Data Troubles: Explaining Discrepancies between Official Votes and Exit Polls in the 2004 Presidential Election

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It is a comment on the current lack of trust in the U.S. voting system that scientists and scholars in disciplines ranging from astrophysics to statistics ask me if the Presidential election in 2004 was stolen in Ohio. After butterfly ballots (Wand, Shotts, Sekhon, Mebane, Herron, and Brady 2001) and punchcards (Mebane 2004) in 2000, I suppose as consternating as it may be, the question has become reasonable to ask. If we value the legitimacy of our elections, it is of great importance that questions raised about discrepancies between official vote counts and exit polls be carefully examined. Questions about Ohio are particularly important because the state was pivotal in 2004. The authors—Kyle, Samuelson, Scheuren, Vicinanza and Mitofsky (henceforth KSSVM)—are to be commended for continuing to try to put the questions to rest even though their critics have at times been hysterical.

I agree with the authors that, unlike Florida in 2000, there is no scientific evidence that any of the reported irregularities in Ohio rose to the level of changing the outcome.[1] I also agree that given the grave and increasing difficulties with implementing exit polls, it is hardly surprising that exit polls are often erroneous. Given what we know, it appears to be the case that the official vote count for all of its difficulties was more reliable than the exit poll. This is not to minimize the issues which have been documented. For example, it has been shown that an insufficient number of voting machines was allocated to some heavily Democratic precincts which resulted in inordinately long lines and lost votes particularly among African-Americans (Mebane and Herron 2005).[2]

Notwithstanding these areas of agreement, I wish to critically discuss three of the core claims that KSSVM make. First, they argue that the entire discrepancy between the official vote count and the exit poll results can be explained by a partisan bias in nonresponse. Second, the authors contend that they are unable to release the exit poll data in an unaltered

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[1]The most thorough investigation to date has been a report commissioned by the Democratic National Committee. And the authors of that report concluded that the number of lost votes was enough to be "a serious concern, but not enough to change the election outcome in Ohio in 2004" (DNC 2005).

[2]It is important to note that there is no credible evidence that such a machine allocation decision was made in an effort to harm the Democratic vote. Indeed, in a meeting with the DNC task force this author attended, members of the DNC simply had no idea how voting machines were allocated in Ohio in 2004 or in previous elections such as 2000. Such a state of affairs highlights the importance of studying how to mitigate voting times as explored by another contribution in this volume: Allen and Bernshteyn (2006).

form because of privacy concerns. Third, the authors propose a way to detect some election irregularities.

The authors spend much of their article examining what could account for the poor performance of the exit polls. They offer three models for those who refused to respond to the exit poll: that nonrespondents were equally likely to be voters of either major party, that all nonresponders were Bush voters and finally that all nonresponders were Kerry voters. By going from the second to the third alternative they are able to bound the results for each precinct, and in Figure 1 they show that for 47 of the 49 precincts the blurred official vote proportion is within this bound. The authors conclude that "differences in attribution of refusals would be sufficient to account for the differences between exit poll estimate[s] and the blurred official results." This is indeed correct for 47 of the 49 precincts, but data provided in a preceding working paper produced by Edison Media Research and Mitofsky International (2005) makes it clear that the nonresponse issue is not straightforward. The precinct partisanship vs. completion rates table provided in the Edison-Mitofsky paper shows that there is no aggregate relationship between the completion rate and the partisan makeup of a precinct.[3] The fact that there is no aggregate relationship between partisanship and response rate does not mean that there is no such relationship at the individual level, but some model and some data need to be presented which square this circle.

There are of course many reasonable models of the exit poll bias consistent with the publicly available data such as one which conjectures that interviewers are more likely to select Democrats rather than Republicans to interview. This latter model would account for the fact that the bias is much higher for heavily Republican precincts even though the response rate is no lower. For example, nationally in precincts where the Kerry vote proportion was less than .2, the mean within precinct error (WPE)[4] was -10.0 percent while in precincts where the Kerry vote proportion was greater than 0.8, the mean WPE was

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[3]The authors, assert "[there] was no significant difference between the completion rates and the precinct partisanship" (Research and International 2005, 37).

[4]WPE is the average of the difference between the percentage margin between the leading candidates in the exit poll and the actual vote for all sample precincts in a state.

0.3 percent (Research and International 2005, 36).[5] My goal here is not to put forward a definitive explanation for the biases found in the 2004 exit polls, but to point out that we don't know what the best explanation is. The simplest model, Republicans simply have a higher nonresponse rate orthogonal to other covariates, does not appear to account for the observed patterns.

Problems with the simple model of nonresponse have led many to argue that the authors make the data available so people can conduct their own analyzes in the hopes of finding a more compelling model. For example, it would be interesting to see the table of interviewer characteristics by partisanship and error rate in Ohio.

This brings up the second issue: blurring of the partial microdata which has been released. Blurring is indeed a well establish method of protecting privacy while still providing data which contains some of the key information which researchers may need. It does, however, make certain kinds of analyze impossible to conduct.[6] For example, one may wish to determine if the exit poll results follow a (second digit) Benford's Law distribution and whether this is consistent or not with the official vote returns (Mebane 2006). Such an analysis is not possible given the blurring. The authors argue that the blurring is necessary because it would be possible in small highly skewed precincts to determine, for example, who exactly the few Democratic voters were. But the same issue arises in the official vote return records. Some precincts are so small (e.g., retirement homes) that it is often possible to identify individuals from public vote returns if one has some additional information. Of course the exit poll makes identification easier given the individual-level covariates it includes (e.g., age, gender, race). Thus, it would be possible to release the unblurred exit poll data but without the respondent's demographic covariates included—i.e., only report the respondents' reported votes. This would make the situation no different than the public voting records.

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[5]Moreover, the Edison-Mitofsky paper presents evidence that the error rate in precincts varies with interviewer characteristics such as age and training.

[6]As the National Research County (2000) report on "Improving Access to and Confidentiality of Research Data" notes: "Data alternation allows for broader dissemination, but may affect researchers' confidence in their modeling output and even the types of models that can be constructed" (29).

And such a data release would allow researchers to create a variety of tables that they may find interesting. One could also release data including covariates but only from precincts which are large and heterogeneous enough to protect the identity of respondents.

As well as releasing a pruned exit poll dataset, it would be possible to allow qualified researchers access to the full dataset in the same way that various highly sensitive Government census and administrative dataset are made available to qualified and screened researchers under strict confidentiality requirements (Brady, Grand, Powell, and Schink 2001). If sensitive administrative data, such as welfare, health and IRS tax data, can be obtained by qualified and carefully screened researchers, then it would seem that it should be possible for some appropriately screened researchers to access the full exit poll data.

My view is consistent with that of Bethlehem, Keller and Pannekoek (1990, 45) who writing in *JASA* note:

> [I]f microdata are released under the conditions that the data may be used for statistical purposes only and that no matching procedures may be carried out at the individual level, any huge effort to identify and disclose clearly shows malicious intent. [...] [W]e think disclosure protection for this kind of malpractice could and should be taken care of by legal arrangements, and not by restrictions on the data to be released.

Allowing researchers access to the data in a controlled setting has obvious financial costs as can be seen by the Census Research Data Centers initiative which allows researchers to go to various locations around the country to work with nonpublic census data after their research project has been approved. But certain academic centers have the experience and capacity to work with highly sensitive nonpublic microdata, and I am certain that they would be willing to provide the infrastructure needed to allow restricted access to the exit poll data given its importance.

The last issue I wish to discuss is the authors' attempt to search for substantial irregularities in the vote counting process. This effort has so little dispositive power that it may have been

better to omit it. It has only served as a lightening rod for critics.[7] The authors graph the proportion of votes that Bush got in 2000 versus the proportion he got in 2004 and conjecture that if significant irregularities "favoring the Republicans occurred in 2004 but not in 2000, that precincts in which Bush did much better in 2004 than in 2000 would also be precincts in which the exit poll more seriously overestimated Kerry's proportion of the official vote." But it is simply not true that irregularities favoring Bush would require that his two-party vote proportion be higher in 2004 in *any* precinct than it was 2000. For example, the random removal of votes cast in heavily Democratic precincts would not change (in expectation) the precinct level vote proportions, but could certainly change the outcome of the election. The state wide winner is not determined by simply averaging the precinct proportions and ignoring the number of ballots cast in each precinct.

Moreover, the entire art of electoral fraud, as ample evidence from history shows, is to manipulate the outcome only to the extent required. For example, note Lyndon Jonhson's two attempts to secure for himself a senate seat in part by fraud, the second of which was successful by a margin of 87 votes (Caro 1982, 1990). And very little manipulation may have been required in Ohio (not that there is any evidence that such manipulation occurred). Bush's vote proportion in 2000 was 49.97% which was sufficient for a plurality while he received 50.81% of the vote in 2004. Even if one were to assume that this entire increase was due to fraud, I do not see how an increase of 0.84% is large enough to be detected by the method the authors have outlined. At the very least the authors should try to evaluate the sensitivity of their method against various types of hypothetical fraud (e.g., fraud concentrated in Democratic precincts or, alternatively, in Republican or swing precincts). For example, are they by this method able to find the well documented irregularities in Florida in 2000—such as the problems caused by overvotes (Mebane 2004) and the butterfly ballot (Wand et al. 2001)? There are alternative methods which have been just so validated (even though they are far from perfect) and may be the authors should turn to them. For

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[7]For example, see `http://electionarchive.org`.

example, see Mebane and Sekhon (2004).

To be clear: I complete agree with the authors that "the available evidence from the exit polls does not support the conclusion that any such attempt [at fraud] succeeded *on a scale sufficient to alter the outcome of the election*" (emphasis in original). But I think that the authors need to be careful about what they are able to show and they are not. I think we are still awaiting a full explanation for why the exit polls performed so poorly.

Exit polls have always been as much art as science and their problems have been getting worse just as presidential elections have been getting closer. For example, the national average precinct WPE was -6.5 percent in 2004, larger than it has been in recent elections (Research and International 2005, 34). But a Democratic bias has existed at least since 1988 (Research and International 2005, 34). Exit polls are unreliable enough that they cannot be solely used to judge the validity of an election. But in conjunction with other information, exit polls may be of some value in judging the integrity of an election. As a country we need all of the extra information we can get in protecting the legitimacy of the vote count. The need to have checks on our voting system has if any thing grown in recent years with the rise of electronic voting machines which do not have an audit trail and which have been demonstrated to have serious security flaws.[8] It is unfortunate that exit polls have if anything decreased in reliability just as the need to be vigilant has increased. It is imperative the we do a better job of understating exactly why the exit polls are currently so unreliable and what we can do to fix them.

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[8]For example, see `http://accurate-voting.org/` and `http://www.blackboxvoting.org/`.

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