Appreciating Functions:
Ed Keenan in the Early History of Formal Semantics

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As I work on a book project on the history of formal semantics, I come to appreciate how everyone involved has a unique story, and Ed Keenan certainly does. To me, one of the many things that make him special is the degree of his devotion to *functions* and their role in semantics. So for this short squib I want to pick out a few pieces of Ed Keenan's story that illustrate his love of functions.

Keenan is not a functionalist in the usual "formalists vs. functionalists" sense, and it's a pity that the term is now attached to that sense. Otherwise he could be a proud functionalist in at least two other senses. On the one hand, he has found it very fruitful to think about how various syntactic constructions in different languages contribute to the expressive power of those languages — to think about the semantic function of having certain things in the syntax. I'll illustrate that with a Malagasy story. And on the other hand, Ed really likes to think of semantics in terms of the functions that are denoted by various expressions. When I interviewed him, this came up many times in different contexts, and I mostly want to write about that, because it probably sets him apart from many linguists who think of semantics in terms of some sort of "logical forms" or in terms of expressions in some logical language or some other semantic representation language, treating the model-theoretic interpretation as secondary or of no great interest.

Ed went to Madagascar in 1969–70 to do fieldwork on Malagasy as an NSF Postdoctoral Fellow. He had just finished his Ph.D. in Linguistics at Penn in 1969, with a program that included a lot of logic and recursive function theory, a minor in the school of Information and Computer Science, and a dissertation offering a three-valued logic for treating presuppositions, including a completeness proof. His dissertation supervisor was John Corcoran, an Assistant Professor of Linguistics at Penn whose own Ph.D. was in mathematics at Berkeley (supervised by Robert McNaughton, himself a student of Quine). Corcoran was a young Assistant Professor then, and although Ed was in principle doing his dissertation with Zellig Harris, Harris was away in Israel much of the time and Corcoran was his real advisor, and Ed and Harris both agreed Corcoran should get the recognition as his advisor. Corcoran later went on to become Professor of Philosophy at Buffalo, and he's known as a logician, philosopher, mathematician, and historian of logic. Penn at that time was a very heterogeneous department — Henry Hiż was also not a linguist by training but part of a group of Polish logicians. No one was teaching natural language semantics; when I asked Ed whether he agreed with something I had read that said that Zellig Harris was extremely skeptical about semantics and didn't consider it an empirical subject, Ed said that that was

at least consistent with what he knew of him — that Harris's concern was entirely with distributional patterns on the surface, with defining transformations via substitution classes based on patterns of co-occurrence restrictions. What Ed knew of semantics was mainly from logic; he knew considerably more logic than most linguists of our generation, and when he eventually encountered Montague's work, he found it not so surprising as it was to many of us.

But I want to report a nice episode from his year in Madagascar, about passives. Ed says that the year he spent in Madagascar was "a real learning experience", "one of the things that in a sense partly kept me in the field". "The only thing that was easy about it was that it was obvious you had a lot to learn, and in that sense it's rather different from, say, going to [...] England where you think you think you sort of know everything except the accent, and you realize too late that you don't. [...] But when I'm in this little rice growing village, it was totally obvious that I had to adapt, and I didn't have much of an idea how to start." But quite remarkably, a young fellow from the next village showed up on his doorstep who had heard that Ed knew English, and he wanted to learn English, and Ed agreed to do it as a trade: " 'You teach me Malagasy and I'll teach you English.' And we did that for the whole year. And this guy wasn't like some super-genius in the rough; but [...] he was careful, and assiduous. So after about six months, we didn't even speak any French any more except every once in a while [...]."

Ed reports on an 'aha' moment from his learning about Malagasy with the kid from the next village. He had been finding out that only subjects are "extractible" to form relative clauses, wh-questions, cleft sentences, etc., and that there was a rich system of morphology that went on the verb to mark the theta-role of the subject as instrument, agent, benefactive, etc. And he reports on how he came to see that as a rich 'voice' system and to see what it was "for".

> And I mean I'm sitting there working with this kid from the next village, sitting there watching the cows with him, and we're going through my sheets, and checking things off, or putting x's , [chuckling] and finally at one point, this kid just loses patience, he takes my sheet and he just starts going through and filling in — we're still saying it orally, but —, and then I finally saw the pattern, and I'm thinking, Why does the language have six kinds of passives? This is crazy! What do you need them for? And then I realized, what it's there for is to feed the extraction rules. Only we weren't calling them extraction then, but to feed movement rules, to feed things like question formation, cleft formation, relative clauses. And at the time it never occurred to me to think that there was any voice other than passive. I didn't think much of English passive actually — you know, if you lost it, I don't think anybody but a few linguists would notice. Whereas if you lost the voice systems in these Philippine languages, you'd have to change how you do relative clauses, imperatives, reflexives, questions, you know, the core grammar rides on the voice system in a very essential way.
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> [...] But this was like a discovery. You know, I realized what the function of this voicing system was in the language, and I'm thinking: This is like the real linguistic pattern. It's independent of my theories, and it's not something I'm getting from English. And that was exciting, like that was the first linguistic regularity I ever really noticed. Something that you might call a 'law of nature'

or something, a pattern in nature. That was exciting.

And that made all of my careful work with this guy worthwhile. You know, there's still 10% of the cases that were fuzzy and didn't quite fit, but the overwhelming pattern was clear. And I could see why little kids learn that voice system, you know, if they want to talk about the clothes that John is washing, it's got to be the clothes that are being washed by John. And so on. You know, the axe that John killed the chicken with *has* to be 'the axe that was killed-with by John the chicken'.[1]

That illustrates one kind of interest that Ed has in functions: why did Malagasy need six kinds of "passives"? Because the rules for the formation of relative clauses, wh-questions, etc., all targeted only subjects.

But even more striking to me is his interest in functions as the denotations of linguistic expressions. As Lewis (1970) had put it, "I promised simplicity; I deliver functions from functions from functions to functions to functions from functions to functions. And worse is in store if we consider the sort of adverb that modifies ordinary adverbs: the category $((S/N)/(S/N))/((S/N)/(S/N))$. Yet I think no apology is called for. Intensions are complicated constructs, but the principles of their construction are extremely simple." (p. 12 in the reprint in Partee 1976).

Keenan reports that he first encountered Montague's work in the early 1970's via colleagues in Germany, mainly Christian Rohrer and his group at Stuttgart. "Conceptually, I didn't find his work all that startling, because it's like it was in logic [...] — you have your syntax, compositionally interpreted — that's what I thought semantics was! What I thought was totally great was the first article [Montague (1970a)], that you can treat English as one of these languages." Ed goes on to say that he liked the later work [Montague (1970b, 1973)] less, because of Montague's decision in those papers not to do direct model-theoretic semantics as in Montague (1970a) but to proceed indirectly via translation into intensional logic:

We could as in (Montague 1970a) introduce the semantics of our fragment directly; but it is probably more perspicuous to proceed indirectly, by (1) setting up a certain simple artificial language, that of tensed intensional logic, (2) giving the semantics of that language, and (3) interpreting English indirectly by showing in a rigorous way how to translate it into the artificial language.

Montague (1973), page 256 in the 1974 collection

Linguists in general embraced that "indirect" method. Keenan: "What it meant was that people who thought they were doing semantics in my judgment weren't so much studying meaning any more, they were studying translation from one language to another. So they were doing syntax. Admittedly your target language was one that was semantically interpreted, so you can say, yes, yes, you take the composition of the interpreting function with the translation function and you've got an interpretation."

Keenan was glad that there were people who kept their eye on the real semantics, and didn't just pay attention to translation into their favorite logical language. I have the sense

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[1]Later on, expressing his negative reaction to the idea that the main recursion in grammar is recursion on sentences, Ed talks about seeing this all in terms of predicate-formation, rather than as extraction from a sentence.

that Ed thinks, probably wisely, that if you want to stay attuned to possible differences in the way the semantics of different languages works, you should try to think as directly as possible about the functions denoted by various expressions or involved in various constructions. Working by translation into a given logical language could have some of the same kind of prejudicial effects as trying to treat every language through the lens of English. One could counter by saying that linguists have often sensed the inadequacy of a given logical language for capturing some phenomenon in some language and have accordingly proposed extensions or revisions of the logical language and its semantics, as was done in Link (1983), in Kamp and Heim's work in the early 1980's, and others. The reply might be that those proposals were made by people who thought first and foremost about how the denotations needed to work, and the languages they designed were undoubtedly guided by their "real semantic" sense.

Drawing a contrast with those who did 'semantics' by translating into some logical language, Keenan says, approvingly:

> But a lot of the later people didn't do that, so there were things that we found out later of a semantic nature that I think Montague would have been delighted with, had he lived. I think he would have been fully supportive of the kind of work done in generalized quantifier theory. And the regularities that people have found there, properties like conservativity and others. But none of that was discovered working through this PTQ-style translation semantics.
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> And I have to say more – I really didn't like all these sub-stars and super-stars and caps and cups, you know, it just kept getting in the way. And it's still true: I much prefer to define the functions I'm interested in and study the functions, rather than write down an appropriate definition with a lot of lambda expressions with typed variables, which in theory can say the same thing. And some people seem to do extremely well that way. Van Benthem thinks he looks at the world through lambda lenses, as he puts it. But I like to look at the functions, you know, that's what I sort of see as real.

Ed considers type theory useful just insofar as it helps clarify what the domains and codomains of the various functions are that interpret expressions of various categories. But even there he sees some danger of straitjacketing our semantic analyses by type theories that make it hard to express certain generalizations. So for example, in the domain of quantification, he believes that

> the field has failed to make a kind of basic generalization. In Heim and Kratzer (1998) for example, even in van Benthem's writings, they look at the object of a transitive verb and they say 'you've got a type mismatch' — because with, say, *every student* in, say, *Bill knows and likes every student*, they want to think that the transitive verbs are of type $\langle e, \langle e,t \rangle \rangle$, which they are, just binary relations — the extensional ones — but the noun phrases are type $\langle \langle e,t \rangle, t \rangle$ — they just take properties to truth values. So Heim and Kratzer say there's a mismatch. [. . .] It won't cancel to what it should cancel to, namely $\langle e,t \rangle$ — the whole thing's a verb phrase. And so people have provided lots of solutions to that [. . .]. And it seems to me intuitively that the right generalization should be the following: Noun phrases, by which I'm including *every student* and *John* and

all the complicated things, are not simply functions that map unary predicates to 0-ary ones, which are truth values, they in general just map $n+1$-ary relations to n-ary relations. And the type notation has misled us — it's given us a fixed type, $\langle\langle e,t\rangle ,t\rangle$, because you started looking at subject-predicate sentences, and then you had in that sense the same problem that Frege faced, how do you get two quantified arguments on the same predicate?

And if you think of it just at a lower level, suppose I tell you I've got, say, some kind of string function — where you've got a vocabulary V with at least two elements $\{a,b\}$, and I define a function f let's say from $\{a,b\}^+$, it takes the set of finite non-empty strings and it maps each string to the string you get by deleting the last symbol from the string. OK? Seems like a both straightforward and uninteresting function. It wouldn't occur to you tell me, "No, no, Keenan, you've got your head messed up, what you've really defined there is an infinite set of functions. One of them maps strings of length 1 to strings of length 0; a second maps strings of length 2 to strings of length 1; and so on." And I'm saying "No, no, it's just one function, all it does is gobble up the last thing." And that's what the NPs do, only instead of looking at sequences of length n, you're looking at sets of sequences of length n, and what the NP does is gobble up the last argument and reduce it. So I find it much happier thinking that way.

Ed acknowledges a debt to Lewis (1970). "Montague (1970a) was *hard* to read; Lewis was very clear. And he assigned a type to the quantifier words like *every*; PTQ didn't." For Keenan, one of the important advances in Lewis's and Montague's work was the categorial grammar notation, which gave a way to think about what the denotations could be. As a graduate student, Ed had had frustrations, because he had only a small set of Boolean things in his semantic toolbox, and had for instance no way at all to think about what the denotation of a preposition might be. As both Keenan and Bach quickly observed, categorial grammar had been dismissed by the generative syntacticians too quickly as soon as it was shown to be equivalent in generative capacity to context-free phrase structure grammar. Only after semantics came into the picture was it reappreciated as showing how to think semantically about function-argument structure associated with syntactic categories.

One of Keenan's papers that had a great influence on me in my early work was his paper, "The functional principle" (Keenan 1974). The Functional Principle that gives the paper its name is stated as follows:

The Functional Principle (FP)

(i) The reference of the argument expression must be determinable *independently* of the meaning or reference of the function symbol.

(ii) Functions which apply to the argument however may vary with the choice of argument (and so need not be independent of it). (Keenan 1974:298)

Keenan gives examples from mathematics, but his main concern is to show the far-reaching usefulness of the principle in explaining various properties of *subjects* of simplex sentences, *heads* of restrictive relative clauses, and *possessor phrases* of possessive constructions. Later on, Bach and Partee (1980) drew on this work in arguing for the usefulness of a more semantic analog of the "c-command" relation in accounting for constraints on

antecedent-anaphor relations, invoking a relation to which David Dowty (1980) gave the apt name *f-command*, for "function-argument command". The idea was to replace the constraint "A pronoun may not c-command its antecedent" with a constraint "A pronoun may not be the argument of a function containing its antecedent" (Bach and Partee 1980, p.127 in Partee 2004). The idea that it's the argument that f-commands the function, rather than vice versa, would have seemed counter-intuitive to us if not for what Keenan had already demonstrated in Keenan (1974).

Keenan has been thinking about functions for decades since that early work, with fruitful and original results of many kinds. For me it's interesting to see how far back we can find him focusing on what sorts of things denotations really might be, rather than on finding some logical notation for writing down representations of meaning. His work constitutes a good argument for pursuing direct rather than indirect semantic interpretation.

Acknowledgements

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