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Review Article

On the Forests and Trees of 'The Distant Democracy'

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In *Scandinavian Political Studies*, Vol. 2 (New series) No. 2, 1979, Willy Martinussen has commented on my earlier article in SPS (Vol. 1, No. 4, 1978), in which I take up certain central findings from his book *The Distant Democracy*. For the sake of space, I will limit my comments here to brief replies to Martinussen's direct points of criticism.

(1) Martinussen feels that I have missed his forests for his trees and that I have read his work the way the devil reads the bible. In defense of both my arboreal perception and scriptural sincerity, let me simply state that I believe that I have grasped the major intent and conclusions of *The Distant Democracy*, and that I have reported these with nonimpish intent. I disagree, however, with Martinussen's contention that his book is primarily descriptive and seeks only to 'generate hypotheses instead of testing models'. While it is true that he does not test models in any rigid methodological sense of the term, it is also true that his study clearly aims at testing hypotheses and arriving at descriptive conclusions. These hypotheses and conclusions are, moreover, not 'generated' during the course of the study, but clearly stated in explicitly normative terms at the outset. We are thus informed in the introduction that the 'main thesis' of the book is that democratic ideals are not being realized in Norway because of systematic, cumulative inequality in the possession of political resources. This 'main thesis' is tested for and found valid throughout the study, and it is this finding which I find problematical.

(2) Martinussen feels that I have – in line with my own purposes – been highly selective in reporting his results on political participation. He claims that he has used five measures of participation and analysed them with a wide number of different techniques, whereas I have reported only two measures in tems of only one of the methods. I will return to methodology below, and reply to the first point as follows:

(a) My article concentrates on only two of Martinussen's measures ('representational activity' and 'organizational activity') because it is these two measures which cover the same two types of participation as those studied by Nie, Powell and Prewitt, who provide the point of departure for my own analysis. Furthermore, these two indicators clearly tap the major channels of influence in Norway as originally described in Stein Rokkan's 'two-tier' model of decision-making.

(b) In his comments, Martinussen refers to all five of his measures as 'different measures of political participation'. In his book, however, he specifically refers to two of these measures – 'information seeking' and 'political discussion' – as measures of 'political preparedness'. That he himself does not consider these activities as participation *per se*, is also apparent from his own description of the amount of participation in Norway, where they are excluded from his scheme of 'political stratification' (pp. 32–33).

(c) As for the fifth measure, 'direct action', I have raised doubts about the measure because of its extremely skewed distribution as a two-value indicator (88 to 12); a feature which causes serious difficulties for correlational analysis, particularly for the gamma coefficient which Martinussen relies on so heavily. Martinussen misinterprets the problem here by claiming that I have neglected direct action because so few people engage in it, but this is not the case at all.

(3) Martinussen also feels that I have misrepresented him on a number of methodological points, to which I would reply as follows:

(a) I have relied mainly on the results of Martinussen's MCA analysis (rather than the other techniques) because: (i) they reflect the most powerful multivarate technique applied; (ii) Martinussen's problem requires such a technique (see below); (iii) Martinussen himself relies on the same results for his summary conclusions; and (iv) Martinussen's other techniques are either too limited in their implications (the three-variable graphs) or ambiguous because of indicator construction (the gamma coefficients).

(b) I have stated that Martinussen apparently believes that the eta coefficient measures only linear correlation; something he now emphatically denies. My contention here is based on the following statement: 'the eta and beta coefficients have statistical variance as point of departure, and presuppose linear correlation between the variables' (*The Distant Democracy*, p. 65, footnote 33), plus the fact that none of Martinussen's interpretations of eta allow for the possibility (always present) of nonlinear relationships.

(c) Martinussen also claims that he has not said that it is the gamma coefficient which gives the best impression of his data. My feeling that he had done so is based on the fact that he specifically defends gamma on two different occasions as the best measure for grasping both the *form* (p. 68) and *strength* (p. 236) of his hypothetical relationships. Admittedly, this is expressed in relation to other measures of correlation, but his claim now that it is the simple co-distributions of variables which *do* provide the best impression, seems even more questionable. These co-distributions contain the least amount of information (statistically) of all three of Martinussen's techniques. Furthermore, the three-variable graphs are applied to some relationships, but not to others. Finally, they cannot be decisive in relation to his hypotheses since the latter clearly demand a multivariate technique which can control for more than one variable at a time. The essence of Martinussen's 'main thesis' is the existence of *systematic, cumulative inequality;* a state of affairs which can only be confirmed by powerful multivariate techniques.

(d) Finally, on the question of causality, Martinussen apparently feels that to demand causal techniques of his analysis is somehow unfair. To this I can only say that I have, in fact, not criticized Martinussen on this point, but merely indicated that one of the links in his systematic inequality hypothesis (the mediating effects of political attitudes) *requires* a causal technique, and that, without it, his conclusions on the matter – which are categoric and repeated throughout the study – do not hold up.