Linguistic Intuition and Reductionism:
Comments on Katz's Paper

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In his paper "Common sense in semantics" Katz claims that his approach to semantics is "unique among approaches to the subject in the philosophy of language, linguistics, and logic" (p. 174). Katz tells us that what renders his approach unique is, firstly, that his theory is not "reductionistic" and, secondly, that on his approach "meanings are taken as they present themselves in our ordinary experience with natural language" (p. 174). I shall discuss separately each of these alleged differences between Katz's approaches and other approaches.

Katz on linguistic intuition   According to Katz, his approach is "distinctive in accepting the commonsense notion of meaning as the proper object of study in semantics" (p. 180). He wants to claim that "there *is* a particular way in which senses or meanings present themselves to us [and that] knowledge of this way is a matter of common sense" (pp. 174-175). Pretheoretic, linguistic common sense or intuition is "both our initial source and final arbiter" (p. 179). It supplies the data, it provides the facts, and all that remains for the theorist to do is to codify, systematize, or formalize these pretheoretically given facts. In this sense, the pretheoretically given intuitive facts are strictly prior to all theoretization; no feedback from the latter to the former is allowed. Thus there is a well-defined subject matter of semantics which provides the firm foundation for the theoretician's efforts. Furthermore, the subject matter is absolute, incorrigible, and undefeatable by any theoretical considerations. The intuitive facts supplied by 'linguistic common sense' "are as certain as the propositions on G. E. Moore's list in 'A Defense of Common Sense'" (p. 175). "In irreconcilable conflicts with clear intuition, it is always the nonintuitional source of evidence that goes" (p. 178).

These are strong methodological claims. Below, I shall argue that Katz's

methodological perspective on semantics, as outlined in his paper, is radically wrong. I shall argue, contrary to Katz's position, that there is no well-defined, pretheoretically given, incorrigible subject matter of semantics; pretheoretic linguistic common sense does not provide an absolute and undefeatable domain of raw facts a semantic theory is obliged to systematize and characterize. On the contrary, my thesis will be that the domain of semantic data is not given in advance (pretheoretically) but is determined by such matters as one's overall picture of language, one's conception of the relations and division of labour of various areas in the study of language, often tacitly assumed methodological assumptions, and even by scholarly taste.

Let us attack the problem in terms of an example.

Consider the semantics of the quantifier phrase "every boy" in the following two examples:

(1)   Every boy was running.
(2)   The kinds were playing in the yard. Every boy was running.

Suppose each of these sentences is understood literally. Our pretheoretic linguistic common sense, in Katz's sense or anybody else's, provides us with the following pretheoretic *facts*. First, (1) is not true in a situation where at least one boy did not run; secondly, (under one natural reading of it) (2) could be true in such a situation. Furthermore, all of these facts are semantic and draw from our pretheoretic intuitions about the meaning of (1) and (2).

Now if a semantic theory is the study of meanings "as they present themselves in our ordinary experience with natural language" and "theories which fail to square with them are *ipso facto* rejected" (p. 175), then the "intuitive facts" quoted above *alone* suffice to refute any logically oriented semantics for quantifier phrases. Even if model theoretic semantics for quantifiers works for (1), it does not work for (2), because the kind of 'sentential cohesion' involved in (2) calls for features going beyond the formal semanticist's resources.

One might think that these considerations just strengthen Katz's case. Katz has never been a sympathizer of formal (model theoretic) semantics. Thus, as far as he is concerned, any argument against it is welcome. The above argument was a direct application of Katz's methodological maxims. Ergo: so much better for Katz, one might think.

But is semantic argumentation really *that* easy? Surely not: the above 'refutation' of model theoretic semantics for quantifiers overlooks the fact that the theory was never even *intended* to capture the kind of phenomenon the 'refutation' is based on. Model theoretic semantics deliberately overlooks facts of text-semantics, simply because it endorses a methodological working assumption that runs somewhat as follows: let us first develop a semantic theory that applies to sentences; after that, let us move on to more continuous text, to phenomena that carry over sentential boundaries.

Furthermore, one can argue that this methodological working assumption, which restricts one's data base, is not arbitrary or *ad hoc*. For instance, it is plausible to assume that even if we restrict attention to sentences we can characterize interesting semantic features of quantifiers *vis-à-vis* one another. Likewise, it is also plausible to assume that as we move to larger semantic units

(across sentential boundaries), the phenomena that arise encounter all quanti-
fiers alike. Thus, for instance, the phenomenon we face in (2) manifests itself
with equal force if we substitute "several boys", "two of the boys", or "the
boy in a red overall" for "every boy" in (2). All of this goes to show that we
are not dealing with a peculiarity of the quantifier "every" in (2). Thus it is at
least possible that the semantic peculiarities of this quantifier could be charac-
terized even if the kind of phenomenon exemplified by (2) were ignored. Thus,
the counterexample to model theoretic semantics for quantifiers would
collapse.

   No such ignoring-pretheoretic-intuitive-data is permissible for a Katzian
student of semantics. For him, the data base (pretheoretic semantic facts) is
delivered once and for all. "Theories which fail to square with them are *ipso
facto* rejected" (p. 175).

   According to the view I am trying to defend here, a theory that fails to
square with intuitive semantic facts is *not ipso facto* rejected. The above
example readily suggests why this is the case. The reason is that *even if* we
assume for a moment that 'linguistic common sense' provides us with absolute,
incorrigible and undefeatable data, 'linguistic common sense' does *not* deter-
mine just which data are 'semantic', which are not. Much less can it provide,
qua pretheoretically given data, answers to questions such as 'Can a successful
overall semantic theory be built on a semantic (sub)theory that operates on
such-and-such a level of abstraction and ignores such-and-such phenomena (e.g.,
ignores text-semantic phenomena)?' It is indeed absurd to assume that pre-
theoretic intuition could supply *any* informative, much less undefeatable and
absolute answers to such theoretical questions. However, the relevance of the
'intuitive semantic data' exemplified by (2) for an evaluation of model theoretic
semantics of quantifiers depends crucially on one's position on this kind of
theoretical question.

   This means that the problem a semanticist faces is twofold: first, how to
choose one's data base, and only secondly, how to develop a theory that
squares with that data base. The former problem, while crucial, does not find
any place in the "common sense methodology" that Katz outlines.

   Let me take another illustration:

(3)   Socrates owned a dog, and it bit Socrates.

A number of scholars throughout the years have suggested that (under the
natural anaphoric reading of (3)) the behaviour of "a dog" and "it" corresponds
strictly to the behaviour of an existential quantifier and a variable bound by it
in ordinary quantification theory. (See, e.g., [1], for a proposal along these
lines. The same idea has been used extensively in Montague Grammar.) One's
semantic representation for (3) would thus look like this:

(4)   (Ex) (Owns(Socrates,x) & Bit(x,Socrates)).

It is clear that (4) (together with standard model theory for first-order logic)
captures a fair amount of the semantic intuitions our "linguistic common
sense" associates with (3). However, it is equally clear that (4) does not capture
the unmistakable intuitive fact that in our actual understanding of (3), the

anaphoric pronoun "it" *refers back* to the dog mentioned earlier in the same sentence. That is, intuitively the anaphoric pronoun is processed *later* than its antecedent. This fact is not reflected in the semantic representation, for in standard quantification theory each occurrence of a variable bound to a quantifier is evaluated at the same time as the quantifier.

The question is: should the intuitive fact just mentioned be codified into any adequate semantic theory of anaphoric pronouns?

No unequivocal answer to this question is in the offing. On the one hand, one could argue for the positive answer by endorsing an overall picture of a semantic theory according to which semantics should in some way model actual semantic processing (among other things). But on the other hand, one could argue for the opposite position by claiming that the kind of data related to semantic processing just encountered is not a concern of semantics properly so called—any more than it is a matter of semantics to square with the intuitive fact that in (5), the second conjunct is semantically processed after the first one:

(5)   Socrates owned a dog, and Plato owned a cat.

The two positions just outlined are not tailored for our present methodological needs but represent actual positions. The two positions are represented in the literature in procedural semantics and Montague Grammar, respectively. Which one of these quite different semantic approaches is 'correct' is a complicated matter that depends on issues that are both highly theoretical and at the present state of the art undecidable. I mean questions such as: which theory provides, or can be used to provide, a better overall picture of language? Once again the crucial matter is a theoretical one; however absolute, incorrigible, or certain intuitive pretheoretic data concerning semantic processing may be, it is a controversial scholarly issue what to make of that data—whether it should be incorporated into the data base of semantic theory in the first place, or not.

What I believe the above discussion shows is that theoretical and methodological considerations can restrict the collection of 'intuitive semantic facts' one's semantic theory is designed to codify and systematize. Thus the subject matter of semantics is not given in advance (pretheoretically). Contrary to what Katz assumes, the subject matter of semantics is not independent of theoretical and methodological assumptions, and more generally of assumptions concerning the overall picture of language and its substructures.

The breakdown on Katz's methodology is illustrated strikingly in that *Katz himself* restricts his 'semantic data base' on theoretical grounds.

Consider his discussion of the notion of 'sense' in connection with the semantics of proper names.

Katz contrasts his own conception of "sense" (*Sinn*) with Frege's. Katz's notion of sense (which elsewhere in the paper he equates with "meaning") is characterized as "nonderivative". This means that Katz takes

> "senses to be whatever it will be necessary to take them to be to explain the meaningfulness, ambiguity, synonymy, and *all* other semantic properties and relations. In short, a sense is whatever the semantic theory that best saves the semantic phenomena of language says it is." (pp. 196-197)

The trouble with Frege's conception of sense, as Katz sees it, is that it is "derivative": Instead of basing the notion on data supplied by "linguistic common sense" Frege defined his notion of sense with a particular technical interest in mind. Since Frege's interest "in sense is not with it for its own sake" (p. 196) he is bound to run into conflict with intuitive data supplied by "linguistic common sense".

As such data, Katz quotes the semantics of proper names. Frege's view "leads him to claim that proper names are meaningful" (p. 197) and that coextensive proper names may differ in sense. According to Katz, this is against "linguistic common sense".

But of course no pretheoretical common sense can tell us whether or not proper names are meaningful. Ordinary speakers of language do not have any stand on the issue or, if they do, what they associate with "meaningfulness" may be something totally unrelated to the age-old controversy in the semantics of proper names. The issue on which Kripke and Frege have opposite positions is a theoretical one and undetermined by anything our "ordinary experience with natural language" supplies us.

Consider the intuition-based data that *Frege* was concerned with, i.e., the pretheoretic data that *Frege* thought any adequate theory of the semantics of proper names should account for. As such data, there is the unmistakable fact, not challenged by anybody, that

Morning star = Morning star
Morning star = Evening star

differ in cognitive import; the latter can serve to convey information, the former cannot.

This fact is "linguistic common sense", "as certain as the propositions on G. E. Moore's list in 'A Defense of Common Sense'". What isn't linguistic common sense and what isn't as certain as the propositions on Moore's list is whether the semantics (theory of meaning) of proper names *should* account for such data. And, as we know, on this issue scholars disagree. The fact that Katz doesn't share Frege's conception of what the "pretheoretically given data base" is, illustrates how Katz himself restricts the pretheoretic data on theoretical grounds. What better demonstration is needed of the inadequacy of Katz's methodology?

The methodological point we face here strikes the same note as our foregoing discussion. Linguistic intuition—even that part of it that has to do with 'meaning', intuitively understood—supplies an unstructured totality of data only some of which is relevant. One's semantic theory should not square with all the facts, only with all the *relevant* facts; but what counts as "relevant" is a complicated theoretical issue not decidable on the basis of a "linguistic common sense".

Should a semantic theory in some direct way try to simulate actual semantic comprehension? Should a semantic theory acknowledge the 'dynamic' (process-like) nature of semantic computation? Should epistemic (cognitive) aspects of meaning be accounted for in a semantic theory? These kinds of foundational issues are of crucial importance when discussing the semantics of, say, quantifiers, pronouns, or proper names. The issues are crucial, because

one's position concerning them goes in part to determine what one recognizes even in principle as 'raw data'. In a Katzian common sense methodology, with its emphasis of a pretheoretically given "initial source and final arbiter", there is no place for such considerations, however.

The above discussion has concerned the question whether semantic intuitions are unshakable by theoretical considerations. *Pace* Katz, I have answered to this question in the negative. I shall now turn to another major inadequacy in Katz's theory, an inadequacy that concerns the nature of key semantic notions. I shall argue, again against Katz's view, that our pretheoretic intuitions about basic semantic notions such as "synonymy", "ambiguity", or "translation" can be radically misguided and therefore cannot serve as the foundation for a semantic theory.

What I have in mind hits the surface in Katz's discussion of Quine's indeterminacy thesis (pp. 193-194). Katz suggests that Quine's field linguist, pondering the translation of 'gavagai', should address the native informant questions employing notions such as 'synonymy', 'bears the same meaning relation', etc. Evidence from the native informant's judgments about intensional relations would allow the field linguist to avoid the indeterminacy argued for by Quine. Katz bases the advice on his general methodological doctrines:

> "there is nothing illegitimate about presupposing common understanding of semantic notions or the possibility of explicating them. One pursues theory construction in science on the basis of such presuppositions." (p. 194)

Thus Katz's 'common sense in semantics' is supposed to undermine Quine's indeterminacy argument. Among the data a semanticist is given is "common understanding of semantic notions".

What Katz's account does not seem to acknowledge is the possibility that our intuitive commonsense knowledge, including "common understanding of semantic notions", can be confused, biased, and even contradictory. As far as the intuitive notion of translation is concerned, Quine's indeterminacy argument (if correct) brings this out. Sure enough our common sense understanding of semantic notions allows for determinate translations. Quine's point is to show that there our intuitive understanding errs. Pretheoretic common sense is wrong in assuming "common understanding of semantic notions" (in the "intensional sense" intended by Katz), if Quine is right.

Of course one could argue, as many have done, that Quine's conclusion is wrong. However, that is something we need not take a stand on. For our methodological purposes it suffices to point out that surely it is at least *in principle* possible that Quine is right. For Katz, however, such a possibility is ruled out even in principle.

Let me generalize. Katz's view is that "the primary facts in semantics are facts about meaningfulness, meaninglessness, ambiguity, synonymy, and other such intuitively discernible properties and relation of sentences" (p. 176). These notions, however, do not reveal themselves *directly* to us; indeed what each of them amounts to may turn out to be quite different from what we currently believe they are. Semantic notions such as synonymy or ambiguity illustrate the semantic structure of language through their consequences, manifestations if you like—in any case derivatively. But what phenomena *are*

the relevant manifestations of "synonymy", "ambiguity", or what have you is highly controversial. Furthermore, this element of controversy does not merely concern some borderline cases but often hits the very crux of the matter. What kind of phenomena relating to substitutivity signals synonymy? Should substitutivity in belief contexts be acknowledged as a criterion of synonymy? No generally accepted answer to these questions is forthcoming. In their absence, we lack also common criteria for evaluating semantic theories of such expressions as proper names, indexicals, demonstratives, and natural kind terms, just to name a few topics in connection with which substitutivity in belief contexts has played a key role in recent discussions. (For a lucid discussion of some of the methodological issues resulting from the problematic nature of propositional attitude contexts, see [3] and [4].)

These points illustrate one further inadequacy of Katz's methodological framework. There is a strong objectivity assumption in Katz's methodology concerning semantic notions such as "synonymy" or "ambiguity", an assumption which seems quite unwarranted. Pretheoretic semantic data just don't come to us categorized under titles such as "synonymy" or "ambiguity". Once again, it is up to the theoretician to decide what goes under what heading, and the success of the theoretician's efforts is measured by the overall merits of the proposal. And needless to say, any hazy intuitions and associations a man-of-the-street might be able to offer are a hint in the best of circumstances.

The faith Katz puts in the existence of a well-defined subject matter in semantics, delivered to us by linguistic common sense, also seems ungrounded in view of what we know of systematizations of intuitive notions elsewhere (outside semantics). Take Hempel's classical study of the logic of confirmation. What it shows is that our intuitive, commonsense understanding of the notion of confirmation is nothing less than inconsistent. Thus systematizations of the notion of confirmation just *cannot* be codifications of "pretheoretically given", incorrigible data, supplied by intuition. On what grounds could we assume a priori that similar situations should not occur in semantics? I do not think any arguments for the special status of semantics and semantic intuition is in the offing. (Nor has Katz provided such.) Intuitions in semantics, just like in set theory, logic, or in the logic of confirmation, are not absolute, incorrigible, and undefeatable by theoretical considerations. (For an excellent criticism of Katz's methodology along these lines, see [2].)

Katz's antireductionism   According to Katz, another key difference between his approach and all the other approaches—yes: the claim concerns *all* other approaches—is that the latter are reductionistic, whereas his own is not.

Here I shall confine myself to the general features of Katz's discussion about 'reductionism' in semantics. I shall not comment on Katz's discussion of various specific semantic theories that Katz charges of 'reductionism'. Suffice it to point out that Katz's interpretations of alternative approaches are sometimes less than accurate. Thus, e.g., we find him claiming that "Davidson's program posits that English has only two meanings" (p. 183). But of course Davidson's program is more subtle than that.

The following is how Katz defines 'reductionism':

"Other approaches are reductionistic. They seek to reduce the ordinary notions
of sense and meaning away, replacing them with something else regarded from
the metaphysical perspective of the reductionist as philosophically more re-
spectable or scientifically more tractable . . . we have had one attempt after
another to treat meaning as something else." (p. 174)

The trouble with this characterization is that it is not immediately obvious
what counts as "reducing the ordinary notions of sense and meaning away".
The discussion that follows reveals what is meant by the term. Searle's and
Grice's theories seek to analyze meaning in terms of use and therefore "replace
the ordinary notions of sense and meaning with something else", viz. 'use'.
"Another important tradition of reductionist thinking", Davidson's program,
attempts to study meaning in terms of truth conditions. Thus it "eliminates
the ordinary notion of meaning" in favour of one allegedly preferable on some
sort of philosophical grounds" (truth conditions). Another "form of reduc-
tionism" is possible worlds semantics. It analyses meaning in terms of exten-
sions in possible worlds, thus "reducing the ordinary notion of meaning away",
"replacing it with a completely different notion" (extensions in possible
worlds).

In none of the cases Katz studies does he offer any evidence that the
proponents of the approaches in question wanted or intended to "replace the
ordinary notions of sense and meaning with something else". The sole evidence
in favour of their alleged 'reductionism' that Katz mentions is that the pro-
ponents of these approaches have used some other notion than 'ordinary
meaning' in their explication.

It is of course controversial just what counts as 'reduction' and 'reduc-
tionism'. Here I do not want to take a position concerning this issue. However,
for a semantic theory to be 'reductionist' in any nontrivial sense of the word, it
surely is not enough that the approach explicates the intuitive notion of
meaning in terms of some other notion. Explications in general use in their
explicanda different notions than those found in their *explicandum*, typically
notions that are "philosophically more respectable or scientifically more
tractable". To do thus and so is not to "reduce" the *explicanda* to the
explicandum, or to "eliminate" the *explicanda.*

Consider the intuitive notion of countability. Various explications of this
notion have been proposed throughout the years, including ones in terms of
recursive functions and Turing machines. Did the proponents of the former
seek to "eliminate the ordinary notion of countability"? Was the approach of
the proponents of the latter explication 'reductionistic', did they attempt to
"replace the ordinary notion of countability with something else"? The answer
is obvious. It serves to illustrate the difference between "explication" and
"reduction", a distinction apparently missed by Katz.

Bringing the discussion to semantics, consider the intuitive notions of
possibility and necessity. For these notions various scholars have proposed
explications in proof-theoretic terms (in terms of deducibility in certain
axiomatic systems). Some others have made an effort to characterize these
notions in model-theoretic terms (in terms of truth in a model). Neither of

these approaches reduces away, or aims at reducing away, the ordinary notions they seek to characterize.

What I believe these considerations show is that Katz has not demonstrated other approaches in semantics to be 'reductionistic', except in the sense in which any explication of 'meaning' in terms of something else is 'reductionistic'. The latter result is however trivial. Furthermore, it does not bring out any crucial, methodological difference between Katz's approach and other approaches: just like any other approach discussed by Katz, his own studies 'ordinary (intuitive) notion of meaning' in terms of notions considered "philosophically more respectable or scientifically more tractable"—by him.

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