## DISCUSSION

The Richardson model on arms race discussed in the Ferejohn paper and Herbert Simon's [2] mathematical treatment of the verbal propositions in <u>The Human Group</u> by George C. Homans [1] are classical items for anyone interested in the use of mathematics and statistics in the social sciences. A new paper proposing a more extended arms race model is therefore received with considerable interest. However, my own experience in this area has been rather limited, and these comments can only be of a general nature.

One may say that to empirically identify an arms race there is no need for mathematical models or differential equations. Tense international relations and large arms budgets are all too obvious just from reading newspapers, but that is an unfair comment on the Ferejohn model. His model seems very promising for the understanding of the complicated relationships that exist between nations.

Any model of social phenomena is only as good as the assumptions that go into the model. Such assumptions are abstracted from the substantive theory developed to account for the phenomena. International relations has become a special topic of study within political science, and one could have expected that the assumptions in the Ferejohn model would reflect some of the work done in international relations. With the assumptions more deeply anchored in substantive theory the consequences of the model would have been more relevant for current political research.

The Ferejohn model deals with an extremely difficult problem. Many variables enter into determining the arms budgets in different nations. That would lead one to believe that in order to explain these phenomena more fully, larger and more complicated models are needed. Such work undoubtedly goes on under classified cover, which means that it is difficult to see the Ferejohn model in full perspective. But in spite of this the Ferejohn model is a welcome addition. The model shows imagination in its dealing with a difficult problem, and one can only hope that this work will be pursued and made better known.

The paper by Geisel, McGuire, Rosenthal and Kies deals with a problem that is guaranteed not to have a solution. This is no criticism of the paper, however, quite on the contrary. With problems of this kind one is more free to formulate models and investigate the properties of the models. The basic underlying problem dealt with in this paper and which does not have any solution, is the problem of estimating the cell entries in a set of contingency tables where the margins are known and the cell entries are unknown. One can look at such tables until one is blue in the face without the tables divulging their secrets. But information about the cell entries may be obtained from the observed margins by insightful modelling of the relationships between the cell entries and the margins. The social science world is full of unidimensional distributions from census statistics, voting records and other sources. With substantive theory as a collection of statements on how variables are related one wants to relate the variables from the distributions above. But with unidimensional variables the relationships can only be measured on the group level, even though for most purposes one would want the relationship measured on the level of the individual. This, however, is only possible if the cell entries are known.

The strength of the paper lies in its statistical parts, which is appropriate for this meeting. The main contribution consists of the estimation procedures that are developed. But I am not certain that the methods are sufficiently justified. It may be possible to show that ordinary least squares estimators have undesirable statistical properties and thereby conclude that one should move on to two and three stage least squares. But at the same time one can show examples with known cell entries that the estimates obtained from the margins using ordinary least squares are very close to the true cell entries. This is the type of problem where it may be more profitable to invest more heavily in the model and spend less on the estimation methods. I am not coming out against complicated estimators with good statistical properties; I am arguing that simple estimators should not necessarily be excluded, and we need in this case to know more about why ordinary least squares should not be used. One point that can be noted in this connection is that the correlation between ei and Rki is not as important as the magnitudes of the ei's in deciding whether ordinary least squares is appropriate or not.

The vitality of the statistical profession is dependent on the input and challenges from those who are using statistical methods in their substantive area to obtain results otherwise not obtainable. This paper is a good example of such a challenge. But joint papers also run the risk of being disjointed. Sections 2, 3 and 4 are highly technical and cannot be read or understood by most social scientists. These sections have no references to the substantive problem, even most of the notation is changed.

In some ways it seems as if Sections 5 and 6 lost sight of Section 1. For example, in the choice of variables it is not clear why EI 48- is selected for Rafi-K and LOW.ED is selected for Rafi-M. Perhaps a more serious question has to do with the phenomenon of coattails. The concept does not seem to be clearly defined in terms of the model that is analyzed. This may have to do with a lack of distinction between aggregate and individual level data. In Section 1 we get set up to investigate the individual level data, but in the later sections we do not make it back again. After analyzing the structural regression equations we only make it back in the end to the proportion model but not to the original equations in Section 1.

The substantive model ought to do justice to the estimation procedures that are developed. With that the authors will have made a distinct contribution to the whole topic of cross level analysis where data are available on one level and we want to do analysis on another level. The paper by Richard Juster was not received in time to be included in this discussion.

## REFERENCES

- Homans, George C., <u>The Human Group</u>, New York: Harcourt, Brace and Company, 1950.
- [2] Simon, Herbert, A., "A Formal Theory of Interaction in Social Groups," <u>American</u> <u>Sociological Review, 17</u>(1952).