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Jerrold Katz, *Language and other abstract objects*. Basil Blackwell, Oxford, 1981. 251 pp.

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Since the mid-seventies a lively debate has been going on concerning the question whether or to what extent linguistics is an *empirical* science, 'empirical' being defined, generally, as 'testable on the basis of spatio-temporal evidence' (cf. e.g. Itkonen 1974, 1978; Ringen 1975; Dahl 1975; Lass 1976; Linell 1976; Hammarström 1978). At the XIIth International Congress of Linguists a round-table discussion was devoted to this topic, and three of the eight participants, namely Itkonen, Kac, and Ringen, represented the view according to which the intuition-based grammatical theory, as exemplified by Saussurean autonomous linguistics or by the standard type of transformational-generative analysis, qualifies as non-empirical (cf. Dressler and Meid 1978: 155–162). The majority opinion on the issue has been expressed e.g. by Chomsky (1980: 102–109), who denies that there is any difference between linguistics (understood as part of psychology) and the natural sciences. For a long time, Jerrold Katz was one of the most fervent proponents of the same position. For instance, Chomsky and Katz (1974: 354) explicitly claimed that "linguistics is an empirical science, not a branch of logic and mathematics". In the book under review, however, K. has

made a complete methodological and philosophical turn-about. He now argues that linguistics (in the sense of 'grammatical theory') is, after all, non-empirical in character and thus comparable to logic and mathematics.

As far as the scientific status of linguistics is concerned, K. distinguishes between three principal positions, namely nominalism, conceptualism, and Platonism, which he sees as represented by Harris, Chomsky, and himself, respectively. This requires three comments. First, it would be terminologically and historically more accurate to operate with the trichotomy 'physical–psychological–conceptual', as it occurs e.g. in Popper's (1972) conception of the 'three worlds'. Second, the real issue here concerns the question whether linguistics is a science in some sense *autonomous* vis-à-vis psychology or merely constitutes one of its subdomains; adopting Platonism is only one way of adopting the former alternative. But then it is seriously misleading, from the viewpoint of the history of linguistics, to confine the discussion to Chomsky and K., and even to imply that 'autonomism' is a novelty vis-à-vis psychologism. The opposite is the case. The tradition of Western linguistics, from Dionysios Thrax via the medieval modistae and the Port-Royal grammarians to Saussure, Hjelmslev, and Harris (sic!), is that of autonomous linguistics (cf. Yngve 1981). The same is true of the Pāṇinian tradition in India (cf. Misra 1966: 12). In the West, psychologism emerged as a serious alternative only in connection with Steinthal (1871) and Paul (1880). Third, from today's perspective too, it is misleading to confine the discussion to Chomsky and K., because detailed arguments both for autonomism and for psychologism have become rather commonplace (cf. Kac 1980 and Derwing 1980, in addition to the references given above).

In the remainder of this review I shall proceed as follows. I shall first present K.'s overall argument, which I take to be valid. I shall then proceed to suggest some respects in which his argument could, in my opinion, be improved upon.

K. holds *intuition* to be a concept which, in spite of its centrality, has been poorly understood. As a type of knowledge, it has to be distinguished from perception, on the one hand, and from introspection, on the other (178). In all three instances, furthermore, the act (or the source) of knowledge must be distinguished from the object of knowledge (77–78). When we speak e.g. of the (intuitive) knowledge of English, we must be able to define 'English', i.e. what the knowledge in question is knowledge of (81). Although the act of intuition is psychological in character, it is a mistake to assume that the same is true of the object of intuition. To see this, one only has to think of mathematical intuition or of logical intuition, which are about such non-psychological entities as numbers and implication relations, respectively. Analogously, linguistic intuition is about sentences, which must then be taken to be *abstract objects* of some sort. And just as the intuition-based formal logic quite obviously differs from the experimental psychology of logical thinking, so grammatical theory too differs from psycholinguistics. Frege and Husserl have exposed the fallacy of psychologism in formal logic, and the same argument applies to grammatical theory

too (160–179). In particular, psychologism (including Chomsky's nativism) is unable to account for necessary truth, because the only entities accepted by psychologism, i.e. physical and/or psychological states and processes, are contingent in character (5–6, 115).

Taking grammatical descriptions to be about psychological states and processes means unnecessarily limiting their abstractness, or imposing upon them a set of irrelevant, non-grammatical constraints (82–87). (Here it is tempting to construe K.'s argument so as to place grammatical theory within the *axiomatic* tradition. This would produce the necessary distance vis-à-vis psycholinguistics because, as recent studies have amply demonstrated, the (untrained) human mind does not operate axiomatically, i.e. with a minimal amount of (non-redundant) axioms and inference rules and, in consequence, with long derivations.) Psycholinguistic descriptions are about the *causation* of linguistic behavior, whereas grammatical descriptions, being about abstract objects, are necessarily non-causal (49, 65). It is for the same reason that grammatical descriptions qualify as non-empirical. It is important to realize that the 'empirical–non-empirical' distinction is value-free. In particular, it must not be confused with the distinction between testable and untestable, or between what there is and what there is not evidence for (23, 73).

Intuition provides a direct access to sentence structures, whereas the information given by observation/experimentation is mediated and distorted by intervening factors. This means that, contrary to Chomsky, there is a significant distinction to be made between direct (or 'internal') and indirect (or 'external') evidence, a distinction corresponding to that between grammatical theory and psycho-linguistics (71–72). (This could be clarified as follows. Linguists typically try to answer two different types of question: *What* are the properties of e.g. the English sentences? and *How* are such properties acquired, stored, perceived, produced, or changed? The what-question, to be answered by grammatical theory, is the logically primary one, which means that the how-question, to be answered by different forms of causal linguistics, is in reality always a what-and-how-question.)

The correctness of K.'s general position is directly confirmed by the grammarians' actual descriptive practice. Unlike empirical scientists, they do not use experimental or observational techniques to establish regularities of spatio-temporal events. Rather, their 'data-gathering' consists in reflecting upon their own intuitive knowledge (of abstract objects). If grammatical theory were an empirical science, it would be a complete mystery how, within it, "a single clear case can warrant certainty" (215). This mystery is removed by equating grammatical theory with such non-empirical, intuitional sciences as logic and mathematics.

Up to now, K.'s argument does not noticeably differ from that presented e.g. in Itkonen (1978, 1981). It is his Platonism which constitutes K.'s original contribution to the standpoint of autonomism. It would seem more correct to me, however, to let historical and social *norms* (of language), rather than 'eternal' Platonic entities, stand for the abstract objects analyzed by grammatical theory. Recall the claim that

we must be able to define the 'English' which the knowledge of English is knowledge of. Now the supporters of psychologism are likely to reply that the knowledge of English is about regularities of (beliefs and/or expectations about) actions of a certain complex type, thus reducing grammatical theory to socio- or psycholinguistics. Apart from the issue of necessary truth, Platonism would have great difficulties in refuting this argument. But it can be rather convincingly refuted by demonstrating that there is no way to reduce norms of language to (non-normative) regularities of (beliefs and/or expectations about) linguistic actions (cf. Itkonen 1978: 7.0). The non-empirical character of grammatical theory is demonstrated at the same time.

There are some additional reasons why a norm-based interpretation of autonomism seems preferable to a Platonist one. K. rightly emphasizes Frege's (1884) and Husserl's (1913) importance in refuting psychologism, but he manages to de-emphasize the ultimately *normative* justification they give to this refutation. As quoted by K., Frege takes "laws of logic to be laws that prescribe the way in which one ought to think" (171), and Husserl stresses the paramount importance of "the fundamental, essential, never-to-be-bridged gulf between ... normative and causal regulation" (176). Later on, Husserl (1954) comes to provide the norms of logic with a social interpretation by 'deriving' them from the prescientific 'life-world' (cf. Itkonen 1978: 46–48, 53–54). This development anticipates constructivism in logic, as represented, in different forms, by Lorenzen and Dummett. It is a mistake to think, as K. does (185), that constructivism is a species of psychologism, because within it proofs are taken to be constructed in accordance with *intersubjectively* valid norms, which may occasionally be violated.

K. explicitly claims that a human language is not a social, but a mathematical entity (7–8, 48). It is hard to see the justification for this claim, because languages quite obviously differ from mathematical entities in being subject to diachronic, geographic, and social variation. It would be much more natural to compare grammatical theory e.g. to social philosophy, which is also a discipline analyzing concepts liable to change. Any comprehensive version of sociology contains social philosophy as a necessary component (cf. Ryan 1970: 145). If K. is right, social concepts and meanings in general must be reinterpreted as mathematical entities. This view may not be literally false, but at least it is very odd. Just consider the Platonist conception of linguistic change. According to it, the infinite number of all "conceivable and inconceivable" (9) languages exist as timeless possibilities, and a linguistic change consists in the fact that a community ceases to entertain one possibility and begins to entertain another. This account is certainly much less natural than the standard teleological view according to which people change their ways of speaking in order to achieve a better (local) compromise between the economy of linguistic form and the effectiveness of linguistic communication. The social conception of language (including linguistic change) agrees with constructivism insofar as language and logic are regarded as *man-made*. Once again, the common denominator is the concept of norm. The difference between formal logic and grammatical theory is, in turn,

accounted for in terms of the universality (or immutability) vs. the particularity (or mutability) of the respective norms.

It makes perfectly good sense for K. to concentrate on *clear cases* because these constitute the inescapable starting point for any grammatical description (cf. Lyons 1977: 385). However, there is also a huge number of less-than-clear cases, as the representatives of different schools of 'variationism' have been anxious to point out, and K.'s treatment of this issue is not very satisfactory. He clings to Chomsky's categorical view of language, according to which sentences are 'in reality' either grammatical or ungrammatical, and he even argues that accepting a continuum of grammatical correctness is just an 'artifact' of Harris' grammar-conception (32, 57, 124). Once again, it would be more natural to resort to the concept of norm and to admit that some norms (i.e. those governing the clear cases) are absolutely binding, whereas others are less and less so.

Considering that K. rests his case entirely on the analogy between formal logic and grammatical theory, he gives a surprisingly sketchy account of it. No systematic comparison is made between grammars and systems of logic, and passing references are given only to classical propositional and predicate calculus. As a result, K.'s argument remains less convincing than it could be. A revealing analogy between formal logic and grammatical theory can be established only by comparing grammars to systems of such a non-classical type of logic as deontic logic. In both instances, old theories have been (non-empirically) falsified, and superseded by new ones, which remain (non-empirically) falsifiable (cf. Itkonen 1975, 1978: 10.0). It should also be mentioned that while grammatical theory is content to describe existing norms (of speaking), formal logic also creates new norms (of inferring). Moreover, K.'s position entails that "the question of whether [grammatical objects] have this or that property is necessary" (208), but he nowhere explains what it means, precisely, for e.g. morphological descriptions to be 'necessary'. One solution to this problem, borrowed from Pap (1958), is given in Itkonen (1978: 11.2–3).

Let us finally have a closer look at K.'s constructive proposals. What are Platonic abstract objects like? They are defined to be non-temporal, non-spatial, objective, and "cohesive in the sense of having logically inseparable basic properties" (186). How can we get to know such objects? This presents K. with a problem. In perception, for instance, there is a causal relation between the knower and the known such that the latter affects the former. This possibility is not open to K. because his abstract objects, being non-temporal and non-spatial, cannot enter into causal relations. K. tries to escape from this dilemma by making the following assumptions (202–206). First, we are able to construct internal (i.e. psychological) representations which approximately correspond to abstract objects. Second, we possess an innate idea of abstract object. Third, we possess an innate idea of a 'knowledge-of' relation, exemplified by internal representations and abstract objects. Does this rather exotic apparatus succeed in showing that we can have knowledge of something that has no causal relation to us? I do not think so. It remains quite unexplainable why there

should be so good a fit between abstract objects, on the one hand, and innate ideas and internal representations, on the other. It seems more reasonable to me to accept the existence of causal relations, but to reverse the direction of causation: We as *individuals* construct sentences or proofs in accordance with norms entertained by us as a *community*. On this view, we have to posit a dichotomy between (social) norms and (psychological) internalizations-of-norms, a dichotomy reminiscent of K.'s apparently redundant dichotomy between abstract objects and internal representations. We also have to accept a distinction between the *agent's knowledge* (characteristic of the intuitional sciences) and the *observer's knowledge* (characteristic of the natural sciences) as well as a correspondingly 'heterogeneous' conception of truth (cf. Itkonen 1978: 8.1). Although K. explicitly opposes this conception (181, 203), he comes in the end to entertain a view very similar to it: "There is, then, a sense in which the sciences of the intuition do not rest on faith in something beyond us. In these sciences, reason replaces faith that empirical objects and regularities will be like our experiential concepts of them: no faith in nature is necessary because reason and intuition can establish a possibility as the only possibility" (212).

K. has written a book which deserves to be read by anyone interested in the philosophy and the general methodology of linguistics. As I have tried to show, one can accept much of what K. says, without going all the way and accepting his Platonism.

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Janice Moulton, George Robinson, *The organization of language*. Cambridge University Press, Cambridge, 1981. xvi, 389 pp. Cloth US$42.50, paper US$14.95.

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This new book by Janice Moulton and George Robinson (henceforth M&R) is an intriguing new work in syntax. It is extremely well written, and all points are forceful and very clear. Linguists and cognitive scientists with an interest in the organization of syntax should find many of the claims here quite provocative. There are, however, several serious flaws that destine this work to be no more than interesting reading.

This book details a new, nontransformational theory of syntax and conceptual structure. As such, its title is extremely misleading. Far from a complete discussion of the organization of language, this work never addresses phonetics, phonology, or morphology, and such areas as the lexicon, semantics, and pragmatics are not dealt with in detail. However, it does discuss conceptual structure (chapter 2), including a discussion of some relevant experimental work (chapter 3), and syntax, including the drawbacks of some theories (chapter 4), M&R's own version of syntactic representations (chapter 5), and the acquisition of syntax (chapter 6). There are three useful appendices. Appendix A, written by Paul Gruenewald, demonstrates that M&R's theory of syntax can be implemented as a computer model of syntactic parsing. Appendices B and C are intended to give the reader more familiarity with the syntax crystal model and how it may be acquired. They do this by providing information in a game form. Appendix B in particular is not only an effective aid, but is a pleasant way to spend time for the game enthusiast. M&R's treatment of these various areas is quite uneven. Some chapters, and all the appendices, are very good, while others have serious drawbacks.

Several parts of the book provide excellent overviews of controversial topics. Chapter 1 is a very good introduction to syntax, discussing not only traditional syntax, but primitive and iconic syntax as well. It includes a very clear and forceful justification of hierarchical structure in syntax. The first half of chapter 6 is dedicated to the question of the innateness of language. It is a good, readable overview of the arguments, and leads quite compellingly to M&R's conclusion that innateness is not necessary to account for language acquisition. Chapters 2, 3, and 4 extensively discuss case grammar and transformational grammar, arguing that they both provide representations of underlying conceptual organization that are far richer than necessary to account for the facts of language, and pointing out assumptions of the models that in fact seem to be incorrect. While M&R do not prove that either of these approaches is wrong, they do argue that they should be abandoned on the basis of simplicity. Their arguments will be of interest to practitioners of other current approaches to syntax that seek to minimize or eliminate transformations.

Chapters 2 and 5 present the core parts of M&R's theory of syntax. They assume the existence of two linguistic levels relevant to syntax: underlying conceptual structure (equivalent to deep structure, or the conceptual staging area of Bock 1982) and syntactic structure (equivalent to surface structure). To represent information at the conceptual level, M&R introduce a representation known as the "orrery", that consists solely of lexemes connected by a very simple hierarchical structure that encodes only the fact that elements are related to each other (scope) and which of several elements is the most important (dependency). Orreries specifically contain no information about the serial order of lexemes or about the case relations or grammatical relations that the lexemes enter into. M&R maintain that scope and dependency information alone are sufficient to describe all possible syntactic data in any language. However, actual linguistic output must be ordered, since speakers produce one lexeme at a time in specific orders. Orreries are mapped onto another form of representation at the syntactic level: the syntax crystal modules. M&R propose that there are syntactic modules, equivalent in size to nodes in phrase structure grammar. These modules encode how a given lexeme can be combined with other lexemes. Each module is represented as a square with a code on from one to four sides. The code on the bottom is for subcategorization, determining what types of lexemes (or other nodes) can be dominated by that module. Codes on the sides determine what type of elements can immediately precede or follow that module in syntactic structure. Codes on the top connect to higher syntax crystal modules that also determine what can precede or follow that module. Mapping from orreries to syntactic modules takes place in a bottom-up fashion (p. 181). The lexemes represented in the orrery are first selected. These lexemes