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Language & Cognition

What can the study of language contribute to our understanding of human nature? This question motivates research spanning many intellectual constituencies, for its range exceeds the scope of any one of the core disciplines. The technical study of language has developed across anthropology, electrical engineering, linguistics, neurology, philosophy, psychology, and sociology, and influential research of the recent era of cognitive science has occurred when disciplinary boundaries were transcended. The seminar is a forum for convening this research community of broadly differing expertise, within and beyond the University. As a meeting ground for regular discussion of current events and fundamental questions, the University Seminar on Language and Cognition will direct its focus to the latest breakthroughs and the developing concerns of the scientific community studying language.

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**Numerical cognition without words:
evidence from Amazonia for strong determinism**

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There has been a resurgence of interest in recent years regarding the Whorfian hypothesis that the structure of language can affect how we think and perceive the world. Such a hypothesis takes two forms: weak determinism and strong determinism. The former claims that linguistic structure can affect perception and cognition through the habitual ways in which language is used. However, it does not claim that language can prohibit or restrict the range of thoughts and perceptions. This position does not question Brown and Lenneberg's original contention that any idea can be expressed in any language. Strong determinism challenges this assumption, claiming that speakers of one language can fail to entertain concepts spoken in another language. In this talk, I will present data from the Pirahã, a tribe from lowland Amazonia who lack words for numerical quantities greater than 2. Experiments investigating the Pirahã ability to perceive and encode numerosities greater than 3 revealed that below 3, performance on most tasks was close to perfect. Above this number, performance dropped off depending on the cognitive demands of the task and reliance on strategies such as chunking. However, despite poor performance on larger numerosities, Pirahã participants showed excellent estimation for quantities up to 8 or 9, with performance almost perfectly on target and showing level coefficients of variation with increased set size. These results are very much in line with current research in infant, child, adult and animal numerical cognition, consistent with an innate dual system for numerical competence. In addition, they suggest that the lack of numerical words in the Pirahã language prevents bootstrapping their innate individuation abilities into a formal counting system in which mandatory numerical perceptions are possible.

There is an old joke I sometimes tell when starting this talk, which is: There are three kinds of people in this world, those who can count and those who cannot. This is a talk about the latter kind. We're going to talk about the very old question of whether a language can determine the way that you think. This is also known as the Whorfian hypothesis, but he should not take all the credit, or all the blame, depending on your perspective. His predecessors were also in part responsible for some of these ideas.

I want to distinguish between *weak determinism* and *strong determinism*. I will go into this a bit more, but basically weak determinism refers to the way a language habitually refers to time or space. There is a preferred way of referring to reality. Evidence of language effects on people is that people may

show a preference to categorize or think in a particular way compared to speakers of another language, but it does not rule out the ability to construe ideas that are expressed in another language. In other words, I am not saying we cannot think in this other way. That is what I am calling strong determinism; that there are actual incremental abilities between languages, that there are actually ideas that you can express in one language that you cannot express in another. The Whorfian hypothesis is a stronger view, which would claim that you cannot express those ideas because you cannot actually think those ideas. I have heard this referred to as a straw man argument, that this could not possibly be true and that the only interesting opinions of this were for weak determinism. I am going to try and argue that this hypothesis is actually true in the case that I am going to show you.

I am going to talk about a tribe called the Pirahã, who I worked with quite a while ago as a result of interacting with Dan Everett, who became the chair of Linguistics at the University of Pittsburgh, and who had spent about twenty years working and living with this tribe. I am just going to put a tape on in the background to show you some of the way that they live.

[Professor Gordon shows a video of the Pirahã tribe and village.]

They are hunter-gathers so basically they will stay in a village for about a year or two and then move on. Often when people start dying in a village they will think its bad Karma and they will move to a different location. They live along the Maici River. This is a typical village; it has about ten or fifteen people in it. The river they live on, the Maici, runs into the Amazon. They have the philosophy of not telling their children to avoid dangerous situations.

[In background video, child playing with a knife.]

They have a population of about sixty people total in the villages along the river. They are monolingual in their language Pirahã, so they may know one or two words in Portuguese but they are not at all fluent. They resist assimilation to Brazilian culture, primarily because they have had bad experience with local traders who generally take advantage of them. There is some limited trading, sometimes with other tribes, sometimes with traders, but no money. Generally they will exchange things for goods. They have a very limited social structure, there is no clear chief in the village but there might be a dominant male who beats you up if you do not do what he says. They do not have any external representation, they do not write, they do not have any art, or toys.

I am going to talk about counting system in Pirahã which is a *one-two-many* system. The word for one is *hói*, two is *hoi* and many is *baagi*. One and two are distinguished by a falling tone versus a rising tone. One of the things that got me interested in this case initially was that at the time I had been reading Gelman and Gallistel's early work on children's counting. Quite a large part of that book was dedicated to work by Zaslavsky who was essentially trying to get rid of some of the numerical myths of new world cultures. Gelman and Gallistel at the time were putting forward a fairly strong hypothesis that the ability to count was like the ability to use language; that there was some innate disposition there. If you have cultures that do not really count in the way that we know it, it is problematic for that theory. Zaslavsky claimed that cultures

that have *one-two-many* counting systems had alternative finger gestures; so they were able to use their fingers instead of count words. They might use non decimal recursive systems such as the Gumulgal in Australia that essentially have a *one-two* system which is fully recursive so you can say, *one-two*, *two-one*, *two-two-one* and so on.

Zaslavsky also claimed that there are certain taboos and certain things that you cannot count for religious reasons. When I first went to the Pirahã I stayed for about ten days and I basically just asked them some questions so that I could get a sense of what was going on there and try to get to their numerical quantities. There is clearly no evidence either for numerical taboos, they were able to count anything, or recursive systems. Dan has been going for twenty years and has never seen anything like that, so that is clearly not the case. The other interesting thing, that may follow from having a fairly low counting system, is that you do not actually count in the sense of a serial count. The fact is that even though we translate these words as “one” or “two” it is really fairly loose. It is more like when someone says in English, “Give me a couple of those.” If someone gives you three, you do not say, “I said a couple!” It means “two, more or less.” *Hói* in Pirahã means, “one more or less,” so it could express two, three and even quite large numbers. In this first year we did some informal experiments about the meaning and use of these number words.

[Professor Gordon shows a video of experimenter Karen Edwards working with some members of the Pirahã tribe on various number naming tasks.]

This is Karen Edwards, Dan Edwards’s wife, she is laying out objects, lemons and so forth. She is speaking to them in Pirahã but just giving them the Portuguese numbers. She is asking them to make another row that is the same. You will see in this tape that the person is using his fingers but it is not an anchoring device. So when she says three he says two again.

[Video shows a Pirahã participant using hoí for two, three and four items; he uses baagi for five items.]

Dr. Inge-Marie Eigsti: What is the task?

Professor Gordon: She is asking him to say how many in his own language.

[Video shows the Pirahã participants cheering loudly.]

They become pretty enthusiastic when they get it correct. They do not get it wrong all the time. I will show you at the end if you look at the average they actually do pretty well.

[Resumes video. Shows tasks asking Pirahã to make identical lines composed of flashlight batteries or lemons. Next the experimenter knocks twice with a fist on the table, and the Pirahã participant, asked to match the number of knocks, knocks three times.]

This gives you a sense of how they see numerosities. There is quite a lot of research recently that suggests that the weak form of determinism is applicable in cases where people of different cultures may have a propensity to think in ways that reflect their language that may have to do with time, space, number,

and so on. The question is whether there are concepts in one language can be translated into another. My hypothesis is that the Pirahã language is numerically incommensurate with languages that do count, that the Pirahã do not have concept of number or counting that can be equated with a counting culture, the numbers are not rigid designators and they are not used in serial counting as I said before. The accurate estimations of numerosity are limited to those quantities that can be directly perceived, which are those small numbers up until about three.

There is quite a history that the first three or four numbers or quantities show a discontinuity with the rest of the number line. This goes back to the idea of *subitizing*, which we find in Cattell. We find reaction times and error rates fairly level up to about three and then we show an increase after three. These are for brief presentations in which you have to say how many objects there were. There are other experiments in the literature that suggest the same limitation of three or four units. With pre-attentive units, you can follow moving objects in a display amongst other objects. You can keep track of those objects to about three or four and then you are not able to do it. Dehaene has talked about this notion of parallel individuation, which is the number of individuals that you can keep in mind at the same time, is about three. There is also this idea of three being the real magic number not seven. Things actually come in threes more than seven; mom saying, "I will count to three." Or "One, two, three...go!" And, remember all the religious significance of three and anthropological literature where three actually plays a big role. For large numbers, there is a lot of literature. Rather than having an accurate individuator, what you have is a sort of analog estimator of quantity. So the representation is thought to be of more continuous in nature rather than discrete. You have estimation for example in tasks that involve:

[Professor Gordon bangs on table a large number of times]

How many times did I bang? It is too fast to count but you tend to be fairly accurate. As the number gets larger your error rate gets larger. Studies with infants and with animals also show similar properties. I want to talk about where the *one-two-many* counting system fits into a universal possible counting system. The base of the counting system can be either perceptual or body based. In other words, you can use the three-ness as your base and it could either be nonrecursive, in which case the system becomes *one-two-many* or a *one-two-three-many*. There are not too many systems that go beyond that, so for instance you do not get a *one-two-three-four-five-many* system. Nor are there many binary systems like the one that we find in Australian culture, which is recursive. Obviously you can employ the ten base systems which tend to be recursive. There are different cultures that use fingers. There are also cultures that use other parts of the body and you usually get counting that goes up to thirty-three or so.

[Professor Gordon shows slides of the various body parts used as numerical markers.]

In general symbol systems borrowed from the three perceptual base. Even the words for one, two, and three tend to derive from the *I* to one view, *you* to two and *three* as a notion of through, so a merging into that other system. You

see the three as merging in the notations for numerosity. They tend to show these repetitive elements up to three and sometimes up until four but not beyond. In Cuneiform, Etruscan, Roman, Mayan, Chinese, and Arabic we see these repetitive elements and then they go to something else after four. Chinese and Egyptian plurals are indicated by three-ness. You can see the threes emerging in these cave paintings from 1000 B. C.

[Professor Gordon shows slides of the cuneiform and examples of cave paintings.]

There is something fundamental about this three-ness that seems to reflect a limited individuation in a perceptual system.

I want to talk now about a second set of studies that I did when I went back the second year. I stayed there for two months and did some more systematic studies. I wanted to see if the Pirahã could perceive numerosities despite lacking linguistic labels, and I wanted to develop tasks that allowed them to show this without having to count overtly. We started with simple one-to-one matching and we added different configurations to make them harder. You then add things requiring memory and spatial transformations and so on. The studies were carried out with two villages in six weeks. As I said before, these are very small populations. In addition to the small number of people in these villages, it is impossible to test any of the women because of taboos and restrictions on interaction between females and outside males. The children just run away screaming if you try and test them. I had about seven subjects total.

In one of the villages I went to, I essentially went on my own so I had to learn enough of the language to be able to do these tasks, but I am certainly not fluent. It is a difficult language to learn. A missionary living there for ten years never really learned it. He was there to translate the Bible, and he became sick with anxiety every morning because he could not deal with the language. The payment for participation was food and beads. A subject would get easily bored so as an experimenter I would just have to go with the flow. There is a real concern that most people who have worked with them have been threatened, so you have to be nice to them.

[Professor Gordon shows slides and describes the various tasks performed.]

The first task is a matching task using the number of batteries as the sample to be matched and as the constituents used by a subject to perform the task. Accuracy in performance drops off after three. The second task is to copy a line drawn in a notebook.

Professor Paula Rubel: Are the tasks culture bound?

Professor Gordon: Some people might ask if it is appropriate culturally to use batteries. Yes, they actually do use batteries for hunting alligators.

The third task is a cluster line match asking the Pirahã to match batteries to a line of nuts. They did well up to eight, but performance accuracy also drops off after three, although they were able to do some sort of grouping. The fourth task is an Orthogonal Line task. I lined up the batteries parallel to the bar and they had to line them up in an orthogonal manner. You can see performance

dropped off quite precipitously after two. This is a very difficult task because you see you have to do some sort of tagging and do a spatial transposition. The next task is an uneven line match. I had an uneven row of batteries. This is an interesting task which shows a U-shaped variability. The Pirahã subjects did perfectly in three and four and their performance dropped precipitously and then increased again, as if they were doing some sort of chunking or grouping. When you get a large enough number you can start grouping into different sets.

To introduce a memory factor, I was doing a task that you would do in a subitizing test, where you just present an array very briefly. I would have a bunch of nuts on my side of the table and I would cover it with a book and I would lift for a second and then replace it. They would just receive a short amount of time to encode their numbers and they would have to match it with batteries. Again we can see that their performance drops off precipitously after two. That is a very low proportion at six and above.

In the nuts-in-a-can task, an array of nuts is put out on the table and they can inspect that for as long as they want. Then I would put the nuts into the can one at a time and when all the nuts were in the can I would remove them out one at a time. After each one I would say, "Are there still some in there, or is it empty?" At some point the subject has to say that the can is empty. Again you see they are very bad at this task, and performance drops off quite sharply.

The candy in the box task is a task where I would have a cassette case that would have a picture with a certain number of fish on it. I put the candy into the cassette case and I would put it behind my back and I there would be another cassette case behind my back and I would shuffle them around and bring out two of them. One would have the original configuration and one would have either one less or one more and they would have to choose which case had the candy in it.

You can see that performance is not much better than chance, beyond that two versus three.

In summary, these tasks show accurate performance for small numbers although when the task demands are increased you can see that even after about two their performance drops off. Large numbers are inaccurate and I want to argue that their perception of large numbers shows the same kind of analog representation that we might have for much larger numbers like 25. It has been shown that our perception of large numbers when we cannot count them shows these properties.

[Professor Gordon shows graph of average performance across tasks.]

The average response to various target items is almost perfect. The mean of their responses across tasks lies almost directly on target. A second line in the figure represents response variation increasing with set size. So, there is a gradual climb up until about three and you take the coefficient variation, where you divide the variability by the target size. The average is about six across group and tasks, it is the only way you can get a big enough number to do this. But, this finding assures the validity of the measures by opposing the

arguments that the subjects were not trying, that they did not understand the instructions, or that there was a big conspiracy to make fun of us.

Professor Michele Miozzo: They have a notion of “six or seven” but its not as precise as your notion of “six or seven.”

Professor Gordon: Right, they have an analog estimation of it. It is not a sort of precise counting system like we have.

Professor Robert Krauss: Doesn't it strike you that the matching tasks are conceptually very different than the memory tasks?

Professor Gordon: I tried generating these graphs by taking out the memory tasks and the result does look that different.

Professor Paula Rubel: Do you think that if these people became acculturated, that their performance would indicate cultural change, or perhaps an influence attributable to Portuguese coming in? The question has to do with cognition and language is separate. And you are talking about cognition as well as language in a related fashion without separating and them and saying that language and cognition are the same? Does the language make the difference?

Professor Gordon: I am not saying they have some genetic difference where they cannot count. Is that what you are asking?

Professor Rubel: You are saying there is a cognitive difference. That has nothing to do with genetics—it is cognition.

Professor Gordon: Based on the fact that they do not have number words that allow them to be precise. They would be like us if they had number words.

There is an interesting question is whether there is a critical period. So if you introduce those numbers to them as adults would they be able to ever understand those number words. Dan and Karen tried this in downstream villages. They set up a school and had numbers up on the wall and did number tasks. The kids got it right away. The adults came and after a couple of days they could not get it and did not want to come to school any more. That is not knock down evidence for a critical period but there certainly is a difference between how well the kids could grasp it compared to the adults.

Mr. Ezequiel Morsella: This is an ecological validity and testing question. I do not know what animals they hunt, but let us guess that they use bow and arrow. If you showed them six monkeys would they know how many arrows to take? The matching tasks to us seems very straight forward, but just the fact that they are being tested I think is hard.

Professor Gordon: It is sort of an unrealistic situation. I know what you are saying, can you think of a situation where they would have to deal with number in their every day life? There never is a situation where they know precisely how many monkeys there will be. The closest thing occurs when they are framing a hut, and they never build a hut with three legs instead of four, but I do not think it is because they count, I just think they do it geometrically.

Professor Abe Rosman: Has a trader introduced Brazilian money to them at all? When money is introduced, it is reasonable to expect the Pirahã to start differentiating amounts based on number.

Professor Gordon: Actually, the trader is better able to steal from the Pirahã because of their incapacity to handle numbers. Even when they deal with regular trading they are not very good at it. The Pirahã might spend two months in the jungle collecting Brazil nuts to trade, and then the traders give them a couple of cans of condensed milk for all that work and say that the Pirahã are indentured to them and have to collect more. It is a very harsh system. The stores that sell these products from the Rain Forest are really getting these supplies from very disreputable people.

Karen had three guys doing some yard work at this house that she built. They had been working really hard all day and done the same amount of work. To one she gave a big full bag of farina, and the second guy she gave half and the third guy she gave a tiny amount. When the third guy's wife saw how little he got she started laughing. The other two guys were laughing, including the second, he did not realize that he was being ripped off. The way that they deal with quantity is different. There is a lot of trading, and possession is transitory. There is a lot of moving around so everything they have they bring with them in their boat, so personal possessions are pretty much limited to their bows and arrows. There is not a lot of counting that goes on.

Professor Boris Gasparov: Do they have an expression to ask, "How many, how much?"

Professor Gordon: Yes, but there are things that the language does not permit, like, "Which has more?" To say, "This has more than that," you need to use recursive structure in language, but Pirahã lacks the construction of relative clauses. In this language it is possible to say, "This is big," or "This is small," but not "This is more or less than that."

Professor Gasparov: Did you conduct any experiments on unconscious processes, making things into larger or smaller parts?

Professor Gordon: No, they probably could, given that they can estimate numbers in another experiment. They could quantify things, but it is not the first thing to come to mind. I do not know if anyone has read Butterworth's book on the number sense. He talks about sitting down to breakfast and everything has to do with numbers: You pick up the paper, and the date is numbered, the pages are numbered, everything is quantified for us, but in that culture nothing is. When we see arrays of objects we obligatorily quantify them. We cannot see things as not being numerical. Their way is a very different way of seeing the world.

Professor Marco Jacquemet: What about days and the calendar?

Professor Gordon: They measure things by high water and low water. Time of day is noted by the position of the sun in the sky.

Mr. Jamey Hecht: What about age, people growing older?

Professor Gordon: No one knows how old anyone is. When you are doing studies it is impossible to find out how old anyone is, you just have to guess by looking at them.

I think that this evidence shows that the Pirahã do not have integer concepts. Interestingly *one* is not a privileged quantity in their system. The word *hói* can mean “roughly one.” Without a precise notion of *one* there is no way to generate a recursive system of counting. *One* is the central basis for having a recursive number system. We also need to think about what is the difference between what I have been calling strong determinism and expertise. Is this phenomenon different from the contrast of a master chess player who looks at a chessboard and sees configurations versus a novice who looks and just sees black and white pieces? Is it experience with quantification or is it actually the language shaping the way that we think? I think there are fundamental domains of language, and that if you do not understand the word *five* then you have got something wrong with your English. In contrast, if you do not understand the word *molecule* and you do not know what a molecule is, you still speak English. If there are domains that are fundamental to the organization of a language, and if these are the candidates for strong determinism, then it becomes more than a simple matter of having words it is that the language sets up these fundamental ways of thinking.

APPLAUSE

Questions

Professor Rubel: I am not sure whether strong determinism in your mind asserts a lack of translatability. It seemed at the beginning that was what you were saying.

Professor Gordon: I think that is one part of it. It is incremental ability. If you believe that language determines thought, then the consequence of that untranslated ability is that it makes concepts unknowable to the people of the impoverished language.

Professor Rubel: Are you familiar with the work of Berlin and Kay? I wonder what is the color terminology for these people. Is it *black* and *white*?

Professor Gordon: It is *black*, *white*, *red* and *green*. In my third year there I wanted to do something equivalent with colors, but we went to the other end of the river where they did not know me and they were suspicious of me so that I did not get much work done. They thought I was with the Indian agency and they thought I was carrying a gun.

Professor Rubel: It would be really interesting if it were just *black* and *white* and you had only two numerical figures.

Professor Gordon: If you look at the color literature, with Brown and Lenneberg, when they introduce memory tasks there were problems with remembering colors that they did not have words for. I think that color is so hard wired; that it is hard to make the case that you would actually not see. It is *dark*, *light*, *blood* and *bright*, which is *red* and *green*. I did do a sort of experiment; I had a towel which had lots of different colors on it. I would point to something that was yellow and they would say *red*, I would go to green and they would say *green*. I would go to blue and they could not call it *green* because they just called the other green, but on another occasion they might call it *green*. So now they might call it *dark* or *red*. The names that they

give the colors are not really fixed; they shift around a lot depending on the context.

Professor Robert Krauss: I have two questions; one simply has to with the method. I am supposing you gave them the choice between things that differ in quantity. Would they not be able to appreciate that difference?

Professor Gordon: I think it would depend on how big the difference was. If it was the difference between eight and nine then fifty percent of the time they would get it. If it was the difference between eight and four then they would probably get it.

Professor Krauss: The other question, in going back and reading the older literature up to the present you get the sense that in terms of these big metaphors like: language is the mold that thought is poured into, that there was not a real clear sense of the way that language affects thought. Lenneberg, in a paper that is not really well known, made the point that it is very hard to separate the concept from the culture. Is it the fact that they do not have the language, or is it that that culture does not need the concepts, that a language lacks a particular term?

Professor Gordon: I think there are other cultures that live very similar lives to the Pirahã that do have counting systems. I do not think you can necessarily predict based on the simplicity of the lifestyle whether a culture will have a counting system or not. I have a hunch that there is a relation between the language not having recursion and not having an aggregate counting system.

Mr. Jamey Hecht: If language does not have the term then it is unlikely that they are going to learn the concept. You can conceptually separate the notion of counting and how to determine for counting. So, is it the language, or is the language a marker for experience that that person has had?

Professor Gordon: What Whorf says is that language sets up the categories of experience. Depending on your language you experience the world in a different way. John Lucy interprets that as a claim that language affects habitual ways of thinking. I think he is wrong about a lot of things. There are lots of concepts we have in English that other languages do not have, which I do not think count against the hypothesis, concepts like *molecule*. If we go into another culture that does not have science they are not going to understand what a molecule is. I do not think that they see the world in a different way because they do not have that concept. For the Whorfian hypothesis to be interesting, we have to be dealing with some core concepts of the language. Core concepts tend to be things that reflect a cognition that is not just a fundamental way of looking at the world.

Professor Miozzo: I was impressed at seeing how good they were at the tasks, when you presented the graph it showed a linear increase. This is very similar to what I have found in some brain-damaged patients. For example there are patients who cannot recognize that the number 5 is *five* and 3 is *three*. If five is presented as an Arabic digit they would say eight or nine. But if you ask them to do another task that taps into the semantic system for these numbers, not only are they perfect but you also see a linear increase, a kind of distance effect. For example, they are faster at saying that the difference

between eight and five is bigger than the difference between six and five. Perhaps there are different of representations, one that is based on integral systems, which permit an individual to know that 5 is exactly what it stands for, and another system that is reflective of size, so *five* is something that is bigger than *four* and smaller than *six*, but you do not have a possibility of setting very precise contrasts. It is remarkable that some of our subjects demonstrate semantic appreciation it is very similar to what you have found.

Professor Gordon: I think that is important, the idea that the primitive number system is a dual model. The *one-two-three* and then greater is a less precise analog estimator. I think that is what we start out with. What the language has to do is make the rest of the number system look like the beginning of it. I think you have to bootstrap from that small quantity into the large quantity system.

Professor Remez: It would seem to me that there is no strong evidence that the Pirahã have *one*. I am not sure that the evidence requires a conclusion that they have one through three and then the system breaks down. It seems to break down right from the crack of the gun. To make the case for *one-two-many* warrants evidence that there is a conceptual notion of *integer* as opposed to a graded quantity distributed about one. If you could show that they had *one*, then I would be willing to accept that they had something like a number system. I would be very surprised if their language did not have quantifiers, in the normal linguistic sense, which include *some, many, every, all, each, none*. Those expressions I think are intrinsic to language, as intrinsic as prepositions are. As far as numbers go, I think as Bob says they lack the referential practice of counting, and the evidence here is just not strong for *one*.

Professor Gordon: You are right. I sort of assume they have the small numbers just extrapolating from everything else.

Professor Rosman: You mentioned that *I, you, we*, as another aspect of grammar, showed certain similarities to *one, two, three*. When you were asked about color categories, you said they do the same thing; they do not mean to say the color *red*, they mean the color *blood*; they do not mean the color *green*, they mean the color *light*. You have done a lot of analogizing into the different parts of the grammar which sound very much as though what Robert was saying is true, if indeed you could take parts of a grammar and say that it is related to other parts of a grammar. Things like shapes, pouring. In English you can pour liquids but you can also pour sugar. There are analogies everywhere.

Professor Rubel: They must have words for child and old men. You said they did not have words for young or old.

Professor Gordon: No I did not say they did not have words for young and old. They do not have precise ways of saying how old someone is. But Robert, they never made any mistakes with *one* or the quantity of *one*.

Professor Remez: If the measures were sharpened you might see contrasts appear. It might require a test to see if they can use rational numbers.

Professor Michael Studdert-Kennedy: One of the things that struck me about this curve, if you are judging *seven* would they be very likely to give you *eight* or six. So in other words they have got a good idea, an approximate idea. Where do children perform?

Professor Gordon: It is very similar to babies. A lot of the early baby literature that looked at the ability to distinguish numerosities showed that they went up to three. When they try to *three* versus *four* they could not do it. The message early on, is that they could appreciate numerosity because we are showing that they could distinguish *one* versus *two* and *two* versus *three*. They could never get *three* versus *four*, but they can do *eight* versus *four*, but they cannot do *eight* versus *six*.

Professor Studdert-Kennedy: It is normally the situation that an apprehension can be guided by language once they are given the words. I propose that the baby in our culture has a capacity, which seems to be about what the Pirahã have, but then when given words, they start doing it.

Professor Ann Senghas: I am intrigued by Robert's comment about them having *one more or less*. If they are twenty percent off, roughly, then any time you are presenting them with *one* versus *two* they are going to pick the right quantity. I would like to see how they were performing with mass quantities, for instance, with a cup of flour. If they were going to match these things would they also be twenty percent off all the time? If they are treating everything as analog then they would not even have one cup of something. Maybe they are really treating these things as analog that we treat discretely. My second comment is that I am struck by the lack of recursion. Do they have coordinate structures?

Professor Gordon: No, the only thing they can do is to stack adjectives.

Professor Jim Magnuson: Is it likely that in their culture a difference of one does not matter much because there are no precious commodities. Perhaps an operant training regimen would be successful.

Professor Gordon: It is sort of like learning a second language. Some people can learn it really well if they have the right approach and the right kind of teaching they can learn to speak a second language without an accent. But most people cannot. What is possible and what is normal are two different things. Probably with the right training, right circumstances we could probably teach them.

Dr. Inge-Marie Eigsti: Weren't you saying that someone did try teaching them?

Professor Gordon: Yes, but it wasn't very systematic. It was more like they just stopped coming to practice because they did not like it. The kids liked it. All we know is there is a difference between the attitude of the children and the adults.

Professor Remez: Let us thank Professor Gordon and adjourn.

Place: Faculty House
Columbia University
400 West 117th Street

Time: 4:00 PM

Chair: Professor Robert E. Remez, Barnard College, Columbia University.

Attendees: Lila Braine, Mamta Dadlani, Inge-Marie Eigsti, Bridgid Finn, Boris Gasparov, Jamey Hecht, Marco Jacquemet, Ilona Kovary, Robert Krauss, Jim Magnuson, Michele Miozzo, Ezequiel Morsella, Robert Remez, Abe Rosman, Paula Rubel, Ann Senzhas, Sara Stein, Michael Studdert-Kennedy.

Rapporteur: Bridgid Finn



1 NOVEMBER 2001

**The Relation between hearing and language
development:
New data from pediatric cochlear implant users**

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Cochlear implants are sensory aids that bypass an impaired auditory system by stimulating the auditory nerve directly, using electrical currents. They represent the most successful example of a neural prosthesis that replaces a human sense. In the case of pediatric cochlear implant users, language development is arguably one of the most important outcome measures. We will review some of the latest results in language development of children who have perceived speech only through this electronic prostheses. Although standardized language tests can provide important information about overall language development, they do not provide detailed information about specific linguistic skills nor the sequence in which they develop. One possibility [a "Language Instinct" hypothesis.] is that children with cochlear implants develop language in the same sequence as children with normal hearing, but in a delayed fashion. For example, noun plurals will develop first and the use of the uncontractible copula [IS or ARE.] and regular past tense will follow, in that order. An alternative [the "Perceptual Prominence" hypothesis.] is that the pattern of language development in cochlear implant users is strongly affected by the perceptual prominence of the relevant morphological markers. This hypothesis predicts that the uncontractible copula will develop first, followed by noun plurals, and then by regular past tense. Results from the CI users were consistent with the Perceptual Prominence hypothesis. This result represents a rather striking inversion with respect to the usual development pattern in normal hearing children and even in children with SLI. If the Perceptual Prominence hypothesis receives further support in future studies, clinicians who work in language rehabilitation of CI users may choose to target those aspects of grammar that are less acoustically prominent to these children. In addition, and from a theoretical standpoint, these results suggest that although there may well be an innate language acquisition mechanism, patterns of language development can be strongly affected by the input.

I am going to talk about the relationship between hearing and language development with evidence from pediatric cochlear implant users. I am going to start with a brief description of the auditory system as it pertains to cochlear implants. By the way how many of you have heard of cochlear implants before? I guess maybe ten people.

[Professor Svirsky shows a diagram of the human ear.]

In a normal ear, sound comes in through the external ear and sound vibrations move the eardrum: the membrane that separates the external ear from the middle ear. The eardrum is attached to a chain of little bones or ossicles. Whenever the eardrum moves these little bones move. The last one is attached to another membrane that is part of the cochlea. Let us zoom in on the cochlea and roll it out.

[Professor Svirsky shows a diagram of the cochlea rolled out.]

In the previous graph you can see that it is kind of snail shaped. The last membrane that has a bone attached to it is the oval window. The whole of the cochlea is like a tube filled with fluid. Another membrane runs along the length of the cochlea within the tube. This membrane is called the basilar membrane. It has very unique mechanical properties. If you sent a pure tone of low frequency of 200 Hz or so, the maximum vibration will occur at a place near the apex of the cochlea, whereas if you sent a higher frequency, the maximum displacement will occur at a place closer to the base. In this fashion, the cochlea acts as a frequency analyzer. It is very fortunate because we need that frequency analysis to understand speech.

[Professor Svirsky shows a cross section of a cochlea.]

The basilar membrane contains neurons called hair cells. Human hair cells are particularly important because they perform acoustic to electrical transduction. Impulses are normally transmitted by hair cells to spiral ganglion cells and then on to more central neural structures and, ultimately to the cortex. It so happens that these hair cells are impaired in many deaf people because of genetic factors, due to the use of antibiotics or due to exposure to other mechanical or neurotoxic agents. It is a tough kind of deafness, and there has been no cure until a couple of decades ago. In this instance, the neural transducer is dead, and it does not matter if you have as fine a sound as you would with a hearing aid, no information will reach the brain because the acoustic vibration will not be converted into electrical impulses. For people whose hair cells are dead there is a new technology called cochlear implants which can provide hearing sensation and a degree of speech perception. The basic idea of cochlear implants is to put electrodes in the cochlea to stimulate the spiral ganglion cells. In other words, the effect of the cochlear implant is to bypass those hair cells and to stimulate the spiral ganglion cells directly.

[Professor Svirsky shows schematic of cochlear implant.]

What we see on top is schematic of cochlear implant. On the left is outside the patient. On the right side is inside the patient. The cochlear implant has a microphone that picks up the sound, and a small speech processor. Some are a bit smaller than a cigarette box and are worn on the belt and others are even smaller and are worn behind the ear. The speech processor picks up the signal and processes it according to a certain strategy and then encodes that information into a radio frequency and transmits it using this transmitter antenna. This is like a radio station that is picked up only by this receiver. The receiver takes the signal and converts it to a pattern of activity spread across a number of electrodes that are implanted inside the cochlea. The bottom of the

slide gives an example of what the speech processor does with the sound before it is sent to the mechanism. The sound is filtered through four frequency bands that are non-overlapping.

[Professor Svirsky shows a spectral representation of a word looks after filtering.]

The rectified signals drive a band of non-overlapping pulse trains. Each one of those pulse trains is delivered to a different intracochlear area. It is important to note the speech processor replaces acoustic hearing with an electrical version of the ambient acoustic signal delivered directly to the nervous system.

[Professor Svirsky shows a spectrogram of the word CHOICE.]

This x -axis is time, the y -axis is frequency, and color denotes intensity. Each vertical moment is like an instantaneous spectrum of the acoustic signal while the person is saying CHOICE. The bottom part shows an electrograph of the simulation of adaptation to the cochlear implants.

[Professor Svirsky shows a graph of intelligibility tests of adult cochlear implant listeners.]

It is entirely reasonable for you to ask how well these things actually work. The y -axis shows percent correct in an identification test with listeners who were patients presented with a list of single words. The patient's task is to say the word he or she heard. There is no visual information on which to rely, so this is a rather difficult task. You can see variability in performance: some patients did very poorly, whereas others were right two thirds of the time. We have some patients for whom the cochlear implant is a good aid, we have some others who function very well in the world even when they talk on the phone.

In this talk the primary focus is the language development of deaf cochlear implant users.

This population of children is born deaf. Being deaf or hearing impaired has a number of disadvantages: obviously, you cannot hear, and interaction with the hearing world is accordingly difficult. As a consequence, the development of spoken language is impaired. The children can learn language of course, they can develop sign language perfectly, but oral language is a problem for them. There are two reasons to study language development in these children; one of them is to obtain clinically useful information. Parents want to know how well their children are going to do and doctors and clinicians want to know whether it makes a difference if the child receives the implant at the age of two or three or four. A study of this population of cochlear implant users offers to answer such questions about language development. The specific goal of the studies I will describe today is to distinguish a delay of language from a different course of development as a consequence of the difference between sensory samples delivered via the implant in contrast and those produced in a normal auditory system.

Let me start by showing you the tool that I use in this experiment. I use the expressive part of the Reynell Expressive Language Scales: the child receives points for language structure shown throughout the experiment, which is hierarchically arranged from non-speech behaviors to more and more complex language structures. The child also receives points for vocabulary knowledge and also for content, for the creative use of language in describing a picture. These test components are standardized and the credit the child receives for each response is also standardized. One nice characteristic of this test is that it has been conducted on more than 1300 children.

[Professor Svirsky shows a graph of the Language Gap post-implant. The x-axis shows chronological age and the y-axis shows language age.]

An average child with normal hearing defines the normal slope. In other words, at the age of twelve months he will have the language age of a twelve month old. A hearing impaired child with a chronological age of 45 months has the language age less than 24 months.

[The graph extrapolates predicted scores for children of 6, 12 and 18 months had they not received the cochlear implant.]

With CI users, at time of implementation there is a language gap between the chronological age and the language age. After implementation that gap remains about the same size. It does not increase with age, as is the case of the child without a cochlear implant.

[Professor Svirsky shows a graph of performance as a function of age at implantation.]

The next section of the talk addresses some of the tentative questions we have about the role of age of implantation in eventual language ability. If the gap between language age and chronological age remains about the same size after implantation as it was at the time of implantation, then it would be to the patient's advantage to receive the cochlear implant at a younger age when the gap between normal and hearing impaired performance is relative smaller. We can test this conjecture against this data set. Here, we see the performance of three groups of child implant users. The first group is children who were born profoundly deaf who received the implants between the age of 0 and 23 months. The language age increases with chronological age, but there is a performance level difference present at the beginning that stays about the same size after implantation. The second group of children received their implants between 24-36 months. Here as you can see there are some children achieve levels that are typical of normal children. However, there are also a few lagging behind, and as a consequence the average is further from the normal hearing slope than in the younger group of implanted listeners. The third group of children were implanted at 36-48 months. The regression line shown here is even further away from the normal average. Though we again we see some children performing adequately, fewer in this case here to scored nearer to the diagonal, nearer to the performance of normal hearing children. As you can see, though we cannot claim this with strong conviction at this point, there is a relationship: children implanted earlier perform at a level closer to normal performance.

The Reynell Assessment of Language Skills has two parts, one pertaining to expressive language the other to receptive language. Let me show you the regression lines for receptive language, as you can see it is pretty much the same as the data for expressive language.

[Professor Svirsky shows the data for receptive language.]

The conclusion from this part of the talk merits a caution. Statistically, the sample is too small to permit a strong inference, and the performance measures are potentially affected by methodological problems. With this in mind, though, our findings suggest that there is a trend for faster language development and smaller language delays for children who are implanted earlier. For those of you wondering about methodological problems, the inferences have all employed a linear interpolation, which may not be justified in characterizing the progress of language development. There are better ways to approach that problem, and we are working on that so that we will not have to use this interpolation technique. Beyond this, it is hardly adequate to reduce language ability to a score on a single test. In language, different kinds of skills are bound together. We are beginning to study grammatical morphology to test whether these children developed these skills in the same sequence as normal hearing children but in a late fashion, or in a different sequence, perhaps one that is affected by the perceptual dominance of the relevant acoustic markers.

Here we expand on these two hypothesis. The first is my extrapolation of the ideas popularized by Steven Pinker in the book *The Language Instinct*. It is an interesting book. According to Pinker, we are programmed to develop language and we can do so in the face of very minimal input. One possible hypothesis would be that if language skills are gained during normal neural development because of a gene, consistent with this is the idea that language development follows a predetermined pattern even in the cases of delayed development. If that is the case then we should see the patients we are studying develop the same sequence of language forms as normal hearing children. Noun-plurals appear before the use of the uncontractible copula, which in turn appears before the use of regular past tense. An alternative prediction is what I call the Perceptual Prominence hypothesis. Remember these kids do not hear like you and I do. They only have access to a reduced acoustic signal that does not have all the structure and detail normally available. Perhaps the aspects of morphology that are associated with the most prominent morphemes, considered in a sensory form, will develop first. The copula should appear before noun-plurals and these occur before regular past tense. Let me explain.

[Professor Svirsky shows a spectrogram of the utterances MARBLE, MARBLES, PUSH and PUSHED.]

You can see the main difference between PUSH and PUSHED is the final explosion of sound that lasts for a few tenth of milliseconds. The main difference between MARBLE and MARBLES is the presence of friction at the end of the word, longer in duration than the burst of energy at the end of the word PUSHED. By this argument, the sensory difference between MARBLE and MARBLES

should be perceptually more prominent for kids with cochlear implants than the difference between PUSH and PUSHED.

[Professor Svirsky shows a spectrogram of the utterances ARE and IS.]

The difference between MARBLE and MARBLES is the juncture between MARBLE and its final [s]. In contrast, the difference between IS and ARE includes a final [s] as in the plurals, but in addition to that the spectrum for [a] is different than the spectrum for [i]. In addition ARE has a low third formant. My contention would be that the perceptual contrast between the words ARE and IS is much more salient than a singular difference as in the plurals. According to the Perceptual Prominence hypothesis the copula should develop before the non-plural.

Let me show you the instruments that we used to assess these three grammatical skills.

The test was a sentence completion task. In the case of the uncontractible copula, the child receives a picture of a glass full of some liquid on the left and an empty glass on the right. The experimenter points to the picture on the left and says "This glass is full but this glass _____," and then child says, "is empty." We are looking for the use of the uncontractible copula. If the child says "not full" we have to accept it as a correct answer.

[Professor Svirsky shows a picture the child would use in the regular past tense task, depicting a car that has run out of gas. The experimenter asks, "What did the driver do to the car?" And the answer is supposed to be "pushed it".]

I am going to use some data from cochlear implant users but also from comparison groups. The data from comparison groups was obtained by Larry Leonard at Purdue University for an independent study that used the same methods. We had three groups of normal hearing children. One group was identified with Specific Language Impairment [SLI], which is observed as a delay in development of certain grammatical skills, unrelated to hearing loss. Leonard also used hearing children who are identified as SLI. In the sample of cochlear implant users, most were congenitally deaf, with a mean age of implantation of 8 months; the range was from birth to 2 years and 5 months. The average age at testing was 6 years 8 months.

[Professor Svirsky shows the age equivalence for the Peabody Picture Vocabulary Test.]

Cochlear implant users show a vocabulary of a child about 1 year and 3 months younger than their chronological age, which makes sense given that this is about the time they were without hearing.

[Professor Svirsky shows a graph of performances in the use of non-plurals, the uncontractible copula, and the regular past tense.]

Normally hearing children develop non-plurals first then the copula and then the regular past tense. That is true also for children with SLI. When we look specifically at the plural and the copula, which is where the Language Instinct Hypothesis and the Perceptual Prominence Hypothesis make different predictions, performance of the normal children is better on the plural tasks than on copula tasks. SLI children did better with the plural tasks than with the

copula tasks. Did children with cochlear implants follow the same pattern as all these populations of normal hearing children? The answer is, “no.” The non-plural tasks are not better than the copula tasks. Nine did better using the uncontractible copula than with the non-plural. It is basically backwards. We see a significant difference in the performance of the cochlear implant users and the other groups.

In conclusion, children with normal hearing exhibit one procession of grammatical markers while children with impaired hearing follow a different pattern. Children classified as Specific Language Impairment individuals follow the normal hearing course. One possibility is that this difference in developmental pattern is related to the differential perceptual prominence of syntactic markers.

APPLAUSE

Questions

Professor Robert Krauss: As I understand Pinker’s argument, the sequence of acquisition of grammar is really not driven by things perceptual but things inherent to the language. Is it possible to make the argument that it is also perceptual prominence for these kids and that different things are prominent?

Professor Svirsky: My interpretation is that hearing people hear these differences so well that that is probably not a factor. There is no perceptual prominence account that I know of that explains the order of appearance of these forms in the speech of normal hearing children.

Professor Boris Gasparov: My question is about the perceptual prominence of different features regarding the copula and noun ending. In the expressions during conversational speech, the endings can be very inconspicuous. Did you do an analysis at this level?

Professor Svirsky: No because the elements for each one of these probes were chosen that they would be consistent along those lines. For instance we used contexts in which it was obligatory to use the uncontractible form of *is* and *are*. For plurals we just used final *s*.

Professor Gasparov: There is one thing that goes beyond the acoustic perception of the copula. With the noun, plural singular involves just this noun, with the copula the whole sentence is involved.

Professor Svirsky: That is true whether or not you have impaired hearing. For example in the sentence “The flowers are green.” you need to use the plural as well as the copula. So it makes it easier for all groups to learn the copula, but it is not sufficient to explain the inversion.

Professor Robert Remez: You will not get any argument around this table that language is multi-dimensional. The stunning finding is that intercepts differ as a function of age of implantation, based on the Raynell. What is the result if you use other measures, does that finding still hold?

Professor Svirsky: You cannot use other methods, because they have not been standardized.

Professor Remez: I guess I thought all these things had been normed to use for deaf kids in schools.

Professor Svirsky: There now is a test that assesses a number of skills. Growth curves seem relatively smooth. If you looked at a single curve it would look very jumpy. I agree that it would be very interesting to do that, I do not know if the results would be as straightforward.

Professor Teresa Boemio: Could you go back to the slide showing MARBLE and MARBLES and IS vs. ARE ? I do not see the difference perceptually between IS and some pure plurals. In this case for MARBLES it looks like there should be some voiced component on the spectrograph for [mɑɪbəlz], because /z/ is the voiced component. A regular plural can be a voiced fricative like MARBLES, a voiceless fricative like CATS, or a syllable, as in PIECE and PIECES.

Professor Svirsky: We only used s and z plurals not like the syllabic plural morpheme in PIECES.

Professor Boemio: Well I am not sure how that would lead to the use of a copula in development before the use of plurals.

Professor Svirsky: Let us imagine that a child hears the word IS used in different contexts, while at other times something clearly different is used. That might lead the child to be vigilant for a contrast, to make sense of the different forms. In contrast, when MARBLE and MARBLES are used and the child does not quite resolve it as a sensory contrast, it is far less prominent. This less prominent acoustic difference may fail to incite language acquisition.

Professor Michele Miozzo: You have a very indirect way to assess these acoustic features. Did you ever think of testing acoustic abilities in a more direct way, by not having children produce these words? The reason I say that is because there are other general cognitive factors that determine the patterns you observe. You are testing kids with different ages, this can be another factor.

Professor Svirsky: I agree with you that would be nice to determine. My half-baked idea is to do some tests involving reaction time and discrimination, using pairs so that the perceptual properties are analyzed in a stricter fashion. This hypothesis predicts that those discriminations that take more time will appear later in the languages produced by these children. Overall, though, testing hearing-impaired children is difficult in general. At least we could test reaction times with adults and noise and see how difficult it is.

Professor Michael Studdert Kennedy: I was wondering what these children do when they are normally talking? They use ARE and IS correctly but do not use plurals correctly?

Professor Svirsky: Some of these scores show that they do not use either one in a completely consistent fashion. Some of them do very strange things. I wish I could come up with not only scores but also some of the oral responses. They know they have to do something, but they do strange things. They may know that they are not producing the non-plural correctly.

Professor Remez: It is sort of like what happens in a pro-drop language The head noun drops out completely. The number of the agent is signified by the morpheme bound to the verb.

Professor Lila Braine: If I understand correctly children acquire the regular copula at about the same age as normal hearing children?

Professor Svirsky: No, the cochlear implant user's proficiency is much worse than hearing children at the same age.

Professor Braine: What about the earlier periods in development? Are those instances of speech very similar or different? You had said before that some of their responses were very peculiar.

Professor Svirsky: That is a good question, but I do not know the answer. By the time these children are identified as profoundly deaf and come to the clinic, they already have some language in place. The other thing is you will notice that these children are about seven years of age. It is very difficult to test the very young children.

Professor Braine: Were the children who received the implants at a very young age followed?

Professor Svirsky: Yes but they were not followed with instruments as detailed as the grammar probes we have used here.

Professor Braine: In other words no one kept track of the language as it appeared?

Professor Svirsky: We kept track of language at a macro level, using not very detailed tools. Those tools are developmental language scales. We started administering the MacArthur Developmental Inventory, a parent questionnaire. It correlates very highly with behavioral measures. We have used those questionnaires to obtain macro index of development but we have not used them to look at specific individual skills.

Professor Braine: I was thinking maybe it would be easy to use other methods, maybe a parent or someone who deals with the child very regularly keeps a record so you could get better information. I think it would be to your advantage.

Professor Svirsky: I think it would be wonderful but difficult. You would need a lot of full time equivalents. The type of transcriptions that you are talking about are extremely work intensive. Especially so with children that are hearing impaired, transcribing their productions gives another layer of complication to the analysis.

Professor Remez: Let us thank Professor Svirsky and adjourn.

1 NOVEMBER 2001

Place: Faculty House
Columbia University
400 West 117th Street
Time: 4:00 PM

Chair: Prof. Robert E. Remez, Barnard College, Columbia University.

Attendees: Robert Remez, Glenis Long, Lois Putnam, Anne Zsdrommler, Boris Gasparov, Michael Studdert-Kennedy, Lila Braine, Michele Miozzo, Jim Magnuson, Robert Krauss, Ezequiel Morsella, Monica Ciapponi, Dan Kimball, Lisa Son, Teresa Boemio

Rapporteur: Bridgid Finn



27 NOVEMBER 2001

**Putting 'teach' back in 'teacher':
Using MEG to distinguish similarity versus
identity in mental representations**

Alec Marantz

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A recent trend in both anti-linguistic connectionist theories (Seidenberg & Gonnerman, 2000) and in Optimality Theory-inspired morphophonological theories (Burzio, 2000) is to suppose that words are memorized in the mental Lexicon. Lexical relatedness is accountable for the appearance of derivational affixation and for the apparent internal syntax of words. Seidenberg expresses the view by claiming that there is no TEACH in TEACHER. Rather, such apparently morphologically related words support independent representations, connected by strong phonological and semantic similarity. Studies at the KIT/MIT MEG Joint Research Lab have suggested a path to distinguish between identity and similarity of lexical representations, that is, between (1) TEACHER as TEACH plus an additional morpheme and (2) TEACHER as very similar to TEACH along semantic and phonological dimensions. I will review the evidence from our lab indicating that the M350 MEG evoked response component indexes initial activation of lexical representations. Then I will present a cross-modal priming study exploiting the M350 that separates similarity from identity effects, with phonological similarity causing the two sorts of inhibition of the visual target by the auditory prime described above. Pairs consisting of an auditory prime like TEACHER and a visual target like TEACH exhibit neither of the effects expected for phonologically similar but not identical lexical representations, demonstrating that TEACH is in fact identical to part of TEACHER.

This talk is a picture that we would like to give regarding the structure of linguistic knowledge. Linguistic knowledge is a system that connects sound and meaning and the connection is through a combinatoric system called syntax. I wrote a paper recently that is called "No Escape From Syntax." In this picture of language there is no way of relating sound to meaning except through a combinatoric system of syntax. On this view language includes a single generative engine, a single place where you can put pieces together, and that is called syntax. Language production and comprehension involve the same generative machinery. All decomposition and composition is syntactic, that includes morphological decomposition, the decomposition of words into constituent morphemes. The notion of lexical relatedness, that is when are two words related to each other, is defined by identity. Words are related if they decompose into identical pieces. The examples, TEACHER and TEACH are related

because they both contain the identical root TEACH, as in WALK and WALKED. GAVE and GIVE are related because both contain the identical root GIVE.

I do not know how you feel about that view but it has undergone some recent challenges. The one that I am going to concentrate on was published in *Trends in Cognitive Science* by Seidenberg and Gonnerman, who claim that there really is no TEACH in TEACHER. That is, all words have independent representations and TEACH and TEACHER are similar because they are very closely related semantically and phonologically. Recently within linguistics itself, Burzio and others have claimed that all words are memorized and lexical relatedness is mediated by output-output correspondences within the framework of Optimality Theory. These are alternatives to the view that words decompose.

One thing that the transformational view does is say that you have to decompose. By claiming that you have to decompose, you can explain relations among words. In the Seidenberg view words can essentially mean anything and words that share phonological form are likely to share semantic properties. In the transformational view words can not just mean just anything.

[Marantz shows a slide of the words MARGIN, MARGINAL AND MARGINALITY, VISCOUS, VISCOSITY and VIRTUE, VIRTUOUS and VIRTUOSITY.]

The last set looks like it may have the same pattern of noun-adjective-noun, but is that right? No, in fact it is not right. VIRTUOSITY does not mean the state of being virtuous; it is not related to virtuous, it is related to VIRTUOSO. VIRTUOSITY can not mean the state or quality of being virtuous, even though the word exists and the pattern exists. This should be a mystery of the view of lexical relatedness that says that words are related semantically and phonologically, or they are memorized as whole words. We have memorized VIRTUOSITY as a whole word, so why is it not related to VIRTUOUS? In the decompositional view we actually have an account of this. If VIRTUOSITY were related to VIRTUOUS it would have this decomposition.

[Marantz shows slide of the words.]

There is a noun, VIRTUE, adjective VIRTUOUS, and we'd be creating a noun from the adjective. Something we know about noun forming suffixes attaching to adjectives formed with -OUS is that -ITY is not what you say here, you say -NESS. The noun related to VIRTUE is pronounced VIRTUOUSNESS. It's not true that words on principal can mean just anything. It is the same way with sentences. Sentences can't mean just anything if they conform to the grammar of the language. Words and sentences demand decomposition, and decomposition has to obey the principals of grammar.

Professor Jim Magnuson: Are you claiming that in the other views VIRTUOSITY really would be a mystery, that there really is no way to explain VIRTUOSITY upon those other views?

Professor Marantz: Yes. Do you want to challenge that?

Professor Magnuson: VIRTUOSITY and VIRTUE will not pattern together in the same way as VISCOUS and VISCOSITY do.

Professor Marantz: You can either claim that you never say a word you have not heard before which is false, morphology is productive. If you allow morphology to be productive you have to allow generation of forms by analogy. So that the fact that you hear a form VIRTUOSITY you know that form exists. By analogy you should be able to relate it to VIRTUOUS on the Seidenberg view.

The view I am arguing against, which I will call “the wrong view,” says that TEACHER and TEACH are related by a network of similarity relations. They are different words, but they compete. In this view a word must be either TEACHER or TEACH but not both. “The right view” says that TEACHER contains TEACH. If you hear TEACHER and TEACH you hear the same thing twice. To choose between these rival accounts we need to distinguish between activation and access to very similar, closely related lexical representations and repeated activation of the same lexical representation. We need to be able to tell the difference between similarity and identity. Is TEACHER very similar to TEACH or is it in some senses identical? Lexical access involves stages, the first being initial activation of lexical representations and then some process that determines the number of activated representations based on the input and decides which one is the best match to the input. If I see a word which activates representations that match the input in some way, lateral inhibition of representations first occurs and then a competition process ensues among activated representations for a selection process. In the instance of phonological relatedness, if you hear SPINACH, at the SPIN part you are ready to activate SPINACH but also SPIN. You have heard SPIN when you have heard SPIN-ACH. At the -A- part you are going to inhibit the representation of SPIN. We will show evidence that you inhibit it, at its low resting state.

What about stems like TEACHER and REACH, which share sounds? I will show evidence that when you hear TEACHER you excite the activation for REACH, which stays at an excited level. If TEACHER and TEACH were separate words with separate representations, we would expect that someone hearing the word TEACHER would inhibit the representation of TEACH. If you are asked to make a decision about TEACH immediately after hearing TEACHER, you should experience competition between the activated representation of TEACHER and the representation for TEACH. In the brain, is hearing TEACHER does not inhibit or suppress the lexical representation of TEACH. In terms of decision behavior, hearing TEACHER does not cause competition for recognition or decision of TEACH. The results I will show you will support the predictions of the decomposition theory and words like TEACH must decompose into TEACH and -ER.

Before going further with the evidence, I want to show you something about the method. We should consider a typical MEG response called the M₃₅₀, and see how this measure can show why TEACH and TEACHER are identical, rather than simply similar.

[Marantz shows a short movie illustrating the MEG technique.]

The magnetic field emanates as electrical activity from the dendrites of the pyramidal cells. You place the head adjacent to superconductive sensors that can measure very small magnetic fields generated by electrical activity within the brain. From each sensor arrayed around the head you get a graph of the changing magnetic field tracked over time. We overlaid the measurements from a number of sensors. The temporal resolution of this technique is as good as you could want, with a millisecond or better resolution. We use 93 sensors to cover most of the head.

[Marantz shows the MEG response from visual word presentation.]

There is a big peak at about 170 ms that occurs at the back of the head, in the fusiform neighborhood. This response is shown to be absent in dyslexics by a group in Finland. It is a visual word form response. We observe another response at about 250 ms after stimulus onset, which we were exploring, and then we got this response at 350 ms, which has localization very near auditory cortex on the superior temporal lobe.

Professor Magnuson: What is the event that actually changes the magnetic field within a neuron that you can detect, and what is lag between the actual event and when you can detect it? You do not employ a subtraction methodology; you do not have a control task that you can compare?

Professor Marantz: This has a baseline. I do not understand the first question.

Professor Jim Magnuson: In MRI you are detecting changes due to oxygenation and there is a lag between cellular activity and your ability to see what is going on.

Professor Marantz: This is a direct measure of cellular activity.

[Marantz shows MEG response graph for average of 30 similar stimuli.]

It has been baseline adjusted. Stimulus onset is taken to a baseline of zero. The average level of the signal for 100 ms before signal response is taken to zero and the waves after that are adjusted.

Professor Magnuson: You do not try to separate the visual component and the linguistic component here?

Professor Marantz: We do this in the standard method of behavioral psychology. We make that a variable in the experiment. If you compare response to symbols versus letter strings you can modulate the response and that is an indication of the function. We do a standard analysis of variance stimulus manipulation.

Professor Magnuson: When you are talking about inhibition, is MEG capable of showing you a decrease in firing as related to inhibition or is it only capable of showing you the inhibitory firing of the neuron that you are targeting?

Professor Marantz: It should be able to show you both. We set up this experiment in such a way that our interpretation that says it is the inhibitory firing.

Professor Peter Gordon: How do you identify that fairly narrow source?

Professor Marantz: We use a single dipole model that accounts for the M350 for around 90% of the data at the peak of the response. In the same experiment with the same subjects we used tones to fix a functional landmark for sound. We are using a lexical decision task in these experiments. Subjects look at strings of letters.

[Marantz shows a video of subject in MEG experiment.]

We are going to give evidence here for separation between the activation part of lexical access and the selection-recognition part. We are assuming that the stimulus activates representations that point to features of the stimulus. Then there is a competition process between active representation before selection. We want to know what kind of stimulus properties affect activation, competition and selection. We will claim that the M350 response is an index of the activation part and not of the competition and selection. M350 is an index of initial activation. We are doing visual lexical decision for most of these studies and we are assuming that the lexical representation is a connection between sound and meaning, in a traditional linguistic sense of a Saussurian sign. We are talking about the same representations activated through auditory or visual presentation affected by phonological or semantic factors. We are assuming that there is not an orthographic lexicon, a phonological lexicon and a semantic lexicon. We are assuming that the representations are the locus of the relationship between sound and meaning. Given that a brain response that indexes initial activation of lexical representation, as opposed to competition or selection, should vary the stimulus properties that affects lexical access, and should be the earliest response affected by these properties because it is the first part of lexical access. It should be the earliest component shared by visual and auditory word presentation and both phonological and semantic features should affect it. I want to show that the M350 response has all those properties.

Frequency affects lexical access, and more frequent words accessed more quickly than less frequent words. Repetition affects lexical access, and a subject is faster to respond to a recurrence of a word that has recently been encountered. We therefore use two studies: a frequency study and a repetition study. In the frequency study we had six frequency classes of words, from high to low frequency, in a lexical decision task, compared to non-words. In the repetition priming study, pairs of visually presented letter strings were presented and a lexical decision was made on the second member of the pair. Sometimes there was repetition, sometimes there was not. Results for the frequency studies showed that reaction time in lexical decision was slower the lower frequency of the word. In looking at the M350 response latency we see that the lower frequency of the word the slower the M350 response. Brain response parallels behavioral response. In repetition priming we see the same thing. Repeated words are faster than non-repeated words in reaction time and the M350 response is significantly faster for repeated words.

How do we know that the M350 indexes activation as opposed to other things that happen before reaction time, if it is perfectly correlated with reaction time? We want to find a manipulation that shows it to be independent of reaction time. How can we separate activation from competition–decision so we can show that the M350 is independent of reaction time? We need stimuli that give fast activation but slow decision and others that give slow activation but fast decision. Stimuli that have common sounds within them, stimuli with high phonotactic probability show fast lexical activation. The same stimuli with high phonotactic probability come from dense phonological neighborhoods; there are a lot of words that sound just like them. Dense phonological neighborhoods cause increased competition and slow down the reaction. This is work by Paul Luce on the neighborhood activation model of lexical access. High phonotactic probability speeds up activation while high neighborhood density causes competition for selection and slows down lexical decision.

Professor Magnuson: I think I missed the part where you talk about evidence for high probability speeding activation.

Professor Marantz: I did not tell you the evidence I just said that it does. The evidence from Luce is that in reading non-words you are faster for the ones with high probability sounds. If you make a lexical decision on the non-words you are slower for those same non-words.

Professor Magnuson: It seems like there is a large inference to be made that reflects fast lexical activation.

Professor Marantz: Let me show you the data that show that is just the right story.

So again, if you activate a large neighborhood, competition slows down reaction time, if you activate a small neighborhood less competition leads to faster reaction times. We used stimuli from Vitevich and Luce’s auditory study. We converted the stimuli to orthographic stimuli. The words that have high probability sounds come from dense phonological neighborhoods, low probability sounds come from sparse phonological neighborhoods. Otherwise they are matched for word frequency and length. We get just the beautiful result that we predicted. M350 latency we think indexes activation. Higher probability sounds give you a faster M350 but in terms of reaction time slower M350 because more competition slows it down. In the rest of the talk you will see brain and behavior reactions go in opposite reactions again and again. This is evidence that the M350 is indexing the activation part. When we separate activation from the competition–decision we see that M350 is sped up by things that should speed up activation, but its not affected by the thing things that cause competition.

This is all visual, although Vitevich and Luce used an auditory presentation, and our premise requires orthographic to phonological conversion or direct access via orthographic entries. We would like to replicate this in the auditory modality but it is not that easy. We have a study in progress. I will show you what the results should look like and I can show you the results from a few subjects.

If you hear a word, you first get the auditory M100 response you get to the onset of any sound, and then you should get the M350 response.

[Marantz shows localization of response and response latency in one subject.]

We are claiming that this response should be the same latency and same location regardless of whether it is auditory or visual input. It is localized in the neighborhood of the auditory cortex. We are not the only group who has localized the response to that location. There is a group in Japan that has done a similar repetition priming study, that also get a response to this latency range in that location. A group in Finland does what they call M400 studies with MEG. They use classic M400 paradigms and the response they get in their studies at this latency also localizes to this area. For the same subject we can get an auditory and a visual M350. The visual response also shows the M250 response in the middle. But, the auditory M350 did not move around with the stimulus manipulations. And, because we have not moved the auditory M350 around with our stimulus manipulations yet, we have not shown that this indeed is the M350. In order to show that it is the M350 we have to observe its displacement with phonotactic probability. However, I can show you that this response is affected by both phonological and semantic factors.

We are doing a cross modal priming study now. In this project, the participant hears a word and sees a letter string and makes a lexical decision about a letter string. Now, we get the examples I primed you for at the beginning of the talk. We use initial match pairs like SPINACH and SPIN, non-initial match pairs like TEACHER and REACH. There is a semantic priming pair, for example idea followed by notion. We use synonyms for semantic priming. There is a morphologically complex condition that used words like TEACHER and TEACH which I will talk about later. First lets concentrate on the phonological and semantically related pairs.

[Marantz shows a graph of reaction times for semantically primed words and controls.]

The reaction time priming in milliseconds is fairly large for the semantic priming. We also get the M350 latency primed significantly. We have also included the amplitude on the M350. If you look at the literature on the M400 in the ERP literature, word frequency affects the amplitude of the M400 response. Do the effects that modulate the timing of the M350 also affect the amplitude? The notion is that the amplitude represents amount of work and if something has been primed it should be easier and therefore should be a lower amplitude. Lower amplitude goes with faster, higher amplitude with later. You lower the amplitude of the M350 after every latent word. In the literature we know that behaviorally these kinds of matches, either initial match or non-initial match cause slow downs. Phonological similarity in reaction time causes a slow down, although apparently this is up for debate in the literature. We do not know if it is because SPINACH has inhibited SPIN before you see SPIN or if it is a competition effect. For SPINACH-SPIN you have inhibition in reaction time, also inhibition in the M350. The M350 peak is later than it is for MUFFLER-SPIN.

[Marantz shows graphs of the results.]

For TEACHER-REACH reaction time slowed down, but M350 priming both in amplitude and latency. TEACHER- REACH has a faster M350 response than for TEACHER-OCEAN. In the existing literature, Isel and Bacre show the same results. In cross-modal priming, when CARGO is followed by BUS, there is no priming of BUS through CAR. But if CARGO is followed by COME priming is observed of COME through GO. What they suggest is that you get a decay. CARGO does activate BUS through CAR but it has decayed because it is the first syllable. We know now what is going on. In fact, for CARGO-BUScar is inhibited, but in CARGO-COMEgo is primed, explaining the results.

Now, to put TEACH back in TEACHER, we did the same study using morphologically complex words such as TEACHER-TEACH. When words are similar phonologically you always get a competition effect, but for initial matches you always get lateral inhibition of the M350. For non-initial matches you get priming in the M350. If TEACHER and TEACH were separate words, their relation would match SPINACH and SPIN and therefore you expect to observe inhibition of the M350. Of course, there is always semantic priming. You would expect some kind of interaction from the inhibition due to phonological relatedness and the priming due to semantic priming. But, in this case you do not observe an interaction. It looks like you evoke identity priming for TEACHER-TEACH. There is no evidence for inhibition or competition. The problem for the account offered by Seidenberg is that phonological similarity hurts instead of helping either because of inhibition or competition, or both. Semantic similarity and phonological similarity pull in opposite directions rather than adding up.

Suppose it is not a linear effect, though. Suppose phonological similarity causes inhibition and competition between representations up to some threshold level of semantic similarity. If items are really close semantically then the phonological effect is lost. When semantic similarity is sufficient, phonological similarity would have no negative effect, producing no phonological inhibition. But we know that is not right. Phonological similarity without identity is still inhibitory.

Even in the context of semantic identity, we know that this pattern occurs from a study that Allen and Badecker did. Consider a result in the literature that is mind-boggling. Steven Pinker says that regular verbs are different than irregular verbs in the past tense. The evidence for it is that WALKED primes WALK, but GAVE does not prime GIVE therefore regular and irregulars are different. You should be saying to yourself how could GAVE not prime GIVE, forget about past tense mechanisms, semantically GAVE and GIVE are as close as you can get. Why is there no semantic priming here? Badecker showed that you do observe priming if the stem and the past tense are less close phonologically. If you arrange pairs of past tense and stems on the basis of phonological similarity, the more distant they are phonologically, the more priming there is. It means that phonological similarity hurts even in the context of semantic identity. We predict that in the GAVE-GIVE case where is no reaction time priming, we should get M350 priming and then a competition effect. In TAUGHT-TEACH there is also M350 priming but less competition because they are phonologically less similar. In WALKED-WALK it is identity priming.

There is another affect that Harold Baayen has found. Your reaction to a word like ACID depends on the number of derivatives of ACID in the language, what he calls the family size. If you take two words that have the same frequency and are matched for other properties, if they differ in family size your reaction time to them will be different. Particularly, the more derivatives you have the faster you are at responding to the word. The family of ACID includes things like ACIDITY, ACIDIC, ACID TEST, and ACID HEAD: the family includes compounds. You have a cluster of related items, they are all activated by the ACID part of ACID and maybe they reinforce each other in some way. The more there are the more they reinforce each other. On that view you would expect frequency to matter, but it does not, only the number of derivatives matters.

Dr. Sarah Callahan: Do they have to be derivatives that are phonologically similar, do words like TRICYCLE and BICYCLE qualify?

Professor Marantz: I believe that prefixed forms also count as derivatives.

I think we have a handle on this now and the explanation makes direct predictions about a possible MEG experiment. Suppose that in the theory of morphology which my colleagues and I are developing, you do not hear roots, you do not say a root you do not use a root. Every word you say has a root plus at least a bit of morphology that creates a word out of it. There is a root ACID, but the noun form ACID also has an affix on it. It is the root ACID plus noun-forming affix (which in this instance is phonemically null). I wrote a paper entitled "Cat as a phrasal idiom" that embodies this notion that even CAT is not simple. When you hear the word stem you are not hearing everything made from the stem, but, rather, the stem plus its affix. Each affix, like the zero noun-forming affix that you add to the root ACID to make the noun ACID, has to be listed as attaching to ACID. The important entities are things that are not entirely productive, which means that a particular ending like -IC has to be listed as attaching to ACID. These affixes and compounding heads like -HEAD or -TEST in ACID HEAD or ACID TEST will be activated anytime you hear or see ACID because they see ACID on their list. It is listed as attaching to ACID. That should have a facilitory effect of priming ACID. All these things that list ACID should prime ACID when it is heard. There is no reason to think that that can not be frequency based, that there should be priming proportionally to the frequency of the connection to for example, -ITY and ACID and ACIDITY. The M₃₅₀, which is activation of the representation of ACID, should be speeded by family frequency, that is both family size and family frequency. However, reaction time should be inhibited by competition among all these affixes that attach to the root ACID. So even when you hear just the word ACID, it has that zero noun-forming ending, and has to compete with all the other things that could have attached to the root, ACID. The competition effect should be proportional to family frequency as well, that is both number of derivatives and their frequency. The latency between the speeded up M₃₅₀ and the reaction time should be correlated with family frequency. Family frequency, though, is pushing the M₃₅₀ and reaction time in opposite directions under this hypothesis. If we do Baayen's experiment we should see an effect of family frequency on the distance between the M₃₅₀ and the reaction time.

The conclusion is that TEACH is in TEACHER in the most literal sense. TEACH following TEACHING yields identity priming and not similarity priming. What does this mean for Seidenberg's account? He seems to think that CAT and THE CAT are related in some way differently than the way that TEACHER and TEACH are related. It is not true that you memorize sentences, and that "the cat is on the mat" is just phonologically and semantically related to "the cat is in the room." He seems to argue that words emerge as representational units from exposure to sentences. If he believed that, then I do not see why he would not believe that morphemes emerge from exposure to sentences. TEACH should emerge from TEACHER as a unit. If he does not believe in words either, that is, if CAT is not a unit, than he should tell us so. The finds I discussed today should compel someone like Seidenberg to be more explicit about how language works, how units are identified, and how they function. The conclusion should be that identity is not simply similarity at the unit. It does not work with statistically fuzzy things. Knowledge of the relationship between words is knowledge of the identity of their parts. I believe that linguistic theory is literally the best theory we have about brain computations that underlie language use. Every operational linguistic theory is a brain operation. There is not another way you could deal with language. Tom Davers says, "Syntax last," meaning to say that you use strategies first and then when you are stuck, syntax kicks in. But, I have defended an alternative: It is not only "Syntax first," it is "Syntax only." The only way to construct linguistic representations is to go through the grammatical system.

APPLAUSE

Questions

Professor Robert Krauss: The question I have has to do with semantic priming and listening. It is not clear to me how the representation of word that can have many different meanings can prime recognition of the form.

Professor Marantz: You are asking a question that goes far beyond the details of anything I said. There is some literature on the different notions of a word, for example, BANK and BANK. There are the semiotic uses of a word. There is good evidence that semantic priming seems to prime the same representations as phonological factors do.

Professor Michele Miozzo: Did you look at frequency for the following predictions: If you have a word like TEACHER versus TEACHING, which share the same root or stem or the same morpheme, you should find that the cumulative frequency of all of the occurrences of TEACHER predicts the kind of effect and not the frequency of the individual forms.

Professor Marantz: It is complicated because there is sure to be a cumulative effect of all the uses of TEACH, so root frequency should have an effect. But there are added competitor effects on any behavioral result you might make. I think the purest form of the prediction should be that the M350 latency is effected by cumulative root frequency but the reaction time is going to be very complex because there will be different competitive factors for different affixes.

Professor Miozzo: The reason why I suggested this is because you show a very robust effect of frequency so you have an effect that is easy to be manipulated.

Ezequiel Morsella: I want to make sure I have your main point clear. You said that there is one theory that claims there are fuzzy bundles of things and another theory that says there are different kinds of computational units. What you are saying that Seidenberg et al defend a notion of a word as an episodic memory. The priming effects follow the same kind of effects that you observe with object knowledge. What you are saying is that these are not regular objects but are instead computational units. Do syntactic properties determine these effects?

Professor Marantz: I think you are going beyond anything I said. The crucial point is that words, like sentences, are complex objects that are not recognized as wholes but are recognized via composition. We can not discover all the properties of a word like TEACHER just by looking at its relation to other words. The crucial aspect is that it decomposes. For Seidenberg there is no notion of morphological relatedness above and beyond phonological and semantic relatedness. We will put aside the notion of what morphological relatedness might mean, it is not exactly clear but clearly he means that there is no decomposition. Two words are morphologically related if they share an identical part.

Professor Michael Studdert-Kennedy: From what you said at the end it would seem that you would not want to distinguish modalities. You would expect the system to work for the spoken modality as it would for reading. Does that rule out the whole word approach to reading?

Professor Marantz: I believe that yes. What happened before 350 milliseconds? In the reading modality there have to be orthographic and phonological conversions of some sort. Our experiments manipulate the orthographic and phonological conversion for words and see which part of the response is affected by that. For something like reading, there are inconsistencies across populations. I agree with Seidenberg on many of his points regarding reading.

Professor Magnuson: You talked about activation, competition and selection as brief stages in word recognition.

Professor Marantz: First, we have evidence for two stages, the activation stages as opposed to the competition-selection stages.

Professor Magnuson: I am wondering if there is any independent evidence for a distinction between those. In no current model besides your own does anyone distinguish between activation and competition. Words are activated and at the same time they compete.

Professor Marantz: There are two stage models. The computation associated with activation is distinct from the computation you then do over activated entries from selection. I believe that our evidence is most compatible with a claim that the computation of the winner is different than the computation of the activation stage. It will be more compelling when we look at other measures. The only measure we have for decision is the behavioral response.

We should be able to identify parts of the MEG response that correspond to other parts of processing.

Professor Magnuson: I think the piece that is missing that would make this compelling is a linking hypothesis from some model of activation to the MEG response. What is actually going on during word recognition that would give you a bump at 350?

Professor Marantz: Ah, the bump! People in MEG and ERP research are always talking about components or bumps. Does the peak mean something? Now is the time to be more precise about what these ups and downs mean and to link them to computational models.

Professor Miozzo: Is there anything that would lead you to believe that the same kinds of processes are involved in production? There are theories that say that you have TEACHER represented as a whole word when you say TEACHER.

Professor Marantz: If there were evidence that in production you put in these big memorized chunks, that would contradict our hypothesis, but it would also be against the fundamental driving hypothesis of linguistics, which is that there should not be other ways to create linguistic representations. We are looking for the way it is done. Yes, that is the other way you might do it, and you can use these other strategies if you wanted to create the same representations. If you had the other strategies you would not need the beautiful one. But, in my view you need it because it is the only one.

Professor Miozzo: Do you also know that within the domain of morphology there is a compromising view, which says that there are whole word representations only for high frequency words?

Professor Marantz: There is something true about that but that is not the way that you should say it.

Professor Miozzo: Ok, how should I say it?

Professor Marantz: Take past tense for example, for regular past tense there is no predictive value for the stem given the regular past tense. In cases of extremely high frequency there is a small bit of predictive value. People make use of the information available and therefore when you hear past tense a lot with a particular stem there is a predictive value.

Professor Gasparov: I will use a German example, in a word like AUFHERN which means, "to cease." We recognize AUF which means "until" and HERN which means "to hear." The word has nothing to do with its components. Is HERN put back into AUFHERN, and have you tested such cases?

Professor Marantz: We have not tested such cases but the theoretical work I do says absolutely that HERN is put back into AUFHERN. In the view that I exposed today, it is not possible to say a stem by itself; even CAT has an affix that makes it a noun. Once you put the first affix on it that makes it a noun verb or adjective it is fixed. Anything else you do to it is going to look productive. I predict also for these irregular cases there is decomposition to the root across the different meanings.

Professor Remez: Let us thank Dr. Marantz and adjourn.

A copy of Professor Marantz's PowerPoint slideshow is available at:
[http:// web.mit.edu/marantz/Public/Columbia](http://web.mit.edu/marantz/Public/Columbia)

Place: Faculty House
Columbia University
400 West 117th Street

Time: 4:00 PM

Chair: Professor Robert E. Remez, Barnard College, Columbia University.

Attendees: Sarah Callahan, Aili Flint, Brenda Fox, Boris Gasparov, Peter Gordon, Michael Studdert-Kennedy, Robert Krauss, Jim Magnuson, Michele Miozzo, Ezequiel Morsella, Robert Remez, Kelly Remole, Ann Senghas

Rapporteur: Bridgid Finn



31 JANUARY 2002

**Transidiomatic practices:
Language and the global situation**

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In this lecture, I will assess the communicative mutations resulting from the interaction between mobile people and mobile texts. Sophisticated technologies for rapid human mobility and global communication are transforming the communicative environment of late modernity. Yet few linguistic studies encompass global phenomena, and when they do, they tend to depict the worst possible scenarios: linguistic imperialism endangered languages, language death. The experience of cultural globalization and the sociolinguistic disorder it entails cannot be understood solely through a dystopic vision of linguistic catastrophe, but demand that we also take into account the recombinant and multimodal qualities of language mixing, creolization, and technological hybridization. Using linguistic data from different sources (from international call centers to automatic translators) I call for a radical reconceptualization of most linguistic tools, including the concept of language itself.

I think its time for people involved in language to pay attention to the character of globalization. I started as an anthropologist and I saw a lack of attention to language and its importance to the study of globalization. Most of the work now done on globalization studies utilizes the metaphor "flow." People involved in cultural studies and issues of globalization talk about media and migrations as a sort of flow around the world. This is a very healthy metaphor. It is quite different from the studies of Marxist world theory that created what is now the traditional metaphor of "penetration" or "spread." There was a book published a couple of years ago called *The Global Situation* which really prompted this investigation. The author addresses the issue of the metaphorical use of flow versus penetration. Her argument is as follows: flow is a form of circulation; circulation is to globalization what penetration was to capitalism in certain neo-Marxist work system theories. Where penetration always supports a sort of rape, one people forcing their powerful interests onto another set of people, circulation calls forth images of a healthy flow of blood through the body, and a stimulating even handed exchange in the marketplace. In the neo-liberal talks of globalization we get this metaphor of flow while with neo-Marxist theorists we hear talk of spread and penetration.

The few linguists working on globalization have created another metaphorical branch called linguistic imperialism, or language death, in which

all talk revolves around linguistic endangerment. We find very little attention to the healthy, productive aspects of language. Linguistic influence of one culture on another is always perceived as a threat or a spread of a disease. Spread, which is also used in describing a linguistic influence, is another example of a negative metaphor, in this case one that is always tied to disaster and mistrust. I think the situation could be balanced more. A problem that I see is that language is always connected to a territory and to a particular people. The connection between nationalism and language is readily present in our understanding of language. We have bounded communities, and a sense of the homogeneous community. We have to problematize what we are doing here. We can no longer look at languages in isolation.

A book that I was largely influenced by is *Modernity at Large*. In this book the author is identifying a new intersection for the study of society created by the mobility of people and the mobility of the media. Mobile people and mobile text come together to create a new environment, what he calls ethnoscapes. My question after reading this book was – what kind of language do people in ethnoscapes speak? I had to start thinking outside of the form of language per se and to look first at communicative practices.

The notion of communicative practices replaced the notion of language in the 1990s. It is a concept that goes beyond linguistic form. Looking at language as a system of linguistic forms is not enough. Linguistic anthropologists are developing different ways of thinking about and combining context, lexicality and social lexicality as elements in what is called a lexical indexicality. In an index you have a relationship of contiguity between objects; a motivated relationship between the signifier and the signified. For example smoke is an index of fire, it comes from the fire. Michael Silverstein is using this idea in the linguistic community and has come up with an idea of social indexicality. We need to look at language as context related and context producing. In earlier work I have done I looked at social indexicality in address forms and pronouns. These forms are indicators of the social relationship and power relationships between people. You can also transform social relationships with the use of social programs.

[Professor Jacquemet gives an example of typical French use of social index used to address someone with respect]

We look at language as a reflection of context but context is continually negotiated. Linguistic ideology, developed in the 1980s seeks to answer the questions important for understanding the role and use of language, and the struggles behind using that language.

We need to move away from understanding language as a system to an understanding of language as a reality, language in motion and how language is involved in power and transformation of society. This is the first step. The second step asks how these communicative processes can work in this globalized, transnational world.

[Jacket catalogues his study of an Albanian family detailing the richness and complexity of the languages spoken in the family via diversity of marriages, language of current country of residence, and media influences.]

As you can note from the short history of this family, there is simultaneity of codes within multiple channels. The communicative behavior of de-territorialized groups is an interaction of different language uses. This is an occurrence within a range of communicative channels. These transidiomatic practices are found in environments that are characterized by mobile talk and use of electronic media.

International call centers are one example of the current globalization of the economy. An international call center is a new phenomenon in which office services for businesses in the United States are more cheaply provided in other parts of the world. American and British businesses have started out-sourcing services in India. There they find large numbers of English speakers who are willing to make their careers as poorly paid back office operators. These call centers are directly handling customer service. The key to this is that the operator must re-territorialize their cultural and communicative practices to match those of the location of the customer. This is to simulate for the customer that a peer in a nearby location provides the service. Operators are required to acquire a British or American accent and to learn about the popular culture of the caller. They have training sessions that include listening to the soundtracks of popular television shows going on in their callers' countries, for example, *Friends* in the United States. Managers encourage their staff to change their names to something American. To gain an intimate spatio-temporal experience of their customer, on their computer screen not only are they shown the customer's time zone but also the local weather. Each call can be answered with the correct temporal greeting, "Good afternoon. How are you?", as well as appropriate small talk, for instance, about the latest tornado that touched down in the Midwest. The motive behind the rigorous training is that companies are concerned with the potential for misunderstanding, coming from a lack of shared knowledge.

The following is an example of research done on shared knowledge. Students were played a tape-recorded lecture and shown a picture of either an Asian woman or a white woman. Some percentage of those who saw the Asian woman's picture reported that the speaker had a foreign accent, while there was no such perception among students who heard the same lecture but saw the white woman's picture. Students who saw the Asian woman's picture did worse on a comprehension test following the lecture. Perceived foreignness of the lecture made them learn less effectively. There is definitely an importance of perceived geographical proximity and communicative alignment. These are elements that when matched, encourage conversational recombination, thus guaranteeing a smooth transaction.

In the example of phone operators we can see how they are connected with the volatile world of recombination of their communicative, cultural, and social practices. This is what I would call a transidiomatic environment. Perception of shared knowledge is essential to the success of these phone operator position, in order to assure the company's customers of a smooth transaction.

Another element that I think is part of a transidiomatic environment is the interaction between communicative practices and the computer. Within this interaction we can look at languages that are not very well formed. We are

witnessing the integration structure of psycholinguistic mechanisms of linguistic change, social interaction and power relations. I think the overwhelming idea is that the Internet is an English domain. However, although eighty-percent of what is on the Internet today is in English, forty-percent of Internet users cannot use English at all. English as the global language is not a correct picture. There is a capacity for people in this environment to use their own language or use some other created language. Net users bring their own dialects to the on-line potluck. Ideally we would use mechanical translators to be able to access all the information found in other languages. However in using mechanical translators we rarely get an accurate rendition of the original message. On one hand there is often a lot of nonsense that is very disjointed. The relationship between the kind of translation and theoretical languages is an immature one.

[Professor Jacquemet gives an example of machine translation from German to English using mechanical translation programs found in New York City]

On the other hand, through this kind of translation we are still able to make some sense of it and gain some very valuable information.

[Professor Jacquemet gives correctly translated English segments from the machine translated German passage]

This is a type of gibberish English but we do get comprehensive structures. Keep in mind that we do not always speak in well formed phrases and sentences in our own well formed languages.

This brings me to my divergence with many of the ideas of those linguists who are involved in the language death debate. I do not share many of their views predicting a catastrophe happening to language around the world. Many linguists believe that globalization will lead to a severe reduction in the number of languages spoken around the world. According to recent estimates many of the languages alive today are in serious threat of disappearing. Those that are not in danger are called "safe."

In the 1990s the linguistic community responded to this dire prediction by mobilizing the preservation of language and later on to take action against language death. I think that globalization is understood by linguists as the spread of a few dominant languages into fragile linguistic areas with the predictable outcome of a loss. I think this is wrong. There is also an agency involved in changing a language. Behind any declaration of the "death of language" lies the assumption that this language had a fixed, immutable, and formal structure. The idea of the survival of language equates language with a biological species. This dystopic vision is a naive view of the relationship between culture and worldview. I think the situation between language and culture is much more complex than the idea promoting language as the "window into the human mind."

English is the major force behind evolution of new languages, both pidgin and Creole. English is by a large margin the most frequent lexifier language, that is, where people get their new words. Also, English is important in the construction of new idioms for example net pidgins. We can think more in terms of Englishes, instead of just one single English language. Another way of

looking at the entire picture is to identify the new languages that are developing. There are 507 new languages, 372 Creole and 135 mixed. Many of the Creoles and pidgins are becoming national languages. Linguists are straightjacketed by formalist notions of what constitutes a language. Importantly there are new languages in the media-migration environment. One language that has become very popular on the inter net is Europanto.

[Professor Jacquemet gives an example of Europanto.]

Europanto is used on the Internet, and within conversation on chat lines. Europanto can be understood by anyone of average learning, with a basic understanding of the English language. The structure is centrally English but borrows from other languages that have been transformed to make them easier to understand. Imagine for example a French speaker and an Italian speaker who have to communicate in English, even though their two languages are very similar. If the two were to add words from their own languages they would almost certainly find it easier to understand one another. The result would be a contaminated English, with words and forms borrowed from other languages. This is Europanto, or rather, a possible Europanto. The dominance of English cannot be challenged; it has become the universal language of our time. However, Europanto has a different goal. Rather than an artificial language, it is a system creating a new language of the future. It addresses the frustrations the vast majority of people who are forced to use English even though their command of the language is not very good.

In conclusion, I would like to argue that the fuzziness of machine translation and the transidiomatic practices of materialized speakers takes for granted commonsensical notions of what constitutes a language. First, the experience of the de-territorialization and the social linguistic disorder it details requires a serious retrocentralization of the connection between communication and shared knowledge. We can no longer assume that such shared knowledge exists to provide the common ground from which to negotiate conflict and agenda. Common ground has to be understood as a major challenge in the establishment of shared knowledge and the process of communication. Second, globalization forces us to look at the ideological process of making and controlling the boundaries of our social information.

I would like to push our understanding of language to include chaotic recombination, within a historical reference. I would like to look at transidiomatic practices by different social formations. You can imagine that the different languages interact and mutate. I think a good metaphor for the way languages come together is as a cloud or whirlpool. It does recall turbulence, but I think it is a good way to understand the different elements that the languages come together to recombine as seen in the communicative behavior of international communities.

APPLAUSE

Questions

Professor Jim Magnuson: You claim that people who are worried about language death are focused mainly on the synchronic regularization of

languages. This does not capture a lot of what certain linguists in particular are worried about, which is that it is not just historical change, it is the fact that you are not going to have access to a lot of these languages anymore through written records. We are losing an important source of data provided for by variability within languages. Moreover, from a cultural point of view, this is the death rattle of a culture in that you are going to lose access to folklore that gets passed into whatever language has taken over.

Professor Jacquemet: This is an evolving process. If you look at cultural practices, storytelling will not disappear. The argument that you can only tell stories in the old language is suspicious. Linguistic records are important but linguistic records alone are not enough. If you do not also get everything that comes with that linguistic form you are not bringing in what is variable within cultures, such as orthographies or silences. In addition to looking at the way that language has changed we need to look at the way the storytelling has been transformed within the culture.

Professor Magnuson: My real concern is the lack of data.

Professor E. Z. Rothkopf: Isn't this sort of an impossible task. You have to look at all the references of the language. It is a very difficult task to preserve a language.

Professor Jacquemet: That's right. It is freezing a particular vision of the language of a culture, even if that particular culture moves on. Yes, we have an important historical record but it is not an accurate portrayal of that culture.

Professor C. Hill: I think for linguists who have that concern, a record is for formal reasons. They are not reifying form. It is not for referential purposes so much as is for formal properties.

Professor Michele Miozzo: In tracking the major differences in language, is it possible to make a hypothesis about how different populations merge their language with the incoming language? Also, I think you are missing the point. I think we should rush to get as much information about languages as we can. My second point is that I do think that language does afford us the opportunity to get a window into the mind of a population.

Professor Jacquemet: You are making a Whorfian assumption that by simply having a word we will be able to get into the mind of the person speaking.

Professor Miozzo: I am making a more complex assumption than this. Every time I speak it is an attempt to solve a problem. The problem to communicate a thought I have. It is important scientifically to understand which processes we are using to solve this problem.

Professor Jacquemet: If you only have that record I do not think it is enough. I do not think it provides a window into anything. A collection of pottery from Native American societies sits there; we are unable to do much about it. The collection of language as if it were a species to me seems a misplaced enterprise. Has Latin disappeared, is it really gone? People do not worry about losing words, but a language is a different story. We do not do anything about it. If you do worry about losing the language, the funding is there if you cry enough to the particular fund holders.

Professor Robert Krauss: It seems to me you are speaking about different things. One has to do with the effort to get those last three Thompson speakers, and get them to teach their grandchildren to speak Thompson, which is very unlikely to happen. When they die there will be no more Thompson speakers. It is sad, but I assume that the things that supported that language are no longer relevant in the life of these people and their descendants. The other is preserving a record of the history of languages. It seems to me this practice has already proven its value. You are saying that the pottery shards are not very informative, but they are certainly better for example, than the evidence we have for the origins of English, which is the few poems we have, and looking at what words rhyme. My personal view is that there is value in a record for understanding how languages work, but also more for understanding the history of a people, and the changes that a particular language has gone through. Hopefully, it will help us to generalize the underlying foundations of language. One would have to be Kreskin to know what to preserve. It seems to follow almost trivially, that the more we have of a vanishing language, we will be better off in the future in knowing what to do with it.

Professor Jacquemet: I agree with this point. What I am responding to is the taxidermist approach. It does not work.

Robert Remez: Is there a difference between natural Creole and an artificial group language project such as Europanto?

Professor Jacquemet: I do not like to place this boundary between artificial and natural languages. We can only talk about language in the way that we understand language today. In natural languages such as Creole and pidgin, people have learned something in order to be able to communicate with each other. The same thing is happening in Europanto. Europanto is not as developed as natural Creole and pidgin, but I would not like to say that it might never arrive at that point.

Professor Remez: Let us thank Professor Jacquemet and adjourn.

Place: Faculty House
Columbia University
400 West 117th Street
Time: 4:00 PM

Chair: Professor Robert E. Remez, Barnard College, Columbia University.

Attendees: Aili Flint, Massimo Girelli, Arma Hahn, C. Hill, Mizuho Iinuma, Robert Krauss, Jim Magnuson, Michele Miozzo, Ezequiel Morsella, Katherine Nelson, Jennifer Pardo, Robert Remez, E.Z. Rothkopf, Ann Senghas, Andrew Sunshine

Rapporteur: Bridgid Finn



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Language and coordination

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People use language to coordinate on many of their joint actions—what they do together. But they cannot use language itself without coordinating with each other—on attending to, identifying, understanding, and taking up each other's utterances. The central issue is coordination of action—what it requires and how it is accomplished. I will consider evidence on two issues: the role of language in coordinating action, and the role of coordination in using language.

I want to give you a point of view about how to think about language and language use, such that it will allow you ways of thinking about what you do in the realm of language. I want to start with a distinction between what I call autonomous actions, for example, shaking a stick, playing a piano solo, doing a pirouette, moving a chair, or thinking about a problem, with a kind of action I call a joint action something that two people do together, for example shaking hands, or dancing a waltz or moving a table. I want to argue that language use is one of these joint things. You cannot do it by yourself. Life is filled with these joint activities, playing bridge, getting numbers from directory inquiries, playing tennis, etc. These are well defined. There are others that are ill defined like, for example, getting acquainted, gossiping, or driving in a city. These are joint activities that I want to take or assume to be part of the things included in language.

I would like to make three claims about these joint activities. The first is that people cannot engage in a joint activity without coordinating. We cannot conduct the activity without communication. Furthermore, it is the joint activity that comes first. Too often we think of language as something that is just generated. I want to argue that communication derives from the fact that you are trying to carry out this joint activity. Finally, language itself is a joint activity.

[Professor Clark shows a picture of two people moving a table.]

First I am going to talk about how we use language in joint activities, how we coordinate our language to carry out these activities. A simple example is moving a table. Ann stands at one end, Bernie stands at the other. The two of them together pick up the table and then they simultaneously move the table to somewhere else, and then they set it down and then they move around. These are five simultaneous parallel actions. Is this all you are doing when you

are moving a table? No, you have to figure out how to coordinate these five things that you are doing and the way you do it is through language.

[Professor Clark gives an example of a possible conversation between Ann and Bernie in order to move the table.]

During a basic joint activity like moving a table, there is the moving of the table proper and in addition there are the acts that promote the coordination between Ann and Bernie moving this table. It consists of pairs of parallel action, which is the main part of what you are doing. But it is done via a set of things I am going to call a coordinating joint activity. It is there for coordinating the basic actions. It is a series of communicative acts, typically consisting of serial actions. The partitioning principle I want to start with begins with a joint activity of any kind, reduced to what you are really doing. For example if you asked Ann and Bernie what they were really doing they would say, "Moving a table." But, if you asked them if that was all they were doing, for example if they were also having a conversation, they would say that they were, but that it was secondary to moving the table. I want to say that joint activity always divides into these two parts. All communication is in the service of joint activities; this is the radical claim. You are not going to see communication unless there is joint activity. Now, there are some edges to this. Some examples of joint activities are copiloting an airplane, gossiping, but there are other kinds of joint activity that use a different kind of signal, for example a duet on a piano where communication is done via non linguistic ways. Waltzing and playing chess are some examples. People coordinate these joint activities. There are parallel actions during the basic joint activity. Ann and Bernie do something simultaneous, but initially they have to agree to do the activity. There is an agreement to do the activity in preparation of doing it. We do this through a set of serial actions. Traditionally, this has been called *adjacency pairs* in conversational models. Adjacency pairs usually consist of a request followed by an answer. Adjacency pairs are problematic however in excluding anything that is not language.

The premise is that basic actions require chronological states and joint actions are largely serial. We carry out these basic activities by getting the joint members to go along. It may be short or long term. There is a transition into the joint activity, we go through the body of the joint activity and it terminates. It takes real coordination to do 'this' first and to do 'that' next. What I want to point out is that joint activities in general consist of pairs. Every activity we engage in will have an entry and an exit point; there are also entries for each of the intermittent parts. This is an old observation of the hierarchical character of joint activities. The problem is that dialogue is linear. We establish these hierarchies: that we will go from one part of the hierarchy to the next via these projected pairs. Again using the example of moving the table, there is the entry, there is the moving of the table proper and an exit. Notice how you do each of those exit and entry points through language use. Ann says, "Can you help me move a table?" Bernie says, "Sure." Ann, "Take that end." Bernie, "Ok and lift." Things happen, and then they are done. And now there is the exit from this activity in which Ann says "Thanks for your help." and Bernie says "No problem." Each one of their pairs is a projective pair of

action. This is the way joint activity goes in general. To give you an example I will tell you about a piece of research that Shaffer and I did a number of years ago on telephone calls to directory inquiry in Cambridge, England. I am going to give you one example that shows these same properties.

How do you enter this exchange? The caller rings the number. The operator says "Directory Inquiry." How do you proceed? You are going to exchange address and telephone numbers. The caller gives the town, then the name of the person, and then they get the number. Finally you thank the person and you get out of the exchange by hanging up. Regardless of whether we are moving a table, where it is an entirely physical move, or just passing information from one to another, we are coordinating.

[Professor Clark shows a printed version of the earlier dialogue between Ann and Bernie.]

I want to take up a particular issue here that starts with some observations made by a woman named Gail Jefferson. The question is about "ok," which we see done in some conversations, but never in others. What is its use? Gail Jefferson said "mhmm" when seen in conversations is an exhibit of what she called passive reciprocity, which means "I am not taking the floor." Whereas "yeah" says, "I am ready to take the floor." She gives evidence that is consistent with this. But what is "ok"? I am going to give you a well-defined joint activity. There are two people at two ends of a table, each with their own set of Lego blocks. There is a director at one end of the table with a prototype block of Lego blocks put together a certain way. The prototype is hidden from the builder, otherwise they could see everything on the table. This is a modernized version of a classic task. The director has to tell the builder how to put the blocks together to look like his.

[Professor Clark shows a video of subjects performing and conversing about the task.]

If you break this down it consists of a set of cycles. The director says how the builder should put the blocks together, the builder says "mhmm." The director says what to do next; the builder says "mhmm." The director states the final action for putting one section of blocks together and the builder replies "ok." This is a cycle, in the first sense in identifying the object, now we go onto the next block. This is a nice task because it is so well defined. I want to point out that the builder says, "mhmm," and "ok." If we look in great detail and hierarchically at what is going on – we see that when one level is complete you say "ok." If we look at the data, 80% of "mhmm"s are done in the middle of a cycle. Sixty percent of the "ok"s are at the end as a signal to go on. "Ok" is completely a vertical marker, "mhmm"s are horizontal. They are telling you how to move through the hierarchy of the task that you are engaged in. When there is a clear well-defined task, we often see these "ok"s. In ordinary conversation we see them almost never.

What does it mean to be coordinated? Can you exchange the information needed all at once? No, there has to be a transition into the thing that we actually want to do and this is done incrementally.

[Professor Clark gives an example of one secretary calling another to obtain an address.]

There is an establishment of roles, a justification of an inquiry and the actually inquiry whereupon the request starts to be carried out. Joint projective pairs establish these increments. Each of them updates the common ground. They establish the conditions for the next joint action. They can be declined at any point, by saying "No." What is interesting about this kind of introduction is that it is a brief and very efficient way of getting all of these things established, in addition to being opportunistic.

The navigation principle states that within basic joint activities emerge hierarchical joint projects. There are joint things that you do in which one person projects what the two of them ought to do and the other person picks it up. Speaking is linear in time, so that the way that they do this is incrementally through projective pairs. These discourse markers are deliberately added to show where you are in the hierarchy of the joint project. They markers do not serve to tell you where you are in the conversation. This is a kind of social psychology problem that social psychologists ignore. On the language side they do not ask what the language is in service of.

How do people coordinate on language? This is the second half of my talk. What I have talked about so far is the idea that there is a basic joint activity, for instance, moving a table. I am going to call that "track 1." There is a dialogue for coordinating that basic joint activity. But moreover, using language itself is a basic joint activity, in talking with someone we have got to do an enormous amount of coordinating. There are signals for coordinating a dialogue itself, this is what I call "track 2." This is recursive.

I want to start with a basic example to start talking about spontaneous conversation.

[Professor Clark gives an example of a speaker's utterance, "Well Mallet said he felt it would be a good thing if Oscar went." The actual utterance by the speakers is filled with "uh," "well," "I mean," "ah."]

To me what was interesting is that they had no trouble understanding that the coherent sentence is what he produced, the target is understood. I am going to call all the additional junk *performance additions*. These are things that you put in while you are performing these utterances. The question is why are you doing this, why not just say it, or wait until you can say it? What do these performance additions do? Many of them deal with change of direction, some act as replacements, or clarifications, or repeats. There are also delays and silences. Spontaneous conversation has other complexities, for example, when one person completes another's sentence. He does it for the particular reason: to get confirmation. You can get strategic interruptions. What are these for? There is a view among psycholinguistics that these are dysfluencies, these are mistakes, mere performance phenomenon. My perspective is that they are done deliberately in the form of footnotes, annotations. They are mostly planned. Of course, speakers have problems, but what you are seeing is not the problem, but the repair of the problem.

I want to talk about two different types of dialogue. Primary actions are things that I am really doing in language, the thing I am attempting to accomplish. There are other things that have more to do with managing the

dialogue, namely, collateral actions. If I say, "It is cold out, Bob," I could mean, "Shame on you for inviting me this time of year." Finally, I am proposing some kind of joint project for the two of us and he must consider that.

This is called *grounding*. In a joint action, we have to reach the shared belief that we have succeeded on that action. There are levels of grounding. There is one channel level where I have to make sure that my conversational partner is paying attention to me. One way I do that is by looking and making sure. We acknowledge that he is paying attention and that he has understood my utterance and that he is considering my proposal. These are parallel actions. He is attending while I am speaking. The way we do this is through the exploitation of serializers. I display a signal and he gives me evidence, through a reaction. Let me give you some examples of how this works.

[Professor Clark gives example sentences: Bernie says, "How was the wedding?" Ann says, "It was really good, it was a lovely day." Bernie says, "Yes."]

Has Bernie understood this? Bernie says "Yes." Does it mean yes, have understood that it was a lovely day? What this means collaterally is that Ann is asking "Really, do you understand that it was a lovely day?" He says explicitly "Yes", which is a response to Ann's implicit question. You can also get grounding by what I call *next contribution*. The collateral signals here are, "Do you understand my utterance?" and the answer is, "Yes," because the utterance is accepted and Bernie moves on. Another example is grounding by side sequence. "Can I speak to Jim Johnstone please?" The reply is "Senior?" This means he understood everything but which of two Jim Johnstones it was.

I want to take up a set of issues that have to do with timing. Timing is crucial because it is something that you have to coordinate. You need to make sure your listener is attending exactly when you are speaking. If you talk to psycholinguists, they consider pauses or hesitations as an internal timing problem. I want to argue that these are really problems of cross timing.

[Professor Clark shows examples of timing of block building with subjects in the Lego blocks experiment. One is speaking while other is performing a task. One pauses until the builder exhibits the block that he has just told him to use. It is only after the builder exhibits the block that the director continues.]

This is another example of the director asking if what he has said has been understood, and the builder replying, "Yes," by exhibiting his block.

[Professor Clark gives a description of builder holding up his completed set to the director. The director says, "yeah," to confirm that the construction is correct. He continues with another example of a director interrupting himself to say, "yeah," when a builder has completed a section.]

There are many signals for grounding that rely crucially on timing, such as, poisoning or exhibiting, and explicit words like, "yes" and "no."

The final issue I am going to talk about deals with research on the low level of speech coordination. I talked about the synchronization problem before, but it is really this. Bob must be ready to attend to my sounds and gestures, and he must to be ready to parse and identify my words. The readiness principle is an

old principle from 1938, with the first research dating back to the 1890s. It states: If possible speak with ideal delivery by trying to produce what is expected when it is expected and try to produce constituents fluently. In addition, if possible, warn addressees about departures from the ideal delivery. Bob will understand something better if he knows what and when it is expected to come. This means that you usually try to produce what you say fluidly because you know exactly when it is supposed to come. If you can not it is really good to tell your addressee that you can not.

[Professor Clark reads a sentence that has a disruption that suspends speech followed by a hiatus.]

When the delay occurs this subject says, "uh." This is a signal meaning, "I am suspending speech for a minor delay;" "u:h" for a major delay; "um" for, "I am proceeding at a normal pace;" "u:m" to signify that I am proceeding at a delay.

Another example for suspension of speech is the word "the." When we choose to pronounce the word "the" as "thiy," this use says, "I am delaying, I am suspending speech right now." If "thuh" is used, it means, "I intend to continue." Unreduced vowels are used to suspend speech.

[Professor Clark gives an example of Al Gore using "thiy" and "two- uh" for "to" when he is not ready to continue immediately to the next word.]

Dr. Sasha Blair Goldensohn: Is your corpus American English only or are you making this claim for all Englishes?

Professor Clark: All Englishes. We get the same things in American speech as in British English.

The last thing I want to take up is word repetition. This is common in ordinary speech. This has four stages. You make an initial commitment with a repeated word. You suspend your speech, you deal with a hiatus by saying "uh," and then you go on to start entire clause again. You find people repeating when their constituents are more complex. If it is the beginning of the clause you are more likely to do it than if it is at the end. If it is the middle you are more likely to repeat if it is a complex noun phrase than if it is a simple noun phrase. It is good for me to tell my listener what kind of constituent I am about to produce, even if I can not produce it yet. The first word tells you it is a clause, the repeated word tells of the delay and then we begin the whole clause over again. These are called preliminary commitments to the clause. We have a large number of ways in which to deal with timing, the ways of getting the listener to synchronize their actions with mine such that we can communicate.

What I am trying argue is that we are involved in joint activities all the time. It takes communication to coordinate those actions. The way we coordinate is through these intricate devices. If you just look at the communication itself there is the official business or primary signals in addition to the collateral signals, in order to collaborate on those primary signals. I try for an ideal delivery, but when I can not I stutter my speech with these delays.

APPLAUSE

Questions

Professor Robert Krauss: I would like to talk about the status of these signals. I would like to bring up the distinction your namesake, H. Paul Grice, has made regarding natural and non-natural meaning. What really is the status of these signals? When people do things we can infer a lot about internal states simply by observation. Is your “um” put there to tell me that you are going to take a while to produce the next word, or have I learned from experience that this is a sign people make when they do not know what is to come next? The second question asks if there is any evidence at all that listeners attend to the “ums?”

Professor Clark: Christenfeld had some data showing that if you delete all the “ums” and “uhs” people make different judgements of a speaker. There are also more recent studies by G. E. Foxtree. To address your first question, are these signals or are they symptoms of problems? The symptom view would say these things just happen when I am uncertain of what I am going to say. But, it does not explain the English problem of contrasting “ums.” They are treated as words by the English prosody and phonology. For example Gore did not just say, “to regulate” he said, “two-uh regulate.” That says that the “uh” and the “two” have been combined in prosody as a single phonological word. There is a resyllabification of the word. This is an example of the vowel of “two” mapping on to “um” resulting in “two-uh.” There is some qualitative evidence, not experimental at this point, of people attending to these forms. I have an article coming out in *Cognition* that will tell you more than you ever want to know about this.

Professor Herb Terrace: Everyone argues that language is uniquely human, but does that include coordinating activities?

Professor Clark: Clearly chimps and other beasts coordinate with each other and use signals of various sorts. They do not, however, belong to a system like English for example with all its conventionalities. In play they use certain signals to show that they are in play and not serious.

Professor Terrace: So in that sense you separate language from these coordinating activities.

Professor Clark: Well language itself is something that you have to coordinate.

Professor Ann Senghas: Do you think that non-human animal coordination involves an internal hierarchy?

Professor Clark: If you and I go to a movie we know, I must get in the car, I must drive to the cinema. We have a notion of the hierarchical parts of the activity. I have no idea what is going on in animals.

Professor Sam Glucksberg: One possibility is that the “uh” is an unfinished “um.” In other languages you get this same kind of structure.

Professor Clark: Most languages have pairs like “uh” and “um.” The length of the syllable and whether you choose “uh” or “um” is an independent choice.

"Um" is a heavy form of "uh." It is used for a heavy purpose; namely, when I am expecting a long delay.

Professor James Magnuson: Would you distinguish between the signaling hypothesis, where the speaker is having trouble, as opposed to the case where the speaker is able to retrieve the word "the" but not the following word.

Professor Clark: The question is why someone is saying "the" to begin with. I am saying it because I can not come up with the word "book" in the noun phrase "the book." I have to plan some utterance up to the word "the." I have choices. I use a pause between "the" and "book" or I can use what I cause an ad hoc intonation rule. I am going to put ends on "the," I plan it as a unit. I now complete the sentence with the word book. I can not start planning "thuh" and say "thiy." When do you do this? You do this at the point your processor knows that there is going to be a suspension at that point. You do not have to be conscious of any of this.

Mr. Ezequiel Morsella: Do you produce these dysfluencies when you are alone?

Professor Herb Clark: These are things I have to suppress. I am thinking about an addressee even if I do not have one there with me.

Dr. Sarah Callahan: You could look at answering machine messages to see if people do this.

Professor Clark: You are right. I have not looked at that.

Dr. Jennifer Pardo: Most of what you are talking about seems to reflect a cooperative system, in which individuals are trying to establish a goal. What if they are engaging in dialogue that does not have a shared goal. Are these principles global enough to contain non-cooperation?

Professor Clark: We can not have any argument unless we are adhering to the principles of English. I have to coordinate just to make something clear to you that something is part of my argument. There are certain principles that we have to combine on.

Dr. Pardo: I am really trying to drive at the individual goals here.

Professor Clark: Private goals that I have I make clear to you in order to drive the conversational choices that we make. I would not say they are primary business.

Professor Michele Miozzo: What are the effects of a speaker misplacing "yes" and "ok?"

Professor Clark: These words are not intersubstituted. They all mean something different and can all occur within the same individual.

Professor Remez: Let us thank Professor Clark and adjourn.

Place: Faculty House
Columbia University
400 West 117th Street
Time: 4:00 PM

Chair: Professor Robert E. Remez, Barnard College, Columbia University.

Attendees: Patricia Belkiu, Sarah Callahan, Inge-Marie Eigsti, Sam Glucksberg, Sasha Blair Goldensohn, Peter Gordon, Michelle Gumbrecht, Elizabeth Henly, Clifford Hill, Hsin Yi Huang, Kathleen Ianacone, JoAnn Kleifgen, Robert Krauss, Trang Le, Jim Magnuson, Bruce McCandliss, Janet Metcalfe, Michele Miozzo, Ezequiel Morsella, Jennifer Pardo, Lois Putnam, John H. Saxman, Ann Senghas, Michael Torpey, Pei Ju Tsai, Herb Terrace, Harrison White

Rapporteur: Bridgid Finn



4 APRIL 2002

**Spatial representation as specialized systems:
Evidence from preservation and breakdown**

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Williams Syndrome is a rare genetic defect that causes profound spatial impairment together with relatively spared language. The unusual nature of this cognitive profile allows us to examine whether spatial representation is a single all-purpose system, or a set of specialized systems. I will report evidence showing selective sparing of object recognition, perception of biological motion, navigation, and some aspects of spatial language, along with fragility and breakdown in other aspects of spatial representation. As a whole, the evidence suggests that distinct spatial capacities can be selectively targeted by this genetic defect, and that severe breakdown in certain tasks is the result of complex interactions between fragile representations and procedures that operate on them.

The general question that I am going to ask today is whether or not there is specialization in spatial representation, and if there is, whether or not we can see this specialization in its developmental origins. Is there evidence of specialized spatial representations that we start out with or are these only the result of long-term development? There are many different spatial functions that we use in order to negotiate the world: we perceive objects, we reach for and act on objects, we move and navigate through the world. In addition, we can talk about space. Talking about space requires these representations and also a linguistic system that maps onto these representations. We typically accomplish this in a seamless, effortless fashion. We do not have to think about translating between these systems. There is increasing evidence from a variety of areas that these systems of spatial representation may be quite specialized and different. One conceptual argument refers to the different kinds of computations required by these systems. In order to perceive objects we need one highly detailed representation that allows us to perceive the same object from a variety of different viewpoints. A navigation system requires mechanisms to compute spatial maps. There is also evidence from neurological impairment in adults that suggests that these systems of spatial representation can break down from different kinds of brain damage. In the animal literature single and multiple cell recording shows that different kinds of spatial representations can be localized within the brain. There is converging evidence for this specialization.

The question I have in mind is about the origins of this specialization. There are two contrasting viewpoints that have been discussed in the literature. One is that we start with these specialized systems and use them to guide development; the other is that specialization as manifest in neurological impairment or in mature animals is actually a consequence of computational demands and also from practice in the world established in long term development.

I have been working on a set of problems for about six years using evidence from Williams Syndrome, which is an unusual genetic defect. I will start out by telling you about Williams Syndrome and why I think it may be pertinent to these issues.

[Professor Landau shows a drawing of an elephant with a description by an 18 year old individual with Williams Syndrome. The individual has an IQ of 49. The drawing does not closely resemble an elephant, in that it does not preserve any of the spatial relationships, or coherence. The description however is highly articulate, containing many details about elephant characteristics.]

Observations of this syndrome called attention to the possible dissociation between spatial cognition and language. The claim was that this may be a developmental dissociation caused by a genetic defect in which spatial cognition was severely impaired but language, including spatial language, is spared.

[Professor Landau shows a picture of a 9 year old individual with Williams Syndrome.]

Individuals with Williams Syndrome have a particular facial characteristic, analogous to Down Syndrome in which there is a set of recognizable features. In consequence, you can recognize a Williams Syndrome individual fairly easily after you learn the characteristics. It is a genetic defect. It is associated with a micro deletion on the long arm of chromosome seven. People are working on the genes contained in this sequence. Right now, we know this strand definitely contains the gene for elastin. Elastin makes skin and organs elastic. Also implicated is *LimK1*, which is thought to be associated with spatial cognitive deficit. *LimK1* is expressed very early in development of the brain, while elastin is not. Researchers are hot on the trail of the other genes contained in this region, but it looks like its not more than twenty, so it is a very small deletion.

It is diagnosed typically by a combination of phenotypic characteristics which include the facial profile and other medical facts. The phenotypic characteristics include the facial morphology, small bodily stature, and numerous defects of the organs, especially the heart. This is most likely due to the absence of elastin. There have been some passive MRIs done that show reduced brain volume overall, typical of retarded individuals, but there does not look like there is any dysmorphology in the brain. Individuals are mild to moderately mentally retarded which is to say that the IQ is about 65. Most important to cognitive scientists is that they have a highly unusual cognitive profile. There is a profound selective spatial deficit. We are eager to find out just how selective this is, if it is indeed selective, concurrent with relatively spared language.

[Professor Landau shows a drawing of a circle with each quarter colored differently. Next to this drawing is the Williams Syndrome child's replication that does not retain any of the spatial characteristics of the original.]

It is obvious that none of the spatial characteristics are retained except the local characteristics.

[For example, the child replicates only the texture of one of the colored quarters.]

When we conduct this assessment we offer the children their choice of crayons and markers. They have no problem in choosing the right color, though they clearly do not put the components together. This drawing and copying deficit is one of the main symptoms of Williams Syndrome. It does not show up with tracing tasks, so we know it is not simply a motor problem. Block construction tasks are often used to diagnose the spatial cognitive deficit. Williams Syndrome individuals are terrible at this. The spatial deficit is not linked to low-level visual defects, and there is no evidence within very simple tests for visual neglect. Despite this evidence of impairment, certain spatial components are spared as is language. These kids are highly verbal, very conversational. When we play tapes of these children for naïve observers it is very hard for them to recognize that these are individuals with an IQ of about 60. They have very strong vocabulary skills. There is controversial evidence for spared grammaticality. There seems to be spared grammatical judgments, and aspects of morphology are spared.

[Professor Landau shows another example of copy drawing task comparing normal control matched for mental age.]

The deficit also shows up in block assembly tasks. We give the individual a block pattern and ask them to replicate the pattern.

[Professor Landau shows an example of a replicated block pattern by a Williams Syndrome individual. The pattern is disjointed and does not resemble the target pattern.]

Even if they can pick the correct component blocks they cannot assemble them. The phenomenon is very robust.

Does the spatial deficit affect all spatial representations? If a global deficit affects all spatial representations, then spatial representations themselves are not specialized, that they are domain general. A genetic deficit or any other developmental impairment should show repercussions throughout the system. This contrasts with the idea of a specialized breakdown, in which distinct spatial representations are targets of genetic deficits. These alternatives are embedded within larger developmental questions asking if learning is due to domain general or specialized learning processes. Is there global breakdown or are some systems spared? I am looking within the different domains of spatial representation to see if there is a global breakdown within each domain or if spatial representations are spared within certain, but not all domains.

I am going to talk about three different domains: Object representation, perception of biological motion, and spatial language, particularly the language of motion events. These domains have very different computational requirements. They are domains that we think are subserved by specific

functional areas of the brain, that is, object representation is thought to be localized differently than processing of motion. Processing of biological motion is thought to occur in a different region of the brain than other motion processing, such as motion coherence.

Can we talk about the sparing of domains and also talk about general mechanisms breaking down? We have a contrast between perception and action. These are not knowledge domains but they are different mechanisms that can show differential breakdown. Other mechanisms include spatial language, visual spatial memory and attentive object tracking. I am hoping to convince you that there is some sparing within these complex domains.

[Professor Landau shows a slide describing the subject population: ages (range 7-15 years old), Williams Syndrome children and controls; these are normally developing children with same mental ages. Next, a graph of composite IQ scores for Williams Syndrome children, Williams Syndrome adults and control children. The control children have the same verbal score and matrices score. A matrices test is a non-vocabulary test of reasoning or logic.]

We have done a series of studies investigating object representation. The problem we are examining in this series is the ability to recognize objects viewed from the side and from the top.

[Professor Landau shows a picture of a carrot standing vertically viewed from the side and a carrot viewed from the top.]

This is a specific instance of a general problem, that object recognition has to engage multiple viewpoints. Based on adult, neuropsychological studies and animal studies we believe this is localized to the ventral stream in the brain, particularly in the inferior temporal cortex. Naming these objects then requires a connection to the language areas. I should mention that in this case it is not only a different view but it is a highly unusual view. There is some evidence that suggests that identification of objects from unusual views may be a process of the dorsal stream. This is speculative at the moment but it is of particular interest for Williams Syndrome individuals because if they have spatial breakdown in the dorsal stream for representing spatial location. If they can represent objects from these unusual viewpoints it would be quite striking.

In the first experiment we presented a set of 80 common objects presented in 4 view conditions. The first view is a canonical view with a clear image, canonical view with blurred image, non canonical with a clear image and non canonical with a blurred image. People can name the non-canonical objects but they get many wrong. We want to see how the Williams Syndrome individuals do in comparison. They are asked to name the depicted object after a presentation of 500 ms. The task is not trivial.

[Professor Landau shows the four different views of a blender, a cup, a bottle and a stool.]

The unusual condition is the view from underneath. We code the responses as correct for naming the object, as related for naming a pumpkin "a plum," superordinate for naming a desk "furniture," and similar shape for naming a

hairdryer a gun. The perspective view does affect naming an object but it does not differentially affect individuals with Williams Syndrome.

[Professor Landau shows a graph of naming performance. There are no reliable differences for Williams Syndrome individuals and their mental age matches. Canonical views are better than non-canonical, clear views is better than blurred for all groups. Next, she shows a graph of the distribution of responses in each rating category; related, superordinate and similar shape. They are distributed evenly among the different rating categories.]

Perception of objects and the ability to name them after a brief presentation, whether from unusual or canonical views, is intact in Williams Syndrome individuals. You might wonder if we remove surface cues. It would be easy to identify some of the objects, for example, a carrot, by its color. We removed the surface cues and presented a line drawing version of the task. We found basically the same results as the color version. There is an advantage for the color/surface cues version but it is small and the same across all groups.

Object recognition in these subjects appears to be preserved. The objects are recognized under canonical and unusual viewpoints. The unusual views are important because they are not normally encountered and presumably there exists a representation of the object that is transformable cognitively. It may be a solid representation, or it could be multiple plane representations.

We are following up with studies investigating whether the Williams Syndrome individuals can represent spatial structure in objects. We are presenting pictures of novel objects concocted from parts of other objects and we are testing perception and memory to see whether these subjects can retain the spatial structure of these novel objects. Can they recognize exactly the same configuration presented for 5 seconds after a delay, or do they confuse it confused with another configuration made up of the same parts?

Professor E. Z. Rothkopf: Are the canonical and non-canonical views of the object presented in the same set?

Professor Landau: No. Every Williams Syndrome child gets a different set of 80 objects which is the same given to the matched control.

One thing that we have noticed in the new studies is that there is a problem representing direction or right and left asymmetries. It is hard for everyone but it may be even more difficult to represent in memory for Williams Syndrome individuals.

[Professor Landau shows a moving image of a person represented by dots of light at the joints.]

The second case study is biological motion perception. This is an unmistakable percept of a person doing some action as designated by the motion of point lights distributed over the surface of the body. When they move you immediately perceive it as a person rather than a set of random dots.

This is an area that we investigated for several reasons. For one, perception of biological motion requires the integration of global motion from the local point lights that are moving. It also requires the derivation of an impression of form from motion. There is a small amount of evidence that Williams

Syndrome individuals may be impaired in certain kinds of motion perception. Biological motion is highly specialized motion perception that would be a strong test of whether they can perceive and integrate global motion and forms.

This kind of motion is supposed to be localized to an area of the brain that is a neighboring region to areas thought to be involved in social cognition, the superior temporal sulcus. It is well known that these individuals are highly sociable and very sensitive to social cues. So we thought it was possible that if the area in general was preserved that they might be able to carry out perception of biological motion.

We wanted to drive down performance in order to see if Williams Syndrome broke down along the same lines as normal adults and children. We created stimuli that varied in two different ways. One varied the signal to noise ratio by adding noise dots to the display of dots on the point light walker. The other condition is more complicated with three noise dots for every signal dot. One dimension that we varied was the amount of noise. The second dimension we varied was the kind of noise. The first case is static, second is random and third is yoked.

[Professor Landau shows first a group of still dots after which a point light walker movement emerges from the moving dots. There is a 1-1 signal to noise ratio. Half of the dots remain still.]

You see the point light walker moving and the noise dots stay still. The task is to decide what direction the walker is moving. This is the simplest case.

The second case involves a 1-1 signal to noise ratio, but in this case the noise consists of randomly placed dots move along a random trajectory.

[Professor Landau shows an a example similar to the previous clip with the addition of random movement from the noise dots.]

The third level is the most difficult. There are three noise dots for every signal dot and the motion of each noise dot is yoked to a walker dot. Perceptually, we are susceptible to missing the walker by establishing illusory pairs of dots. Despite this disposition among the normal observers, the Williams Syndrome subjects are very good at this task. I think we are justified in saying that there is preserved capacity to perceive biological motion which requires integration.

[Professor Landau shows a graph of judgments of walker's direction broken down by 1-1 and 1-3 signal to noise ratio. Everyone is at ceiling for the static noise. Adults and Williams Syndrome subjects are at ceiling for random noise. Williams Syndrome subjects perform better than the mental age matches. In the 1-3 signal to noise everyone breaks down. Williams Syndrome perform more poorly than the adults but continue to perform better than their mental age matches.]

We are also examining motion coherence in which the individual has to make a judgment about which direction most of the dots are traveling. Williams Syndrome are acute at picking up this type of motion.

I would like to move to the last section on spatial language and the case of motion events. Motion events draw on one kind of spatial language. What I mean by spatial language of motion events is the ability to describe what happens when an object moves through space and ends up at some point. Spatial language engages the ability to represent motion events. By perceiving the event, this impression is projected to individual components of language. Language is a highly specialized system and is very selective about the components to encode in the motion events. Language tends not to encode exact distances, or exact rates but instead gives prominence to the objects involved in the scenario, how they moved, where they started and where they ended.

Williams Syndrome individuals exhibit relatively spared language concurrent with deficits in spatial representation, as we saw from the earlier drawing examples. The obvious intersection is spatial language. Is their spatial language spared along with the rest of the language system or does it fall apart the way that much of the spatial cognitive representation system has?

The following is a standard cognitive and linguistic analysis. I will start with two examples: "The girl jumped into the pool," and "The log fell off of the swing." There are a number of components of the sentence. One is the object or noun phrase that undergoes motion, "the log," the motion or the verb, including the manner in which it moved "fell off," the third is the preposition phrase, including the path, "off the" and the reference object, "swing." It is reasonable to assume that if there is any breakdown in spatial language it should be in the path expression, because it is the most spatial component of the event. By hypothesis we are looking to see which elements are preserved and looking to see if the path is among them.

Language used to express the path can be complex. There seem to be three different path types that English encodes. The first is the *goal* path in which the object moves and ends up at some goal. In the *source* path an object starts out at a goal and then leaves it. The third is the *via* path in which one object moves past another. It is important to encode what kinds of path are in the event because there are different kinds of propositions that must be used for these different paths. For goal path we use goal prepositions such as "in," "to" or "onto;" source: "from," "out," and "off;" and via: "past," "over," "around" and "through."

Another type of complexity is the selectiveness of the path type of the reference object. If something is a container we can say "into" but not "onto." The person learning spatial language must learn the distinction among paths and also the distinction of cross-reference objects.

[Professor Landau shows a picture of a mug and describes the prepositions used to denote motion associated with mugs.]

When Williams Syndrome individuals are shown events, which elements of the larger event do they preserve?

[Professor Landau shows events expressing the path of a moving object.]

When adults see these events they include the reference object. Williams Syndrome subjects and their mental age matches drop the reference object.

They say the girl jumped or the log fell. This is grammatical, but it is not as complete. This only occurs under certain circumstances.

[Professor Landau shows graph of 12 most common verbs used in descriptions. The percentages are the same for Williams Syndrome and mental age matches showing they are appropriately using them to encode the motion.]

In path expression Williams Syndrome show a deficit. Williams Syndrome subjects and mental age matches have exactly the same verbal scores on the IQ test. For the source adult performance is at ceiling, controls included "off" less frequently and Williams Syndrome subjects are doing the worst, saying, "The log fell," rather than, "The log fell off the swing." The same is found for via paths. These are omissions.

[Professor Landau distributes out a list of all the prepositions used by the Williams Syndrome children, showing that the list of prepositions that they use is large.]

While the sentences that Williams Syndrome individuals produced are not ungrammatical, they omitted reference objects. This is the only difference we could find in comparison, but it is very persistent. They included the figure object, they included the motion, but the vulnerable aspect is the path expression. They dropped them more in the source expression than the goal expression. They described correctly a goal event in which something goes into something. But, they were likely to drop the path in an event with a source expression. A student in my lab looking to see if this is specific to motion events or if it ranges across different domains that use spatial knowledge.

Spatial language is a foundation for other aspects of semantic fields such as changes of state, for example, "X turned from red to green," or, you could just say, "X turned green." Transfer events for example include sentences like, "Mary took the keys from John," or, "Mary received the keys from John." You can make it goal verbs or source verbs. This phenomenon of dropping sources occurs across domains with these kids. The explanation I think pertains to their non-linguistic representation of events. Nothing I have told you about is a deficit in the language. If you are viewing an event and something is moving from A to B then your attention is following that object to B. The goal is much more salient and therefore the source gets dropped. We are planning to test this with a change detection task in which we provide events and in the second event we change the source or the goal and see whether Williams Syndrome compared to normals have more difficulty in remembering the source. It is not a language problem. It is my hunch that it we will observe a problem with the non-linguistic representation of events, a problem that is perhaps true of all event representation.

APPLAUSE

Questions

Professor Lila Braine: Can the children do a matching task?

Professor Landau: Yes, that is what we are doing now. The task involves presenting an object and three foils. They are very good at matching their object to the target. When you give them a one second delay, however,

performance deteriorates in very specific ways. For example, if you give them an asymmetric block, red left, green right, they will make an error on the reverse foil. Coding of left-right is very vulnerable in memory.

Professor Julian Hochberg: I would like to ask about the difference between source and goal. Source is more vulnerable to reaction time. After the motion is started it may be that they were not paying attention to where it started.

Professor Landau: So you are saying that it is a memory problem.

Professor Hochberg: No, it is an attention problem; they may not have looked at it.

Professor Landau: Yes, they do look at it. Here is the evidence: In the new experiment that we are doing all of the events have both sources and goals. They do mention the source, but not often. There are cases in which they do not notice it, but is this because it occurred first?

Professor Hochberg: Yes.

Professor Landau: Then why do you get a U-shaped function when you use a list of verbal learning items?

Professor Hochberg: I am saying that this is a deficiency which is not source specific but perhaps time specific.

Professor Landau: I might agree with you.

Professor Herb Terrace: How does the time explanation apply to the first drawing of study?

Professor Landau: Block construction is a better example to use. Like the picture for drawing, the model is present. You have to look at the blocks, segment them and decide which block you want. You have to hold it in memory while you match and hold location in memory while you place. You have to hold that location in memory while you place. With the drawing it is the same process. It is misleading to say that the model is there the whole time. It gives the impression that it does not involve load on memory but it is a very memory intensive task.

Professor Terrace: They would perform much better with simple models.

Cynthia Yang: Do they have awareness of the task as they are doing it?

Professor Landau: They are aware of what they are doing. As soon as you start them on this task they tell you that they are very bad at this. We ask them what they think as they are going through. They look at it and see it is not right and they remove blocks at random until often all the blocks are gone.

Ernie Rothkopf: Can they identify reference objects from their own drawing?

Professor Landau: I do not know for sure but I doubt it.

Ezequiel Morsella: I would like to talk about the difference between memory failure and the deficit for source memories. When they say that something fell, that statement already has some memory component, because falling is a change through time.

Professor Landau: But they have forgotten what the thing was that it fell off of.

Professor Morsella: One test would be to manipulate the rate at which an item fell.

Professor Landau: They do retain something about the source, for example they will say it fell off instead of fell out. This shows some retention of the source object.

Professor Michele Miozzo: If an action goes from point X, to point Y and back to X, according to your theory the kids should be able to report the initial location because it is also the final location.

Professor Landau: Except they should only be able to report it as a goal. It is forgotten as source and remembered as a goal.

Dr. Massimo Girelli: Have you asked them to produce a motion event?

Professor Landau: We have not, yet. The first task has been to see if they forget the source completely. We perform an event and we ask them to choose the objects and reproduce the event. The hypothesis, if this is part of the non linguistic vulnerability of sources, is that they should be able to pick the right objects but err more on the showing the sources than on the goals.

Dr. Inge-Marie Eigsti: It seems as if you are telling us that these children are failing on harder aspects of the task. I am wondering if it would add to your story if you looked at kids with other variations of mental retardation that would tell you if it is the hardness of the task or if it is the spatial aspects that cause these impairments.

Professor Landau: First of all I do not think they are failing on everything that is hard because we see them excel on the biological motion tasks that are extremely hard. The second point is that in working with special populations you run the risk that they will just be bad at things. I do not think it is a good idea to use controls from other special populations. I think when you use other special populations you are comparing two unknowns. If you get the same qualitative pattern from a mental age match you can make assumptions that development trajectory has gone up to that age and then stopped.

Cynthia Yang: How do the memory deficits reconcile with their extremely verbose language?

Professor Landau: Well I do think the problem is spatial. The kind of memory problem we are talking about I think originates in V1. These are very fundamental aspects of the visual system they are impaired. They are basic functions of the visual system. Language does not mean much for the visual system. What do you need to be able to name an elephant? To say he has ears you do not need to recognize left or right. One of the reasons they do so well on language is because it does not matter.

Professor E. Z. Rothkopf: How do you understand the remarkable resistance they had to noise in the biological motion task?

Professor Landau: I do not know. Their auditory perception is highly acute for instance. They may have developed a special sense here as well.

Professor Annie Senghas: I was curious if there were any Williams Syndrome signers.

Professor Landau: There is a case. The claim from the abstract that I read said that they were impaired on those aspects of sign that heavily recruit spatial location.

Professor Remez: Let us thank Professor Landau and adjourn.

Place: Faculty House
Columbia University
400 West 117th Street

Time: 4:00 PM

Chair: Prof. Robert E. Remez, Barnard College, Columbia University.

Attendees: Lila Braine, Mike Drew, Inge-Marie Eigsti, Massimo Girelli, Peter Gordon, Norma Graham, Julian Hochberg, JoAnne Kleifgen, Robert Krauss, Michele Miozzo, Ezequiel Morsella, Jennifer Pardo, Lois Putnam, Ann Senghas, Sara Shuwairi, Anja Soldan, Herb Terrace, Cynthia Yang

Rapporteur: Bridgid Finn



2 MAY 2002

Understanding figurative language

Sam Glucksberg
Department of Psychology
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Figurative language has been considered derivative from and more complex than literal language. In my dual-reference theory of metaphor I argue that people use the same strategies to create and to understand ordinary metaphors as they use to understand literal language. Metaphors create novel categories that are used to characterize topics of interest. Metaphorical categories are special in that (a) the novel categories are based on outstanding examples of the things that they represent, and (b) these categories get their names from the those best examples. Thus, "shark" can be taken as a metaphor for any vicious and predatory being, from an unscrupulous salesperson to a murderous character in the *Three Penny Opera*. "Shark" thus has dual reference: a literal referent and a metaphorical category, as in "My lawyer was a shark."

I will be talking about metaphor, and a little bit about idioms. People have various opinions about what metaphors are good for. Nietzsche had his own view, he said that "... knowing is nothing but working with your favorite metaphors. The drive toward a formation of metaphors is a fundamental human drive that one cannot dispense with in thought for then one would then dispense with themselves." I do not really know what he meant by that but its positive. Hobbs on the other hand is very clearly negative. "Absurd conclusions follow from the use of metaphors, tropes and other rhetorical figures instead of words proper. Such speeches are not to be admitted." Here we have two diametrically opposing views. Perhaps they were speaking of phrases or paragraphs like this which appeared in the *New Yorker*. "Mr. Strauss-Kahn told the French National Assembly yesterday: 'Today everyone should know that Credit Lyonnais is on its feet again; far from being garroted it is freed from the sword of Damocles that was weighing on its shoulders.'"

The mystery is how we understand this. I will not be dealing with such complex metaphors, nor will I be dealing a great deal with idiom twists. For example, "Drill away," say Alaskans who know which side their bread is oiled on." Primarily I will be focusing on nominal metaphors that take the form "X is a Y." For example, "my job is a jail."

We can understand these with no difficulty whatsoever. How do we do so? There is a standard three-stage pragmatic model coming from the philosophy of language and linguistics, and some corners of psycholinguistics. First, derive the literal meaning of the sentence. The sentence is the unit of analysis. Second, assess the interpretability of the sentence against the context. Third, if it is fine,

stop. If the literal meaning does not make sense in the context, search for a non-literal meaning that does. Searching for or changing a non-literal interpretation requires several steps. As John Searle put it in 1979, "Where an utterance is defective if taken literally, look for an utterance meaning that differs from sentence meaning." Synonyms for utterance meaning would be intended meaning or purveyed meaning. Sentence meaning here would be synonymous with literal meaning.

How can literal meanings be defective? Literal meanings can be defective if they violate a rule of conversation as proposed by Grice. For example, people are expected to be relevant. When someone says, "I have an early morning appointment," in response to an invitation to go to a late night jazz club, it is taken as a refusal, not taken as a fact. You are supposed to be informative. When someone asks where you were born the answer, "North America," would be distinctly odd. We are supposed to be truthful. "Some roads are snakes," is literally false, so what do we do?

The standard pragmatic model says that metaphors, such as "some roads are snakes" or "some jobs are jails," are literally false, and that such expressions are treated as if asserting a simile: "Some jobs are like jails" is what the speaker really intended. Now, similes are always true, albeit trivially true, because any two things are alike in some way. In treating metaphors as implicit similes, the standard pragmatic model now handles them like any literal comparison. With this move, the problem of metaphor comprehension was solved.

There are three psychological issues here. The first claim is that literal meaning has priority; literal language should be easier than figurative language to understand. Non-literal meanings are optional; we do not seek them unless the literal meaning does not make sense in context. From this follows that non-literal meaning can be ignored. Second, the standard pragmatic model poses a cognitive comparison problem. If any two things are alike in innumerable ways, how do we identify those ways that are intended in an expression? It is often said that we cannot compare apples and oranges but the fact of the matter is that we can compare apples and oranges *ad infinitum*. They are both fruit, edible, etc. The third psychological issue is to understand the fate of the literal meaning that is supposedly discarded when a non-literal meaning is sought. Are literal meanings merely rejected, are they a used in some way, are they discarded, have they been inhibited?

With respect to priority of the literal, I am going to argue that literal meaning does not have unconditional or default priority. Metaphor comprehension, with respect to the examples I have been giving, is mandatory and automatic. With respect to the comparison process, I will argue that metaphors are not understood by comparison. With respect to the literal meanings, I will argue that they are only selectively used. Irrelevant literal meanings that might interfere with comprehension are actively inhibited.

Let me turn to the first psychological implication of the standard pragmatic model. With respect to idioms, familiar items such as "kick the bucket" are understood faster in their idiomatic sense than their literal sense. You first think of "to die" and then later perhaps the literal meaning. Secondly

metaphors are understood as quickly as comparable literal expressions. Novel metaphors as well as familiar ones are apt.

Just as with literal language, people cannot choose not to understand. Understanding occurs automatically without conscious control of the listener. In order to look at this in terms of figurative language as well as literal language, we model experiments after the original Stroop interference task. The Stroop asks its subjects not to read its words but simply to name the color of the ink that it is printed on.

[Professor Glucksberg performs a Stroop task with the audience.]

There is a conflict with what the word says and the color of ink it is printed in. We took advantage of that kind of conflict with a sentence verification analogue of the Stroop task. The task was to judge the literal truth value of four sentence types. Literally false sentences are perfectly acceptable in this context. One should not look beyond the literal truth or falsity to seek other meaning. The first type of sentences were true: Some fish are trout. Next, some were false: Some birds are trout. Then, we used metaphors: some jobs are jails, some roads are snakes, which are literally false but not defective in this context. Last, we used scrambled sentences: Some jobs are snakes, some roads are jails. The metaphors should produce the same kind of response conflict as the Stroop interference task. The literal truth value is false while the metaphor is true. You should have trouble saying no or false to transparent metaphors

[Professor Glucksberg provides the reaction time results:

<i>True:</i>	<i>Some fish are trout.</i>	<i>1114 ms</i>
<i>False:</i>	<i>Some birds are trout.</i>	<i>1185 ms</i>
<i>Scrambled:</i>	<i>Some jobs are snakes.</i>	<i>1162 ms</i>
<i>Metaphors:</i>	<i>Some jobs are jails.</i>	<i>1239 ms]</i>

People have trouble saying “false” to literally false sentences when there are metaphorical truths lurking behind the literal meaning. Metaphor comprehension therefore is not optional. It is not dependant on the literal statement being defective. One could argue that this could be attributed to a relation between sentence subject and predicate. The control for that is the following: “Some surgeons are butchers” is an apt or good metaphor that people agree with. “All surgeons are butchers” is not. We put these kinds of sentences in our earlier task to see if we get Stroop interference with “Some surgeons are butchers,” but no interference for “All surgeons are butchers.” We control the subject and predicate relatedness but we vary the aptness. We get the interference with “Some surgeons are butchers,” but we do not find it for “All surgeons are butchers.”

The conclusion then for priority of the literal is first, literal and figurative meaning share equal priority. Second, understanding either a literal or figurative expression is automatic. Not in the sense that it takes no cognitive effort, but rather in the sense that it is non-optional, nor is it under conscious control, it is stimulus driven.

We now turn to tackle our second issue: Are metaphors implicit similes? Now that we have shown that metaphors are understood, we need to ask how are

they understood. One argument is that metaphors are implicit similes. In other words, when a statement of the form X is a Y is literally false, then convert it to a simile. In understanding a sentence of the form, X is LIKE a Y, treat it exactly as any literal comparison. How do we understand literal comparisons? Given a claim that A and B are alike, one model argues that you exhaustively extract all the features of A and all the features of B and then look for matches. It strikes me as a cumbersome exercise because there are going to be many properties that do not match up in the context. I want to argue that we do not really do that. We understand literal comparisons not by matching features but by categorizing.

[Professor Glucksberg gives an example of a comparison test from the Wechsler Adult Intelligence Scale (WAIS). The experimenter asks the subject to tell how the items are alike. Possible questions and answers are: How are oranges and lemons alike? One possible answer is that they are both Citrus Fruits. Professor Glucksberg gives more examples to show how any two items can be compared, though difficulty increases with abstractness.]

This type of categorization can be done without worrying about irrelevant features that might match. You only have to look at the category and take the category properties to answer how the two items are alike. You derive the properties of oranges and lemons by accessing the properties of citrus fruits. It is always the most specific category that these two items belong to. How then are lawyers and sharks alike? I want to argue that they are both sharks. Just as any two things can be alike in any number of ways, any two things can be categorized in multiple ways as well.

[Professor Glucksberg gives a model of the cross categorization of LAWYER, SHARK and TUNA using the categories, FOOD, FISH and PREDATOR.]

A lawyer can also be thought of as a predator if he exhibits all the qualities, for example vicious, aggressive, and merciless, that is any of the predatory qualities that a literal shark exemplifies. A literal shark is prototypical of the category of predators. "Shark" used as a metaphor vehicle refers to a type of thing, whereas used literally it refers to an actual token, shark. We can replace PREDATOR with the word SHARK and name that category "shark."

I was not the first to think of this. Roger Brown in 1958 argued that metaphor differed from other superordinate/subordinate relations in that the superordinate is not given a named result. Instead the name of one subordinate metaphorical vehicle is extended to the other.

[Professor Glucksberg provides the example: "My job is a jail." In this example, "jail" refers to the category of unpleasant, confining, situations difficult to get out of that the literal jail is an example of. Both the job and the literal jail belong to the metaphorical category "jail."]

To what extent is this a special naming device in figurative language, and to what extent is this a device that we always use? I want to argue that this dual reference is a general referring strategy. If a language has no name for a superordinate category, then the name of a prototypical exemplar of that

category is used as the category name. When we do this, then we can paraphrase “LIKE” statements as “IS A” statements, and vice-versa.

[Professor Glucksberg provides the example: Scotties are/are like Kleenex.]

When using “are” we are referring to the category “facial tissue” and we use the word Kleenex to refer to that. Apparent tautologies, such as “Boys will be (like) boys.” The initial “boys” are the individual boys and the second is a type of creature called “boys.” People can be used to exemplify a category.

[Professor Glucksberg recounts this verbal exchange:

Israeli: “If he is a Demjanjuk, then he should be condemned to death.”

Reporter: “But he is Demjanjuk, his name is John Demjanjuk.”

Israeli: “I know his name is Demjanjuk, but I do not know if he IS A Demjanjuk.”]

At that time in the Israeli newspapers a Demjanjuk became anyone who did unspeakable acts. We also have explicit dual reference as seen in “Cambodia had become Vietnam’s VIETNAM.” Vietnam is the token and VIETNAM is the type. These examples are all in English, which has superordinate category names. There is a class of languages called *classifier* languages in which superordinate category names are not frequently available at all. In those languages this exact strategy is used.

[Professor Glucksberg gives an example of a southwest Native American language and sign language using these strategies.]

This solves the paradox of unlike things compared. Similes, such as “My lawyer is like a shark,” compare two unlike things, lawyers and sharks. Nevertheless, similes can be paraphrased as categorical assertions, “My lawyer is a shark.” Literal comparisons such as “Coffee is like tea,” compare two like things. This comparison cannot be categorically asserted however, you cannot say, “Coffee is tea.” Why? Because metaphor vehicles such as “shark” refer to two levels of abstraction: categorical, metaphorical level and the subordinate, literal level.

[Professor Glucksberg illustrates the representational assumption: a metaphorical shark is vicious, predatory, aggressive, etc. The literal shark shares all these properties, but also, can swim, has fins, sharp teeth, etc.]

When you refer to a metaphorical shark all the literal properties are simply not brought to mind. This suggests that the different forms of a metaphor, the categorical *IS A* and the simile *LIKE* emphasize their referents differentially. The *IS A* form refers directly to the metaphorical category, for example, the metaphorical shark. The *LIKE* form refers directly to the concrete literal exemplar of that category, the fish shark.

Which comes first, the metaphorical egg or the chicken of similitude? My claim is that the metaphor, “My lawyer is a shark,” is NOT understood by converting it into the simile, “My lawyer is LIKE a shark.” Just the reverse is true. The metaphor is understood by recognizing that both my lawyer and shark belong to a common category, sharks.

Empirical implications are testable. First, metaphors are easier to understand than similes. Available research shows that this is true. For example, “Some

lawyers are sharks," is understood faster than "Some lawyers are like sharks." Second, memory for similes is biased in favor of metaphors. If you ask people to recall what they have read or heard, "Some lawyers are like sharks" tends to be misremembered as "Some lawyers ARE sharks," and not vice versa.

Other empirical implications that we tested in our lab showed that highlighting the literal referent of a metaphor vehicle will interfere with comprehension. For example reminding people that sharks can swim will make it harder to understand "Some lawyers are sharks." Other research showed that interpretations of metaphors and similes will differ systematically: Metaphors will be more "metaphorical."

Let us turn to the first implication, direct highlighting of literal referents. We conducted a property priming study with target metaphors such as, "Some lawyers are sharks." Sentences came on the screen if the string of words was sensible. Half the strings were sensible, half were non sensible. Before each string the subject was primed with topic alone: LAWYERS, or vehicle alone: SHARKS. We expected people to be faster with the topic alone prime than with a string of Xs. Vehicle alone should also facilitate because you gain some information about what is coming up. The critical prime conditions used an irrelevant property of the topic, for example, "Some lawyers are married." We expected this to have only a slight facilitating effect because lawyers can still be sharks whether or not they are married. However, with the prime, "Sharks can swim," we provide subjects with the word "sharks" and in some case we expected this to facilitate understanding. Topic alone facilitated by 300 ms. Vehicle alone facilitated by 200 ms. They are not statistically different. "Some lawyers are married," facilitates in the manner of topic presented alone and vehicle presented alone. "Some sharks can swim," calls attention to the literal shark slows the subject slightly evoking an impression of the wrong kind of shark.

Other empirical implications are that the interpretations of metaphors and similes will differ systematically: Metaphors be more metaphorical. We did a simple experiment asking subjects to paraphrase either similes or metaphors. Instructions were to write down in 1-2 sentences what each expressions mean.

[Professor Glucksberg gives an example paraphrases for "Some ideas are (like) diamonds." Similes: some ideas are rare and desirable, some ideas are so interesting it is as though they shine and glitter, some ideas are very valuable. Properties like "interesting" are emergent, context-dependent, metaphorical. They do not apply to literal diamonds. Metaphors: Some ideas are brilliant and insightful, some ideas are fantastic, and creatively very unique.]

We see more emergent properties for metaphors "are diamonds" than for similes "like diamonds." In metaphor form most of the properties are emergent, coming from the interaction of the topic with the vehicle. Very few are literal. Therefore, metaphors are more metaphorical than similes. There are significantly more basic-level, literal attributions for similes than for metaphors and significantly more superordinate context-dependent attributions for metaphors than for similes.

I would like to summarize my conclusions on categorization versus comparison. First, metaphors are understood directly as categorical assertions. When I say that my job is a jail, I mean that it is not just like a jail, it really is a jail! Metaphors can be paraphrased as similes because the metaphor vehicle can refer either to the metaphor category, or to a prototypical, literal exemplar. This is the linguistic strategy of dual reference.

So to answer the question which comes first, chicken or egg? The philosopher of language Max Black in 1962 said, "It would be more illuminating...to say that the metaphor creates the similarity than to say that it formulates some similarity antecedently existing." Nelson Goodman, another philosopher of language said, "Similarity, ever ready to solve philosophical problems, is a pretender, an impostor, a quack...Similarity does not explain metaphor...a reversal in order of explanation might be appropriate: The fact that a term applies, literally or metaphorically, to certain objects may itself constitute rather than arise from a particular similarity among those objects. Metaphorical use may serve to explain similarity better than the similarity explains the metaphor."

Let me turn now to the last set of issues: the fate of literal meanings. I want to argue that they are used when potentially relevant. For example, in opaque idioms, "Kick the bucket," no one can point to any relation between the meanings of the constituents and the meaning of the phrase. None of the explanations offered have been convincingly demonstrated. Nonetheless the literal meanings of the constituents constrain how we can use the phrase. Kick is a discrete act. The semantics of "kick" constrain its use. You cannot lie kicking the bucket all week but you can lie dying all week.

We did a study looking at how quickly people could understand variants of transparent idioms.

[Professor Glucksberg gives the example, "Despite being tortured, he did not spill a single bean as understood as quickly as "He did not tell a single secret.]

Metaphorical idioms literally have meanings.

[Professor Glucksberg gives the example, "Don't give up the ship" as an idiom that acts the way a metaphor vehicle does. It is the name of a category.]

We can use literal meanings for understanding the basis for idioms in languages other than English.

[Professor Glucksberg gives examples of Spanish idioms:

<i>There are no Moors on the beach:</i>	<i>The coast is clear.</i>
<i>The son of the cat kills mice:</i>	<i>Like father like son.</i>
<i>To another dog with that bone:</i>	<i>You are putting me on.]</i>

However, sometimes literal meanings are irrelevant and can interfere with comprehension. For example, can loan sharks swim? When someone says, "Break a leg" as a way of saying "Good Luck," are there any implications of bodily harm or injury? I want to argue no. The reason is because there is inhibition, active or unconscious, of irrelevant literal meaning.

One study in the 70s asked whether certain kinds of concepts could be primed in a judgment context. Subjects are given the following passage:

Donald spent a great amount of his time in search of what he liked to call excitement. He had already climbed Mount McKinley, driven in a demolition derby, shot the Colorado Rapids in a kayak, and piloted a jet-powered boat – without knowing much about boats. He had risked injury, and even death, a number of times. Now he was in search of new excitement. Etc.

The task is to rate Donald on scales of reckless and adventurousness. Twenty minutes before running is this portion of the experiment subjects participated in another experiment. They were asked to look at a list of words and asked to judge whether each word was concrete or abstract. Some people were given neutral words; they rated Donald as equally reckless or adventurousness. The experimental group was given words that connoted danger or injury. They rated Donald as more reckless than adventurous and more reckless than the control group had. Reminding people of danger in the world lead them to view Donald as more reckless.

We decided to do a similar experiment use prime scenarios for “Break a Leg.” People are asked to read the following passage:

As the snow began to die down, John went through his practice routine one last time. He had gotten to the point where his nervousness had become excitement. About one hour before he was to leave, he shared a cup of tea with his roommate. They sat in the living room with the big bay window, watching the snow begin to fall more heavily again. John realized suddenly that he would have to leave immediately to get there on time.

There are three kinds of primes. The *idiomatic* scenario: As John left for his performance, his roommate said, “Break a leg tonight.” The *literal* scenario: While rushing to his performance, John broke his leg. The third condition there is nothing read about breaking legs.

After reading the primes subjects rated Donald on recklessness versus adventurousness. Does “break a leg” inhibit reckless? “Break a leg” as in wishing good luck should not affect judgments of “adventurous.” “Break a leg” in the literal sense should affects judgments of recklessness. The three conditions had no affect on ratings of adventurousness. In the reckless condition, break a leg to signify good luck rated Donald as significantly less reckless than the controls. This suggests that people are actively inhibiting the literal meaning of “break a leg.”

To return to the three psychological issues that I started with, first, literal meanings are used whenever relevant, and when irrelevant, they are inhibited. Second, literal meanings do not have priority. Therefore, literal language is not easier to understand and non-literal meanings are not optional. They are apprehended whenever available. Thus, just like literal meanings, figurative meanings cannot be ignored. Last, how do we solve the comparison problem? I argue that metaphors are not converted to similes; they are understood directly as categorical assertions. Metaphors can be paraphrased as similes because the

metaphor vehicle can refer either to the metaphor category, or to a prototypical (literal) exemplar. This is the linguistic strategy of dual reference.

APPLAUSE

Questions

Professor Van Lancker Sittis: Through time we have seen metaphors change into the actual definitions of a word. For example the word “file” associated with computers is no longer metaphorical though 10 years ago it was. When does this occur?

Professor Glucksberg: An operational task could separate the differences between the metaphorical meaning and the real definition.

Professor Robert Krauss: There is a principal distinction between what is literal and what is not. Literal meaning is the outcome of linguistic decoding. At some stage in English, the word “grasp” meant “to gain hold.” It has come to mean “to get an idea or to comprehend.” At some point metaphor converts into a literal meaning.

Professor Glucksberg: Jackendoff has done much work that deals with these questions. One thing he investigates is whether the metaphor can be used in distinct grammatical instantiations. For example, Can he grasp ideas? If the question or new form does not make sense it has not changed from an idiom.

Professor Michele Miozzo: Are metaphors such as, “my job is a jail,” stored as a lexical entry that points to a particular kind of meaning?

Professor Glucksberg: There is definitely evidence for a phrasal lexicon where idioms are stored.

Professor Ezequiel Morsella: It seems as if we possess the ability to derive meaning despite the obscurity of a statement, as seen in people attempting communication in a foreign language they may not understand particularly well, for example. Is it a reasonable claim that we can derive meaning from anything?

Professor Glucksberg: You saw, for example, that once they were explained, the Spanish examples were clear.

Professor Morsella: Yes, but we are able to use context to gain meaning.

Professor Glucksberg: People overestimate how well they have communicated. I am not sure about how much comprehension is going on in discourse.

Professor Van Lancker Sittis: How are idioms and metaphors the same or different?

Professor Glucksberg: They are different. Idioms are retrieved from memory while metaphors are generated until they become literal.

Dr. Jennifer Pardo: Why do people use metaphors instead of literal words?

Professor Glucksberg: Metaphors enable you to deal with a lexical gap. Metaphors communicate a pattern of properties, not just a single concept.

Cynthia Yang: What does it take for “some jobs are snakes” to become metaphorical? Is it a function of frequency of occurrence, or of similarity?

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Professor Glucksberg: It is a combination of both. Both are dimensions of relevant attributions

Professor Remez: Let us thank Professor Glucksberg and adjourn.

Place: Faculty House
Columbia University
400 West 117th Street
Time: 4:00 PM

Chair: Professor Robert E. Remez, Barnard College, Columbia University.

Attendees: Lila Braine, Patricia Brooks, Gerald Echterhoff, Gisela Jia, Marco Jacquemet, Ilona Kovary, Robert M. Krauss, Christoph Mensebach, Janet Metcalfe, Michele Miozzo, Ezequiel Morsella, Rebecca Piorkowski, Lois Putnam, Robert E. Remez, John Saxman, Ann Senghas, John J. Sidtis, Herbert Terrace, Diana Van Lancker.

Rapporteur: Ezequiel Morsella.

