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Boekbespreking van: A theoretical critique and synthesis applied to surveys from ten democracies

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Citation

Wolters, M. (1980). Boekbespreking van: A theoretical critique and synthesis applied to surveys from ten democracies. *Acta Politica*, 15: 1980(2), 272-275. Retrieved from <https://hdl.handle.net/1887/3452085>

Version: Publisher's Version

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Note: To cite this publication please use the final published version (if applicable).

hadden voor de Beweging. Organisatorisch rammelde dan ook alles; uiteindelijk dreigde zelfs een faillissement. Schermerhorn en Banning traden meer als volkspedagogen op dan in 't vak doorknede machtspolitici. Hun oproep tot zedelijk reveil was daarvan een voorbeeld. Met hun opvattingen over huwelijk en gezin zou de huidige P.v.d.A. geen raad weten. Beslissend zal geweest zijn dat de zuilenstructuur door de bezetting niet wezenlijk in discrediet was geraakt. Voor zover er verzet werd gepleegd, geschiedde dat vaak via oude zuillijnen. Als de doorbraak quantitatief mislukte, kwalitatief lag dat anders: de doorbraak leverde de P.v.d.A. een sterk kader op.

Tot dusver heb ik samengevat. Bank biedt een verhelderend en bondig exposé, waar niet zoveel op valt aan te merken. Als ik toch wat kritiek heb, dan betreft dit een thema, dat Bank wel aanraakt maar niet verder uitwerkt. Ik bedoel de geringe zuigkracht, welke de N.V.B. op de arbeiders uitoefende. Men zou uit het boek kunnen afleiden, dat juist zij het waren, die het minst van partijvernieuwingen verwachtten. Het was het R.K. Werkliedenverbond, dat zich al in bezettingstijd tegen een fusie van de verzuilde vakbeweging keerde. Zowel Drees, als de raadsman der bisschoppen, pater Stokman, begrepen daaruit dat dit na de oorlog om een politieke evenknie zou vragen. Het blijkt, dunkt me, óók uit de aanvankelijke groei der C.P.N. Het harmonie-model dat de N.V.B. voorstond, oefende, lijkt wel, minder aantrekkingskracht uit dan de oude klassenstrijd der C.P.N. Gelet op recente ontwikkeling in de P.v.d.A. zullen wij het er wel niet gauw over eens worden of dit nu van een progressieve houding of juist van een zeker conservatisme getuigde. Gezegd moet worden dat de toen nog onbekende Den Uyl in een schriftelijke reactie op het N.V.B. program er juist gemis aan socialistische strijdbaarheid in signaleerde. Aan de hand van de theorie van Lijphart over de horizontale en verticale scheilijnen der verzuiling had Bank daar op in kunnen, en moeten gaan.

Overigens: een knáp boékje!

N. Cramer

Ian Budge & Dennis Farlie, *Voting & Party Competition, a Theoretical Critique & Synthesis Applied to Surveys from ten Democracies*. London (Wiley), 1977, x + 555 blz., £ 16,75

In Budge & Farlie's *Voting & Party Competition* a descriptive, explanatory and predictive theory of electoral behavior is presented and tested. The specific purposes of the authors are the interpretation of opinion polls and their use in the prediction of voting and party competition, with the self-imposed restriction that such surveys should not simply tap voting intentions but such more basic variables as are of theoretical value (long-term predispositions, campaign issues and candidates).

I must confess that, originally attracted by its approach which is close to my field of interest, I have fought my way through the book with growing aversion. The work suffers from an indistinct presentation (no informative table of contents, no clear line of thought, ambiguous or obscure sentences), and more basically, the theory is not a theory, and its test deficient.

Let me illustrate these points with the help of page 238 which contains part of a summary of Budge & Farlie's theory (under the heading *Full Assumptions of the*

Synthesis of theorie about Voting and Party Competition based on a General Party-defined Space). I quote:

1. *The polity is democratic in the sense that the single party or coalition of parties which receives most votes in the previous election controls government, electors are fully enfranchised and count equally, results of elections and freedom of political activity are respected, elections are regular and contested by at least two parties.*

Probably nobody had objected if the authors had stopped after the fourth word of the sentence. However, as things are now, most multi-party polities (such as The Netherlands, Belgium, Italy) are excluded by the immediate link demanded between plurality of votes and control of government. The same can be said for Congressional elections in the U.S.: these have no direct impact on government composition at all. Apparently, the authors were heavily biased towards the Westminster system when they made up their assumptions. It is therefore inconsistent to find tests based on Congressional elections, or elections in The Netherlands, Canada or Germany, or the 1968 U.S. presidential election with Wallace as a third party candidate.

I quote again:

2. *Elector's propensities towards given 'parties' can be adequately represented by their position in a space consisting of a vector $(p_{i1}, p_{i2}, \dots, p_{im})$ where there are m parties subject to a normalizing restraint $\sum p_{ij} = 1$. Each entry in the vector (p_{ik}) is drawn from the probability distribution of each 'party's' adherents (II_{5k}) over the combined space of background characteristics and issue-preferences. It consists of the ratio of the probability of an elector of the j th 'party' and k th 'party' being at the position of an elector with a particular set of characteristics and preferences. The electoral 'parties' include non-voters.*

These few lines illustrate, of course, the ambiguity and obscurity in Budge & Farlie (ab)use of the English language. Not a space but *their position* consists of a vector; not m parties but a vector is subject to a constraint; *the ratio* is not that of *the probability* (...) and *kth 'party'*; *the electoral 'parties'* do not include non-voters, except for the one 'party' that includes all non-voters and none of the voters.

A (still) more serious objection against these lines is that several concepts are used without any indication of what is meant. The *probability distribution of each 'party's' adherents over the combined space of background characteristics and issue-preferences* is such an obscure key concept. Here the authors may object that I am quoting a summary. They would be right, of course, but neither the table of contents, nor the Introduction, nor the Index provide any clue as to where in the 555 pages of the book an explanation can be found.

This brings me to my most fundamental objection. The quotation refers in fact to the introduction of the LiRaS technique in a section of the chapter on the Social Group Theory of Voting, but is here presented as a basic theoretical assumption. Now, theory and technique should be distinguished in any scientific publication. That electors' propensities can be adequately represented by positions in a space is of no more theoretical value than the contention that the moon can be represented by an Edam cheese.

There is, of course, an element of theory involved in the quoted lines: electoral votes depend on electoral background characteristics and issue-preferences. But there is nothing new or controversial in this statement and, besides, it is not tested

(what can be concluded from the tests in this respect, is that the relationship between voting on the one hand and characteristics and preferences on the other is quite stable over time).

The third and fourth assumptions are:

3. *Within this space the probability of electors voting for a scheduled alternative is given by their relative distance from the party end-points.*

4. *Distance in the space is estimated by Euclidean distance in the space $\log S_1, \log S_2, \dots$, where S_1, S_2, \dots are scores locating electors in the likelihood space.*

Assumption 4 tells us that the authors do not use the space they claim to use in Assumption 2 (but a log derivation of that space); Assumption 3 is (given what is communicated in Assumption 2) a tautology (but useful as Assumption 2 is put in understandable language). Both are technical statements rather than theoretical assumptions.

5. *Party end-points are the limiting positions of the party defined likelihood ratio space corresponding to complete identity with a party (including non-voters).*

Just a minor point is that 100% probability to vote for a party is here expressed as *complete identity with a party*. The interesting aspect of the fifth assumption is that it secures Budge & Farlie's theory to be maximally uninformative, for two reasons. The first is that the spatial representation of m alternatives (here: 'parties') as an m -dimensional figure is mathematically uninformative since it imposes no restrictions whatsoever on the alternatives and their interrelations: it is such restrictions that would constitute a formalized theory.

The second reason is that the use of party labels as end-points of the dimensions of the space has not theoretical meaning. This refers to the author's discussion of *Party-Defined Spaces* as against *Party-Inferred Spaces* and, especially, *Policy-Defined Spaces*. In the latter, party positions are defined in terms of policies; in *Party-Inferred Spaces* party positions are at least defined in something else (though it can only be inferred in terms of what they are defined), but in *Party-Defined Spaces*, parties are the defining rather than defined elements.

It should be noted in passing, that Budge & Farlie do not really have all parties in a polity define party end-points. In multi-party systems, parties are grouped into tendencies; for example, in the case of The Netherlands they do not discriminate between a vote for BP and one for VVD — even though these may have dramatical impacts on the outcome of the election in terms of coalition building.

6. *A mean position of parties and non-voters can be estimated within party-defined likelihood space through the average value of likelihood vectors where the average taken over party scores ($S_{1i}, S_{2i}, \dots, S_{ki}$) are $S_1, S_2, S_3, \dots, S_k$ where $S_j = \sum(S_{ji})/N$ and N is the number in the party or the number of non-voters.*

We are now dealing with a 'mean position of 'parties'', which is not a 'mean position of party end-points' which would be a redundant concept. It turns out that 'party' is now defined in terms of its 'adherents' the authors mean all electors to the degree (probability) that they incline to the party or simply all electors that actually vote for that party in a certain election (but if it is the latter, what is left to be explained?).

It now seems a bagatelle that we have (in Assumption 2) electors' position $\{p_{ij}\}$, (in Assumption 4) scores locating electors $\{S_i\}$ and (in Assumption 6) party scores

$\{S_{ji}\}$ referring to aggregated electors.

The trouble with a study like this is, that one can only show that it is of little value by quoting the book in full. Taking the risk of being biased in my selections, I highlight just a few topics from the rest of the book.

In various chapters something called *path of party end-point* is discussed; it is even analyzed empirically in chapter 10. In wish to point out that it is a nonsensical concept in a party-defined space: since party end-points are the defining elements of such a space, in relation to what could they ever move and follow a path?

On page 284 the authors claim that they are 'using our theory to predict its own performance'; in other words, they have their theory predict under which circumstances it will yield wrong predictions. Such a theory can only be rejected as a theory that is immune for testing. But it should here be stressed that it is not even an immune theory. It all boils down to the claim that long-term predispositions that have done well so far, will do equally well in predictions of the outcome of the next election. Predispositions are selected on statistical (not: theoretical) grounds; their performance is measured in a statistical and not in a theoretical sense.

Menno Wolters

M. P. C. M. van Schendelen, *Regering en parlement in crisistijd*. Alphen aan den Rijn (H. D. Tjeenk Willink, 1979, pp. 82)

'Regering en Parlement in Crisistijd' valt in tweeën uiteen. Het eerste deel bevat een case-study van de Machtigingswet 1974. Aan de hand van een doel/effectiviteitsanalyse wordt nagegaan wat het kabinet-Den Uyl met de Machtigingswet voor ogen stond en waartoe dat heeft geleid. Het tweede deel bevat enkele algemeen-theoretische beschouwingen over methodes om wetten te analyseren. Mede op basis van deel I wordt de daarin toegepaste methode verworpen en een voorkeur uitgesproken voor de effectbenadering. Deze methode zou meer open en ontvankelijk zijn voor de multivariabiliteit, de complexiteit en veranderlijkheid van het analyse veld. Of zij, toegepast door dezelfde auteur, tot andere resultaten zou hebben geleid, is moeilijk na te gaan. Wat dat betreft bijt het boek zichzelf in de staart. De eerste methode wordt na beproefd te zijn, verworpen. De tweede methode wordt, zonder op dezelfde case te zijn uitprobeerd, aanbevolen.

Als politieke actor die in het analyseveld een verhoogd activisme aan de dag heeft gelegd, is het mij opgevallen dat niettemin beide delen eindigen met een niet nader bewezen hypothese.

In deel I ontwikkelt Van Schendelen op basis van een grote hoeveelheid feitenmateriaal de stelling dat van de olienood een dubbele deugd is gemaakt. 'Ten eerste is de olieproblematiek een potentieel excuus voor falend beleid. Ten tweede is het een hefboom om via extra bevoegdheden alsnog inkomens te spreiden. De oliecrisis, aanvankelijk (wellicht) ervaren als een bedreiging, wordt spoedig begrepen en gebruikt als een faciliteit' (p. 23).

Menig wetenschapsbeoefenaar zal, na lezing van zo'n passage, een gevoel van beroepstrots niet kunnen onderdrukken. Het is toch maar een goede zaak dat wij zulke vlijmscherpe, geavanceerde analysetechnieken hebben ontwikkeld, dat heel dat Haagse rollenspel tot op het bot kan worden ontleed en ontmaskerd. Hoe koud is echter de douche die daarop volgt: 'De juistheid van deze interpretatie kan niet