

Group structure and group size among humans and other primates

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Dunbar's basic idea that neocortical size constrains "the number of relationships an animal can keep track of in a complex, continually changing social world" is appealing, but the notion that such a limit leads to a constraint on the size of the social groups in which the individual is embedded is less so.

The problem is in Dunbar's casual treatment of groups. Over 30 years ago Floyd Allport (1961, p. 195) pointed out that because "a group is a phenomenon so familiar to everyone that it is not a question of what a group is, but only of how it works," researchers had simply "assumed the existence of groups." This is precisely what Dunbar has done.

Dunbar's notion that the limit on an individual's information processing capacity imposes a limit on group size depends on how the group is conceived. He defines a group as a subset of a population of conspecifics that "interacts on a sufficiently regular basis to have strong bonds based on direct personal knowledge." For their knowledge to be personal, each individual must interact "on a sufficiently regular basis" with each and every other individual in the group. A subset that is maximal with respect to that property has been formally dubbed a "chque" (Luce & Perry 1949).

The properties of chques can be specified in exact terms. Given a finite collection of individuals $A = (a, b, c, \dots)$ along with a symmetric relation $/$ that links those pairs of individuals in A that interact on a sufficiently regular basis to have "strong bonds," suppose that each individual in A has the relation $/$ with n other individuals; n is then the number of others with whom an individual has a "personal" tie. Suppose further that we find a clique in A of size m .

If Dunbar is right, there must be a relationship between n and m . But the value of m only sets a lower limit on n , $n \geq m - 1$. The upper limit of n depends on the arrangement of the ties linking individuals in A , and there is *no* necessary connection between the number of others with whom an individual has a personal tie and the sizes of the "groups" in the sense they were defined by Dunbar.

Dunbar may, however, have had other (unstated) restrictions in mind when he talked about groups. In his groups, for example, he may have assumed that "friends of friends are friends." In that case, the relation $/$ would be transitive and each group would be a special kind of clique that Davis (1967) called a *cluster*. All individuals within each cluster would be directly linked, and no individuals falling in different clusters would be. In that case, $n = m - 1$, and individual network size would be inextricably tied to group size.

But, at least in the case of human primates, interaction frequencies are certainly not transitive (Freeman 1992b). Humans do display some tendency to strain toward transitivity in

their relations with each other, but their interaction patterning is by no means as simple as Davis's clusters would suggest.

It turns out, however, that human observers of interaction patterns seem to *want* to see them as transitive (DeSoto 1960). Indeed, there is growing evidence that human observers impose transitivity on their observations and thereby construct a simplified and exaggerated image of group structure (Freeman 1992a; Freeman & Webster 1993).

Given this tendency, one cannot help but wonder about the accuracy of the data on group size used by Dunbar. Primate ethologists take it for granted that virtually all anthropoidea organize themselves into groups (Maryanski 1987). This assumption suggests that the groups they report may have little to do with actual interaction frequencies. Indeed, the one study that compares systematic observations of interaction frequencies (among mantled howler monkeys [*Alouatta palliata*]) with an ethologist's classification of them into "troops" showed very little agreement between the two (Sailer & Gaulin 1984).

For similar reasons, the ethnographic reports on human group sizes used by Dunbar must be viewed with suspicion. With respect to humans, we'd certainly be on firmer ground if we forgot about groups entirely and examined data on frequencies of individuals' interpersonal contacts.

Fortunately, such data are available. Gurevich (1961) reported a study in which he tried to estimate the *acquaintanceship volume* for a sample of 27 humans. An individual's acquaintanceship volume was defined as the number of others whom that individual meets repeatedly in such a way that each recognizes the other and each can identify the other by name. This is very close to Dunbar's concern with the number of others an individual is able to keep track of

To estimate this number, subjects were required to keep a diary for 100 days, recording every person they contacted on a given day who met the criteria. The number of different persons contacted in the 100-day period ranged from 72 to 1,043. Of course, many of these were contacts that were repeated again and again. Indeed, the number of contact events varied from 377 to 7,645. The pattern of repetition and the rate of introduction of new names were used to estimate the number of acquaintances who would have been listed had the diary been kept for 20 years (de Sola Poole & Kochen 1978). That number is 2,130; it is a full order of magnitude greater than Dunbar's estimate of 147.8. Yet these are all individuals who meet Dunbar's criteria; they are all known personally by the subjects and known well enough that the subjects could recall their names and faces.

The discrepancy between Gurevich's estimate and Dunbar's is huge, but it does not indicate that Dunbar's basic thesis is wrong. Such a discrepancy could result from the fact that the nonhuman primate data are records of group sizes and Gurevich's human data are records of individual interaction patterns. To determine the implications of Dunbar's ideas for data on individual interaction we would need comparable data on nonhuman primate interaction patterns. Such data are rare and difficult to collect.

I think Dunbar's view is important enough to deserve a more rigorous development and more reliable data. His idea of group requires a more systematic and contemporary treatment. My guess is that he will end up having to consider not only group size but also structural complexity. And on the data question, he will need not the impressionistic reports of ethologists and ethnologists but matrices representing records of systematic long-range observations of interaction frequencies among conspecifics. Only then will these ideas be given the careful consideration they deserve.