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Onderzoek

Facing another gap: An exploration of the discrepancies between voting turnout in survey research and official statistics

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1. Introduction

Debates about the alleged gap between people (i.e. voters) and politics (i.e. politicians in executive-legislative bodies) have dominated the discussion about the voting turnouts in recent Dutch elections. Relatively low turnout figures, especially in non-national elections, have caused concern about the level of political involvement of Dutch citizens and the legitimacy of contemporary democracy. Proponents and opponents of the idea of a widening divide between the (wo)man in the street and professional politicians have ventured their opinions on this theme on numerous occasions.

Remarkable about this discussion is the division of stances taken: practically involved people like politicians and journalists tend to take the widening of the gap for granted, whereas the 'detached' scientific community claims that there are no evident signs of a widening gap. The two sides hold different views on the basis of different sources. Politicians and journalists claim to 'feel' and 'see' everywhere around them that the interest in politics is decreasing. Political scientists base their scepticism in this regard mainly on the findings of the Dutch National Election Studies and the reports of the Social and Cultural Planning Agency (SCP), in which no indications of a decreasing political involvement of voters (i.e. people eligible to vote) seem evident.

Comparing these sources of 'evidence', political scientists apparently have the better case. Isn't the hallmark of science that knowledge is acquired in an objective and systematic way? On the other hand, opinions based on subjective and occasional observations are thought to do a much poorer job. However, scientific knowledge only deserves this supposed superiority over 'common sense knowledge' if it is actually based on impartial and systematic observation. But instead of assessing critically the value of their findings, scientists often all too readily embrace the results of their research. It turns out that the crucial issue of the reliability and validity of survey research has been neglected by and large by the same scientific community. Survey findings

are used uncritically to maintain that political involvement has increased in recent years, despite lower turnout figures in elections and 'gut feelings' of people of non-scientific blood.²

One of the most conspicuous flaws of these election surveys are the voter turnout figures compared to the official turnout statistics. Not only is reported turnout in surveys substantially higher than in the electorate, but this gap has widened over the years. As political involvement is to some extent related to turnout, methodologically it seems not correct to draw inferences about the political involvement of the electorate on the basis of these survey results. With reported turnout percentages in surveys that have reached levels that are about 12% higher than the official statistics, a claim to representative research findings can no longer be sustained.

In this paper the turnout figures in the Dutch National Election Studies (DNESs) will be scrutinized with respect to the possible causes of their overrating. Furthermore, a rather unconventional method is tried out to correct for the overreported turnout. If this method works it could be a preferable alternative to weighting the sample, which poses other problems to the validity of the research. To begin with, in section 2 voting turnout in the official statistics and the Dutch elections studies between 1971 and 1994 are compared, while in section 3 some alleged causes of overreported turnout in surveys in general are discussed. Then, in section 4 attention is directed to the problem of nonresponse in the Dutch National Election Surveys, which is supposed to be the main cause of the difference between turnout in the official statistics and these surveys. In the research literature at least three approaches other than weighting have been used to correct for nonresponse in the sample. These methods are explained in section 5 and one of them, the so-called difficulty approach, is singled out to apply with respect to the national election surveys. The division of respondents into distinct respondent groups according to this method is shown in section 6. In section 7 the results of the comparisons between respondent groups with respect to their reported turnout and other measures of political involvement are reviewed. Unfortunately, the conclusion has to be drawn that the proposed method does not allow to make meaningful corrections for the reported turnout in the DNESs. In section 8 some tentative answers are given for the patterns found, while section 9 ends this paper with some conclusions.

2. Voting turnout in official statistics and surveys

A discrepancy between voting turnout in the official statistics and reported turnout in surveys has always plagued electoral research. From the first election studies that were based on nationwide random samples of the popula-

tion onwards, voting turnout has consistently been overrated (see for American surveys for example Campbell, Converse, Miller and Stokes 1980 [1960]; Katosh and Traugott 1981 and Sigelman 1982; for the United Kingdom Swaddle and Heath 1989; and for Swedish studies Granberg and Holmberg 1992).

In the Netherlands, too, a discrepancy has always existed between the reported turnout in the DNESs and the official statistics. In the first six elections studies after the abolition of compulsory voting this overrating never exceeded 8%, but in the latest two election studies the gap has widened to about 13%. Whereas turnout in the electorate was relatively low in 1989 and 1994, the reported turnout remained at a high level. To show these changes the official and reported turnout percentages in the parliamentary elections between 1971 and 1994 are presented in table 1.

Table 1: Official and reported voting turnout in Dutch elections 1971-1994

	1971	1972	1977	1981	1982	1986	1989	1994
official turnout	79.1	83.5	88.0	87.0	81.0	85.8	80.3	78.7
reported turnout	86.1	89.4	91.1	93.5	89.0	93.1	92.6	92.2
absolute difference	+7.0	+5.9	+3.1	+6.5	+8.0	+7.3	+12.3	+13.5
relative difference	33.5	35.8	25.8	50.0	42.1	51.4	62.4	63.4

This table points to two related phenomena: in general, the higher the official turnout, the lower the absolute difference between this percentage and the reported turnout in the survey. This is due to a ceiling effect. If the overall turnout is high (as was the case in 1977, 1981 and 1986), it is more 'difficult' to reach a high overreporting in the sample. In contrast, if the official turnout is relatively low (as was the case in 1989 and 1994) it is easier to reach a high overreporting of turnout in the sample. The elections of 1971, 1972 and 1982 do, however, not conform to this rule. In the next sections of this paper I will examine these figures in more detail and give some explanations for them.

3. Causes of overreported turnout in surveys

Although the extent to which voting turnout in the DNESs has been overrated shows considerable variation, it is clear that without exception some overreporting always occurs. This is a suspect outcome from the viewpoint of sampling theory: if the sample were truly representative of the population,

the reported turnout would deviate from the population both in a positive and negative direction.

At least three explanations may account for the persistent overreporting of turnout in election surveys, namely 1) response bias, 2) misreporting, and 3) the so-called 'Hawthorne' or stimulus effect. Response bias means that respondents in the survey are not representative of all the eligible voters. In this case some self-selection among respondents occurs in their choice to participate in the survey. Misreporting denotes the phenomenon that respondents do not accurately report their true behaviour. With respect to turnout this nearly always implies that people who have not voted in the election maintain that they did perform the act. Finally, a less well-known source of error with respect to overrated turnout in election surveys is the stimulus effect of the pre-election interview. As respondents are asked about their political attitudes and behaviour at length, especially with respect to the coming election, the interview may stimulate them to vote in this election.

Response bias because of self-selection of the respondents may be the result of several causes. Two factors seem to be especially important. Firstly, people who are more interested in politics are usually more willing to participate in an interview on this subject than people who are not that interested in the subject matter at hand. Secondly, people who are more educated and/or integrated in society may be more willing and adept at answering questions about politics than less integrated people. This is so because being interviewed at length by a unknown interviewer requires some communicative and cognitive skills that are not always present in certain segments of society. Hence, people who are not interested in politics and who are less integrated in society will probably participate at a lower rate in an interview about elections. Consequently, voting turnout in surveys is biased in a positive direction because respondent groups that vote at a higher rate than others are overrepresented.

Misreporting of turnout in a positive direction may account for another part of the gap between reported turnout in surveys and the corresponding official statistics. Apparently, the norm that a 'good citizen' ought to vote in a democracy is still so strong that it is difficult for non-voters to admit in an interview that they did not fulfil their citizen duty. The extent to which this norm is adhered to varies, however. In higher status groups this norm holds more vigorously than in lower status groups, which are less integrated in society. The fact that higher status groups report the highest turnout in surveys may be partly caused by this 'norm abiding' effect: for a voter of higher status it is more difficult to admit that he or she did not vote than for a voter from a lower status group (Smeenk 1994). In this respect Calahan states that the self-image of the voter is also at play: 'If a respondent has a self-image of being a public-spirited citizen, he would be more likely to

exaggerate his behaviour in a prestige direction.' (Calahan 1968: 611) This implies that respondents sometimes lie about their electoral behaviour because of self-esteem. Calahan's research indicated that younger voters, who are the least likely to vote, were most honest about their nonvoting; however, of those younger voters who maintained that they had voted, a larger part tended to lie about their behaviour than of the older voters (Calahan 1968: 621).

After combining eleven Swedish parliamentary election studies (held between 1956 and 1988) and comparing the reported turnout to the official records, Granberg and Holmberg found that 92% of the voters interviewed were true voters, 2% were false voters, 5% were true nonvoters and 1% were false nonvoters (people who said that they had not voted, although the records indicated that they did). Among voters the percentage that told the truth was very high (more than 99%), but only 74% of the nonvoters admitted that they had not voted. Hence, more than a quarter of those voters who did not vote in a particular election were not honest about their turnout behaviour (Granberg and Holmberg 1991: 450-451). Interestingly, voters with the highest educational level were the least likely to admit that they had not voted. But it also appeared that people with a high interest in politics were less honest about not having voted than people who were less interested in politics. Similarly, people who claimed to be an adherent of a party were less likely to acknowledge nonvoting than people who had no or only a small preference for a party. Thus, not only membership of a higher status group, but also attitudes that are favourable to voting, make it more difficult to admit that one has broken the norm to vote. According to Granberg and Holmberg this result can be interpreted as follows: '(...) the norm prescribing that people ought to vote is implanted, in varying degrees of strength, more strongly among people who are more predisposed to vote. Nonvoters may have broken a norm, but it may be easier for people not strongly predisposed to vote to acknowledge not having voted, since they are less committed psychologically to the norm.' (Granberg and Holmberg 1991: 453)

This effect seems particularly evident if the stated intention to vote is compared to the honesty of reported turnout. Respondents who stated an intention to vote in the pre-interview were less likely to be honest about nonvoting than those respondents who were undecided at the time of the first interview, or who declared that they were not going to vote in the coming election (Granberg and Holmberg 1991; see also Silver, Abramson and Anderson 1986).

A third cause of the biased turnout in surveys in an upward direction is the so-called 'stimulus effect'. Clausen has signalled that the reported turnout was lower in surveys that consisted only of a post-election interview. He estimated that in the American presidential election of 1964 about 6% of the

15% overreporting in a particular survey was caused by the stimulus effect of the pre-election interview³ (Clausen 1968: 595-596). Granberg and Holmberg found that voters who were interviewed before the election voted at a (validated) rate of 95% versus 93% of those who were interviewed after the election. These figures were based on six elections in which the official voting turnout varied between 86.0% and 91.8%. The 2% difference in the Swedish context might seem very small, but a ceiling effect is present (Granberg and Holmberg 1992: 243). Actually, the relative size of the stimulus effect was about 29% in the Swedish case versus 21% in the American case.⁴

If we consider table 1 again, it is obvious that the rate of overreported turnout was relatively low in 1972 and 1982. These are precisely the two studies in which no pre-election interview was conducted. (The elections of 1971 form an exceptional case in that these were the first elections after the abolition of compulsory voting. A lot of factors may account for the relatively low overreporting of voting turnout in that year.)

Clausen has also hypothesized that the stimulus effect was larger for people who have no or only a limited interest in politics compared to people who are more interested and informed about politics (Clausen 1968: 604). Indeed, Granberg and Holmberg found that '[t]he stimulus effect was 7.5 percentage points among people who were not at all interested, compared to 2.6 for those who were not especially interested, 0.7 for the quite interested people and 0.2 for those who were very much interested in politics. The difference in turnout between people interviewed before and after the election was significant for each of the two lower interest categories ($p < 0.01$) but not for the two higher interest categories ($p > .05$).'⁵ (Granberg and Holmberg 1992: 244)⁵

The overreported turnout in the DNEs may be attributed to a combination of the above mentioned causes. Since no voter validation studies can be conducted in the Netherlands (unlike Sweden, the United Kingdom and the United States), no distinction can be made between self-selection and the stimulus effect on the one hand, and misreporting on the other hand. It is obvious that these sources of overreporting pose a threat to the external and internal validity of the election studies.

If self-selection occurs among the respondents in the gross sample, the answers of the respondents in the net sample will not be representative of all voters. Generalizing the sample results to the Dutch electorate may lead to unwarranted conclusions. Misreporting and a stimulus effect threaten the internal validity of the research, because they can lead to erroneous conclusions about the determinants of voting. Correlations that are found in the sample do not adequately reflect relationships in the population. Quite rightly, Granberg and Holmberg warn against the bias introduced into the sample as a consequence of the stimulus effect: 'If the Hawthorne effect oc-

curs disproportionately among people who are for a variety of reasons not predisposed to vote, then the observed strength of the relationships in the sample would most likely underestimate the true strength of the relationship in the population.'⁶ (Granberg and Holmberg 1991: 245-246).

In the next sections of this paper, the reported turnouts in the DNEs will be studied in more detail. We will try to answer the question what explanations may account for the discrepancies between the official turnout statistics and the reported turnout of the respondents in the election surveys.

4. Rates and causes of nonresponse in the DNEs of 1971-1994

Ever since survey research and election studies have been performed, non-participation of a part of the intended respondents from the gross sample has posed a problem. Moreover, most of the DNEs-surveys have been structured as panel studies, which implies that nonresponse can occur both in the pre-election and post-election interview(s). In general, nonresponse in face-to-face surveys has increased over the past decades. On the basis of a meta-analysis of 45 studies in various countries in Western Europe and the United States Hox and De Leeuw have concluded that the response in face-to-face interviews has fallen from a mean of 82% in 1947 to a mean of 65% in 1992 (Hox and de Leeuw 1994: 338).

Table 2 shows the response rates of all the DNEs in the course of the years. The percentages are not wholly comparable because of the varying study design over the years.⁶

Table 2: Response rates DNEs 1971-1994: pre-election interview and/or post-election interview

DNEs	1971	1972	1977	1981	1982	1986	1989	1994
pre-interview	74.9		70.2	69.3		58.8	46.1	47.5
post-interview	59.5	68.2	54.3	54.5 48.7	62.3	49.1	39.5	40.0
gross sample	3,330	2,237	2,642	3,326	2,472	2,772	3,808	3,816

Notwithstanding some differences in study design, the rate of nonresponse seems to have grown over the years in the DNEs as well. Relatively speaking, the response rates were reasonably high from 1971 until 1981. From 1982 onwards the nonresponse rate has grown fast. This is especially clear in

the election studies of 1989 and 1994 in which less than half of the respondents in the gross sample agreed to participate in the survey.

There is no easy explanation as to why nonparticipation in the surveys has shown such a dramatic increase in the most recent elections. Part of the explanation might be found in the routines of the agencies that were responsible for the fieldwork of the surveys. The fieldwork has been done by five different organisations, the most recent being the Dutch Central Bureau of Statistics (CBS) that carried out the 1989 and 1994 surveys.⁷ Throughout the years different sample bases have been used, comprising the electoral registers of municipalities (1971), households (1972), a so-called 'minicensus' of households (1977), mail delivery registers (1981, 1982, 1986) and a geographic base register (1989 and 1994).⁸ Also, the manner of addressing the households or persons in the sample shows some variation. In the first studies an interviewer would visit the sampled households without prior notice, whereas in later studies a letter of introduction was sent to the households before they were visited. Besides, the various wordings of these letters may have produced different stimuli to participate in the study.

Before we speculate any further about the causes of the nonresponse, it is instructive to study table 3, which lists the main causes of nonresponse in the DNEs.

Table 3: Causes of nonresponse in the DNEs of 1971-1994 (in percentages)

DNEs	1971	1972	1977	1981	1982	1986 ⁹	1989	1994
no contact	?	27.5	17.5	25.7	17.7	17.6	15.6	9.3
refusal	?	?	82.5	57.3	60.5	70.7	72.3	81.9
other causes	?	?	-	17.0	21.8	11.7	13.0	8.7
total		100.0	100.0	100.0	100.0	100.0	100.0	100.0
number of cases	805	491	872	1,021	931	1,142	2,054	2,004

In every election study a pertinent refusal to participate is the most important cause of nonresponse. The percentages that are presented are, moreover, low estimates. Reasons like 'no time' are coded in a separate category (with the exclusion of 1986). 'No time' may be a refusal in disguise, however. The same might apply to a part of the respondents who were not at home. If we neglect the DNEs of 1977 (because of the incommensurability of the coding categories), it appears that pertinent refusals have increased with 10% to 20% from 1982 onwards.

The question is whether this increasing percentage of refusals signifies that the politically interested citizens participate in the interviews at a higher rate? Not necessarily so. Only if these figures are significantly higher than

those found in other survey research, one could draw such a conclusion.

Recently, methodologists and social researchers in the field have voiced concern about the high rate of nonresponse in Dutch surveys in general (De Heer and Israëls 1992; NIMMO-report 1994). Interview aversion seems to be ubiquitous in the Dutch population and is much higher than in some other European countries.¹⁰ In a study on the causes of nonresponse in Dutch surveys, experts have suggested a lot of possible causes. Apart from reasons like absence or lack of time, a lack of interest in surveys in general, a lack of interest in the subject matter of the survey, feelings of privacy or distrust, and response overload were mentioned relatively frequently as alleged causes of nonresponse (NIMMO-report 1994: 21-24). Hence, although a lack of interest in the topic of the survey is seen as an important cause of nonresponse, it seems premature to conclude that the high level of nonresponse in recent election studies is to be accounted for solely by a lack of interest in politics among the general public (cf. also Hox and De Leeuw 1994).

Whether the figures in table 3 point to a lack of interest in surveys in general, no interest in the topic of the survey, interview fatigue, or some other cause remains unclear. This inability to draw final conclusions might be illustrated by comparing the voting turnouts in the electorate with the causes of nonresponse in the corresponding election studies. For example, the year 1986 was characterized both by a relatively high voting turnout and a relatively high percentage of explicit refusals to participate in the interview. On the other hand, in 1989 and in 1994 relatively low voting turnouts went together with a relatively high nonresponse. These two examples may be explained by interview aversion or fatigue on the one hand and a lack of interest in the topic of the survey on the other hand.

Although these two tables do not provide any conclusive answers, it is plausible, however, to hypothesize that part of the nonresponse might be attributed to a decreasing interest in elections and politics. The relatively low voting turnout in various elections from 1989 onwards, together with the steady decline of the percentage of Dutch voters who are a member of a political party, may be indications of a reduction of political interest in the Dutch electorate.

5. Some approaches to correct for nonresponse bias

With respect to nonresponse, the adage that prevention is better than cure is applicable. Nevertheless, the problem of nonresponse is always part of survey research. An assessment of the nonresponse bias and a correction for nonresponse after the survey are the only means possible to keep the damage within bounds. Over the years a number of methods have been de-

signed to estimate the characteristics of nonrespondents as well.

Making corrections for nonresponse by weighting the data is the most commonly used method. The application of weights is not without problems, however. The well-known danger of 'capitalizing on chance' poses a serious threat to the validity of the weighting procedure. If some subgroup is underrepresented in the sample – in this case the voters that reported not to have voted in the parliamentary elections – every respondent from this subgroup will get a weight that is larger than 1. In the 1994 survey 7.8% of the nonvoters represented 21.3% of the electorate. Thus, a correction by weighting entails that the non-voting respondents have to get a mean weight of 2.7 to equal the proportion of nonvoters in the electorate. If however, the nonvoters in the sample are not representative of the nonvoters in the population a mean weight of 2.7 can seriously distort the outcomes.¹¹

For these reasons we will not use a weighting procedure to correct for nonresponse and the related problem of overreported turnouts. Instead, we will review some other methods that have been proposed in the research literature that do not have the aforementioned drawbacks and try out one of them. If one of these alternatives works it might be a desirable alternative to using a weighting model.

Apart from weighting the sample data on the basis of aggregate statistics three other methods have been suggested in the research literature, namely: 1) the Politz-Simmons adjustment; 2) extrapolation based on difficulty; 3) conversion adjustments (Smith 1983: 387). These three methods all use data of respondents to estimate characteristics of nonrespondents.

In the Politz-Simmons or 'times-at-home'-approach a correction for nonresponse is based on the number of times respondents were at home at the time of the interview during the last *x* number of days. This frequency is taken as their probability of being available to be interviewed. Respondents are then weighted according to the inverse of the number of days they were home in the *x* days preceding the interview. Thus, respondents with the lowest probability of being interviewed are given the largest weight. This method presumes that nonresponse is caused by absence and that weighting according to availability adequately adjusts for nonresponse bias. With the causes of nonresponse in the DNESs in mind it is obvious that this presumption does not hold in these surveys.

A more promising method to adjust for nonresponse is the so-called 'difficulty approach'. Here, some measure is constructed to indicate how difficult it was to have an interview with the selected respondent. Then it is determined whether certain variables are related to this difficulty: 'If a linear or some other regular relationship is found, then this association is used to impute the distribution of the variable among the nonrespondents.' (Smith

1983: 391) This method departs from the assumption that 'difficulty' is related to final nonresponse. However, it is clear that if the final nonrespondents differ from 'difficult' respondents this procedure, too, makes a wrong estimation of the characteristics of respondents who have been included in the gross sample but are not in the net sample.

The third method makes use of converted refusers to estimate characteristics of nonrespondents. In this approach the converted refusers are seen as substitutes for final nonrespondents, and this assumption is used to make an extrapolation. The first group consists of respondents who agreed to participate immediately, the second group consists of converted refusers, and the third group is made up of the final nonrespondents. Again, the appropriateness of the method rests on the supposition that the final refusers are similar to temporary refusers, or at least more similar to them than cooperative respondents.

Some research has already been done to measure the usefulness of these methods. Stinchcombe, Jones and Sheatsley (1981) concluded, for example, that it is not useful to revisit intended respondents that were not at home, on a second, third, fourth or further occasion. It turns out that their answers are more in line with the general population than those of final refusers. Hence, more efforts should be made in trying to convert initial refusers than in trying to interview (initially) absent respondents. The authors have concluded that: '... the accuracy of estimation of the nonresponse bias depends on the number of converted refusers who finally get interviewed.' (Stinchcombe, Jones and Sheatsley 1981: 375)

O'Neil (1979) has also made a comparison between the answers of respondents who immediately agreed to participate in a telephone survey and those who initially refused. To begin with, estimates of the population were computed on the basis of the outcomes of the cooperative respondents only. Then the estimates were computed on the basis of both the cooperative and the reluctant respondents. It turned out that the two groups differed considerably. People with lower education and lower socioeconomic status and people who participated less in social life were more likely than their counterparts to resist the first interview attempt. The author has combined his findings with research results of Dunkelberg and Day about the effects of callbacks on sample estimates. From their research it appeared that respondents who were contacted after several telephone calls belonged predominantly to the younger, higher educated and higher socioeconomic status groups. With successive callbacks, the response of these groups increased. Thus, it does not seem adequate to put respondents who initially refused to participate on a par with respondents who were absent at first. The following conclusion seems warranted: 'Comparing these findings with other research provides strong evidence that the effects of nonresponse on sample estimates

do depend on whether nonresponse is due to insufficient numbers of callbacks or to explicit refusals.' (O'Neil 1979: 229)

In the next section I will try to establish whether the DNESs provide opportunities to correct for the bias in voting turnout due to nonresponse, without weighting the data. To this end I will apply a variant of the difficulty approach. Three respondent groups will be distinguished: people who immediately agreed to participate (the cooperators), those who were initially absent or were detained by other causes (the difficult to reach), and those who initially refused to participate and had to be converted (the temporary refusers). The central question is whether these respondent groups differ with respect to their reported turnout at elections, while other indicators of political involvement are of interest as well. If the distinct respondent groups do differ significantly from one another in these respects, the DNES-data can be adjusted.

6. Distinct respondents groups in the DNESs

6.1 Readiness to participate in the DNESs of 1981, 1982, and 1986

Nonresponse has haunted the National Election Studies from the beginning. Apart from the first of these studies, the main causes of nonresponse have been documented. Less adequate documentation is available about the number of callbacks that have been made before a respondent could be interviewed. In most studies no information is available about whether serious attempts have been made to convert initial refusers to participate in the interview.

Fortunately, the 1981, 1982 and 1986 DNESs do offer some possibilities to make a comparison among respondent groups. In the DNESs of 1981 and 1982 a distinction can be made among 'cooperators', respondents who were 'difficult to reach' and 'temporary refusers'. In the DNES of 1986 a rough distinction can be made between cooperative respondents and those respondents who could be interviewed only after a second attempt (comprising the difficult to reach and the temporary refusers).

To begin with, in table 4 the respondents participating in the 1981, 1982 and 1986 DNESs are divided into 'first try' and 'second try' respondents. As the DNESs of 1981 and 1986 were panel studies, interviews have been conducted before and after the election. In 1982 only one post-election interview has been conducted.

From table 4 it is clear that the extra efforts to draw (intended) respondents into the survey were relatively successful in 1981 and 1982 compared to

Table 4: Readiness to participate in the DNESs of 1981, 1982 and 1986

DNES	1981	1982	1986
1st wave			
first try respondents	1,844 (80.0%)	1,167 (75.7%)	1,471 (91.2%)
second try respondents	461 (20.0%)	374 (24.3%)	159 (9.8%)
2nd wave			
first try respondents	1,779 (98.2%)	-	1,326 (97.7%)
second try respondents	33 (1.8%)	-	31 (2.3%)
3rd wave			
first try respondents	?	-	-
second try respondents	?	-	-

1986. In the former two studies a fifth to nearly a quarter of the net sample consisted of respondents who could be interviewed only after more than one attempt was made. This relative success might have to do with the more serious approach that was employed to interview additional respondents in 1981 and 1982.¹² (If only one attempt would have been made to select respondents, the response in the first wave of interviews would have been 55.4% in 1981, 47.2% in 1982 and 53.1% in 1986. Hence, the rather unsuccessful second try in 1986 cannot be accounted for by the greater success in the first try.) In the second wave of interviews, the extra effort was less successful. This is not surprising given the fact that respondents knew what kind of survey awaited them after the first interview.

Apart from a distinction between respondents who were 'easy' and 'difficult to reach', the causes that were responsible for the initial failure to participate in the interview were listed in the DNESs of 1981 and 1982. Refusals accounted for a majority of these causes: 53% in 1981 and 61% in 1982. (Unfortunately, in the DNES of 1986 this information is not available.)

Voting turnout is the central variable of interest. However, the tripartition (or division) will also be used to measure political attitudes and behaviour and other relevant characteristics of the respondent groups. With the help of this categorization into distinct respondent groups it is possible to find an answer to the question whether estimates of population parameters change when respondents are divided into 'easy' and 'difficult' interviewees. If 'difficult respondents' are (more) representative of the final refusers in the sample, who make up 30% to 50% of the voting population, the answers of the former group need to be weighted accordingly. When the groups differ the estimates of the population parameters will change.

The supposition of selective participation in the DNESs, which lies at the root of the comparison between respondent groups, implies that 'cooperators' are more involved in elections and politics in general than

'temporary refusers'. To the degree that respondents who were difficult to reach were really absent, they should be a cross-section of the population. If, however, their absence was merely a refusal in disguise, the difficult to reach should be more similar to the temporary refusers. If no differences can be found between these categories, in particular between 'cooperators' and 'temporary refusers', the converted refusers are unlikely to be representative of the final refusers in the sample. In that case, population estimates that are based on an extrapolation of the results of the distinct categories will not correct for nonresponse.

6.2 Readiness to participate in the first wave and panel attrition (1981 and 1986)

The research design of the DNESs has not always followed the 'classical' idea of one pre-election and one post-election interview, as became clear in the foregoing section. The data of the national election survey of 1981 are the result of a panel study with no less than five waves. In the light of this paper the first, second and third wave are the most interesting ones. The first wave of interviews was conducted in the months of January and February, several months before the start of the election campaign. The second wave of interviews was held during the election campaign, whereas the third wave consisted of interviews that were held immediately after the parliamentary elections of 26 May 1981. Once again, the response in the first wave amounted to 69.3%, 54.5% in the second and only 48.7% in the third wave of interviews.

In table 5a the respondents of the three waves of 1981 are grouped on the basis of their readiness to participate in the first wave, indicating the panel attrition within each group as well.

Table 5a: Totals of respondents and panel attrition within groups in the DNES of 1981

	cooperators	difficult to reach	temporary refusers	total
first wave	1,844 (100.0%)	215 (100.0%)	246 (100.0%)	2,305 (100.0%)
second wave	1,462 (79.3%)	169 (78.6%)	181 (73.6%)	1,812 (78.2%)
third wave	1,314 (71.3%)	142 (66.0%)	164 (66.7%)	1,620 (70.3%)

Of the 2,305 respondents who have participated in the first wave 80.0% (1,844) were cooperatives, 9.3% (215) were initially difficult to reach and 10.7% (246) were temporary refusers. It is interesting to note that the panel attrition in the second wave was nearly the same for cooperators and those

respondents who were initially difficult to reach. Temporary refusers dropped out at a slightly higher rate between the first and the second interview. In the third wave of interviews the cooperative respondents were slightly more likely to continue their participation than the other two groups that defected at a comparable rate. Nevertheless, the differences are not very large. The fact that the respondents who initially refused to participate in the survey have not dropped out at an particularly large rate, seems to be an indication that they are not fundamentally different from the respondents who were willing to cooperate from the start.

Thus, in the second wave the cooperative respondents' share has slightly risen to 80.7%, while the initially detained respondents make up 9.3% again, and the temporary refusers 10.0%. Due to the somewhat more uneven attrition in the third wave, the panel is at that point made up of 81.1% initial cooperators, 8.8% initially difficult to reach respondents and 10.1% initial refusers.

In 1982 only one wave of interviews was conducted, namely after the elections of 8 September. Because of the untimely fall of the Van Agt-II cabinet (a coalition of CDA, PvdA and D'66), no time was left to implement a pre-election interview. Of the 1,541 respondentent who participated in the survey (a response of 62.3%) 75.7% could be regarded as cooperative respondents, 9.4% as difficult to reach and another 14.9% were converted after an initial refusal.

The DNES of 1986 was designed as a two-wave panel study around the parliamentary elections of 21 May. Because the municipal elections were due on 20 March of that year, the first wave of interviews started in the last week of March, directly after these elections. This made it possible to ask questions about the municipal elections as well. The second wave of interviews was conducted directly after the parliamentary elections and was carried on throughout the month of June. Unfortunately, in 1986 the possibilities of distinguishing between respondent groups are limited; instead of three categories of respondents only two categories can be formed. On the one hand, the net sample consists of respondents who immediately agreed to participate in the interview; on the other hand, a part of the sample is formed by respondents who needed some additional effort to make them participate in the study. No further refinement of the latter category could be made.

In table 5b the two respondent groups of 1986 are categorized after their readiness to participate in the first wave of the survey. Again, it also indicates the extent to which panel attrition has occurred within these groups. It shows that panel attrition after the first wave of interviews was somewhat less among the cooperative respondents; consequently only 8.7% (instead of 9.8%) of the second wave was formed by respondents who initially refused to participate or were not able to participate.

Table 5b: Totals of respondents and panel attrition within groups in the DNES of 1986

	cooperatives	detained/converted refusers	total
first wave	1,471 (100.0%)	159 (100.0%)	1,630 (100.0%)
second wave	1,239 (84.2%)	118 (74.2%)	1,357 (83.3%)

7. Results of the comparison: Do the different respondent groups really differ?

If self-selection among respondents has occurred in the national election studies, the three groups are not similar on all variables. In particular, we expect that people who are more interested in party politics are more prone to participate in an interview on elections and politics in general than their non-interested counterparts. Hence, cooperative respondents as a group should be more politically involved than the respondents who were initially difficult to reach, while the converted refusers should be the least politically involved.

Furthermore, answering questions in an interview on political topics also requires some cognitive and communicative skills on the side of the respondent. Therefore, we expect that people who are more integrated in society, have a higher educational level or belong to higher status groups are more willing to participate in the interview. Hence, we expect again that cooperative respondents have higher scores on these variables than the respondents who were initially difficult to reach, while the converted refusers as a group should be least integrated or educated.

Thus, the three (or two) respondent groups have been compared on a set of variables that are relevant to our purpose. As an indication of the degree to which any of the relationships between respondent groups and these variables exist tau-c values have been computed. In table 6 the outcomes of these comparisons are presented.

It is immediately clear from this table that all tau-c values are very low, indicating that only weak, if any, relationships exist between the respondent groups and their political attitudes, political behaviour and other relevant characteristics. Notwithstanding these weak relationships, some significant differences do come up between the groups. They are relatively often present in the one wave, post-election survey of 1982, which contained the largest group of second try respondents.

As to the level of political involvement, in the DNES of 1981 and 1982

Table 6: Comparison between respondent groups in the DNES of 1981, 1982 and 1986; entries are tau-c values.¹³
Significance for one-tailed tests: * $p \leq .05$; ** $p \leq .01$; *** $p \leq .001$.

	81-1	81-2	81-3	82	86-1	86-2
political involvement and reported turnout						
reads about national news	.02	.00	.01	.04*	.00	
talks about national problems	.05***	.04**	.01	.05**	.00	
reads about foreign news	.02*	.01	.01	.04*	-.01	
subjective interest in politics	.01	.01	.02	.07***	.02	
objective interest score	-.02*	-.01	-.01	-.04*	-.00	
strength of party identification	-.03	-.00	-.00	-.08***	-.05**	
membership of a party	.01			-.01	.02**	
ever been member of a party	.01				.01	
voted in previous parl. elections	.00	-.00	-.00	.01		
voted in municipal elections					.02*	
vote intention coming parl. elections	.02*	.01			.02*	
voted in parliamentary elections			-.00	-.00		.00
read about campaign news						.02
watched election debate on tv						.03
number of campaign activities			-.03	.03*		.01
civic participation						
did (not) contact cabinet ministers			-.01	.00		.00
did (not) contact member of parliament			.01	.01		.01
did (not) sign a petition			-.00	.03		.02
did (not) try to activate interest group			.01	.04***		.00
did (not) try to activate radio or tv			.00	.01		.01
did (not) try to activate political party			.00	.03**		-.00
did (not) contact mayor or alderman			-.00	.01		.01
did (not) contact city councillor			.01	.01		-.00
did (not) join civic action group			.01	.04**		.01
did (not) join demonstration			.02*	.02		.01
did (not) try to activate newspaper			.01	.01		.00
did (not) lodge a complaint			.00	-.02*		.00
political attitudes						
political efficacy - item 1	-.02	-.00	-.02	-.05		-.00
political efficacy - item 2	-.04*	.01	-.02	-.05**		-.02
political efficacy - item 3	-.03	-.02	.00	-.04*		.00
political efficacy - item 4	-.01	-.02	-.02*	-.02		.00
political distrust - item 1			-.01			
political distrust - item 2			-.04*			
political distrust - item 3			-.02			
political cynicism - item 1			-.02			-.01
political cynicism - item 2			-.01			.01
political cynicism - item 3			-.03*			-.00

Table 6 continued

	81-1	81-2	81-3	82	86-1	86-2
Indicators of social integration and cognitive skills:						
marital status	7.20			6.62	2.82	
religion	13.07			7.36	1.22	
attendance of religious services	.04*			.00	.02	
employment status	.01			-.03	-.01	
degree of urbanisation	.03*			.04*	-.02	
education	-.04***			-.05**	-.02	
income of respondent's household	.02			-.02	-.01	
social class- self image	.01			-.00	.04**	
participation in unions		-.01		.01		.01
participation in profes. organisations		-.00		-.01*		.01
participation in neighbourhood organisations		-.01		-.01		-
participation in women's organisations		-.02		-.02*		-
participation in other organisations		-.03		-.01		-

the three respondent groups differ from each other in some respects. In the DNES of 1986 no such differences can be found, which may have been caused by the fact that only two respondent groups have been included. Inspection of the corresponding crosstabulations indicates that in 1981 cooperative respondents are somewhat more interested in politics than the other two respondent groups. In the third wave of interviews in 1981 these differences have disappeared, probably due to panel attrition. In the DNES of 1982 both the subjectively and objectively politically interested respondents are found to a larger extent among the cooperative respondents. The least politically interested respondents are found more among the initial refusers. The respondents that were difficult to reach take in a position somewhere in between.

With respect to strength of adherence to a party the respondent groups differ significantly from one another in 1982 and 1986. In both years the adherents of a party are found disproportionately among the cooperative respondents, which is all the more true for the convinced adherents. As to party membership, it turns out that only in 1986 cooperative respondents are more likely to be a member of a party than the other respondents (8.3% versus 5.3%).

Crucial with respect to the comparison between groups are the variables that relate to (reported) turnout at elections. A quick glance at table 6 indicates again that the results are rather disappointing. No differences are

present in the percentage of respondents that claim in retrospect to have voted in the (previous) parliamentary elections of 1977, 1981 and 1982.

In the first wave of 1981, the reported turnout in the elections of 1977 was quite similar in the three respondent groups and amounted to nearly 88%. The vote intention in the elections of 1981 was significantly higher, however, on the part of the cooperative respondents. The percentages were 95.4%, 91.9% and 93.8% respectively. This applies as well to the vote intention of the cooperative respondents in 1986 when they are compared to the less cooperative ones. Of the former group 95.6% stated an intention to vote, versus 90.9% of the latter group.

It is striking that no differences whatsoever can be detected in reported turnout of the groups in the latests parliamentary elections. In the DNES of 1981 circa 93.5% of the respondents in all three groups claimed that they had voted. In the post-election study of 1982, 89.5% of the cooperative respondents reported to have voted, a percentage that was exactly equal to that of the converted refusers, while 84.1% of the difficult to reach said they had voted. In 1986, notwithstanding the difference between groups in intention to vote, 93% of each group claimed that they had voted in the parliamentary elections of 1986. Notably, the intention to vote of the 'second try respondents' was lower than the reported turnout. On closer inspection it appeared that 15 of the 47 respondents who had previously indicated that they did not intend to vote, later reported that they had voted. With respect to reported turnout at the municipal elections of 1986, the cooperative respondents fared better again: 81.5% of this group reported to have voted in these subnational elections, compared to 75.5% of the other respondents.

The national election studies always include a set of questions about non-electoral participation as well. The answers to these questions on civic participation are another indicator of the political involvement of the respondents. Again, it is expected that cooperative respondents are more prone to engage in actions of this kind than respondents who were initially difficult to reach, or those who refused to cooperate at first.

Table 6 shows that, except for joining a demonstration, differences between groups only existed in the survey of 1982. (In 1981 cooperative respondents were somewhat more likely to have joined a demonstration than the other two respondent groups, whereas the converted refusers were least likely to have performed this kind of political action.) In the other two studies, cooperatives did not appear to be more prone to contact a politician, or to activate a political party or interest group, or engage in any activity of this sort.

However, in 1982 a higher percentage of cooperative respondents tried to activate an interest group or political party than was the case in the other two groups. This result also applies to the chances of having lodged a complaint.

The converted refusers appeared to be the least active in this regard. Both cooperative and initially detained respondents have joined a civic action group more often than the converted refusers.

If possible, attitudes towards politics like efficacy, distrust and cynicism have been compared.¹⁴ We expected that cooperative respondents would have less negative attitudes towards politicians and elections than the other two groups, whereas the converted refusers would be most hostile. Indeed, some of these differences present themselves, except for 1986. This is probably caused by the cruder categorization in two respondent groups, which may dilute differences.

In the first wave of 1981, those respondents who did not agree with the statement that 'Political parties are only interested in my vote and not in my opinion' (efficacy item 2) more often belonged to the group of cooperative respondents than respondents who did agree with this statement. In the third wave of interviews cooperative respondents were less likely to agree with the statement that 'So many people vote in the elections that it doesn't matter whether I vote or not' (efficacy item 4). The converted refusers agreed with this statement relatively often. Furthermore, the converted refusers more often agreed with the statement that 'Too many political decisions are made in secret in the Netherlands' (political distrust item 2) and more often held the opinion that 'One becomes member of parliament because of one's political friends, rather than because of skill and ability' (cynicism item 3). In the DNES of 1982 differences between the groups came up on the first, second and third efficacy item. Cooperative respondents appeared to agree less often with the statement that 'Members of parliament don't care much about the opinions of people like me' (efficacy item 1) than the initially detained and converted respondents. Converted refusers were more negative than the other two groups about the responsiveness of politicians. On the other two items, converted refusers were more negative than the other two respondent groups, which appeared to have almost the same opinion. They agreed to a larger extent than the other two groups with the statements that political parties were only interested in their vote and that people like them didn't have any influence on what the government does (efficacy item 3).

As mentioned above, the respondent groups have also been compared on a selection of variables that indicate the extent to which they are integrated in society or are able to be interviewed at length about politics. We expect that people who are more integrated in society or have a higher educational level are more willing to participate in an election survey. Hence, cooperative respondents should be more integrated in society and/or should have a higher social-economic status than the other two respondent groups, the converted refusers being the least integrated or educated respondent group.

Table 6 makes clear that some differences do exist between the groups,

but that they are not the same over the years and do not exhibit a coherent pattern. The magnitude of the relationships again is not very impressive. In the DNES of 1981 the respondent groups were not equal with respect to their educational level. However, these differences did not allow a straightforward interpretation: a smaller part of the respondents with elementary education or, in contrast, a higher level of education (with the exception of respondents with a higher level of vocational education) belonged to the group of cooperative respondents than voters from the other educational levels. Furthermore, of the respondents who (almost) never attend religious services, a proportionately higher rate was initially detained to participate in the survey. Finally, if we regard the degree of urbanisation of a municipality as an indicator of social cohesion, it appeared that the percentage of cooperative respondents in the bigger cities was relatively low. This was partly due to their absence at the first try, however; the percentage of converted refusers was not very different from the percentage of refusers in the less urbanized municipalities. This result was confirmed in 1982.

In the survey of 1982 the three groups differed with respect to educational level, too. Here, a more straightforward relationship was apparent: cooperative respondents tended to have a higher educational level than the other respondents. The above mentioned findings on indicators of social integration were not corroborated in the 1986 study. In that year the only difference between the (two) groups that could be detected pertained to the subjectively perceived social class. Cooperative respondents considered themselves to be (upper) middle class more often than the other respondents.

All in all the DNESs of 1981, 1982 and 1986 offer scant evidence to sustain the claim that cooperative respondents are better integrated in society or have better cognitive skills to participate in an interview. This is all the more true if we look at the tau-c values for participation in social organisations. Only in 1982 cooperative respondents were somewhat more likely to be a member or (rather) active member of a professional or women's organisation than the other respondent groups. Needless to say the differences were tiny again.

In sum, the following picture of the different respondent groups emerges: In the 1981 and 1982 election surveys the respondents who were cooperative from the start were somewhat more interested in politics than the other respondents. In 1982 and 1986 they were also somewhat more likely to be a (convinced) adherent of a political party than the other respondents. The converted refusers were the least politically involved in these respects. Except for 1986, converted refusers tended to have a somewhat more negative opinion on politicians and elections than the other two groups. In 1982 cooperative respondents were also slightly more politically engaged in non-electoral activities.

Nevertheless, the reported turnout of the different respondent groups was

exactly the same. On the basis of these findings no adaptations of the data can be made with respect to the turnout percentages. Thus, the differentiation in respondents groups cannot provide a basis for bridging the gap between the official turnout statistics and the survey results in these years.¹⁵

The question is how the patterns that emerged from the analyses of the DNES of 1981, 1982 and 1986 can be explained. In the next section of this paper some tentative answers are given.

8. Some explanations for the patterns found

The three election studies that have been investigated in more detail have one feature in common: the reported turnouts in the most recent parliamentary election have been quite similar in the distinct respondent groups. Nevertheless, the answers to the questions on turnout and other aspects of political involvement seem to point to a different pattern in each study. Probably this is caused by the varying designs of the studies: the DNES of 1981 consisted of three waves (and another two in 1982 and 1986), the DNES of 1982 was a single post-election survey, and the 1986 study consisted of two waves of interviews, one before and one after the election.

In the 1981 study, the converted refusers were politically less interested than the other two respondent groups, but the differences were rather small. Moreover, the vote intention of the groups showed barely any difference and the three groups reported to have voted at exactly the same rate. Three explanations may account for this pattern.

Firstly, converted refusers were not really different from the other respondent groups with respect to their political involvement and other relevant characteristics. If so, the converted refusers are not representative of the final refusers in the sample. Estimates of the population based on the corrected sample will still lead to an overrating of the level of political involvement of Dutch voters. Secondly, the converted refusers were indeed somewhat less politically involved, but misreporting may have been somewhat higher on their part. This explanation does not seem plausible considering the fact that misreporting tends to be higher on the part of (highly) politically involved respondents (again, see Granberg and Holmberg 1991). Thirdly, the converted refusers truly voted in the large amounts they claimed because of tradition. They consider it as their citizen duty to vote, irrespective of their actual level of political interest. In view of the small differences between the groups in the sample the latter explanation can only have limited value. Hence, the evidence points to the conclusion that the three respondent groups in the DNES of 1981 were quite similar, but distinct from the final refusers in the sample.

The respondent groups in the 1982 survey seemed to differ from one another somewhat more markedly, despite the lack of a significant difference in reported turnout at the parliamentary elections of that year. A stimulus effect cannot explain the outcomes because only a post-election survey was conducted. Again, the converted refusers were somewhat less interested in politics and had a more negative opinion on politicians. They were also less active with respect to some forms of non-electoral political participation and membership of a professional or women's organisation. Also, they tended to have a lower educational level. In this case, it is plausible that converted refusers are somewhat more traditionalistic than the other two respondent groups. Therefore, their rate of voting in elections was quite high, compared to their actual interest in politics.

In contrast to the 1981 and 1982 DNESs no differences in political involvement between the two groups could be detected in 1986, apart from membership of a party. Nevertheless, the groups of converted refusers and detained respondents together were less likely to have voted in the municipal elections of 1986. Also, their vote intention was lower than that of the cooperative respondents. However, the reported turnout in the 1986 parliamentary election was 93% in both groups. This pattern seems to point to a stimulus effect of the pre-election interview. The survey may have directed the attention of the 'second try respondents' more heavily to the coming elections. Since this group did not appear to be particularly disinterested in politics or show anti-political attitudes, the interview may have induced them to vote.

9. Concluding remarks

In this paper an attempt has been made to explore the causes of the discrepancy between voting turnout in official statistics and in the Dutch National Election Studies. Furthermore, a method has been tried out to correct for the overrated turnout in these surveys. The central questions were, firstly, what explanations could account for the gap between these two data sources and secondly, whether adaptations to the data could be made to narrow this gap.

It was shown that in the DNESs nonresponse has grown over the years. Also, the percentage of pertinent refusals to participate in the interview has increased and has become the most important cause of nonresponse. It is not clear however, whether a refusal to participate in a relatively lengthy face-to-face interview on elections signifies that the intended respondent is not interested in politics. Some other reasons for refusals may play a role in the refusal to participate in a survey. Thus, it is not correct to infer from the available information that only voters who were not interested in politics refused to be interviewed.

To assess the possible bias of the samples with respect to political involvement and, in particular, the reported turnout of the respondents, respondent groups were divided according to their readiness to participate in the first interview. This approach could be used in the DNESs of 1981 and 1982, and, to a lesser extent in that of 1986. If these distinct respondent groups would differ with respect to their reported turnout at elections (and other indicators of political involvement), a correction for the bias in the sample could be made.

The analyses showed that the cooperative respondents were somewhat more involved in politics than the other group(s). The differences were small, however. Moreover, the respondent groups reported to have voted at about the same rate. Therefore, a revision of the estimates of turnout for the samples and the population was not possible.

The outcomes point to several conclusions. Firstly, the converted refusers in the DNESs are not representative of all the refusing respondents in the gross sample. Although they seem to be somewhat less involved in politics than the cooperative respondents, they vote at a high rate. This rate is higher than might be expected if they were truly representative of the final refusers. Thus, the converted refusers are not really different from the cooperative respondents or they are rather traditional voters who regard it as their citizen duty to vote. Secondly, the high rate of turnout in the group of converted refusers may be caused to some extent by the stimulus effect of the pre-election interview. In the DNES of 1986 this effect may have occurred. The analysis of the DNES of 1981 shows, however, that this effect need not be operative in every study that consists of a pre-election and a post-election interview.

In the light of the increasing gap between official turnout statistics and the reported turnout in the most recent DNESs of 1989 and 1994, self-selection among respondents is likely to have become the most important cause of the inflated turnout figures. No reason comes up easily why misreporting or a stimulus effect would have increased over the years. Rather, it is plausible to assume that misreporting is a consequence of the decline of the norm that a good citizen ought to vote. With every election the era of compulsory voting is farther removed from the experience of the present electorate. Indeed, turnout percentages in the nineties have shown that a relatively large amount of voters does not feel obliged to vote at elections. Since the voting turnout has been relatively low in recent elections, it is plausible to assume that the politically interested citizens in particular will consent more readily to an interview about elections and politics in general than people for whom (governmental) politics is a matter of modest concern.

If no steps are taken, the gap between voting turnout in surveys and aggregate statistics will remain wide, which implies that the survey-based fig-

ures on the political involvement of the voting population have to be interpreted with caution. To enhance the validity of the survey results the nonresponse should be reduced, especially among the nonvoting part of the electorate. Hence, extra pains should be taken in reaching these respondents. A critical examination of the letter of introduction that is sent to the intended respondents, stressing the importance of their participation in the survey even if they are not interested in elections, might be a first step. A monetary reward for cooperation in the survey is another option to consider, as it has been established that this kind of incentive has a positive effect on the response.

Notes

1. I would like to thank prof. dr J.J.A. Thomassen for his comments on a first draft of this paper.

2. An example of this rather uncritical use of the research findings of the Dutch National Election Studies is a recent publication of Van Gunsteren and Andeweg. They claim that the political interest of the Dutch voting population is 'unmistakably' moving in the direction of more, rather than less political interest (Van Gunsteren and Andeweg 1994: 29).

3. In this election the reported turnout was 78% for the respondents who were interviewed before the election, versus turnouts of 71% and 72% for respondents in surveys that consisted of a post-election survey only. The official turnout was 63%.

4. The stimulus effect was calculated as follows: the turnout for the post-election interviewees is taken as the baseline. In the Swedish case the maximum amount of change is 100 minus 93%, i.e. 7%. The amount of change between the pre-election and post-election interviewees was 2%, so the relative size of the stimulus effect was 2% divided by 7% is 29%. In the American case the relative size of the stimulus effect was about 6% (78%-72%) divided by 28% (100%-72%), i.e. 21%. Hence, the stimulus effect was even stronger in Sweden than in the United States.

5. Although the authors do not mention it, a ceiling effect is again present. This means that the relative size of the stimulus effect for voters with low political interest was 30% (3/10), compared to 25% (1/4) for the high interest voters, a less impressive result than suggested by the percentage points quoted.

6. The elections of 1972 and 1982 were held after the premature fall of the governmental coalitions. No pre-interview was conducted in these years because it was not possible to organize such an interview in the time available.

7. In 1971 and 1972 the fieldwork for the DNES was done by N.V. v/h Nederlandse Stichting voor Statistiek (The Hague), in 1977 by Interact BV (Dongen), in 1981 and 1982 by Intomart (Hilversum), in 1986 by Inter/View (Amsterdam) and in 1989 and 1994 by CBS (Heerlen). These agencies based their gross sample on various sample frames.

8. Uninhabited dwellings, wrong addresses or foreigners have always been excluded from the gross sample; hence, the differences in the ratio between gross and net sample over the years cannot be accounted for in this way.

9. In this year the division of the causes of nonresponse is not wholly comparable with

the other years. In the codebook of 1986 a division has been made into: 1) illness, etcetera, 2) no contact, 3) refusal and other causes.

10. Of the 15 social surveys that the Central Bureau of Statistics has conducted on a regular basis, the response rate is lower in face-to-face interviews than in short telephone interviews. With respect to the latter kind of survey, no decrease in response has been witnessed over the years (De Heer and Israëls 1992: 6).

11. The CBS has developed a weighting procedure to adjust for the voting turnout figures in the DNESs (Schmeets & Molin 1990; CBS 1993). It appeared that a correction for nonresponse on the basis of socioeconomic variables only had no effect on the reported turnout in the samples. So, although the underrepresentation of women, young people between 18-24, unmarried people, inhabitants of large municipalities and those from the western part of the country could be corrected for by weighting, these corrections did not decrease the proportion of reported turnout in the surveys. Consequently, a weighting model was constructed that incorporated voting turnout and party choice as well. However, inclusion of independent and dependent variables into one weighting model can seriously threaten the validity of the survey results. Moreover, the weights in this model take on extreme forms, ranging between 0.09 and 6.8 in 1989, which makes the problem of capitalizing on chance acute.

12. The intended respondents who could not be reached initially, or who refused to participate at first received a letter from the project staff explaining the goal and value of the research. If possible, another interviewer came along to conduct the interview. There is no information available about the exact procedure that was used in the 1986 study to contact these respondents.

13. Marital status and religion are measured by the chi square statistic.

14. The wordings of the various statements about political attitudes read as follows (though some slight alterations have been made in the DNES of 1986):

Efficacy - item 1: 'Members of parliament do not care about the opinions of people like me.'

Efficacy - item 2: 'Political parties are only interested in my vote and not in my opinion.'

Efficacy - item 3: 'People like me don't have any say about what the government does.'

Efficacy - item 4: 'So many people vote in elections that it doesn't matter whether I vote or not.'

Political distrust - item 1: 'Political parties promise much, but don't deliver.'

Political distrust - item 2: 'Too many political decisions are made in secret in the Netherlands.'

Political distrust - item 3: 'Quite a bit of the taxpayer's money is spent wrongly by the government.'

Political cynicism - item 1: 'Politicians consciously promise more than they can deliver.'

Political cynicism - item 2: 'Cabinet ministers and under-ministers are first of all working for their personal interests.'

Political cynicism - item 3: 'One becomes member of parliament because of one's political friends, rather than because of skill and ability.'

15. Some adaptations could be made only with respect to political interest. These corrections are small, however. For example in 1982, on the base of a differentiation in the three respondent groups the percentage of Dutch voters who were very interested in politics would decrease from 15.9% in the original sample to 13.9%; the group of fairly interested voters would remain rather constant (58.3% instead of 58.4%) and the percentage of not politically interested voters would increase slightly from 25.6% to 28.0%.

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Literatuur

Lijphart vs Lijphart¹

Marcel Hoogenboom

1. Inleiding

De uitslag van de Tweede Kamerverkiezingen van mei vorig jaar ging in pers en wetenschap vergezeld van een definitieve begrafenis van de verzui-ling. De kiezers, zo wilde de mare, hadden eindelijk het bewijs geleverd van hun pertinente breuk met de oude zuilen; de vroeger zo vanzelfsprekende band tussen levensbeschouwing en stemgedrag was voorgoed doorsgesneden. Niettemin klonk tussen de regels door nog enige twijfel. Deze groeide toen later bleek dat op basis van de verkiezingsuitslag in relatief korte tijd een monsterverbond tussen socialisten en liberalen gesloten kon worden. Het Paarse Kabinet, en de wijze waarop het tot stand kwam stelt waarnemers van de Nederlandse politiek voor een lastig vraagstuk. Het is echter een vraagstuk dat zich reeds in de tweede helft van de jaren '80 aandiende, toen na een lange "Tijd van Troebelen"² het Nederlandse politieke stelsel opnieuw in rustiger vaarwater aanlandde. Zo schreef Wolinetz in 1990: 'The renewed moderation of the PvdA gives contemporary politics an air of consensus reminiscent of the 1950s and early 1960s.' (1990: 404)

Wolinetz blijkt geen roepende in de woestijn: steeds meer auteurs komen tot de conclusie dat het netto resultaat van de 'revolutionaire ontwikkelingen' van de 'roerige jaren '60' bij nader inzien vrij mager is te noemen. Zij menen dat in het beeld van drie decennia Nederlandse politiek, uiteindelijk de continuïteit de veranderingen overschaduwde, al benadrukken zij daarbij verschillende aspecten. Andeweg (1989) bijvoorbeeld wijst op de standvastigheid van de politieke structuren en instituties, die de stormloop van hervormers vrijwel ongeschonden blijken te hebben doorstaan. Andere auteurs benadrukken, evenals Wolinetz, vooral het gedrag van de politieke actoren *binnen* deze structuren. Na jaren van polarisatie en bittere politieke concurrentie lijken dezen langzaam maar zeker te zijn terug gekeerd naar de oude gebruiken van samenwerking, matiging en compromisbereidheid.

Dit laatste geldt volgens een aantal auteurs, tenslotte, tevens voor de verstandhouding tussen de overheid en belangengroeperingen. Katzenstein bij-