakoff and Johnson's conceptual metaphor theory (Lakoff & Johnson 1980) should be situated in the context of an interest in metaphor on the part of psychologists during the 1970s, an interest which Honeck regards as a side-effect of the demise of transformational psycholinguistics. Likewise, prototype theory, predominantly rooted in psychology, would be instrumental in reviving lexical semantics in the U.S. (Fortis 2018). But note that in both fields,

the ties of linguistics with psychological and neuroscientific research, initially at least, were thin. Thus, after a simplified version of prototype theory had been imported into linguistics, linguists lost interest in the evolution of psychological theories of categorization, and very little experimental research was done. More significant interdisciplinary connections, however, would be progressively established in the field of conceptual metaphor, and increasingly so thanks to an intersection between the empiricist assumptions of this theory and pleas being made for a reevaluation of the role of the body in cognition from the 1990s on²⁵. Today, this intersection is quite manifest in research conducted in the rather vast domain that goes by the name of “grounded cognition” (e.g. Barsalou 2008).

On the syntactic front and mostly after the 1970s, we can discern a turn to a conception of processing in which syntax forms a self-contained system again. This turn seems to have two main motivations. First, experiments showed that syntactic parsing strategies were applied in an “automatic” way, in particular immune from semantic biases. Early studies in this direction were able to cast doubt on what seemed to be a particularly robust manifestation of such a semantic bias, namely the role of argument reversibility in passives (see *supra*, section 6). By teasing out the effect of reversibility and the inherent plausibility of situations depicted in carefully controlled sentences, Forster & Olbrei (1973) showed that processing time was a function of syntactic complexity, whether the sentences were reversible or not. In addition, a whole literature on “garden-path sentences” and their online treatment developed out of an interest in the default principles used in building up syntactic structure (for an overview, see Frazier & Clifton 1996, introduction). Through this avenue of research, online parsing strategies exploiting purely syntactic principles offered the dynamic view of constituent structure that psycholinguists had been so glad to seize upon in the time of TG. The second motivation for regarding the syntactic processor as a self-contained system lay in the evolution of linguistic theory itself. This aspect is clearly articulated in Frazier’s defense of her Garden Path Theory of syntactic processing: her conception of grammar is modular in the

25. E.g. Varela *et al.* 1991; Lakoff & Johnson 1999; Joseph 2018 for a long-term historical account.

sense in which Government and Binding Theory is, assuming for example a separate processor for thematic relations. The irrepressible nature of the processing of linguistic form was also adduced by Fodor in support of his modular conception of mental architecture, in his *Modularity of Mind* (1983). Against any view arguing for an influence of contextual content on parsing, he declared rather tersely that “linguistic form recognition can’t be context-driven because context doesn’t determine form; if linguistic form is recognized at all, it must be by largely encapsulated processes” (Fodor 1983: 90). His plea for “encapsulated” subsystems did not rest, in the case of linguistic parsing, on a thorough review of the literature, yet its appeal was considerable, consonant as it was and would be with research conducted in a nativist spirit, for example studies aimed at demonstrating the existence of domain-specific processors, or “proto-theories” at early stages of human development (e.g. Spelke 1988, 1990). The “encapsulation” of syntax as a hypothesis defensible again in psychology should be understood in this wider epistemological context, of which we have been able to offer but a sketch.

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