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NOTE

Towards a more precise model of pronoun case error: a response to Schütze*

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INTRODUCTION

In my paper *Patterns of Pronoun Case Error* (Rispoli, 1998b), I documented two aspects of pronoun case errors arising out of principles of lexical retrieval that are operative during the acquisition of the word specific pronoun case paradigms of English. The two aspects were; (a) the 'double-cell effect,' a supercharging of the *her* for *she* error rate; and (b) the 'antagonism' between the stereotypic objective for nominative replacement (e.g. *him* for *he*) and the rarer nominative overextensions (e.g. *he* for *him/his*). In my response, I shall address Schütze's criticisms in both of these areas. The response begins by addressing issues surrounding the double-cell effect. It then moves to issues surrounding antagonism.

Double-cell effect

Schütze (1999) begins his criticism of my double-cell effect hypothesis by stating that 'no account is provided of how this prediction actually follows from the model' (p. 752). Nothing could be further from the truth. The model I have proposed is based on the retrieval strength of word forms in word-specific paradigms. The filling of two cells contributes to an increase in retrieval strength for the word form *her*. Schütze then compares the *her* for *she* substitution rate with the combined *me* for *I* and *my* for *I* substitution rates. He correctly notes that the *her* for *she* substitution rate has been observed to be higher than the combined first person singular (1Psg) oblique (i.e. objective and genitive) for nominative substitution rates. He then implies that I hypothesized that the double-cell effect is additive in nature, and that the *her* for *she* rate should be equal to the sum of the *me* for *I* and *my* for *I* rates. Schütze is attacking a straw man. Never in Rispoli (1998*b*) was it claimed that the double-cell effect was an additive effect of the two

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overextension rates, [i.e. (*objective* for *nominative*) + (*genitive* for *nominative*)]. I doubt that such a linear, 'additive' model can work for the following reason, a reason that is embedded in my overall framework for understanding pronoun case errors. In earlier research on 1Psg pronoun case errors, I pointed out that the data suggested 'the *me* for *I* replacement and the *my* for *I* replacement are highly antagonistic' (Rispoli 1998a, p. 388). The two replacement rates are not independent, but negatively related. It may well be that one replacement option INHIBITS the other replacement option, and consequently, reduces the overall error rate for the 1Psg pronoun. This simply would not happen in the third person singular feminine (3Psg fem) pronoun, because the same word form fills both oblique cells of the paradigm. Interaction effects need to be figured into a more precise model of pronoun case error. However, Schütze seems to be suggesting that the interaction effects arise only out of the spreading of positive, excitatory activation. My point is that interaction effects arising out of the spread of negative inhibition might also contribute to the double-cell effect.

At this juncture, it should be reiterated that the high *her* for *she* replacement rates have not been successfully handled by a purely syntactically based approach. Schütze implicitly acknowledges this fact when he offers his own hypothesis concerning the *her* for *she* replacement rate. Given that Schütze's approach is otherwise syntactically based, his invocation of frequency-based input effects seems *ad hoc*. It underscores the weakness of his overall framework. He proposes that the ratio of the input frequency of the nominative form to the input frequency of the oblique forms combined is responsible for cross-pronoun differences in nominative replacement rate. His hypothesis is not based on type frequency, as is mine, but on TOKEN frequency. More specifically, it is based on the ratio of the TOKEN frequencies of nominative to oblique forms. As evidence of the plausibility of this account, Schütze uses the token frequencies from Francis & Kučera (1982). The ratio of occurrence of *I* to those of *me* and *my* combined is approximately 2·3 to 1 (the actual frequencies are *I* = 5,844; *me* = 1,186, *my* = 1,314). In contrast, the ratio of occurrence of *she* to *her* is approximately 1·0 to 1·0 (the actual frequencies are *she* = 2,986; *her* (*obj*) = 1,107, *her* (*gen*) = 1,933). But what of this ratio? One cannot merely focus on these two pronouns and claim that a plausibility argument has been presented. One must also consider what is also effecting the error rate in the third person masculine (3Psg masc) pronoun. Consider what Francis & Kučera report concerning the 3Psg masc pronoun. The ratio of occurrence of *he* to *him* and *his* combined is approximately 1·1 to 1·0 (the actual frequencies are *he* = 9,775; *him* = 2,625, *his* = 6,994). Based on these ratios, we would expect the rate of nominative pronoun case error in the 3Psg masc pronoun should be about the same as that of the 3Psg fem pronoun. Clearly, this expectation is entirely wrong. Francis & Kučera is a frequency analysis based on written text from variety

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of LITERARY genres. Schütze's proposal cannot be taken seriously until it is pursued with research into the conversational, interactional milieu surrounding and involving the child.

Before we leave the topic of the double-cell effect, I think a final point should be interjected. I compared the rates of *her* for *she* replacement with other objective for nominative replacement rates among the THIRD PERSON PRONOUNS. I did so for developmental reasons. It is undoubtedly true that the 1Psg pronoun is acquired before the 3P pronouns. As I pointed out in Rispoli (1994), the oblique for nominative overextension rate might be lower because of the effects of learning, age and experience. Children begin using the 1Psg pronoun earlier, and so there are probably more rote forms involved. In Rispoli (1994) it was found that, between ages of 1;6 and 3;0 children produce the 1Psg nominative form *I*, far more often then all of the 3P pronoun forms combined. It seems safe to say that, on average, children acquire the 1Psg pronoun paradigm earlier than they do the 3P pronoun paradigms. I think it is prudent to assume that an explanation of the difference in error rates between 1Psg and 3P pronouns will involve the effects of age and experience. These effects should not be discounted as we build a more precise model of pronoun case error.

Antagonism

The remainder of my response concerns Schütze's criticisms concerning antagonism. He claims that he is testing my hypothesis concerning the antagonism of two types of pronoun case error 'more precisely' (Schütze, 1999 p. 751). He tries to do so by using a summary of data from my earliest paper on pronoun case errors (Rispoli, 1994). I think this is a flawed attempt at reanalysis. It underscores Schütze's bias against a developmental approach to pronoun case errors that allows for considerable individual and longitudinal variation. There are three primary limitations to his critique. First, he uses a summary of data that were not collected with the elicitation of third person singular masculine (3Psg masc) pronoun cases errors in mind. Second, he ignores the particulars of design and sampling in Rispoli (1998*b*), overlooking important issues of comparability. Third, and most important, he collapses two years worth of data into a single data point without regard for the developmental course of the error patterns over time, thus obliterating all evidence of antagonism.

Recall the particulars of design and sampling in Rispoli (1998*b*). The design was cross-sectional, not longitudinal. The children were between the ages of 2;6 and 4;0. Each child contributed a single data point. The sampling regimen was instituted to insure that each child was sampled at a particular point in developmental time. Samples were based on approximately two hours of caregiver–child interaction. Measurements of the stereotypic error rate of *him* for *he* were based on a minimum of five attempts at the target *he*.

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The production of nominative overextensions, that is *he* for *him*/*his*, was treated as a categorical variable. A child was either observed to produce a nominative overextension or was not observed to do so. The reason why the two variables, stereotypic error and nominative overextension, were treated as different types of variable can be seen by examining Figure 1, which

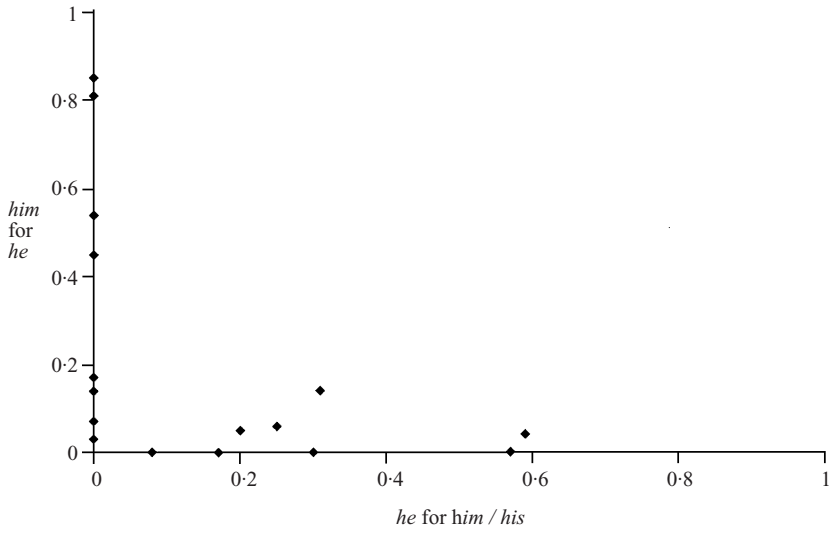


Fig. 1. Scatterplot of *him* for *he* by *he* for *him*/*his* in Rispoli (1998*b*)

displays the data from Rispoli (1998*b*) on a two dimensional scatterplot. The values of the *he* overextension rate are plotted along the abscissa and the values of the *him* for *he* rate are plotted along the ordinate. Both errors, when treated as continuous variables are highly, positively skewed. The abnormality of these distributions can be readily explained when the two variables are plotted together. The relationship between the two variables does not appear linear, but distinctly curvilinear, and indeed, as reported in Rispoli (1998*b*), clearly negative. In this case, either variable could have been treated as categorical. However, we have come to expect children to make the stereotypic objective for nominative error. The history of research on this error is longer (dating back at least to Gruber, 1967). Therefore, it is interesting to a wider audience if we can say something definite about its distribution. Fewer researchers are aware of the existence of nominative overextensions, and the history of research on this error is less extensive (the earliest reference the author has found is Chiat, 1981). For this reason, the stereotypic *him* for *he* error was chosen as the continuous variable, whereas nominative overextensions were chosen as the categorical variable. In effect,

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when the median test reported in Rispoli (1998*b*) was performed, the *him* for *he* error was treated as a quasi-dependent, outcome variable, whereas the *he* overextensions were treated as a subject grouping variable.

Schütze (1999) seems to have ignored the differences in the designs and sampling regimens between Rispoli (1994) and Rispoli (1998*b*). In his reanalysis of Rispoli (1994), he uses a summary of data collected with a design and sampling regimen quite different from Rispoli (1998*b*). He does so without any expressed justification. Moreover, the age of the child participants, design and sampling regimen for Rispoli (1998*b*) were chosen to address specific short-comings of Rispoli (1994). The data that I used in Rispoli (1994) were not originally collected with the elicitation of 3Psg pronoun case errors in mind. The design was longitudinal, with sixty-minute samples taken monthly from 1;0 to 3;0. Note the age differences between Rispoli (1994) and Rispoli (1998*b*). The latter study concentrated on older children, because of the comparative lack of 3Psg masc pronoun case errors in Rispoli (1994). Thus, older children were specifically targeted in Rispoli (1998*b*). Perhaps most importantly, the data used by Schütze collapsed twenty-four data points. Nevertheless, Schütze maintains that this two-year period can be collapsed into a single data point that would have relevance to the question of antagonism. This is completely contrary to the design and sampling regimen used in Rispoli (1998*b*). The latter study was geared towards capturing each child at a particular point in developmental time.

If anything, the techniques used in Schütze's (1999) attempt to test the hypothesis of antagonism are far less precise than those used in Rispoli (1998*b*). A child's pronoun production system changes over time, according to processes with significant probablistic components (Rispoli, 1998a; 1998b). Therefore, to test the hypothesis of antagonism one must come as close as possible to isolating the relationship between the stereotypic *him* for *he* errors and the nominative overextensions of *he* at a singular point in development. The longer the developmental period collapsed into a single data point, the less precise the measurement becomes. If one were to use the Rispoli (1994) data to test the hypothesis of antagonism, one would have to focus in on discrete points in development. One should not use a summary of the data as Schutze has attempted to do. The longitudinal data from a child in Rispoli (1994) (previously unpublished) illustrate why this is so. The child in Table 1 began to produce 3Psg masc pronoun forms at 2;0. Monthly samples reveal that when the child was observed to produce nominative overextensions (2;1 and 2;3), the child was not observed to produce the stereotypic objective for nominative replacement. Conversely, when the stereotypic error was observed (2;9 and 2;11), nominative overextensions were not observed. If this child's data are reduced to a single data point, representing 13 months of observation, as Schütze has done, the temporal complementarity of the two error types is missed.

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TABLE 1. *3Psg Masc pronoun production of a single child**

Age	Correct productions			Errors	
	he	*him*	*his*	*him* for *he*	*he* for *him/his*
2;0			1		
2;1	1				1
2;2	5		4		
2;3	21	2	7		1 (0·10)**
2;4	8		2		
2;5	4		1		
2;6	8	2	2		
2;7	6		2		
2;8	1	2			
2;9	20	1	2	1 (0·04)	
2;10	7	11	2		
2;11	31	10	4	1 (0·03)	
3;0	14	4			

* The data are that of Child 6 from Rispoli 1994. ** Error rates are calculated only when a minimum of 5 targets were attempted.

TABLE 2. *The relationship between nominative overextensions and objective for nominative errors in monthly samples from Rispoli (1994)*

Child	Age	Correct *he*	*him* for *he*	Nominative overextension
1	2;6	16	2 (0·11)	0
1	2;10	8	0	1
1	3;0	8	1 (0·11)	0
4	2;8	8	1 (0·11)	0
6	2;9	20	1 (0·05)	0
6	2;11	31	1 (0·03)	0
7	2;10	19	1 (0·05)	0
9	2;11	10	2 (0·16)	0
10	2;8	10	0	1
10	2;11	8	0	1
10	3;0	8	1 (0·11)	0
11	2;9	18	2 (0·10)	1
11	2;10	8	0	1
11	2;11	12	0	10
11	3;0	19	3 (0·14)	1

Schütze (1999) should have asked, 'how can we make the data reported in Rispoli (1994) more comparable to the data reported in Rispoli (1998*b*)?', before proclaiming that 'Rispoli's earlier study shows that the pattern predicted by antagonism is not consistently found for English children' (p. 752). We can still ask that question here. In keeping with the maxim that the

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more narrowly defined a developmental period the better, we should look at the data from Rispoli (1994) sampled monthly. First, a large enough sample of productions must be taken to calculate a reasonable rate. Recall that in Rispoli (1998*b*) the rate of correct production of the subject pronoun was based on a minimum of 5 attempts at the target. Therefore, only monthly samples that have a minimum of 5 attempts at target *he* should be considered. Second, the age of the children in Rispoli (1998*b*) ranged from 2;6 to 4;0, so to increase comparability, only monthly samples from 2;6 and older should be used. Lastly, as in Rispoli (1998*b*), the monthly samples must contain at least one 3Psg masc pronoun case error. If we narrowed our focus to just the monthly samples contained in the Rispoli (1994) data that fit the above three criteria, we would have a subsample that is much more comparable to Rispoli (1998*b*). This subsample is presented in Table 2. Note that not all twelve children from Rispoli (1994) are represented in the subsamples of Table 2. In fact, only seven of the twelve children had monthly samples that fit the above criteria. The data in Table 2 show that, for the most part, when nominative overextensions were observed, stereotypic *him* for *he* errors were not observed. Only two monthly samples, both from the same child, child 11, show instances of both errors co-occurring. Clearly, the mode for the stereotypic error rate in samples with nominative overextensions was 0.00 (five of the seven samples). In contrast the modal stereotypic error rate in samples lacking a nominative overextension was 0.11 (four of the ten samples). There is no convenient inferential statistic available (like a Pearson product–moment correlation) to test whether this distribution is most likely due to chance or not, because the seven children in the subsample contribute an unequal amount of data. In short, there is no easy way to collapse these data. However, it is clear that when one focuses on the monthly samples, the two data sets, Rispoli (1994) and Rispoli (1998*b*) are very similar.

When trying to test the hypothesis of antagonism between the stereotypic pronoun case error (e.g. *him* for *he*) and nominative overextensions (e.g. *he* for *him*/*his*), we are testing a hypothesis which demands developmental sensitivity. The origin of these two types of errors lies in the lexical retrieval process, and in particular, the retrieval strengths of the word forms in the word specific paradigms of the 3Psg pronoun (Rispoli, 1998*a*; 1998*b*). Therefore, we can expect a dynamic relationship that changes, perhaps even fluctuates, before the word specific paradigm is fully learned. A more 'precise' characterization from the longitudinal data indicates that the retrieval strengths of these word forms relative to each other do, indeed, vary as learning proceeds. Therefore, the way to test the hypothesis of antagonism is to sample a child's pronoun production system at as narrowly defined a developmental period as possible. In Schütze (1999), a biased perspective, that views the phenomenon as non-developmental and relatively invariant, unfortunately leads him to ignore relevant data and alternative hypotheses.

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CONCLUSION

I would like to address my final comments to the broader audience of child language researchers who are not intimately involved in the minutia of pronoun case errors. Pronoun case errors have attracted relatively little attention. I find this curious. They are a hallmark characteristic of the development of English grammar. They involve grammatical knowledge of a highly abstract sort and bear on the central question of how children relate grammatical features to phonological form. Recently, two alternative approaches to the phenomena have emerged. Schütze's work is representative of an approach which views the phenomena as the result of changing and developing syntactic mechanisms. My work is representative of an approach which holds that there are also significant learning and performance factors involved. The resulting debate is an almost classic juxtaposition of approaches. One is based on hypotheses about the development of syntactic competence, the other is based on hypotheses about morphosyntactic learning and lexical retrieval. It is my fervent hope that our debate will spur further data collection, analysis and research into pronoun case errors, because I believe these phenomena can tell us much about the basic mechanisms of morphosyntactic development.

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