

# Review Article

## On the Inability of Seeing the Forests for the Trees

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In *Scandinavian Political Studies*, Vol. 1 – New series – No. 4 1978 William M. Lafferty pays my book *The Distant Democracy* a visit with the intention of presenting a new interpretation of my findings, to support some conclusions from his own study of three small industrialized communities in Norway. Reinterpretation of research findings is a main characteristic of a viable science, and should be welcomed as a matter of course. Such an undertaking requires, however, that the original interpretations and the frame of reference within which they are presented are correctly reported. Lafferty does not quite meet this requirement. I therefore must ask for space to furnish some corrections and clarifications.

Since I – within a brief commentary – can touch upon just a few points on which Lafferty in my opinion is wrong or gives a biased impression of my work, it is necessary to emphasize that I agree with parts of his argument, especially with his reasoning about social development and political participation. I cannot see that I 'rely on Nie, Powell and Prewitt' in this connection – I have only cited their work as one of the many empirical studies from one point in time of the synchronic relationships between social status, organizational involvement and political activity which gives the same main impression as my own findings about the Norwegian political system.

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Lafferty's most sensational hypothesis is that social stratification is not a major cause of participatory inequality. Whether this is to be considered the main argument of my book depends on the interpretation of the term 'cause'. I would still maintain that the conditions inherent in social class differences make for much of the inequalities of political activity among citizens. *The Distant Democracy* tries to specify some of the processes which create these inequalities. However, I find it more difficult to speak of causes and effects than Lafferty does. On pages 14–15 in my book I am careful to stress this, and point out to the readers that the analysis is quite descriptive, and seeks to generate hypotheses instead of testing models. The concept of 'political resources' is introduced as my main tool for specifying *conditions* for political participation, *not causes* in the classical sense of the term.

To prove me wrong about the relationship between social status and political participation, Lafferty performs a reanalysis of some of my data. Our first disagreement seems to concern the question of what is to be regarded as 'political participation'. In *The Distant Democracy* I introduce a wide conception of 'the

political', from which it follows that political participation may cover a wide spectrum of activities, ranging from informal political communications to the exertion of control over specific national or local decisions. Consequently, I develop five different indicators of political participation. Lafferty mentions three of them, and uses two in his reanalysis, one of which I myself assert is weakly correlated with social status characteristics for particular reasons.

In my analysis, I also distinguish between several aspects of social status, finding occupational position more important as a condition for some types of activity than education is, also for good reasons.

Thirdly, I use a whole range of statistical techniques to display the empirical results of my study. Both in the text and in an appendix I warn against relying too heavily on correlation methods, especially the MCA technique, and I use them for purposes of summary only. Lafferty cites a sample of the MCA results, without mentioning my discussion of the problems concerning the gamma coefficient or the MCA method.

The main point, then, is that I see the political world and the citizens' place in it as complex, making it necessary to use a whole 'forest' of measures of political activity as well as socio-structural and attitudinal characteristics, investigated with the help of a varied set of statistical methods. Lafferty selects a few trees from this forest of measures and methods – accidentally those which suit his argument best?

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Let us take a closer look at the most central of these arguments.

### (1) *The measures of political participation*

Two of them, comprising the seeking of information about politics and personal political communication, are not mentioned at all. I see such activities as important elements of the political process. A third measure – an index of direct political action covering such activities as writing about political matters in the local papers, participating in demonstrations, supporting petitions, contacting public officials, taking up a question in an interest organization and similar activities, is discarded because only 12 percent of the population have been active in these and similar ways. Seen in the light of democratic theory, this is a most important fact calling for research. We do not stop researching criminality because only a small minority of us are criminals in the usual sense of the word.

Simpler statistical methods can tell a lot about the distribution of direct political actions, for instance that among citizens with a low level of education such activity increases from zero among housewives with children at home to 20 percentage points among persons with an independent work position. In the group of highly educated people, the increase is from zero to 47 percentage points for the same two groups, with subordinate occupations from 14–20 percentage points, and senior officials with 25 percentage points (*The Distant Democracy*, page 57). This clear tendency holds when the respondents' sex is controlled for.

It is correct that housewives with no children (and no other occupation!) are left out of the analysis *when this particular occupational variable is used*. The reason is that they are too few to be representative – comprising only 8 percent of the sample and ranging from elderly widows to young girls temporarily out of the work force.

(2) *The relationship between social status and political participation*

As already mentioned, it is no secret that the correlation between my fourth dependent variable (representational activity) and most measures of social class position (particularly educational level) is weak. I have given reasons why this should be expected: a higher statistical probability that people in small communities are engaged in various forms of political representation combined with the fact that most of them have a low educational level and that in general such activities are monopolized by the political parties. It is therefore no surprise that Lafferty in his own material finds insignificant correlations between electoral activity and social status characteristics.

It could be mentioned that in my national sample, representational activity increases from almost zero percentage points among housewives, over 15–20 percent among blue and white collar workers, to 25–30 percent among the independents, regardless of educational level. The other four participation variables are correlated with the indexes of social class position to various degrees and with various curve shapes.

(3) *The relationship between organizational membership and political participation*

Lafferty is right that I find membership in organizations as the most important political resource and an alternative road to political influence for the lower classes. I cannot see that I have failed to comment on the problems which the use of this variable raises:

- a. By the special construction of the index of organizational membership, I take care of the fact that membership in interest organizations must be distinguished from membership in other voluntary organizations.
- b. I am not 'forced to admit' that the relationship between organizational membership and organizational activity is largely tautological – I simply take it for granted that membership in an organization is a condition for being active in an organization.
- c. The same goes for the relationship between membership in organizations and representational activity, although most components of this participation index are *not* party-dependent. And to repeat: in this connection I do not imply anything about causes, but restrict myself to phrases like 'the significance of' and 'dependence on'.

In any case, it still holds that members of organizations – especially parties and interest organizations – are most actively seeking political information, discussing politics, and taking part in political actions. I maintain that organizational membership must be regarded as an important political resource, in the same way as formal political rights are – for people who are members of the Norwegian electorate.

Even in the footnotes Lafferty tries to create the impression that I am not quite aware of what I am doing in the analysis. This concerns my use of the gamma coefficient, which I chose on exactly the grounds Lafferty mentions in note 2 of his article: its ability to register monotonous curvilinear associations. In appendix III I warn against its weaknesses, presenting six figures illustrating the sizes of gamma for varying types of relationships. However, I do *not* feel that the best impression of my data is provided by the gamma coefficients, but by more accurate representations of the co-distributions of the variables. In appendix III I also discuss the

cautions necessary when using the MCA technique. I do *not* maintain that the eta coefficient does measure only linear correlation, only that the MCA program is insensitive to interaction effects, which is a somewhat different matter.

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Another central argument in Lafferty's paper is that civic attitudes do not decisively mediate the relationship between status and participation. Here he does not refer to the results of my study, which shows that they do, but relies exclusively on his own results. I will not take up a discussion of his measures of civic attitudes as compared to those of *The Distant Democracy*, since the question of the representativeness of his study seems to me to be of prior importance. It is hard to decide to what degree Lafferty's data are indirectly representative of Norway as a whole. The three local communities chosen would, for instance, cover at most three of the cells in our own 38-fold stratification scheme for drawing a national sample of the voters.

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Given this fact, it also would be wise to postpone judgement on his third assertion: that 'sex is the most important "structural" determinant of political participation in Norway'. We all know that sex differences are extremely great for some forms of participation, but not all (among them voting). Little is said about the 'something' which could explain *why* this correlation is so strong. Could it be lack of political resources in combination with political alienation among women, as I suggested several years before the last wave of studies on this particular question? (*The Distant Democracy*, pages 160–161 and 200).

A further analysis of this problem is presented in Ola Listhaug and Ola Kindseth: *Sex, Resources and Political Participation: Direct and Indirect Effects*, presented as a paper to the 10th Nordic Congress of Sociology, Åbo, 1979.

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For some reason or other Lafferty has chosen to read *The Distant Democracy* in much the same way as the Devil reads the Bible. When the book was written around 1971, access to techniques for testing causal models was very limited, and I certainly do not operate with such a model as my point of departure. Furthermore, most relationships between social background factors, civic attitudes and political participation are probably dialectical or reciprocal, giving reason to be extremely careful in the construction of such models. Many of the assumptions one necessarily must make depend upon the type of political activity taken as one's dependent variable. Until better analyses are presented, I stick to the results given in *The Distant Democracy*, which paints a picture of political participation and its conditions as a multi-faceted and complex phenomenon.