



interest of the investigator. For example, if the interest is primarily syntactical or semantical, a detailed transcription emphasizing either phonetic or prosodic features will probably not be made, in order to reduce somewhat the amount of effort required to obtain a workable transcription. This is the procedure we followed, and we have used normal word boundaries where possible. Thus, our transcriptions are not satisfactory for studies of the development of phonology or prosody. On the other hand, we have directly entered the transcriptions into the files of the Institute's PDP-10 computer system, and we are able to investigate the edited transcriptions in great systematic detail; the superb assortment of sophisticated programs written by Dr. Robert L. Smith for this purpose has been especially helpful.

### *Writing the Grammar*

The objective of this step is to write a generative grammar for the entire corpus of the child's speech. By and large we have attempted to write context-free grammars with transformations entering only where absolutely required. The level of complexity of children's speech in the age range mentioned above is sufficiently low that most of the spoken speech fits rather naturally into a context-free grammar, but we have no ideological position against transformations and believe they should be used whenever simplification of the grammar results.

The initial measure of success is the percentage of the utterances of the corpus that are parsed by the grammar, but already the use of such an evaluation measure has to be treated with care. It would, for example, be trivial in every case to write a universal grammar in terms of the vocabulary such that any concatenation of the child's vocabulary would be a well-formed utterance and thus the grammar would properly parse any utterance whatsoever. What is to be regarded as a natural restriction on the grammar in the case of a young child's speech is not obvious, although in practice what is done by a great many investigators is to write a grammar that deviates from the standard adult usage only when necessary. Of course, with this approach the grammar is simpler than one concerned with adult usage but corresponds rather closely to a fragment of adult usage with certain notable exceptions. In broad terms, this is the strategy adopted by Suppes, Smith, and Léveillé<sup>6</sup> for the spoken French of Philippe.

Excellent examples of the construction of such grammars are to be found in the books and articles of Roger Brown and his collaborators.<sup>7,8</sup>

### *Testing the Probabilistic Fit of the Grammar*

For persons with a background in mathematical models in the social sciences, especially probabilistic models, a natural further step to take to tighten the criteria for goodness of fit of the grammar is to introduce probabilistic parameters for each production rule and to estimate these parameters from the data. On the basis of the estimated parameters, a straightforward goodness-of-fit test in standard statistical terms can be applied to the grammar. Psycholinguists who are not familiar with parametric models or linguists who abhor statistical linguistics find this step from writing a grammar to testing its probabilistic fit a difficult one to accept. I have engaged in polemics on this matter several times in the past,<sup>9,10</sup> and so I shall not engage in a further defense here. I can say from experience that the attempt to fit a grammar probabilistically can lead to insightful and important changes in the details of the grammar. What it especially affects is the level at which various production

rules are introduced. A common effect of fitting a probabilistic grammar is to raise in the hierarchy of rules the position of those that generate holophrastic utterances, that is, single-word utterances that seem to have the semantic content of complete utterances but not the grammatical form.

### *Constructing Semantics*

The next step is to put a semantic hand into the syntactical glove and show that it fits snugly. The approach in this case is to assign to each production rule of the grammar a semantic function and to build in an appropriate way a model-theoretic semantics for the child's speech. This approach is outlined in Suppes,<sup>2</sup> and a detailed working out for the corpus of Erica is to be found in Smith.<sup>1</sup> If the grammar is fully written before the semantics is begun, in all likelihood the working out of the semantics will cause a revision in the grammar. If space permitted I would illustrate this point with examples of grammatical production rules that are often suggested by linguists but that cannot lead to a reasonable semantics. Examples of this character and a detailed discussion of such matters as the semantics of propositional attitudes in children's speech are discussed in Suppes.<sup>3</sup>

I shall have something more to say about model-theoretic semantics below and so I shall not attempt a further explication at this point. I do, however, want to make the point that model-theoretic semantics for children's speech and for natural language in general is a natural outgrowth of the long tradition of semantics in logic and philosophy, a tradition that has been too much ignored, at least until recently, by most linguists and psycholinguists.

### *Testing of Developmental Models*

With a systematic syntactical and semantical apparatus at hand it is then possible to test specific developmental models of children's language. The overview of development does not have to be restricted to consideration of a few salient instances of speech, but can be examined in a more systematic and global way. From the standpoint of development of grammar it is a virtue of the probabilistic approach that it provides a natural tool for studying grammatical development. In Suppes, Léveillé, and Smith,<sup>4</sup> specific alternative models of an incremental or discrete-stage sort are tested, and I shall have something later to say about what I term the myth of stages.

Roughly speaking, the methodology is this. The grammar, and if desired the semantics as well, can be written to cover the entire range of the corpus, but then probabilistic parameters can be estimated for each block of time. The changes in the parameters directly reflect changes in the uses of grammatical rules, some of the more complex rules, for example, having probability zero in the early period. Alternative models then deal with the conceptual way in which the changes in usage of rules take place. The two natural and simple polar opposites are a continuous incremental model versus an all-or-none stage model. In the data we have analyzed thus far, the continuous incremental model is supported more by the data, although, as is not surprising, neither model, given its simplicity, has as good a fit as one would like.

These ideas about developmental models may be illustrated by drawing on the data and analysis given in Suppes, Léveillé, and Smith.<sup>4</sup> The basic assumptions of the all-or-none stage model are two. First, development is discontinuous and may be represented by a relatively small number of stages. Second, within each stage there is

a constant probability of a rule being used. The specific assumption appropriate to this situation is that the probabilities of rules being used within a given stage constitute a multinomial distribution, and thus satisfy assumptions of independence and stationarity. In the data reported in Suppes, Léveillé, and Smith,<sup>4</sup> six distinct time sections were analyzed. Thus, by having six stages, a perfect fit to the data would be obtained. In order to have a reasonable test and also because of the relatively restricted time span, a two-stage model was tested against a linear incremental model. From rather natural qualitative assumptions the following equation is derived for the incremental model in the report.<sup>4</sup> The exponential distribution occurring in the model is a natural generalization for the continuous-time assumption of the usual geometric distribution characteristic of discrete time processes. The equation is:

$$p(t,r) = \pi_r - (\pi_r - p_r)e^{-\alpha(t-t_1)}$$

where  $t$  is the time parameter,  $r$  is a given grammatical rule,  $\pi_r$  is the asymptotic probability of  $r$ 's being used,  $p_r$  is the initial probability of the rule being used at the beginning of the time  $t_1$  in which observations were made of Philippe's beginning French, and  $\alpha$  is the learning parameter estimated from the data.

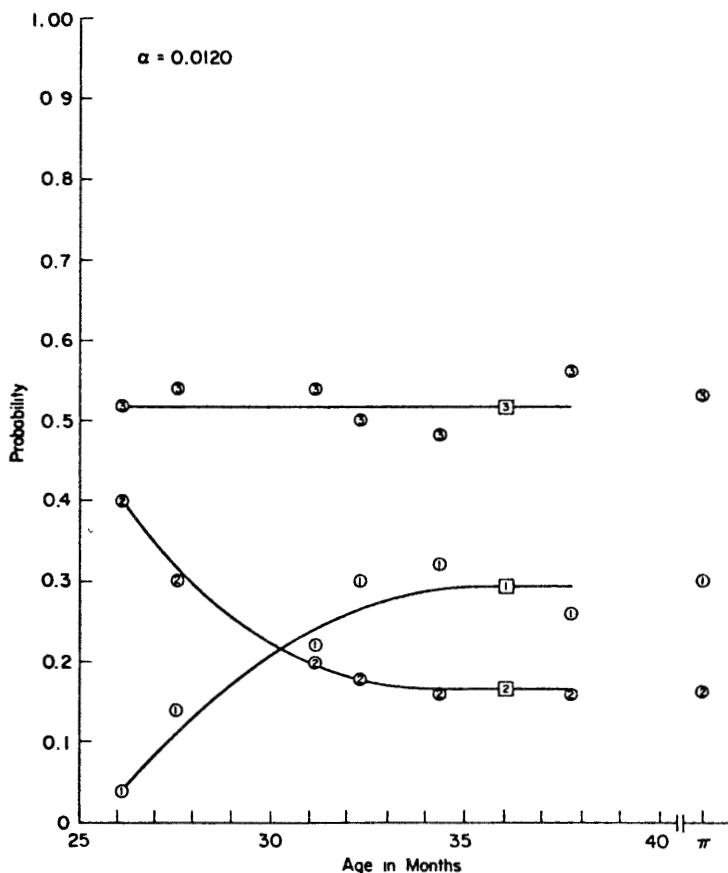


FIGURE 1. Fit of incremental model for production rules at the highest level.

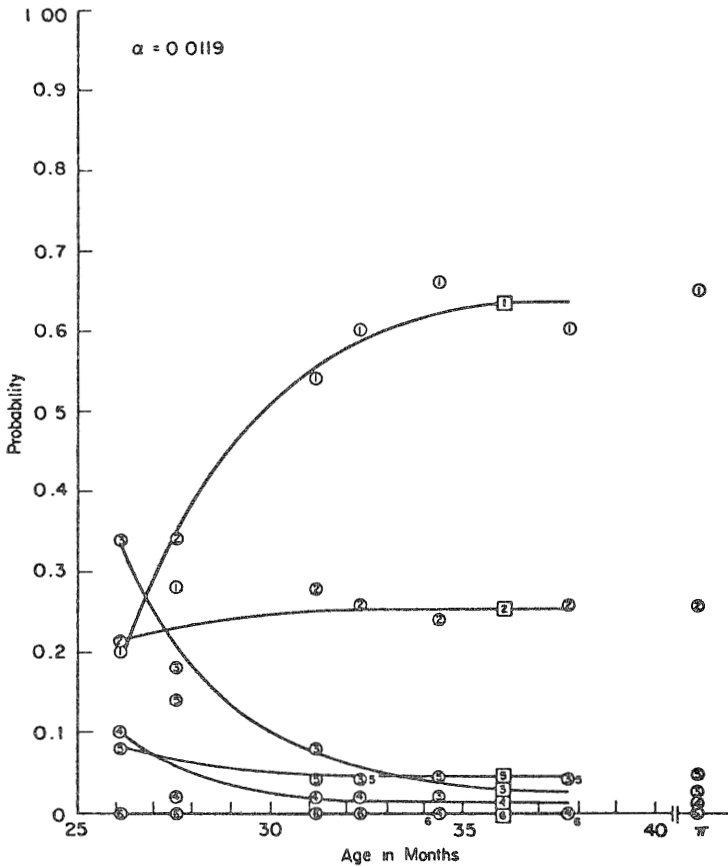


FIGURE 2. Fit of incremental model for high level production rules generating incomplete utterances.

As remarked, the fit of the incremental model was considerably better. I exhibit in several figures the sense of that fit. In these figures, Philippe's age in months is plotted on the abscissa, and the ordinate shows the probability of use of the rules. The rules are divided into subgroups, and thus the probabilities are conditional probabilities of use within a given subgroup. The curves, labeled in square boxes, show the theoretical functions predicted by the incremental model. The data points of the individual rules are numbered; for example, the numeral 1 in a circle indicates a data point for rule 1 of the group, etc.

FIGURE 1 shows data for the highest level rules in the grammar, for example, production rules for sentences. The production rules, beginning with the start symbol *S* of the grammar, divide the types of utterances into: first, short utterances consisting primarily of adverbs, locutions, interjections, and numerals; second, utterances consisting of noun phrases and adjective phrases that stand as utterances without a verb; and, finally, utterances in which a verb is present or questions that occur with or without a verb.

FIGURE 2 shows the data and theoretical curves of top-level rules that produce incomplete utterances. FIGURE 3 shows the data and theoretical curves for the rules

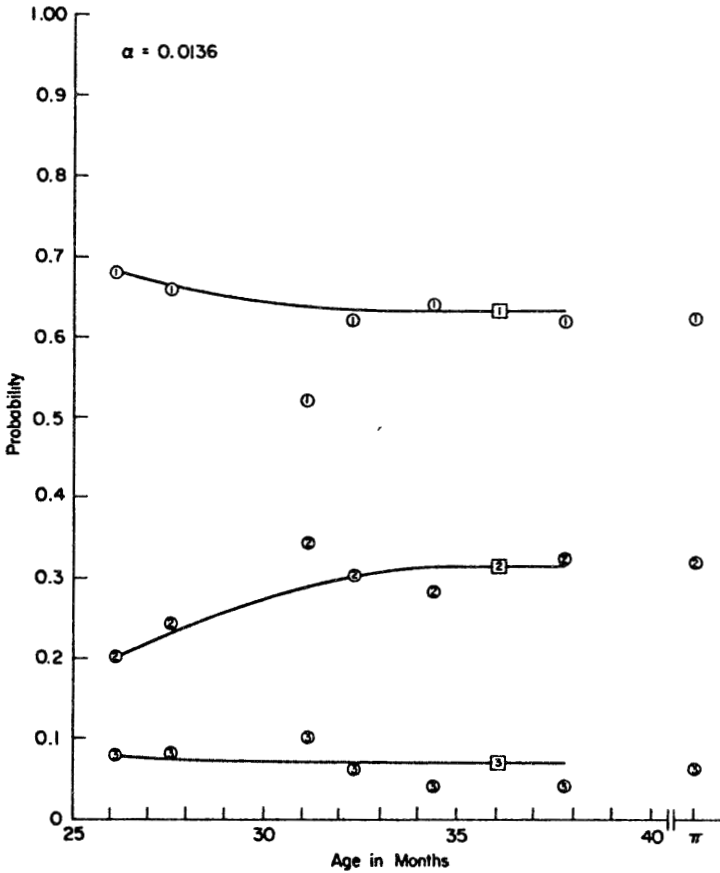


FIGURE 3. Fit of incremental model for production rules generating object noun phrases.

that generate object noun phrases, that is, noun phrases that occur as objects and not as subjects of verbs. The rules for incomplete utterances mainly generate terminal nodes directly to which it is then only necessary to apply lexical rules. The rules for producing object noun phrases have the form one would expect, but they also have some special features that will not be examined here for noun phrases in object position.

FIGURE 4 shows data and theoretical curves for rules that produce verb phrase structures. The 15 rules of this group have been generated into seven subgroups in order to consolidate the data. The first subgroup consists of the single main rule represented by Curve 1, which generates simple verb phrases. (What I mean by *simple* will be clear from the description of the other subgroups.) Subgroup 2 generates verb phrases that begin with a preposition or a personal pronoun. Subgroup 3 introduces auxiliaries, and Subgroup 4 generates several sorts of verb phrases that include an auxiliary or a modal. Subgroup 5 generates verb phrases with a modal followed by a verb phrase that begins with a preposition or a personal pronoun. Subgroup 6 generates verb phrases with a modal followed by a simple verb

phrase, that is, one governed by Subgroup 1. Finally, Subgroup 7 generates verb phrases with a personal pronoun before the modal or the auxiliary. As can be seen and as would be expected, the simple verb phrases generated by Subgroup 1 dominate Philippe's usage of verb phrase structures. The fit of theory to data is especially good for FIGURE 4.

It is to be emphasized that what I have given is a very brief description of a considerably more complicated analysis in Suppes, Léveillé, and Smith.<sup>4</sup>

QUESTIONS AND ISSUES  
*Size of Grammar*

I have in the past characterized the present stage of our knowledge of how to construct grammars of children's speech as the pre-Ptolemaic stage. What I have in mind is that we do not yet seem to have even the degree of fundamental insight characteristic of Greek astronomy with respect to the subtle and complicated data it

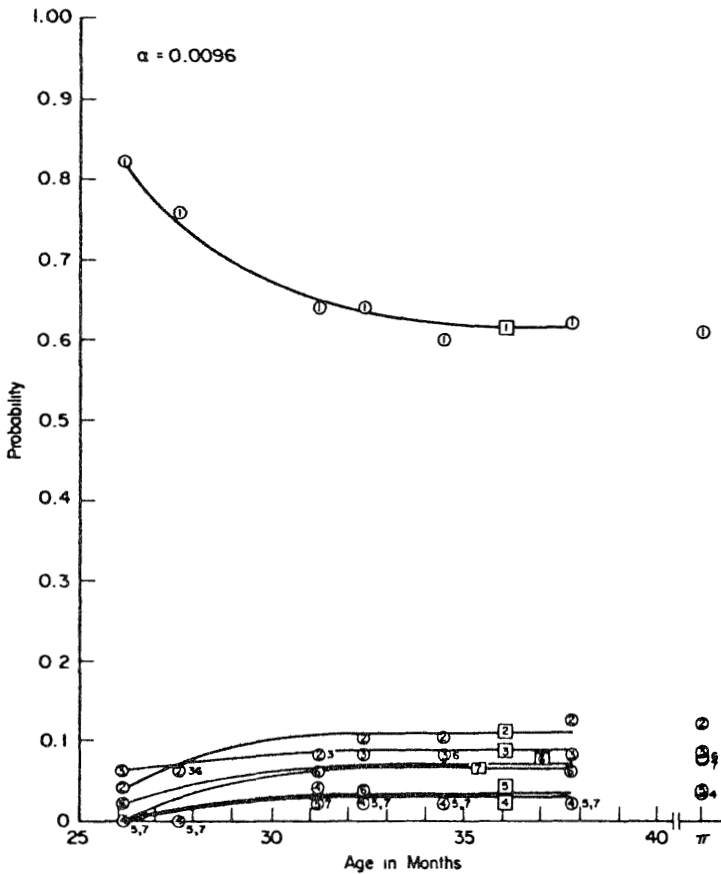


FIGURE 4 Fit of incremental model for production rules generating verb phrase structures.

faced. One way of expressing this concern is by considering the large size of all of the grammars I know of that have attempted to encompass a corpus in complete detail. The grammar, for example, that we have written for Philippe<sup>4</sup> is already extremely large, consisting of more than 300 rules, and yet the grammar does not reflect all of the requirements of inflection nor does it fit more than about 75 percent of the utterances in the corpus. This might be thought to be a part of the irregularity and spontaneity of a young child's first speech, but something like the same situation obtains when we turn to what should be one of the most regimented domains one can think of: written language of elementary mathematics. The dissertation of Nancy Smith<sup>11</sup> was directed at the problem of writing a grammar for the written language of elementary-school mathematics. This seems like a simple, relatively artificial fragment of English, but in fact it turns out to be as full of difficult and subtle problems as the corpus of any of the children's speech we have discussed. It is awe inspiring to contemplate what must be the full set of rules required to describe the speech of a young child from the age of 24 months to 48 months. I suspect we are not approaching the problem in the correct way, but what I do not have is a good insight as to how to change the approach and I see no reasonable alternatives available to us in the work of others.

#### *Nature of Semantics*

It is a commonplace that constructive production of speech as meaningful commentary in a given perceptual and social context could scarcely proceed simply on the basis of model-theoretic semantics as a complete theory of meaning. There are many ways of stating the difficulty. One way is that it will not do to talk about the set of red objects as the extension of the concept *red* in the world as it is. Other more constructive methods of computing along intensional lines are surely required. The model-theoretic semantics we have used in our analyses is meant to provide an analysis of semantics and correspondingly to provide a theory of meaning of children's speech at the level of abstraction characteristic of set theory in general.

I have come to recognize what I feel is the root of misunderstanding on the part of many psycholinguists and some linguists about the nature of model-theoretic semantics. It is characteristic of a great deal of empirical work in psycholinguistics that it is concerned with the acquisition of the meaning of individual words. The problem of how children acquire the meaning of individual words is an important and central one, but it is only one part of an adequate theory of meaning. What has been the primary concern in logic and philosophy has been the development of a theory to account for the meaning of a complex utterance as built up from the meaning of its parts. This is the problem that Frege recognized as central to a theory of meaning, and it was first given a constructive and concrete formulation in Tarski's famous monograph on truth.<sup>12</sup> Model-theoretic semantics is addressed to the problem of giving an account of how the meaning of the whole is built up from the meaning of its parts—I here use *meaning* in a way that includes the theory of reference and the theory of truth or satisfaction. What goes on in the child's head will need more detailed constructive characterization. The many sophisticated efforts currently under way to characterize procedural semantics for computer programming languages provide perhaps one of the best lines of attack on this problem. I do want to emphasize that out of the tradition of psycholinguistics proper, or linguistics itself, there has been as yet no really serious competitor to model-theoretic semantics and there is no evidence that either psycholinguists or linguists will offer on their own a serious theory of meaning. The chances do seem good that within the



framework of computer science the proper constructive extension of model-theoretic semantics is being put together and will be ready for appropriate mutation as a theory of the semantics of natural language. Development of procedural semantics for children's language is, in my judgment, the most important next step in the theory of children's language.

### *Myth of Stages*

The developmental models described earlier, in which we tested the two alternative hypotheses of incremental change or all-or-none change by stages, decisively supported the incremental change, even though the detailed fit of neither model was as good as could be expected. The talk about stages has affected the conceptual apparatus of psycholinguistics to such an extent that the myth of the reality of stages will not be easily eradicated. On another occasion<sup>13</sup> I have criticized the myth of stages in Piagetian theory. The ubiquitous use of the concept of stages by Piaget has encouraged a similar use in psycholinguistics. In view of the lack of serious tests in almost all cases of alternative hypotheses, the presumed existence of stages has exactly the epistemological status reserved for myths. From the evidence I know about, language development at the level ordinarily discussed by psychologists proceeds by continuous change and not by stages. The case for stages is to be found in the microlevel of the learning of individual items of great simplicity. At this microlevel, evidence for stages can be found, as was demonstrated amply in the early 1960s in the enormous literature on this subject in mathematical learning theory. There is, in contrast, no serious evidence in support of stages on the scale of a child's language performance between the ages of 18 months and 36 months, if we have in mind grouping his development within five or six stages, and not at the level of calling the learning of each new word a stage.

On the other hand, it is clearly an open research problem for the future to characterize in a more precise and satisfactory manner the actual development of children's language. The models and theories we have at hand at present are clearly too simplistic and too simple to do an adequate job.

### *Nativism and Rationalism*

The strongly empirical tone I have adopted might lead to the conclusion that I advocate some simple empiricism as the appropriate framework for a detailed theory of children's acquisition of language. This is not the case. A simple *tabula rasa* theory seems to me as much out of place as a correspondingly simple rationalistic theory. Platitudes about these matters are easy. A child obviously comes to the age of language learning with an enormously flexible apparatus for perception and learning, much of it clearly fine tuned to what he will hear. We have, it seems to me, as yet too little theoretical definiteness about the way in which children acquire language to parcel out the variance between genetic endowment and environmental influences. The extensive empirical work we have done on children's language over the past seven years seems to weigh very little on either side of the issues, except perhaps to discourage premature closure on any simple theoretical position.

There is one amusing point of numerical comparison that I sometimes like to use when engaged in dialogue with nativists. Elementary computations on the corpora we have collected indicate that a child between the ages of 24 months and 36 months will produce about half a million utterances and will hear from his parents and other

persons almost a million utterances. This is just in the one-year span. In contrast, a child who is learning elementary-school mathematics will ordinarily work at most about 4,000 exercises a year for a total of 24,000. In gross terms, these data suggest that the more developed innate potential really present in children is arithmetic and not linguistic. All children do not learn arithmetic simply because they do not get the few thousand trials needed to activate the rationalist potential. Given similarly limited exposure to language, certainly the same would be true. Perhaps arithmetic should replace language as the new nativist stronghold.

### *Theses about Expressive Power*

In other papers in this volume there has been considerable dispute about the relative expressive power of various natural languages. Keenan, for example, has defended the thesis that natural languages vary greatly in their expressive power. Katz, on the other hand, has essentially advocated the thesis that all natural languages have the same expressive power. To make the discussion interesting, additional distinctions are needed. There is, for example, a large literature in logic coming at the problem of expressive power from a number of different directions, and a variety of interesting results has been established. I am not suggesting for a moment that the results in logic can be interpreted in any direct way as bearing on the issue of what is the expressive power of a given natural language. What that tradition does suggest, however, is that it is important to think in terms of appropriate kinds of distinctions and to settle the issue not once and for all but in terms of a particular aspect of the general and vague concept of expressive power. For example, the many interesting results established by Tarski<sup>14</sup> for the interpretability of one theory expressible in first-order logic in terms of another introduces a number of concepts that seem applicable with appropriate changes to the current controversy. I restrict myself to one other example, a more recent one. In general discussions of elementary logic it is standard dogma to make the point that definitions play no essential role if they are proper, that is, if they satisfy the standard criteria of eliminability and noncreativity. Working practice in logic and mathematics shows very well that this is far from the case. Any substantial mathematical theory requires the introduction of a succession of definitions to be manageable, to have, in other words, the right expressive power. One thesis lurking in the background in discussing the expressive power of natural languages is that one can always define new concepts by the resources available in the language. Thus, if we take a primitive language, the claim would be we could bring it up to the standards of modern science by appropriate introduction of new terms by definition. The plain man would, of course, regard this much-extended language as a new language, and so claims about the expressive power of the thus extended language would not amount to the same thing as claims about the original language. The rationalist who believes in the quality of the expressive power of all natural languages might want to report that he can in principle dispense with the new terms by using their definitions and by expanding any scientific language back into the original language. I do not believe that any rationalist precisely holds that such explicit expansion into the original natural language of all scientific terminology is possible, but approximations to this thesis seem to be present and sufficiently close to make the point I want to make. The point is that the expressive power of languages with or without explicit definitions is of quite a different character. This can be shown in several ways, but let me just cite one interesting recent example. Statman<sup>15</sup> has shown that if we consider mathematical argument or proofs as an example of expressive power, and if we use the genus of the

graph of a proof as the measure of its complexity, then there is no upper bound on the genus of proofs that result from eliminating the use of explicit definitions. In other words, if the genus of a proof that uses explicit definitions is  $n$ , then a proof of unbounded complexity is in some cases required of the same theorem once explicit definitions are eliminated. Clearly the case of mathematical proofs is rather special, but it is exactly around issues of complexity that the more detailed discussion of expressive power should center; and it would be my conjecture that as the discussion becomes more definite and specific, examples can be constructed of conceptual importance that support Keenan's thesis.

In contrast, in examining the grammar and semantics of English, French, and Chinese that we have constructed for the children's speech available to us, we have found no significant differences in expressive power; or if they do exist in the corpora we are not sensitive to them. On the basis of what evidence I have seen, fragmentary though it may be, I could imagine myself believing a thesis of approximately uniform expressive power for most natural languages as used by very young children, but being skeptical that such a uniformity thesis can be maintained as we move up the scale to older and more sophisticated speakers and listeners.

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