# WHO MISLED WHOM ? THE POLLS AND THE VOTERS IN THE 1992 BRITISH ELECTION

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The preelection polls failed badly to predict the result of the 1992 British general election. The final polls predicted a 1.4% Labour lead in the popular vote, on average. But the Conservatives won by 7.6%, "by far the largest discrepancy in a British general election since polling began" (Crewe, 1992).

This paper investigates the causes of this 9% discrepancy between the polls' predictions of the Conservative-Labour gap and the election result. Various familiar explanations for the large discrepancy are investigated, and an important new one - sampling bias by the polls throughout the campaign period. The paper concludes that present polling methods have serious shortcomings, and methodological improvements are suggested. And these shortcomings are not new. British polling methodology has changed little since the 1970s, despite warnings (eg Collins, 1988) that the techniques were much in need of refreshment.

### *The Polls*

There were 56 national voting intention polls carried out in the 29 days between the March 11 announcement of the general election, and polling day on April 9. The great majority of these were conducted by Britain's five major polling organisations - Gallup, Harris, International Communications and Marketing (ICM), Market and Opinion Research International (MORI) and National Opinion Polls (NOP) - and were commissioned by the mass media, mostly the national press. Since two of these 56 polls were based on samples of fewer than 600 respondents, we will restrict our comments to the remaining 54. Six were telephone polls, the rest face-to-face. There is a case for excluding the telephone polls, and concentrating on the main face-to-face interview methodology, but the results produced by different modes are sufficiently consistent for this to make no material difference to the argument of this paper.

We ignore regional polls, which were conducted principally in Scotland, the private polls carried out for the political parties, and the exit polls on election day.

Except for two panel surveys (each with four waves), every poll used a fresh sample, generally of around 1000 respondents or more. In the last week of the campaign, most increased their samples to around 2000.

All the polls were based on quota, as opposed to probability, sampling methods. That is, interviewers in a sample of parliamentary constituencies were issued with target numbers of various categories of respondent (e.g. by gender and by each of several age bands and social grades), and instructed to locate and interview people within those categories. Individual respondents are not preselected - the choice within category is left entirely up to the interviewers. For anyone who refuses or who is unavailable then and there, another person with the same characteristics is substituted. So quota sampling is based on the premise that people within each specified category (quota control) are interchangeable. The obvious advantage of quota sampling is speed. But there are also obvious disadvantages, which we come to later.

Most of the face-to-face interviewing took place in the streets, although MORI and NOP required a minimum number of in-home interviews, and Harris conducted all interviews in-home. Quota controls and targets were usually based on estimates from the continuous National Readership Survey, a large national market research survey (achieved N = 29,000), based on a probability sample [1].

The wording and ordering of the voting intention question varied slightly between polling organisations, as did the base on which party shares were calculated. MORI and ICM filtered out people who said they were not registered to vote; others did not[2]. Nearly all polls tried to "squeeze" a voting intention from the "don't knows". And all then excluded from the base the persistent "don't knows", as well as those who declined to disclose their intentions, as if their influence on the result would be nil. The published headline figures were therefore based only on those people who were able and willing to disclose their voting intention.

### *The Poll Results*

Table 1 shows the findings of the voting intention questions from the 54 campaign polls. The average levels of party support are shown first for all the polls, then divided into four time periods corresponding roughly to each of the 4 weeks of the campaign, and the "final" polls (the so-called "predictive" polls, for which field work ended on April 7/8). The penultimate row shows the election result itself as expressed in the popular vote in Great Britain (excluding Northern Ireland).

The last row of table 1 shows the gulf between the average final poll predictions and the actual result. Note that the "predictive" polls were barely closer to the result than those of the earlier weeks.

Moreover, looking at the gap between Conservative and Labour, there is no evidence of any trend over the

*Table 1. Campaign Polls: Week-by-week Voting Intentions (Percentages)*

| | No of Polls | Voting Intention: Conservative | Labour | Liberal Democrat | Other Party |
|---|---|---|---|---|---|
| ALL POLLS | 54 | 38.2 | 40.0 | 17.7 | 4.1 |
| Field work end dates: | | | | | |
| March 11-17 | 11 | 39.2 | 40.8 | 15.5 | 4.4 |
| March 18-24 | 15 | 39.0 | 40.2 | 16.8 | 3.9 |
| March 25-31 | 11 | 37.5 | 40.1 | 17.9 | 4.4 |
| April 1-6 | 11 | 36.9 | 39.0 | 19.9 | 4.3 |
| April 7-8 (final polls) | 6 | 38.1 | 39.5 | 19.1 | 3.2 |
| GB: election result | | 42.8 | 35.2 | 18.3 | 3.7 |
| Discrepancy: final polls | | -4.7 | +4.3 | +0.8 | -0.5 |

period. An examination of the full sequence of poll results suggests they are just as variable within a narrow time segment as they are overall. Polls taken on the same day are likely to differ from one another almost as much as polls taken at random from the entire sequence (and as much as the final polls at each of the last four British general elections - after allowing for sample size differences). The fluctuations from poll to poll are consistent with the hypothesis that they are due solely to sampling error (see also Collins, in press), and that the election campaign saw little or no actual change in the Conservative lead.

Given that the polls showed no movement *during* the campaign in the gap between the two main parties, it seems unlikely that there would have been a sudden, unprecedentedly large shift of support away from Labour and toward the Conservatives between the time of the final polls and the election itself - a mere day or two.

***What Went Wrong?***

In looking for further explanations for the inaccuracy of the polls, polling company effects can quickly be ruled out. All the companies got similar average results, so differences in methodology between the companies were unlikely to yield any explanation. We are looking for effects that applied more or less equally to all polling companies.

Immediately after the election the Market Research Society (MRS), worried at a possible loss of public confidence in surveys, set up an inquiry into the failure of the polls. Their initial report (Market Research Society, 1992) estimates that a combination of late swing, differential registration, and differential refusals to the voting intention question, might account for around half the error of the polls. The rest remains unexplained.

The notion of "late swing", which has been widely proposed as a major factor in the difference between the polls and the election results, is not new. In the 1970 general election, a Conservative party victory was preceded by a set of polls consistently showing Labour in front. An inquiry set up at that time by the MRS (1972) concluded that late swing was in part to blame. In those days, however, final polls were not so close to election day. All but one completed field work two days before the election, and the late poll came closest to the result. Since then, polling has continued as close as possible to election day. This time, with polling taking place up to the evening before the election, the swing would have had to be unprecedentedly late and unprecedentedly large to account for the discrepancy.

Commentators on the 1992 result seem to have used the term "swing" in a variety of senses, ranging from late decision-making (differential party support from the "don't knows"), to a change of mind on the part of voters (perhaps as a result of some campaign event or eleventh-hour change of heart). It has also been used to connote dissonance, that is, a gap between what people think they will do and what they will actually do, regardless of when the election day arrives. Most party canvassers have come across this phenomenon, when dyed-in-the-wool supporters of their own party protest that "this time" they will desert to the opposite party. In the end, party identification will tend to draw them back, or at least stop them from going to the "opposite" party; hence the large vote that centre parties in Britain tend to get in elections (mostly by-elections) when for one reason or another traditional party loyalties are under strain. "Late swing" has even been used to refer

to differential party support from the "won'tsays",and differential turnout.

The term "swing" is, of course, also used by psephologists to quantify shifts in vote. In that context it is defined as one half of the change in the gap between parties: e.g. a swing of 2% occurs if 2% of the electorate deserts one party for another, narrowing the gap between them by 4%.

But neither this precise meaning of the term nor its various looser meanings are helpful for the purposes of this paper. Instead we prefer to look one by one at the possible factors - whether or not they may be described as late movement - that might have contributed to a difference between the polls and the actual results. We discuss them in three broad groups.

The first group ("party switchers", "don't knows" and "won't says") comprises cases, where, for various reasons, the answer given by a potential voter to a preelection poll does not predict his or her eventual vote. Included here are people who in the event vote for Party Y, having said in advance they would vote for Party X (whether because they deliberately wanted to mislead the pollsters, or because of dissonance, or because they consciously changed their mind), in addition to those who cannot or will not nominate a party (the "don't knows" and the "won't says"). The second group (differential propensities or opportunities to vote) contains effects to do with the polls' inability to predict differential turnout by one or other party's supporters, the differential impact of overseas voters on the ballot, and differential registration. The third group of issues (nonresponse and selection biases), are to do with sampling.

The best evidence available on these first two groups of issues comes from repeat interview surveys. This paper considers three studies: the panel surveys carried out by MORI for the *Sunday Times*, a two-wave survey carried out by ICM for *The Guardian*, and the Social and Community Planning Research/Nuffield College British General Election Study (BGES) panel survey. These three studies are described in more detail elsewhere - see, e.g.,the appendix to Jowell et al (1993).

By analysing vote transition matrices from these studies, the magnitude and direction of effects can be estimated. It should be noted, however, that persistent lying, in which those interviewed lie not only in the preelection poll but also in the postelection poll, would not be detectable in the data reported.

*Party Switchers*

There will always be people who consciously change their minds between the time of a poll and election day. Others whose actual voting behaviour differs from their intention because of what we have described as dissonance may go through a different process (less self-conscious and more or less predestined), but the impact on forecasting would be much the same. There may also be people who deliberately lie to pollsters.

All the studies showed little evidence of direct switching between parties. The proportion of respondents who reported voting for a different party from the one they had intended to ranged from 4.9% on the ICM study, through 7.4% on the BGES panel to 9.7% on the MORI study. In each case there is a very modest net shift away from Labour, but none of the estimated changes was significantly different from zero.

Using the ICM study as an example, estimation of "swing"(in the more precise sense of the term referred to earlier) proceeds as follows: 1.9% of respondents who revealed a voting intention shifted to the Conservatives from other parties, while 1.0% shifted the other way, making a net 0.9% Conservative gain. In similar fashion, 1.2% shifted to Labour, and 1.3% from Labour, a net 0.1% loss. So the Conservative-Labour gap increased by 1.0%, a swing of 0.5%. The MORI panel also revealed a net swing from Labour to Conservative of 0.5%, and the BGES panel showed 0.7%. So all the evidence suggests that any late swing to the Conservatives was modest indeed. These estimates of swing are summarised in table 2.

*Don't Knows*

These studies can also be used to examine whether the "don't knows" did actually vote, and whether or not their votes were cast in the same proportions as others - which is what poll forecasts rely upon. The data suggest that the "don't knows" were slightly more likely to vote Conservative, but the differences are small. In the BGES panel, of the 8.2% of people who were "don't knows" in the pre-election interview, around one in four (27%) voted Conservative, and around one in five (18%) voted Labour. So if all the "don't knows" who subsequently revealed their vote had been able to say beforehand for which party they would vote, the Conservative "lead" would have increased by 0.4%.

The MORI panel found very few "don't knows" in its final preelection wave - just 11 - so little can be concluded about them. And in the ICM study, where 5.1% were "don't knows", the differences were even smaller than in the BGES panel: 26% voted Conservative, and 23% Labour. Prior knowledge of the votes of the "don't knows" would have increased the Conservative-Labour gap by just 0.1%.

The evidence from each study is scanty, and the vote distribution of "don't knows" is not significantly different from others. But, since all the evidence points in the same direction, it suggests that the very modest difference might be real. The "don't knows" appear to have been more likely to vote Conservative. These effects of the differential distribution of "don't knows" are also summarised in table 2.

*Table 2. Factors Affecting Pre-Election Estimates of the Conservative-Labour Percentage-Point Gap*

| Study | Field work dates: Pre-election | Post-election | n[1] | Switching (2 x swing) | "Don't Knows" | Non-Disclosers | Differential Turnout | Total Effect |
|---|---|---|---|---|---|---|---|---|
| MORI Panel | 4/1-3 | 4/10 | 833 | -1.0 | -0.4 | -0.3 | -1.2 | -2.9 |
| ICM Recall | 4/8 | 4/10-22 | 1,203 | -1.0 | -0.1 | -1.6 | -0.1 | -2.8 |
| BGES Panel | 3/19-4/8 | 4/10-12 | 1,203 | -1.4 | -0.4 | -2.0 | +1.4 | -2.4 |

[1] The number of sample members responding to both the pre- and post-election waves.

Note: A negative number indicates that the factor (eg. "switching") had the effect of reducing the preelection estimate of the Conservative lead, by the stated amount (percentage points).

*Won't Says*

A stronger pattern emerges for those who declined in the preelection interview to disclose their voting intention but who were willing after the event to say which party they had voted for (the "won't says", or nondisclosers). The BGES panel showed that a substantial majority of these people voted Conservative: 1.7% of the sample had refused to reveal their intention but said afterwards that they had voted Conservative, while 0.4% refused to reveal their intention and then voted Labour. Again, the ICM survey showed a similar pattern, with 1.8% of voters refusing to reveal their intention but subsequently saying they voted Conservative, and 0.8% saying they voted Labour. The MORI panel found only 0.3% were nondisclosers who voted Conservative, and none voted Labour. Again all the evidence points in the same direction: non-disclosers are disproportionately Conservative voters.

Moreover, examination of the 1987 party preferences and the political values of BGES panel members who refused to disclose at both the campaign and post-election waves, shows that these non-disclosers ("double non-disclosers") are significantly more pro-Conservative and generally more right wing than average. They are more likely, for instance, to oppose redistributive and prowelfare policies. There is thus little doubt that non-disclosers are more Conservative-inclined than others, and this was also true in the 1987 election[3].

Table 2 presents estimates of the effects of non-disclosers. These estimates are based on the vote of those who, after the event, revealed their party ("single nondisclosers"), and on the assumption that double nondisclosers distributed their votes in the same way as single nondisclosers on the same study. If anything, analysis of attitudinal data suggests that double nondisclosers could be even more heavily Conservative than single nondisclosers, so these estimates may actually understate the effects of nondisclosers.

*The "Shame" Factor*

An alternative explanation for the polls' failure in 1992, put forward by, for instance, Harris (1992), is that large numbers of people lied to the pollsters this time round, perhaps as a result of a so-called "shame factor" - a notion of "closet" Conservatives who were too ashamed to admit it to the pollsters. This may have arisen in 1992, it is argued, because the Labour party were proposing to raise taxes to pay for increased social spending while the Conservatives were decidedly not. Thus a large number of, say, working class voters who were intending to abandon their perceived class interest in favour of their perceived self interest might have been too ashamed to admit it. In addition, perhaps, some Conservatives may have lied to pollsters as a protest about the effects of government policies, while never intending to vote for any other party than their own.

Any people who lied in the preelection polls but revealed their vote after the event have already been allowed for in the analysis. They constitute part of the set of people who said they intended to do one thing, and then did another.

Effects from people lying consistently, both before and after the event, cannot be quantified. If there were such people, and they were disproportionately Conservative voters, then this would have contributed to the discrepancy between the polls and the election result. But we think it unlikely to have been a serious factor. Not only is there ample evidence that people do generally try to tell the truth to survey interviewers, but we doubt that a shame factor - if it exists at all - would have suddenly come into play during the period of a Major-led Conservative government when it apparently failed to appear during successive Thatcher-led governments.

*Self-Prediction of Turnout*

Some people state a voting intention and do not in fact turn out to vote; others say they will not vote and

subsequently do. The panel studies give conflicting evidence about the effect of respondents' inability to predict whether they will vote. The MORI data support the notion that it helped depress the Conservative lead, the BGES panel indicates the opposite, and the ICM study suggests no net effect. It is therefore possible that people's inability to give an accurate forecast of their likelihood of voting might have depressed the polls' estimates of the Conservative-Labour difference, but the effect, if any, is likely to have been small.

*Overseas Voters*

The 1989 Representation of the People Act extended eligibility to vote to a greater number of British citizens resident abroad. Such people are most likely to vote Conservative, and of course they are not part of the universe from which the pollsters sample. But the total number of voters registered overseas was under 32,000, out of an electorate of some 44.5 million. So even if they had all voted Conservative, the effect on the overall vote distribution would have been negligible.

*Differential Registration*

Another suggested hypothesis is that Labour supporters may have been less likely than Conservatives to be registered. But this would only have affected the polls if unregistered people stated a voting intention, rather than saying they would not vote. And even then, if such people in the panel surveys subsequently admitted they had not voted, the effect will have already been taken account of in the above analysis of turnout. Only if these people reported to have voted in a way consistent with their stated intention would they not have already been accounted for in our analysis. Then, they would come under our category of "consistent liars".

*Non-Response and Selection Bias*

By summing the estimated individual effects, each of the three surveys provides an estimate of the net effect of all the factors considered so far. These totals appear as the last column of table 2, and range from 2.4% to 2.9%. So all these factors cannot between them account for all of the discrepancy between the poll forecasts and the election result, unless consistent lying fills the gap - which is too implausible to take seriously, for reasons already noted. What remains is the possibility that the samples were biased. So are the quota control procedures of the polls, which differ only slightly between organisations, adequate to the task they set themselves ?

Leaving aside questions about the lack of theoretical underpinning for quota sampling, it must be conceded that the time constraints imposed by the media during election campaigns rule out the use of high quality random samples using face-to-face interviews at electors' homes. And telephone polling is virtually impossible, given the biased penetration of phone ownership, the inherent resistance of the British telephone numbering system to RDD, and the absence from directories of around 25% of phone owners.

In quota sampling, the selection of individual respondents is left to the interviewer, and there is no a priori reason why such a selection method should lead to representative samples. However, the possibility of bias may be reduced first by selecting areas on a probability basis, and second by the use of appropriate quota controls. Quota controls are in many ways analogous to explicit stratification in a random sample. In both cases they will not have a beneficial effect unless they are related to the dependent variable. They must, of course, also be implemented correctly in the sense that the quota or stratum proportions must match the population proportions (after any appropriate weighting has been applied).

The polls typically use as quota controls gender, age and "Social Grade". Some polls also control working status; others insist that all (or a specified proportion) of their interviews should be conducted at people's homes rather than in-street; some require a proportion of interviews outside working hours - all in an attempt to replicate social and demographic factors that the pollsters believe related to voting behaviour.

However appropriate and well-applied the quota controls may be, there remains the possibility of biased selection of individuals within cells. Of course, this will not be the case if all individuals within a cell have the same relevant behaviour and opinions, nor will it matter if the selection biases are uncorrelated with the behaviour and opinions under study, but the first is demonstrably untrue and the second seems improbable.

One potential cause of selection bias stems from refusals to take part. Refusal rates in quota samples tend to be very high in comparison with random samples - perhaps at least 40-50%. There are, in addition, noncontacts among people approached at home, and these people are not revisited or pursued. (In street interviewing, there are no noncontacts in this sense, but it is highly likely that some people "avoid" interviewers.) With such a large total nonresponse, systematic bias must be a serious possibility. Weighting the data back to the quota variables, and others, can help to mitigate bias, but only if the weighting variables are strongly correlated with vote.

One of the more obvious potential sources of bias in quota samples is that interviewers are not required to call back on those who are not successfully interviewed on first approach. Thus immediately unavailable and unwilling people are substituted by immediately available and willing ones. While the potential bias is reduced by the application of the quota controls, this will not remove any bias present within the cells.

To estimate the possible effect of this bias, we

examined data from the 1992 British General Election Study (N=2,855),a random cross-section sample. The subsample of respondents to the 1992 BGES who were interviewed at the interviewer's first call at an address, were rim-weighted to simulate the effect of applying quota controls[4]. Our hypothesis was that respondents interviewed in our random sample at first call (analagous to all quota-sampled respondents) would not be representative of the total sample in respect of voting behaviour. This proved to be the case. After weighting, the first call sample still showed a distinct pro-Labour bias - of some 6% in the gap between the parties - compared with the whole sample. To the extent that this replicates the biases of quota sampling, it is a salutary reminder of an inherent weakness of nonrandom techniques.

There are also two pieces of evidence to indicate that sample bias may have been worse in 1992 than in previous elections.

First, the bias towards Conservative voting among people who decline to disclose their party beforehand, but who are prepared to do so after the event, is getting greater: it was larger in 1987 than in 1983, and greater still in 1992 (Heath et al, 1992). And it is a reasonable supposition that nondisclosers are a subclass of a larger group who are disinclined to participate in political surveys or polls at all. Second, the first-call pro-Labour bias measured on the 1987 BGES data was 4% lower than that found in 1992. These two findings suggest differential nonresponse may be getting worse.

### *Conclusions*

It seems the polls' downfall was caused partly by people not voting in the way they said they would ("switching" and "turnout"), and partly by the way pollsters treat the "don't know"s and "won't say"s, but perhaps mainly by selection or nonresponse biases.

With the benefit of hindsight, it appears that the Conservatives were actually ahead throughout the campaign, and would have won whenever the election had been held in that period.

### *Implications*

British polling organisations should now be doing more methodological work. Most importantly, to tackle the worry of sampling bias, they should be investigating ways of developing better quotas, and ways of reducing the numbers of refusals and non-contacts. They might also consider inventive ways of using random sampling techniques. And they should also look at ways of modelling the likely voting behaviour of "don't know"s and "won't say"s, and making more use of other survey variables to assess the robustness of reported voting intentions, rather than relying on one simple question.

An encouraging start has been made in post-election experimental work by ICM (1992). First, ICM now ask people's voting intentions by means of a ballot box technique in order to reduce the number of non-disclosers. Second, on the basis of preliminary findings, ICM will in future first ask their respondents whether they definitely intend to vote, and only then ask voting intention. But there remains much work to be done.

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1. NOP's targets came from their own omnibus survey, based on a probability sample. MORI's were based on 1981 census constituency data, updated by regional OPCS population estimates (age and gender), and NRS data.

2. NOP's early polls also filtered out the unregistered, but this practice was discontinued when analysis showed it made no difference.

3. Of course, this relationship might vary over time. The underlying phenomenon might be that refusals are more common among supporters of, e.g., the party in power, or the "underdog", rather than the Conservative party per se.

4. Quotas varied slightly between the polling companies. The figures presented here are based on 2 categories of social grade, 4 age bands, 2 working status bands, and gender, using NOP targets. Slight variants produced similar results. Social grade of head of household is not collected directly on the BGES, but has been derived from a combination of questions. The derivation does not produce an exact replica of social grade, but only small subgroups could have been misclassified; and any misclassification would not be detrimental to comparisons of first call respondents with others.