ANOVA, $F(1,8) = 11.89$, $p < 0.009$). The linear regression to predict tactical deception from neocortex ratio is

$$td = -16.5 + 8.16\,nr$$

(where td = number of records of tactical deception observed minus number expected from frequency of studies, squared and divided by number expected; nr = Dunbar's neocortex ratio). Relative neocortical volume explains 60% of the variance in usage of tactical deception. Too much reliance should not be placed on the exact numbers, but it is encouraging that a relationship is present at all with such rough and ready measures.

First, this gives strong support to the belief that larger brains allow more intelligent behaviour – in this case more ready acquisition of socially useful tactics, probably by means of quicker learning. Second, it encourages confidence in the use of Dunbar's neocortex ratio measure for brain size, a measure which is not independent of body size and consequently not insensitive to absolute brain volume. This makes sense, if brains are to some extent "onboard computers," because computers – as approximations to Turing machines – are limited by their number of elements not the weight of their container. Finally, it gives increased confidence that primate tactical deception is partly a product of intelligence.

A gesture in the right direction?

Michael C. Corballis

Department of Psychology, University of Auckland, Private Bag 92019, Auckland, New Zealand
Electronic mail: *psy_mcorballis@ccnovi.aukuni.ac.nz*

The Achilles' heel of Dunbar's thesis lies in the following sentence: "The fact that language can be interpreted as fulfilling the same role as social grooming suggests that, rather than being the selective factor driving brain evolution, ecologically related information-exchange might be a subsequent development that capitalised on a window of opportunity created by the availability of a computer with a substantial information-processing capacity." The mixed metaphor may not be too serious, given that computers these days *do* seem to have windows (but not heels). The more substantive problem is that there are powerful reasons, articulated by Pinker and Bloom (1990) among others, for supposing that language is designed precisely for the serial production of propositional communication and cannot be simply a matter of taking advantage of a general-purpose computer that evolved in some other context. Seidenberg and Pettito (1987) also warn against assuming that apes and humans differ simply in cognitive capacity; "the dichotomy between the apes' cognitive and linguistic capacities," they write, "is the single most important finding to have emerged out of modern ape research" (p. 284). In other words it is language, not general intellect, that truly sets us apart.

My guess then is that Dunbar's argument might be more plausible if inverted; the selective pressures for the evolution of language probably had to do with propositional communication, and this secondarily solved the problem of social grooming among large groups. Even granted our propensity for gossip and its role in social cohesion (although this might be disputed these days in royal circles), one is hard pressed to find any structural principles common to grooming and human language; it is a bit like seeking the origins of the eye, often compared to language for its calibration and functional intricacy, in the navel.

I can, however, offer one thought that might be of assistance. I have tried to revive the idea, originated by Condillac (1746/1947) and more recently championed by Hewes (1973), that human language may have originated in manual gestures (Corballis 1991; 1992). I think there are some arguments in addition to those offered by Hewes in support of this idea. One is that it is becoming increasingly clear that manual sign language such as ASL has all of the properties of a natural language (e.g., Poizner et al. 1987), including a critical period in development (Newport 1990). Deaf children exposed only to sign language even go through a "babbling" stage analogous to the vocal babbling of normal infants (Pettito & Marentette 1991).

I have also argued that language may have evolved as a primarily manual system from *Homo habilis* through archaic *H. sapiens*, and that one of the features that distinguished *H. sapiens sapiens* was the switch to a predominantly vocal mode (Corballis 1991; 1992). This switch would have freed the hands from involvement in communication, which may explain why the "explosion" in the manufacture of tools and other artifacts seems to have occurred well after the emergence of *H. sapiens sapiens* as a distinct species (Pfeiffer 1985).

It might be easier to sustain the view that language relates to grooming if it is also supposed that language was initially based on manual gestures.

Grooming and language as cohesion mechanisms: Choosing the right data

Marina Cords

Department of Anthropology, Columbia University, New York, NY 10027
Electronic mail: *mc51@cunixf.cc.columbia.edu*

Dunbar's hypothesis combines many sorts of information. I found the idea intuitively attractive, but the evidence was not altogether convincing. I shall limit my comments to two areas.

Is a special cohesion mechanism needed? Dunbar argues that a cohesion mechanism other than grooming was needed by ancestral humans because they associated in very large groups whose integrity and cohesion would have demanded an extraordinarily (and impossibly) large proportion of time spent grooming. This conclusion is based on extrapolation from a linear regression equation relating grooming time to group size in 22 extant primate species. These data were selected from an earlier paper (Dunbar 1991): only catarrhine species living in cohesive groups of stable size were included. But why should the four species with fission-fusion societies be excluded? After all, the human societies included in Table 1 are also characterized by hierarchical fission and fusion: this is why overnight camps can be distinguished from bands or villages. Our concern is behavior that maintains ties within larger social units whose members may meet infrequently. There are roughly analogous units in the excluded nonhuman species. Rather than exclude such species, it seems we should consider them with special interest.

Including these four species affects the relationship between group size and grooming time and hence the predicted grooming time for humans living in groups of 148.7. Although Dunbar acknowledges this fact parenthetically (sect. 3.2), and though his statements are strictly correct, it was not until I manipulated the data myself that I realized how much these four data points influence the picture. First, when they are included, the relationship of grooming time and group size looks more asymptotic than linear, mainly because of the points for species with group sizes greater than 100 (Fig. 1). This change exemplifies the danger of extrapolating beyond the limits of available data. Assuming that maintaining cohesion gets increasingly difficult in larger groups (which seems likely), the full dataset suggests that in very large groups, some behavior other than grooming operates to maintain cohesion. Possible candidates are distance-regulation behavior, monitor-and-adjust behavior (Rowell & Olson 1983), or postconflict reconciliation (de Waal 1989). Some nonhuman primates live in groups considerably larger than

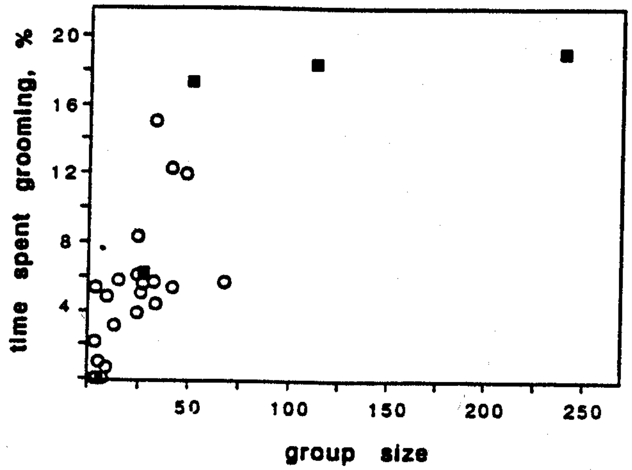

Figure 1 (Cords). Mean percentage of time spent grooming plotted against group size for 26 species of group-living catarrhine primates. Open circles indicate points included by Dunbar in generating equation 2; filled squares indicate the four fission-fusion species originally excluded from that analysis (data from Dunbar 1991).

those predicted for humans. For example, Japanese macaques can maintain cohesive groups of over 1,000 members: these groups are provisioned, however, so feeding time is reduced and more time should be available for social interchange. As far as I know, actual time budgets have not been published for these large groups. Wild Rwandan black and white colobus monkeys have also been reported to live in very large stable groups (up to 350 members, Vedder, personal communication). How these groups maintain cohesion is not yet known, but Vedder reports that neither grooming nor vocalization occurs especially frequently. If alternative mechanisms for maintaining cohesion operate in monkeys and apes, would language be necessary or even advantageous? It seems we should know much more about the alternatives: comparing language to grooming is only part of the task.

Second, if we overlook the apparent nonlinearity of the data (transformations do not improve the fit), the data points from fission-fusion species change the linear regression equations. Specifically, the predicted amount of time that ancestral human groups of 100–200 would spend grooming is *reduced*. Simple least squares regression on the full dataset predicts that humans should spend 16% of their time grooming; the 95% confidence limits on predicted group size yield grooming times of 12–23%. These values mostly fall within the range of those reported for living primates. (Reduced major axis regression gives percentages 2–6 points higher.) Thus even ignoring the possibility of alternative cohesion mechanisms, it is not clear that grooming could not do the job in fairly large groups of early humans.

Finally, we have assumed that early humans had time budgets quite similar to those of extant primates; if cooperative foraging or tool use improved efficiency, however, early humans might have had more time available for social grooming than most nonhuman primates have now, even if total energy requirements were greater because of their relatively large body size: who knows? Pinpointing the maximal amount of time that could be spent grooming seems a rather speculative exercise.

Language as a bonding mechanism. The theoretical argument that language is better than grooming for maintaining group cohesion because it allows the acquisition of information about group-mates not present and the classification of types of group-mates is persuasive. The data supporting the role of language in maintaining group cohesion, however, are not very convincing. First, I question whether conversations in a university refectory are representative of all conversations. Second, I find it hard to judge the proportion of time spent gossiping as being high without having some expectation of what it should be. What the null expectation for the distribution of conversation acros possible topics? Finally, even if language did evolve to allo gossip, language may not be used today for the same purpos the hypothesis implies only that language could have been c can be used for this purpose. Thus the data on content c refectory conversations seem relevant only insofar as they sho that social relationships are a possible subject. This "finding" consistent with the hypothesis, but it certainly does not "imply that the hypothesis is correct.

A further argument for language's suitability as a cohesio mechanism relates to its efficiency with respect to grooming Dunbar limits the concept of efficiency to the number c interactants that can be reached in social exchange. Perhaps however, we also need to consider how they are reached. Ca we assume that one would learn as much about each of the thre people in a conversation group as one would about a singl grooming partner? Would one learn equal amounts in equa periods of one-on-one conversation and grooming? The idea c efficiency is vague because the information transmitted throug social interchange has not been specified. That is, we do no really understand how social interchange maintains group cohesion. If Dunbar's hypothesis stimulates research directed at thi question, it will have served an important purpose beyond th presentation of a new and interesting idea.

Confounded correlations, again

Terrence W. Deacon
Mailman Research Center, McLean Hospital, Harvard Medical School, Belmont, MA 02178
Electronic mail: *deacon@harvarda.harvard.edu*

In the last decade alone dozens of papers have endeavored t demonstrate that brain size (or the size of some brain structure) is correlated with some important socioecological variable (such as social structure, home range size, foraging strategy, diet, day journey, group size, etc.). It is presumed that discovering such a correlation will answer the question: What selects for increased brain size? I happen to think this is a misguided question. Dunbar (1992a) carefully debunks many previous claims for correlations between brain size, body size, encephalization, and various ecological variables, but at the same time he suggests another correlation: between relative neocortical volume (either total neocortical volume or else the ratio of neocortex to the rest of the brain) and social group size. His target article further extrapolates from this claim to suggest explanations for human brain evolution, social structure, and the origins of language.

Dunbar makes four principal claims:

1. Group size is a function of relative neocortical size in nonhuman primates.
2. Extrapolation from this regression trend predicts group size in traditional human societies.
3. Social grooming is a function of social group size in primates because it functions to maintain group cohesion by decreasing agonistic interactions that might otherwise split the group.
4. Language evolved as a more efficient means than grooming of maintaining group cohesion, thus allowing larger mean group sizes.

Together they lead to the principal hypothesis: our large neocortex ratio accurately predicts large group sizes in human societies, because the neocortex expanded to meet the information demands of the increased social interactions provided as language replaced grooming as a buffering mechanism.

The first claim is the starting point for all subsequent extrapolations and so its weaknesses ramify throughout the remaining arguments. Despite much care taken to test and control for the