

# COUNTING LIKELY VOTERS IN GALLUP'S TRACKING POLL

By Robert S. Erikson

In the final week of its 1992 tracking poll for CNN and *USA Today*, Gallup changed its methodology to report the vote intentions of likely voters rather than registered voters. This change cut about four points from Bill Clinton's margin over George Bush in the CNN/*USA* tracking poll. Initiated just when the race was actually tightening, the methodological switch attracted considerable attention and some criticism. Gallup had switched to a likely-voter base in previous elections without similar controversy. What was different about 1992?

In the last issue (Vol. 4, No. 2) of *Public Perspective*, Larry Hugick of Gallup, Guy Molyneux of CNN, and Jim Norman of *USA Today* discuss the performance of the 1992 tracking poll and defend Gallup's final-week switch to a likely-voters-only survey. The article counters several objections that have been made to the Gallup procedure. But the real problem was that Gallup used a crude and inaccurate method of correcting for likely voters. The result of this mistake was to seriously *overcorrect*—actually biasing the result in Bush's favor.

To identify likely voters, Gallup probed registered voters with questions about frequency of past voting, interest in the campaign, knowledge about the location of their local voting place, and the like. Then, in addition to approximately 25% of their initial sample who said they weren't registered, they culled a further 15% or so who claimed registration but were in the bottom percentiles in terms of the likely voter probes. As a proportion of the original sample contacted, the likely electorate represented a residue of about 60%, which approximated the anticipated turnout in the election.

Gallup, in effect, placed the vote intentions of registered voters into two piles. One pile, the preferences of most likely voters, were fully counted. The other pile, the preferences of those who were less likely to vote, were ignored totally. As is common knowledge among survey statisticians, this procedure overcorrects for any vote differential between likely and less likely voters. Less likely voters should be counted to some extent, because some do vote; similarly, likely voters should be discounted somewhat because some fail actually to cast a ballot.

Instead of weighting each respondent either 1.0 or 0.0, the appropriate

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procedure would have been to weight respondents according to their estimated likelihood of voting. For instance, if the probes indicate that the respondent fits the profile of a person with a 70% chance of voting, that person should be weighted .70. Another might be weighed at .40, and so on. A weighted mix of 100 (.70) respondents and 100 (.40) respondents would yield the right mix of 55% voters and 45% nonvoters, with the preferences of the .70's counting more than those of the .40's by the proper ratio of 7 to 4.

Gallup's either-or procedure totally discounted the preferences of voters with attributes of people less likely to vote, such as those who had not voted in the past. The selection bias is largely irrelevant if less-likely voters and likely voters differ little in their preferences. And in

fact, the vote preferences of likely voters and less likely voters generally do converge. Thus, it is normally of no practical concern whether or not to correct for likely voters, let alone how. Indeed, the minutia of the exact weighting procedure has attracted little controversy in past election campaigns. But selection bias matters whenever the less likely voters are different in their preferences from those more likely to vote. The election of 1992 was such a case.

To see why, consider some elementary math. On the day when Gallup began to report the preferences of likely voters, the Clinton/Bush gap was +6 among registered but only +2 among the selected likelies. From this information, we can figure the gap among less-likelies to be a whopping +22 or thereabout.

**Mathematically:**

**Prop. Less Likely \* Gap among Less Likely + Prop. Likely \* Gap among Likely = Gap among Registered.**

**For the numbers to add up, the gap among less likely voters must have been about +22:**

$$.2 * (+22) + .8 * (+2) = +6$$

Thus, Gallup fully counted about 80% of the registered respondents, who collectively gave Clinton a slim +2 edge, but discarded the remaining 20% who collectively were for Clinton by a +22 margin! If none of the discarded less-likely voters were to vote, and all of the retained likely voters *were*, no problem is presented. But no survey instrument can detect nonvoting with anything approximating such accuracy. Since about 80% of registered voters *do* vote in presidential elections, it is very difficult to figure in advance which ones will be in the missing 20% on elec-

tion day. For example, the vast majority of the newly registered will vote, albeit at a slightly lower rate than those who have been habitual voters in the past.

An arbitrary but reasonable guess is that about 85% of the likely voters voted while 60% of the less likely voted. (These numbers add up to 80% of registered voting.) If so, the true projected margin would be neither the +2 for likely voters alone nor the +6 for registered voters as a group, but instead +5.

$$\frac{.8 * .85 * (+2) + .2 * .60 * (+22)}{.8} = +5$$

This estimate is much closer to the registered voter margin of +6 than to the likely voter margin of +2. This +5 estimate could be achieved by properly weighting respondents by their likelihood of voting.

Did those less likely voters who did go to the polls in 1992 vote disproportionately Democratic? At least for those who were judged less likely because they were first-time voters, we can get some help on this from exit poll data. According to the VRS exit poll, self-described first-time voters comprised 11% of the electorate and supported Clinton by a strong +18 margin—which is generally consistent

with the argument made here. For the overall numbers to add up to the national +5 Clinton margin, the 89% of the electorate who had voted previously must have given Clinton roughly a +3 margin. This is an unusually large gap in the vote by new vs. established voters.

The moral of the story is twofold. First, surveys usually do not need to correct for turnout beyond screening out non-registrants. Second, in the rare case where screening for turnout matters, it is better not to correct at all than to correct incorrectly.

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## COUNTING LIKELY VOTERS: A REPLY TO ERIKSON

*By G. Donald Ferree, Jr.*

Robert Erikson raises provocative questions about the propriety of screening for “likely voters” and its impact on the projections of the vote. These questions are worthy of close attention in the survey community. But his conclusion—that any scheme other than assigning a probability of voting to each respondent and then weighting on the basis of that probability is methodologically indefensible—is seriously flawed.

Erikson assumes that because introducing a likely voter screen lowered the Clinton margin (as compared to all registered voters) at the start of the week prior to the election, it must follow that this screen systematically overstated Bush’s vote. But, as Hugick *et al.* noted in their piece (see *Public Perspective*, January/February 1993, pp. 12-13), the Gallup surveys showed a widening gap later in the week—as did others. The final Gallup tally in fact *overstated* Clinton’s margin. If Erikson is right, and adopting

a likelihood-weighting procedure for all respondents would have boosted Clinton’s estimated vote, Gallup’s use of this procedure would have left it with an even higher overestimation!

Some of Erikson’s argument is unobjectionable. Compared to the possible electorate, the actual electorate consists

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of 100% of those who had a certainty (p=1.0) of voting, 90% of those who actually had a .9 chance of voting, 80% of those with a .8 chance of voting, and so on

down.<sup>1</sup> If there *were* a way of accurately assessing this actual probability prior to the election, no sane researcher could possibly object to weighting respondents by this factor.

But what if, as is undoubtedly the case, it’s not possible to determine accurately individuals’ likelihood of voting? Does *any* attempt, however inaccurate, do more good than harm, as Erikson would seem to assert? The answer depends on such factors as how similar turnout patterns are across elections, how estimated turnout relates to preference, and whether events occurring between the survey and election day change individuals’ likelihood of voting.

Many concerns here parallel those that apply to any question of weighting. It is a commonplace that any actual sample may differ from the theoretical population from which it is drawn not only randomly (quantified in the so-called “margin of error”), but systemati-