OP25

The Opinion Polls and the 1992 General Election

A Report to
The Market Research Society

July 1994

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INTRODUCTION

by David Butler

The failure of the eve-of-election opinion polls to reflect the actual result of the British General Election on 9 April 1992 was the most spectacular in the history of British election surveys. Four respected and experienced polling organisations produced estimates of each major party's share of the vote that were close to each other, but were four percentage points away from the actual outcome for both the Conservative and Labour parties.

The record of the polls in the previous three elections had been good and the pollsters had every incentive to get the right answer. Their failure in 1992 led professionals as well as sceptical outsiders to ask fundamental questions about the techniques employed. Were there flaws in the accepted procedures for selecting samples? Did the voters lie to the interviewers? Did they change their vote or their intention to vote at the last minute?

The Market Research Society, believing that an exhaustive study was required, convened a group of experts to look at every aspect of the subject. This was the initial working party, who produced an Interim Report in 1992.

John Barter, retired chairman of NOP
Martin Collins, City University Business School
John Curtice, Strathclyde University
John O'Brien, BMRB
Sue Stoessl, Market Research Society

Subsequently further members were asked to join the committee:

Nicholas Sparrow, ICM
Robert Worcester, MORI

and Michael Warren took the place of Sue Stoessl when he succeeded her as Director-General of the Market Research Society. First Sue Stoessl and then Michael Warren also served as Secretary to the Committee.

From early 1993 John Barter chaired the working party, but in 1994 he found himself unable to continue as Chairman for personal reasons, and I was asked, as someone outside the polling field, but for many years a close observer of it, to oversee the final stages of drafting and to chair the group. I was ably assisted in drafting the report by Roger Mortimore, who bore the main burden of a very complex editorial task.

The report bears the signature of all the participants. It represents a collective view on the sources of error in the 1992 polls and on some of the possible remedies for the future.

The report is arranged to answer :-

a. The possibility that the electors misled the pollsters by late switches, by abstentions or by outright lying;

b. The possibility that there was mathematical sampling error or bias in the pollsters' construction of their samples;

c. The possibility of error through problems with the electoral register, with overseas votes, or with postal votes;

d. The possibility of error through weighting to compensate for imperfections in the samples; and

e. The whole range of interpretative problems involved in poll predictions - ranging from the treatment of 'don't knows' to the link between votes cast and seats won.

The report is prefaced with a summary of our conclusions. At the end we set out in appendices some basic data, together with a list of the evidence submitted to us. We recognise that this report is a detailed and demanding document: however, this reflects the complexity of the issues with which it deals.

Our group was diverse, composed of polling practitioners, market researchers experienced and knowledgeable about polling, and academic specialists. We should stress that a majority of the members of the group were not currently involved in the political polling industry (although it did include representatives of two of the five major polling organisations and we kept in close touch with the other three who have had the opportunity to comment on our findings). This report is not an apology but an attempt to find the truth.

ACKNOWLEDGEMENTS

We are deeply grateful to all those who gave time and trouble to helping our researches. We must place on record our thanks to the polling organisations - ICM, Gallup, Harris, MORI, NOP and System 3 - for their help and co-operation, without which this report could not have been compiled.

We are grateful to the staff of the Social Statistics Laboratory at the University of Strathclyde for their assistance with some of the research undertaken for this report. In particular Sarinder Hunjan extracted a wide range of information from the 1991 Census while Ann Mair helped us in the analysis of the National Readership Survey. Steve Harris of BMRB also gave valuable help in processing data from the National Readership Survey.

We are also grateful to MORI for the loan of Roger Mortimore, who was released to work for the Committee in drafting our report, and for considerable secretarial assistance.

Finally, we are extremely grateful to the Market Research Society for all their support, including providing a meeting place for the Committee and secretarial and administrative back-up.

SUMMARY: CONCLUSIONS AND RECOMMENDATIONS

CONCLUSIONS

Any attempt to predict the future depends on it resembling the past. There is no reason to suppose that this will always be the case with the political behaviour of the British electorate. No two elections are entirely alike. Moreover, the primary business of pollsters - whatever the media might demand - is not to predict the future but to measure the present; all concerned must accept that the future - even the near future - is to some extent unpredictable.

The standards of accuracy which are demanded from pre-election polls - and which the pollsters seem forced implicitly to accept - are far more stringent than those applied to any other form of survey research, and may well be unrealistic. The media and public expect much greater accuracy than can be delivered, granted the immutable statistical margins of error. Some degree of variation from perfection is inevitable, and this must be understood by those who use the polls. However ideal their methods, polls **cannot** measure, much less predict, party vote shares correct to one decimal point; nor can they, given the uncertainties of the British electoral system, predict the distribution of seats except within a wide margin.

Nevertheless, the gap between the polls' findings and the final result in 1992 was greater than their consumers are entitled to expect; indeed, it was greater than had ever been delivered since polls began.

There were three main factors that seem to have contributed more or less equally to the error in the polls' forecasts in April 1992. The nature of the evidence makes it impossible to ascribe specific percentages to each factor.

- There was a late swing: some voters changed their minds after the end of interviewing; furthermore, Conservative supporters proved more likely to vote on the day. The Conservatives gained from both these tendencies. (Paragraph 29 *et seq*).

- Some inadequacies were revealed in the operation of the quota system to select representative samples. (Paragraph 83 *et seq*). This arose partly because quotas and weights did not reflect sufficiently accurately the social profile of the electorate, and partly because the variables used as the basis of quotas and in corrective weighting were not closely enough correlated with voting behaviour to ensure that the samples were fully representative of the distribution of political support amongst the electorate.

- Conservative supporters were less likely to reveal their loyalties than Labour supporters (as has also been the case in earlier elections). This certainly operated through 'item refusal', reluctance by those interviewed to answer the voting intention question, both by outright refusal and by disingenuously answering 'don't know' (Paragraph 299 *et seq*). A similar effect was probably also caused by a differential refusal to be interviewed, although there is no solid evidence on this point. (Paragraph 176 *et seq*).

It is not possible to assign more than a tentative weight to the effect of each of these factors, especially since they interacted with each other. In particular, the quota method is in part designed to overcome the differential refusal of some groups to take part, and it was because of the weaknesses in its implementation that differential refusal could have so much impact. Late swing, the most easily isolated of the factors, probably contributed between a fifth and a third of the total discrepancy.

In addition to these major causes of error, a number of other factors had a minor impact.

- The selection of constituencies as sampling points by the companies had weaknesses (paragraph 230 *et seq*); all but one of the companies used a selection of points that was slightly biased to Labour. This might have had a slight effect on the result of the final polls, but most of its potential risk was probably counteracted by the operation of the quotas and weighting.

- There was probably a slight bias to Labour created by interviewing respondents who were not in fact on the electoral register and hence could not vote. (Paragraph 246 *et seq*). However, it seems likely that the impact of this was small, especially as the polls based their social profiles on a source that drew upon the electoral register rather than on the whole adult population, and since some of the polling companies attempted to exclude those who said they were not registered or didn't know whether they were or not. Particular concern has been expressed about the impact of deregistration specifically to evade the 'poll tax', but its effect on the opinion polls appears to have been small.

A number of other factors which have been suggested as partial explanations for the error of the polls did not, in our view, have any measurable effect.

- We do not believe that deliberate lying to pollsters occurred to any significant extent. (Paragraph 277 *et seq*).

- The behaviour of postal voters who may not have been interviewed by pollsters probably had little effect (paragraph 270 *et seq*). Less than 2% of voters voted by post and, in contrast to earlier elections, their support seems to have been fairly evenly divided between the two main parties.

- The result was not affected by the votes of Overseas Voters. (Paragraph 272 *et seq*).

- Interviewing procedures played no apparent role. There were no systematic differences between those polls which interviewed in street and those which were conducted in home (paragraph 201), those which interviewed for just one day and those for more than one day (paragraph 207), or between polls which interviewed at weekends and those which did not (paragraph 208).

- Sample size was not a contributory factor - bigger samples would not have produced significantly better or worse results. (Paragraph 67 *et seq*).

RECOMMENDATIONS

To reduce the risk that the problems of 1992 will recur, we recommend that immediate attention should be given to the three main sources of error summarised above. One should certainly be easy to avoid in future, one will be much more difficult to solve, while one may be beyond solution. More generally we wish to emphasise the vital importance of methodological research, both to develop more robust techniques and to address potential new problems as the climate in which survey research is undertaken changes. Further, we recommend much greater attention be given to the limitations of polls by the pollsters and by the media when reporting their results. Any poll will inevitably be an approximation based on statistical sampling of a moving target - the voting intentions of the British electorate.

- **Quotas.** The sources used for setting quotas and target weights must be selected more carefully and verified wherever possible. Reliable and regular sources for these variables need to be established; the use of out-of-date figures, incorrect data and unreliable sources of information must be avoided.

  However we also believe that further consideration needs to be given to the operation of the quota system itself. Pollsters should certainly consider trying to identify other variables more closely related to voting behaviour than those currently used in setting their quotas.

  The principal alternative to quota sampling is probability sampling. Despite the difficulties in the operation of quota sampling in 1992, we do not recommend its wholesale abandonment in favour of probability sampling. Probability sampling needs a longer time period to be implemented than is currently available during a general election and even when fieldwork is spread over several weeks non-response rates of 30% to 40% are common. But we would welcome further research and experimentation to compare the two approaches.

- **Refusals:**

  (a) **Refusals to the voting intention question ('item refusal').** A comparatively small number of respondents agree to participate in a survey but then refuse to answer the voting intention question. These, together with those who say that they are 'don't knows' rather than overtly refuse need to be encouraged as far as possible to give their party preference. Use of secret ballot techniques is one approach: we would welcome further experiments to establish whether it constitutes a worthwhile departure from past practice. We also feel that there should be further exploration of ways of compensating for any difference in the willingness of each party's supporters to say how they are going to vote; it is possible to weight or re-allocate "don't knows" on the basis of their reported past vote, or on the basis

of their attitudes to other attitudinal questions. However, we recognise that because of the infrequency of major elections in Britain, there is limited scope for assessing the validity of possible methods which renders it difficult for pollsters to develop such techniques.

Given their importance, we recommend that the proportion of 'don't knows' in any voting survey should always be published as part of the technical details of any poll.

(b) **Refusals to participate ('non-co-operation').** This is more intractable. Any method that might encourage lower refusal rates should certainly be explored but this will clearly not solve the whole of the problem. We suggest that more effort should be devoted by the Market Research Society and by all market researchers to try and increase response rates in all survey research and also to persuading the public of the importance of survey research and the real value of their participation.

We also noted in the course of our investigation how little reliable evidence there is on refusal to participate in quota surveys or on its possible impact on their results. It would be beneficial if all market research surveys were as a matter of routine to collect as much evidence as possible on refusal rates and the reasons for refusal. All researchers should give consideration to possible techniques of weighting to compensate for these factors. Ideally the aim should be to deal with differential refusal by developing more effective quota and weighting systems which ensure that, as far as possible, those not prepared to be interviewed are replaced by people who are prepared to be interviewed but otherwise hold similar opinions.

- **Late swing.** There is little the pollsters can do directly about late swing. They can (and usually do) poll as late as possible to minimise the impact of last-minute changes but, as 1992 shows, this cannot always deal with the whole problem.

  It may be that, with the experience of 1992 in mind, the pollsters will be able to assess the likelihood of a late swing. Certainly the possibility of examining other attitudinal evidence before polling day needs to be explored, but inevitably such forecasting methods will be problematic.

There is a great deal yet to be learned about the effectiveness of the various techniques involved in survey research. Our knowledge can only be improved by consistent and wide-ranging experimental work. The political pollsters have recently been active in this direction and we recommend the entire industry to support them and follow them in the endeavour. This should encourage the development of more

sophisticated research techniques which may go beyond the production of simple headline voting 'predictions'. We would encourage methodological pluralism; as long as we cannot be certain which techniques are best, uniformity must be a millstone - a danger signal rather than an indication of health. We should applaud diversity; in a progressive industry experimentation is a means of development. No pollster should feel the need to be defensive about responsible attempts to explore in a new direction and all pollsters should regard it as their duty to publish details of the results of their experiments.

Finally we would encourage the media to support this work and, in particular, to be willing to accept that - inconvenient though it may be - research cannot always be responsibly reported in a single sentence or accurately condensed into a succinct headline. There is a particular need for care by the media in their secondary reporting of research they themselves have not commissioned. We hope that the polling industry will redouble its efforts to ensure that its work is reported accurately and in context.

1. WHAT DID THE POLLS SAY IN 1992?

1.1 The Pre-election Polls

1. The final polls in any election are normally regarded by press and public alike as a relatively accurate forecast of the actual result: they have been correct within a narrow margin in most elections since polling began. (See Appendix 3). While the polls would never claim to be able to predict the number of *seats* won by each party, they ought to be able to get reasonably close to the percentage share of the *vote*, which is what they try to measure. The criterion by which the accuracy of the polls is judged is a stringent one; they are routinely reported as if they can be relied upon for a numerical precision which would never be expected from other forms of survey research. Perhaps this demand for rigid accuracy is natural, and may be necessary for some of the means to which media and public would like to put the findings, but it is nonetheless unrealistic.

2. Even so, the polls in 1992 were not successful enough to fulfil even reasonable expectations of their capabilities. They proved to be well wide of the mark, faring worse than in any previous election. Four polls[1] were carried out in the last couple of days, ending on the day before polling day, and published on the day, 9 April. If any polls could be expected to provide forecasts, it is these. Table 1 sets out their 'predictions' of the vote in Great Britain[2].

Table 1: Four eve-of-poll polls

	Fieldwork Dates	Sample		Con	Lab	LDem	Oth	C lead
NOP	7-8/4	1,746	%	39	42	17	2	-3
ICM	8/4	2,186	%	38	38	20	4	0
MORI	7-8/4	1,731	%	38	39	20	3	-1
Gallup	7-8/4	2,478	%	38.5	38	20	3.5	+0.5
Average			%	**38.4**	**39.2**	**19.2**	**3.1**	**-0.8**
Election (GB)	**9/4**		%	**42.8**	**35.2**	**18.3**	**3.7**	**+7.6**
Error			%	*-4.4*	*+4.0*	*+0.9*	*-0.6*	*+8.4*

3. The differences between these four surveys were only the small variations that would be expected. The overall picture is of a failure to forecast the result to an acceptable standard of accuracy. On average, the

[1] The fifth major political polling organisation, Harris, completed their last poll earlier than the other companies. Nevertheless, the results of their final poll (4-7 April, n=2,210) were very similar: Con 38%, Lab 40%, LDem 18%.

[2] Note that all regular British voting intention polls measure share of the vote in Great Britain; none of the pollsters normally include Northern Ireland.

polls suggested a small Labour lead of 0.8%; the election itself produced a substantial Conservative lead of 7.6%. It is this difference of 8.4%, the equivalent of a 4.2% swing (see glossary), which has provoked this investigation.

Trends during the campaign

4. The five major national polling organisations between them conducted 49 published polls during the 1992 election campaign (see Appendix 1) for a wide diversity of media clients. In addition there were a small number of British telephone polls, a few polls by other agencies, polls in Scotland by two of the British pollsters and by two Scottish-based companies (see Appendix 2), and scores of local polls, constituency polls and unpublished polls. All the quality daily and Sunday national newspapers commissioned polls, as did many of the middle-market and regional papers; almost all those that did not gave extensive coverage to the polls published elsewhere.

5. The polls gave a consistent message. While there were variations between individual polls of the sort that are always to be expected, the average picture was fairly clear. Throughout the campaign period, the polls indicated a small Labour lead. Table 2 shows how the tracking polls indicated little *net* movement of opinion during the campaign between the Conservatives and Labour; they record no major trend except for a rise in Liberal Democrat support at the expense of the two larger parties[3]. A more detailed statistical analysis[4] comes to a similar conclusion, finding evidence of only a small 'step' shift in favour of the third party in the middle of the campaign. (This was very much the pattern in 1987, with a similar shift after the first week of the campaign but little or no other systematic trend)[5]. Although compared with polls carried out earlier in the campaign, the late findings did suggest there might be a last-minute recovery in the Conservative vote and a corresponding reduction in the Labour lead.

[3] Within the overall pattern of relative stability, there is a suggestion of a short-lived peak in the Labour lead immediately before that party's Sheffield rally on 1 April, with three polls (fieldwork 29-31 March) showing Labour leads of 7%, 6% and 4%. Over the following week these polls showed a swing of 3%, 2% and 2% respectively. We cannot be sure that this apparent peak did not arise from sampling fluctuations, but we accept that the pattern is intrinsically plausible and may well have been real.

[4] P.Clifford and A. Heath, 'The Election Campaign', in A. Heath, R. Jowell and J. Curtice with B. Taylor, Labour's Last Chance? (Aldershot: Dartmouth, 1994).

[5] M. Collins, 'Lessons from the Polls' (1988 MRS Conference Papers).

Table 2: Polls during the campaign grouped by first day of fieldwork

	No of polls		Con	Lab	LDem	Oth	C lead
11/3-13/3	(8)	%	39.3	40.6	15.3	4.9	-1.3
15/3-21/3	(11)	%	39.1	40.5	16.5	3.8	-1.4
22/3-30/3	(14)	%	38.0	40.3	17.5	4.2	-2.3
31/3-6/4	(13)	%	37.1	39.4	19.3	4.2	-2.3
7/4	(4)	%	38.4	39.2	19.2	3.1	-0.8

Comparing companies

6. The polling agencies during the campaign were all close to each other in their findings. Their average figure was within 1½% of the median share (see glossary) for each party[6].

Table 3: Similarities between polling agencies

	No of polls		Con	Lab	LDem	Oth	C lead
Gallup	(5)	%	38.9	38.4	19.4	3.3	+0.5
Harris	(13)	%	38.9	39.8	16.8	4.5	-0.9
ICM	(9)	%	37.4	40.0	17.9	4.7	-2.6
NOP	(9)	%	38.2	40.9	16.7	4.2	-2.7
MORI	(7)	%	37.7	40.9	18.0	3.4	-3.2
Range			**38.1±0.8**	**39.7±1.3**	**18.1±1.4**	**3.9±0.6**	

7. There were small differences between the agencies in terms of methodology - over question wording and question order, differences in weighting strategy and quota details (see glossary) - which could have contributed to these small differences in results. This was accentuated by the approach of using 'master samples' whereby a company sticks to the same constituencies in successive polls (see paragraph 230 *et seq*). This could introduce elements of systematic similarity between polls within a series and systematic differences between different series. If this effect was present in 1992, however, its impact appears to have been minimal. There may be some 'company effect' on a trivial scale[7] but there is nothing that would help to explain the overall error. The most important reason for variation between polls was, almost certainly, the random variation inherent in sampling.

Polls as forecasts

8. Pollsters throughout the world point out that their findings should be regarded not as attempts to *forecast* election results but as snapshots of opinion *at the time the interviewing took place*, operating as an

[6] The table excludes the later waves of the MORI and NOP panels. See Appendix 1 for the findings of these surveys.

[7] The Clifford and Heath analysis ('The Election Campaign' in A. Heath, R. Jowell and J. Curtice with B. Taylor, Labour's Last Chance?, Aldershot: Dartmouth, 1994) found statistically significant evidence of a minor company effect.

opinion **thermometer,** not a behavioural **barometer**. This opinion may or may not be translated into votes on the day. This 'snapshot' principle is especially important weeks and months before polling day; too often media reports treat poll findings as forecasts before writs have been moved, candidates selected and campaigns fought.

9. Nevertheless, it is clear that many journalists *do* treat polls as if they were forecasts, and that the polls are the nearest approach to an objective forecast that is available to the general public. The polls' consumers want and expect the polls to provide something as near to a forecast as possible - as do the consumers of weather forecasts. In any case, the 'snapshot' argument would miss the point in 1992. Even if polls are not forecasts, they do claim to be accurate snapshots, so they should only diverge from the actual result when the nation changes its mind after having been polled. The four final polls undertook their interviewing on the two days before polling day, leaving little time for any late swing to occur. And unless it did, the real question about the polls in 1992 would seem to be whether there was a degree of error throughout the campaign.

10. The consistency between the polls both throughout the campaign and in the eve-of-poll surveys can be likened to a marksman 'zeroing-in' a rifle on the target, initially shooting a tight pattern and then adjusting the rifle's sights to bring the bullet's trajectory into the centre of the target. In the case of the polling agencies during the 1992 British General Election, the pattern was tight but not near enough to the bullseye; the analogy would suggest either that the sights were not correctly adjusted (sampling error and response error) or that the wind blew the bullets off course (late swing and differential turnout).

1.2 Exit Polls

11. In addition to the campaign polls, both ITN (through Harris) and the BBC (through NOP) commissioned exit polls, asking large samples of voters as they left the polling stations to report how they had just voted. These too somewhat underestimated the performance of the Conservatives and overstated that of Labour, but to a smaller degree than the campaign polls.

12. Each organisation in fact carried out two exit polls. One was a national poll designed to provide material for editorial comment (an 'analysis' poll), but not to predict the outcome in seats; the other was undertaken only in marginal constituencies, and was the main (though not the only) source of information used to produce a forecast of the outcome in seats (a 'prediction' poll), to be broadcast between the close of polls (at 10 p.m.) and the declaration of the first results.

13. The **prediction** exit polls were designed to minimise the number of refusals to participate, using only a very short 'secret ballot' questionnaire. But because these polls were only undertaken in marginal constituencies, we cannot assess their accuracy by comparing them with the overall national result. Rather we have to compare their estimate of the change in each party's share in the constituencies they surveyed with what actually happened in those constituencies[8]. In each case the results were reported separately for the two main categories of marginal seats - those where Conservative and Labour were the main contenders and those where the Conservatives and the Liberal Democrats shared the first two places.

[8] The figures quoted here are based on all respondents. The forecasts broadcast at 10 p.m. were inevitably made before the results of all the interviews were available. In the case of the BBC at least the final poll results were noticeably different from (and more accurate than) those available at 10 p.m. See J. Curtice and C. Payne in I. Crewe and B. Gosschalk, Political Communications: the British General Election of 1992, (Cambridge University Press, in Press).

Table 4: Marginal Exit Polls ('Prediction Polls')

	Change in % vote share since 1987		
	Con	**Lab**	**LDem**
(a) Con/Lab marginals			
NOP/BBC estimate	-3.1	+8.4	-6.5
Actual	-1.2	+6.9	-6.6
Error	*-1.9*	*+1.5*	*+0.1*
Harris/ITN estimate	-2.4	+8.1	-6.6
Actual	-0.7	+6.4	-6.6
Error	*-1.7*	*+1.7*	*0.0*
(b)Con/LibDem marginals			
NOP/BBC estimate	-4.2	+4.9	-1.5
Actual	-1.7	+2.3	-1.4
Error	*-2.5*	*+2.6*	*-0.1*
Harris/ITN estimate	-0.5	+0.5	-1.2
Actual	-0.8	+2.7	-3.1
Error	*+0.3*	*-2.2*	*+1.9*

Source: J. Curtice and C. Payne in I. Crewe and B. Gosschalk, Political Communications: the British General Election of 1992 (Cambridge University Press, in press); see also G. Mathias and D. Cowling, 'The ITN Exit Poll' (paper presented at 1992 EPOP Conference, University of Essex)

14. As can be seen both prediction polls overestimated the swing from Conservative to Labour in Conservative/Labour marginals by a little under 2%, less than half the error of the four eve-of-poll surveys conducted a day or two before polling day.

15. The national **analysis** polls involved longer questionnaires and anticipated greater problems in gaining co-operation. Hence they were not used directly to forecast the outcome in seats. In practice however both polls underestimated the Conservative performance by approximately the same amount as did the exit prediction polls, again less than half the under-estimate of the eve-of-poll polls.

Table 5: National Exit Polls ('Analysis polls')

		Con	Lab	LDem	Oth	Con lead
Actual Result	%	**42.8**	**35.2**	**18.3**	**3.7**	**+7.6**
NOP/BBC	%	40.0	36.3	18.3	5.4	+3.7
Error		*-2.8*	*+1.1*	*0.0*	*+1.7*	*-3.9*
Harris/ITN	%	41.1	36.3	18.0	4.6	+4.8
Error		*-1.7*	*+1.1*	*-0.3*	*+0.9*	*-2.8*

Source: J. Curtice and C. Payne in I. Crewe and B. Gosschalk, Political Communications: the British General Election of 1992 (Cambridge University Press, in press); see also G. Mathias and D. Cowling, 'The ITN Exit Poll' (paper presented at 1992 EPOP Conference, University of Essex)

16. On the morning of polling day the final polls suggested that Labour were still in the lead, but that the lead had narrowed. The exit poll results then further contributed to the impression that there was a late swing in the final hours of the election. An exit poll undertaken for the *Sun* by ICM, which finished polling at 4 p.m. and was released in the early evening, also suggested that Labour were still in the lead. At 10 p.m. both the other exit polls suggested that the Conservatives would win most seats, but not enough to secure an overall majority (see Table 6). ITN's underestimate of the lead in votes was also broadcast, and although the BBC did not broadcast a vote share forecast, the seats forecast implied a narrow Tory lead in votes. And then when the results came in, the Conservative lead proved to be even larger. Together, this sequence of events gave the impression of an electorate that had slid continuously towards the Conservatives in the final hours of the campaign.

Table 6: Exit Poll based Seats Forecasts at 10 p.m.

	Con	Lab	LDem	Oth
BBC/NOP	301	298	24	28
ITN/Harris	305	294	25	27
Actual Result	**336**	**271**	**20**	**24**

Source: J. Curtice and C. Payne in I. Crewe and B. Gosschalk, Political Communications: the British General Election of 1992 (Cambridge University Press, in press); see also G. Mathias and D. Cowling, 'The ITN Exit Poll' (paper presented at 1992 EPOP Conference, University of Essex)

17. The methodological problems involved in conducting an exit poll are in many respects different from those which face opinion polls. Further, the forecasts of the outcome in seats broadcast by the television companies at 10 p.m. are not simply based on the results of their exit poll, but also depend upon the judgments used to convert votes into seats (see Appendix 7). But the inaccuracy of the forecasts undoubtedly contributed to the general perception that 'the polls got it wrong'.

2. HAS IT BEEN DIFFERENT IN THE PAST?

18. Much of the impact of the polls' failure in 1992 came because it was generally so unexpected. In recent years the polls had seemed accurate. The media had become reliant upon them; they were quite unprepared for a collective and systematic error. Even so, the impact would have been less, had the election not appeared to be so close - seemingly the closest for twenty years - meaning that the overall outcome was in doubt; the fact that the poll error misled most observers as to which party was likely to form the next government considerably magnified its impact.

19. In 1992 the fact that the results of the four final polls were so close to each other also added to the shock. In the previous three general elections (1979, 1983 and 1987) one polling organisation produced a late estimate remarkably close to the actual result while the others were a little further away but were within sampling tolerances. This did not happen in 1992; the variation in the final four polls was, in fact, smaller than at any general election since 1959 (when there were also four eve-of-poll forecasts).

20. Once the magnitude of the polls' failure in 1992 became clear, it would be expected that a close examination would be made of the record of the past as well as the present. Was the recent 'good performance' of the polls illusory? Did 1992 merely reflect a more dramatic than usual manifestation of a flaw already present?

21. The 1992 election was not the first in which the polls were 'wrong'. When Mr Heath won in 1970 all the polls (except a last-minute update from ORC) suggested a Labour victory. In February 1974 most of the polls put the Conservatives comfortably ahead - the hung result was a general surprise. In October 1974 the polls appeared to point to a Labour landslide, yet the party secured only a three-seat margin. However, polling error can only fairly be judged in terms of percentage share of the votes, which is what the polls measure.

22. The interim report of the MRS enquiry was taken by some to suggest that there might be evidence of a long term trend for the polls to overstate support for the Labour party. This has not been true over the last two decades, and the interim report was never trying to suggest that there was a consistent failure in the methodology of the polls, but rather to examine whether or not the historical record of the polls suggested the industry should not over-react to 1992.

23. Historically, there has not been any consistent bias in the polls. (See Appendix 3). Between 1945 (when general election polls were first published in Britain) and 1987, of the 55 published final polls, 31 overestimated the Conservative share of the vote and 22 underestimated it; 31 overestimated the Labour share of the vote and 21 underestimated it.

24. After the 1970 election, the MRS launched an enquiry into the errors of that year (up to that time the worst performance in British polling history), producing a number of recommendations[9], and there was much soul-searching among the pollsters and reappraisal of their methods. When we average the polls from 1974 to 1987 we find that there was a very slight tendency to overstate Labour support (0.3% on average) and to understate the Liberals by the same amount. The overestimates and underestimates of Conservative votes over the period exactly counterbalanced each other. Put another way (judging each election to the nearest full percentage point) Labour's vote was overstated slightly in three of the five elections and understated in two; the Tory vote was overstated in one and understated in two. There is no evidence that the polls have systematically favoured one party or another. On the contrary, until 1992 their margin of error had generally been within the expected limits[10].

25. All in all, we feel that the record of the past has been relatively good. We have borne this in mind in considering how readily to recommend change in the polls' procedures - we have been aware of the risk of throwing out the baby with the bathwater, and have been cautious in recommending change without hard evidence that it would be effective. Consequently, our recommendations are for the most part directed at details of the polling procedure rather than at a wider criticism of the entire process; nevertheless, we feel that they are important, with substantial implications for the entire market research industry.

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[9] Public Opinion Polling on the 1970 Election (Report of the MRS Committee on the performance of polls in the 1970 election); Conservative Central Office also carried out their own enquiry and produced a detailed internal report.

[10] This despite the fact that the polls do not measure postal votes, which in the past have been thought to strongly favour the Conservatives - although, of course, their number, and their effect, is small (probably at most affecting the lead by 1% in any particular election).

3. HOW CAN THE ERROR IN 1992 BE EXPLAINED?

26. Why were the eve-of-poll polls 'wrong' in 'forecasting' the result of the 1992 General Election? Broadly speaking, there are three main potential sources of error.

(1) **The pollster can ask the wrong people,** so that the sample is not representative of the electorate in the answers that they give. This may be (a) sampling variation (the effect of the chances inherent in sampling), (b) bad sample design, or (c) differential refusal to participate by some of the population.

(2) **The pollster can ask the right people, but get the wrong answers.** That may be because people simply change their minds - though they honestly tell the interviewer how they *think* they will vote, they eventually vote for a different party, or do not vote at all. Alternatively, the answers may be deliberately misleading, or in error because those who take part in the survey but fail to reply to the vital question are disproportionately supporters of one party. A further possibility is that the pollster may be misled by putting the questions in the wrong form, order or context; this may make them ambiguous or misunderstood, or may cause bias in the answers that they elicit. These are all factors which might have resulted in a poll getting the wrong result even though it was interviewing a perfectly representative sample.

(3) **The pollster may get the right answers from the right people, yet misinterpret the data.** Moving from the raw data to the finished article - usually a media client's report of a poll's findings - is almost as complex a business as the survey itself. Any of the assumptions involved in producing the final figures - how the data should be weighted, how to interpret 'don't knows' (see glossary), whether to make adjustments to allow for those who say they are less certain to vote - may be wrong. Furthermore, the way in which the poll is reported may be misleading or distorting.

27. Our report will consider each of these possibilities, examining whether the evidence suggests that they were a problem in 1992, and it will examine what steps can be taken to improve performance in the future.

28. The first point to be considered is whether the electors actually changed their minds during the campaign, especially in the last few days when the change was too late to be detected by most or all of the polls. There is a sense in which errors caused by late swing are a different kind of error from those caused by other factors. Polls affected in this way may be 'right' at the time they are taken but 'wrong' in the final outcome. Our investigation of the evidence for a late swing has been fairly exhaustive, because we are conscious that the question needs to be settled as far as is possible before entering upon the more

speculative process of diagnosing and quantifying the other causes of error - polls being wrong in every sense even at the moment the answers were given.

4. DID THE VOTERS REALLY CHANGE THEIR MINDS?

29. We have reached these conclusions with regard to 'late swing':-

(i) There were signs of a swing in the week preceding the final surveys; indeed, this was noted by commentators at the time. It should perhaps have warned all concerned to exercise more caution than they did.

(ii) After the final interviews there was a further swing to the Tories. It seems likely that this was the cause of a significant part of the final error. The scale of this swing was certainly not sufficient to offer the pollsters an excuse for their failure, and most of the discrepancy remains to be explained. Nevertheless, late swing is part of the story. Since it could happen again in the future, pollsters and the media need to bear the possibility in mind and if necessary allow for it in the interpretation of their findings.

(iii) Despite some appearances to the contrary, the British electorate was more than usually volatile in 1992.

30. What we call late swing can be classified into three distinct components:

(a) **Turnout.** Not all those who had said that they would vote and expressed a preference for a party actually went to vote on the day, and some of those who said that they would not vote did so after all. The Conservatives benefitted from the differential effect of these decisions. (Although this 'differential turnout' would normally be distinguished from 'swing' by psephologists, its effect on the polls is the same and is in essence another facet of the same problem - respondents not doing what they had honestly told the pollsters that they believed they would do.)

(b) **Switchers.** Some people who had said they would vote for a particular party decided on the final day to vote differently. All parties lost some voters and gained others but the Conservatives made a net gain; the largest group were people who had said they intended to vote Liberal Democrat but finally switched to the Conservatives.

(c) **Late deciders.** Some people made up their minds to vote and decided which party to support on the very last day. The Conservatives gained more of these last-minute votes than the other parties.

31. We can try to measure each of these aspects directly. We believe all of them played a part in 1992. However, the data are inevitably conflicting or ambiguous. In the light of this, we need to consider, in addition to the direct evidence of late swing, the mood of the electorate throughout the campaign to see if there were clues suggesting that a late swing of unusual proportions could have been anticipated. In the second half of this chapter, we show that the conditions existed to produce a larger late swing than usual in 1992.

4.1. Late swing and differential turnout

32. The most direct evidence of late swing is provided by ICM's post-election recall study[11], in which those voters who were actually interviewed in that company's eve-of-poll survey were questioned again. This survey offers the only direct opportunity to examine how those who were interviewed in one of the final polls actually voted, and found clear evidence of a late swing. Of course, there is some risk of inaccuracy in the reported vote, but this should be minimised by the short period between the election and the date of the recall interviews.

33. Table 7 shows the effect of adjusting ICM's eve-of-poll survey successively for each of the three factors. The last line of the table consequently shows what the result would have been if the changes revealed by the recall could have been predicted in advance.

Table 7: ICM's eve-of-poll survey as corrected in the light of recall survey data

		Con	Lab	Ave error (all parties)	Con lead
Original data	%	38	38	2.3	0
Adjustments for:					
Turnout	%	39	39	2.0	0
Switchers	%	40	39	1.8	+1
Late deciders	%	40	38	1.7	+2

Source: ICM

[11] ICM recontacted 1,203 people out of the original sample of 2,186 interviewed the day before the election (55%). The recall interviews were conducted by telephone wherever possible on 12-15 April 1992 and by postal questionnaires to those who did not give a telephone number, with all questionnaires returned by 23 April included in the figures.

34. The second useful source of evidence is supplied by panel surveys (see glossary). Two national panel surveys were conducted, one by MORI for the *Sunday Times*[12] and one by NOP for the *Independent on Sunday*[13]. The panel respondents were interviewed during the second half of the week in which the election was called, and again during each of the three full weeks of the campaign; they were then reinterviewed after the election to find out which way they had voted[14]. These surveys can, of course, give no direct evidence on **last-minute** swing as their last pre-election interviews were on 3 April, six days before voting. However, they can measure the total swing in the last week, which is an important part of the broader picture. They both found a last-week swing entirely consistent with the last-minute swing recorded by ICM.

35. Over the period of the last week, the panels found a larger swing - 2½% according to MORI, 4% according to NOP. (See Table 8). Perhaps most significantly, MORI's panel shows real evidence of quite significant change among those who had expressed a voting intention in week four[15].

36. The swing suggested by the panels was broadly consistent with the swing recorded by the cross-section polls over the final week of the campaign. The apparent swing back to the Tories over the last week was noted by almost all the commentators at the time, among them David McKie who warned in the *Guardian* on election day of the possibility of further late swing:

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[12] The MORI/Sunday Times panel baseline consisted of a nationally representative sample of 1,544 adults aged 18+ in 65 constituency sampling points, and both initial interviews and re-interviews were carried out face to face; 1,257 (81%) were re-interviewed in the second wave, 1,292 (84%) in the third wave and 1,265 (82%) in the fourth wave. Re-interview responses were weighted by first wave voting intentions to ensure comparability. For the post-election recall, interviewing by telephone only on the Friday (April 10) - the Sunday Times going to press on the Saturday - 934 interviews were achieved (60%).

[13] The NOP panel had 2,155 respondents in its initial wave, interviewed face-to-face; subsequent interviews, by telephone, contacted 1,004 (47%) in wave two, 1,000 (46%) in wave three and 1,006 (47%) in wave four. After the election, 620 were re-contacted on April 10-12 (29%).

[14] A third panel, of a specially selected panel of potentially 'floating' voters, by MORI for the BBC TV programme On the Record, is of less value for quantifying overall change as it was not intended to be representative of the whole electorate, although in fact its findings were fully in line with those of the other two. For details of the findings of all three panels, see Table 62 and appendix 5A and 5B.

[15] Of course, there is always some suspicion of the accuracy of post-election recalls. The number on the panel admitting to having not voted is very low - 6% of those who responded to waves I and V, 5% of those who responded to waves IV and V, which is in stark contrast to the official turnout figures that state that 22.1% of the electorate did not vote. However, this is consistent with other MORI/Sunday Times studies in earlier general elections, suggesting that the most significant 'panel effect' is to heighten interest in the election among panel participants. Of course, real percentage turnout of the electorate is somewhat higher than the official figure in any case, since not all names on the register represent potential votes: there is a significant (unknown) number of electors legitimately registered twice but only permitted to vote once, and the number of 'dead names' (both actual deaths since the register was compiled and names that were mistakenly included in the first place) is also substantial. However, this may be offset by anything up to 9% of the qualified adult population being apparently not registered (see S. Smith, Electoral Registration in 1991, HMSO/OPCS, 1993, for this estimate), although the unregistered population may also be less likely to be polled.

THE TIMES
THURSDAY APRIL 9 1992
Late surge by Tories closes gap on Labour in final hours of campaign
Polls put parties neck and neck

FINANCIAL TIMES
EUROPE'S BUSINESS NEWSPAPER
Opinion polls indicate last-minute swing from Labour to Tories
Election poised on knife-edge

The Guardian
s put parties neck and neck ■ Labour strategists look to tactical voting in marginals ■ Lib Dems fear 'drift' in South
Tory hopes rise after late surge

Daily Mail
30p
TORIES CLOSING IN LAST POLLS

The Daily Telegraph
45p
ESTABLISHED 1855 NO. 42,547
LONDON AND MANCHESTER
THURSDAY, APRIL 9, 1992
Polls put parties neck and neck
'It's going to be a sunny day – a sunny day in every way,' says Major as Tories narrow gap

Daily Express
WEATHER COLD AND WINDY
30p
TORY SURGE
olls show
ate boost
or Major

DAILY STAR
BRITAIN'S BRIGHTEST NEWSPAPER
THURSDAY, APRIL 9, 1992
25p
MAJOR GETS POLL PERK
Tories boosted by late surge

Evening Standard
IT'S UP TO THE DON'T KNOWS
Neck and neck say polls but Tories gaining speed

'ICM puts the swing over the last week at 2%. Should this swing have continued after the pollsters had put away their clipboards, the Conservatives, written off for most of the campaign, could yet finish up ahead of Labour.'

The same swing back to the Conservatives over the last week was also recorded by MORI and Harris in their cross section surveys (3% and 2% respectively) though not by Gallup (¼%) or NOP (½% swing to Labour); almost all the newspapers reflected the apparent swing in their election day headlines.

37. The findings of one of the two surveys undertaken by the British Election Survey, a panel study which re-interviewed respondents who had first been interviewed in 1987[16], agrees in general with the other evidence on changes of opinion, although its design is not well-suited to investigating late swing because it interviewed its respondents over several weeks during the campaign.

How big was the late swing?

38. Table 8 compares the findings of each poll series or panel over the last week and, where possible, the reported vote of participants when re-interviewed after the election. The swing over this period breaks down into two sections, shown in the first two columns of the right hand half of the table. The first column shows swing between the start of the final week and the end of eve-of-poll interviewing, comparing the findings of the penultimate and final surveys in each of the five main poll series. The second column shows swing between the end of interviewing and the vote itself. Only ICM, who re-interviewed their eve-of-poll sample, directly gives a measure of the swing over this period. The table for completeness also includes the swing implied by comparing Gallup's final poll with their post-election poll (which used a fresh sample) and comparing the eve-of-poll cross-section surveys by MORI and NOP with each company's post-election recall of its panel, but it should be borne in mind that here we are not comparing like with like and there are theoretical objections to measuring swing by comparing cross-sections with panels, so the figures should be treated with caution. The final column measures swing over the whole period.

[16] Telephone interviews were conducted during the course of the whole campaign among original respondents on the survey who were available by telephone, who were willing to be re-interviewed and who could be found. In all 1,050 interviews were conducted during the campaign out of an original sample of 3,826 (27%). Respondents were contacted again after the election to check on what they had actually done. Because of the high level of attrition the achieved samples were weighted on the basis of declared voting behaviour at the 1987 election (as given by the whole BES sample). The panel was unrepresentative of the 1992 electorate, since it consisted only of survivors of a panel originally recruited in 1987. This means, of course, that it excluded entirely all those not registered to vote in 1987 (including all those who were too young, amounting to about 10% of the adult population); thanks to five years' attrition it consisted of only those members of the original panel who were stable enough in their lifestyle and housing to be re-contacted after that time, very possibly an atypical group. A full explanation of the findings of the BES survey are to be found in A. Heath, R. Jowell and J. Curtice with B. Taylor, Labour's Last Chance? (Aldershot: Dartmouth, 1994).

Table 8: Evidence of late swing in the polls

	Start of final week	Eve of poll	RE-CALLED VOTE	Change - last week		Change - last day		Change - total	
ICM/*Guardian*	31/3, n=1126	8/4, n=2186	12-15/4, n=1203						
Conservative	37	38	40	+1	2% swing	+2	1% swing	+3	3% swing
Labour	41	38	38	-3		0		-3	
Lib Dem	18	20	18	+2		-2		0	
MORI/*Times*	30/3, n=1080	7-8/4, n=1731							
Conservative	35	38		+3	3% swing				
Labour	42	39		-3					
Lib Dem	19	20		+1					
Harris/ITN	29-30/3, n=2152	4-7/4, n=2210							
Conservative	35	38		+3	2% swing				
Labour	41	40		-1					
Lib Dem	19	13		-1					
Gallup/*Telegraph*	2-3/4, n=1043	7-8/4, n=2478	10-11/4, n=1880						
Conservative	37.5	38.5	39	+1	¼% swing	+½	¼% swing	+1½	½% swing
Labour	37.5	38	38	+½		0		+½	
Lib Dem	20.5	20	19	-½		-1			
NOP/*Independent*	31/3-1/4, n=1302	7-8/4, n=1746							
Conservative	37	39		+2	-½% swing				
Labour	39	42		+3					
Lib Dem	19	17		-2					
MORI/*S. Times* panel	1-3/4, n=1265		10/4, n=934			*Swing from MORI/Times*		Swing in panel	
Conservative	37		40			+2	2% swing	+3	2½% swing
Labour	39		37			-2		-2	
Lib Dem	21		21			+1		0	
NOP/*Ind. on S.* panel	2-3/4, n=1006		10-12/4, n=620			*Swing from NOP/Indpt*		Swing in panel	
Conservative	38		42			+3	4% swing	+4	4% swing
Labour	41		37			-5		-4	
Lib Dem	17		17			-2		0	
RESULT			9/4						
Conservative			43						
Labour			35						
Lib Dem			18						

39. ICM found all three of the component factors of late swing - switching, late decisions and differential turnout - were present in 1992, and together accounted for about a quarter of the total difference between the final poll and the outcome of the election, or a late swing of about 1%.

Who claimed to have decided at the last minute?

40. If the Conservatives won as the result of a late swing, it might be argued, this ought to be detectable in the votes of those who claimed that they decided late. Gallup found an advantage to the Conservatives, but not of such proportions as to explain the error in the eve-of-poll surveys: 12% of Conservative voters said they decided 'in the last few days' compared with 9% of Labour, equivalent to a swing of about 1% (and part of this presumably took place before the eve-of-poll surveys). 11% of each group said they had decided 'two or three weeks ago'. The MORI *Sunday Times* panel recall found a similar number of late deciders, but the party effect was different, being fractionally to Labour.

41. However, it appears that those who wavered in the last week had predominantly intended to vote Labour before they wavered; consequently an even split of their votes between the parties amounts to a swing to the Conservatives - although the late deciders voting for each party at close of play were approximately equal, a greater proportion of the late deciders who voted Conservative represented real gains. The MORI/*Sunday Times* panel gives evidence of this: of those who said they intended to vote Labour at the last pre-election interview (a week before voting), 8% subsequently said they made up their mind 'in the last 24 hours' and a further 12% 'during the last week' (most of which was after interviewing ended); this compared to 3% and 8% respectively among Conservatives.

Further evidence on differential turnout

42. The constituency election results offer further evidence to suggest that Labour supporters were in fact less likely to turn out. Across the country as a whole, turnout was, at 77.9%, 2.4% higher than in 1987. But it rose more in the South of England and the Midlands, where the Conservatives were strongest in 1987, than elsewhere. Equally it rose more in (Conservative) rural seats than in (Labour) urban ones. Indeed, the stronger the Conservatives were in a constituency, the more that turnout rose, while it fell in many safe inner city Labour seats. Overall, in those seats won by the Conservatives in 1987 turnout rose in 1992 on average by 3.5%, while in Labour seats it barely changed (+0.1%)[17].

43. This pattern strongly suggests that compared with the position in 1987 Conservative supporters were more likely to turn out and voting than were Labour supporters. This may not have been because more Labour supporters stayed at home than did in 1987, but because more Conservative supporters who

[17] J. Curtice and M. Steed in D. Butler and D. Kavanagh, The British General Election of 1992 (Basingstoke: Macmillan, 1992).

abstained in 1987 turned out in 1992. The results of both the 1992 BES cross-section[18] and panel studies suggest that this was the case. Whereas both reveal similar proportions of those who voted Labour in 1987 saying they did not vote in 1992[19], they both show a clear Conservative advantage in 1992 amongst those who abstained in 1987. In the cross-section study 26% of 1987 abstainers said that they voted Conservative, while only 19% voted Labour. In the panel study the respective figures are 30% and 24%[20].

44. It must remain a moot point how far this greater propensity among Conservative supporters to turn out and vote was actually reflected in the answers respondents gave to the polls before polling day. There has long been evidence to suggest that some electors are not able to predict accurately whether they will in fact turn out and vote[21]. Insofar as this was the case in 1992, the possibility that the polls were partly wrong because of differential turnout must be taken seriously.

Evidence from the Post-Election Polls

45. Further evidence that 'late swing' was part, but only part, of the explanation for the difference between the final polls and the eventual outcome also comes from four face-to-face polls undertaken soon after polling day which asked voters how they had voted. Their findings are set out in Table 9. While these were closer to the actual outcome than were the final pre-election polls, on average they still underestimated the Conservatives' lead.

[18] A random sample of 3,534 adults on the electoral register, including a boosted sample of 957 in Scotland, representing a response rate of 73%, were interviewed between 10 April and July 1992. For full details see A. Heath, R. Jowell and J. Curtice with B. Taylor, Labour's Last Chance? (Aldershot: Dartmouth, 1994), pp 302-4.

[19] In the cross-section study 8% of those who said they had voted Labour in 1987 abstained in 1992, compared with 7% of 1987 Conservative voters. In the panel study the figures are 6% and 8% respectively.

[20] The percentages are based on 332 in the cross-section study who abstained in 1987, and on 223 in the panel.

[21] C. Marsh, 'Predictions of Voting Behaviour from a Pre-Election Survey', Political Studies, 1985.

Table 9: Reported vote in surveys after the 1992 election

		Con	Lab	LDem	Oth	C lead
Post-election cross-section polls with fresh samples						
Gallup post-election (10-11 April)	%	39	38	19	4	+1
ICM Rowntree Reform Trust poll (10-11 April)	%	40	38	19	4	+2
MORI post-election (25-28 April)	%	39	37	18	6	+2
ICM post-election (8-9 May)	%	44	37	15	4	+7
Average	%	**40.5**	**37.5**	**17.8**	**4.5**	**+4.0**
RESULT	%	**42.8**	**35.2**	**18.3**	**3.7**	**+7.6**
Error	%	*-2.3*	*+2.3*	*-0.5*	*+0.8*	*-3.6*
Surveys re-interviewing respondents to pre-election polls						
ICM recall survey	%	40	38	18	4	+2
MORI Panel recall	%	40	37	21	2	+3
NOP Panel recall	%	42	38	17	3	+5
Average	%	**40.7**	**37.7**	**18.7**	**3.0**	**+3.6**
RESULT	%	**42.8**	**35.2**	**18.3**	**3.7**	**+7.6**
Error	%	*-2.1*	*+2.5*	*+0.4*	*-0.7*	*-4.0*
Exit Polls						
NOP Exit poll	%	40.0	36.3	18.3	5.4	+3.7
Harris Exit poll	%	41.1	36.3	18.0	4.6	+4.8
Average	%	**40.6**	**36.3**	**18.2**	**5.0**	**+4.3**
RESULT	%	**42.8**	**35.2**	**18.3**	**3.7**	**+7.6**
Error		*-2.2*	*+1.1*	*-0.1*	*+1.3*	*-3.3*

46. If the polls' problem had been *only* late swing, we would expect these surveys taken after the election to be very close to the true result, but they were not. Either their samples were imperfect or the answers they received were misleading. Furthermore, as Table 9 shows, the reported vote in the final wave of the panels and in ICM's recall survey also understates the Conservative lead. This may suggest that the problems they suffered were similar, and that the errors in 1992 did not arise from unique problems peculiar to that election campaign. Together the two sources of evidence suggest the existence of a pro-Labour imbalance in the 1992 final polls which accounts for at least half of the difference between the results of the final pre-election polls and the eventual outcome. Nevertheless, they also confirm that part of the problem was, indeed, late swing.

4.2. Volatility of the electorate

47. Some commentators have argued since the election that late swing should be discounted as an explanation since they saw no evidence that the electorate was any more volatile than in past elections, when late swing has been very limited. Why should we believe in a sudden late swing if the electorate was, up to that point, as stable in its views as normal? In any case, there is in fact concrete evidence that the electorate was more volatile in 1992 than in the three previous elections: the campaign panels, the only direct evidence of mind-changing during the campaign, show this clearly.

Evidence of gross change

48. Panels offer the best means of examining the volatility of the electorate during the campaign[22]. The snapshot cross-section polls are at best able to measure net movement, when a much larger movement is certainly going on under the surface with respondents moving in opposite directions cancelling each other out. Panel surveys can go further, and directly measure gross change. By re-interviewing the same respondents they are able to detect all the movement of opinion occurring within the sample.

49. Of course, panels can have their own disadvantages. A sampling error in the initial wave of a panel will persist in a way that it would not in a series of snapshot polls. There are also two specific 'panel effects' - attrition (that is, distorting effects caused by inevitable drop-outs from the original sample - see glossary) and panel conditioning (the simple fact of participating in the panel making the participants atypical and unrepresentative of the population). These potential problems must lead to some caution in interpreting panel results but do not outweigh the major advantages of our ability to look at gross change.

50. We are able to examine two groups separately, the 'switchers' (those who transfer their loyalties directly from one major party to another - see glossary) and the 'churners' (those who switch from don't know to a major party or vice-versa)[23]. We can see the history of the size of these groups from the findings of MORI's *Sunday Times* panel at each of the last four elections. Table 10 shows the percentage of the panel in each case whose responses at the start of the campaign were different from those they gave at the final wave of interviewing before polling day; these are broken down into those who switched

[22] For an extensive review of panel studies, see I. Fallon and R. Worcester in I. Crewe and B. Gosschalk, Political Communications: the British General Election of 1992, Cambridge University Press. (In Press).

[23] Unfortunately, panel studies are not an ideal tool for getting a good overall picture of 'don't knows' or of those who ultimately failed to vote. By their nature, panels are subject to a 'selection effect' (those least interested in politics are most likely to drop out or to refuse in the first place) and a 'conditioning effect' (the effect of being repeatedly interviewed may make the panel members atypical, in particular by stirring up their interest in the election); consequently panels tend to have fewer 'don't knows' than the population as a whole, and they may well be unrepresentative of all 'don't knows'. In fact the numbers in these categories in the panels were far too small to draw any statistically significant conclusions about them, although they can contribute to the overall figures.

support between the three main parties and the 'churners'. (For fuller details of the figures see Appendix 5A.)

Table 10: Changes of mind during election campaigns 1979-92

	'Switchers' % of electorate switching between main parties during campaign	'Churners' % of electorate switching to or from others/don't knows	Total % of electorate changing answers during campaign
1979	5.6	6.9	12.5
1983	7.8	7.1	14.9
1987	8.4	10.1	18.5
1992	9.4	11.6	21.0

Source: MORI/Sunday Times panels

51. In 1992, MORI's figures (Table 11) are closely confirmed by NOP's panel study for the *Independent on Sunday* (Table 12), though there are no data from earlier years from that study.

Percentages of the electorate switching during the campaign 1992 (not including final week)

Table 11: Switching in the 1992 MORI/*Sunday Times* panel

One week before election %	Week 1 (After election announced)				
	Con	Lab	LD	Oth/DK	TOTAL
Con	(29.2)	0.8	1.4	3.5	5.7
Lab	1.4	(29.8)	1.2	4.6	7.2
LD	2.6	2.0	(12.9)	2.1	6.7
Oth/DK	0.6	0.6	0.2	(7.1)	1.4
TOTAL	4.6	3.4	2.8	10.2	21.0

Source: MORI

Table 12: Switching in the 1992 NOP/*Independent on Sunday* Panel

One week before election %	Week 1 (After election announced)				
	Con	Lab	LD	Oth/DK	TOTAL
Con	(32)	1	1½	2½	5
Lab	1	(32½)	2	4	7
LD	2½	2	(9½)	2	6½
Oth/DK	½	1½	½	(5)	2½
TOTAL	4	4½	4	8½	21

Source: NOP

Admitted changes of mind

52. Although there was more **behavioural** volatility in 1992 than previously, there is no evidence that the social psychology of the electorate was any different - the voters did not 'feel' more volatile. But this merely emphasises that the potential for substantial sudden swing has been present in the past - what was different in 1992 was that the electorate were more likely to act upon it.

53. For example, questions asking respondents how likely they thought they were to change their minds, elicited broadly similar responses to previous years. But these still revealed very considerable potential for volatility, even if the potential has not been realised at previous elections. 42% of respondents to the first wave of interviews in the MORI/*Sunday Times* panel (11-12 March) said either that they were undecided which way they would vote (14%) or might consider switching from the party they then supported (28% of the entire panel and 32% of those naming a party).

54. In MORI's post-election omnibus survey (25-28 April 1992), respondents were asked to choose between the three options:

"Q. Which of these descriptions applied to you during the election campaign?"

"I never seriously considered switching my vote to another party"	68%
"I did seriously consider switching my vote to another party, but decided not to"	18%
"I did switch my vote from another party"	11%
Don't know	3%

Source: MORI
Base: All who said they voted (1,623)

In other words **three voters in ten** were prepared to admit, even after the event, that they had at least wavered.

55. The 'when did you decide?' questions tell a similar tale. Both Gallup and MORI in their recall surveys produced figures comparable to those in previous elections, which were nevertheless high. When Gallup asked 'How long ago did you decide which way you would finally vote?', 73% said 'a long time ago', 13% 'two or three weeks ago' (i.e. during the campaign) and 14% in the 'last few days'. The number admitting having decided late was no higher than in previous elections[24]. The MORI recall of the *Sunday Times* panel found a similar number of late deciders to Gallup. But the figures indicate that, far from being stable, the electorate has always had the potential for substantial volatility. (The Harris exit poll, in fact, did find unusually high numbers admitting late decisions: 21% claimed to have decided in the last week and a further 9% on polling day itself.)

56. The 1992 BES cross-section study, too, found comparable figures in 1992 to those in the past, as Table 13 illustrates, but once again it is equally clear that the number of potential switchers has always been high and the possibility of a late swing has always been present.

Table 13: Time of decision 1974-92

	% of voters who said they decided during campaign	% of voters who said they thought of voting for another party
Feb 1974	23	25
Oct 1974	22	-
1979	28	31
1983	22	28
1987	21	28
1992	24	26

Source: BES

57. The volatility of the electorate in 1992 suggests a substantial potential for late swing. The fact that this potential may have been equally present in previous elections, yet remained unrealised, does not weaken this argument but, rather, emphasises the possibility that late swing may occur again at future elections.

58. A feature of the 1992 campaign seems to have been that the electorate were unaware how volatile they were. Considerably more of the MORI panel admitted after the election that they had come to a decision at the last moment than had earlier indicated that they were in any doubt. A week before the election only 10% of the total said that they might change their vote; when it came to the point, 15% of those who said

[24] Compared to the 73% who claimed to have decided 'a long time ago' and 13% 'two or three weeks ago' in 1992, Gallup found 73% and 15% respectively in 1979, 78% and 14% in 1983, and in 1987 when the question was asked regularly throughout the campaign the figures were 73% and 14% at eve of poll; hence only in 1983 does there seem to have been significantly less last-minute decision making.

they actually voted had changed parties. This in its turn is well short of the 21% who later said that they decided within the last week[25]. This evidence is what we would expect if there was a late swing: voters who had earlier been convinced that their minds were made up changed their opinions right at the end of the campaign, and only in retrospect was it possible to detect the degree of uncertainty which ultimately existed.

4.3 Late swing: conclusion

59. We estimate that late swing, in all its forms, probably accounted for between a fifth and a third of the total error in 1992. The net effect of this late swing is relatively small in statistical terms but the increase in the Tory lead could have made the difference between the polls suggesting that Labour would be the largest single party in a hung parliament and suggesting that the Conservatives would be the larger of the two, a fraction short of an overall majority.

60. Although the electorate was unusually volatile in behavioural terms - more of them actually changed their minds than in the past - they do not seem to have **felt** more volatile than at previous elections. The potential for a late swing has always existed. The possibility of its happening again cannot be discounted, and pollsters need to be prepared for its consequences as far as possible. We discuss electors' inability to predict their own behaviour, especially in the context of a close election, in chapter 9.

61. Meanwhile, we have seen that late swing explains only part of the poll error; we have still to explain most of the problem. Were the samples unrepresentative? Were the answers the pollsters were given misleading? Was the data misinterpreted? These are the questions we must examine next.

[25] Taking into account the whole election campaign, 27% of the MORI panel said they might change party at the first survey, and a further 4% of those who claimed to be committed did in fact change by polling day. This 31% compares with the 37% who said that they finally made up their minds after the campaign started. Of those who said at wave I that they had 'definitely decided' which way they were voting and that there was not 'a chance that [they] might change [their] mind', 15% said on the recall wave that they finally decided during the campaign. There was a distinct party effect, with only 8% of initial 'committed' Tories but 17% of 'committed' Labour and 23% of 'committed' Liberal Democrats subsequently wavering.

5. WOULD IT HAVE BEEN BETTER WITH BIGGER SAMPLES?

5.1 What is Sampling Error?

62. One of the most basic tasks in survey research is to ensure that the sample interviewed is representative of the population being observed. If the polls were wrong in 1992, the possibility that they interviewed the wrong people is perhaps the simplest and most obvious explanation. This could arise in a number of ways.

63. Even the most efficient polls are subject to the risk of sampling variation. Since only a sample rather than the whole population is interviewed, they are dependent on the law of averages to give them a representative sample. The extent of likely error is limited, and can be mathematically calculated[26].

64. However, if flaws enter the sampling procedure, other errors are possible. Any procedure which allows a *biased* sample - one which makes it more likely that one section of the population will be interviewed than another - is obviously dangerous. Of course, it is not enough to eliminate bias in deciding who to approach for interviews - the most perfect sample survey cannot expect to achieve 100% response, and bias may enter by the refusal of some groups to participate. Some means of compensating for this possibility is necessary. One means of attempting to achieve this is 'quota sampling', which targets types rather than individuals for interviews, attempting to make samples representative of the population by ensuring that they conform to the appropriate proportion of various demographic sub-categories; quota sampling is used by most modern opinion polls in Britain, and was the methodology used by all the election polls in 1992. Weighting is also used to add further accuracy. We have to see how far these methods were successful in eliminating the possibility of bias in the sample.

65. Another problem for pollsters is to ensure that they are sampling the right population. The polls for the most part focus on the adult population of Great Britain; if those with the right to vote are a significantly different group, or if the pollsters fail successfully to isolate the subsection who will actually vote, error could creep in.

66. The polls in 1992 might have gone wrong in any of these respects, and consequently ended with unsatisfactory samples.

[26] Although, as we discuss below, strictly speaking these mathematical calculations of probable error apply only to pure random samples.

67. One of the criticisms that is sometimes levelled at surveys is that they rely on samples that are too small; with a sample size of between 1,000 and 2,000, it is suggested, you cannot hope to get a representative picture of the nation's opinions except by a fluke. This criticism is misconceived, and statistical error arising from small sample sizes - sampling variability - cannot have contributed significantly to explaining the error of the polls in 1992.

68. **Sampling variability** must be distinguished from **sampling bias**, which we discuss in the next chapter. Sampling variability is the inevitable mathematical consequence of probability: you cannot draw a perfect sample every time. In layman's terms, sampling variability is that aspect of polling where the pollsters are forced to rely on the law of averages, and where the degree of accuracy could theoretically be improved by using bigger samples. Sampling bias, by contrast, is a systematic error in the sampling process which makes some types of individual more likely to be selected than others, and which consequently skews the results; this is not related to sample size, since a large but biased sample will perform just as badly as a small one.

69. The variation between individual polls during the 1992 campaign was comparatively small. (See paragraphs 4 to 7). A fraction at most of this variation is accounted for by systematic differences between the companies. The remaining variation between the polls taken at the same time was probably the effect of sampling variability, as was most of the variation over time. Allowing for these factors, we find the polls in substantive agreement, and still adrift from the actual vote. The difference between the final polls and the actual result is certainly not to be accounted for by sampling variation.

5.2 The Theory of Sampling Variability

70. When we use a sample to produce estimates of the characteristics of a whole population we have to accept a degree of imprecision. We cannot expect any single sample to be a perfect representation of the population. Nor can we expect any two samples to be perfect replicas of each other. This is the concept of sampling variability; it underlies the cautionary disclaimer almost always attached to reports of opinion poll results, the warning that the results are subject to sampling error of, say, plus or minus three percent for a sample of 1,000 people.

5.3 The Extent of Variability

71. How much variation should we expect in the polls? Unfortunately, this is a very complex and technical question, which causes much confusion for the layman and is difficult to convey simply (above all in a

popular newspaper). The usual assumption is that the margin of error can be calculated by a relatively simple formula based on sample size, but strictly speaking this formula applies only to one-off pure random (or probability) samples (see Appendix 4 and glossary); for normal two-stage, clustered, random samples, and for quota samples, the position is more complicated.

72. The extent of variability - or the size of likely error - attached to any sample-based statistic can be estimated from the results of a single survey. With complex sample designs, as must be used in the polls, however, this can be a demanding and laborious process.

73. An alternative approach is to use variations *between* different polls as empirical estimates of sampling variability. In this approach, a set of polls is regarded as a series of independent replications of the same process: this is a valid assumption provided that the polls used essentially the same methodology and that there was no movement in the variables being estimated (or, alternatively, that the pattern of movement is known). We know that the methods used by different pollsters vary little; if they produce systematically different results the difference is too small to be readily detected[27]. We also know that, over the four-week campaign period in 1992, there was little underlying movement in aggregate reported voting intentions, except for the rise in Liberal Democrat support. We can therefore proceed by assuming, after allowing for this minor trend, that we would expect all polls to produce the same estimates; variations between them are taken to arise from sampling variability; this enables us to achieve a numerical estimate of the variation.

74. Analysis of variations between 50 polls carried out during the campaign gives us an estimate of about 1.5% as the 'Standard Error' of an estimate of a single party's share of vote. (This result is almost identical to that calculated in the same way after the 1987 General Election[28].) The standard error is a measure of the extent of imprecision inherent in any given poll result. It is the kind of fluctuation from overall trend that must be *expected* to arise. We should not be surprised at even greater fluctuations: for example, one in twenty poll estimates can be expected to deviate from the overall trend by more than twice the standard error (plus or minus 3% in the case of a single party share). This is the '95% Confidence Interval' (the level of accuracy that 95% of polls can statistically be expected to achieve) and is the figure usually quoted as an estimate of the sampling error of a poll's results. On the other hand, most polls will be more accurate than this - nineteen in twenty can be expected to be within the 3% band, but half can be expected to have an error of no more than 1%.

[27] Clifford and Heath's analysis in A. Heath, R. Jowell and J. Curtice with B. Taylor, Labour's Last Chance? (Aldershot: Dartmouth, 1994) found evidence of a company effect, but its impact was very small.

[28] M. Collins, 'Lessons from the Polls' (1988 MRS Conference Papers).

75. Our empirical analysis of variation between the polls confirms that the standard ±3% margin of error usually quoted is a realistic statement of the sampling error normally to be expected in opinion polls.

76. Any poll's estimate of the gap between two parties (e.g. between Labour and Conservative) will be less precise, since it is calculated as the difference between two imprecise estimates. From the same 50 polls we can estimate the standard error of the gap between the two largest parties at about 2.5%. The 95% confidence interval is plus or minus 5%. (Again these are very similar to the estimates derived in 1987.) Thus, we should not be surprised if a single poll produces an estimate of the gap or lead which deviates from the overall trend by 5%. In a series of 50 polls we expect two or three to deviate from trend by even more. In 1992, two did so: a Harris poll for the *Daily Express* in the second week of the campaign showing a 5% Tory lead, and a MORI poll for *The Times* in the third week showing a 7% Labour lead. Even these deviations *could* be due to sampling variability, although the MORI 7% lead, which coincided with leads of 6% (Harris) and 4% (ICM) in other polls, may be a reflection of a genuine movement.

5.4 The Effect of Averaging Polls

77. These margins of error sound large, but they are not nearly large enough to explain the error in the polls' estimates of the final result. The results of a single poll, using a sample of around 1,500 or 2,000, will be subject to some imprecision. As an example, the last poll in the campaign - an ICM poll for the *Guardian* carried out on 8 April - produced an estimate that Labour and Conservative were exactly level in terms of voting intentions. The 95% confidence interval around this estimate (on the assumptions outlined above) was of a 5% Labour lead through to a 5% Conservative lead: a wide range, but unlikely at the margins. Much more likely, a two-to-one bet, is that it would fall within a 2.5% Conservative to 2.5% Labour range.

78. Even for this one poll, however, sampling variability is an extremely unlikely explanation of the discrepancy between the poll estimate and the result the following day of a Conservative lead of close to 8%. And we are concerned in this report not with a discrepancy for a single poll but a systematic discrepancy for many different polls.

79. The main determinant of sampling variability is the size of the sample: the larger the sample, the lower the sampling variability. When we look at the average results yielded by a number of polls we are effectively considering a larger sample. Thus, we saw in Chapter 1 that the four polls carried out in the last two days of the campaign suggested, on average, a Labour lead of about 1%. With a combined sample size close to 8,000, the 95% confidence interval around the average estimate of the gap between the two

largest parties was roughly from a Labour lead of 3% through to a Conservative lead of 1%: a much narrower range, well distant from the actual result.

80. If we look at the campaign as a whole, the effect is even more dramatic. Then we have 50 polls, with an aggregate sample size of around 75,000 interviews. In terms of sampling variability, the average estimate, of a Labour lead of about 1.5%, had high precision. The 95% confidence interval - about one-third of one percent either way - would encompass only a range in the Labour lead from just over 1% to just under 2%. Plainly, this slight imprecision is not even a significant contributory explanation of the difference between the polls' estimate of a Labour lead of 1.5% and the outcome of a Conservative lead of 7.6%.

5.5 The ICM Press Association Poll

81. In confirmation, we can note the results of the very large poll carried out by ICM for the Press Association about a week before the election. With over 10,000 interviews, this poll would be expected to have high precision, with 95% confidence intervals of about plus or minus 0.5% on a party share and about plus or minus 1% on the lead.

82. In the event, the poll suggested a Labour lead of 2.5%, slightly greater than the average estimate of the lead in other polls at the time, but close in party vote shares to all of them. (See Appendix 1). In other words, the bigger sample made no appreciable difference to the findings[29]. Sampling variability can probably explain why the poll deviated somewhat from others; it does almost nothing to explain why its results bore little resemblance to the actual election result.

[29] The same was true of the 1991 Press Association poll, which, with a 10,000 sample, yielded findings closely in line with the other polls (based on smaller samples) conducted at the same time.

6. WERE THE SAMPLES CHOSEN WRONGLY?

83. In the previous chapter we examined one type of sampling error, ***sampling variability***. A second, and potentially much more dangerous form of sampling error is ***sampling bias***: this is what happens when there is a systematic flaw in the method of drawing samples which skews the whole process with the result that the average sample - the 'expected' result - will be unrepresentative. We concluded that sampling variability does nothing to explain the error in 1992; sampling bias, by contrast, we believe to have been a significant contributory factor.

6.1 Quota sampling

84. The vast majority of polls published during the campaign used a very similar quota sampling methodology. Quota sampling is a method by which interviewers instead of targeting specific individuals pre-selected from a list (as in random sampling) target ***types*** of individuals to fit pre-determined target numbers or 'quotas'. The population is divided up into a number of categories, and the number in each category or 'cell' of the quota reflects that category's size in the population as a whole - for example, if a quota was set by age the interviewers might be instructed to carry out 13% of their interviews with respondents aged 18-24, since that is the proportion of 18-24 year olds in the entire adult population.

85. The purpose of quota sampling is to ensure that the sample is representative of the population when judged by the variables used in the quotas (e.g. age), and it is hoped thereby to ensure that it will also be representative of the other factors which the survey is attempting to measure (e.g. voting behaviour). The quota methods used by most polling companies aim to mirror the structure of the adult population within selected variables. Typically they ensure that the achieved sample is representative by sex, by age (up to four age groups would be specified), by social class (up to four groups) and some polls include working status.

86. These methods of necessity simplify the segmentation of the adult population and there is a possibility that within each cell of the quota an unrepresentative sample is contacted. The risk that any sample may look representative by accepted classification methods but contain some other bias remains. This may be particularly likely if those within any cell who are most available to be interviewed, or who are most willing to be interviewed, are unrepresentative. This is the next possibility that we must investigate: there are clear signs that the samples achieved were, indeed, unrepresentative. If so, are there alternative or variant methods available which will reduce the risk or enable the pollsters to compensate for it?

How the quota method was operated in 1992

What were the quota controls used?

87. All the companies used virtually the same basic sets of variables in their quota controls. Age, sex and social grade (see glossary) featured in all quotas and most companies also used working status. The National Readership Survey (NRS)[30] was generally used as the source for quotas, with additional use of OPCS mid-1990 population projections and by NOP of their own random omnibus survey (which in turn is weighted on the basis of NRS and OPCS figures). There were variations in the extent to which the quota variables were interlocked with each other. Table 14 summarises the quotas set by each of the polling organisations.

Table 14: Quota controls used by each polling organisation

		Number of cells	
Gallup	*Telegraph*	22	Five age (18-24, 25-34, 35-44, 45-54, 65+), within sex; four class (AB, C1, C2, DE) within sex; working/not working within sex.
Harris	ITN	12	Four age groups within sex (18-29, 30-44, 45-64, 65+); four class (AB, C1, C2, DE).
Harris	*Observer*	12	Four age groups within sex (18-24, 25-44 ,45-pension age, over pension age); four class (AB, C1, C2, DE).
Harris	*Express*	12	Four age groups within sex (18-24, 25-44 ,45-pension age, over pension age); four class (AB, C1, C2, DE).
ICM	*Guardian*	12	Sex, three class (ABC1, C2, DE), four age (18-24, 25-44, 45-64, 65+) and three working status (Full time, part time, not working).
MORI	*Times*	12	Sex, four age (18-24, 25-39, 40-retirement age, pensioner), four class (AB, C1, C2, DE), working status (working, not working).
NOP	*Independent*	13	Three age (18-34, 35-54, 55+) within three class (ABC1, C2, DE), Working status within sex (Male full time, male other, female full or part time, female not working).

88. With one exception, these quotas were operated through taking an equal number of interviews at each of a number of constituency sampling points, chosen collectively to be representative of the nation as a whole. (Each company had its own individual set or sets of sampling points; we discuss the selection of sampling points below, paragraph 230 *et seq*).

89. The Gallup sample design for their final poll was unlike the others. It was a mixture of 150 representative sampling points with 10 interviews in each and an additional 50 Conservative marginal seats

[30] The National Readership Survey is a random survey consisting of over 30,000 interviews a year, undertaken on a continuous basis by Research Services Limited, which measures a number of demographic variables. Up to 1992 the sampling frame was the electoral register. For further details see National Readership Survey: Tables Relating to January to December 1992 (London: NRS, 1993).

with 20 interviews in each yielding about 2,500 interviews. Substantial weighting was applied to correct the oversampling in Conservative marginals[31].

90. If the quota system were operating effectively, we would expect the polls' measures of other demographic variables not included in the quota to be fairly accurate. As we shall see, this was not the case. The estimates of at least two important variables were very poor - the samples were unrepresentative. This led naturally to error in the voting intention estimates.

How did the pollsters weight their findings?

91. Hand-in-hand with quota techniques goes the weighting of the data. Most pollsters applied corrective weighting to their polls. Those who did apply weights made sure that any small failures in the field to meet quotas were corrected. MORI and NOP used additional variables in their weighting which were not part of the quota controls.

92. Weighting is a useful and normally necessary adjustment to check and correct for any deviation. However, the investigation of classification variables indicates that the use of these additional variables in weighting in 1992 probably made the voting intention estimates worse by inflating Labour support. But this seems to have arisen for the most part from the use of inaccurate data as a basis for weighting; had the weightings been correct, the adjustments could have improved the estimates.

Table 15: Weighting strategies used by each polling organisation in final polls

Gallup	*Telegraph*	Weighted for sex, class, region and marginality of constituency to correct the oversampling of Conservative marginals.
Harris	ITN	No weighting applied to final poll
Harris	*Observer*	No weighting applied to final poll
Harris	*Express*	No weighting applied to final poll
ICM	*Guardian*	Weighted for age, sex, class, working status and region.
MORI	*Times*	Weighted for age, sex, class, phone ownership, housing tenure and region.
NOP	*Independent*	Weighted for age, sex, class, working status, housing tenure and car ownership.

93. One conclusion that has emerged from our investigation is that there are a few cases where decisions of weighting were not taken on a consistent basis. Although this does not seem to have been a serious contributory factor to the 1992 error, it is potentially dangerous. It is preferable for weighting strategies

[31] This unusual design may conceivably explain why Gallup were the only company to find a slight Conservative lead in their final poll, although their polls throughout the campaign were a little more Tory than the average of the other polls.

to be designed in advance and adhered to throughout a poll series, rather than based on ad hoc decisions taken for each particular poll.

94. In detailed examination of the quotas set and achieved, and of any corrective weighting, a few minor inadequacies were found. For example, one company used the proportion of 15-24 year olds in their quota for 18-24s; another used somewhat out-of-date NRS statistics as the basis of their quota controls while another used an alternative social grade profile not based on NRS results (and found fewer ABC1s). But minor inadequacies of individual polls do not contribute to the explanation of the generally poor performance of the polls collectively. We need to find some significant shortcoming that was common to all.

Variations in poll findings on classification variables

95. We can test how well the polls succeeded in achieving representative samples by examining their measures of classification data - demographic information for which reliable objective data is now available for comparison from other sources. The polls differed considerably in the amount of information collected. Some gathered a lot of data on their respondents while others restricted themselves to just those variables used in the quota controls. However, there is some evidence that the general sampling methodology and quota controls employed failed to provide consistent and valid measures of certain key variables. In particular, the measures of housing tenure and car ownership give indications that the general quota sampling approach used may have had some weaknesses. Each of these variables should have remained unchanged during the campaign and we would expect the polls to produce a range of estimates normally distributed around the true figure. In fact, there were three distinct danger signs: the variance of the unweighted figures was considerably higher than should be expected; the average of the unweighted figures was some way from the true proportion, indicating a bias in the samples; and the application of weighting to correct for bias actually made the figures worse, not better.

Housing Tenure

96. Housing tenure is quite strongly correlated with voting behaviour. In particular, council tenants have a much higher probability of voting Labour. Table 16 shows the proportion of council tenants measured in 22 campaign polls which collected this data. The polls are rank ordered on the unweighted percentage of council tenants. (Not all the polls in the table are weighted by housing tenure - in the case of those that are not, the difference between the weighted and unweighted figures reflects the effect on the measure of tenure of weighting by other factors. It should be noted that even those polls which were weighted by tenure do not all perfectly reflect the target figure. This is an effect of rim-weighting: this technique allows

polls to be simultaneously weighted by a number of control variables, but it is not always possible to precisely meet all the targets.)

Table 16: Proportion of council tenants measured in 22 campaign polls

Pollster	Poll	Unweighted %	Weighted %
Census 1991			**19.5**
NRS 1991			**21.5**
NOP	*Mail on Sunday* 2	17.5	24.0 *
NOP	*Independent* 3	18.8	24.0 *
Gallup	Final poll	19.0	20.0
NOP	*Mail on Sunday* 4	19.7	24.0 *
Gallup	Second poll	20.0	21.0
Gallup	Third poll	20.0	20.0
NOP	*Independent* 1	20.0	24.0 *
NOP	*Mail on Sunday* 3	20.4	24.0 *
NOP	*Independent* 2	20.5	24.0 *
MORI	*Times* final poll	20.6	24.0 *
NOP	*Independent* final	20.7	22.9 *
MORI	*Times* fourth poll	21.6	23.0
ICM	PA 10,000 poll	22.0	22.0
ICM	Final poll	22.0	22.5 *
MORI	*Times* second poll	22.4	24.0
Gallup	First poll	23.0	23.0
Gallup	Fourth poll	23.0	17.0 *
Harris	ITN second poll	23.0	23.4 *
MORI	*Times* third poll	23.2	23.2
MORI	*Times* first poll	23.2	25.0
Harris	ITN final poll	25.0	25.0
Harris	ITN first poll	26.0	

* Polls weighted by tenure

97. The estimate of council tenants in Table 16 ranged from 17.5% to 26% before any weighting was applied, a variation of 8.5% on a measure that should have been static throughout the campaign.

98. Those companies which weighted by housing tenure[32] seem to have adopted 24% as the correct national target for this variable. The publication of 1991 census data (which were not available in April 1992) suggests that the correct proportion of adults in council housing during 1991 was just over 19%. When weights were applied they generally led to an overstatement of council tenants and therefore inflated slightly the Labour vote.

[32] Only NOP and MORI weighted their final poll by tenure.

99. This confusion seems to have arisen from two sources. One was the use of the percentage of households in council houses as the target rather than the percentage of individuals: this was measured at 24% by the General Household Survey and 25% by the National Readership Survey. Second, it appears that even allowing for the confusion between households and individuals, the figures used were higher than those eventually published in the Census, which found only 21.4% of households were council tenancies and only 19.5% of adults lived in these households. The NRS over the relevant period in fact found 21.5% of adults were council tenants; the results for June 1992 - May 1993 based on the Postcode Address File sample and computer assisted personal interviews shows council tenants down to 20.0% (see paragraph 107 *et seq* below). This is much closer to the 1991 census results.

100. It is worth noting that although there was considerable variation in the unweighted findings on tenure, with one exception the effect of weighting was to worsen (or leave unaltered) rather than to improve the accuracy. In this respect the quota methodology appears to have performed better than the weighting. This was the case even for those companies who were not weighting by tenure, which indicates that the cause of the unrepresentativeness of the samples involved more than just the fact of using one incorrect weight.

101. The exception, the second poll which Harris conducted for ITN, clearly had some sample problems. The weighting applied reduced the council tenants from 23% to just 17%. This survey still produced a 6% Labour lead, one of the highest of the campaign. It would have been even higher without this corrective weighting. This was probably a rogue poll (see glossary) and Harris used the housing tenure profile of respondents from their exit poll at the previous General Election as the base for corrective weighting. This was an unusual weighting strategy.

Car ownership

102. Car availability is another variable related to voting intention which was measured by some of the pollsters. Adults living in households with two or more cars are more likely to vote Conservative. Table 17 shows the results from fifteen campaign poll estimates of the proportion of adults living in households with two or more cars. The unweighted measures are in the range 24% to 27%, but after weighting the range widens to 21% to 27%. The NOP weighting plan used the car ownership variable and the profile is based on results from their regular random omnibus survey.

Table 17: Proportion in two or more car households measured in 16 campaign polls

Pollster	Poll	Unweighted %	Weighted %
NRS 1991			**31.8**
Census 1991			**30.1**
NOP	*Mail On Sunday* 2	24.4	21.1
NOP	*Mail On Sunday* 3	26.1	21.1
NOP	*Mail On Sunday* 4	23.8	21.1
NOP	*Independent* 1	n/a	22.0
NOP	*Independent* 2	n/a	22.0
NOP	*Independent* 3	n/a	22.0
NOP	*Independent* Final	n/a	22.0
NOP	*Mail On Sunday* 1	24.4	23.2
Gallup	Second poll	25.0	24.0
Gallup	First poll	24.0	25.0
Gallup	Third poll	26.0	25.0
Gallup	Final poll	25.0	25.0
Harris	*Observer* 2	25.0	25.0
Gallup	Fourth poll	26.0	27.0
Harris	ITN Final	27.0	27.0

103. Again, it seems that the polls generally underestimate this characteristic. The 1991 NRS gives an estimate of 31.8% for adults aged 18 and over living in two-car households. As we have observed with housing tenure, if the polls had achieved more accurate estimates of car ownership, the Labour vote would have been reduced.

104. On the evidence presented so far, it seems that well-controlled quota samples which produced representative samples based on age, sex and social grade did not usually produce reliable estimates of council tenure or car ownership. Both these variables are related to voting patterns, and the measurement error was in the same direction - a tendency to overstate Labour support. Nor was the error a minor one. The difference between the worst estimates of the polls and the figures they would have achieved by weighting to a correct estimate of either of the two variables would have been equivalent to a swing of around 1%, a quarter of the total error.

Trade Union membership

105. By contrast, a third variable measured by some pollsters that also correlates with voting intention did not demonstrate bias in any particular direction although there was once again some variation in the pollsters' estimates. Table 18 shows the results from fifteen campaign polls which estimated union membership. The estimates range from 15% to 21%. There is no definitive source of validation material for this estimate but based on past surveys the pollsters' expectation was 19% union membership. This was indeed precisely the figure found by the British Election Study.

Table 18: Proportion of Trade Union members measured in 15 campaign polls

Pollster	Poll	Unweighted %	Weighted %
BES			**19.0**
Harris	ITN Final poll	15.0	15.0
Harris	ITN First poll	16.0	16.0
Gallup	Fourth poll	17.0	17.0
Harris	ITN Second poll	17.0	18.0
MORI	*Times* First poll	17.9	17.9
Gallup	First poll	18.0	18.0
Gallup	Final poll	18.0	18.0
Gallup	Second poll	19.0	19.0
Gallup	Third poll	19.0	19.0
MORI	*Times* Final poll	19.4	19.5
MORI	*S.Times* Baseline	19.6	19.2
MORI	*Times* Second poll	19.7	19.5
MORI	*Times* Fourth poll	19.8	19.4
MORI	*First Tuesday* poll	21.2	20.8
MORI	*Times* Third poll	21.2	21.1

106. In this respect our findings are more encouraging: most of the polls' estimates are close to the expected figure. Trade union membership is, presumably, better correlated with the variables used for quotas (not least working status) than are housing tenure, number of cars or, most importantly, voting behaviour. This illustrates the importance of having effective variables as the basis for quotas (or weighting) if quota sampling is to be successful.

Data sources for weighting and quotas

107. Of course, the success of the quota methodology also depends crucially on the availability of accurate information on the distribution of the social characteristics on which quotas are set. All of the opinion polls relied in whole or in part upon the results of the National Readership Survey as their source for this information[33], although measuring the social profile of the country is not one of the explicit objectives of the survey. This survey uses random rather than quota sampling and is undertaken on a continuous basis by Research Services Ltd[34]. At the time of the election, the most recent information available from the survey was for 1990; in two years social change is unlikely to have been a substantial source of error.

[33] Some used the mid-year population estimates for 1990 published by OPCS as their source for the distribution of age and sex, while NOP also used their own random omnibus survey. The NRS was itself weighted to the OPCS mid-year estimates.

[34] For further details see National Readership Survey: Tables relating to January-December 1992 (London: NRS, 1993).

Contrary to some press reports after the election, none of the pollsters relied upon the results of the 1981 Census which was of course substantially out of date by the time of the 1992 election[35].

108. The recent publication of results from the 1991 Census provides an opportunity to assess the social profile of the National Readership Survey. We have been able to make a number of comparisons between the results for all of the 1991 NRS surveys together and the results of the 1991 Census, although there are a number of differences between the methodology of the Census and the NRS, which make comparison difficult. Most importantly, the Census does not use the Social Grade classification scheme, so we are unable to check accuracy in that respect. The NRS does however also classify its respondents by the standard occupational classification used in the Census, so we can compare the distribution of occupations in the two sources using that criterion.

109. Table 19 shows a number of comparisons between the results of the NRS and the Census. In many respects the results of the NRS are similar to those of the Census. Its gender and age distribution (which is weighted to the OPCS mid-year estimates) is very close to that of the Census. But in other respects there are some important differences[36].

[35] It is true, however, that the stratification procedure used to select sampling points for the National Readership Survey still relied in part on figures from the 1981 Census.

[36] In the table, note the following:
Housing tenure: this is the percentage of persons aged 18+ living in households of each tenure type, and not the more commonly quoted but less relevant figure of the percentage of households in each tenure type.
Car Ownership: this is the percentage of persons aged 17+ living in households without a car, rather than the percentage of households without a car.
Economic Activity: Based on persons aged 16+. those working less than 8 hours are excluded from the count of part-time workers in the NRS and those working less than 10 hours in the Census. The NRS additionally recorded 0.8% of respondents as working less than 8 hours a week.
Major Occupational Groups: Based on employees and self-employed aged 16+.
Registrar-General's Social Class: Based on economically active persons. Those not classified were respondents for whom social class could not be ascertained or who were members of the armed forces.
Socio-Economic Group: Based on economically active persons. Those not classified were respondents for whom socio-economic group could not be ascertained.

Table 19: Comparing the National Readership Survey and the Census

	NRS 1991 %	Census 1991 %
Gender		
Male	48.1	47.7
Female	51.9	52.3
Age		
18-24	13.4	13.2
25-44	37.9	38.1
45-64	28.0	28.7
65+	20.6	20.1
Housing Tenure		
Council houses	21.5	19.5
Owner occupied houses	69.2	69.9
Car Ownership		
No car	24.3	25.9
One car	44.8	43.8
Two cars	23.7	22.7
Three or more cars	7.6	6.6
Economic Activity		
Work full-time	44.5	45.2
Work part-time	10.3	9.8
Unemployed	5.0	5.7
Retired	14.7	19.0
Students	4.7	3.8
Other	20.0	16.2
Major Occupational Groups		
Managers & Proprietors	11.8	15.9
Professionals	9.0	8.7
Associate Professionals	8.1	8.7
Clerical and Secretarial	17.1	16.1
Skilled trades	17.5	14.5
Protective & Personal Service	9.8	9.1
Sales	7.0	7.2
Machine Operators	10.3	10.3
Agriculture & Other Elementary	9.3	8.6
Registrar General's Social Class		
I Professional etc	3.1	4.7
II Managerial and technical	21.6	27.3
IIINM Skilled non-manual	22.4	22.8
IIIM Skilled Manual	24.5	21.3
IV Partly skilled	16.4	15.9
V Unskilled	6.5	6.0
Unclassified	4.7	2.0
Socio-Economic Group		
Employers and Managers	12.4	14.7
Professionals	3.0	4.5
Junior non-manual	32.1	32.6
Skilled manual	25.0	21.2
Semi-skilled manual	16.1	15.8
Unskilled manual	6.0	5.5
Unclassified	4.7	3.4

For notes to the table see footnote 36

110. The most important is in respect of occupation. On all three measures in the table - major occupational groups, Registrar General's Social Class, and Socio-Economic Group - the NRS has fewer persons than the Census in professional and/or managerial occupations and more in manual (especially skilled manual) ones. Given that persons in professional and managerial occupations are more likely to vote Conservative and those in manual occupations to vote Labour this suggests that quotas based on the NRS may result in too many Labour voters being interviewed and too few Conservative voters.

111. In addition, the NRS also seems to have found slightly more council tenants[37], but this was at the expense of those who were privately renting rather than owner occupiers. On the other hand, the NRS does not systematically contain more persons in social groups likely to vote Labour, for it reports a higher number of persons living in households without cars than does the Census.

112. The NRS also seems to contain fewer retired and unemployed persons. These differences in particular may well arise because of differences in the way in which the Census and the NRS are administered. In any event, all of the polls simply set quotas for the proportion who were working and not working (where the two sources are largely in agreement) and not the different categories of those not working.

113. These findings suggest that there is a need to examine whether the opinion polls should continue to use the NRS as the basis for setting quotas. Although some or even all of the differences may reflect differences in methodology rather than any sampling bias in the NRS, it is evident that the NRS may not provide a wholly accurate measure of the social profile of the country. Greater use might be made in future of government surveys which do aim to measure the country's social profile such as the General Household Survey rather than relying upon a survey for which that objective is secondary.

114. Since 1991 the NRS has made some important changes to its methodology. In particular, since July 1992 interviewing has used the CAPI technique[38], while since January 1992 the postcode address file has been used as a sampling frame (see glossary) rather than the electoral register[39]. And indeed as Table 20 shows, there is some evidence to suggest that the social profile of the NRS has become more similar to that of the 1991 Census[40]. However, some of the differences between 1993 and 1991 will reflect real social change while the NRS still has fewer persons classified as Managers and more in Skilled Trades than were

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[37] Though note that in January-June 1992 the proportion of council tenants in the NRS was just 19.8%.

[38] CAPI - computer assisted personal interviewing - uses a questionnaire administered by a portable computer with responses entered directly by the interviewer rather than the traditional paper-based technique.

[39] The survey was also able to incorporate information from the 1991 Census in its stratification procedures from January 1993.

[40] Note, however, that the impact of any change may have been delayed because of weights applied to smooth differences in the social profile of the sample between one month and the next.

recorded by the Census. Our conclusion that the NRS may not be the best source of information for establishing targets for quotas and weighting would still seem to be valid.

Table 20: Recent trends in the National Readership Survey

	1991 %	1992 %	1993 %
Housing Tenure			
Council houses	21.4	20.0	18.8
Owner occupied houses	69.5	70.5	70.7
Car Ownership			
No car	23.4	22.6	24.0
Economic Activity			
Work full-time	43.9	41.7	40.6
Work part-time	10.9	11.3	11.2
Unemployed	4.9	6.3	6.2
Retired	14.5	14.1	17.3
Others (inc. Students)	25.8	26.7	24.6
Major Occupational Groups			
Managers & Proprietors	11.9	12.3	13.5
Professionals	9.0	9.4	9.5
Associate Professionals	8.1	8.3	9.2
Clerical and Secretarial	17.1	16.5	15.9
Skilled trades	17.5	17.9	16.6
Protective & Personal Service	9.8	10.0	10.0
Sales	7.0	6.7	6.7
Machine Operators	10.4	10.2	10.4
Agriculture & Other Elementary	9.3	9.0	8.4

Source: Research Services Ltd

115. The Social Grade profile of the NRS in particular has also been subject to a further methodological change since the general election. Until July 1992 each respondent was assigned a Social Grade on the basis of the occupation of the Head of Household, but since July 1992 the assignment has been on the basis of the Chief Income Earner. The Head of Household is the person who is responsible for the property in which a respondent lives (or, if that person is a married woman, her husband) whereas the Chief Income Earner is simply the person in the household with the highest income. The reported distribution of respondents by Social Grade in the NRS was not immediately affected by the change, however, because a weighting procedure is used to smooth differences in the Social Grade profile of each month's sample. But the impact of this smoothing procedure had disappeared by July 1993, and as the following table shows, even after allowing for possible real changes in the distribution of the Social Grade, the change does seem to have increased somewhat the proportion who are classified as being in the AB and C1 categories and reduced the number in the C2 and DE categories.

116. This change does not, however, have any relevance to an assessment of the utility of the NRS as a source for targets for quotas and weighting. The change of methodology changes how those who are interviewed are classified, not who is interviewed. But it clear that if opinion polls continue to use the NRS as a source of information they should now also classify their respondents by the Social Grade of the Chief Income Earner.

Table 21: Recent trends in the National Readership Survey

	1991	1992		1993	
		Jan-Jun	Jul-Dec	Jan-Jun	Jul-Dec
	%	%	%	%	%
AB	17.9	18.0	18.1	19.3	20.4
C1	24.1	24.2	25.2	26.2	27.0
C2	27.1	27.6	26.9	25.0	23.9
DE	30.8	30.2	29.7	29.4	28.8

Source: Research Services Ltd

Do quota samples suffer an availability bias?

117. While the quotas used may have been accurate this is not the same thing as saying that they were adequate. As we have already seen (paragraph 95 *et seq*), the election surveys did not always achieve a representative sample of the population in terms of a number of key social characteristics. A further investigation into the adequacy of the quota methodology in the 1992 election has been undertaken by Jowell et al[41]. They suggest that even when opinion polls have an accurate social profile of the population in terms of the quotas which are used, they are still more likely to interview Labour supporters. They argue that this is because quota polls are normally undertaken over a short period of time with no attempt made to recontact those who are initially unavailable for interview, and that Labour supporters are more readily available for interview than Conservative supporters.

118. If the quota rules allow interviewers discretion to pick unwittingly unrepresentative samples within each quota cell, which groups are most likely to be excluded?

119. In terms of political leanings, the potential bias can work both ways. Certain groups are less likely than others to be found by interviewers filling a quota; this is especially true of in-street interviewing. The elderly (especially those who are living in institutions or are housebound), and members of the armed forces are possible examples. It has also been claimed that the very rich, living in large houses at the end of long drives, and people in high rise tower blocks in inner city areas with restrictive entry systems would

[41] R.Jowell, B.Hedges, P.Lynn, G.Farrant, and A. Heath, 'The 1992 British Election: The Failure of the Polls', Public Opinion Quarterly, LVII, 238-63.

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Pollster	Poll	Unweighted %	Weighted %
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Harris	ITN First poll	16.0	16.0
Gallup	Fourth poll	17.0	17.0
Harris	ITN Second poll	17.0	18.0
MORI	*Times* First poll	17.9	17.9
Gallup	First poll	18.0	18.0
Gallup	Final poll	18.0	18.0
Gallup	Second poll	19.0	19.0
Gallup	Third poll	19.0	19.0
MORI	*Times* Final poll	19.4	19.5
MORI	*S.Times* Baseline	19.6	19.2
MORI	*Times* Second poll	19.7	19.5
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Economic Activity: Based on persons aged 16+. those working less than 8 hours are excluded from the count of part-time workers in the NRS and those working less than 10 hours in the Census. The NRS additionally recorded 0.8% of respondents as working less than 8 hours a week.
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Table 19: Comparing the National Readership Survey and the Census

	NRS 1991 %	Census 1991 %
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45-64	28.0	28.7
65+	20.6	20.1
Housing Tenure		
Council houses	21.5	19.5
Owner occupied houses	69.2	69.9
Car Ownership		
No car	24.3	25.9
One car	44.8	43.8
Two cars	23.7	22.7
Three or more cars	7.6	6.6
Economic Activity		
Work full-time	44.5	45.2
Work part-time	10.3	9.8
Unemployed	5.0	5.7
Retired	14.7	19.0
Students	4.7	3.8
Other	20.0	16.2
Major Occupational Groups		
Managers & Proprietors	11.8	15.9
Professionals	9.0	8.7
Associate Professionals	8.1	8.7
Clerical and Secretarial	17.1	16.1
Skilled trades	17.5	14.5
Protective & Personal Service	9.8	9.1
Sales	7.0	7.2
Machine Operators	10.3	10.3
Agriculture & Other Elementary	9.3	8.6
Registrar General's Social Class		
I Professional etc	3.1	4.7
II Managerial and technical	21.6	27.3
IIINM Skilled non-manual	22.4	22.8
IIIM Skilled Manual	24.5	21.3
IV Partly skilled	16.4	15.9
V Unskilled	6.5	6.0
Unclassified	4.7	2.0
Socio-Economic Group		
Employers and Managers	12.4	14.7
Professionals	3.0	4.5
Junior non-manual	32.1	32.6
Skilled manual	25.0	21.2
Semi-skilled manual	16.1	15.8
Unskilled manual	6.0	5.5
Unclassified	4.7	3.4

For notes to the table see footnote 36

110. The most important is in respect of occupation. On all three measures in the table - major occupational groups, Registrar General's Social Class, and Socio-Economic Group - the NRS has fewer persons than the Census in professional and/or managerial occupations and more in manual (especially skilled manual) ones. Given that persons in professional and managerial occupations are more likely to vote Conservative and those in manual occupations to vote Labour this suggests that quotas based on the NRS may result in too many Labour voters being interviewed and too few Conservative voters.

111. In addition, the NRS also seems to have found slightly more council tenants[37], but this was at the expense of those who were privately renting rather than owner occupiers. On the other hand, the NRS does not systematically contain more persons in social groups likely to vote Labour, for it reports a higher number of persons living in households without cars than does the Census.

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113. These findings suggest that there is a need to examine whether the opinion polls should continue to use the NRS as the basis for setting quotas. Although some or even all of the differences may reflect differences in methodology rather than any sampling bias in the NRS, it is evident that the NRS may not provide a wholly accurate measure of the social profile of the country. Greater use might be made in future of government surveys which do aim to measure the country's social profile such as the General Household Survey rather than relying upon a survey for which that objective is secondary.

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recorded by the Census. Our conclusion that the NRS may not be the best source of information for establishing targets for quotas and weighting would still seem to be valid.

Table 20: Recent trends in the National Readership Survey

	1991 %	1992 %	1993 %
Housing Tenure			
Council houses	21.4	20.0	18.8
Owner occupied houses	69.5	70.5	70.7
Car Ownership			
No car	23.4	22.6	24.0
Economic Activity			
Work full-time	43.9	41.7	40.6
Work part-time	10.9	11.3	11.2
Unemployed	4.9	6.3	6.2
Retired	14.5	14.1	17.3
Others (inc. Students)	25.8	26.7	24.6
Major Occupational Groups			
Managers & Proprietors	11.9	12.3	13.5
Professionals	9.0	9.4	9.5
Associate Professionals	8.1	8.3	9.2
Clerical and Secretarial	17.1	16.5	15.9
Skilled trades	17.5	17.9	16.6
Protective & Personal Service	9.8	10.0	10.0
Sales	7.0	6.7	6.7
Machine Operators	10.4	10.2	10.4
Agriculture & Other Elementary	9.3	9.0	8.4

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115. The Social Grade profile of the NRS in particular has also been subject to a further methodological change since the general election. Until July 1992 each respondent was assigned a Social Grade on the basis of the occupation of the Head of Household, but since July 1992 the assignment has been on the basis of the Chief Income Earner. The Head of Household is the person who is responsible for the property in which a respondent lives (or, if that person is a married woman, her husband) whereas the Chief Income Earner is simply the person in the household with the highest income. The reported distribution of respondents by Social Grade in the NRS was not immediately affected by the change, however, because a weighting procedure is used to smooth differences in the Social Grade profile of each month's sample. But the impact of this smoothing procedure had disappeared by July 1993, and as the following table shows, even after allowing for possible real changes in the distribution of the Social Grade, the change does seem to have increased somewhat the proportion who are classified as being in the AB and C1 categories and reduced the number in the C2 and DE categories.

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Table 21: Recent trends in the National Readership Survey

	1991	1992		1993	
		Jan-Jun	Jul-Dec	Jan-Jun	Jul-Dec
	%	%	%	%	%
AB	17.9	18.0	18.1	19.3	20.4
C1	24.1	24.2	25.2	26.2	27.0
C2	27.1	27.6	26.9	25.0	23.9
DE	30.8	30.2	29.7	29.4	28.8

Source: Research Services Ltd

Do quota samples suffer an availability bias?

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[41] R.Jowell, B.Hedges, P.Lynn, G.Farrant, and A. Heath, 'The 1992 British Election: The Failure of the Polls', Public Opinion Quarterly, LVII, 238-63.

tend to be left out of in-home interviews. Such deficiencies in the sample may be mitigated by the fact some of these excluded groups are precisely those who are least likely to vote.

120. A change to random methods could solve some of these problems but possibly would increase others. If the difficulty is one of access, it may apply equally whether the interviewer is trying to approach a specific individual or only a generic group. Elderly people living in institutions are not easy to contact as wardens may be concerned about allowing interviews to be conducted; when the interviewers give a pre-selected name this may add to the warden's unease. Equally, interviewers will find it difficult to gain access to a high rise tower block, whether or not an individual has previously been identified by reference to the electoral register or postal address file. Young people are also less likely to be at home and available to be interviewed in pre-selected name or address surveys while more likely to take part in quota surveys. These problems are not exclusive to quota sampling.

121. Jowell et al base their claims on an analysis of the 1992 British Election Study. This survey was undertaken using random sampling methods, but they suggest that those who were successfully interviewed on that survey the first time that they were contacted by an interviewer are similar to the kind of people who are interviewed in a typical quota poll. They therefore attempt to measure the political consequences of 'availability bias' by comparing the reported vote of those who were successfully interviewed on first contact with the reported vote of the whole sample.

122. Those who were successfully interviewed on first contact were not of course a representative sample of the population. Jowell et al therefore weighted these respondents so that their profile matched the profile of a quota sample undertaken during the election campaign. This, they argue, enabled them to test the effectiveness of quota controls in producing a representative sample. They found that even after weighting the lead of the Conservatives over Labour was 6% lower amongst those who were interviewed on first contact compared with all those who were eventually interviewed.

123. There are however some difficulties with this analysis. In weighting their data they used a social grade profile used by only one of the companies, which was different to that used by all the other companies. Consequently they required that 61% of their respondents be in the C2DE social grades while all but one of the polling companies set a quota of 59%. If Jowell et al had weighted their first contact data to this more typical quota their estimate of the 'availability bias' would have fallen to 5%.

124. It is also important to be aware that in calculating 'availability bias', Jowell et al compare the reported vote of their first contacts with those of all those who were eventually interviewed. However, the reported vote of all respondents to the British Election Study (in sharp contrast to the opinion polls) produced a lead for the Conservatives which was 3% higher than actually occurred at the election. If instead we were

to compare the reported vote of first contacts (weighted to a typical social grade profile) with the actual election result, the estimate of 'availability bias' falls to just 2%[42].

125. Further, it is unclear how far first contact respondents to a random survey conducted in home for an hour mirrors those who are interviewed in a quota survey. The majority of polls undertook some or all of the polling in the street rather than at home, so those who are easily available for interview in such polls may well be different than was the case for the election study. Equally, faced with the prospect of an hour-long interview, respondents may well have been more likely to ask an interviewer to return at a more convenient time than would be the case for a brief five-to-ten-minute interview typical of an election poll. So the availability bias may again be different. Finally, it should be noted that many opinion polls attempt to counteract possible 'availability bias' by requiring interviewers to conduct a proportion of their interviews in the evening. Only 14% of the BES first contact interviews were undertaken in the evening[43].

126. So the arguments set out by Jowell et al may well exaggerate the extent of 'availability bias' under the quota control procedures used at the last election. Nevertheless, it is important to ensure that the quota controls are adequate and as we have already seen above (paragraph 95 *et seq*) this was not clearly the case in 1992. More effective methods of quota sampling are clearly required.

Conclusion

127. Our investigation has suggested that present practice did not achieve accurately representative samples in 1992. However, this seems to have been because the selection of quota variables was inadequate and the data on which quotas were based were inaccurate, and not necessarily because quota methodology, properly operated, is incapable of delivering adequate samples for polling. However, if confidence in the quota methodology is to be maintained, consideration must be given to three separate factors.

128. It is essential that the polling companies establish reliable and regular sources of quota control and weighting variables. The fact that the necessary figures were not available from the 1991 Census at the time of the 1992 election was a source of uncertainty. The use of out of date profiles, incorrect data and unreliable data sources should be avoided in future. Weighting the existing quota variables correctly, together with use of other variables already collected, ought to reduce or even solve the problem. While there is no room for complacency about the failure of the quota method to deliver better unweighted

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[42] It is debatable which procedure is correct. Jowell et al's method assumes that the pro-Tory bias in the BES sample occurs equally amongst those who were interviewed at first contact and those who were interviewed subsequently, and that this bias is not in any way corrected by the weighting applied to first contact respondents. However it is possible that some or all of the source of any pro-Tory bias amongst first contact respondents may have been corrected by the weighting procedure.

[43] The potential importance of evening interviewing is shown in the BES, where a higher proportion of Conservative respondents were interviewed in the evening than of Labour respondents.

measures of key variables, the fact that the deviation can be seen so clearly indicates that adequate weighting would be at least a partial answer.

129. We recommend that the use of alternative quota controls or weighting, using variables more closely related to voting behaviour, should be considered. If it proves impractical to adopt a completely new set of quota controls for sampling, it should certainly be possible for the pollsters who do not already do so to collect and weight for variables such as housing tenure and car ownership. Correct weighting on these kinds of variables would probably improve the overall accuracy of the poll measurements. The first stage is plainly experimental work to establish which variables are best correlated with voting behaviour, and we would certainly recommend that this be explored. We discuss some alternative variables, the use of which has already been canvassed, in the next section.

6.2 Should they have weighted by different variables?

Different variables from the NRS

130. Many of the potential pitfalls of unlucky quota sampling or inadequate sampling frames can in theory be mitigated or even circumvented by the use of a suitable and effective weighting strategy. As we have seen, only a small number of variables were used for weighting in 1992, all of them demographic variables which are regularly collected on the National Readership Survey.

131. However, demographic weighting as used in 1992 produced relatively little improvement in the polls' performance. Newspaper readership is one alternative that has been suggested as a variable by which polls could be weighted. However, this is impractical for several reasons, most importantly that patterns of newspaper readership change at election times. (Indeed, they almost certainly change precisely because it is election time). Consequently pollsters would have no reliable 'target figure' for each newspaper's readership to use as the basis for their weighting.

132. We suggest that pollsters look for other variables more closely associated with voting intention by which polls can be controlled either through their inclusion in the quota and/or by using them as weighting variables. Housing tenure is one such variable which ought to be considered for this purpose, but as we have seen in 1992 not all the companies used it in their weighting and those that did relied upon inaccurate data. The NRS measures many other variables, some of which may prove suitable for weighting the polls - we suggest that the companies should investigate these possibilities.

Using attitude variables

133. Attitudinal variables might also be used in the weighting procedure. Jowell et al suggest four prime candidates[44] :

(1) Party identification. ('Generally speaking, do you think of yourself as Conservative, Labour, Liberal Democrat, or what?')

(2) Past voting. ('Which party did you vote for at the last General Election?')

(3) Strength of support for, or opposition to, all parties.

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[44] R. Jowell, B. Hedges, P. Lynn, G. Farrant, and A. Heath, 'The 1992 British Election: The Failure of the Polls', Public Opinion Quarterly, LVII, 238-63.

(4) A scale question including several statements measuring right and left-wing values. Respondents, for example, intending to vote Labour but scoring high on right-wing values could then be downweighted.

134. Of these, past voting (2) is discussed in the next section. The other three are attitude variables which would be expected to move over time with party support. The obvious difficulty for the pollsters would be in deciding whether changes in both attitudes and party support were as a result of a real change in opinion or a deficiency in the samples. Certainly MORI's test (see Appendix 6B) seems to confirm that party identification is a less fragile measure than voting intention. On the other hand, unless some evidence can be adduced that it is more strongly related to present voting behaviour than is voting intention, there does not appear to be any way in which it could be usefully employed to modify or weight poll responses, there being no independent yardstick of party identification strengths with which it could be compared.

135. Such weighting could only be contemplated by reference to a large sample of the population, interviewed recently and by the highest sampling standards. At the moment no such data source exists.

Past voting

136. One variable that is likely to be closely correlated with current voting intentions is the recall of past voting behaviour[45]. Pollsters could simply ask respondents how they voted in the last election. The results can then be weighted by matching the declared past voting of respondents back to the actual percentages of votes cast for each party at the last election. This is a technique adopted by polling companies in Germany and France with considerable success. This international evidence is discussed in the following section.

137. It is obvious that if *accurate* measures of respondents' past voting could be obtained, it would be ridiculous not to use it in weighting. Past voting was used regularly by some British pollsters up to the mid 1960s. Its fall from favour coincided with a growth in support for the Liberal Party. In particular, it was noted that the recall of voting Liberal generally fell to low levels compared with their actual past performance. It seemed that people just forgot (or perhaps preferred to forget) that they had voted Liberal. Up to and including the election in 1992, weighting by past voting therefore made such a prediction worse, not better. For example, when the final ICM poll before the 1992 election had an average error of 2.5%; weighting by past voting would have resulted in an average error of 3%.

[45] See, for example, D. Butler and D. Stokes, Political Change in Britain (2nd edition, London: Macmillan, 1974)

138. Evidence that voters tend to have a selective memory about the way they voted in the past is strong. The 1992 BES panel survey found that people were unlikely to remember either having voted for the Alliance or having abstained. It was found that as many as 21% gave a different answer when asked in 1992 what they had done in 1987 compared to what they said shortly after the 1987 election. We have examined this effect by looking at the relationship between reported vote and recalled vote according to how people voted in 1992. Of a total of 341 voters whose vote was different from their recall, only 53% aligned their recall vote with their current voting intention. (For further details, see Appendix 6C.) The tendency for declared past voting to move in sympathy with voting intentions was found to be particularly strong amongst Conservative and Labour voters, less so among Liberal Democrats. One consequence is therefore that weighting by declared past vote will tend to overestimate the level of third party support.

139. This phenomenon may not reflect a tendency either for people to lie or to wish they had voted differently. It may be due to alternative question ordering, an effect widely known to exist in all forms of market research. People may be conditioned by having already reported their current party loyalties to misremember the past and report voting behaviour that is not strictly accurate. MORI's experiment in 1993 (see Appendix 6B) seemed to show, however, that this is not currently the case - question order affected voting intention significantly, but had no effect on vote recall. This may reflect a genuine change in the accuracy of vote recall since 1992. As Table 22 shows, since the 1992 election the proportion of people saying they voted Liberal Democrat in 1992 has been much closer to the true proportion than in the past. It is possible that the Liberal Democrats have reached a position at present where their natural supporters are likely to vote for them on more than one occasion and are therefore more likely to remember doing so.

Table 22: Vote recall measures between the 1992 election and September 1993

		Con	Lab	LDem	Oth
1992 G.E.	%	**43**	**35**	**18**	**4**
Gallup	%	39 ±2	41 ± 2	16 ± 2	4 ± 1
ICM	%	41 ±3	39 ± 3	16 ± 2	4 ± 2
MORI	%	39 ±2	40 ± 2	18 ± 3	3 ± 1

140. Recent polls from the three companies identified above do not suggest even a modest decline in recalled Liberal Democrat vote, even within the narrow bands identified. In October 1993 ICM registered declared past voting for the Liberal Democrats at 17%, MORI recorded 16% and Gallup 17%, compared to the actual 18.3%.

141. Polls conducted by ICM and Gallup about eighteen months into previous Parliaments have shown the proportion of declared past Tory voters within 2% or 3% of their actual share of the vote while

Labour have historically enjoyed higher declared past vote figures (up between 4% and 8½%) than they actually achieved. (Overclaiming votes for left-wing parties and underclaiming past votes for right-wing parties are a feature of the polls in both Germany and France as the next section shows.) The tendency for Liberal Democrats to forget seems to have declined recently.

Table 23: Average error in vote recall surveys

		Con	Lab	Lib	Oth
Post 1983	%	0	+8.5	-7.5	-1
Post 1987	%	+2	+4	-7	+1
Post 1992	%	-3	+6	-1	-2

Source: Gallup

142. The effect of weighting by past voting in polls conducted since the last election would be to reduce, by about half, the leads Labour has enjoyed in the polls over the Conservatives, but without boosting the Liberal Democrat vote, so long as the Liberal Democratic vote recall is accurate.

143. What this would do to the accuracy of the polls depends entirely upon why they are finding inaccurate recall figures. One possibility is that interviewers simply interview too many left-wing party supporters and too few right-wing party supporters. (It is interesting to note that the recall of voting at the 1992 election in polls conducted since the elections, like the polls conducted immediately before the election, have found too many Labour supporters and too few Conservatives.) In this case, weighting by past voting could be the answer. Alternatively, there could be some people in the sample, who having voted for the Conservatives, subsequently wished they had not done so and denied it when asked to recall their vote. In this case weighting simply assumes they will do the same again, profess support for other parties but vote for the Conservatives when the next election comes. This is a potentially perilous assumption.

144. Previous experience with past voting may discourage many pollsters from using it in future as a weighting variable. However, some of the pollsters intend to continue to experiment with the use of past voting weighting to see if it can be used with more confidence in the future to compensate for unrepresentative samples and misleading answers. (A separate, more limited, use of past voting might be in compensating for the 'don't knows'. This is discussed further below - paragraph 323 *et seq*).

International evidence

145. There is, of course, almost limitless information from around the world on different weighting procedures employed to overcome the deficiencies of quota, random or telephone interviewing.

146. Past voting is used widely, if not universally, by polling organisations in both France and Germany, in some cases with startling results. Indeed, pollsters in France insist that it would be wrong to publish a poll without weighting by past voting ('political weighting'). For example, there is a clear unweighted left-wing bias in polls carried out during the French legislative assembly election in 1993 and regional elections in March 1992. The poll for the legislative elections represents a 5% overestimation of the left vote and there is a 13% overestimation in the regional poll.

Table 24: 'Political Weighting' of French Opinion Polls, 1992-3

French Assembly elections, 1993

	Unweighted %	Weighted %	Difference %	Result %
Left-wing Parties	52	46.5	-5.5	44.8
Right-wing Parties (inc. FN)	48	52.5	+4.5	55.2
National Front (FN)	6	11	+5	12.5

French regional elections, March 1992

	Unweighted %	Weighted %	Difference %	Result %
Left-wing Parties	58	44.3	-13.7	41.2
Right-wing Parties (inc. FN)	39	51.4	+12.4	55.7
National Front (FN)	5	14.2	+9.2	13.7

Source: IPSOS

147. Part of the reason why polling companies have used past voting in France is to overcome this leftward bias and address the problem of undeclared support for minority and extremist parties. This is particularly the case with the National Front vote, which is also seen in the figures above. Nearly all the underestimation of the right-wing vote in the legislative assembly elections is due to the undeclared support for the National Front. However, while the National Front vote is also underestimated by 9% in the regional elections, nevertheless there is also an underestimation of other right-of-centre parties. National Front voters have tended to be people who have previously refused to participate in polls or are people who interviewers have tended not to find. However, pollsters claim that this is only part of the reason for the use of past voting weighting, and point to the regional elections as proof that it is necessary to remove a more general left-wing bias in the polls.

148. In the 1992 French referendum on the Maastricht Treaty, published polls showed that the result would be very tight and that the 'Oui's' would win by a margin of perhaps 2% (or 4% in the lead). This result was not obtained by the application (as used in Britain) of demographic weighting. The closeness

of the prediction was largely a result of weighting by past voting. The example below shows the effect of various weighting strategies on the prediction of a poll by CSA conducted a few days before the election.

Table 25: French Referendum on Maastricht, 1992

	% 'Yes' lead over 'No'
Unweighted	+14
Weighted (demographics only)	+14
Weighted by presidential vote	0
Weighted by regional vote	+6
Weighted by both the above	+4
Actual result	**+2**

Source: CSA

149. In Germany political weighting has the same effect of increasing the percentage of votes for right-wing parties, indicating that the same left-wing biases are at work. Table 26 below has been supplied by Forsa, and is based on over 64,000 interviews in 1993. It shows that on average, demographic weighting does not alter the percentage support for each party. Political weighting has the effect of reducing the estimate of SPD (left-of-centre) and increasing CDU/CSU (right of centre) votes.

Table 26: Weighting by past vote in Germany, 1993

	Unweighted	Demographic weighting	Demographic plus political weighting
	%	%	%
CDU/CSU	32	32	34
SPD	45	44	40
FDP	5	5	7
Green	12	15	10
Other	7	7	8

Source: Forsa

150. The evidence of declared past voting in Britain, France and Germany is therefore similar, that recall of voting for right-wing parties is too low and recall of voting for left-wing parties is too high, hence the effect of political weighting. In all three countries polling companies find too many left-wing supporters and too few right-wing supporters or, perhaps, there is a tendency for a group of people to espouse left-wing tendencies except when they actually vote. Whatever the reason, the fact that in both France and Germany predictions of election results are **only** made accurate by the application of political weighting which reduces an inherent left-wing bias is evidence which the British polling companies may find persuasive.

151. Of course, electors in both France and Germany go to the polls more often than we do in the UK, and turnout tends to be much higher for local elections or European elections than in the UK. Therefore, recall of past actions tends to be based on a more recent event, memory has less time to play tricks and voters have less time to think of what they now wish they had done rather than what they actually did.

152. Obviously, pollsters in Britain must be careful when applying any lessons learned from abroad. In France and Germany, simple demographic weighting is not found to be sufficient to guarantee the accuracy of a voting intention survey; in Britain, demographic weighting has, in the past, been sufficiently effective. The political context in France, at least, is also very different - parties have tended to be more short-lived and identification with a position on the left-right axis is stronger than that with specific parties. Nevertheless, as we have seen it is not perfect, and the effectiveness of political weighting abroad may suggest it is worthy of serious consideration in the future. But we reiterate that use of the technique as it is employed abroad would not have helped the polls in Britain in 1992.

The Spiral of Silence

153. The so-called 'spiral of silence' was described by Elisabeth Noelle-Neumann[46] and has been used by various people to explain the failure of the polls in the British elections in 1992.

154. Simply described, the spiral of silence suggests that when a group of voters sense that the climate of opinion is hostile to their chosen party they react to opinion pollsters through a tendency to silence. They either refuse to participate altogether, resulting in there being too few interviewed, or they make no statement as to their last voting decision or their future intention, or they just lie.

155. Historically, in Germany and in Britain, polls immediately following a general election have found that a larger proportion of people **claim** to have voted for the victorious party than **actually** voted for them. However, in Germany after the 1965 election Noelle-Neumann found a peculiar result, that **too few** people claimed to have voted CDU, the victorious party. This was contrary to previous experience and was, for Noelle-Neumann, a 'signal for alarm'.

156. During the late 1980s we can see the same peculiar finding emerging in the UK. Gallup figures suggest that the familiar pattern was evident in 1983, but in 1987 and more especially in 1992, the proportion **claiming** to have voted Conservative shortly after each election is **lower** than the actual percentage vote.

[46] E. Noelle-Neumann, The Spiral of Silence (Chicago: University of Chicago Press, 1984).

Table 27: Vote recall shortly after British General Elections 1983-92

	Con %	Lab %	Lib-SDP/LD %
1983 actual	43.5	28.3	26.0
Recall of vote, July 1983	46.5	26.0	25.7
1987 actual	43.3	31.5	23.1
Recall of vote, July 1987	41.4	35.1	21.4
1992 actual	42.8	35.2	18.3
Recall of vote, May 1992	38.5	39.6	17.9

Source: Gallup

157. These figures are confirmed in 1992 by cross-section and panel recall surveys, most of which found a lower proportion claiming to have voted for the Tories than actually did so. However, the BES cross-section survey found the opposite - indeed, the exaggeration of the real Tory lead was worse than in the past. It is also possible that the timing of the 1992 Gallup survey - following the local elections - may have had some effect.

158. For Noelle-Neumann in Germany, confirmation came in the answers to a number of questions regarding willingness to espouse publicly one's support for a political party. These showed that there has been a climate of opinion working against the CDU, a (moderate) right-of-centre party, which may have inclined its supporters towards silence. This finding has been replicated by Noelle-Neumann on a number of occasions.

159. The spiral of silence theory, by attempting to explain why declared past voting figures differ from reality, offers a theoretical justification for, and explanation of the effectiveness of, weighting by past voting. In Germany Noelle-Neumann weighted the raw poll figures by declared past voting because, the theory suggests, supporters of the CDU were inclined to silence. She did so with startling success. In 1972 a 15.9% lead for the SPD over the CDU was adjusted to a 3.4% lead. The actual margin of victory was 3.5%. In 1978 a 0.9% lead for the SPD was adjusted to a 5.7% lead for the CDU. The CDU actually won by 5.2%.

160. Attempts to replicate the spiral of silence findings in Britain since 1992 have been less convincing. MORI asked respondents which of a number of things they would do for the party they most support. (See Appendix 6A for details). They found Tory supporters were slightly more likely to claim they would do nothing to actively show support for the Conservatives, thus giving some support to the spiral of silence theory. However the results are not nearly as dramatic as those from Germany. Nor do they display what

Noelle-Neumann considers the most characteristic effect, greater reluctance to perform the most public or visible actions.

161. ICM found some evidence of voter shyness and fashionability in a poll conducted in December 1993 for the *Guardian*. Among those interviewed, Labour voters were the group who seemed least coy about declaring their party allegiance. Both Conservative and Liberal Democrat voters were less likely to say that they might give a friend or colleague an accurate answer.

Table 28: 'Voter shyness': ICM/*Guardian* survey, December 1993

Q. If a friend or colleague asked you how you intend to vote, would you ..... ?

	Con %	Lab %	LDem %
Tell them	**66**	**76**	**66**
Refuse to say	21	15	18
Say 'don't know'	10	8	13
Say a different party	2	0	2
Total misleading	***33***	***23***	***33***
Don't know	0	0	1

Source: ICM

162. People also felt that it was more fashionable to be a supporter of the Labour Party (36%). Less than a quarter thought it fashionable to be a Conservative and only 15% nominated the Liberal Democrats.

163. The data from MORI and ICM therefore lends some weight to the spiral of silence theory, and if those Conservative voters who are inclined to silence are also unlikely to participate in polls, the effect of the spiral may be greater than those polls have estimated.

164. In any case, there are no straightforward lessons to be learned. The techniques employed to correct for the spiral of silence, so successfully used by Noelle-Neumann in Germany, would not have worked in Britain in 1992 or in any election going back at least to the mid-seventies. Simply this is because any spiral of silence effect which might have been apparent in the declared past voting figures immediately before the 1992 election was completely swamped by the tendency of SDP/Liberal Alliance voters to forget how they voted in 1987 (as shown above, paragraph 137 *et seq*).

6.3 The Measurement of Social Class

165. One obvious area to explore in trying to improve the performance of the quota system is the effectiveness of the social classification employed; social classification is simultaneously the least reliable of the standard quota variables and the most important in the sense that is likely to be most strongly correlated with voting behaviour. As we have already seen (paragraph 87), social class was universally used as a quota control; age and gender were the only other characteristics similarly used as a quota control by all the companies. Thus any failure to measure social class accurately or adequately could well result in an unrepresentative sample.

166. All opinion polls use the class schema known as 'social grade'. This class schema has been developed by the Institute of Practitioners in Advertising and is used in nearly all market research[47]. Under this schema the coding of respondents is undertaken by interviewers in the field. The primary criterion used in determining the social grade of a respondent is occupation, normally that of the head of household (though some market research companies now use the Chief Income Earner in the household)[48]. However where information on occupation is not obtainable, 'the assessment of social grade is based on environmental factors such as the type of dwelling, the amenities in the home, the presence of domestic help and so on'[49]. Persons who are reliant on state benefits (including unemployed and retired persons whose income is not above state benefit level) are assigned to a separate class. These practices are consistent with the primary aim of the social grade scheme, which is to categorise people according to the kind of lifestyle that they lead. Lifestyle of course plays a key role in influencing patterns of consumer spending which is what much market research is designed to analyse.

167. The categories of the social grade schema are as follows:-

A	High grade professionals and managers
B	Lower grade professionals and managers
C1	Other clerical and non-manual
C2	Skilled manual workers and foremen
D	Semi- and unskilled manual workers
E	Pensioners and others on state benefit.

In practice, all of the companies combined grade A and grade B in setting their quotas and grade E was combined with grade D (to restrict the number of categories to a practicable number).

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[47] A summary of the system, plus a detailed list of occupations and their associated social grade codes, is contained in 'Occupation Groupings: A Job Dictionary', published by the Market Research Society.

[48] The Head of Household is the person who owns the accommodation occupied by the household or is responsible for the payment of rent. However, where that person is a married woman whose husband is a member of the household, the husband is the head of household. The Chief Income Earner is the person with the largest income in the household.

[49] National Readership Survey: tables relating to January to December 1992 (London: NRS, 1993)

168. But the value of using this schema in opinion polls has been questioned on two grounds. The first is that coding by interviewers in the field is less reliable than coding undertaken by experienced coders in the office. Indeed, the discretion that this procedure inevitably gives interviewers could be used by them to assign respondents to the quota category that they need to fill rather than the one to which a respondents probably ought to be assigned. The second objection is that the social grade schema is relatively poor measure of social class for use in public opinion research because it discriminates less effectively between respondents in terms of such characteristics as voting behaviour.

169. The potential unreliability of interview coding using the social grade schema has been demonstrated by O'Brien and Ford[50]. In a panel undertaken by BMRB they found that 37% to 41% of respondents appeared to change their social grade within a period of 10 to 12 months. Half of these changes were found to be due to misclassification by interviewers, while another quarter were due to inadequate interviewer probing.

170. The possibility that, whatever its merits in market research, social grade may not the best measure of social class to use in public opinion research has been demonstrated in an experiment conducted by SCPR and Nuffield College, Oxford, on an NOP random omnibus survey in June 1987[51]. The respondents to this survey were classified by interviewer coded social grade in the normal manner. But in addition respondents were also coded in the office according to the Goldthorpe class schema[52], which has become the schema most widely used in the academic study of electoral behaviour in Britain in the last ten years[53]. The full version of the schema has eleven categories, but the version most widely used in the

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[50] S. O'Brien and R. Ford, 'Can we at last say goodbye to social class?', Journal of the Market Research Society, Vol. 30 No. 3 (July 1988).

[51] As reported in A. Heath and S. Witherspoon, 'Respondent coding of social class', ESRC Survey Methods Centre Newsletter, Summer 1988.

[52] Unlike the social grade schema, the Goldthorpe class schema codes respondents solely according to the characteristics of their occupation and employment status (or in the case of retired or unemployed persons their last occupation). Rather than attempting to capture differences in lifestyle, the schema attempts to group together occupations which are similar in the 'their sources and levels of income, their degree of economic security and chances of economic advancement' and at the same time have similar levels of authority over others and autonomy in performing work-tasks. See J. Goldthorpe (with C. Llewellyn and C. Payne), Social Mobility and Class Structure in Britain (Oxford: Clarendon, 1980). There are three main differences between this schema and the social grade schema:-

i) the Goldthorpe schema does not separately identify those living on state benefit;
ii) the Goldthorpe schema separates foremen from most other skilled workers but (in the five-category version at least) does not discriminate between skilled and unskilled manual workers;
iii) the Goldthorpe schema separates out self-employed non-professional workers from employees undertaking the same occupation. Thus a self-employed carpenter is placed in the petty bourgeoisie whereas an employed one is a member of the working class.

[53] See for example, A. Heath, R. Jowell and J. Curtice, How Britain Votes (Oxford: Pergamon, 1985); D. Robertson, Class and the British Electorate (Oxford: Basil Blackwell, 1984).

study of electoral behaviour is a collapsed version with just five categories[54]. Because each respondent was coded according to both schema it is possible to compare the discriminatory power of the two schema directly. The Goldthorpe schema proved to have the greater discriminatory power[55].

171. There is one major drawback to use of an office-coded Goldthorpe schema - time[56]. No opinion poll can realistically hope to administer such a procedure given the very severe time constraints under which it operates during a general election. In any case, if the information could only be coded in the office, it would be useless for setting quotas.

172. However, a number of experiments have been conducted with a quicker version of the Goldthorpe procedure, based on three self-assignment questions, which an interviewer could use in the field. (See Appendix 8.) Speed is of course of no use if it is bought at the expense of unreliability. However a number of experiments undertaken using SCPR's Social Attitudes survey and other surveys have suggested that self-assigned Goldthorpe class data has a reasonable level of reliability[57]. The 80% reliability found in this experiment is only modestly lower than the average level of reliability of 87% found by OPCS in the assignment of Registrar General's Social Class (which is a six-fold schema) in a number of experiments

[54] The five categories are:-
Salariat (or service class): Professionals, managers and administrators
Routine Non-manual: Clerks, secretaries and sales workers without supervisory responsibility
Petty bourgeois: Non-professional small employers and self-employed
Foremen and technicians: Supervisors of manual workers
Working class: Skilled, semi-skilled and unskilled manual workers, personal service and agricultural workers without supervisory responsibility.

The proportion of respondents to the 1992 British Election Study in each category was Salariat, 28%; Junior Non-manual, 24%; Petty bourgeois, 7%; Foremen, 5%; and Working Class, 36%. These figures are based on respondents' own (last) occupation. Figures based on the head of household or chief income earner would be a little different. See also A. Heath, R. Jowell and J. Curtice, How Britain Votes (Oxford: Pergamon, 1985), Chapter 3.

[55] An Index of Dissimilarity was calculated for the two schema which measured how much the level of Conservative and Labour voting varied between each class. (The index is simply the sum of the absolute difference between the percentage voting Conservative and the percentage voting Labour in each category, divided by two.) For the interviewer-coded social grade schema the index of dissimilarity was 29; but in the case of the office-coded Goldthorpe schema the index was 35. Evidence from the British Election Studies shows that the discriminatory power of the Goldthorpe class schema is also greater than that of Registrar General's Social Class and Socio-Economic Group.

[56] It typically takes 4-5 minutes of questionnaire time to administer the necessary questions in the field, and two minutes per questionnaire to code the occupations in the office. (The Goldthorpe class schema in fact uses exactly the same information used by the Office of Population Censuses and Surveys to derive the two social class schema used in government surveys, viz. Registrar General's Social Class and Socio-Economic Group. So the same difficulty also applies to any attempt to implement these schema.)

[57] In the 1987 British Social Attitudes study 1,200 respondents were asked both the Goldthorpe self-assigned class questions and the questions needed for an office-coding. As many as 80% of respondents were assigned to the same class by the two procedures. The errors were mostly found amongst the foremen and technicians (44% agreement) and amongst routine non-manual workers (72%). Many of the 'errors' occurred because of differences in the supervisory status assigned to the respondent, an area where, even amongst experienced coders, reliability is at its lowest. Further, this marginal loss of reliability does not appear to have a serious effect on the discriminatory power of the class schema. This can be ascertained from the June 1987 NOP random omnibus survey which classified respondents by interviewer-coded social grade and office-coded Goldthorpe class and also administered the Goldthorpe self-assigned class questions. The Index of Dissimilarity for self-assigned class was at 37, actually slightly higher than that for office-coded Goldthorpe class.

they conducted into office-based coding and would seem to be higher than the reliability of interviewer-coded social grade implied by the findings of O'Brien and Ford reported above.

173. We are not in a position on the basis of the limited experimental evidence available so far to recommend that opinion poll companies should switch to using self-assigned Goldthorpe class (or any other self-assignment procedure). But the evidence which has been made available to us does suggest that further investigation into the merits of a change is justified. The fact that there is a greater association between self-assigned Goldthorpe class and vote than between social grade and vote suggests that the use of Goldthorpe class in setting quotas could be a more powerful tool than social grade in ensuring the political representativeness of opinion polls. (There would, of course, be a need to establish reliable baseline data were such a change to be implemented.)

6.4 Refusal to take part

174. Even assuming that the performance of the quota method could be improved by using different variables, the sample could still be unrepresentative if those interviewed are likely to be systematically unlike other (uninterviewed) members of their quota cell.

175. This can arise in two ways:

a. Certain groups may be inherently less likely to be approached by interviewers. This we have already discussed above. (Paragraph 118 *et seq*).

b. Of those who are approached by interviewers, a proportion are, inevitably, unwilling to be interviewed. If this group of refusers are systematically different from those who co-operate, the sample will of course be unrepresentative as a result.

Refusals in the campaign polls

176. Not everybody that an interviewer approaches will be prepared to reveal their voting intentions. Some will refuse to co-operate altogether; others will agree to take part in the survey, but will refuse to answer some of the questions ('item refusal'). Selection bias arising out of the refusal to participate of a large proportion of those approached by interviewers is obviously one potential cause of the error in the polls in 1992. If refusers are different, as a group, from those whom pollsters get to participate in an interview, the results of any survey will not reflect the views of the whole population. (The closely related problem of refusal to answer the voting intention question by those who do participate in the poll is also a cause of error in the poll results. But the potential solutions to this problem, either by persuading respondents

to answer or to compensate accurately for their silence, are rather different. Consequently, we discuss this 'item refusal' separately, in section 8.2 below.)

Overall levels of refusal

177. The level of refusal to participate in surveys certainly seems high enough to be a cause for concern. The number of refusers in quota surveys are not usually recorded. Indeed, with in-street interviews as used by most of the companies during the election, reluctant members of the public can simply avoid the interviewer and refusal rates cannot be meaningfully measured at all. However, both ICM and Harris have researched the rate of refusal during in-home surveys.

178. ICM conducted two experiments[58]. On the first survey each interview was introduced as a political poll. Refusals amounted to 35% of all attempted contacts on quota. On the second survey the poll was introduced as a market research interview to avoid the possibility that people averse to giving party preference would refuse an interview altogether. Refusals were 37% of all attempted contacts.

179. Harris conducted one test on a survey that was introduced as being 'a survey on a number of issues including current events'. They found 38% refusals.

180. The overall refusal rates hide a large variation in the reported refusal rates by interviewer. This may reflect the differences between interviewers on what they consider to be a refusal or the style and manner of the initial approach by individual interviewers. Since each interviewer has his or her own different quota to fill, a variation in refusal rates between interviewers offers further scope for unrepresentativeness in the sample.

The profile of refusers

181. Having established that the refusal rate is high enough to be a potential problem, what clues can we find to tell us how this will affect the final sample? One clue as to whether they are the same or different is their demographic profile. If the profile of refusers were to be markedly different from the population as a whole then we might guess that they are different in other ways also. In fact, investigation of the refusers shows that their social profile differs little from that of the whole population except in age.

182. Both ICM and Harris interviewers were asked to estimate the age, sex and social class of each refuser immediately after attempting an interview. The tables below show the estimated demographic profiles of those refusing to participate, compared with the profile of the whole adult population as measured by the National Readership Survey.

[58] See Results of Tests to Improve Voting Intention Polls (report published by ICM) for details.

Table 29: Gender and class profile of refusers in quota polls

	ICM 'Political poll'	ICM 'Research survey'	Harris	All GB (NRS)
	%	%	%	%
Male	46	44	44	48
Female	53	55	55	52
AB/C1	38	39	40	43
C2/DE	62	61	59	56

Source: ICM, Harris, NRS

Table 30: Age profile of refusers in quota polls

ICM 'Research Survey'			ICM 'Political poll'			Harris		
Age range	Survey	All GB (NRS)	Age range	Survey	All GB (NRS)	Age range	Survey	All GB (NRS)
	%	%		%	%		%	%
18-24	11	14	18-34	37	34	18-34	25	34
25-44	35	38	35-64	36	46	35-54	33	32
45-64	28	28	65+	27	20	55+	40	34
65+	26	20						

Source: ICM, Harris, NRS

183. All three studies, including the 'political poll', showed that refusers were more likely to be older people. They were also a little more likely to be female rather than male. There was a slight tendency in the ICM surveys for people classed C2 or DE to refuse to participate rather than those classed AB/C1. If it is true that the reason for refusal among older females reflect worries about letting strangers into their homes, then it is unlikely that the profile of refusals will be the same for street surveys. Indeed the NOP exit poll, conducted outside polling stations, found men rather than women were more likely to refuse.

184. Even so the absolute numbers involved are so small that too much should not be made of them. The differential response by gender, for example, on the interview introduced as a market research survey is accounted for by a total of 31 people. The discrepancy among those over 65 is accounted for by 48 people out of a total of 2,268 attempted or productive interviews. The similarity of the demographic profiles lends some weight to the theory that people who refuse to take part in either research surveys or political polls are indeed fairly normally distributed in the population.

185. However, it is perfectly possible that, even though the demographic profile of refusers looks normal, their political affiliation is not. Demographics are a relatively poor indicator of voting intentions and the possibility of collecting other information from refusers is understandably limited, especially on variables

likely to be more closely linked to voting behaviour. For example, ICM asked those who refused to participate in the 'research' survey one quick question on economic competence. Refusal to answer this question was also relatively high, at 58% of those who refused to take part initially, which might well be expected. The results from those answering the question show a roughly even split between those saying the Conservatives had the best policies on the economy and those nominating Labour, results which are broadly in line with those obtained from the successful interviews conducted at the same time.

186. If some potential respondents become aware that the survey is about voting intentions before agreeing to participate, there may be a problem if those intending to vote for one particular party are more reluctant to take part. In the last election, some Conservative voters may have decided not to participate in the interview at all. (This is, of course, one of the basic phenomena that gives rise to a spiral of silence, as discussed above; see paragraph 153 *et seq*)[59].

187. One clue to the political affiliation of refusers may be found in examining those who do agree to take part. It seems likely that those who refuse to participate because they are reluctant to disclose their voting intention will be similar to those who agree to participate but refuse to answer the voting question (although there are probably in addition groups who refuse to participate for other reasons, who may be very different). Here there is certainly a differential refusal effect operating to depress Conservative support. Gallup, ICM and NOP have all found a substantial proportion of those who agree to an interview but then refuse to answer the voting intention questions are from those who say they previously voted Conservative.

188. The ICM election recall survey also suggested that differential refusal by party allegiance was partly responsible for the failure of the polls in 1992. This found that refusals to answer in the eve-of-poll survey were predominantly by Conservatives; the demographic profiles of refusers to the other companies' polls and analysis of their answers to other politically relevant questions suggest a similar conclusion. (We examine the question of refusal to answer in more detail below, paragraph 299 *et seq.*)

189. Differential refusal to participate, then, is probably a significant problem in achieving representative samples. To counter this, the polling companies need to make strenuous efforts to include all adults in

[59] There is some evidence that differentially low Conservative participation may arise not only from a conventional 'spiral of silence' effect (reluctance to admit support for the 'unfashionable' party), but from a greater scepticism about surveys - and sociological research in general - among Conservative-supporting groups in the population. Of those who do respond, Conservative voters are considerably more sceptical about opinion polls. This would naturally lead to a higher Conservative refusal rate.

their samples, whether or not they intend to vote, and to encourage all people, regardless of their political persuasion, to participate in the interview. However, this is unlikely to be easy[60].

Refusals in the exit polls

190. Further evidence of the problem of refusals can be seen in the results of the two prediction exit polls conducted by NOP for the BBC and Harris for ITN. As we have seen (Table 5), both these surveys produced results which were considerably closer to the actual outcome than the eve of election polls, but they still produced results which were biased towards Labour.

191. Exit polls can go wrong for two main reasons. One is that they interview in an unrepresentative sample of polling locations (see paragraph 243 *et seq*). The other is that those who refuse to disclose how they have voted differ in political persuasion from those that do. Thus differential refusal is likely to be a major factor in any discrepancy between the results of an exit poll and the actual outcome[61].

192. The problem of differential refusal is clearly understood by both Harris and NOP. Both companies asked people to fill in ballot papers rather than say how they had voted, to overcome the reluctance of some people to state openly their voting behaviour. Both also deliberately kept their questionnaires very short.

193. The two companies differed, however, in how they attempted to compensate for those who refused to participate. When Harris' interviewers found a person who refused to participate they recorded the fact and then looked for the next person emerging from the polling station who was of the same age and sex as the original refuser. Overall they found that 26% refused.

194. NOP's interviewers noted the sex and estimated age of each refuser. Weights were then applied to the data to compensate for (i) differential refusal by polling station and (ii) differential refusal by age and sex. They found that refusers (who were 16% of all attempted interviews) were more likely to be male (in contrast to the opinion poll findings of ICM and Harris above) and aged over 65 (in common with opinion poll findings). The higher refusal rate among men may reflect the fact that they were more likely to be in full-time work than women and hence were less likely to have the time to stop and be interviewed.

[60] Refusal to participate (and non-contact, which has the same effect) are, of course, a problem for random as well as for quota samples, although in the case of random sampling refusal rates may be easier to measure, and somewhat lower. (It is arguable that there is also a greater incentive for the interviewer to get a response with a random survey). This problem is particularly acute in the context of election polls when a quick turnaround is essential and repeated attempts at contact are hence restricted.

[61] Respondents might also lie about who they have voted for. It would however seem mischievous if any substantial number of people agreed to be interviewed only to lie about which party they voted for. It is much more probable that they would simply refuse. Returning Officers insist that people conducting exit polls clearly identify themselves so it is obvious to respondents what they are doing. Voters could therefore decide at a very early stage whether they wish to report their voting behaviour or decline to take part.

Table 31: Age and gender profile of exit poll refusers

		% of refusers	% of participants	Ratio
Male				
	18-29	13.8	12.3	1.12
	30-49	12.8	16.1	0.79
	50-64	13.2	10.4	1.27
	65+	14.2	8.5	1.67
Female				
	18-29	6.8	13.7	0.49
	30-49	13.0	17.5	0.74
	50-64	10.1	10.6	0.95
	65+	16.0	10.6	1.51
Total		100%	100%	

Source: NOP exit poll

195. However, NOP's weighting procedure had very little impact on their estimate of the outcome. Weighting by age and sex simply increased the estimate Conservative performance by just 0.1% in both Con/Lab and Con/LibDem marginals. For while the weighting by age increased the estimated Conservative performance (because amongst the productive interviews elderly people were more likely to have voted Conservative) the weighting by sex reduced it (because men were less likely to vote Conservative than women). Weighting for differences in the level of refusals between polling stations was only a little more useful, increasing the estimated Conservative performance in Con/Lab marginals by 0.4% (but reducing it by 0.2% in Con/LibDem ones).

196. Subsequent analysis of the NOP/BBC exit poll by Curtice and Payne[62] has highlighted other evidence which suggests that differential refusal by Conservative voters may have been responsible for at least half the error in the exit poll. They demonstrate that voters living in Conservative areas were more likely to refuse to participate than those living in Labour ones. Alternative weighting procedures that reflected this pattern might have been more successful. Thus the weight of the evidence from the BBC exit poll confirms our suspicions as to the likely effect of refusals on quota polls.

197. Unfortunately the sex and age profiles of the refusers were not recorded in the ITN/Harris exit poll, making it harder to assess how far differential refusal might have been responsible for its error. Given the weak correlation between age or sex and voting behaviour, however, it seems equally unlikely that Harris' procedures for replacement will correct for differential refusal by Conservative voters. Interviewers may simply replace, for example, an older male Conservative voter with an older male Labour voter.

[62] In I. Crewe and B. Gosschalk, Political Communications: the British General Election of 1992 (Cambridge University Press, in Press).

198. Further, it is also worth noting that the record of the Harris/ITN analysis exit polls reveals a long-term increase in its under-estimation of the Conservative vote. This lends weight to suggestions that differential refusal may be a growing problem which the polling industry needs to address.

Table 32: Error in Harris/ITN Exit polls 1979-92

Election	Average Error
1979	0.3%
1983	0.7%
1987	0.7%
1992	1.2%

Source: Harris/ITN exit polls

199. As we have already seen (Table 9), the exit polls, panel studies, ICM's post-election recall and the cross-section polls conducted after the election all found fairly similar measures of past vote, showing a small bias in favour of Labour which is independent of survey method or place of interview. This is certainly consistent with a persisting tendency of lower Conservative participation through differential refusal exacerbated by greater Conservative reluctance to admit having voted Conservative. However, while it is tempting to assume that all these errors arise from a common cause, it must be admitted that there are alternative explanations which could also account for these results.

6.5 Are there better ways of selecting samples?

The impact of survey methodology

200. A number of the techniques or details used by some or all of the companies have been criticised as being especially liable to cause unrepresentative samples under the quota method[63]. We examine each of these in turn, as well as some alternative methodologies which have been suggested.

In-street and in-home interviewing

201. Some polls were criticised during the campaign for their reliance on street interviewing techniques[64]. In-home polls, it was said, give a better cross section of the population especially if conducted at the week-end.

[63] In the tables that follow results of polls are averaged without regard to sample size. However, weighting by sample size would not alter any of the conclusions. In addition to the polls listed in appendix 1, they include the results of telephone polls by ASL and Nielsen, unless otherwise indicated.

[64] R. Waller, Observer, 22 March 1992.

202. The evidence of the polls conducted during the 1992 election campaign shows that the place of interview did not significantly affect the poll predictions. Table 33 shows that, on average, polls conducted exclusively in street (and all on one day) produced exactly the same results as those conducted partly in home and partly in street (over two days). Polls conducted only in home produced results that were only 1% different.

Table 33: Comparison of in-street and in-home polls: average party share

		Con	Lab	LDem	Oth	C lead
In street	%	38	40	18	5	-2
In street and in home	%	38	40	18	4	-2
In home	%	39	40	17	5	-1

203. The average results for those polls conducted exclusively in home hide much higher volatility in the results they produced. For example the Harris poll conducted on 21-23 March produced a 5% lead for the Conservatives but the same company produced a 4% lead for Labour in another in-home poll conducted in the same sampling points on 22-23 March. Thus, while a few in-home polls produced results during the campaign that were closer than any others to the final outcome, nevertheless others produced by the same company using an identical sample design, the same sampling points, some of the same interviewers and overlapping or consecutive fieldwork dates, produced quite different figures.

204. There is no evidence to support the view that street polling is preferable to in-home polling, or vice-versa.

Telephone polls

205. Telephone polls now seem to produce results very similar to face-to-face polls. In the early eighties when telephone polling was first introduced, there were clear biases in the samples achieved and the technique was not widely adopted. However, as telephone penetration has increased (it is now over 90%) and weighting strategies for coping with potential biases have improved, the future for the use of telephone polls looks better.

206. The evidence from 1992 suggests that those telephone polls conducted during the campaign produced results broadly in line with face to face polls, but with slightly lower measurements of Labour support[65].

[65] For a more detailed discussion of telephone methodology, see J. Clemens, 'The telephone poll bogeyman: a case-study in election paranoia' in I. Crewe and M. Harrop, Political Communications: the General Election Campaign of 1983.

Table 34: Comparison of phone polls and face-to-face polls: average party shares

		Con	Lab	LDem	Oth	C lead
Phone polls (7)	%	38.8	39.5	17.3	4.4	-0.7
Face-to-face polls (47)	%	38.2	40.0	17.7	4.1	-1.8

One-day polls

207. There is **no** difference in results ascertained over different interviewing periods; this holds whether or not telephone polls and panel recalls are excluded.

Table 35: Comparison of one-day, two-day and longer polls

		Con	Lab	LDem	Oth	C lead
All polls (54)	%	38	40	18	4	-2
All one-day polls (15)	%	38	40	17	5	-2
All two-day polls (24)	%	38	40	18	5	-2
All longer polls (15)	%	38	40	18	4	-2
Excluding telephone polls (47)						
One-day polls (13)	%	38	40	17	5	-2
Two-day polls (23)	%	38	40	18	4	-2
Longer polls (11)	%	38	40	18	4	-2
Excluding telephone polls and later waves of panel studies (44)						
One-day polls (13)	%	38	40	17	5	-2
Two-day polls (23)	%	38	40	18	4	-2
Longer polls (8)	%	39	40	17	4	-1

This would seem to scotch any suggestion that length of interviewing period was a significant factor in the outcome of the polls.

Weekend polling

208. It made virtually no difference to the results whether or not fieldwork was partly conducted at weekends. MORI, Gallup and ICM had no weekend interviews; NOP and Harris did in some surveys, as did the telephone polling companies.

Table 36: Weekend and weekday polls: average party shares

		Con	Lab	LDem	Oth	C lead
All polls (54)	%	38	40	18	4	-2
With some Saturday or Sunday fieldwork (17)	%	38	40	18	4	-2
With no Saturday or Sunday fieldwork (37)	%	38	40	18	4	-2
With some Saturday fieldwork (14)	%	39	39	18	5	0
With some Sunday fieldwork (10)	%	39	39	18	4	0

Comparing cross sections with panels

209. Another technique, much more widely used in some foreign countries than in Britain, is the panel study. We consider both panels and cross-sections to be useful techniques which complement each other, providing different and valuable perspectives on the campaign. There was some suspicion in 1987 that 'panel conditioning' effects made the panels slightly less reliable than the cross-section polls[66]. The evidence of 1992, however, does not suggest that either technique is systematically superior or inferior to the other.

210. Panels are a more reliable way of tracking trends than cross sections, but have pitfalls of their own. There is the risk that a sampling error might occur in the recruitment of the initial panel; this error would then be built into the design of the panel. In contrast to a series of snapshot polls in which a rogue sample is likely to be detected by comparison with other surveys taken at the same time, and discounted, if panel study baseline samples are out of line, it will also be detected, but cannot be discounted; instead, this bias must be corrected by weighting the baseline data to cross-section poll findings or, alternatively, be 'lived with'. Even so, the risk of unrepresentativeness of this sort in the panel does not detract from its principal contribution, which is that we can identify what kind of individuals are changing their views or voting intentions. The sampling error associated with the estimate of swing is also lower than in the case of two separate cross-section polls. The demands of recruiting a panel did not seem to produce a less representative sample than the cross-section polls (although with only two representative panels being conducted, no panel effect on voting intention would be likely to be detectable by comparison with the cross-section polls, even if such an effect existed: the degree of statistical significance could not be high enough unless the discrepancy were huge).

211. 'Panel effects' are a separate matter. The inevitable attrition in the membership of the initial panel provides one potential complication, but this can be largely dealt with by judicious weighting. The problem of 'panel conditioning', by which panel members behave distinctively precisely because they are panel

[66] See M. Collins, 'Lessons from the Polls' (1988 MRS Conference Papers).

members, is more intractable, but can be controlled by weighting to cross-section poll results; however this has not been necessary in the past.

212. In fact, the two nationally representative panels in 1992 showed consistent results which matched those of the cross-section surveys being taken at the same time so far as two of the parties are concerned (see Appendix 5B). Nor is there any evidence that the effect of attrition on the panels was to leave them behaviourally unrepresentative once weighting had been applied[67].

213. Plainly there is insufficient evidence from these two panels to draw any concrete conclusions, but as far as the evidence goes there is nothing in the experience of 1992 to suggest the systematic superiority in sampling terms of cross-section polls over panels or vice-versa. This conclusion is much in line with the evidence at previous British elections. Both panels' base samples were recruited using standard quota sampling methodologies. It is possible that a panel recruited at the start of the campaign (when speed is not of much concern) using a closely controlled random sample might produce more accurate measurements. However, on the evidence available, panels conducted using quota samples to select their baselines do not seem to offer a more reliable measurement than conventional cross-section quota samples.

Alternative sampling procedures

Improved quota sampling - random location

214. If the pollsters are asking the wrong people, a natural solution is to consider alternative sampling methods that might deliver a more reliably representative sample.

215. Opinion polls normally use large primary sampling points - constituencies - to construct their samples. Interviewers can interview wherever they like within the constituency in order to fill their quota. Using this design makes it impossible to control, as part of the sample design, the type of area in which interviews are conducted.

216. There are alternative techniques available (used by all the polling agencies on occasion for other types of survey). These use census and geodemographic information about small areas to select the areas in which interviews are to be carried out. Interviewers are given lists of addresses, typically a census

[67] In addition to the voting trends, a further, independent test is newspaper readership, which was not used in the quotas or the weighting; if those dropping out within each quota and weighting category were unrepresentative (i.e. if attrition skewed the panel), we might well be able to detect it by altered patterns of newspaper readership among the panel. In fact, comparing the first and last wave of the MORI panel, there was no perceptible effect of this sort.

enumeration district averaging around 150 addresses, and have to fill their quotas from those streets. The small area selection effectively controls for such factors as social grade, tenure and other area characteristics. There is no need for interviewer quotas to control these. Information on factors such as age, working status and whether or not women have children at home must be obtained for quota controls as these are related to respondents' probability of being at home to be interviewed.

217. This form of sample design has some of the advantages of random sampling, particularly the detailed control of exactly where interviewers conduct their interviews, without all the time and cost penalties of random sampling. Its benefits are, however, highly dependent on the quality of the prior stratification applied to the EDs before sampling. Any weakness in this respect is likely to yield high variability in survey results due to very tight geographical clustering of interviews. Pollsters use this method for sampling by-elections in individual constituencies, and have done for some years, as they and others have for social survey and commercial work. It also allows quite accurate matching of successive surveys by ensuring that the area characteristics of the sample are kept constant.

218. None of the published national opinion polls during the campaign used random location sampling. However, Research Services Ltd did use this method throughout the campaign for their weekly CAPI omnibus survey. They collected measures of voting intention on five surveys during the campaign which, though never published, have been made available to this enquiry. The results of the five surveys are shown in Table 37. On average they showed a 2% Conservative lead during the campaign. This still under-represents the eventual vote for the Conservatives, but to a lesser extent than the published polls.

Table 37: Five unpublished RSL random location surveys

Fieldwork	Sample size		Con	Lab	LDem	Oth	C lead
6-9 March	2,097	%	40	37	16	7	+3
13-16 March	2,160	%	41	35	17	7	+6
20-23 March	2,087	%	39	39	17	5	0
27-30 March	2,133	%	39	36	20	5	+3
3-6 April	2,042	%	38	37	20	5	+1
Average		%	39	37	18	6	+2
Result		%	**42.8**	**35.2**	**18.3**	**3.7**	**+7.6**

Source: Research Services Ltd

219. However, random location sampling was also used in some constituency polls during the 1992 election. These tended to produce results entirely consistent with those being produced by conventional quota sampling[68].

220. It is not clear on this evidence that random location sampling is necessarily superior to conventional quota sampling, but we recommend that this question be explored further.

Probability sampling

221. All the face-to-face polls during the 1992 election used a quota sampling method to select respondents, allowing interviewers freedom to select people in each selected constituency at will within the quota controls. This approach has been criticised by some for over-representing those who are more accessible and more willing to take part. It has been suggested that this bias may have led in 1992 to over-representation of Labour sympathizers, even given the sample controls[69]. These critics would favour the adoption of probability (or 'random') sampling, where respondents are preselected by application of formal sampling procedures.

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[68] ICM's constituency polls in Southampton Test and Gravesham are a case in point.

	Southampton Test		Gravesham	
	27-29.3	Result (9.4)	20-22.3	Result (9.4)
	%	%	%	%
Conservative	38	43	44	50
Labour	46	42	45	40
Lib Dem	15	13	10	9
Other	2	1	2	1
Swing since 1987	**10.5**	**5.6**	**8**	**3**

[69] R.Jowell, B.Hedges, P.Lynn, G.Farrant and A. Heath, 'The 1992 British election: The failure of the polls', Public Opinion Quarterly, Summer 1993.

222. The main argument against this change relates to timing. The use of probability sampling demands that fieldwork should extend over at least a week in order to maximise the likelihood of contacting the selected respondents. Although we find little evidence of dramatic changes of opinion during the 1992 campaign, pollsters are probably right to argue that their clients would not be interested in material that was seen to be 'out-of-date' by a week or more in the course of a three-week election campaign. Interest would be further deflated by the fact that the costs involved would be considerably greater than those associated with a quota sample poll.

223. The timing problem would have been exacerbated in 1992 had probability sampling been used for the final pre-election polls, since there does appear to have been a small shift of opinion during the last week of the campaign. Supporters of probability sampling have however suggested a solution to the problem, involving re-interviewing people selected by probability sampling and first contacted earlier in the campaign. In 1992, this would probably have worked and we would welcome experimentation along such lines in future.

224. Evidence from the past suggests that polls based on probability samples have performed no better, and mostly worse, than those based on quota samples. This evidence is not conclusive, however, in that probability sampling - when used - has often been poorly applied. Commercial pressures have led to tolerance of non-contact rates that would not be accepted, for example, in public sector research - carried out by some of the same polling agencies and others - where probability sampling is the norm[70].

225. Our investigation has thrown up some problems with quota sampling *as currently applied* in polling, in respect of the use of inadequate sample controls and of matching samples to incorrect estimates of population characteristics. In 1992, the consequent inaccuracies probably exceeded those that would have arisen as a result of opinion shifts after the end of interviewing had probability sampling been used.

226. We do not dismiss the arguments against quota sampling and in favour of the probability sampling alternative and would welcome experimental comparisons between the approaches. We believe, however, that it would be unrealistic at present to recommend the change. Instead, we have recommended that pollsters should address as a matter of urgency the need for improvements to their quota sampling methods.

[70] The only major probability sample conducted at the time of the 1992 election was the BES cross-section study conducted after the election. This found figures for recalled 1992 vote (Conservative 46%, Labour 34%, Liberal Democrat 17%) as far from the real result as did the quota recall polls, although the error was in the opposite direction. This emphasises that, even if random samples were to solve the problem of 'missing Tories' that seems to have afflicted the quota polls, they would not necessarily be any more accurate.

Polls in groups of marginal constituencies

227. Most parliamentary constituencies do not switch from one party to another at a General Election. The movement in the marginal seats is likely to determine the outcome of the election. It seems appealing to concentrate opinion polling in groups of marginal seats in order to try to estimate the swing in that category, and hence how many of them will change hands. This underlies the methodology adopted by some of the exit polls.

228. Polls in marginal seats *may* offer a more effective means of predicting the election outcome than estimates of national shares of the vote (see Appendix 7), and if conducted effectively may be a worthwhile addition to the range of polling information being produced. However, they are not an alternative that could replace national polls.

Conclusion

229. None of the alternative sampling designs or interviewing techniques we have investigated seem better suited to solve the problems of the polls than are existing methods. In 1992, in-street, in-home and telephone interviews performed equally well (or badly). There was no difference between one-day and two-day polls, nor did it have any effect if weekend polling was included. Panels performed similarly to cross sections. On alternative sampling methodologies, although some of these appear to have attractions, we do not feel that there is a sufficient case for recommending that any of them should replace the conventional quota methods without further methodological testing.

6.6. Selection of sampling points

Campaign poll sampling points

230. Interviewing individuals who are unrepresentative of the population in each sampling point is only one possible source of sampling bias. Polls can also be wrong because they do not interview in a representative sample of districts. There is plenty of evidence that people's voting behaviour differs not only according to who they are but also where they live. For example, people with the same social characteristics are more likely to vote Labour if they live in the North of England, Scotland or in a working-class area than if they live in the South of England or a middle class area.

231. In choosing where to interview, all the polling companies initially selected a set of parliamentary constituencies. Although the precise details of the selection procedure varied, all the companies employed the same principle. This was first of all to list parliamentary constituencies in order by one or more criteria (such as region and percentage Labour vote at the last election), and then to select the desired number

of constituencies by taking a systematic sample from this ordered list. Because constituencies vary in the number of voters they contain but the polls undertake the same number of interviews in each, this selection was done in such a way that the chance of any individual constituency being included was proportional to the number of electors within it. This ensures that the probability of any individual voter being selected for interview is the same, whichever constituency he or she lives in.

232. Of the polling companies, only Gallup selected the constituencies in which they interviewed especially for the election. In most cases the companies had selected a set of constituencies to which they repeatedly turned in each poll they conducted between 1987 and 1992. This meant that the sample selection was undertaken with probability proportional to electorate size in 1987. However, since 1987 some constituencies had gained electors while others had lost them. Furthermore, there was a systematic pattern to this. In those constituencies which the Conservatives won in 1987 the electorate had on average grown by 657 electors by 1992, whereas in those won by Labour it had *fallen* by 1,088 electors[71]. It has been pointed out that this meant that the polls could have conducted more interviews in strong Labour constituencies than was justified by the number of electors in them at the time of the 1992 election. (The claim made by Butler and Kavanagh[72] that the companies had failed to take any account of differences in the size of constituencies is however incorrect. All of the companies took account of the electorate size of constituencies at the time that they undertook their selection).

233. Just how representative was the selection of sampling points made by each company? We can ascertain this by comparing the votes cast in the sample of constituencies in which each company polled with the result in the country as a whole. This however is not as straightforward as it seems. We have to bear two things in mind:-

a. How should we measure the outcome in each sample of constituencies? The most obvious answer would seem to be simply to add up the votes won by each party in all of the selected constituencies, and so calculate their ***overall*** share of the vote. But in this calculation bigger constituencies will contribute more to the result than smaller constituencies. Yet each polling company undertook the same number of interviews in each constituency; as we have already explained, differences in the electorate size of each constituency were allowed for in the selection procedure. So the representativeness of each company's selection is best measured by giving each constituency equal weight in our calculation. This can be done by calculating the average or ***mean*** vote won by each party in each company's sample.

[71] J. Curtice and M. Steed in D. Butler and D. Kavanagh, The British General Election of 1992 (Basingstoke: Macmillan, 1992).

[72] D. Butler and D. Kavanagh, The British General Election of 1992 (Basingstoke: Macmillan, 1992), p. 154.

b. Against what criterion should we judge each selection? Again one might imagine that this should be the overall share of the vote won by each party, albeit this time across the country as a whole. But our measure of the outcome in each company's sample, the mean share, not only eliminates differences between constituencies in the numbers of electors they contain, but also differences in the proportion of electors who turned out and voted. But the polls would expect to uncover the latter kind of difference so if any gap between the mean share in each company's sample and the overall result were simply caused by differences in turnout this would not be evidence that a company's selection of sampling points was unrepresentative. To overcome this we have used as our criterion against which to judge each company's selection what the overall share of the vote would have been across the whole country if the turnout had been the same in each constituency. We call this the ***equalised turnout share***. Because turnout was higher in seats won by the Conservatives than in those won by Labour this equalised turnout share produces a slightly higher Labour share of the vote than the actual overall share.

234. How well each company's selection of constituencies measured up to this yardstick is indicated in Table 38.

Table 38: Test of Representativeness of Constituency Selection in 1992

		Con	Lab	LDem	Oth
Gallup mean	%	42.2	36.3	17.9	3.6
Harris mean	%	41.1	36.9	17.9	4.1
ICM mean	%	42.5	35.7	17.3	4.5
MORI mean	%	42.3	37.1	17.1	3.5
NOP mean	%	41.4	37.8	16.8	4.0
Average	%	*41.9*	*36.8*	*17.4*	*3.9*
All seats- equalised turnout	%	**42.3**	**35.8**	**18.1**	**3.8**

235. So far as the Conservatives are concerned, three of the companies were close to being spot on, but two, Harris and NOP, were around 1 per cent below the target figure. As many as four of the five companies have a higher mean Labour vote share than the target figure, while three are close to 1 per cent below the Liberal Democrat target figure.

236. Now, we would not anticipate that each individual company's selection would be exactly the same as the target figure. But if the error was simply random sampling error one would anticipate that some companies would be above each target figure while some would be below. However, there are not any instances of a company's mean share being clearly above the Conservative target figure, none at all of being below the Labour figure, or above the Liberal Democrat target. There would seem to be *prima facie* evidence here of a possible small source of anti-Conservative bias in the conduct of the polls.

237. One possible explanation for this is that the companies were unlucky. For example, the rise in Labour's share of the vote could have been higher in the election in those constituencies where the companies polled than those where they did not. However, comparison of the change in each party's share of the vote in each company's selection of constituencies with what happened in the country as a whole shows that there was no systematic difference of that kind.

238. That leaves two possibilities. One is that, indeed, the failure of the companies to take into account the changes in constituency electorate size between 1987 and 1992 rendered their initial selections unrepresentative. The other is that their selection procedures proved to be biased in the first place.

239. We can attempt to adjudicate between these possibilities by repeating the exercise that we have undertaken for the 1992 election for the 1987 outcome. If the original selection was clearly representative then we should find that the companies' selection should vary about the target figure. If on the other hand the original selection procedure was at fault we should find that the differences between the target figure and each company's mean share should largely be identical to 1992.

Table 39: Test of Representativeness of Constituency Selection in 1987

		Con	Lab	LDem	Oth
Gallup mean	%	43.1	32.4	22.4	2.1
Harris mean	%	42.4	32.9	22.5	2.2
ICM mean	%	43.3	31.6	22.6	2.5
MORI mean	%	42.9	32.5	22.9	1.7
NOP mean	%	42.4	33.7	22.1	2.0
All seats - equalised turnout	%	**43.1**	**31.8**	**23.0**	**2.1**

240. Inspection of the table reveals that at least so far as the Conservative and Labour shares are concerned, the difference between each company's mean share and the target share was in most cases as bad in 1987 as in 1992. Again we find that the companies' mean share for the Conservatives tends to be lower than the target and that for Labour above target. Only one company had a Conservative lead (slightly) above target in 1987 while in the remainder the lead was underestimated by between an unworrying 0.5% to a more serious 2.6%.

241. The evidence thus points to the conclusion that there was some error in the companies' selection procedure. This arose because of the way in which the original sample selection was undertaken in 1987 and not because of a failure to take into account changes in the electorate size of constituencies since 1987. We have not been able to identify why the companies' procedures should have erred but recommend that in future companies pay close attention to the representativeness of their sample selection, in part by using the kind of procedure used here.

242. Nevertheless, although we recommend greater attention to this issue, we doubt whether the apparent bias in the companies' selection was a serious source of error in the polls. One company, ICM, did have a selection of constituencies which was very close to the national norm, yet it had as much difficulty as all of the other companies in getting the result correct. Most of the variation between constituency voting reflects demographic differences, and therefore most of the effect of selecting unrepresentative sampling points is cancelled out by the operation of the companies' quotas. The actual effect of the selection of constituencies is almost certainly only a fraction of what the tables seem to suggest.

Exit poll sampling points

243. The selection of a representative sample of polling locations is essential to the success of any exit poll. This is not easy to achieve however in Great Britain because, unlike in most other countries, votes are not counted separately in each polling station. At best it is possible to know that a polling station is in a local authority ward that voted in a particular way in a recent local election, but the political character of individual polling stations may still vary considerably within a ward.

244. Analysis of the results of the NOP/BBC exit poll by Curtice and Payne[73] suggests that the selection of polling locations may have been partially responsible for the error in that poll. In particular, there is some evidence that, amongst Conservative/Labour marginals, the exit poll had a slight pro-Labour bias in the mixture of polling locations where interviews were conducted.

245. Unfortunately, we have not been able to obtain any similar analysis of the Harris/ITN exit poll.

[73] In I. Crewe and B. Gosschalk, Political Communications: The British General Election of 1992 (Cambridge University Press, in press).

7. WAS THE ELECTORAL REGISTER TO BLAME?

246. Another explanation widely offered for the discrepancy between the results of the final polls and the eventual outcome was that the population whom the pollsters were sampling was different from those actually eligible to vote: some electors would never be polled while other ineligible adults might be polled. On the one hand it was argued that the pollsters were interviewing (Labour inclined) people who could not vote because they were not on the electoral register, often because they were trying to evade the poll tax. On the other hand the polls were unable to interview some (Conservative inclined) people who were on the register such as overseas voters. In this section of the report we examine how far such explanations based on the operation of electoral law might help us understand what happened in the 1992 election.

7.1 Electoral Registration and the Poll Tax

247. During the late 1980s the number of electors on the electoral register stopped growing, even though according to the Registrar General's estimates the adult population was continuing to increase. The total number of electors on the electoral register in Great Britain was just 20,000 higher in 1992 than in 1987 whereas the 1987 figure had been as much as 900,000 higher than in 1983. In 1987 the total number of parliamentary electors in England and Wales represented 97.4% of the adult population in 1987, but in 1992 they constituted only 95.7% [74].

248. This decline led to considerable speculation that some people were removing themselves from the electoral register in order to avoid paying the community charge (or 'poll tax')[75]. True, the shortfall in the growth of the electorate began in 1988 in Scotland and in 1989 in England and Wales, in each case a year before the actual introduction of the tax, but this was at a time when the publicity which preceded its introduction was growing.

249. There are three things which need to be established before we can assess how far the introduction of the poll tax might have been responsible for the difficulties of the polls:-

a. What was the size of the shortfall in the electoral register, and how much of that can be accounted for by the introduction of the poll tax?

[74] Boundary Commission for England, Newsletter No. 5 (27 July 1993)

[75] See J. Smith and I. McLean, 'The Poll Tax and the Electoral Register' in A. Heath, R. Jowell and J. Curtice with B. Taylor, Labour's Last Chance? (Aldershot: Dartmouth, 1994).

b. How would those who removed themselves from the register have voted if they had had the opportunity to do so?

c. What measures were taken by the polling companies to avoid interviewing persons who were not on the electoral register?

The Level of Non-registration

250. The 1992 electoral register contained 42,554,664 names in Great Britain. The Registrar General's estimate of the total population who were of a sufficient age to be registered however was 44,484,049[76]. This represents a shortfall of 1,929,385 persons[77].

251. However as we have already indicated there was already a shortfall in the electoral register in 1987, albeit a smaller one. 1,073,436 names were 'missing' from the register in which the 1987 general election was fought. Thus the additional shortfall which could at most be accounted for by the poll tax was 855,949 names.

252. The rise in nonregistration has been confirmed by the results of the 1991 Electoral Register Check[78]. This piece of research, undertaken by OPCS, involved checking the accuracy of the 1991 electoral register against the results of the 1991 Census. This exercise revealed that 7.1% of those who were identified by the Census and who appeared to be eligible to appear on the register were not in fact registered to vote. The equivalent figure at the time of the 1981 Census was 6.5%. However, OPCS also found that more people failed to complete the 1991 Census than previously. Thus some people who fail to appear on the electoral register but should do so also failed to respond to the Census. Taking those into account OPCS estimate that the total level of electoral non-registration lies between 7.4% and 9.0%.

253. We cannot assume that all of the increasing gap between the population estimates and the electoral roll has been caused by poll tax deregistration. For example, it has been argued that Electoral Registration Officers have become less willing to allow the names of persons for whom no registration form has been

[76] The qualifying date for electoral registration is 10 October. The new register comes into force on 16 February the following year and remains operative until 15 February in the year after that. Persons whose eighteenth birthday falls at any time during the currency of the next register are entitled to appear on that register. This means that persons who are as young as 16 on the qualifying date are entitled to appear on the register. The figure quoted here therefore represents all those aged 17 and above in the Registrar General's population estimates for mid-1991 together with two-thirds of those aged 16.

[77] Parliamentary Written Answers from the Secretaries of State for Health and Scotland to Harry Barnes, MP, 26 October 1992.

[78] S. Smith, Electoral Registration in 1991 (OPCS/HMSO, 1993).

returned to remain on the register[79]. Certainly, the 1991 Electoral Register Check found a reduction in the number of redundant names on the electoral register compared with 1981. They estimated that between 6.0% and 7.9% of the names of the electoral register did not correspond with those who were actually living there at the time of the 1991 Census, compared with between 7.1% and 10.4% in 1981 - a fall of between one and two-and-a-half percentage points.

254. Further, the rise in nonregistration between 1981 and 1991 as measured by the Electoral Registration Check fits a longer-term trend which long pre-dates the poll tax. Previous checks had found a rise in non-registration from 4.0% in 1966 to 6.5% in 1981, a rise of 2.5% over that fifteen-year period. If we take the mid-point of the range quoted for OPCS in 1991 when the inaccuracy in the 1991 Census is also taken into account, viz. 8.2%, this would mean that there was a further 1.7% rise in the following ten year period, an almost identical rate of increase. Further, when OPCS asked those who were not on the electoral register why they were not on the register they found that the proportion who said they did not wish to be registered was unchanged between 1981 and 1991 at just 19%. In contrast the proportion saying that they had not received the form to register - an indicator of the efficiency of the canvass undertaken to compile the register - rose from 5% to 13%. The continuous rise in nonregistration is clearly a challenge for the polls, but the evidence suggests that it cannot simply be accounted for by the poll tax.

Table 40: Level of electoral registration in England and Wales 1981-93

Year	Registered Electorate (000s)	Electorate as proportion of estimated resident population of appropriate age %
1981	37,183	97.4
1982	37,492	97.6
1983	37,708	97.7
1984	37,949	97.7
1985	38,080	97.3
1986	38,319	97.3
1987	38,568	97.4
1988	38,629	96.9
1989	38,559	96.3
1990	38,596	96.0
1991	38,509	95.5
1992	38,654	95.7
1993	38,634	95.4

Source: Electoral Statistics, OPCS

[79] R. Jowell, B. Hedges, P. Lynn, G. Farrant and A. Heath, 'The 1992 British election: The failure of the polls', Public Opinion Quarterly (Summer 1993). However, J. Smith and I. McLean dissent strongly in A. Heath, R. Jowell and J. Curtice with B. Taylor, Labour's Last Chance? (Aldershot: Dartmouth, 1994).

255. The most sophisticated and comprehensive analysis of the impact of the influence of the poll tax has been undertaken by Smith and McLean[80]. Looking at the relationship between the rate of decline in the electoral register and the level of the poll tax across Great Britain, they estimate that as many as 600,000 names might have been missing from the electoral register in Great Britain because of poll tax deregistration, or only about two-thirds of the rise in non-registration between 1987 and 1992.

Who Did Not Register?

256. Deregistration can only have been responsible for the difficulties of the polls if the supporters of one of the major parties were substantially more likely to have deregistered than the supporters of the other. It has been widely assumed that those who did not register would have mostly been Labour supporters (or perhaps Labour and SNP supporters in Scotland).

257. However, no substantial national study of the likely voting behaviour of nonregistrees has been undertaken. In fact there are some strands of evidence which suggest that the image of the typical nonregistree as an archetypal Labour voter may well be overdrawn. When they returned after the election to those respondents whom they had interviewed in their final pre-election poll, ICM identified 30 persons who said they had not voted because they were not registered or did not have a poll card. Of these, seven had originally said they were going to vote Conservative, eight Liberal Democrat and just seven Labour. Another survey, undertaken by Granada Television among nonregistrees in Hampstead & Highgate, found that 21% would have voted Conservative and just 42% Labour. Even amongst those citing the poll tax as their reason for not registering the proportion intending to vote Labour was only 51%[81].

258. The 1991 Electoral Registration Check also suggests that it is only partly true that the social characteristics of those who are not registered are similar to those which are commonly associated with Labour voting. Major factors associated with non-registration are youth and geographical mobility. As many as one in five of those in their early twenties are not registered while as many as 28% of those who have moved in the previous year do not appear on the register. Young people are somewhat more likely to vote Labour than older people; however, what evidence there is on mobility suggests that those who

[80] J. Smith and I. McLean, 'The Poll Tax and the Electoral Register' in A. Heath, R. Jowell and J. Curtice with B. Taylor, Labour's Last Chance? (Aldershot: Dartmouth, 1994).

[81] Quoted in I. Crewe, 'A Nation of Liars? The Opinion Polls in the 1992 Election', Parliamentary Affairs, Vol 45, No 4 (October 1992).

move house are more likely to be Conservative[82].

259. Further, although the level of non-registration is, at 20%, considerably higher in Inner London - an area of relative Labour strength - than in the country as a whole, it is also above average in Outer London (10%) where the Conservatives are the stronger. There is little difference between the mostly Labour bastions of the former English metropolitan counties and the rest of England.

260. There are, however, some signs to back the claim that typically Labour voters are more likely not to appear on the register. Most dramatically, over one in three of New Commonwealth citizens do not appear on the register. But in addition, while only 4.9% of owner occupiers are not registered, 6.4% of local authority tenants are unregistered.[83]

261. Caution is needed about the assumption that deregistrees would necessarily have voted Labour. At most it seems that they were somewhat more likely to vote Labour than the general population. But even on the most extreme assumptions about both the level of poll tax deregistration and the propensity of the deregistered to vote Labour, we can see that its impact upon the national share of the vote won by the parties would have been made only a small contribution to the discrepancy between the final campaign polls and the eventual outcome.

262. We can see this by making the following assumptions:-

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[82] A MORI survey in 1991 found that mobility was associated with a stronger Conservative vote.

Q. "Have you always lived within about ten miles of here, or have you ever lived somewhere else?"

	All n=1,230	Always lived within about ten miles of here n=585 (48% of total)		Lived somewhere else n=628 (51% of total)	
	%	%	% of 3-party vote	%	% of 3-party vote
Con	31	25	32	37	46
Lab	34	42	54	26	33
Lib Dem	14	11	14	17	21
Oth/DK	21	22		20	

Fieldwork: 11 April-2 May 1991
Source: MORI

A similar conclusion can be drawn from David Denver and Keith Halfacree's study (North West Regional Research Laboratory, Lancaster University, Research Report No 24: Migration Into, Out of and Within Parliamentary Constituencies in England and Wales) of migration between constituencies, which found that both the highest in-migration and out-migration was found mainly in Conservative seats, and conversely that the lowest was found mainly in Labour seats.

[83] The highest level of nonregistration is however amongst those renting privately where approximately 1 in 4 are not registered. This group is typically less likely to vote Labour than council tenants though more likely to do so than owner occupiers.

a. As many as 850,000 electors (2% of the eligible population) failed to register to vote because of the poll tax. This effectively means that we assume that all of the widening of the gap between the numbers on the electoral roll and the mid-year population estimates was caused by the poll tax.

b. If they had been on the register, 65% of these electors (i.e. 1.3% of the electorate, had they been registered), would have turned out and voted. (This assumes that those who were prepared to withdraw their names from the electoral register on account of the poll tax are a little more reluctant to go out and vote than the general population.)

c. Of those who would have voted, 75% would have voted Labour, 15% Liberal or SNP and 10% Conservative.

Table 41: Maximum Likely Impact of Poll Tax Deregistration

	Actual Result %	Adjusted for Possible Deregistration %
Conservative	42.8	42.3
Labour	35.2	35.8
Others	22.0	21.9

263. These assumptions would be enough to give Labour 414,375 more votes, the Conservatives 55,250 and other parties 82,875. As Table 41 indicates this would increase Labour's share of the vote by 0.6% and reduce the Conservatives' share by 0.5 %. Thus at the very most poll tax deregistration could have caused the polls to have underestimated the Conservatives' lead by just 1%. In practice it seems more likely that it would have affected the polls' estimate of the lead by half a point at most, and that the total effect of failure to register for any reason cannot have been more than one point.

The Measures taken by the Polling Companies

264. Even this more conservative estimate assumes that the methodology adopted by the polling companies failed in any way to take into account the possible impact of deregistration. In practice this was far from the case.

a. MORI and ICM (and, on their early surveys, NOP) did in fact ask their respondents whether or not they were on the electoral register before proceeding with the interview. Of course this

procedure is not infallible in that respondents may not always be aware that they are not on the register[84].

Certainly, the polls that did adopt this procedure did not produce results that were consistently more pro-Conservative than other polls conducted at the same time. This of course gives further reason for caution about assuming that poll tax deregistration had a substantial partisan impact.

b. As the quota control procedures used by every company relied on the National Readership Survey in deciding what proportion of those in each social grade, etc., should be interviewed, and the NRS at this period used the electoral roll as its sampling frame, the under-representation of certain groups on the register was already taken into account by the polls. This reduced any impact that de-registration might have otherwise had on the poll results.

265. Furthermore, there is likely to be substantial overlap between that part of the population which the electoral registration officers fail to find and which opinion polls fail to interview - their methods are to some extent similar and the difficulties which they face likely to be the same. Hence those adults not on the register are probably less likely to be interviewed than those that are. This was reflected in the low numbers of unregistered adults found by those surveys that did attempt to screen them out: MORI found very consistently during the campaign that 2% said they were not registered and 3% didn't know.

266. It has however been claimed that these measures were inadequate. In particular, Kellner has argued that the result of the exit polls show that there was a substantial difference between the social profile of voters and that of the general population. Typically, 23-24% of respondents to pre-election exit polls were council tenants while in the BBC exit poll just 15% said they were council tenants and in the ITN poll as few as 13%[85]. Further only 53% of respondents to the ITN survey were placed in one of the C2DE social grades while most pre-election polls aimed for 59%.

267. We have already seen that there is reason to believe that the polls did over-represent council tenants (Table 16 above); the 1991 Census suggests that only 19% of people live in council houses. However, this error has nothing to do with the levels of electoral registration.

[84] No solid figures are available, but there is substantial anecdotal evidence that significant numbers who thought that they were registered were in fact not, and were thus prevented from voting. (See, for example, D. Butler and D. Kavanagh, The British General Election of 1992, p. 232. Whether this factor would have had any party effect must be questionable.) The rise from 2% to 4% saying they were unregistered between campaign and post-election surveys may reflect respondents who discovered they were unregistered only when they tried to vote.

[85] P. Kellner in the Independent, 1 May 1992.

268. But we are still left with a lower proportion of council tenants in the exit polls than in the Census, and with the lower percentage in the C2DE social grades as apparent evidence of the impact of poll tax deregistration. However, these differences are almost undoubtedly a methodological artefact. In the exit polls information on social grade and housing tenure was ascertained through a self-completion questionnaire while in the polls they were ascertained by an interviewer. So far as social grade is concerned, we can anticipate that respondents will tend to place themselves in a higher social grade than interviewers would. Meanwhile, so far as council tenants are concerned, some of them appear to have classified themselves as 'other rental'; the proportion of owner occupiers in the exit polls - 67% according to the BBC and 70% according to ITN - is well in line with the figure in the 1991 Census of 70%[86]. Once again the evidence of any substantial registration effect is wanting.

269. The claim that the polls were led astray by the poll tax initially seems a simple and attractive one. But on closer examination it can clearly be seen that even if the whole effect of deregistration had been reflected by the polls, on arithmetic grounds alone it could not be a major explanation for the error - at most the error it could account for would be ½% swing. And in practice the procedures adopted by the pollsters did take into account, at least to some degree, the possible impact of electoral non-registration. It may have had some effect, but its contribution to the overall error was only a minimal one.

7.2 Postal Votes

270. Another possible cause of the discrepancy in the opinion polls could lie in their failure to allow for the behaviour of postal voters. Such voters are commonly thought to be more Conservative than other voters. The circumstances which give people the right to a postal vote, such as being away from home for work or chronic illness, may well also make it less likely that they will be interviewed in an opinion poll. For example, interviewing is not normally conducted in residential homes for the elderly.

271. However, only 692,139 postal votes were cast at the 1992 election[87]. This figure is in fact more than 100,000 lower than in 1987, when 793,062 postal votes were cast. Further, there is some evidence to suggest that Labour were more successful at securing postal votes than at previous elections. The postal vote rose on average by 50% in fifty-one of Labour's target seats. Of the 38 respondents to the 1992

[86] It is also worth noting that Harris found a similar discrepancy in their 1987 exit poll which also used a self-completion questionnaire but was undertaken before the introduction of the poll tax. Only 17% of their respondents were classified as council tenants compared with a population estimate of 25%. See R. Waller, 'The Harris/ITN Exit Poll', Journal of the Market Research Society, XXIX, (1988), pp 417-28.

[87] Election Expenses, HMSO, April 1993.

British Election Study who were issued with a postal (or proxy) vote, almost as many said that they voted Labour (43%) as Conservative (44%) whereas in the 1987 study (n=75) the Conservatives (38%) had a clear advantage over Labour (24%). Meanwhile a survey of 921 postal voters in 15 Conservative-Labour marginals undertaken as part of the BBC's exit poll in marginal seats found that whereas 42% said that they had voted Conservative, as many as 45% said that they voted Labour. But even if this were not the case and we assumed that the Conservatives won as much as 55% of the postal vote and Labour just 25%, this would account for no more than 0.4% of the Conservatives' lead.

7.3 Overseas Voters

272. The Representation of the People Act 1989 enlarged considerably the number of British citizens resident abroad who were eligible to vote in a general election. Whereas previously only those who had only been abroad for less than five years were allowed to vote (and had to sign a declaration that they had not permanently emigrated), the 1989 Act allowed people who had been abroad for as long as 20 years still to vote. These people are of course impossible for the opinion pollsters to interview, and it is widely assumed that overseas voters are much more likely to vote Conservative than the general population.

273. However, the level of registration for the overseas vote has been low. Just 31,942 overseas voters were on the 1992 electoral register (actually a slight fall on the 1991 figure when the new law first applied). This is clearly too small a number to have had any material impact on the election outcome - even if they all voted and all supported the Tories, they would amount to less than 0.1% of the turnout.

7.4 Conclusion

274. We conclude that non-inclusion of some adults on the electoral register (whether or not caused by deliberate non-registration to evade the poll tax) probably had some effect on the error in the polls, but only a slight one. Those companies that did so were probably wise to attempt to allow for this factor by excluding those who admitted they were not registered, although in fact it seems to have made little difference to the results. This is clearly a factor that the companies should keep an eye on in the future, and if the scale of non-registration should rise or the political impact become more marked, further measures to compensate for the problem may be necessary.

275. Postal voting probably had no effect on the error in the polls, and Overseas Voters certainly had no effect.

8. DO THE POLLS GET CLEAR AND ACCURATE ANSWERS?

276. Quite apart from the hazards of late swing (when answers that are accurate at the time they are given but prove misleading because the respondent later changes his or her mind) we need to investigate the possibility of answers that are misleading even at the time of interview.

8.1 Do respondents lie?

277. We have found no evidence to make us believe that deliberate lying was a significant problem in the polls in 1992.

278. It is natural to wonder how far the answers given by respondents can be trusted. If a sample is perfectly representative, will the voting figures it shows really reflect the actual voting behaviour of the population? Plainly there are a number of possible pitfalls. Some will simply refuse to answer. Others may give an answer that is more actively misleading, falsely claiming to be don't knows when in fact they support a party. Some of those who say they would not vote could be deliberately concealing a party loyalty, preferring not to give an outright refusal or admit to indecision. Finally, there could be outright lying - naming a party for whom the respondent has no intention of voting.

279. In the aftermath of the 1992 election, there was considerable publicity for the suggestion that a substantial error in the polls could be attributed to lying[88] - respondents who knew that they intended to vote Conservative but deliberately named other parties or said that they would not vote. This, it was suggested, was because a 'shame factor' had come to be associated with voting Conservative: the Labour party had succeeded in occupying the moral high ground. It was alleged that Conservatives were ashamed of their voting intention and their motives, but believing they were in a minority, concealed it. This theory was particularly popular with some journalists. (One said 'I always lie to pollsters', yet, when asked by which polling agency he had been interviewed and when, admitted he had never been interviewed at all.)

280. While the lying hypothesis cannot, by its nature, be conclusively disproved, all of the evidence available is against it. Since nobody has produced evidence of this occurring in the past, and since a wholesale change in the behaviour patterns of the British public seems unlikely, this is not an explanation deserving credence unless it offers a better explanation of the observed facts than alternative explanations. In fact, all the evidence is against lying being to any significant degree the explanation for the poll results in 1992; in some respects it is directly contradicted by observed facts, and overall it seems to add nothing to the degree of explanation offered by other factors.

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[88] Notably R. Harris in the Sunday Times, 12 April 1992.

281. The most direct evidence available on lying is that from the BES cross-section. There are also the voters' own accounts of when they made up their mind which way to vote.

Evidence from the BES cross-section study

282. The 1992 British Election (cross-section) Study provides one way of testing the claim that the polls were wrong because people were lying to the pollsters during the election campaign.

283. The 1987 and 1992 BES were able to ascertain whether or not people gave an honest answer about whether they had voted or not. The honesty of the BES respondents about whether they had voted was ascertained by examining the marked-up electoral registers which are available for examination in the Lord Chancellor's Office for twelve months after the election. These marked-up registers tell us whether a voter voted or was issued with a personal or proxy vote. (Unfortunately they do not tell us whether or not a postal voter returned his or her vote. Further a handful of voters might be inaccurately identified as lying about having voted because they were registered at more than one address and exercised their franchise somewhere other than at the sample address). The relevant entry for all but 146 of the 2,855 BES 1992 respondents was successfully located from the marked-up registers.

284. If the polls were led astray because people were lying to the pollsters then we might expect to find that more people lied to the BES about whether or not they had voted than did in 1987. In particular we might anticipate an increase in the proportion of voters who actually voted while saying that they did not vote - because they had voted Conservative and did not wish to admit the fact. Table 42 shows the level of honesty amongst those voters whose electoral register entry was traced, excluding those voters who were issued with a postal vote, in both the 1992 and 1987 election studies.

Table 42: The Honesty of Reported Voting

	% Honest			
Reported Behaviour	**1987**		**1992**	
Voted	95.0	(n=3,139)	95.9	(n=2,337)
Did Not Vote	94.5	(n=509)	90.0	(n=333)

Source: BES

285. Given that turning out to vote is widely regarded as an obligation, we would anticipate that some respondents would say that they voted when in fact they did not do so. Indeed, we find that in the last two elections between 4 and 5 per cent of those who said that they had voted in fact had not done so.

However, there is no evidence of any substantial change in the level of overclaiming between the two elections.

286. We also find that in 1987 just over 5 per cent of those said they had not voted did in fact do so. In 1992, the level of this form of dishonesty nearly doubled. The difference is statistically significant at the 5% level of probability[89]. It would seem that here we do have some signs of a marginally increased reticence amongst some respondents to say that they had voted.

287. But did these reticent voters favour the Conservatives? There are only 33 reticent voters in total in the survey, so the evidence can only be indicative. And of course because they said that they had not voted, they were not asked for which party they had voted. But all respondents were asked how they felt about all of the main parties on a five-point scale that ranged from 'strongly in favour' to 'strongly against', and nearly all of the reticent voters answered these questions.

288. We can convert this party feeling indicator into a mark out of five for each party, with a response of 'strongly in favour' receiving a mark of five and 'strongly against' a mark of one. This reveals that, far from being closet Conservatives, these reticent voters were more likely to feel favourable to the Labour party. The Conservatives were given an average mark of only 1.63 and the Liberal Democrats, 1.82; Labour on the other hand scored 2.14.

289. The social profile of these voters corroborates this evidence. 44% of them were in the (Goldthorpe) working class compared with 34% of all respondents in the survey, only 18% were members of the salariat (27%) and only 44% were owner-occupiers (72%).

290. The BES does thus contain some evidence that voters might have been more likely to lie to the pollsters than five years previously. But it does not corroborate the claim that Conservative voters were more ashamed of their choice than were Labour supporters.

291. Indeed ironically the smaller proportion of reticent voters in 1987 show more sign of being closet Conservatives. They gave the Conservatives an average score of 2.00, and the Liberal Democrats 1.92, but Labour only 1.76. The social profile of these electors is also less distinctive. Although they are somewhat more likely to be working class, their housing tenure profile is almost a mirror image of the whole country.

292. The partisanship of those who said that they voted but did not is also of interest. If these electors were more likely to be Labour than those who really did vote, then two points might follow. The first is

[89] Chi-square = 5.82, df=1

that it might provide a reason why the post-election opinion polls still underestimated the scale of the Conservatives' success because Labour supporters were more likely to overclaim about having voted than Conservatives. The second is that we might infer that when respondents were asked in the final campaign polls whether or not they would go out and vote, the polls may have overestimated the level of turnout amongst Labour supporters.

293. Table 43 shows the reported vote of those who actually voted (including postal voters) and those who said they had voted but did not. We can see that indeed the 'liars' were indeed somewhat more likely to say that they had voted Labour . Their reports were sufficient to reduce the estimated Conservative lead by a half percent. The social profile of the 'liars' was also consistent with this pro-Labour stance. 40% were members of the working class; only 22% were in the salariat and only 63% were owner occupiers.

Table 43: Reported Vote of Voters and Liars 1992

		Reported Vote					
Actual Behaviour		**Con**	**Lab**	**LibDem**	**Other**	**Refused etc.**	
Actually Voted	%	45	32	17	2	3	(n=2,276)
Liars	%	38	39	13	5	5	(n=96)

(Excluding Other and Refused columns, Chi-sq=3.13, df=2)

Source: BES

294. The pro-Labour character of the liars was also noticeably stronger in 1992 than in 1987 (see Table 44). The evidence does therefore seem to be consistent with the claim that the polls findings may have been led astray by differential abstention by Labour supporters who were reluctant to admit they would not vote or had not voted Labour. However, the difference between the behaviour of those who voted and the 'liars' is not statistically significant at the 5% level. So the results can at most only be regarded as suggestive.

Table 44: Reported Vote of Voters and Liars 1987

		Reported Vote					
Actual Behaviour		**Con**	**Lab**	**Alliance**	**Other**	**Refused etc.**	
Actually Voted	%	43	30	23	1	3	(n=3,049)
Liars	%	43	33	22	0	1	(n=158)

Source: BES

Evidence from the campaign polls

295. Although it is obviously impossible to prove from the polls taken during the campaign whether respondents were being honest with interviewers, the consistency of their other answers with their declared voting intentions tells against this suggestion. If voters were lying, they must have been lying consistently, which is never an easy thing to do (and one wonders, in any case, why they should bother). Very few of the respondents who said they intended to vote Labour gave Conservative responses to the other questions they were asked[90]; they certainly appeared to be genuine enough Labour (or anti-Conservative) supporters at the time.

Evidence from the 'When did you decide?' questions

296. If the lying hypothesis has substance, we would expect answers to polls taken after the election to be more accurate and honest than those that came before. In this context, the evidence of post-election surveys asking *when* voters decided for which party they would vote is against the lying hypothesis. Consider the case of a respondent who lied during the campaign, saying that he intended to vote Labour but who actually voted Conservative; then a comparison of his declared voting intention with his actual vote would show that he had changed his mind. In fact, however, he would not have changed his mind and would presumably subsequently say so after the election (when admitting to having voted Conservative). Consequently when all such respondents are combined and included in the poll figures, the result would be that the apparent number of changes of mind during the campaign (i.e. the discrepancy between the poll and the result caused by lying) would be greater than the admitted number of changes of mind in post-election surveys. In fact the opposite is the case, as is clear from both from actual changes detected in panel studies and from willingness to change found in cross-section polls. More voters said after the election that they had made up their mind after the campaign started than admitted during the campaign that they were undecided or subsequently turned out to have changed their vote from their declared intention.

297. It could be argued that this merely indicates that respondents lied by saying that they had made up their minds when they knew that they had not, but this seems a fairly implausible suggestion. On this evidence, the lying hypothesis does not stand up.

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[90] MORI tested responses to their poll for The Times which interviewed on 30 March, which found a Labour lead of 7% (the highest of any poll during the campaign). Fifteen other attitudinal questions whose answers could be reasonably classified as pro-Tory and anti-Tory were cross-checked for consistency against voting intention. Of those who said they intended to vote Labour (n=412), less than 1% agreed with as many as six of the fifteen pro-Tory statements (e.g. that John Major would make the best Prime Minister, that the Tories had the best policies on defence or on law and order, etc), and 95% rejected at least 11 of the 15. All the demographic and other indicators were similarly consistent.

Conclusion

298. We conclude that there is no evidence that should make us believe that deliberate lying was a significant problem of the polls in 1992.

8.2. Won't Says and Don't Knows

299. When asked about their voting intention, not all respondents will name a party. The remainder form a group that have often been referred to as the 'don't knows' but which actually form three separate categories. These are:-

a. Those who say they definitely ***will not vote.***

b. Those who say they are ***undecided*** or don't know who they will vote for.

c. Those who ***refuse to say*** who they will vote for, referred to elsewhere as 'item refusal'.

300. In 1992 all the polling companies 'squeezed' the people who said they did not know who they would vote for. Simply, they were asked a follow-up question[91] to try to elicit a party preference. However, while some polling companies asked the follow up (squeeze) question of both the 'don't knows' and 'refusers', ICM only squeezed the 'don't knows'. This may account for the small differences between companies in item refusal rates shown in Table 45 below.

301. After squeezing, the remaining 'refusers' and 'don't knows' were usually excluded from poll calculations along with declared non-voters. The resulting figures are usually taken as estimates of party share in any election. If 'refusers' and 'don't knows' end up not voting or vote in the same way as those who declare support for different parties then this process will not distort the figures. However, if 'refusers' and 'don't knows' end up voting disproportionately for one party then the polls will not be accurate.

Table 45: Proportion of respondents not declaring a voting intention in 1992 published cross-section polls

	Gallup n=6,692 %	Harris n=14,122 %	ICM n=20,405 %	MORI n=7,138 %	NOP n=9,978 %
Refused	n/a	3.2	4.5	2.6	2.0
Don't Know	n/a	12.9	6.6	5.9	3.8
Will Not Vote	n/a	4.0	4.1	4.2	8.1
Total	7	20.1	15.2	12.7	13.9

Gallup recorded refusals, don't knows and will not votes as a single category.

[91] For details of the wording, see Table 49.

302. Although all the companies 'squeezed' their respondents, there was a remarkable variation in the proportion of their respondents who failed to give a voting intention. The proportion varies from 7% to 20.1%. This variation is startling when we consider how similar were the vote shares reported by the companies.

Item refusal, don't knows and overall refusal rate

303. The committee finds that item refusal (people who refuse to answer the voting intention question) was a significant problem for the polls in 1992. Furthermore, it seems probable that some people specifically wishing not to disclose their voting intention nominated the 'don't know' response. It follows that if some of these people became aware that they would be asked voting intentions at an early stage they might have refused to participate in the polls at all.

304. There is considerable evidence to suggest that those who refused to answer the voting intention question were more likely to vote Conservative. In ICM's experiment to measure refusal rates (see paragraph 178 *et seq.*) refusers to the voting intention were asked which party was best able to handle the economy; as Table 46 below shows they were more likely to nominate the Conservatives. (However it should be noted that the base sizes are small and thus the findings are not statistically significant.) NOP noted the same effect in their election polls. Meanwhile, the British Election Study found four times as many of those who refused to say how they would vote before polling day reported afterwards that they voted Conservative than said they voted Labour.

305. Item refusal is a specific reluctance by a group of voters to state their party preference. It is very likely that item refusal is the tip of a larger problem for the polls. Only a proportion of those who refuse actually say they are doing so. Another group of people who refuse simply select the 'don't know' option as a more polite form of refusal.

306. The 'don't knows' show some similarities to those who refuse to answer suggesting that some say 'don't know' when they actually mean they are not willing to say. ICM found that while they were less likely to do so than refusers, 'don't knows' were more likely to name the Conservatives as the best party on economic competence than those who did declare a voting intention. This again is confirmed by NOP, although again the sample sizes involved are small.

307. It obviously follows that if potential respondents are told before an interview starts that they will be asked how they will vote then they may simply refuse or otherwise excuse themselves from the interview

Table 46: Best party on economic competence: the view of the 'don't knows'

	Refusers to the voting question n=37 %	Don't knows n=49 %	Productive sample excluding don't knows n=1,827 %
Conservatives	86	76	51
Labour	8	22	37
Lib Dems	5	10	12

Source: ICM

altogether. Thus item refusal may not even be recorded as interviewers move on to find more willing respondents.

308. In tests on secret ballots, ICM found 7% item refusal when interviewers were carefully instructed to introduce the poll as a market research survey. The same company found 8% item refusals to reported vote in the Rowntree Reform Trust poll of almost 10,000 people interviewed in the two days after the 1992 election. This was introduced as an opinion poll but, as the actual election was so recent, respondents could not claim the don't know/can't remember option as an alternative to a straight refusal.

309. Despite the direct evidence of the campaign polls it therefore seems likely that the true level of item refusal lies at between 5% and 10%. This is disturbing because refusers were (and remain today) a group of people who are predominantly previous Conservative voters.

310. It is also interesting to note that the level of 'item refusal' is higher when respondents cannot claim they 'don't know' or 'can't remember'. ICM found 8% item refusals in the Rowntree Trust poll of almost 10,000 people interviewed in the two days after the 1992 election.

311. In its tests on secret ballots, ICM found that 'item refusal' rose to 7% when interviewers were carefully instructed to introduce the poll as a market research survey. Thus again suggests that the problem of refusal is greater than implied by the level of manifest item refusers. In a poll. Some respondents refuse to participate in opinion polls altogether if they are made aware at the start that they will be asked how they will vote.

312. Analysis of ICM's final poll before the 1992 election shows an inclination of some 1987 Tory voters to refuse to answer or select the 'don't know' option. Before squeezing this combined group contained 114 admitted 1987 Conservative voters who said they were certain to vote in 1992, 60 Labour voters and 36 previous SDP/Liberal Alliance voters. After squeezing 34 previous Conservative voters said they were certain to vote but did not know which way they would vote against only 13 Labour supporters and 13 SDP/Liberal Alliance voters.

313. If this group of people were a random sample of all adults then only 33% of them should have admitted being Conservative voters. However the figures above suggest that 57% were previous Conservative voters. The process of excluding them from the figures assumes that they will either not vote (even though they have stated their intention to do so) or vote in the same way as the rest of the sample. Their heavy skew towards previous Conservative voters suggests that this would not be so.

314. Analysis of ICM's recall survey suggests that 60% of the don't knows and refusers to the initial voting question turned out to vote for the party they previously said they supported. Of the remaining 40%, the Liberal Democrats were the main beneficiaries of switching amongst previous Labour and Conservative voters while the Conservatives were the main beneficiaries amongst wavering Liberal Democrats.

315. ICM's finding is corroborated by the British Election Study which had available to it the 1987 vote of its panel respondents as they reported it after the 1987 election. Of those who refused to say before polling day to say how they had voted or said they didn't know, 58% of them eventually voted in line with their 1987 vote. The proportion amongst 1987 Conservative and Labour voters was as high as 70%.

316. The difficulty of overt and covert item refusal is difficult but not impossible for the pollsters to overcome. All respondents must be encouraged to answer the vote intention question wherever possible. But most importantly, pollsters cannot assume that those who do not answer the question will vote in the same way as those that do. Two sources of evidence suggest that a better assumption would be to reckon that 60% of them will vote for the party they voted for at the last election. It may also be possible to ask other questions on general attitudes, issues and leaders that allow the pollsters to guess at item refusers true intentions.

317. Item refusal as outlined above should be differentiated from an additional problem for the pollsters which are people who remain genuinely unsure of who they will vote for, even up to the last moment. The don't know effect is discussed below.

Explaining the impact of the 'don't knows'

318. The extent to which the voting question elicits a 'don't know' answer may reflect what some have called the fashionability of parties or be attributed to the spiral of silence (paragraph 153 *et seq.*). People who are natural supporters of a particular party which is widely perceived to be doing badly may not switch to another but become unsure of what they will actually do on polling day. They therefore answer the voting intention question with 'don't know' or 'won't vote' or 'I would rather not say'. In the end some of them may return to the party they have voted for before. This fashionability factor, if it exists, rather neatly fits the pattern of bias found in past elections. For example, the 1983 election was held after the Falklands

war victory and at a time when economic fortunes were improving. Labour, under the leadership of Michael Foot, proposed interventionist economic policies and a defence strategy based on unilateral nuclear disarmament. In such circumstances some traditional Labour supporters may have become less sure of their voting intentions or more reluctant to admit that they would still vote Labour.

319. Certainly, this theory would fit the evidence of more recent surveys. Table 47 below is taken from the January 1994 *Guardian*/ICM poll, when the Conservatives were very much the least 'fashionable' party.. The findings are consistent with all ICM polls conducted in 1993 and the same overall picture emerges from MORI surveys. ICM found that 68 current 'don't knows' owned up to voting Tory last time. The equivalent figure was only 21 for past Labour supporters and 21 for Liberals. Aligned to this, 17 people who previously voted Tory said they now didn't think they would vote at all, against only 7 previous Labour supporters and 4 previous Liberal voters who said the same.

Table 47: Declared past vote by current voting intention, January 1994

	Declared past (1992) vote:						
Voting Intention:	**Con** *n*	**Lab** *n*	**Lib** *n*	**Oth** *n*	**Did not vote** *n*	**Ref-used** *n*	**Don't know** *n*
Conservative	240	5	5	0	28	1	2
Labour	41	409	22	2	60	2	1
Lib Dem	46	11	138	0	24	0	0
Other	7	3	5	29	5	0	0
Will not vote	17	7	4	2	59	2	10
Refused	0	0	0	0	5	36	1
Don't know	68	21	21	2	72	19	27

Source: ICM

320. This analysis confirms that the choice of the 'don't know' option does not necessarily reflect the hesitancy of a random group of voters of all political persuasions. It reflects instead something that is in the same vein as differential refusal. At a time when there is an anti-Conservative climate of opinion some of their previous supporters fail to resolve the dilemma of whether to stick with them in a new election. Such people are not evading the pollsters or telling lies - they are genuinely unsure. This should cause no surprise. But if, when the phenomenon recurs during a general election campaign, these unsure voters are in fact - though they may not know it - likely to vote and to revert to their previous loyalty in the polling booth, the polls need to take this into account.

321. One further problem is suggested by the fact that polling companies uncover a relatively small percentage of people who say that they will not vote, typically less than 10% of the productive sample. Part of the reason for this is that some people treat the voting intention question as a party support question, that is to say they have a clear view on which party they support but have no intention of actually going

to vote. It is possible to make some allowance for this factor. (See Appendix 6E). However, even after making allowance for turnout many polls simply contain too many voters. One possibility is that there is some correlation between a willingness to participate in elections and a willingness to give a market research interview. Another is that more doubtful voters decide not to vote on the day. It is also possible that some who feel they 'ought' to vote are reluctant to admit that they do not intend to do so. Finally, informants may become aware that the interview will be about voting intentions and, because they have no intention of voting or have no interest in politics, decline to participate at an early stage.

322. This would not be a problem were it not for the quota system. The effect is that each interviewer will find a quota of voters who, through the quota system, are made representative of the population. But the profile of all voters is not the same as the profile of all adults aged 18+. Certain groups of the population are unlikely to vote, especially young people, people in inner city areas, and ethnic minorities. For the polls to be accurate, it is necessary that the correct proportion of these non-voters should be interviewed and recorded as such - if they are simply replaced by some of the minority of demographically similar people who will vote, the sample will be skewed.

Methodological implications

323. One option with those people who say they don't know who they would vote for in an immediate general election is to re-allocate either all of them or some of them back to the party they say they voted for in the previous general election. Throughout 1994 the effect of re-allocating 'don't knows' in this way has been to reduce Labour's lead over the Conservatives.

324. ICM has re-analysed its post-1992 recall survey and found that 60% of those who said they did not know who they would vote for on the eve of the election actually turned out for the party they voted for in 1987. The remaining 40% either did not vote or, where they did, the changes in their votes were compensated for by equal numbers moving in the opposite direction. The same conclusion has been reached through analysis of the BES survey. Table 48 compares the effects of re-allocating all don't knows and re-allocating 60% of them on ICM's *Guardian* survey for which fieldwork was on 20-21 May 1994

Table 48: Recalibration of don't knows in ICM's May 1994 poll

	Raw poll figures %	All 'don't knows' reallocated %	60% of 'don't knows' re-allocated %
Conservative	25	28	27
Labour	49	47	47
Liberal Democrat	23	22	23
Other	3	3	3
Con lead	-24	-19	-20

Source: ICM/Guardian poll, May 1994

325. This analysis suggests that the estimation of actual party share in any election should not be limited to simple exclusion of the 'don't knows'. One solution would be to reduce the number of don't knows - one attempt to achieve this, using a secret ballot, is discussed below (paragraph 334). The alternative for pollsters is to find a means of estimating how the don't knows might vote, and the evidence is that more sophisticated techniques are required than are used at present. It is dangerous to rely on the assumption that 'don't knows don't vote', or that those who will are normally distributed by party allegiance.

8.3. Questionnaire details

326. How the questions are asked can make a significant difference to the answers. But it is not easy to determine which is the right way to ask them. Quite apart from deliberate lying or refusal (overt or covert), it is well established that different questionnaire design can affect respondents' answers. Question wording, question order, and questions asked before voting intention are all potential sources of difference between the polls. It is, indeed, a factor which attracted attention during the period of the 1987-92 parliament[92]. There were some particularly acute differences between the companies at the time of the formation of the SLD. However, except in such special circumstances when there is a real chance of misunderstanding or confusion in the answers, the most notable effect has usually been caused by previous questions ('position bias') which remind respondents of the existence of the centre parties. This is possibly of limited relevance during an election campaign when the profile of the centre parties is relatively high.

327. As Table 3 shows, the variation in results between polling companies was far too small in 1992 for there to be any detectable effect of variations in question wording, question order or other details of

[92] See in particular Robert M. Worcester in British Public Opinion Newsletter, April 1988 (Volume X, No 3), p 3.

methodology. If questionnaire design played any role in biassing poll results, the effect must have operated equally regardless of the companies' variations of detail. In fact there seems no reason to suspect that any such effect occurred.

328. There were minor differences in question wording between the companies in 1992, but there seems no reason to suppose they were significant.

Table 49: Question wording on final polls, 1992

Harris	"For which party do you intend to vote in the forthcoming General Election?" ("Which party are you most inclined to support?")
Gallup	"How likely is it that you will go and vote in the General Election?" "If you vote, which party would you support?" ("Which party would you be most inclined to vote for?)"
ICM	"In the general election tomorrow, which party will you vote for?" ("Which party will you be most likely to vote for?")
MORI	"How do you intend to vote at the General Election on April 9?" ("Which party are you most inclined to support?")
NOP	"What party will you vote for in the General Election?" ("Which party would you be most inclined to support?")

The two whose results diverged from each other most were Gallup and MORI. The difference in question wording could conceivably have made a difference, Gallup's making the possibility of abstention more explicit, but it seems unlikely.

329. Unlike the other companies, who asked voting intention as their first question, Gallup asked three questions including a 'best Prime Minister' question before voting intention. Since Neil Kinnock tended to score worse than Paddy Ashdown and John Major, it would not be unexpected if the result were to tilt the voting intention question against Labour (although even if it were proved to do so, this would still leave open the question of which method produced most realistic results). This 'warm start' as opposed to the standard 'cold start' seems the most likely explanation if any significant discrepancy between the companies' results can be detected. There seems little doubt that the use of a 'warm start', even with questions that ought to be comparatively innocuous in terms of biasing subsequent answers, can have a significant effect on answers to the voting intention question (see below).

330. MORI conducted experiments in 1993 with matched samples to test the effects of preceding the voting intention question with other political questions (see Appendix 6B), and discovered that there is a significant and indeed dramatic effect. When the voting intention question is immediately preceded by either of two other political questions (general party identification or recall of vote at the 1992 election) Labour's share of the vote rose five points. At the very least, it seems fair to conclude that adopting either of the question orders with which MORI experimented would probably not have solved the problems of

the polls in the 1992 election. Of course, the party effects of such methodological variations are not necessarily constant: they may vary with the political climate. It is quite possible that the warm start favours the leading party in general, rather than Labour in particular. Further experiments at different stages of the political cycle will elucidate this point further.

331. More generally, however, the experiment emphasises how vulnerable the standard voting intention question can be to distortion, and makes it clear how potentially perilous any form of warm start technique might be (even if its apparent effect is to nudge the voting figures in the right, rather than the wrong, direction). Nevertheless, this is by no means to conclude that the cold start method is perfect or even necessarily that it is theoretically preferable.

332. The only other significant methodological difference between the polls was that MORI (together with ICM and NOP in some polls) used filter questions intended to weed out those not registered to vote in the appropriate constituency. This, if it had an effect at all, should, logically, depress the Labour vote; in fact, MORI and NOP had the highest Labour ratings of the five companies. Clearly this difference can be discounted as insignificant.

333. In any case, it is clear that there were only very small differences between the results produced by the different companies (see Table 3). It would be hard to make out a case for a substantial question wording effect, or for any other effect depending on differences in company methodology.

8.4 Secret ballot technique

334. One possible explanation for the failure of the polls in 1992 was that Conservative supporters were more likely to decline to be interviewed, and more likely to refuse to give their voting intentions in the interview or nominate the 'don't know' option as a polite refusal. If so, the privacy offered by a secret ballot method may be a way of reducing the possible distortions caused by the reluctance of some Tories to state their party preference openly. The use of similar techniques is well established in other forms of market and social research to reduce the reluctance of some people to answer sensitive questions. The use of secret ballots for exit polls is also well established. ITN and Harris have used them for exit polls since 1974. More recently, tests by NOP showed that refusal rates were reduced by 6% as a result of using a secret ballot to record voting behaviour instead of a simple verbal question. The same reduction of 6% in refusals to the voting intention questions was obtained by ICM in a split sample test conducted in September 1992. Further, the table below also shows the Conservatives 7% higher in the secret ballot.

Table 50: ICM split-sample comparison of secret ballot and conventional interviewing, September 1992

	Conventional poll %	Secret ballot %	Difference %
Conservative	28	35	+7
Labour	33	33	-
Liberal Democrat	12	12	-
SNP	3	2	-1
Others	1	3	+2
Will not vote	12	11	-1
Refused	7	1	-6
Don't know	5	4	-1

335. Tests by NOP and MORI have not produced support for the secret ballot. But following their tests, ICM introduced a secret ballot for their regular monthly polls conducted on behalf of the *Guardian*. The analysis which follows covers the period between September 1992 (when ICM introduced the secret ballot) and April 1994. In Table 51 all 49 conventional polls conducted during the period[93] have been added together to produce an average estimate of party vote shares. In the same period 18 secret ballot polls have been published by ICM. Over the whole period the secret ballot polls produced results that were, on average 3% more favourable to the Conservatives than the conventional polls. However, when the whole 18 month period is split into two equal segments of 9 months each it is apparent that the pro-Tory effect of the secret ballot was evident in the earlier period but not in the latter. In the most recent 9 month period the secret ballot has produced results that are identical to the conventional polls.

[93] Except that all polls conducted in December 1993 and January 1994 have been excluded as in these two months ICM added other experiments to their polls in addition to the secret ballot.

Table 51: Comparison of secret ballot and conventional interviewing, 1992-3

		Con	Lab	LDem	C Lead
September 1992-April 1994					
Verbal (49 polls)	%	30	46	22	-16
ICM Secret ballot (18)	%	31	44	20	-13
September 1992-May 1993					
Verbal (25)	%	33	46	16	-13
ICM Secret ballot (9)	%	35	44	16	-9
June 1993-April 1994					
Verbal (24)	%	27	45	23	-18
ICM Secret ballot (9)	%	27	45	23	-18

Based on all published polls, September 1992 to April 1994 except those December 1993-January 1994.

336. It is possible that the effects of the secret ballot will vary over the whole course of the political cycle and that the circumstances which surrounded the 1992 election do not now apply. Obviously a much longer time series is required before any firm conclusions can be drawn.

9. AFTERWORD: MODELLING BEHAVIOUR FROM INTENTION

337. Polling is a complex operation. There are many points at which it is possible to make mistakes in their conduct or to misinterpret their findings. We have endeavoured to sift through these to see which was most responsible for the errors of 1992. The conclusions which follow from our exploration are laid out on the preceding pages. They do not offer a single headline answer. Several factors contributed minimally to the pollsters' discomfiture but three - differential refusals, inadequately rigorous quotas and late swing - seem to have been the main culprits.

338. A basic difficulty of all survey research arises when respondents do not behave as they say they intend to, particularly when voting forecasts are at stake with their high requirements of precision. The parallel with market research into new product development is instructive. It is well known that an estimate of purchase volume based on stated intention to buy can be either very good (in the case of major purchases such as durables or cars) or very bad. It can also vary from product to product depending on the amount of competitive clutter in the retail outlets. Does this problem apply to voting polls?

339. In Britain, intention has in the past been a good predictor of behaviour. Perhaps things have changed so that this is no longer the case, or maybe the closeness of the race in 1992 created exceptional circumstances. In some other countries it is well known that stated intention will never be a good predictor. Major adjustments have to be made. In France, for example, as we have seen (paragraph 146 *et seq*) this involves weighting for claimed behaviour at previous elections and other adjustments to overcome anticipated underclaiming of behaviour that might be deemed to be socially undesirable. If such a problem has arisen in Britain, pollsters will need to abandon simple reporting of voting intention totals, and develop models by which actual voting can be more accurately be predicted from the data. This, of course, would involve considerable research; it would be most unwise to introduce such techniques untried or without full recognition of the pitfalls involved.

340. Of course, such implications have considerable drawbacks for the simple reporting of poll research. Media clients may balk at complex interpretations which do not lend themselves to succinct headlines and unequivocal conclusions. Nevertheless, it could well be that the most responsible way to use the polls in future will be to extrapolate from the raw data to more sophisticated calculations which carry a degree of extra risk and need to bear a 'health warning' as well as considerable explanation for the interested viewer or reader. In the long term this may be a development that the polling organisations - and their clients - will have to consider.

341. Pollsters' reputations depend on the way in which their findings are used. The media and the public seem to expect them to be able to predict election results to a degree of accuracy far more stringent than that applied to other market research, and indeed more stringent than is realistic. Pollsters must, of course,

do all that they can to ensure that their findings are as accurate as possible, and certainly should be more accurate than they were in 1992; we believe that the recommendations of this report are a step towards achieving that. But pollsters must also insist on the limitations of their craft being understood and accepted. As our report has shown, there is a lot yet to be learned and there may be many improvements and refinements which can yet be made. The pollsters and, indeed, all market researchers should be exploring this. Nevertheless, the fact remains that - disappointing as it may be to some - polls will never be perfect. To be able to ensure that they remain within the standard of margins of error is a success in itself. If the pollsters can do this in the future, they can be well pleased with themselves.

POSTSCRIPT: THE 1994 EUROPEAN ELECTIONS

342. The European Parliament elections of 9 June 1994 took place after this report had been prepared. However, they did provide a test of some of the techniques discussed in the previous pages. The crude figures from many of the surveys suggested the existence of a pro-Labour bias similar to that in 1992. But the application of the lessons about weighting, differential refusals and secret ballot techniques set out above enabled some at least of the forecasts to come close to the outcome.

343. The European elections were somewhat different from a general election. The turnout (36%) was less than half that in a general election, and therefore presented the pollsters with very different problems. There were many fewer polls conducted during the campaign, and only one (by ICM) was conducted wholly within four days before the election. A further difficulty may be that the result of the 1994 European election was never in any real doubt; thus, if the apparent closeness of the 1992 contest affected electors' behaviour or their responses to interviewers, this factor was largely absent in 1994.

344. There were four surveys carried out within the last nine days of the campaign, although none could properly be considered to be an 'eve-of-poll prediction'. Of the four, only one - ICM's survey on 6 June for the *Daily Express* - used a broadly similar method to the polls conducted in the 1992 general election campaign (although with the addition of a secret ballot and of more extensive use of quota, demographic and consumption weighting variables). Both MORI (on 2-6 June for *The Times*), Gallup (on 1-7 June, unpublished[94]) asked their questions on omnibus surveys conducted in-home over several days, and the NOP poll, a private poll for the Labour party on the weekend of 4-5 June which was leaked to the media, was by telephone, neither being methods used by the major pollsters during the 1992 campaign. Table 52 shows the unadjusted voting intention findings for each survey[95].

345. NOP's telephone poll produced a result that was identical to the nearest percentage point with the final percentages for both Conservatives and Labour, while ICM was 1% out in each case. Both MORI and Gallup reported vote shares that turned out to be higher than the final Labour vote and lower than the final Conservative vote. All the polls correctly identified regional variations in support for the parties. However, none of the polls indicated a strong showing by the minor parties or the effect that this would have on the Liberal Democrat share. (This may well be an effect of late swing, of which part at least might consist of protest voters being reminded of the existence of "other" parties by the ballot papers).

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[94] The Gallup figures are an aggregation of voting intentions from three separate overlapping surveys over the period, since Gallup did not publish the figures from any single one of the surveys.

[95] Figures given for the NOP poll are based on "certain to vote", since those would have been the figures that NOP would have used if they had published the poll; they differ from the figures leaked to the media, which were based on all respondents.

Table 52: Polls during the 1994 European election campaign - unadjusted figures

	Fieldwork		Con	Lab	LDem	Oth	C lead	Sample
ICM/*D Express*	17 May	%	27	46	22	5	-19	1,019
ICM/*Guardian*	20-21 May	%	26	47	23	5	-21	1,420
MORI/*Times*	19-23 May	%	27	47	22	4	-20	1,929
Gallup/*D Telegraph*	25-30 May	%	23	53.5	19	4.5	-30.5	1,042
MORI/*Times*	2-6 June	%	23	51	20	6	-28	2,669
NOP/Labour party	4-5 June	%	28	44	19.5	8.5	-16	c. 1,000
ICM/*D Express*	6 June	%	27	45	22	6	-18	1,050
Gallup/unpublished	1-7 June	%	24.5	49.5	18	8	-25	2,939
Result	**9 June**	**%**	**27.8**	**44.2**	**16.7**	**11.2**	**-16.4**	

346. The election offers the opportunity to test the suggested methods of adjusting raw survey figures from the final polls[96]. These are shown in Table 53: Gallup adjusted by weighting for reported past vote, MORI by including the reported past vote of the 'don't knows', and ICM by using both techniques.

Table 53: Adjusted figures in the final polls, 1994 European election

	Fieldwork		Con	Lab	LDem	Oth	C Lead	Sample
MORI/*Times*	2-6 June	%	27	47	20	6	-20	2,669
ICM/*D Express*	6 June	%	29	43	20	8	-14	1,050
Gallup/unpublished	1-7 June	%	28	44	19.5	8.5	-16	2,939
Result	**9 June**	**%**	**27.8**	**44.2**	**16.7**	**11.2**	**-16.4**	

347. The results suggest that, at the moment at least, these techniques are effective in coping with some of the difficulties caused in 1992. The effects of the adjustments were as follows:

a. ICM's unadjusted figures in Table 52 show a bias to Labour of 2% in the estimation of their lead over the Conservatives. Weighting by past voting and re-allocating a proportion of the don't knows produces an error of equal size in favour of the Conservatives.

b. The raw Gallup figures show an average 8.6% over-estimate of Labour's lead, but weighting by reported past vote achieves exactly the correct percentages for both major parties.

c. The unadjusted MORI figures show a 11.6% over-estimate of the Labour lead, but re-allocation of refusers and 'don't knows' by reported past vote reduced the error to a 3.6% overestimate.

All three polls failed to accurately identify the high level of support for minor parties.

[96] ICM and MORI's published 'headline' figures were in both cases the unadjusted figures.

348. One clear lesson reiterated by the 1994 European election polls appears to be that electors are very poor predictors of whether they will vote, at least in European elections. In ICM's poll 55% said they were "certain to vote", in MORI's the percentage was 45% and in Gallup's 54%; in fact the turnout was only 36%. In their post-election survey ICM also found considerable "overclaiming" - a much higher proportion of respondents claimed to have voted than the actual turnout.

349. Two post-election surveys conducted and published before the votes were counted took advantage of the unique opportunity offered by a European Election (which has a three-day hiatus between voting and counting of the votes) to test their methodology against an objective standard - the actual result - by asking how voters had voted before the result had been announced. Being surveys of those who had voted, they were not vulnerable to differential turnout in the same way as the pre-election surveys; nor, with the result unknown, was there any danger of inaccurate answers being caused either by a bandwagon effect or by a new and different political atmosphere having developed following the results.

Table 54: Post election polls in the 1994 European election

	Fieldwork		Con	Lab	LDem	Oth	Sample
ICM/*S Express*	10-11 June	%	25	48	19	7	2,954
Unadjusted figures		%	26.7	45.3	19.6	8.4	
Published (adjusted) prediction		%	27.8	44.2	16.7	11.2	
Result	**9 June**						
MORI/ERS Exit Poll (London)	9 June	%	29	51	13	7	3,893
Result (London)	**9 June**	**%**	**29.8**	**50.3**	**12.1**	**7.8**	

350. These two surveys, one a quota survey of the general public and one an exit poll, offered encouraging findings. An ICM survey published in the *Sunday Express* was based on an in-street quota methodology similar to that used by the polling companies in 1992, but also using a secret ballot and more extensive weighting by demographic and consumption data. The unadjusted figures show a bias to Labour of equivalent size and direction to that in the polls in the 1992 general election. Adjusting the figures by re-allocating 'don't knows' and - more especially - weighting by past voting brought ICM's figures to within 1.1% of the vote shares of both the Conservatives and Labour. The survey seems to confirm that quota polls are unlikely *at present* to achieve accurate unadjusted figures, and offers considerable support to the techniques used by ICM (with which they have experimented for some months in reporting their regular surveys for the *Guardian*.)

351. A MORI (random) secret ballot exit poll in London for the Electoral Reform Society, adjusted only by weighting to age and gender to compensate for the known profile of refusers, was also very close to the true result. As with the ICM survey, the implication is that the problem of differential refusal to participate can be overcome.

352. The experience of the 1994 European Elections demonstrates that the problems manifested by the polls at the time of the 1992 election still persist. But it also offers encouraging evidence that the experiments along the lines outlined in this report may go a long way towards overcoming these problems.

Appendix 1: PUBLISHED NATIONAL OPINION POLLS IN THE 1992 ELECTION

Fieldwork	*Company*	*Source*	*Sample*	*Con*	*Lab*	*LDem*	*C Lead*
11 Mar	NOP	*Mail on S*	1,050	41	40	15	1
11-12 Mar	MORI	*Times*	1,054	38	41	16	-3
11-12 Mar	MORI	*S.Times* (P1)	1,544	40	39	18	1
11-13 Mar	Harris	*Observer*	1,054	40	43	12	-3
11-13 Mar	Harris	*D.Express*	1,086	39	40	16	-1
11-13 Mar	Harris	LWT	2,186	37	41	17	-4
12-13 Mar	NOP	*Ind.on S.* (P1)	2,155	40	41	14	-1
13 Mar	ICM	*S.Express*	1,059	39	40	16	-1
15-16 Mar	Harris	*D.Express*	1,081	41	38	17	3
16 Mar	MORI	*Times*	1,099	38	43	16	-5
17 Mar	ICM	*Guardian*	1,100	38	43	16	-5
17-18 Mar	Gallup	*D.Telegraph*	984	40.5	38.5	18	2
17-18 Mar	NOP	*Independent*	1,262	38	42	17	-4
18-20 Mar	MORI	*S.Times* (P2)	1,257	38	41	19	-3
19-20 Mar	Harris	*Observer*	1,096	40	39	17	1
20 Mar	ICM	*S.Express*	1,115	37	42	16	-5
19-21 Mar	NOP	*Ind.on S.* (P2T)	1,004	39	41	15	-2
20-21 Mar	NOP	*Mail on S*	1,085	38	40	16	-2
21-23 Mar	Harris	*D.Express*	1,000	43	38	15	5
22-23 Mar	Harris	ITN	2,158	38	42	16	-4
23 Mar	MORI	*Times*	1,109	38	41	17	-3
23-24 Mar	Gallup	*D.Telegraph*	1,092	40	40.5	16.5	-0.5
24 Mar	ICM	*Guardian*	1,096	39	40	17	-1
24 Mar	NMR	*European*	1,105	38	39	19	-1
24-25 Mar	NOP	*Independent*	1,326	39	42	14	-3
25-27 Mar	MORI	*S.Times* (P3)	1,292	38	40	20	-2
26-27 Mar	Harris	*Observer*	1,057	40	38	17	2
27 Mar	ICM	*S.Express*	1,136	36	38	20	-2
27-28 Mar	NOP	*Mail on S*	1,099	37	41	18	-4
26-29 Mar	NOP	*Ind.on S.* (P3T)	1,000	39	40	16	-1
28-30 Mar	Harris	*D.Express*	1,108	40	39	17	1
29-30 Mar	Harris	ITN	2,152	35	41	19	-6
30 Mar	MORI	*Times*	1,080	35	42	19	-7
31 Mar	ICM	*Guardian*	1,126	37	41	18	-4
31 Mar-1 Apr	Gallup	*D.Telegraph*	1,095	38	37.5	20.5	0.5
31 Mar-1 Apr	NOP	*Independent*	1,302	37	39	19	-2
31 Mar-3 Apr	ICM	Press Assn	10,460	36	39	20	-3
1-3 Apr	MORI	*S.Times* (P4)	1,265	37	39	21	-2
2-3 Apr	Gallup	*S.Telegraph*	1,043	37.5	37.5	22	0
2-3 Apr	NOP	*Ind.on S.* (P4T)	1,006	38	41	17	-3
3 Apr	ICM	*S.Express*	1,139	37	39	18	-2
3-4 Apr	NOP	*Mail on S*	1,104	35	41	20	-6
3-4 Apr	Harris	*Observer*	1,090	38	40	17	-2
4-6 Apr	Harris	*D.Express*	1,093	37	38	21	-1
4-7 Apr	Harris	ITN	2,210	38	40	18	-2
6-7 Apr	MORI	YTV	1,065	37	40	20	-3
7-8 Apr	MORI	*Times*	1,731	38	39	20	-1
7-8 Apr	NOP	*Independent*	1,746	39	42	17	-3
7-8 Apr	Gallup	*D.Telegraph*	2,478	38.5	38	20	0.5
8 Apr	ICM	*Guardian*	2,186	38	38	20	0

Source: *Times Guide to the House of Commons, 1992*

Appendix 2: SCOTTISH POLLS

The problems of opinion polls at the 1992 General Election were not confined to the major surveys representative of the whole British electorate. Ten polls specifically designed to measure public opinion in Scotland were carried out between the start of the campaign and polling day. Like their counterparts polling across Great Britain, they all measured a level of support for Labour substantially higher than that eventually recorded in the ballot box and underestimated Conservative support. They also overstated support for SNP and underestimated the Liberal Democrat share. In fact, each of the ten polls placed the Conservative party in third place behind the SNP. The Conservatives eventually polled 25.6% of the votes, more than four percentage points ahead of the SNP.

Table 55: Scottish Polls in the 1992 election

Fieldwork	Pollster	Sample size		Con	Lab	LDem	SNP	Oth	Con lead over Lab
12/3	MORI	1,054	%	23	42	11	24	0	-19
19/3	MORI	1,009	%	20	43	9	27	1	-23
19/3-24/3	System 3	1,036	%	22	44	7	26	1	-22
22-23/3	ICM	1,052	%	22	41	9	27	1	-19
26/3	MORI	1,059	%	20	42	10	27	1	-22
28-30/3	MR Scotland	1,079	%	21	38	10	31	0	-17
	Pre April average		%	21.3	41.7	9.3	27.0	0.7	-20.3
2/4	MORI	1,060	%	21	44	12	23	0	-23
2-3/4	MR Scotland	1,133	%	18	43	11	27	1	-25
4-5/4	System 3	1,087	%	21	40	12	25	2	-19
5-6/4	ICM	1,056	%	22	41	11	25	1	-19
	April average		%	20.5	42.0	11.5	25.0	1.0	-21.5
9/4	**Result**		%	**25.6**	**39**	**13**	**21.5**	**0.8**	**-13.4**
	Difference		%	*-5.1*	*+3.0*	*-1.5*	*+3.5*	*+0.2*	*-8.1*

During the campaign the polls suggested static support for both Conservative and Labour parties, with indications of declining support for the SNP and slight gains for the Liberal Democrats.

Four polling companies were involved in Scottish polls. System Three and Market Research Scotland are Scottish companies with no involvement in national polls, MORI and ICM both conduct polls across Great Britain as well as special Scottish polls. It is clear that all four companies produced broadly similar results during the campaign.

Table 56 compares the difference between poll findings and the election result for each company. Market Research Scotland were a little out of line with the other three. Their results produced consistently higher support for the SNP and lower support for the Conservatives. With this exception, the poll results were

remarkably consistent and, like their national counterparts, contain the same under-estimate of Conservative support and overstatement of Labour share.

Table 56: Scottish Polls: Summary of difference from final result, by Pollster

		Con	Lab	LDem	SNP	Oth	Con lead
ICM							
Final Poll	%	-3.6	2.0	-2.0	3.5	0.2	-5.6
Average of polls	%	-3.6	2.0	-3.0	4.5	0.2	-5.6
MORI							
Final Poll	%	-4.6	5.0	-1.0	1.5	-0.8	-9.6
Average of polls	%	-4.6	3.3	-3.0	4.5	-0.1	-7.9
MR Scotland							
Final Poll	%	-7.6	4.0	-2.0	5.5	0.2	-11.6
Average of polls	%	-6.1	1.5	-2.5	8.5	-0.3	-7.6
System 3							
Final Poll	%	-4.6	1.0	-1.0	3.5	1.2	-5.6
Average of polls	%	-4.6	3.0	-3.5	4.0	0.7	-7.1

It is not surprising that the four companies polling in Scotland produced similar results with the same measurement problems seen in the national polls. The methodology used by all four polling companies was broadly the same and similar to that employed by the national polls. All four pollsters used face to face interviews with samples controlled by quotas on main demographic characteristics. Age, sex and social class were common to all four while three also included working status in the quota controls.

Samples sizes were all just over 1000 interviews selected from a number of parliamentary constituencies ranging from 41 (S3) through 52 (ICM) and 53 (MORI) to 66 (MR Scotland). No panel surveys were conducted during the campaign and no evidence has been presented based on post election recalls on Scottish electors interviewed in the pre-election polls.

Given the similarity of the Scottish results to the national picture, it is not unreasonable to suspect that the factors which affected the national polls were also at work in Scotland.

Appendix 3: THE HISTORICAL RECORD OF THE FINAL POLLS

Table 57: Final polls and election results 1945-87

	Sample		Con	Lab	Lib	Oth	C lead	Lead error	Average error	Max error	Method
1945											
Gallup (24-27 Jun)		%	41.0	47.0	10.5	1.5	-6.0	+3.5	1.5	1.8	Q
Result (5 Jul)		%	**39.3**	**48.8**	**9.2**	**2.7**	**-9.5**				
1950											
Daily Mail (5-11 Feb)		%	45.5	42.5	n/a	12.0*	+3.0	+6.8	2.9	4.3	n/a
Daily Express (17-21 Feb)		%	45.0	44.0	10.0	1.0	+1.0	+4.8	1.4	2.8	n/a
Gallup (17-20 Feb)		%	43.5	45.0	10.5	1.0	-1.5	+2.3	0.9	1.8	Q
Result (23 Feb)		%	**43.0**	**46.8**	**9.3**	**0.9**	**-3.8**				

* Mail poll combined Liberals and others

	Sample		Con	Lab	Lib	Oth	C lead	Lead error	Average error	Max error	Method
1951											
RSL (n/a)		%	50.0	43.0	n/a	7.0*	+7.0	+7.8	3.9	5.8	
Daily Express (19-23 Oct)		%	50.0	46.0	3.5	0.5	+4.0	+4.8	1.5	2.8	n/a
Gallup (22 Oct)		%	49.5	47.0	3.5	0.5	+2.5	+3.3	1.1	1.8	Q
Result (25 Oct)		%	**48.0**	**48.8**	**2.5**	**0.7**	**-0.8**				

* RSL poll combined Liberals and others

	Sample		Con	Lab	Lib	Oth	C lead	Lead error	Average error	Max error	Method
1955											
Daily Express (n/a)		%	50.2	47.2	2.2	0.4	+3.0	-0.3	0.65	0.8	n/a
Gallup (21-24 May)		%	51.0	47.5	1.5	0.0	+3.5	+0.2	1.2	1.3	Q
Result (26 May)		%	**49.7**	**46.4**	**2.7**	**1.2**	**+3.3**				
1959											
NOP (2-5 Oct)		%	48.0	44.1	n/a	7.9*	+3.9	-0.3	0.8	0.8	Q
F Stat (1-4 Oct)		%	48.6	45.7	5.1	0.6	+2.9	-1.3	0.6	1.0	RM
Daily Express (n/a)		%	49.1	45.4	5.0	0.5	+3.7	-0.5	0.6	1.1	n/a
Gallup (3-6 Oct)		%	48.5	46.5	4.5	0.5	+2.0	-2.2	1.0	1.9	Q
Result (8 Oct)		%	**48.8**	**44.6**	**6.1**	**0.6**	**+4.2**				

* NOP poll combined Liberals and others

	Sample		Con	Lab	Lib	Oth	C lead	Lead error	Average error	Max error	Method
1964											
RSL (n/a)	n/a	%	45.0	46.0	n/a	9.0*	-1.0	+0.9	1.65	2.4	n/a
NOP (9-13 Oct)	1,179	%	44.3	47.4	7.9	0.4	-3.1	-1.2	2.0	3.5	RP
Gallup (8-13 Oct)	3,829	%	44.5	46.5	8.5	0.5	-2.0	-0.1	1.6	2.9	Q+R
Daily Express (n/a)	n/a	%	44.5	43.7	11.0	0.8	+0.8	+2.7	0.8	1.6	n/a
Result (15 Oct)		%	**42.9**	**44.8**	**11.4**	**0.9**	**-1.9**				

* RSL poll combined Liberals and others

	Sample		Con	Lab	Lib	Oth	C lead	Lead error	Average error	Max error	Method
1966											
RSL (n/a)	n/a	%	41.6	49.7	8.3	0.4	-8.1	-0.8	0.8	1.2	n/a
NOP (27-29 Mar)	1,693	%	37.4	54.1	7.5	1.0	-16.7	-9.4	2.7	5.4	Rr
Daily Express (n/a)	n/a	%	41.6	50.6	7.4	0.4	-9.0	-1.7	1.4	1.9	n/a
Gallup (24-28 Mar)	3,596	%	40.0	51.0	8.0	1.0	-11.0	-3.7	1.2	2.3	Q+R
Result (31 Mar)		%	**41.4**	**48.7**	**8.8**	**1.1**	**-7.3**				

	Sample		Con	Lab	Lib	Oth	C lead	Lead error	Average error	Max error	Method
1970											
ORC (13-17 Jun)	1,583	%	46.5	45.5	6.5	1.5	+1.0	-1.4	1.0	1.7	Q
Harris (n/a)	2,661	%	46.0	48.0	5.0	1.0	-2.0	-4.4	2.1	4.2	R
NOP (12-16 Jun)	1,562	%	44.1	48.2	6.4	1.3	-4.1	-6.5	2.2	4.4	R
Gallup (14-16 Jun)	2,190	%	42.0	49.0	7.5	1.5	-7.0	-9.4	2.6	5.2	Q
Marplan (11-14 Jun)	2,267	%	41.5	50.2	7.0	1.3	-8.7	-11.1	3.2	6.4	R
Result (18 Jun)		%	**46.2**	**43.8**	**7.6**	**2.4**	**+2.4**				
February 1974											
Bus Dec (21 Feb)	1,056	%	36.0	37.5	23.0	3.5	-1.5	-2.3	1.7	3.2	Q
ORC (n/a)	2,327	%	39.7	36.7	21.2	2.4	+3.0	+2.2	1.2	1.4	Qr
Harris (26-27 Feb)	3,193	%	40.2	35.2	22.0	2.6	+5.0	+4.2	1.8	2.8	Rr
NOP (23-27 Feb)	4,038	%	39.5	35.5	22.0	3.0	+4.0	+3.2	1.5	2.5	Rr
Gallup (26-27 Feb)	1,881	%	39.5	37.5	20.5	2.5	+2.0	+1.2	0.7	0.9	Qr
Marplan (n/a)	1,649	%	36.5	34.5	25.0	4.0	+2.0	+1.2	2.9	5.2	Qr
Result (28 Feb)		%	**38.8**	**38.0**	**19.8**	**3.4**	**+0.8**				
October 1974											
Bus Dec (2 Oct)	2,071	%	35.5	40.0	20.0	4.5	-4.5	-1.0	0.7	1.2	Q
NOP (2-5 Oct)	1,978	%	31.0	45.5	19.5	4.0	-14.5	-11.0	3.0	5.7	R
ORC (5-9 Oct)	1,071	%	34.4	41.8	19.4	4.4	-7.4	-3.9	1.2	2.3	Qr
Harris (5-9 Oct)	2,701	%	34.6	43.0	19.3	3.1	-8.4	-4.9	1.6	2.8	Rr
Gallup (3-8 Oct)	954	%	36.0	41.5	19.0	3.5	-5.5	-2.0	0.8	1.3	Qr
Marplan (8 Oct)	1,024	%	38.3	43.8	19.5	3.5	-10.5	-7.0	2.1	3.6	Qr
Result (10 Oct)		%	**36.7**	**40.2**	**18.8**	**4.3**	**-3.5**				
1979											
Gallup (30 Apr-1 May)	2,348	%	43.0	41.0	13.5	2.5	+2.0	-5.2	1.6	3.4	Q
MORI (30 Apr-1 May)	974	%	44.4	38.8	13.5	3.3	+5.6	-1.6	0.6	1.1	Rr
Marplan (1 May)	1,973	%	45.0	38.5	13.5	3.0	+6.5	-0.7	0.5	0.8	Q
NOP (1-2 May)	1,069	%	46.0	39.0	12.5	2.5	+7.0	-0.2	1.2	1.3	Q
MORI (2 May)	1,089	%	45.0	37.0	15.0	3.0	+8.0	+0.8	0.5	0.9	Q
Result (3 May)		%	**44.9**	**37.7**	**14.1**	**3.3**	**+7.2**				
1983											
ASL (7 Jun)	1,100	%	46	23	29	2	+23	+7	2.2	5	T
Harris (7-8 Jun)	567	%	47	25	26	2	+22	+6	1.5	3	Q
Gallup (7-8 Jun)	2,003	%	45.5	26.5	26	2	+19	+3	0.8	1.5	Q
Marplan (8 Jun)	1,335	%	46	26	26	2	+20	+4	1.0	2	Q
NOP (6-7 Jun)	1,040	%	46	28	24	2	+18	+2	1.0	2	Q
MORI (8 Jun)	1,101	%	44	28	26	2	+16	0	0.0	0	Q
Result (9 Jun)		%	**44**	**28**	**26**	**2**	**+16**				

1987	Sample		Con	Lab	Lib	Oth	C lead	Lead error	Average error	Max error	Method
ASL (9 Jun)	1,702	%	43	34	21	2	+9	-2	1	2	T
Harris (10 Jun)	2,122	%	42	35	21	2	+7	-4	1.5	3	Q
Gallup (8-9 Jun)	2,005	%	41	34	23.5	1.5	+7	-4	1.25	2	Q
Marplan (10 Jun)	1,633	%	42	35	21	2	+7	-4	1.5	3	Q
NOP (10 Jun)	1,668	%	42	35	21	2	+7	-4	1.5	3	Q
MORI (9-10 Jun)	1,688	%	44	32	22	2	+12	+1	0.5	1	Q
Result (11 Jun)		%	**43**	**32**	**23**	**2**	**+11**				

Key to methods:
+: Poll combined two separate samples
M: Poll in marginal seats only
P: Panel
Q: Quota sample
r: Reinterview of previously selected sample
R: Random sample
T: Telephone

Source: Robert M Worcester, British Public Opinion; D. Butler and R. Rose/A. King/M. Pinto-Duschinsky/D. Kavanagh, The British General Election of ... 1959, 1964, 1966, 1970, October 1974.

APPENDIX 4: WHY THE CONVENTIONAL SAMPLING ERROR FORMULAE DO NOT APPLY

As outlined in Section 5.2 above, the sampling errors discussed here are derived empirically from study of variations between polls. This is a simple but adequate substitute for the statistical analysis needed to derive estimates from within a single survey.

It should be noted that the sampling errors so derived are slightly greater than would emerge from simplistic application of classical sampling theory formulae. The latter are appropriate only when a survey sample is selected using 'simple random sampling' - an approach seldom used whether in academic social surveys, market research or political polling. (It should be emphasised that 'random' sampling is not haphazard - a random sample is one that is systematically drawn to ensure that each member of the population being sampled has an equal probability of selection.)

In surveys of the British public, even the most carefully drawn probability samples are not truly 'random'. They start by drawing a sample of constituencies, local authority districts or other area samples (the 'first stage') and then, within those, a sample of households or individuals. If households are chosen (the 'second stage'), then a random process of selection must be used to designate the individuals to be surveyed. Consequently, no survey of the public is a 'pure' random sample; all are to a greater or lesser degree 'clustered'.

Polls also deviate from simple random sampling procedures, in two important respects. First, they almost invariably use the non-random sampling process called quota sampling, where interviewers are allowed to select respondents who meet certain demographic and socio-economic targets. This will not necessarily have any impact on sampling variability; its potential effect is more likely to be to introduce sampling *bias*.

Second, poll samples tend to be selected in two stages: at the first stage, a sample of geographical areas is selected; subsequently, respondents are selected only within these areas. The procedure (multi-stage sampling) is perfectly sound and is well justified by the time and resources it saves, enabling polling at speed. It does, however, usually result in greater sampling variability than would be achieved with a simple random sample of the same size. The effect - known as the Design Effect - is outlined for example in Worcester and Downham[97] and dealt with in detail in more advanced texts on survey sampling.

[97] R. Worcester and J. Downham (eds), Consumer Market Research Handbook (3rd edition, London: McGraw Hill, 1986), Chapter 4.

Appendix 5:
Detailed evidence on the 1992 election

Appendix 5A: Evidence of MORI panels

Table 58: Percentages of the electorate switching during the campaign 1979-87

1979 MORI/*SUNDAY TIMES* PANEL

Final wave before election %	Initial wave (Start of campaign)				
	Con	Lab	Lib	Oth/DK	TOTAL
Con	(39.0)	0.8	0.6	1.3	2.7
Lab	1.6	(37.0)	0.9	2.2	4.7
Lib	1.1	0.6	(6.3)	0.6	2.3
Oth/DK	1.7	0.7	0.4	(5.3)	2.8
TOTAL	4.4	2.1	1.9	4.1	12.5

1983 MORI/*SUNDAY TIMES* PANEL

Final wave before election %	Initial wave (Two weeks before election announced)				
	Con	Lab	SDP/Lib All	Oth/DK	TOTAL
Con	(38.5)	0.7	1.5	1.7	3.9
Lab	0.0	(23.0)	1.5	1.1	2.6
SDP/Lib All	1.4	2.7	(17.6)	1.9	6.0
Oth/DK	1.2	0.7	0.5	(5.4)	2.4
TOTAL	2.6	4.1	3.5	4.7	14.9

1987 MORI/*SUNDAY TIMES* PANEL

Final wave before election %	Initial wave (After election announced)				
	Con	Lab	SDP/Lib All	Oth/DK	TOTAL
Con	(35.7)	0.8	1.8	2.3	4.9
Lab	1.1	(23.8)	2.0	3.3	6.4
SDP/Lib All	1.8	0.9	(17.4)	2.1	4.8
Oth/DK	0.9	0.7	0.8	(4.8)	2.4
TOTAL	3.8	2.4	4.6	7.7	18.5

Table 59: Percentages of the electorate switching during the campaign, 1992
1992 MORI/*SUNDAY TIMES* PANEL

Final wave before election %	Initial wave (After election announced)				
	Con	Lab	LD	Oth/DK	TOTAL
Con	(29.2)	0.8	1.4	3.5	5.7
Lab	1.4	(29.8)	1.2	4.6	7.2
LD	2.6	2.0	(12.9)	2.1	6.7
Oth/DK	0.6	0.6	0.2	(7.1)	1.4
TOTAL	4.6	3.4	2.8	10.2	21.0

Table 60: Percentages of voters switching during final week, 1992
1992 MORI/*SUNDAY TIMES* PANEL
(Base: Those reporting voting)

Recalled vote after election %	Wave IV (1-3 April)				
	Con	Lab	LD	Oth/DK	TOTAL
Con	(34.1)	1.1	1.9	2.3	5.3
Lab	1.2	(34.5)	0.5	1.6	3.3
LD	0.9	2.0	(17.0)	0.6	3.5
Oth	0.1	0.0	0.6	(1.8)	0.7
Refused	0.0	0.1	0.0	(0.2)	0.1
TOTAL	2.2	3.2	3.0	4.5	12.9

Table 61: Percentages of supporters of each party switching, 1992

CHANGE WEEK 1 to WEEK 4 (MORI/*Sunday Times* panel. Base: All answering both waves)

	Wave I party						
	Con n=438 %	Lab n=404 %	LDem n=222 %	Oth n=34 %	Would not vote n=41 %	Un dec-ided n=112 %	Refu sed n=14 %
Wave 4 party							
Conservative	86	3	9	-	10	26	44
Labour	4	89	8	19	8	36	19
Liberal Democrat	8	6	82	16	5	15	-
Other	*	2	-	69	-	1	7
Would not vote	1	-	*	-	74	1	-
Undecided	1	1	*	-	3	19	-
Refused	*	-	*	-	-	3	30

CHANGE WEEK 1 to VOTE RECALL (MORI/*Sunday Times* panel. Base: All answering both waves)

	Wave I party						
	Con n=336 %	Lab n=287 %	LDem n=170 %	Oth n=31 %	Would not vote n=32 %	Un dec-ided n=71 %	Refu sed n=7 %
Reported Vote							
Conservative	87	4	13	-	12	33	37
Labour	4	82	8	26	11	32	32
Liberal Democrat	5	8	73	13	13	21	-
Other	-	2	2	61	2	3	-
Did not vote	3	3	4	9	62	9	31
Refused	*	1	1	-	-	2	-

CHANGE WEEK 4 to VOTE RECALL (MORI/*Sunday Times* panel. Base: All answering both waves)

	Wave IV party						
	Con	Lab	LDem	Oth	Would not vote	Un dec-ided	Refu sed
	n=311	n=275	n=183	n=28	n=21	n=11	n=4
Reported Vote	%	%	%	%	%	%	%
Conservative	92	3	9	4	18	48	58
Labour	3	88	3	32	9	24	-
Liberal Democrat	2	5	82	4	9	-	21
Other	*	-	3	64	-	-	21
Did not vote	2	4	4	-	64	20	-
Refused	-	*	-	-	-	9	-

CHANGE WEEK 4 to VOTE RECALL (NOP/*Independent on Sunday* panel. Base: All voting and answering both waves)

	Wave IV party						
	Con	Lab	LDem	Oth	Would not vote	Un dec-ided	Refu sed
	n=245	n=231	n=101	n=17	n=2	n=18	n=6
Reported Vote	%	%	%	%	%	%	%
Conservative	94	3	13	-	100	28	50
Labour	3	92	3	6	-	33	-
Liberal Democrat	2	4	82	6	-	28	17
Other	*	1	2	88	-	12	34

CHANGE EVE OF POLL to VOTE RECALL (ICM/*Guardian* post-election recall. Base: All answering both waves)

	Wave IV party						
	Con	Lab	LDem	Oth	Would not vote	Don't know	Un-will-ing
	n=433	n=389	n=197	n=51	n=34	n=52	n=47
Reported Vote	%	%	%	%	%	%	%
Conservative	93	2	6	4	5	26	33
Labour	*	92	5	2	-	24	16
Liberal Democrat	2	2	77	11	5	14	8
Other	*	-	3	63	-	8	-
Did not vote	4	4	8	18	88	18	-
Unwilling to say/Don't know	1	1	2	1	2	10	43

Appendix 5B: Panel trends and cross section trends compared

Table 62: Panel studies 1992 - net changes in voting intentions

	Base-line %	Week 1 %	Week 2 %	Week 3 %	Week 4 %	Actual Result %	Reported Vote %
MORI/*Sunday Times* [98]							
Interviewing dates		11-12.3	18-20.3	25-27.3	1-3.4	9.4	10.4
Base		1,544	1,257	1,292	1,265		934
Conservative		40	38	38	37	43	40
Labour		39	41	40	39	35	37
Liberal Democrat		18	19	20	21	18	21
Other		3	2	2	3	4	2
Con lead over Lab		+1	-3	-2	-2	+8	+3
Swing in final week of campaign							+2½%
NOP/*Independent on Sunday*							
Interviewing dates		12-13.3	19-21.3	26-29.3	2-3.4	9.4	10-12.4
Base		2,155	1,004	1,000	1,006		620
Conservative		40	39	39	38	43	42
Labour		41	41	40	41	35	37
Liberal Democrat		14	15	16	17	18	17
Other		5	5	5	4	4	4
Con lead over Lab		-1	-2	-1	-3	+8	+5
Swing in final week of campaign							+4%
MORI/BBC *On The Record* (Panel of floating voters)							
Interviewing dates		5-7.3	12-14.3	19-20.3	26-27.3	2-3.4	10-11.4
Base		2,650	1,072	1,219	1,173	1,146	1,090
Conservative		41	40	37	36	34	37
Labour		32	32	33	32	32	29
Liberal Democrat		22	25	27	30	31	30
Other		5	3	3	2	3	4
Con lead over Lab		+9	+8	+4	+3	+2	+8
Swing in final week of campaign							+3%

Table 63: Cross-section polls conducted contemporaneously with panel studies, 1992

	Week 1 %	Week 2 %	Week 3 %	Week 4 %
Conservative	39	38	38	37
Labour	41	40	39	38
Liberal Democrat	15	16	18	20
Con lead over Lab	-2	-1	-1	-1

(Polls included were 11-13.3: NOP, MORI, Harris/Observer, Harris/Express, Harris/LWT, ICM; 18-21.3 NOP, Harris/Observer, ICM; 25-29.3 NOP, Harris/Observer, ICM; 1-3.4 Gallup, ICM.).

[98] The figures given for the MORI/Sunday Times panel in the table are for those responding both to the recall and to wave IV. However, the recall is slightly suspect because when the recall wave was weighted by wave I voting intentions, its voting intentions on wave IV did not tally with those of the whole panel. (Whereas the voting intention of the whole panel at wave IV was Conservative 37% Labour 39%, the wave IV voting intentions of those interviewed on the recall wave was Conservative 38% Labour 38%.) It may be preferable to measure swing in the last week on the basis of the actual changes of mind of those interviewed, weighted to the composition of the whole panel detected at wave IV; by this method the swing in the last week is reduced to slightly under 2%.

If we compare the mean results of the national face-to-face cross-section polls being carried out at the same time as the nationally representative two panels, we get very similar trends for both Conservatives and Liberal Democrats (although the downward trend for Labour is different).

Although one would generally expect the trends detected by panels to be more reliable than those shown by a small number of cross-section polls, there is one piece of evidence that suggests that possibly the downward Labour trend found by cross sections is more accurate. In MORI's *Sunday Times* panel the rate of attrition was significantly higher among initial Labour supporters than among initial Conservative supporters; consequently the final panels had to be weighted somewhat in favour of the remaining members who had originally supported Labour.

A broadly similar pattern though less pronounced can be observed in the MORI *On the Record* floating voters' panel. It is a reasonable assumption that a high proportion of those who dropped out of the panel were generally uninterested in politics, and that these were the group of Labour supporters most liable to abandon the party. Consequently it may have been the case that the real rate of Labour's decline during the campaign was concealed in the panel by the fact that those who dropped out were disproportionately likely to be switchers; weighting to the original voting strengths cannot compensate for this and may, indeed, make matters worse by overweighting the most solid Labour supporters. Interestingly, no similar effect can be identified in either the 1987 or 1983 MORI/*Sunday Times* panel studies.

Appendix 5C: Anticipated result of the 1992 election

Gallup asked on four occasions during the campaign 'Irrespective of how you yourself will vote, who do you think will win the next General Election?'

	11-17.3 %	18.23.3 %	25-30.3 %	1-6.4 %
Conservative	48	39	35	24
Labour	31	37	37	47
Liberal Democrats	0	0	0	1
Nationalist	0	0	0	0
Hung Parliament	5	14	18	18
Don't know	16	9	9	9

Source: Gallup

In MORI's first two surveys for *The Times* (11-12 March and 16 March) respondents were asked 'What do you think will be the outcome of the forthcoming general election?' and in the opening *Sunday Times* panel survey (12-13 March) 'What do you think will be the outcome of the next general election?'. Responses were:

	Times 11-12.3 %	*Times* 16.3 %	*Sunday Times* 12-13.3 %
Conservative overall majority	30	28	28
Con-Lib Dem power sharing	20	18	22
Lab-Lib Dem power sharing	19	20	21
Labour overall majority	17	19	17
Other	2	3	n/a
Don't know	12	12	12

Source: MORI

In MORI's *Sunday Times* panel in answer to 'What do you think will be the outcome of the next general election?', responses broke down as follows:

	All %	Con %	Lab %	LDem %
Conservative overall majority	28	48	11	17
Con-Lib Dem power sharing	22	30	8	42
Lab-Lib Dem power sharing	21	9	33	27
Labour overall majority	17	3	38	5
Don't know	12	10	10	9

Source: MORI

Appendix 6:
Experimental work since the election

Appendix 6A: MORI spiral of silence experiment

In MORI's January 1993 omnibus, respondents were asked a question on willingness to carry out various political activities for the party they supported, a question which has been used in the past in testing the spiral of silence hypothesis.

Q I'd like to ask you a question about what you think you might do for the party that you most support. From this list which, if any, would you do? Please read out the letter or letters next to all those that you would do.

Put up a poster for the party in my window	28%
Put a party sticker on my car	21%
Go to a party meeting	19%
Wear a party badge	14%
Help distribute campaign literature	11%
Give money to the party's campaign	9%
Take an active part in a discussion at a party meeting	8%
Put up posters in public places for the party	8%
Speak out for my party at another party's meeting	4%
Canvass in person for the party	4%
Canvass by telephone for the party	2%
Stop strangers in the street and discuss the advantages of the party	2%
None of these	43%
Don't know	3%

Judging by the activities that Britons are least reluctant to perform, the reluctance does not arise from classic spiral of silence motives - they are prepared to publicly proclaim their loyalties with posters in their homes, car stickers and badges; they are less keen on activities that involve more commitment to the party even if they are more anonymous. This suggests that the problem may be weakness in commitment to the party rather than inhibitions about supporting unpopular views. Of course, the political climate in January 1993 was different from that in April 1992 and conclusions can only be drawn tentatively, but we would expect that the fall of Conservative popularity in the interim ought to heighten rather than weaken the inhibitions of the party's supporters.

Q I'd like to ask you a question about what you think you might do for the party that you most support. From this list which, if any, would you do? Please read out the letter or letters next to all those that you would do.

	Con	Lab	LibDem
	%	%	%
Put up a poster for the party in my window	25	36	36
Put a party sticker on my car	20	25	27
Go to a party meeting	19	21	23
Give money to the party's campaign	10	12	10
Wear a party badge	10	21	16
Help distribute campaign literature	9	13	18
Put up posters in public places for the party	7	12	8
Take an active part in a discussion at a party meeting	6	9	8
Speak out for my party at another party's meeting	4	5	3
Canvass in person for the party	3	5	5
Stop strangers in the street and discuss the advantages of the party	2	3	1
Canvass by telephone for the party	1	2	5
None of these	43	35	37
Don't know	3	2	2

In fact, as can be seen from the party breakdown, although Conservatives were more reluctant than supporters of other parties to indulge in any of these activities, the same pattern is plain, that public affirmations of party support such as displaying posters are substantially more popular than more useful and less public commitments involving more time and effort. This evidence seems further to confirm that the Conservatives' problem has been disillusionment and lack of enthusiasm among their supporters rather than a shame factor.

Appendix 6B: MORI's question order experiments

In July 1993 MORI carried out a warm start experiment using a question on party identification before the voting intention question, standard practice in the USA but not in Britain. Its purpose was two-fold, to explore the effect of this question order and to offer evidence as to whether party identification ought to be considered for weighting purposes in future polls. This was then followed up in August with a second experiment in which the voting intention questions were preceded by a question on how the respondent voted in the 1992 general election.

Party Identification before Voting Intention

The July 1993 regular MORI omnibus (fieldwork 22-26 July, n= 2,578 in three matched versions) tested the effect of asking the Michigan party identification (ID) question before the standard voting intention (VI) questions. Versions 1 and 2 of the omnibus were of the standard form, with the VI questions asked first and the ID question and a 1992 voting recall question (VR) at the *end* of the political section (at Qq. 12 and 13); Version 3 asked the ID question first, followed immediately by VI, with 1992 VR after the economic optimism question from the standard political section (at Q.5). The effect of the experimental method was to increase Labour (voting intention) support by 5%.

The questions asked, detailed in the table below, were as follows:

Q.1 **How would you vote if there was a General Election tomorrow?**

Q.2 (To those undecided or refused at Q.1) **Which party are you most inclined to support?**

Q.1/2 Combined VI. (In the table, the first column excludes don't knows - the standard headline VI figures - while the second includes them)

ID **Generally speaking, do you think of yourself as Conservative, Labour, Liberal Democrat or what?**

VR **Which party did you vote for at the last General Election, in April 1992?**

First Experiment

22-26 July 1993

	Standard method (VI first) Base: 1,753					Experimental method (Party ID first) Base: 825				
	Q.1/2		Q.1	ID	VR	Q.1/2		Q.1	ID	VR
	%	%	%	%	%	%	%	%	%	%
Conservative	27	22	18	28	32	27	23	20	28	32
Labour	43	36	33	35	33	48	41	39	36	34
Liberal Democrat	26	21	17	17	15	22	19	17	15	15
Nationalist	2	2	2	2	2	1	1	1	*	*
Green	1	1	1	1	*	1	1	1	1	1
Other	1	*	*	1	*	1	*	*	1	*
WNV/DNV		8	7		13		5	5		
None of these				9					8	
Undecided/DK		7	19	5	1		6	14	8	18
Refused		3	3	2	2		4	3	2	
Too young					2					

The very close correspondence of answers on the 1992 VR question suggests that the two samples are well matched and that the variation in ID (slight) and VI (greater) can be put down to the effects of question order.

Results of the first experiment

It might be supposed that the effect of starting with the ID question would be to increase divergence between party ID and measures of voting support, since asking the more general question first ought to minimise any distorting effect of question order. (Heath and Pierce[99], considering the related effect of question order on the relationship between ID and VR, surmised that this would be the case.) In fact, in the case of ID followed by VI precisely the opposite has occurred: with ID asked first, adherence to that party at the voting question was considerably higher (77% as against 71% for Conservatives, 95% as against 88% for Labour, and 81% as against 80% for Liberal Democrats). The idea that Britons, like Americans, draw a clear distinction between party ID and voting behaviour, which has been concealed by question order under the traditional method, is plainly false on the basis of this test.

In fact, the clearest conclusion established is that proximity of questions has a far stronger effect than their order. Heath and Pierce are probably right that a psychological desire to give consistent answers is indeed operating. However, there is no sign of a greater propensity to be inconsistent when moving from the

[99] 'It was Party Identification all along: Question Order Effects on Reports of Party Identification in Britain', Electoral Studies Vol 11, No 2 (June 1992). The MORI experiments are not directly comparable with the results reported by Heath and Pierce, which used a different form of the party identification question.

general (party ID) to the specific (VI) than in the opposite direction; if it exists at all, it is swamped by the effect of having the two sets of questions following on immediately from each other in the experiment. (In the standard version nine questions intervened between VI and ID.) Methodologically, the conclusion must be that MORI's traditional method - putting ID at the end of the political section when memory of declared VI has had a chance to fade, at least six and as many as forty questions intervening - is far more robust than the American question order. As far as getting a meaningful measure of party ID is concerned, divorcing the two questions in this way is clearly preferable. However, this leaves aside the question of which method is better for measuring VIs.

The most obvious effect of the experimental method was to reduce the number of don't knows at VI - in fact, it acted as an extra spur to producing a party allegiance, with three consecutive party questions being asked instead of the normal two. The scale of the effect can be seen especially clearly by comparing responses to Q1 of the standard VI test - 'would not vote' fell from 7% to 5% and 'undecided' from 19% to 14%, with corresponding rises in both Conservative and Labour shares. However, the follow up Q2 still succeeded in eliciting further responses. On the combined VI, 'would not vote' on the experimental method was down to 5% - lower than it has been in any monthly MORI poll under the traditional method since the election. (It has not fallen below 7% in 1993-4.)

This reduction of don't knows had a direct party effect, the difference in party strengths resulting from the experimental method being statistically significant. The components of the effect can be seen by comparing the ID x VI matrix for each method. Twice as many Lib Dem identifiers said they would vote Labour under the experimental method as under the traditional method - the gain coming mostly from don't knows, though the Lib Dem identifying Tory vote was also extinguished completely; Labour achieved a 7% increase in VI among its own identifiers, again arising from the fall in combined don't knows from 8% to 2%. Overall, the Labour share increases 5% as a share of total sample and 4% on the headline figure; Lib Dem falls 2% on total share and 3% on headline figure. Interestingly, the Conservative share is unaffected.

First Experiment: VI by party ID

	Con identifiers		Lab identifiers		LD identifiers	
	Trad	Expt	Trad	Expt	Trad	Expt
	n=497	n=225	n=636	n=293	n=291	n=127
	%	%	%	%	%	%
Don't knows excluded						
Conservative	81	86	1	-	3	-
Labour	5	5	96	97	9	16
Liberal Democrat	14	9	3	3	87	84
Other	1	-	*	-	1	-
Base: All						
Conservative	71	77	1	-	3	-
Labour	4	4	89	95	8	15
Liberal Democrat	12	8	3	3	80	81
Other	*	-	1	-	1	-
WNV	5	4	3	1	2	2
Undecided	5	4	3	1	5	2
Refused	2	3	1	*	1	-

Clearly the question that needs to be answered is how realistic this effect is in terms of votes - are the extra respondents to the VI questions merely would not votes who have had an answer extorted from them, or are they as likely to vote as any other ambivalent voters who nevertheless have some party preference? The latter seems plausible - it is possible that there are respondents who would not answer the normal VI question (or who would equivocate) but who will answer the ID question and, having done so, are then prepared to answer the VI question as well.

It is plain that part of the decrease in don't knows *does* arise simply from an extra warm-up question as opposed to a cold start - the number of don't knows to the ID question when asked first was considerably higher than when it was asked later in the interview (8% as against 5% - in fact the figure did not surpass 6% in any MORI survey using the Michigan-style ID question since the 1992 election). However, the number of 'none of these' answers was slightly down (though probably not significantly so - 8% as against 9%).

The cold start cannot explain the whole of the effect: total non-party answers on the ID question when used first (18%) were far lower than on VI Q1 (29%) and matched the figure for the combined VI questions when asked first. It is plain that there is less initial resistance to the ID question than to the VI question.

The Liberal Democrat vote falls on the experimental method (ID first) even though the VI question is preceded by a question naming the Liberal Democrats. It seems to be linked to (i) the lower number of Liberal Democrat identifiers and (ii) the greater loyalty of Conservative identifiers (only 8% as opposed

to 12% of them declaring a Lib Dem VI). The first of these is the primary effect - the relationship between total LibDem identification and vote was consistent across the two tests, the vote (as a proportion of total responses) being 4% higher than the party ID in both cases. However, the detailed relationship between ID and vote was different, and the LibDems lost a further two points on the headline VI figure as the don't knows fell.

The lower number of LibDem identifiers when asked the ID question was asked first does not seem to be a simple sampling error. If we look at ID as measured against VR, LibDem identification is lower both among those who say they voted LibDem in 1992 and those who say they voted Tory.

First Experiment: Party ID by recalled 1992 vote

	Con vote '92		Lab vote '92		LibDem vote '92	
	Trad	Expt	Trad	Expt	Trad	Expt
	n=555	n=262	n=591	n=274	n=261	n=121
	%	%	%	%	%	%
Conservative	76	74	1	1	6	7
Labour	4	5	88	86	8	10
Liberal Democrat	8	5	6	5	71	65
Other	*	-	1	1	3	3
Don't know	5	10	1	3	6	8
None	6	5	3	4	6	6
Refused	1	1	*	-	-	1

In short, there were as many potential Liberal Democrats in the sample. However, less of them chose that party ID when the question was asked first, and the shortfall then carried through into VI. Why? One possibility is that some respondents are unwilling to admit being tactical voters, and consequently give consistent answers to the VI and identification questions, answering truthfully whichever is asked first and making the second answer conform (whether consciously or unconsciously). Thus a group of Labour identifiers who will vote tactically for the LibDems say they are Labour if asked the ID question first but that they are LibDems if asked the voting questions first. If this is the case, it is clearly preferable to persist with the traditional method, since accuracy on VI is the more important consideration. Note, however, that this explains less the half the rise in the Labour VI figure; the remainder comes from the more efficient squeeze of the don't knows, which may well be much more closely related to actual voting behaviour.

Past Vote before Voting Intention

The second experiment used the August 1993 regular MORI omnibus (fieldwork 19-23 August, n= 2,742 in three matched versions), using the same questions but testing the effect of VR as a first question rather

than ID. Versions 1 and 2 of the omnibus were again of the standard form, with the VI questions asked first and ID and VR at the *end* of the political section (at Qq. 22 and 23); Version 3 asked the VR question first, followed immediately by VI, with ID after the economic optimism question from the standard political section (at Q.5). Again, the most notable result of the experimental method was to increase the recorded Labour voting intention by 5%.

Second Experiment

19-23 August 1993

	Standard method (VI first) Base: 1,827					**Experimental method (Vote Recall first)** Base: 915				
	Q.1/2		**Q.1**	**ID**	**VR**	**Q.1/2**		**Q.1**	**ID**	**VR**
	%	%	%	%	%	%	%	%	%	%
Conservative	**28**	23	17	27	30	**25**	21	16	27	27
Labour	**42**	35	32	34	31	**47**	40	37	36	31
Liberal Democrat	**25**	21	18	18	17	**25**	21	18	17	13
Nationalist	**3**	2	2	1	2	**1**	1	1	1	1
Green	**1**	1	1	1	*	**1**	1	1	1	*
Other	*	*	*	1	*	**1**	1	1	1	*
WNV/DNV		9	8		15		8	7		17
None of these				11					9	
Undecided/DK		6	19	5	1		4	16	6	1
Refused		2	2	2	2		4	3	2	9
Too young					2					2

The most startling result of the experiment is that it found a 9% refusal rate for VR when asked first. (MORI normally finds a refusal rate of 2-3% for this question.) Assuming that this is not the result of a freak sample (which seems unlikely, as refusal to the VI questions that followed was only 4%) it appears that at the time there was considerable resistance to being asked vote recall from a cold start - far more so than is ever normally found when VI is the first question. Plainly, therefore, it is an inefficient question order in that it dramatically reduces the VR information available, even though its effect on VI response rates appears to be small. (Refusal was up slightly, but total don't knows were slightly reduced. This fits in with the conclusion from the first experiment that having a warm start in general helps squeeze don't knows at VI, in this case the effect being counteracted by increased inhibitions raised by an opening VR question.

The most obvious result of the experiment in party terms is that again the warm start produces a Labour voting intention figure five points higher than the standard method. Judging by the ID question, it is possible that the sample for the experimental version was slightly more pro-Labour, but this may equally be a distortion of the ID question caused by following so soon after VI. At any rate, even if there was a difference between the samples it was not nearly big enough to explain the effect on VI. The main contrast with the result of the first experiment is that when asking VR first it is the Conservative vote that drops

most whereas asking ID first it was the Liberal Democrats that suffered. This is directly contrary to received wisdom, which would hold that the ID question (which mentions the Liberal Democrats by name) ought to be more favourable to them than the VR question (which does not).

By comparing voting intention with party ID and with recalled vote, we can see that two separate effects are probably operating to affect VI when VR is asked first. We see that the responses of Labour identifiers appear unaffected by the different method. However, Conservative identifiers are substantially less likely to say they will vote Conservative, switching either to the Liberal Democrats or to the don't knows; Liberal Democrat identifiers, meanwhile, are less likely to say that they will vote Liberal Democrat, switching instead to Labour; these two movements cancel out, the overall effect being a rise in the Labour figure at the Conservatives' expense.

Second Experiment: VI by party ID

	Con identifiers		**Lab identifiers**		**LD identifiers**	
	Trad	**Expt**	**Trad**	**Expt**	**Trad**	**Expt**
	n=489	n=238	n=619	n=337	n=321	n=148
	%	%	%	%	%	%
Conservative	85	80	1	2	5	4
Labour	5	6	94	94	7	14
Liberal Democrat	10	14	3	3	86	80
Other	*	-	2	4	2	2
WNV	4	7	2	3	3	4
Undecided	3	5	2	*	2	2
Refused	1	5	*	*	1	2

Why is this happening? It seems clear from the refusal rate that asking VR first has the effect of putting respondents more on the defensive than normal. It seems probable that this caused Conservatives to offset their support for an unpopular government by saying that they would not vote for it again. This might have been also the cause of the effect on the Liberal Democrat vote (LibDems ashamed of their party having split the vote, saying they would vote Labour next time to get the government out) if the same pattern was present in the table of VR against VI.

Second Experiment: VI by recalled 1992 vote

	Con vote '92		Lab vote '92		LibDem vote '92	
	Trad	Expt	Trad	Expt	Trad	Expt
	n=549	n=242	n=559	n=283	n=313	n=110
	%	%	%	%	%	%
Conservative	76	72	1	1	3	3
Labour	8	7	94	95	12	9
Liberal Democrat	15	18	4	3	84	88
Other	1	3	1	1	1	-
WNV						
Undecided	6	5	2	*	3	*
Refused	4	5	3	1	4	1
	1	4	1	1	1	1

In fact, this table shows the effect reversed: those who claimed to have voted LibDem in 1992, as opposed to LibDem identifiers, were *more* likely to say they would vote LibDem at a future election and less likely to support Labour if VR was asked first. Plainly this is a tactical voting effect: asking VR first reminds Liberal Democrats who voted tactically that they will want to do so again, and voting intention is modified accordingly. Thus consistency is higher for LibDems between VI and VR, and lower between ID and VI, because the tactical voters, having been reminded what they did at the last election, remember to take account of it in their VI. For the Conservatives, by contrast, the effect is similar in both tables, indicating that they are genuinely less willing to admit a future Conservative voting intention.

Appendix 6C: BES evidence on Accuracy of Recall of Voting

It is possible using the 1987-1992 BES panel study to re-evaluate the claim made by Himmelweit et al[100] that voters' memories of their previous voting behaviour is faulty, and that in particular voters are likely to make their past behaviour consistent with their current preference. In the panel study voters were asked after the 1987 election how they had voted in the 1987 election. They were asked the same question again in 1992.

Table 64: Reported and Recalled Voting in 1987

	Reported Vote in 1987 %	Recalled Vote in 1992 %
Conservative	37	44
Labour	26	32
Alliance	20	13
Other	1	1
Refused/DK	2	3
Abstained	14	7

(n=1604) Source: BES

As Table 64 shows, there was a clear difference between the answers respondents gave in 1987 and what they said five years later. In particular voters were unlikely to remember having voted either Alliance or having abstained. Across the whole sample as many as 21% of all respondents gave a different answer in 1992 than in 1987, identical to the results of the 1966-70 Butler and Stokes panel study. Most seriously, if the polls had reweighted their samples so that recall vote was the same as the actual vote in 1987 they would probably have over-represented the number of former Alliance voters in their samples and thus the likely Liberal Democrat performance in 1992. (This was the problem faced by ASL in their telephone polls in the 1983 General Election, when they overestimated the Alliance vote by three percentage points.)

The particular inability of voters to remember having voted for a third party is shown in Table 65. The table also confirms that there is nothing new about this. Liberal voters had been equally forgetful of their previous behaviour in the 1960s as well.

[100] H. Himmelweit, M. Biberian and J. Stockdale, 'Memory for Past Vote: Implications of a Study of Bias in recall', British Journal of Political Science 8, pp 365-375 (1978).

Table 65: Correct recall of vote 1970 and 1992

Reported vote in 1987/1966	% Correct Recall *Of 1987 vote in 1992*	*Of 1966 vote in 1970*
Conservative	93 (558)	92 (290)
Labour	89 (385)	87 (369)
Liberal/Alliance	54 (292)	45 (71)
Abstained	26 (160)	45 (104)

Source: BES

We can examine how far these differences can be accounted for by voters aligning their past behaviour with their current preference by looking at the relationship between reported vote and recalled vote according to how people voted in 1992. Of a total of 341 voters whose vote was different from their recall, as many as 53% aligned their recall vote with their current voting behaviour. This again is almost identical with the results of the 1966-70 Butler and Stokes survey where 55% of misremembering voters aligned their behaviour (n=137). This tendency towards consistency was however primarily a feature of Conservative and Labour voters of whom as many as 73% aligned their past behaviour with their current preference. In contrast only 22% of Liberal Democrat voters did so. So not only do those who voted for the Liberal Democrats in the past forget having done so, but those who switch to the Liberal Democrats are much less likely to forget not having done so in the past. Together the two processes account for the lower level of third party support in the recall vote figures than in the reported ones.

These results suggest that the difficulties which Himmelweit et al identified in the use of recall data to weight the sample profile of a poll still exist. Any such procedure is always liable to overestimate the level of third party support. Both MORI and ICM have also found since the 1992 election that considerably fewer respondents admit having failed to vote than must actually have done so, when asked a standard vote recall question.

Appendix 6D: Voting Recall surveys 1992-3

Q. Which party did you vote for at the last General Election, in April 1992?

	GE 1992 %	*S.Times* Panel Recall %	25-28 Apr 1992 %	Nov-Dec 1992 %	Feb 1993 %	Mar 1993 %	Apr 1993 %	May 1993 %	June 1993 %	July 1993 %	Aug 1993 %	Sep 1993 %	Oct 1993 %
Conservative	33	37	33	33	33	33	33	34	31	32	30	32	33
Labour	27	34	31	35	33	34	33	33	34	33	31	31	33
Liberal Democrat	14	19	16	14	15	14	14	14	14	15	17	15	12
Nationalist	2	1	1	1	1	1	1	1	2	2	2	2	1
Green	*	*	*	*	*	*	*	*	1	1	*	*	*
Other	1	1	*	*	*	*	*	*	*	*	*	*	*
Did not vote	22	6	15	13	12	13	13	13	13	13	15	14	15
Too Young	n/a	n/a	n/a	n/a	2	2	2	2	2	2	2	3	3
Can't remember	n/a	n/a	n/a	*	1	1	1	1	1	1	1	1	1
Refused	n/a	1	3	3	2	2	2	2	2	2	2	2	1

Source: MORI

Appendix 6E: ICM's experiments with exclusion of non-voters

As noted above (paragraph 321 *et seq*), the polls seem to find too few people who say they definitely will not vote, typically less than 10% of the sample. This low figure may be partly attributed to the practice of what has become known as *'squeezing the don't knows'*. Simply, those who reply to an initial voting intention question with a 'don't know' response are encouraged to say who they are most likely to support in the election. Those who remain in the 'don't know' category or who refuse to answer are then treated as non- voters along with those saying they definitely will not vote. Most pollsters then use a certainty-to-vote question to examine likely voting behaviour at different levels of turnout. In most elections adjusting for turnout in this way has made very little difference to the final voting intention figures. (In 1992, MORI excluded from their final poll all those who said they were not certain to vote; the result was to reduce the Labour lead by 1%; Gallup report that had they done so it would have increased the Conservative lead by 1½%.)

Some evidence has emerged since the last election that people answer the ***voting intention*** question with a ***party support*** answer. ICM asked a sample of 1,400 people, first of all, how likely they would be to vote in an immediate general election. 13% said they definitely would not vote. Immediately following this, all respondents were asked who they would vote for in an immediate general election. Only 6% said they would not vote. Thus, some 7% of the sample said they ***definitely would not vote*** in an immediate general election and then proceeded to choose a party ***they would vote*** for in that hypothetical election.

The same has also been true of past voting. Vote recall questions consistently find too few who admit not having voted. The indications are that some replies to the question ***'Who did you vote for last time?'*** merely reflect the party they ***supported*** last time.

In December 1993 and January 1994, ICM attempted to find out how many actual non-voters were interviewed in the sample by introducing a question specifically designed to measure turnout.

'Q. I would like you to think back very carefully to the last general election in 1992. Some people were not able to vote in that election. Can you remember did you actually go to your polling station and cast your vote?'

ICM found 19% of its sample in December and 17.5% in January said they did not vote, against 22% actual non voters according to analysis of the electoral register. At least part of the difference can be accounted for by deficiencies in the electoral register rather than the poll itself (and some non-voters are presumably inaccessible to interviewers).

The evidence therefore suggests that polls do actually interview a sample which is representative in terms of likelihood of voting. The appearance of finding 'too many' voters is because people answer voting behaviour questions by saying who they **support**, not who they actually did or would go and vote for.

As a result, ICM has also introduced a ten point scale asking people to say how likely they are to go and vote in a future election before asking voting intentions. The results are instructive. Those least likely to vote, 1,2 and 3 on a ten point scale, are younger people in the DE social class, typically Labour voters.

Table 66: Likelihood of voting on a ten-point scale, 1994

	Least likely to vote (1,2 or 3 on 10 point scale
	n= 133
Male	8%
Female	11%
18-24	12%
25-34	12%
35-64	7%
65+	9%
AB	5%
C1	7%
C2	7%
DE	15%

Source: ICM

These figures correspond quite well to the pattern of actual turnout at the 1992 general election indicated by other sources. This suggests that such questions can, indeed, be useful in adjusting the polls to allow for turnout.

APPENDIX 7: SEATS AND VOTES

Parliamentary Seats from Poll Results

National polls are a method for estimating the share of the national vote that would be won by each party in an immediate general election, but the 'winner' of a general election is the party which wins an overall majority of seats in the House of Commons. Most media clients of the polls want to know which party, if any, will win a majority, and the likely size of the majority. In order to be able to estimate accurately the number of seats that would be won by each party under the 'first-past-the-post' electoral system, one needs to have detailed information on the geographical distribution of each party's vote, constituency by constituency. No single opinion poll of 1,000-2,000 people is large enough to be able to provide such detailed information; it would take 651 separate polls to accurately estimate the result in every constituency.

Because in Britain the swing is usually fairly uniform, a simple assumption is commonly made in order to estimate the number of seats that would be won by each party. This is to assume that the change in each party's share of the vote is the same in each constituency, or at least that any variations are mutually cancelling.

In practice, the change in each party's share of the vote does vary from constituency to constituency at a general election. But so long as the variation is random and in particular is not strongly related to where a party did well or badly at a previous general election[101], the variation will have little net impact on the number of seats won by each party. So far as Conservative and Labour are concerned at least, the number of seats won by each party has not deviated substantially from this rule at most post-war elections[102].

The 1992 election result revealed, however, the potential pitfalls of this procedure. If the change in each party's share of the vote since 1987 had been uniform in each constituency, the Conservatives would have secured an overall majority of no less than 61 seats compared with the 21 seats they actually received. Labour made a net gain (compared with 1987) of 39 seats from the Conservatives when they might have expected to have secured only 20. This happened for a number of reasons but primarily because the swing to Labour was higher in marginal seats than elsewhere. This was in part because of apparent tactical voting by Liberal Democrat supporters. Further, the Liberal Democrats themselves made a net gain (again

[101] For further details see G. Gudgin and P. J. Taylor, Seats, Votes and the Spatial Organisation of Elections (New York: Pion, 1979).

[102] See J. Curtice and M. Steed, 'The Results Analysed', in D. Butler and D. Kavanagh, The British General Election of 1992 (Basingstoke: Macmillan, 1992), and the statistical appendices to the earlier volumes in the series.

compared with 1987) of four seats from the Conservatives, rather than the net loss of two that uniform swing would have implied; here again tactical voting played an important role[103].

The narrow Conservative lead in terms of seats compared with the position in terms of votes made the discrepancy between the results of the final polls and the actual outcome a little less apparent. However, it also revealed how fragile interpretation of the polls can be. Before the vote, it was universally assumed by all commentators on the polls that, with a general election result giving a Labour lead of 0.8% (the final polls' average), Labour would win most seats but would be well short of an overall majority. In fact, if the national result had been in line with the polls but the geographical distribution of each party's vote had been in line with what actually happened in 1992 (a distribution which proved to be highly favourable to Labour) Labour would have won a narrow overall majority. In that case it might well have seemed to the general public that the polls had got it wrong when in fact they had been entirely accurate in estimating what they are designed to estimate, the national share of each party's share of the vote.

It can, of course, be asked whether in the light of the experience of the 1992 election better methods might be adopted to estimate the outcome in seats. One possible approach is to concentrate opinion polling in marginal constituencies; the assumption of 'uniform swing' will only be undermined if the behaviour of these constituencies is different from that of the country as a whole. As discussed above (paragraph 227 *et seq*), conducting polls in marginal seats is not easy because of the lack of reliable information on which to base quota controls. (In any case, despite the rise in the amount of tactical voting in 1992, it is by no means clear how far this indicates a long-term increase in propensity to vote tactically[104].)

The public should clearly be made aware of the uncertainty attached to any translation of a poll result from votes into seats. Methods have been suggested to derive confidence bands for any such estimate, based on similar principles to the confidence bands that are used to indicate the sampling error attached to any poll. Essentially, instead of assuming that swing will be uniform, these methods assume that there will in fact be a degree of variation which is normally distributed. From this a *range* of likely outcomes in

[103] J. Curtice and M. Steed, 'The Results Analysed', in D. Butler and D. Kavanagh, The British General Election of 1992 (Basingstoke: Macmillan, 1992)

[104] A. Heath et al, Understanding Political Change (Oxford: Pergamon, 1991); R. Niemi, G. Whitten and M. N. Franklin, 'Constituency Characteristics, Individual Characteristics and Tactical Voting in the 1987 British General Election', British Journal of Political Science 22 (April 1992), pp 229-54; G. Evans and A. Heath, 'A Tactical Error in the Analysis of Tactical Voting: A Response to Niemi, Whitten and Franklin', British Journal of Political Science 23 (1993), pp 131-7; R. Niemi, G. Whitten and M. N. Franklin, 'People who live in Glass Houses: A Response to Evans and Heath's Critique of our note on Tactical Voting', British Journal of Political Science 23 (October 1993), pp 549-53.

terms of seats can be calculated[105]. The polling companies may wish to consider encouraging their media clients to report their polls in terms of such ranges in future.

Exit Poll Conversion to Seats

While it may be the case that the primary objective of an opinion poll is to estimate each party's share of the vote rather than the number of seats they will win, exit polls are commonly commissioned with the explicit purpose of attempting to estimate the outcome in seats. Indeed, as described earlier, both BBC and ITN commissioned specific exit polls focused on marginal seats in order to make an estimate of the outcome in seats at the beginning of their election night programmes. (These polls were entirely separate from the 'analysis' polls which both companies conducted, which were based on nationally representative samples.)

This practice has however caused some confusion. It has not always been understood by viewers that the seat projections produced by the television companies are based on a different poll from that which produces an estimate of the national share of the vote. (It was partly for this reason that the BBC did not broadcast the vote shares from their analysis poll in 1992.) That confusion was exacerbated in 1992 because, as some commentators pointed out, if the two companies had estimated the outcome in seats by applying to every constituency their analysis polls' estimate of the change in each party's share of the vote since 1987, they would have produced a Conservative majority very close to the eventual outcome[106].

That was however entirely fortuitous. Both analysis exit polls in fact underestimated the Conservatives' lead by 3-4 percentage points. But as we have seen, the swing to Labour was higher in marginal seats than in the country as a whole. As it happened the error in the analysis' polls estimate of the national swing was more or less the same as the difference between the national swing to Labour and the swing in marginal seats. Thus, purely by chance, the estimated swing in the analysis polls happened to match the actual swing in marginal seats. The errors cancelled each other out.

In fact, the difference in behaviour between marginal seats and the rest of the country clearly indicated the value of basing any exit poll based seat projection on marginal seats rather than the country as a whole - so long of course as the exit poll is accurate. But there is one clear limitation to any such exercise. We understand that both companies based their exit polls on seats which the Conservatives were defending against challenges from either Labour or the Liberal Democrats. Neither company polled in, for example,

[105] P. J. Brown and C. Payne, 'Election Night Forecasting', Journal of the Royal Statistical Society (A), 138 (1975), pp 463-97; P. J. Brown and C. Payne, 'Forecasting the 1983 British General Election, The Statistician, 33 (1984), pp 217-8; R. Hugh Morton, 'Adjusting the Electoral Pendulum for Variance, Electoral Studies 11 (1992), pp 58-61.

[106] See, for example, R. Worcester, Letter to the Editor, The Independent, 5 May 1992.

seats which the SNP were challenging (or defending against) the Conservatives or seats where the contest was between Labour and the Liberal Democrats. This is because there are relatively few examples of each of these types of marginal seats; a highly disproportionate amount of resources would have to be devoted to polling in these seats if a reliable exit poll based forecast of the outcome were to be produced. (The sampling error for any poll is determined by its sample size; so it requires a poll of the same size to predict the outcome with the same degree of confidence in three seats as it does in thirty.)

Both television companies attempt to get around this problem by forecasting the outcome in unusual kinds of marginal seat by using expert judgements. These judgements are made on the basis of, *inter alia*, national and local opinion polls, local election results and historical precedents. In 1992 however, the expert judgements proved to be fallible. In the BBC's case, the inaccuracies knocked 9 seats off the companies' estimate of the Conservatives' total[107]. An exact figure has not been produced by ITN but it would appear that their errors reduced their estimate of the Conservatives' seats total by 13 seats.

The validity or otherwise of the methodology used to make these expert judgements - or indeed the wisdom of making them in the first place - lies outside our remit. But the outcome of the 1992 election clearly indicates the need for a careful review of the procedures used. Of course, in so far as the expert judgements relied on campaign polls for some of their evidence, it meant that despite their use of their own exit polls, the television companies' procedures were not wholly insulated from the errors in the campaign polls. But it is clear that not all of the error in either company's seat projections was caused by the exit polls themselves, and thus the seat projections themselves are not a fair measure of the accuracy or otherwise of those polls.

There were some detailed differences in the way in which the two companies used the exit polls to make a seat projection. But their impact was marginal. The BBC, for example, used a detailed regional breakdown of the marginal seats in its sample and produced its seat outcome by applying these to the marginal seats in each region. This had the fortunate impact of producing an estimate of the Conservative number of seats which was two seats higher than would have been the case if no regional breakdown had been applied. ITN, meanwhile, found a discrepancy in the result produced by those sampling points which they had previously used in 1987 and those which were using for the first time. They decided to weight the result of the poll in favour of the former on the grounds that they had greater confidence in those sampling points, but with the unfortunate consequence that their estimate of the number of Conservative seats was reduced by four.

[107] J. Curtice and C. Payne, 'Forecasting the 1992 Election: the BBC Experience', in I. Crewe and B. Gosschalk (eds.), Political Communications: The British General Election of 1992, Cambridge University Press. (In press).

Appendix 8: Procedure for using the Self-assigned Goldthorpe Class Schema

The procedure relies upon asking respondents themselves to state whether a occupation belongs to a particular category of occupation giving examples of the kinds of jobs that fall within each category. The precise form of the question has varied from experiment to experiment, but the following is the form which has been most often used (the question has been designed as a self-completion question but could equally well be administered in a face-to-face interview):-

Q. What best describes your work? If you are not working now please think about the most recent job you held.

(a) Farmer or farm manager
(b) Farm worker
(c) Skilled manual work (e.g. plumber, electrician, fitter, train driver, cook, hairdresser)
(d) Semi-skilled or unskilled manual work (e.g. postman, machine operator, assembler, waitress, cleaner, labourer)
(e) Professional or technical work (e.g. doctor, school teacher, engineer, social worker, accountant, computer programmer)
(f) Manager or administrator (e.g. company director, manager, executive officer, local authority officer)
(g) Clerical (clerk, secretary, telephone operator)
(h) Sales (e.g. shop assistant, commercial traveller)
(i) Other (Please specify)
(j) Never had a job

In addition, details of the respondent's employment status are ascertained as follows:-

Q. Are you self-employed or do you work for someone else as an employee?

Self-employed
Employee

Q. As your position at work, are you (or were you)..

A supervisor or foreman of manual workers
A supervisor of non-manual workers
or Not a Supervisor or Foreman?

The respondents' social class is then determined by the following grid:-

	Self employed	Non-manual supervisor	Manual supervisor	Employee
Farmer	Petty bourgeois	Petty bourgeois	Petty bourgeois	Petty bourgeois
Farm worker	Petty bourgeois	-	Working class	Working class
Skilled manual	Petty bourgeois	-	Foreman class	Working
Semi- and unskilled manual	Petty bourgeois	-	Foreman class	Working
Professional or technical	Salariat	Salariat	Salariat	Salariat
Manager	Petty bourgeois	Salariat	Salariat	Salariat
Clerical	Petty bourgeois	Salariat	-	Routine non-manual
Sales	Petty bourgeois	Salariat	-	Routine non-manual

ADDENDUM: HOW SHOULD FINDINGS BE PRESENTED?

By Robert M. Worcester,

adapted from *British Public Opinion,* (Oxford: Basil Blackwell, 1991)

Opinion polls are an expensive form of journalism, yet nearly every national and Scottish newspaper sponsors its own poll at election times (and many between elections as well). All newspapers report all the others' polls, sometimes at considerable length, as do the television news programmes.

George Brock, in charge of the election coverage for *The Times* during the 1987 election, said that the write-up of a political poll was one of the most complicated exercises in the life of a newspaper, involving as it usually did the editor, news editor, editorial page editor and sometimes op-ed editor, the subs, graphics, systems, the political editor and the journalist responsible for writing up the story not to mention the pollsters involved in obtaining the data in the first place and checking the copy and graphics in the end.

We thought that laying out systematically some suggestions for editors and for journalists writing up poll findings might be thought to be helpful by those who have the responsibility for understanding and reporting British public opinion.

Pollsters in Britain take considerable pains to see that their clients get it right, don't overclaim on marginal shifts, insist on fieldwork dates, sample sizes, and so on, but in the end have little control over headlines, story placement, and length of coverage. They have less, much less, control over secondary coverage in other papers or on radio and television.

Television has by its nature even more difficulty in reporting poll results, no matter how hard it tries. It is even shorter on copy deadlines and time to include the essential information of when the poll was conducted and other details.

That polls make television news is shown by McKee[108], who reported that ITN used poll data to determine programme focus on issues. Further, they asked the public how they thought television should cover the campaign and how election news - including polling results - could be improved. The first way they tried to improve poll data presentation was to put it into as clear a framework as they could, relating it to the campaign, clearly sourcing it both as to polling organisation and to sponsoring newspaper, and to blend in other information beyond voting intention, such as issues, leadership ratings, and regional polls. The second initiative they took was to wrap up the week's polling coverage on Sunday night, to

[108] P. McKee, 'ITN's Use of Opinion Polls', in R. Worcester and M. Harrop, Political Communication and the British General Election of 1979.

point out trends and implications. They also ran an extended feature on how a polling organisation works (as had the BBC earlier).

Some see the solution to the poll-newspaper conflict at the sponsor level in having the copy written by the polling organisation and commented on by journalists and/or academics. Others employ journalists or academics who specialise in the write-up of polls.

Polls are the captive of the professionalism of the editor who commissions them, the journalist who writes them up, the graphics artist who designs the graphs to illustrate them, the sub-editor who subs the journalist's copy and writes the headline over them, and the pollster himself who must argue with any or all of the above to ensure that the pressures of the newsroom or the carelessness of the weakest link in the chain isn't allowed to misinform the reader.

Headlines

The 'bottom line' is most often the headline, and the headline is most often the weakest part of an opinion poll story. So often the editor has fought for the space and found the budget, the pollster has agonized over the questions and sent scores of interviewers to a carefully selected sample of hundreds or even thousands of good people who have taken the time and trouble to give their opinions, the computer has been programmed and has faithfully produced the figures, the journalist has strained and produced the copy, checked by the pollster, only to have a sub-editor put the words into a headline that fit the space rather than the story to catch the reader's eye. And many more readers will see the headline than will read the story, analyse the numbers or ponder on the graphics (if any). Sometimes headlines imply a forecast when the story scrupulously reports the findings of the poll as reflecting when the fieldwork was done. Sometimes headlines suggest change when no trends are available. And sometimes headlines have been written which summarise poll findings into a meaningless average.

Text

Text is tricky when it comes to reporting polls. The average journalist is a wordsmith, not a number cruncher, yet is asked to wrap words around statistical tolerances, percentage change over time, compare sub-groups, possibly calculate swings and meet the deadline for copy in just two hours flat. It's a superhuman task; no wonder it so often goes wrong.

Some of the problems include these, and are sometimes compounded by a careless headline:

Generalising beyond the sample: Too easy to extrapolate a survey finding from a region, or a sector of the public, to the nation.

'Forgetting' the limitation of the poll: Forgetting to mention the poll was done in April and now it's September, or that it's a telephone poll in a country where only 75% of people are on the phone, or that there was a qualifying phrase to the question that was asked, or that it was released by a pressure group, or that it was done by a pressure group or political party with their axe to grind, or that the question was 'filtered', that is only asked of a sub sample, etc.

Spurious accuracy: Carrying the responses to one decimal point, thus suggesting a degree of accuracy beyond the reliability of the data.

Hyping the figures: By repercentaging, leaving out the 'don't knows', a no-no except in election/referendum comparisons.

Predicting the future: poll results are a snapshot at a point in time and that time is when the fieldwork was taken, not three days, three weeks or three months into the future. Foreign correspondents shouldn't write that 'Latest polls in France forecast such and such an outcome'; they do not, they report the state of the parties on the day or days the interviews were taken, not the outcome a month hence.

Telling half the story: Finding the figures that support the best/most interesting/most likely to startle the reader story and ignoring the follow-up responses which modify or contradict the sexy bits.

Giving the answers but not the questions: Pollsters and some journalists work very hard to ensure questions aren't simplistic, are complete, balanced and capable of being answered by typical respondents. Balanced questions include asking, eg. 'Do you favour or oppose ...'. Some journalists and sub-editors delight in saving the two words 'or oppose', thus misleading the reader and inviting the letter writer protesting at the 'biassed questions' in the poll. Or worse, not reporting the question wording at all.

Not checking the facts: Alan Watkins, then writing in the *Observer*, several years after the 1983 General Election wrote about the unreliability of the polls, stating 'they underestimated the Alliance support at the last General Election'. In fact, the Alliance got 26% of the vote, MORI, Marplan, NOP, Harris and Gallup all forecast an Alliance share of 26%. Only Audience Selection (by telephone) got the Alliance share wrong - and they overestimated Alliance support, not

underestimated it. Watkins' response when this was pointed out: 'Thank you for your letter. Wrong again'. The pundit did not take the opportunity to run a correction in a subsequent column. It is no wonder there is a substantial proportion of the public who believe the polls always get it wrong when journalists who get us wrong - although when cheerfully admitting it privately - fail to correct the public record.

Tables

Leaving out the questions: The tables are the best place to put the exact question wording of the question and answers asked: too often the sub-editor sees this as an optional extra; it isn't.

Numbers or percentages?: Often it is confusing to the reader as to what is represented. If, as usual, 'all figures are percentages', why not say so as a footnote? And make clear if the percentages read down, or across, to add to 100% and if the figures do not add to 100%, say why (e.g. multiple answers accepted).

Source: Sometimes the tables are extracted or reprinted apart from the text; it is good practice to source the tables so that the reader can judge the reliability of the data and objectivity of the questions according to the reputation of the organisation responsible for the survey; also to give an indication of to whom the interested reader can go for further details of the methodology or the findings.

Indicating the bases: When subgroups are reported, some caution needs to be taken of small sample bases; it is good practice, as indicated above, to indicate in the table the size of sub samples.

Left to Right: Most people read from left to right; it is amazing how many editors and even pollsters think tables should read from right to left.

Calculating trends: It is useful to the reader to be spared the effort of calculations. If a table includes a trend analysis, you might add a column that shows the change over time, eg:

Q. Do you approve or disapprove of the Government's proposal to do away with British Summer Time?

	April 1993	Sept 1993	Change
	%	%	%
Approve	47	53	+6
Disapprove	50	45	-5
Don't know	3	2	-1
Net Approve	-3	+8	+11

Base: Adults in ABC (n,nnn) Source: XYZ

Graphics

The elements of the graphic are similar to the tables: headline, question wording, data, base and source.

There are three basic graphic techniques:

Pie charts - showing share, when only one answer per respondent is permitted (i.e. when all the answers add to 100% and no trends are to be shown). Tip - start at 12 o'clock and move clockwise, leave the don't knows 'til last, and be careful to get the proportions right.

Bar charts - showing distribution, especially when multiple responses are allowed and responses add to more than 100%. Tip - start with the largest response category and work downward - be careful of leaving answer categories out and if you do so, say you have. Put the 'don't knows' at the bottom. Can be used to show single trends, but gets messy.

Trend lines - best for showing trends over time. Tip - remember do not use equal spacing when the polls have been done at differently timed intervals. Also - be fair, don't squeeze the distance to over-emphasise small changes.

One guaranteed way to get poor graphics is to keep the pollster and the graphic artist at arm's length and not let them discuss their work together. Another is to spring the poll findings on the artist an hour or so before deadline. Instead, let graphics in on the act early, say while you're discussing the questions to be asked.

Editorialising

Most polls have an editorial message for the enterprising leader writer. After all, it's vox populi that the poll represents, and your readers are interested in what you think about what they think. Ask the pollster to identify your paper's readers as a subgroup in the analysis - see where they think the same as the public generally and where they are different. Look at the poll's details; it's not only the main lead that lends itself to editorialising.

Secondary Reporting

When picking up somebody else's poll, remember you've still got an obligation to your readers to play it straight. (Besides, it's almost certain to be copyright.) Don't report 'voodoo polls' as if they were gospel truth, don't misrepresent findings by glossing over suspect question wording, thin or skewed samples, or 'forget' to source them.

Use your Pollster

When you embark on publishing a series of commissioned opinion polls you are making a commitment to inform, educate and possibly even entertain your readers or viewers. The best models of media use of polls are those where a genuine collaboration of effort has been made between the pollster and the client.

Discuss your ideas with your pollster. Talk about your paper's future plans. You can trust him; he's out of business double quick if he can't keep a secret, and his obligations to his professional bodies, MRS, ESOMAR and WAPOR, require him to maintain his clients' confidences.

Above all, check with him: clear copy, check tables and graphics, read over editorials and use him as your advisor on the uses, and abuses, the power, and the limitations, of polls.

Other pointers

In looking at numbers of responses to survey questions, a journalist should first determine the response rate for the entire survey and then ask about survey questions where a noticeable number of persons did not respond. Widely varying numbers of responses to different questions may indicate a sensitive topic, unclear question wording, poor interviewer training and supervision, or sloppy key punching and verification, to name just a few possibilities. Most likely however, is a low salience to the respondent.

In looking at percentages of answers and interpreting what they mean, a journalist should know what the base of these percentages is and should report this total number in any tables using percentages. Percentages based on different totals should not be directly compared without letting the reader know what these totals are. Differences in percentages should not be interpreted as real or meaningful unless these differences exceed twice the sampling error for the survey.

When comparing different groups of people to each other in a survey or poll, remember to base the percentages on the total number of people in each group, not on the total number of people in the different answer categories. And remember to present the total number of people in each group in the table, so the readers can see the base for each percentage.

If several answer categories are collapsed into one category, try to indicate in parenthesis what the original answer categories were, so the readers will understand how the new categories were constructed.

In using averages to summarise survey results - or the results of any other kind of research - don't automatically rely on the mean, the most commonly-used average measure. Remember that the mean is sensitive to unusually low and high values, and therefore can be artificially inflated or depressed by a few unusually low or high cases. If the mean and the median differ substantially, use the median (middle point) as your measure of the average, if the median and the mode differ considerably, think about reporting both in your story.

Swing

Use of the concept of swing (see glossary) can be very useful to the journalist trying to summarise a complex set of data in a single figure.

GENERAL ELECTION SHARE OF VOTE (GB)

	1987 %	1992 %	Difference %
Conservative	43	43	0
Labour	32	35	+3
LibDem/Alliance	23	18	-5
Other	2	4	+2
Conservative lead	+11	+8	
Swing			+1½%

A simple example would be in a referendum, comparing a poll taken after the 1974 general election, six months before the EEC Referendum with the actual result, repercentaging the opinion poll results to eliminate the 'don't knows,' so putting the poll findings comparable to those of the referendum result.

	Opinion Poll %	Referendum %	Difference %
'In'	45	67	+22
'Out'	55	33	-22
'In' lead	-10	+33	(+22% swing)

The journalistic description of these figures might have read as follows: 'So a 10 point 'out' poll became a 33 point 'in' vote when the referendum came, which was the result of a 22% 'swing' in the electorate, among those who voted/expressed a voting intention.' It is for this reason that the custom in Great Britain is to reallocate the 'don't knows' (will not votes/undecideds/refused) when reporting voting intention although it is good practice to report those who express no opinion as a percentage of the total, e.g. 'The 'outs' had a ten point lead over the 'ins', after reallocating 15% who said they did not intend to vote, were undecided or refused to express a view.'

Reallocation is done by repercentaging the share figures for each party on the base of those expressing voting intention, e.g. dividing 32% by 83%, 30% by 83%, 20% by 83% and 1% by 83% as in the example below:

Q. *'How would you vote if there were a general election tomorrow?'*

	Poll %	Reallocated %
Conservative	30	36
Labour	33	40
Liberal Democrat	18	22
Other	2	2
Voting Intentions	83	100
Would not vote	8	(8)
Undecided	6	(6)
Refused	3	(3)

This reallocation should **always** be done when comparing voting intention to actual voting results and never be done at any other time.

Technical Details

Every report of a public opinion poll, whether a British poll or foreign, political or social, should carry the size of the sample, the number of sampling points, the universe sampled, the dates of fieldwork and the methodology. It is good practice also to report if the figures were weighted, and if voting intention is part of the poll, the percentage of 'don't knows' which have been reallocated in calculating voting intention. One form of wording might read: ' The poll was carried out by XXX, exclusively for YYY newspaper among a quota sample of 1,248 British adults 18 and over in 123 constituency sampling points throughout Great Britain on 12-15 November 1993. The data were weighted to reflect the profile of the population.'

GLOSSARY

Attrition

In panel surveys (q.v.), the gradual loss of panel members over time, so that the number interviewed on later waves of a panel study is lower than the original baseline sample. Since the factors which cause attrition (unwillingness to continue to be interviewed or inability to be contacted by the pollsters) are not random and may well be correlated with attitudes to some of the issues being studied, attrition may make a panel steadily less representative and less reliable.

Baseline

The initial sample on which a panel survey is based.

CAPI

Computer Assisted Personal Interviewing - a polling technique where the traditional printed questionnaires are replaced by programming the questions into a portable personal computer. This frees the interviewer from responsibility for administering question order and routing, and since responses are entered directly onto the computer obviates the need for an extra data entry phase after interviews are completed.

Churners

Electors who in the course of an election campaign 'enter' or 'leave' the major party battle, either by switching to or from minor parties (i.e. all except Conservative, Labour and Liberal Democrat) or by becoming or ceasing to be 'don't knows'. (For example, an elector who does not originally intend to vote but subsequently decides he will vote Conservative is a churner; so is one who originally intends to vote Conservative but later has misgivings and is no longer sure which way he will vote, or decides to vote for the Scottish National Party. However, if he switched his support from the Conservatives to the Liberal Democrats, he would be a 'switcher', q.v., not a churner.)

Clustering

Selecting those to be interviewed in two or more stages, where the first stage selects a limited number of geographical areas (e.g. constituencies or polling districts) and individuals are then selected from within these areas.

Cross-section poll

A poll for which a new sample, representative of the population, is drawn. (See also tracking poll, panel survey).

Don't knows

A term sometimes used ambiguously. Generally, the name applied to respondents to a survey who do not give a positive answer. In voting intention surveys, these comprise three groups who fail to name a party even after 'squeezing'- those who say they are undecided which way they will vote, those who say they will not vote and those who refuse to say how they will vote. These are all normally excluded from the percentages in the published voting intention figures. The term is also, perhaps more frequently, used more specifically to refer only to 'undecided' respondents.

Median

A form of 'average' which is not distorted by extreme high or low figures. The median of a series of numbers is the middle of the series when all are arranged in order. For example, the mean (average) of the numbers

2, 3, 4, 5, 16

is 6 (the sum, 30, divided by the number of cases, 5), but the median is 4. For some purposes the median is a more suitable measure of the most typical member of a series than the mean.

Panel survey

A series of polls in which the same sample (as far as possible) is interviewed a number of times, allowing changes in the opinions of individuals to be measured and if necessary followed up. Each individual poll in the series is usually called a 'wave' of interviewing.

Probability sample

An alternative name for a random sample (q.v.)

Quota sample

A sample selected by a sampling method in which the interviewer is free to choose any person to interview within the confines of a sub-division of the population into pre-determined subgroups whose sizes reflect the proportion of the population that they comprise. For example, an interviewer might be instructed to interview within a given geographical area, such as a parliamentary constituency, and to carry out twelve interviews of which six should be with men and six with women, 2 with ABs, 4 with C1s, 3 with C2s and 3 with DEs. The intention is to ensure a sample that can be readily contacted while remaining representative of the most important subdivisions of the population.

Random sample

A sample that is systematically drawn to ensure that each member of the population being sampled has an equal probability of selection. (Random does not mean haphazard). Interviewers are required to contact a pre-determined list of persons or addresses.

Respondent

A person interviewed in, and contributing responses towards, a survey.

Rogue poll

A poll whose results are far out of line by a statistical freak. Since the accuracy of all sample surveys is dependent on probabilities, it is inevitable that occasionally the law of averages throws up a freak sample outside the normal margins of error. (For further discussion of this point see Chapter 5.)

Sampling error

Any failure of a sample to be representative of the population it is intended to represent. This may be sampling variation - the operation of the law of averages - or sampling bias - a failure in the sample design or methodology making some individuals systematically more likely to be selected than others, making the sample unrepresentative of the population in some particular respect.

Sampling frame

A physical representation of the population (such as a list of electors, enumeration districts, postal addresses, etc) from which individuals to be interviewed are selected.

Social Grade

A schema for classifying the population by the occupation of the Head of Household, widely used in market research. For further details see paragraph 166 *et seq.*

Swing

A summary measure of net change in support for two parties. The swing from B to A is the change in A's share of the vote minus the change in B's share of the vote, divided by two. Alternatively, it can be calculated as half the change in A's lead over B, measured in terms of the percentage of the vote.

Example of swing

	Election 1 %	Election 2 %	Change %
Party A	41	42	+1
Party B	39	36	-3
Party C	20	22	

2% swing
from B to A

Switchers

Electors who in the course of an election campaign switch their support from one of the three major parties to another. The term is most frequently used in discussing panel studies, when it refers to those members of the panel who say that their intention is to vote for one major party on one wave of interviewing and at a subsequent wave name a different major party. Switchers are distinguished from churners, q.v.

Tracking poll

One of a series of polls (normally cross-section polls) intended to measure changes in attitudes to the same question over a period of time.

Weighting

A mathematical adjustment of survey figures, making the answers of some respondents 'worth more' than others, to correct imperfections or inequalities in the sample. Whatever the means of drawing a survey sample, it is likely that respondents will not be precisely representative of the population as a whole in terms of the proportions falling into various sub groups; weighting is the means of correcting this. For example, if the sample has only 15% who live in council housing, and it is known that 19% of the population as a whole live in council housing we *weight by housing tenure,* giving greater weight to the responses of the council tenants in the sample so as to redress the balance, in this case by giving each council tenant's responses a value of 19/15 when adding up the responses of the whole sample. (The calculations are of course done by computer.) It is possible to weight simultaneously by a number of variables, although this is a more complex operation.

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