

**Beyond Exit Poll Fundamentalism:
Surveying the 2004 Election Debate**

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Beyond Exit Poll Fundamentalism

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Was the 2004 U.S. presidential election stolen? A vocal minority firmly believe that it was. More specifically (since “stolen” admits of various interpretations), some observers believe that John Kerry won a majority of votes actually cast (and would have won a majority of electoral votes), but that vote miscounts threw the election to George W. Bush. Observers adduce varied evidence for this proposition (e.g. Miller 2005), but many cite the exit polls as compelling summary evidence of decisive vote miscount, presumably due to massive fraud.¹

What do the exit polls tell us about the possibility of fraud? The interview data collected by Edison/Mitofsky (E/M) for the National Election Pool (NEP) indicated that John Kerry had approximately a three-point margin in the popular vote (E/M 2005, 20), and that he led (albeit within the margin of error) in four states that Bush eventually won – including, decisively, Ohio. The discrepancies between exit poll results and official returns cannot plausibly be attributed to sampling error alone. Facially, the discrepancies can plausibly be attributed to vote miscount. Alternatively, they can be attributed to some form or forms of error in the survey – most often, to a differential propensity of Bush voters and Kerry voters to consent to participate (non-response bias). Or of course they could be attributed to some combination of miscount and polling error. These alternative hypotheses do not evaluate themselves: analysts must bring other evidence and arguments to bear.

The belief system that I term “exit poll fundamentalism” posits that the answer is clear: Kerry won the popular vote, and the exit polls provide compelling evidence of the fact. Indeed, the evidence is so compelling that villains have worked to conceal and/or to obfuscate it. As one advocate states the importance of the exit polls: “Of all the evidence of election fraud, the exit polls are the unifying force, the only national evidence, the slam dunk indicating fraud, the one vote on election day with a COMPLETE PAPER TRAIL.” (“Autorank” 2005b)

Note well: “exit poll fundamentalism” does not refer to the hypothesis that Kerry received more votes, nor the belief or hypothesis that the exit polls evince fraud. These are empirical issues amenable to rational debate, and reasonable people may disagree. Still less does it refer to any and all criticisms of the 2004 election or of election systems. Exit poll fundamentalism as I have encountered it, and as I define it here, amounts to a closed belief system that forecloses further discourse and discovery.

This belief system, alien to most survey researchers, warrants attention, and its inaccurate assumptions should be challenged. Exit poll fundamentalism merits incisive critique – not to discourage serious inquiry into election integrity or to block election reforms, but to support

¹ The terms “vote miscount(s)” and “fraud” obviously are not synonymous. One might reasonably conclude that the exit polls provide compelling evidence of vote miscount but not of fraud. So far, I have never encountered anyone who believes that massive vote miscount occurred but does not attribute it to fraud. To anticipate the later discussion: in the context of actual debates about the 2004 election, the insistence that “vote miscount” does not imply “fraud” seems akin to the literally accurate insistence that “Intelligent Design Theory” does not entail belief in God. God does seem to be the Number One candidate for the role of designer, and I cannot descry a runner-up.

them. The *conviction* that “Kerry triumphed, and the exits tell me so” is based on poor reasoning and is likely to promote misdirected actions.

A sketch of “exit poll fundamentalism”

Because “fundamentalism” is widely and often carelessly employed as a pejorative, it seems useful to recall its historical origins before explaining my use of “exit poll fundamentalism.” Fundamentalism partly takes its name from a series of booklets, *The Fundamentals*, published in the United States around 1910 to enunciate a particular view of sound Christian doctrine. Early fundamentalists sought to affirm the authority of various beliefs that they regarded as essential to Christianity – in particular, the authority of the Protestant biblical canon. The Presbyterian Church in the USA in 1910 adopted the Doctrinal Deliverance, which enumerated five such “essential doctrine[s]” sometimes called the “five fundamentals,” beginning with the inerrancy of the “Holy Scriptures” (PCA Historical Center 2005).² Various articulations of fundamentalism generally share an emphasis on biblical authority, and in particular upon treating the Bible as an authoritative historical text in the face of modern science.

Fundamentalism’s conception of biblical “inerrancy” is a subtler matter than mere literalism. Fundamentalists do generally treat biblical narratives as historical rather than metaphorical. For instance, they generally believe that in the time of Noah, a great flood covered the whole earth. Most also believe that God created the heavens and earth in six days about 6,000 years ago (and thus are “Young Earth Creationists”). However, when a literal reading of biblical texts entails a contradiction, then fundamentalists are quite resourceful in harmonizing the texts, or at times selectively ignoring them.³

The archetypal manifestation of fundamentalism in scientific discourse is creation science, or its contemporary variant, Intelligent Design (ID) theory. The conclusions of creation science are circumscribed by its prior credal commitments: correct science must uphold the authority of Scripture (as elastically as that authority may be understood). Much creation or ID “science” proceeds by enumerating anomalies that alternative scientific accounts have (allegedly) not adequately explained, and then offering God – or an unnamed designer – as the explanation. The impulse to reconcile science and a particular understanding of religion, on terms dictated by the latter, has thrived in the United States for a century or more.

I define exit poll fundamentalism as a system of credal convictions – not a formal “creed,” to be sure, but a set of tenets that typically coexist. These tenets jointly elevate and defend the exit polls as authoritative evidence of fraud: perhaps not conclusive legal proof, but a basis for subjective confidence. Exit poll fundamentalists often portray themselves as “asking questions”

² The exact formulation is: “the Holy Spirit did so inspire, guide and move the writers of the Holy Scriptures as to keep them from error.”

³ For instance, on a literal reading, Genesis 1 states that all other living things were created before man, while Genesis 2 states that Adam was created, then other living things, and finally Eve. One might infer that the texts cannot reasonably be interpreted as authoritative history, but fundamentalists conclude instead that Genesis 2 should be translated so as to be consistent with Genesis 1 (e.g., Batten 1996). Given the premise of biblical inerrancy, this conclusion is quite reasonable. Likewise, given the premise of biblical inerrancy, there is no ‘real’ contradiction between the reference to “forty thousand stalls” in I Kings 4:26 and the reference to “four thousand stalls” in II Chronicles 9:25, although both refer to the same horses.

about the 2004 election (rather as anti-evolutionists may portray themselves as challenging the flimsy Darwinian consensus), but their answers rigidly conform with these tenets:

Accuracy: exit polls are generally accurate to within the limits of random sampling error – “usually accurate within a fraction of a [percentage] point,” says the presumably authoritative Wikipedia.⁴

Scientific evidence of theft: the 2004 NEP presidential exit poll gave John Kerry a decisive lead, as confirmed by scientific experts.

Malign concealment: efforts were made on election night and thereafter to suppress, conceal, and/or misrepresent this evidence of election theft.

The truth must out: releasing the “raw” exit poll data would likely lead to direct proof that the 2004 presidential election was stolen – and only someone who wants to prevent that outcome could oppose the call for disclosure.

Collectively, these assertions form a closed system: in particular, adherents deem challenges to the second tenet as examples of the third. That is, since scientific experts have confirmed that the exit polls evince fraud, anyone who denies it must be a (witting or unwitting) agent of malign concealment. The insistence on an unlikely and, in my view, essentially futile data release further impedes empirical inquiry and discovery. At the risk of facetiousness, I propose a heuristic field test: if you challenge the assertion that the exit polls evince fraud, and someone immediately tars you as a Bushite apologist or a Mitofsky shill, he or she is probably an exit poll fundamentalist.

Fair credit is due: my conception of an exit poll fundamentalist “creed” was inspired by the DemocraticUnderground.com (DU) poster “Autorank.” Autorank adapted the Apostles’ Creed to form a “DU Elections Creed”:

...They descended into hell on election day;
the third day they rose again from the shock;
they ascended into battle,
and sitteth on the right hand of Truth, Justice & The American Way;
from thence they shall come to judge the thick and the febble.⁵
I believe in the Exit Polls;
the Conyers case for fraud;
the communion of USCountVotes.Org;
the forgiveness of nothing;
the resurrection of the electorate,
and honest elections everlasting.
Amen. (‘‘Autorank’’ 2005a)

⁴ Wikipedia, http://en.wikipedia.org/wiki/2004_U.S._presidential_election_controversy%2C_exit_polls (last accessed 5/3/06). The claim is attributed to another online encyclopedia – ironically named Disinfopeedia – but the link is dead.

⁵ “Febble” is the screen name of Elizabeth Liddle on several blogs including DU. At the time this creed was posted, “Febble” had recently emerged on DU as a vehement critic of the exit poll fraud arguments offered by “TruthIsAll.”

While the literary form may be whimsical, the Manichaeic bent is not. Another DU participant rejects

a very bad scientific assumption – that the 2004 election occurred on some sort of neutral democratic ground, wherein normal, neutral number crunching can be expected to give you a reliable result.... The context of corruption was/is pervasive – from the corporate news monopolies who are profiting from the war, to those lying, murderous S.O.B.’s in the White House, whom the American people voted to oust.... THEY had all the power. And they are lying, deceitful, thieving, murderous sacks of [expletive deleted]. (“Peace Patriot” 2006b)

Of course, once one abandons “normal, neutral number crunching,” every outcome and its opposite can be interpreted as evidence of fraud. Exit poll fundamentalism manifests a propensity to invest exit polls with great authority while interpreting their results selectively – or, better yet, disregarding the specific content of the exit polls altogether.

Investigating tenets of the Exit Poll Creed

Although adherence to the Exit Poll Creed, as a system, impedes rational discourse, its tenets are amenable to empirical investigation. Analysts who have contributed to the Exit Poll Creed presumably never set out to create a new dogma; some may regard my account as a caricature, especially if they have never actually challenged the creed themselves. Although I challenge every aspect of the creed, I do not advocate an anti-creed in which exit polls are meaningless, every challenge to the 2004 results is meritless, and the election system is flawless.

Accuracy

Where did Wikipedia come by its knowledge that exit polls are usually accurate within a fraction of a point? This claim seems to have been inspired by Steve Freeman in his seminal paper “The Unexplained Exit Poll Discrepancy” (Freeman 2004a), initially circulated within a week of the 2004 election. In an early draft, Freeman argued:

In general, we have reason to believe that exit polls, by which I mean *uncorrected* exit polls, are accurate survey instruments. Exit polls are surveys taken of representative respondents from the overall voting population. Both the logic behind them and experience suggest that we can use these surveys to predict overall results with high degrees of certainty. It's easy to get a statistically valid representative sample; and there is no problem with figuring out who is going to actually vote -- or how they will vote. (Freeman 2004a, 6)

Freeman subsequently hedged this formulation in several respects, acknowledging that exit polls pose “many challenges” that could induce error.⁶ Freeman’s central formulation became more specific, if not necessarily more correct: logic and experience “suggest that these surveys should

⁶ Freeman also revised his evaluation of representative sampling from “easy” (2004a, 6) to “relatively easy” (2004b, 7).

be able to predict overall results within statistical limits” (Freeman 2004b, 7). While Freeman does not say explicitly what “statistical limits” exit polls should be accurate within, he seems to mean that the results should be unbiased, and that one can therefore calculate probability values on the assumption that respondents constitute a simple random sample – perhaps applying a “design effect” adjustment for cluster sampling.

As evidence from experience, Freeman offers results from Germany and Utah. Specifically, he reports that the German exit poll predictions of Forschungsgruppe Wahlen (FG Wahlen) for the network ZDF have been accurate to within an average deviation of 0.26% in vote share in national elections, and 0.44% in European parliament elections (Freeman 2004b, 7-8). He further reports, “In the US, exit polls have also been quite precise. Students at BYU have been conducting Utah exit polls since 1982.” (Freeman 2004b, 8) The BYU exit poll projections in the 2003 Salt Lake City mayoral race, and in the 2004 Utah presidential vote, each were accurate to within a fraction of a percentage point. Freeman also cites the use of exit polls in Mexico, Russia, and “throughout the former Soviet block” (2004b, 9), of course without evidence of accuracy.

Skeptics might wonder whether Germany and Utah offer compelling proof that exit polls are “usually” accurate within fractions of a point. German exit polls, on their face, are not much like U.S. exit polls (Blumenthal 2004). (The 2005 German election implies that even the German case is not straightforward. The initial ZDF exit poll projection, released minutes after the polls closed, overstated the winning margin of Angela Merkel’s Christian Democratic alliance by 3 points.)⁷ For that matter, skeptics might wonder why we should assume that the 2004 presidential election in Utah was fraud-free, and in turn, how the NEP exit poll in Utah turned out. As it happens, the NEP poll initially overstated Bush’s winning margin in Utah by over 6 points (Simon 2004; E/M 2005, 22), a discrepancy *greater than the national average* and at least approaching statistical significance.⁸

From Freeman’s somewhat ad hoc attempt to marshal evidence of exit poll accuracy, it is a long way down. Many sources seem simply to assume that exit polls should be, or have been, accurate within sampling error. Some observers cite Warren Mitofsky’s reputation for making accurate *calls* – apparently not understanding that Mitofsky and other exit pollsters incorporate official vote counts before making predictions in competitive races.

Relatively little systematic information has been available about exit poll accuracy (Lindeman and Brady 2006 surveys much of the available literature). The 2004 controversy itself has shaken loose some facts that do not support confidence in U.S. exit polls’ extreme accuracy. The

⁷ The ZDF projection gave the Christian Democrats (CDU/CSU) 37% of the vote and the ruling Social Democrats 33% [Deutsche Presse-Agentur 2005]; the final official shares were 35.2% and 34.2%, respectively. The ARD projections were essentially accurate: 35.5% and 34%, respectively.

⁸ When Freeman wrote in late 2004, he would not have had access to the Edison/Mitofsky evaluation report, which shows that Bush’s Utah margin was understated by 6.2 points ($t = -1.7$) in the interview-only “Best Geo” estimate and by 7.1 points ($t = -2.3$) in the “Composite” estimate incorporating pre-election expectations (E/M 2005, 22). Presumably he would have access to Jonathan Simon’s article of November 11 (Simon 2004), in which Simon estimated that the Utah exit poll tabulation of 12:22 AM ET gave Bush a 39.0-point lead – a 6.5-point understatement. Bush’s average margin, across all states, was understated by 5.0 points in the Best Geo estimate, and by 3.6 points in the Composite estimate incorporating pre-election expectations.

Edison/Mitofsky evaluation report gives average “Within Precinct Error” (WPE) values for the largest exit polls in the last five elections (including the CBS/*New York Times* exit poll in 1988). (Within Precinct Error or WPE is, simply put, the difference between percentage margins in the exit poll sample and the vote count.) In *all five cases*, the exit polls on average overstated the Democratic share of the official vote, a result coded as a negative average WPE. The average WPE was -6.5 points in 2004, -5.0 points in 1992, and close to -2 points in 1988, 1996, and 2000 (E/M 2005, 34).

Simply put, *the 1992 exit polls were almost as far off as the 2004 exit polls*,⁹ but hardly anyone seems to have noticed at the time. Presumably the 1992 discrepancies were little noticed because preliminary results were not posted on media websites such as CNN.com as the polls closed – not even to mention partial results leaked and posted on other sites before the polls closed – and because the outcome was not much in doubt. One wonders how the accuracy tenet endures in the face of this clear counterexample. It is, of course, logically possible that the 1992 presidential election saw massive vote miscounts, but then only bare assumption supports the claim of accuracy. It is also *possible* that the 2004 exit polls were accurate even if the 1992 exit polls were not. But it is far from a “slam dunk,” paper trail or no.

Scientific evidence of theft

The next credal affirmation is succinctly voiced by sociologist Dennis Loo: in order to believe that Bush won the election, “Half a century of polling and centuries of mathematics must be wrong” (Loo 2005). The verdict of science, it seems, is clear: since (as we have seen) the exit polls are Accurate, the results point to fraud, if they do not quite *prove* fraud. Michael Keefer informs us, “[P]rofessionally conducted exit polls have been repeatedly shown to have a high degree of accuracy (significantly higher than that of any other kind of polling)... [T]he 2004 polls were conducted with elaborate professional care by one of the most highly respected pollsters in the business” (Keefer 2005) – whom, of course, Keefer proceeds to disregard when it comes to interpreting the results. Oregon activist “Beth P.” assures us that “Statisticians and Ph.D. researchers believe wholeheartedly in the accuracy of scientifically designed exit polls” (Beth P. 2006b). She also cites “analysis studies done by prominent researchers like Steven Freeman, Ph.D. showing that the election results were statistically impossible” and “US Count Votes’ report and analysis done by 13 prominent experts that totally refutes the official Edison/Mitofsky report” (Beth P. 2006a).

Freeman indeed deserves ample credit for the “statistically impossible” meme, although he was careful not to say that the *election results* were statistically impossible. Freeman initially estimated the odds against the exit poll discrepancies in Florida, Ohio, and Pennsylvania occurring jointly due to chance alone at “250 million to one” (2004a, 10). This odds estimate, which Freeman (2004b, 11) fairly characterizes as “headline-grabbing,” attracted criticism from observers who noted that it rested on doubtful assumptions (e.g. Brady 2005). Freeman downgraded the ratio to 662,000:1 (2004b, 13). I will not opine on the correct value, since the

⁹ WPE is probably not the fairest measure of “exit poll error,” because it does not incorporate weightings for observed non-response or differential probabilities of selection. As Mark Blumenthal creatively demonstrated in a post on MysteryPollster.com, close examination of the documentary *The War Room* confirms that early estimates of Bill Clinton’s performance in various states (incorporating weights) also ran high (Blumenthal 2005).

basic result is not in dispute: the exit poll discrepancies cannot sensibly be attributed to chance sampling error alone.

As I have already noted, for many analysts, a serious discussion would begin here: are exit poll discrepancies attributable to vote miscount, non-response bias and other poll error, or perhaps some combination? From the beginning – that is, from Freeman’s discussion on – many observers have given the non-response bias hypothesis short shrift. Freeman sniffed that “although we can imagine why some Bush voters might not to participate, we can also imagine why some Kerry voters might not want to participate either. The problem with this ‘explanation’ ... is that it is not an explanation, but rather a hypothesis” (2004b, 16).¹⁰ And indeed, as of December 2004, there was little available evidence to convince a skeptic that non-response bias accounted for the discrepancies.

What sets thoroughgoing exit poll fundamentalism apart is its blithe confidence that non-response bias does not exist, or at least is irrelevant to exit polls. The so-called “reluctant Bush responder” (rBr) hypothesis is widely derided; the prolific Internet writer TruthIsAll dubbed rBr the “Mother of All Myths” (TruthIsAll 2005a). Elsewhere, TruthIsAll explained, “How do pollsters handle non-responders? They just increase the sample-size!” (TruthIsAll 2005b). Michael Keefer averred that non-fraud explanations of the exit poll discrepancy were “frankly implausible” (Keefer 2005). Baiman and Dopp (2006, 6) seem hardly aware that non-response bias could be seriously considered: they assert that three Ohio precincts in particular “have results indicating either massive vote miscount or psychologically implausible behavior such as Bush voters lying much more than Kerry voters.”¹¹

Non-response bias does exist; indeed, survey researchers continually worry about possible effects of non-response bias. At the same time as I present this paper at AAPOR’s annual meeting, two entire panels are addressing non-response bias (among others during the proceedings). TruthIsAll’s proposal to “just increase the sample size” indeed just increases the sample size. Larger samples yield smaller “margins of error” due to random sampling error, but they are in no way immune to non-response bias and other forms of bias. The infamous *Literary Digest* mail poll of 1936 had over two *million* respondents, and predicted that Alf Landon would win by 14 points. In the event, Franklin Roosevelt won the two-party vote by about 24 percent – a far wilder “statistical impossibility” than anything figured by Freeman. Nevertheless, for those who operate under the exit poll creed, non-response bias is routinely ignored or derided as a plausible account of the exit poll discrepancies – sometimes with the (mistaken) assertion that it is unsupported by “the smallest shred of evidence” (Keefer 2005), sometimes with the claim that US Count Votes or other experts have decisively refuted it. I will return to the task of assessing

¹⁰ Similar text appears in the earlier version.

¹¹ Baiman and Dopp may believe that they have conclusively refuted the non-response bias hypothesis because it “could not produce the exit poll discrepancies... and response rates” in the E/M evaluation report (Baiman and Dopp 2006, 5). As Baiman and Dopp’s former colleague Bruce O’Dell (2005) and others have noted, this argument is not persuasive. Baiman and Dopp’s premise – that non-response bias would have had to be much larger in the (relatively few) exit poll precincts where Bush achieved large majorities, in order to account for the observed red shift in those precincts – is empirically tenuous (Liddle 2005). Even if the premise is correct, higher non-response bias in high-Bush precincts is not inherently implausible. Finally, even if there were statistical evidence of fraud in high-Bush precincts, none of the “impossible” Ohio precincts is a high-Bush precinct; in fact, the most anomalous precincts are near the center of the vote share spectrum (38% Kerry and 40% Kerry).

the evidence. For now, I simply note the selective invocation of scientific authority to forestall actual scientific discussion.

Malign concealment

Public assertions of an exit poll cover-up began as early as 1:53 AM Eastern on Election Night. At that time, a Democratic Underground member posted screen shots from CNN.com, demonstrating that the Ohio exit poll tabulation had recently been changed to indicate that Bush was ahead. “It’s been showing the following all night: [Picture] Kerry up 2 among men, up 6 among women. At 1:41am they changed the result to favor Bush: [Picture] Now Kerry is suddenly down 5 among men, tied among women.” The member drew the obvious inference: “The media is trying to steal this for Bush.” (“EarlG” 2004)

Michael Keefer, on November 5, offered the portentous observation that as of 9:06 pm (ET) on Election Night, CNN’s report of the vote count gave Bush a 9-point lead, while Bush trailed in the national exit poll by “nearly 3 percent.”

One can surmise that instructions of two sorts were issued. The election-massagers working for Diebold, ES&S (Election Systems & Software) and the other suppliers of black-box voting machines may have been told to go easy on their manipulations of back-door ‘Democrat-Delete’ software: mere victory was what the Bush campaign wanted, not an implausible landslide. And the number crunchers at the National Election Pool may have been asked to fix up those awkward exit polls. (Keefer 2004)

“EarlG” and Keefer had extrapolated well beyond the available evidence. Freeman (2004a) offered a reasonable account of how the pollsters had “recalibrate[d]” the exit poll tabulations displayed on CNN.com to conform to the official totals (although Freeman wrongly conjectured that the initial tabulations were based only on exit poll interviews). More recently, US Count Votes/National Election Data Archive has offered this somewhat tendentious account:

Buried in the methodology statement of the National Election Pool (NEP), the protocol of Edison/Mitofsky (E/M), the private company that conducted the National Exit Poll, calls for the gradual Election Night replacement of genuine exit poll data with incoming vote counts. As polls close and vote counts become available they are used to “adjust” or “force” (the term of art used by E/M) the exit poll results to conform with emerging final vote tallies -- *basically the exit polls morph from being Exit Polls at 9 pm to being virtual carbon copies of the vote tallies a few hours later....* Once the full-sample authentic exit poll results were replaced in each state and for the national sample between midnight and 1 a.m., the intention was never to post or publish the authentic exit poll results again. (Dopp 2005c, 2)

Actually, the exit poll tabulations contain a great deal of information that could not possibly be inferred from the vote counts; it is specious to describe them as “virtual carbon copies.” Nor are they ever merely “genuine exit poll data” in the sense presumably intended here (interview data alone): the initial projections incorporate pre-election expectations. The responses from over

70,000 exit poll interviews, archived with the Roper Center and with ICPSR, seem to count as “authentic exit poll results.” All evidence indicates that the exit pollsters did always intend to make these data available, as with previous exit polls dating back over 30 years. Yet somehow the myth has thrived that the exit pollsters and/or the mainstream media attempted from the very beginning to suppress the results – or perhaps not the “very beginning,” since the preliminary results had been up “all night” (in the case of Ohio, for about six hours), but from some crucial juncture where the pollsters and media faced a stark choice between honesty and complicity. Arguably the most odious cover-up in history, it took place in open view.

One might think that exit pollsters’ public reports and statements about the polls would at least mitigate the charge of malign concealment. On the contrary, they are often construed as a new phase of obfuscation. Ernest Partridge comments that Warren Mitofsky “is willing to take his lumps and affirm the validity of the official election tallies. Crossing the GOP and the White House is just not a good business decision.” (Partridge 2005) Partridge asserts that Mitofsky’s discussion of likely error sources “betray[s] a hint of desperation.... These are, strictly speaking, not ‘explanations...,’ they are hunches.” (Coincidentally, William Jennings Bryan once declared: “Darwinism... is only a guess and was never anything more” [Bryan 1922].) Michael Keefer assaults a broader target: “the corporate-media hacks and think-tank trolls, whose collective mission it is to conjure away the most glaring evidence, normalize the abnormal, and twist or bludgeon critical thinking into conformity” (Keefer 2005). The pseudonymous Peace Patriot adds, “I shall never trust E/M again on any matter. They are liars in the pay of people who helped lead us into unjust war – into murder and torture, into the loss of our very soul as a nation. What they did was the height of irresponsibility. It was unforgivable.” (“Peace Patriot” 2006a) A more charitable observer once speculated that someone had threatened Mitofsky’s children.

Clearly such statements resist reasoned discourse, but they are not irrational. If one stipulates that the evidence clearly points to election fraud and theft, one is driven at least to conjecture that any knowledgeable observers who deny the truth are willfully complicit in lies.

The truth must out

The ultimate concealment, some observers argue, is the refusal of NEP and E/M to release all the exit poll data, including precinct identifiers. If the exit poll discrepancies evince fraud, then investigating particular exit poll precincts could yield direct proof that the election was stolen – or, in the alternative, it might turn out that the election wasn’t stolen. Given the stakes, how can responsible people insist on continuing to conceal the evidence?

Steve Freeman forcefully articulated the themes of concealment and the need for release in a joint appearance with Mitofsky last October. First Freeman complained that for Mitofsky to “say [in his abstract] that the exit polls did not indicate a victory for John Kerry, is stunning. He might as well stand before us and say ‘blue is red’” (Freeman 2005, 12). (Well, no. The exit poll margin for John Kerry in Ohio was well within even conventional margins of error [E/M

2005, 22], so Mitofsky's statement was perfectly reasonable.)¹² Freeman went on to say, "When he [Mitofsky] writes about 'inexperienced people trying to make sense of complex statistical data,' it means an excuse for keeping under wraps what may well be evidence of a stolen election." Yet, Freeman concluded, he regards Mitofsky as "not only brilliant, but funny and at his core, honest, decent, a man of rare experience and even wisdom. He may well have it within him to truly 'set the record straight.' And perhaps you in the audience have it within your powers to help compel him to do so." (Freeman 2005, 12)

Is it responsible to refuse to release precinct identifiers for the exit poll information? I believe that it is. NEP exit poll surveys include an extensive suite of demographic questions. Given the precinct that a batch of surveys came from, assiduous investigators could probably match some surveys with actual voters: an egregious breach of respondent confidentiality.

But wouldn't some slight risk to confidentiality be amply justified by the prospect of resolving the election debate? Perhaps – if releasing the data afforded any such prospect. Consider that the state of Ohio had over 11,000 precincts in the 2004 election, of which only 49 were included in the exit poll. Consider further that Walter Mebane and Michael Herron conducted an intensive statistical review of Ohio election returns, in the course of which they identified scores of outlier precincts that might merit further investigation (Mebane and Herron 2005). Why would a handful of outliers based on exit poll data be more essential to any investigation than a larger number of outliers based on election returns? As Stewart (2004) and others have noted, the exit polls provide insufficient statistical power to detect many plausible forms of fraud.

Perhaps surprisingly, we do have a limited case study of direct investigation driven by precinct-level exit poll results. An assiduous group of Internet researchers were able to identify some of the Ohio exit poll precincts. One such precinct was Cincinnati 4-M, which had one of the largest exit poll discrepancies in Ohio. In fact, Baiman and Dopp (2006, 6) identify it as one of three precincts with "virtually impossible" vote counts. A DU participant reports:

According to the observers, the NEP interviewer at Cincy4m was totally out of his depth. **He managed to conduct 31 interviews out of the 1754 voters[...]**¹³ fanning in/out from the polling place (the polling place held 4 precincts). It was raining and he was stationed 100 feet away from the door, in a group of campaigners who were vying with each other for the attention of voters entering the polling place.

The observers said that "**Republican types**" **just streamed by him**. They didn't refuse to be interviewed. They didn't even notice him.

¹² Kerry's margin in the "Best Geo" estimator based on interview data alone was 6.5 points, with an estimated standard error of 3.9 points. In the composite estimator reflecting pre-election expectations, Kerry led by 3.4 points with a standard error of 2.6 points. From Freeman's standpoint, these margins may count as indications. However, Mitofsky's task was actually to make robust predictions on election night, and these margins could not support robust predictions. Thus, for Mitofsky, the interview data "indicate[d]" an election too close to call, a judgment that seems difficult to challenge on statistical grounds. Why does Freeman liken it to the assertion that blue is red?

¹³ The original post here reads, "(times 2, because they went by him twice)." Presumably the interviewer at least could distinguish between voters *entering* and *exiting* the polling place.

Maybe the guy was able to note the age, gender and ethnicity of every Nth person in the fanned mob, but I doubt it very much.

He was totally discouraged, took several long breaks and went home early.
("kiwi_expat" 2005, emphasis added)

Cincinnati 4-M appears to be the precinct identified in the Election Science Institute study (Kyle et al. 2005) as "precinct 25"; thus, the sample apparently contained 68% Kerry voters, i.e., 21 Kerry voters and 10 Bush/other voters. The precinct official count was Bush 312, Kerry 211, other 3 (i.e., 40% Kerry). (Note that voters from all four precincts may have been interviewed.) While the discrepancy between the exit poll and official results is dramatic, these findings hardly seem materially to advance a fraud investigation. They do, however, provide anecdotal evidence of non-response bias – which, for whatever reason, Baiman and Dopp have disregarded in concluding that the "results indicat[e] either massive vote miscount or psychologically implausible behavior" (2006, 6).

One might argue that the exit poll data at least should be available for analysis by independent analysts in ways that protect the privacy of individual respondents. The Election Science Institute (ESI) study, to which I will return, offers some such independent analysis, but many more questions could be explored analytically. As an academic, I would like to see more such transparency. As a participant-observer in the fraud debate, I have become increasingly skeptical that any degree of exit poll transparency could resolve the debate.

Neglected correlations pointing away from massive, widespread fraud

Freeman (2005, 11) cites what he perceives as "neglected correlation after neglected correlation pointing toward fraud." On the contrary, I have been more impressed by crucial correlations (and non-correlations) that point away from fraud – at least fraud of the scope and scale implied by the exit polls. I describe such fraud as massive and widespread: *massive* because Freeman finds that "the numbers indicate" that John Kerry won the popular vote by some 6.5 million votes; *widespread* because exit poll "red shift" appears in dozens of states. I find no persuasive evidence supporting massive, widespread vote miscount or fraud.

Later I will turn to Freeman's correlations and explain why I find them less impressive than he does. But it should be said at the outset that Freeman poses different questions than I do. Freeman (2005, 1) asks, "How much confidence do we have in the official count – and the explanations about the exit poll discrepancy?... How much doubt must we have before we *demand* answers?" In this frame, any note of skepticism about the exit polls can be construed as an expression of "confidence in the official count." And of whom, exactly, are we supposed to demand answers about what? What question about machine allocation in Franklin County (Ohio), or malfunctions in Mahoning County, or undervotes in New Mexico, or election-day voting in North Carolina, or any other aspect of the election could the *exit poll data* resolve? Freeman's questions strike me as more rhetorical than analytical.

My questions *about the exit polls* are: Do the exit polls support the hypothesis of massive, widespread fraud? And secondarily, Do the exit polls support or suggest other hypotheses of

fraud or miscount of whatever scope and scale? I do not argue that these are the most important questions about the 2004 election, or about election integrity in general.

Machine type and red shift

The day after the election, a colleague came to my office and suggested a simple, obvious empirical inquiry: whether exit poll discrepancies were greater in precincts using Diebold machines than in other precincts. This line of reasoning can readily be extended to other tests: Are exit poll discrepancies greater on Direct Recording Electronic machines in general (not necessarily limited to Diebold) than on other equipment? Are they greater on optical-scan equipment, which some people regard as vulnerable to hacking? The answers appear to be No, and No, but these answers have had remarkably little impact on the “debate.”

Charles Stewart (2004) reported that at the state level, there existed no significant relationship between types of equipment used and state-level exit poll error. In fact, red shift was (insignificantly) *negatively* correlated with use of DREs and optical scans, and *positively* correlated with use of other technologies including paper.¹⁴ Edison/Mitofsky (2005, 40) reported a similar result at the precinct level, but found that precincts with mechanical voting (lever) machines had the highest mean red shift, and that precincts with hand-counted paper ballots had the lowest mean red shift (-2.2, presumably indistinguishable from 0 for the small sample of just 40 precincts).

Table 1: Within Precinct Error by voting technology, 2004

Type of equipment used at polling place	Mean WPE	median WPE	mean Abs(WPE)	N
Paper Ballot	-2.2	-0.9	11.2	40
Mechanical Voting Machine	-10.6	-10.3	16.3	118
Touch Screen [DRE] ¹⁵	-7.1	-7.0	14.8	360
Punch Cards	-6.6	-7.3	14.2	158
Optical Scan	-6.1	-5.5	12.6	573

Source: Edison/Mitofsky 2005, 40

These results would seem to pose distinct difficulties for “black box voting” accounts of election theft. Undaunted withal, Freeman (2005, 8) argues, “If the ballots are never manually counted anyway or if there is a lax chain of custody on the ballots, then all machines are as vulnerable to programming fraud as electronic machines.” While lever machines are hardly foolproof, the assertion that they are “vulnerable to programming fraud” is idiosyncratic. In any case, Freeman (2005, 8) chooses to emphasize the paper ballot result: “look at the difference between Paper ballots and machines of all types!”

¹⁴ Stewart (2004) constitutes an addendum to a Caltech-MIT Voting Technology Project report (Voting Technology Project 2004) that inadvertently incorporated final, weighted (and therefore essentially “error-free”) exit poll estimates for most states. After this mistake was pointed out, Stewart issued an addendum (2004) that substantively replicated the initial non-finding for several alternative error data sets. Freeman (2004b) and others rightly criticized the original paper; the addendum, in my experience, is rarely acknowledged.

¹⁵ The E/M category “Touch Screen” appears to include all Direct Recording Electronic equipment, regardless of input device. Most DREs employ either touch screens or pushbuttons.

Edison/Mitofsky did look at the paper ballot difference, and pointed out that it was confounded: almost all the paper ballot precincts were in small towns and rural areas, where red shift tended to be smaller on all technologies. E/M reports that 33 of the 40 paper ballot precincts were in rural precincts, where the overall mean WPE was -3.6. The mean WPE for paper-ballot precincts in rural and small town areas was -1.6 (all but two of these precincts were in fact rural) – statistically indistinguishable from the overall rural mean.¹⁶

Freeman asserts that E/M

attempts to partition this difference [between paper and other technologies] away.... But to do so requires: First, a partition to create a statistically meaningless group [of urban-area paper ballot precincts] (N=5), second ignoring that paper has lower PLD [WPE] than machines in “urban areas” regardless. And third, [also ignoring] a sharp difference between paper and machines in the statistically significant “rural/small town category.” (Freeman 2005, 8)

Not really. First, dividing paper-ballot precincts into urban and rural is not a ploy to “create a statistically meaningless group,” but a response to the fact that urban and rural precincts have very different mean red shifts. Second, it takes chutzpah to assert that this “statistically meaningless group” of urban-area paper precincts nonetheless “has lower [WPE]” – all the more considering that its *median* WPE is larger than that of any other urban-area technology except lever machines. And third, Freeman may assert a “sharp” difference between paper and machines in the rural precincts, but the difference does not even approach statistical significance.

In short, there is no evidence that exit poll red shift was significantly smaller in paper-ballot precincts than in other precincts, nor that red shift was larger in DRE and optical-scan precincts. These results do not correspond with any fraud hypothesis I have encountered. Activists who aver the urgency of getting rid of DREs, or DREs and optical scanners, lest future elections be stolen ‘just like the 2004 election was,’ seem not to have considered these findings. (Note that other rationales exist for getting rid of DREs and/or optical scanners.)

Non-response bias: evidence from the Edison/Mitofsky report

Freeman, even as late as October 2005, argues that Edison/Mitofsky offer no “independent evidence (evidence other than survey results that do not match the count) of non-response bias in this or other exit poll[s].”(Freeman 2005a, 4)¹⁷ But Freeman almost immediately concedes, tacitly, that this is untrue. Under a secret ballot system, it would be hard for us to find *direct* evidence of non-response bias, notwithstanding the anecdotal evidence from Cincinnati 4-M

¹⁶ Although E/M does not report standard errors, they do report that the mean absolute WPE for the 35 paper rural/small town precincts is 10.5. The standard deviation is likely to be larger – most likely at least 12, based on simulations assuming normally distributed WPE. Thus the standard error of WPE for these precincts is probably over 2.0, and a conventional margin of error would be on the order of ± 4.0 or larger.

¹⁷ Freeman strangely asserts, “(experiments actually conducted suggest *no* response bias, every [even?] when participation rates are much lower than 53%)” (4). I agree that low participation rates do not necessarily imply response bias or non-response bias. Nevertheless, I am mystified what experiments supposedly suggest that exit polls are immune from bias – or whatever Freeman intended to claim. Lindeman and Brady (2006) survey some of the contrary evidence, including experiments in which survey mode and length influence exit poll results.

cited above. Neither do we have direct evidence that the discrepancies were due to fraud. However, we can frame hypotheses regarding precinct-level variables that are likely to covary with non-response and/or selection bias (cf. Edelman and Merkle 1995). (By “selection bias” I mean that some interviewers may have had different propensities to *approach* Bush and Kerry voters, whereas non-response bias refers to the voters’ propensities to complete the survey if approached.) Indeed, as Freeman admits, the E/M evaluation report offers support for several such hypotheses.

First, if voters vary in propensity to be interviewed, factors that influence how easily voters can simply avoid interviewers are likely to affect the magnitude of non-response bias. Hence, we can conjecture that bias would be larger in large precincts, and in precincts where interviewers were far from the polling place (see, once again, Cincinnati 4-M; see also Edelman and Merkle 1995, Table 7, reporting *absolute* error). These hypotheses receive strong support. Precincts where interviewers were instructed to approach every, or every other, voter (indicating relatively small polling places) had a mean WPE of approximately -3.6; precincts where interviewers approached every 10th voter (large polling places) had mean WPE of -10.5 (E/M 2005, 36). Precincts where the interviewer stood inside the polling place had mean WPE of -5.3; precincts where the interviewer stood more than 100 feet away had mean WPE of -12.3 (E/M 2005, 37). Of course these results *might* be attributed to a confound with fraud: fraud might be concentrated in large precincts (or polling places) where interviewers were (futilely?) deliberately kept far from the polling places.¹⁸ But the mere existence of an alternative explanation does not entitle us to ignore the evidence altogether.

Moreover, if voters are (for instance) reluctant to engage with interviewers they perceive as likely to disagree with them, then non-response bias may vary according to interviewer characteristics. Contrariwise, interviewer characteristics are unlikely to affect the magnitude of fraud. The E/M report documents, inter alia, that mean WPE was -7.7 points among interviewers under 35, but approximately -5.5 points among interviewers 35 and over (E/M 2005, 43). Moreover, mean WPE was -3.9 points among interviewers with a high school education or less, but -7.3 points among interviewers with one to three years of college (45). Combining these results, it seems eminently plausible that some Bush voters were predisposed to steer around college-age interviewers (and/or vice versa). It is conceivable that college-age interviewers coincidentally were sent to the precincts where fraud was largest, but to my knowledge no one has offered a reason to expect or to believe that this is so. (Unfortunately, E/M did not report WPE statistics for other interviewer characteristics that might influence non-response bias, although they do enumerate several that I will not discuss here.)

Thus, Freeman cannot very well stake his argument on the claim of “no independent evidence” of non-response bias. Instead, referring to E/M’s discussion of interviewer effects, he notes that “this explanation cannot conceivably provide a complete explanation for the discrepancy” (Freeman 2005a, 4). Well, of course not. Non-response bias, by definition, is not directly

¹⁸ Freeman argues, “it’s not at all clear why *mean PLD* [WPE] should increase [with distance from the polls]. On the other hand, one also must ask why an election officer, or a secretary of state would be trying so hard to keep observers from being close to the polling place” (Freeman 2005a, 5). I am all in favor of asking that, but if keeping interviewers 100 feet from the polls is so useful in disguising fraud, it does not seem labored to posit that it would also affect non-response bias.

measurable, and there is no *a priori* reason to expect measured variables to account statistically for *all* the red shift. Nevertheless, the evidence in the E/M report does support the conclusion that at least part of the red shift is explained by non-response (and/or selection) bias. In fact, the differences cited above are generally larger than the differences cited by Freeman in support of fraud, which I discuss later.

Does Bush do surprisingly well in red-shift states?

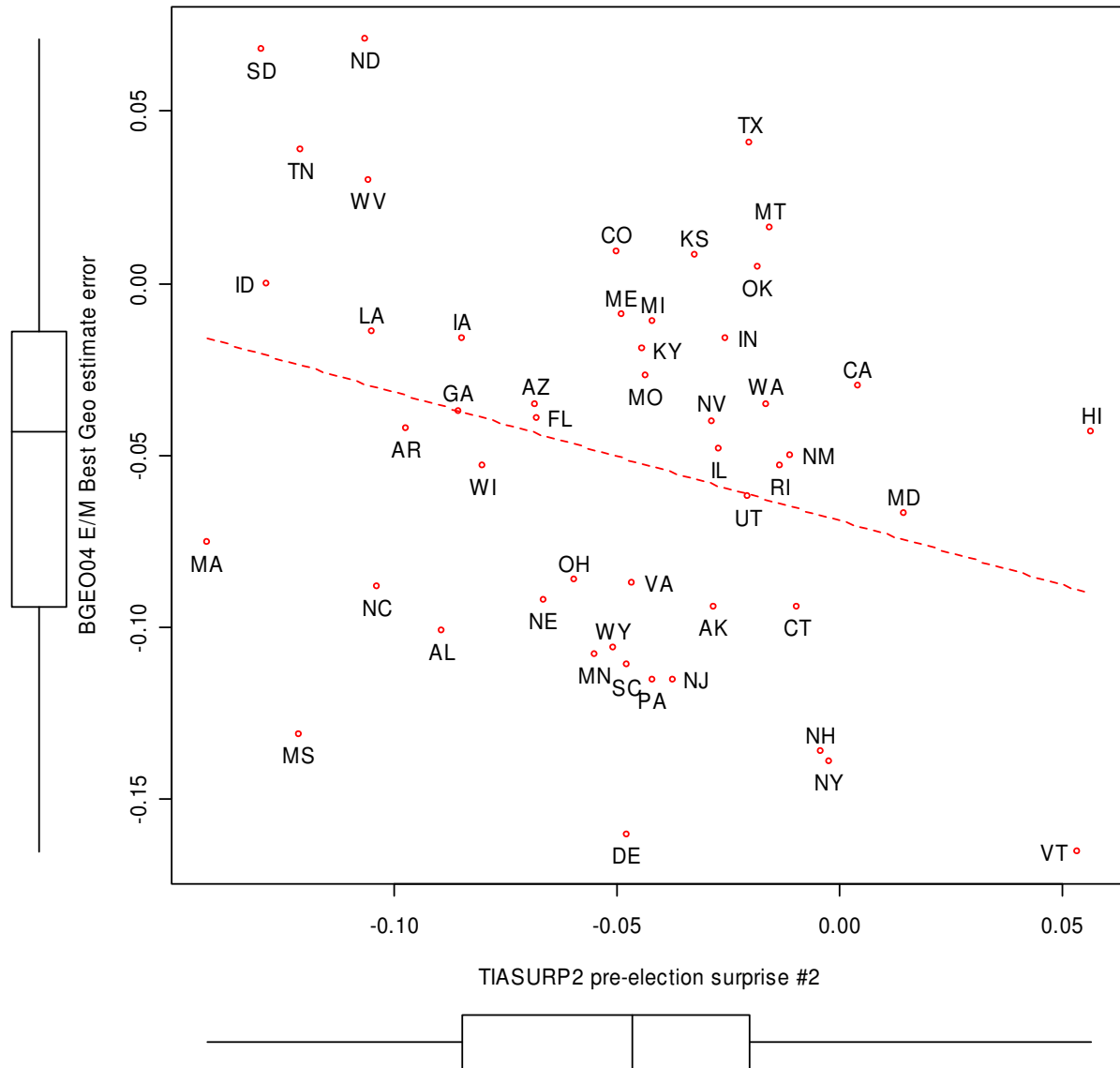
If variations in red shift reflect variations in the magnitude of vote miscount favoring Bush, then we would expect Bush to do better than expected in states and precincts with large red shift. Expectations can be considered in at least two ways. First, for states, we can compare pre-election polls with the election returns. Second, for states and/or precincts, we can compare the current election returns with previous election returns, leveraging the fact that election results are highly correlated across elections. The pre-election approach offers a more direct expectations measure, although state pre-election polls could evince some bias that varies across states.

Here, to assess pre-election expectations, I draw on TruthIsAll's (TIA's) Election Model spreadsheet, using his favored measure, which allocates 2/3 of undecided voters to Kerry (see Lindeman 2005c). Presumably no one can accuse TIA's measure of being biased toward suppressing fraud. Although I regard this measure as biased toward Bush, I see no reason to expect this bias to vary systematically so as to affect the analysis. Indeed, alternative measures, including Edison/Mitofsky's own prior expectations (courtesy of Warren Mitofsky), yield substantively similar results. The difference between TIA's projection and the official results (TIASURP2, for "TIA's Surprise #2") has mean -0.05 (i.e., -5%), indicating that Kerry did less well on average than TIA expected. Red shift is measured, on the vertical axis, as the difference between E/M's Best Geo estimator (projections based on interview data alone) and official returns. If red shift and "surprise" both reflect fraud, then we would expect a positive correlation. In fact, the observed correlation is *negative* and statistically significant ($r = -0.293$, $p = 0.04$).¹⁹ That is, red shift tended to be largest (Best Geo error most negative) in states where Kerry did *better* – or at least not much worse – than expected.

So, Bush does not do surprisingly well in red-shift states, at least not by comparison with other states. If anything, the opposite is true. This result can probably be mathematically reconciled with the hypothesis that the exit polls evince massive, widespread fraud, but it does not fit all that well. For instance, TIA's projections imply that Kerry did more than 5 points better than expected in Vermont – but if the exit poll Best Geo model is "right," Kerry actually should have done 22 points better than expected. Really?

¹⁹ Using WPE to measure red shift, the correlation with "surprise" is even larger ($r = 0.389$, $p < 0.01$).

Figure 1: Best Geo red shift by election "surprise"



Or consider the case of New York. According to Freeman’s reckoning (Freeman 2005c), the exit polls indicate that Kerry should have won New York by 29.7 points instead of 18.3 points, as in the official returns. Thus, Kerry appears to have been cheated out of some 840,000 votes in New York (which, incidentally, almost exclusively uses lever machines). It seems unlikely that the better part of a million votes in my state could have been stolen without any poll workers noticing, and without leaving a powerfully bizarre statistical trace in the official returns. It also seems unlikely that Kerry won New York by roughly 30 points considering that the last four polls in the electoral-vote.com database (electoral-vote.com n.d.) gave Kerry leads of +18, +17, +15, and +16.²⁰

²⁰ These polls were conducted, respectively, by Survey USA (57-39), Rasmussen (54-37), Siena College (52-37), and Marist College (54-38).

Similarly, every late poll indicated that Pennsylvania was very close: the final five state polls in the electoral-vote.com database gave Kerry an average 1.6-point margin (ranging from +4 to tied).²¹ TruthIsAll, employing rather heroic assumptions, managed to project that Kerry would win Pennsylvania by about 6.5 points. The exit poll Best Geo estimator gave Kerry a 13.8-point lead. In the official returns, Kerry won by 2.5 points. Should we really prefer the exit poll estimate? If we relax the assumption that Exit Polls Are Accurate, is there any good reason to suppose that this one was?

Does Bush improve on past performance where red shift is high?

Overall, Bush's vote margin was about 3 points larger in 2004 than in 2000 (+2.5 versus -0.5). If red shift evinces vote miscount, we would expect – all else equal – that Bush's margin would improve more in states with large red shift than in other states. In practice, there is no discernible relationship between red shift and election-to-election swing (change in margin) at the state level. This result, or non-result, is difficult to square with fraud, unless fraud was concentrated in states where Bush otherwise would have done much worse in 2004 than in 2000.²²

Election Science Institute examined precinct-level Ohio exit poll data, comparing red-shift with “swing” from 2000 to 2004 (Kyle et al. 2005). Again, if red shift indicates vote miscount, then we would expect red shift to correlate with swing favoring Bush. And again, ESI found no relationship – an odd result assuming massive, widespread fraud, unless the fraud was concentrated in *precincts* where Bush otherwise would have done much worse in 2004 than in 2000.

It may not be immediately obvious why the absence of relationship between red shift and swing poses a problem for fraud accounts. Suppose that malefactors obliterated a 6-point Kerry victory in Ohio (as indicated by the Best Geo predictor) by changing 8% of votes from Kerry to Bush in 50% of Ohio precincts. (Of course we do not need to assume such a cut-and-dried fraud mechanism; it simply illustrates the premise of substantial vote miscount in a considerable fraction of precincts.) By assumption, vote miscount shows up as greater red shift in the rigged precincts than in the non-rigged ones. It would be quite surprising not to also find a difference in swing between the rigged and the non-rigged precincts, unless Bush would otherwise have done 10 points worse in the rigged than the non-rigged precincts.²³ To calibrate the rigging so precisely would seem to require the functional equivalent of foreknowledge of the actual results. That degree of foreknowledge would be trivial if the riggers controlled all the results – but then, why not swing all precincts by some small percentage, more or less? And then, how would we account for all the supposedly suspicious variations in red shift?

²¹ These polls were fielded by, respectively, Survey USA (Kerry 49-48), Zogby (Kerry 50-46), Strategic Vision (Kerry 47-46), Quinnipiac (47-47), and Gallup (49-47).

²² One might retort, “You are assuming that the 2000 returns weren't rigged, too!” However, given the premise of exit poll accuracy, I practically *have* to assume that any rigging in 2000 was much smaller in magnitude than in 2004, since the exit poll discrepancies in 2000 were generally so small as to escape contemporaneous attention.

²³ It is hard to tell whether the natural variation in swing could even support such a large gap between rigged and non-rigged precincts. However, lacking certain knowledge of “true” swing, I will not argue that point further.

Figure 2: illustration of fraud and swing/red shift correlation

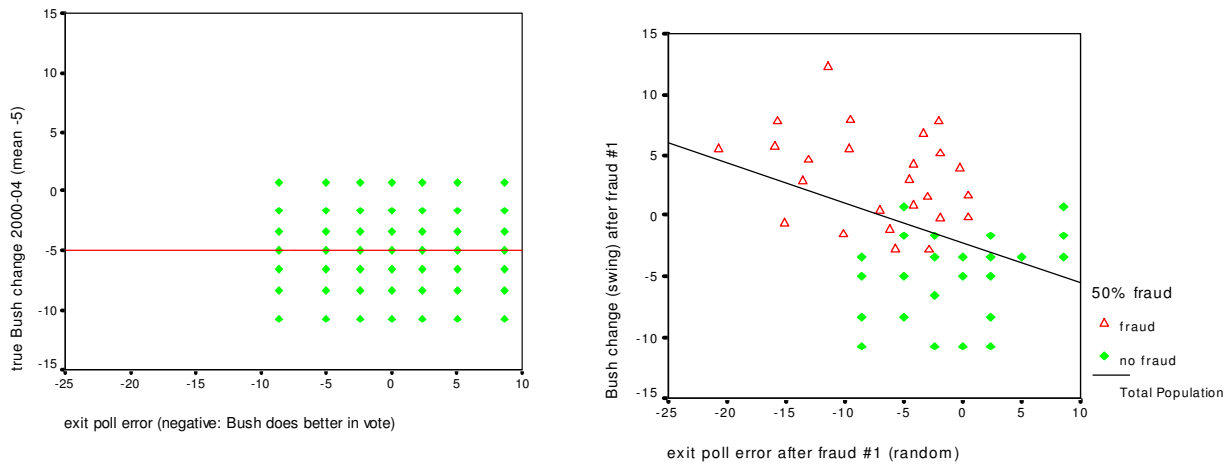


Figure 2 depicts a stylized, and more modest, variation of the scenario described in the preceding paragraph. At left, 49 precincts are distributed symmetrically around 0% exit poll error and -5% change in Bush share of two-party vote 2000-2004 (crudely consistent with the assumption that Bush by about 3½ points in 2000, but lost Ohio by about 6½ points in 2004). At right, approximately half the precincts are shifted in favor of Bush; the mean increase in vote share is 8 points (the standard deviation is 3). Fraud shifts each precinct up (increase in Bush vote share) and to the left (exit poll red shift) relative to its location in the absence of fraud. The political outcome is that Bush gains about 8 points on the margin, presumably enough to win Ohio by about 1½ points upon simplifying assumptions.²⁴ One statistical side effect is a substantial negative correlation between exit poll error (WPE) and swing favoring Bush. Again, this is not a necessary result, but it is likely in the absence of additional assumptions (for instance, that fraud is targeted toward precincts that would otherwise have negative swing).

The ESI study has prompted several vociferous responses from US Count Votes president Kathy Dopp (Dopp 2005a; Lindeman 2005b; Dopp 2005b; Lindeman 2006a; Baiman and Dopp 2006). Dopp demonstrates that it is *mathematically possible* for Bush to win by means of vote miscount without inducing a correlation between red shift and swing (Dopp 2005a, 7-9). She claims thereby to have refuted ESI’s “Test to Rule Out Vote Fraud.” But this is silly: ESI never claimed to have *ruled out* vote fraud. The pertinent question is not whether ESI has proven vote fraud to be mathematically impossible, but how *likely* it is that a plausible massive miscount mechanism can account for the absence of correlation between swing and red shift. Dopp also points out that “vote counts from one election to the next are subject to nonrandom events” (Dopp 2005b; see also Baiman and Dopp 2006, 31). Of course they are: no one supposes that fraud would be the *only* factor influencing swing from one election to the next, or even variation in swing across precincts. Nevertheless, it is far from obvious how the evidence of all that tampering would simply cancel out. (Moreover, given that vote counts in *any one* election are subject to nonrandom events, one wonders why Baiman and Dopp consider their own vote share/red shift analyses legitimate, while ESI’s vote share *change*/red shift analyses are utterly inadmissible.)

²⁴ This scenario assumes that the precincts have equal numbers of voters and that fraud has roughly the same distribution statewide as in the exit poll precincts.

Happily, we now know the result of extending ESI's shift-swing analysis to the entire country. Warren Mitofsky first presented these results in a joint appearance with Steve Freeman in October 2005; Lindeman (2005b) reproduces (with permission) two crucial slides. These slides, again, evince *almost exactly zero correlation between red shift and swing* at the precinct level. How can this be? I think that in order to reconcile this result with massive, widespread fraud, one almost has to adopt one or both of two heroic auxiliary hypotheses. The first would be that fraud was targeted to counterbalance "real" variations in swing, which seems to entail complete control over vote counts in almost all precincts (at least exit poll precincts) nationwide. On this hypothesis, one would almost imagine that Freeman's various "neglected correlations" (discussed below) were planted as red herrings. And so we can offer a second auxiliary hypothesis: perhaps the entire exit poll was rigged, soup to nuts, to frustrate any attempt to reconcile the results with a coherent account of massive, widespread fraud.

I believe that all these findings constitute important evidence *against* massive, widespread fraud. Certainly they argue against the view that the exit polls offer strong evidence of such fraud.

Assessing arguments for vote miscount

But what of all the exit poll evidence that purportedly *does* point to vote miscount? Herein I undertake to review some of the most prominent arguments in this vein. Much of my attention is focused on Steve Freeman's work, which has been widely acclaimed – especially his presentation in Philadelphia last October, at which (as Michael Keefer reports) he "humbled Warren Mitofsky" (Keefer 2006). Many readers will not feel impelled to examine every such argument. The "excess of 2000 Bush voters" argument on page 21 may be of particular interest.

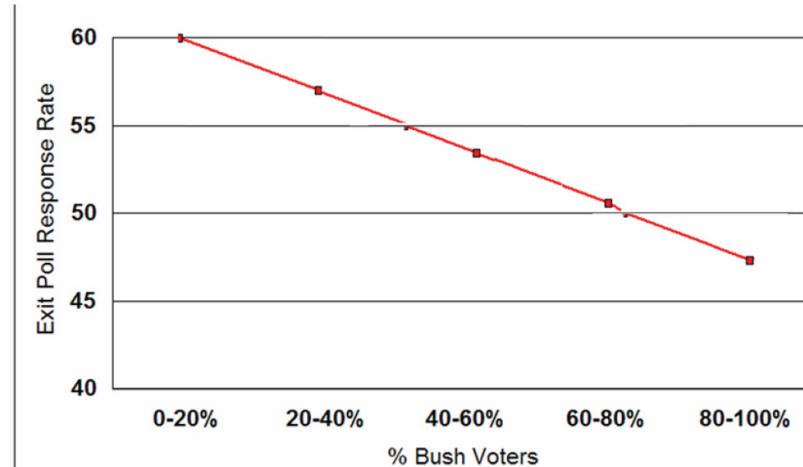
A preliminary caution: the issue at hand is not whether I can conclusively account for every anomaly in the exit poll results. There will always be gaps in the fossil record and in our understanding of evolution, but these generally are not regarded as evidence for creationism or intelligent design. Likewise, a "fraud of the gaps" argument should have little force here. In evaluating the assertion that the exit polls accurately depict the theft of many millions of votes around the country, we should not be unduly distracted by arguments that turn out to hinge on small numbers of votes in a few states. By the same token, we should eschew monomaniacal pursuit of the null hypothesis. Even if the exit polls do not support the inference of massive, widespread fraud, they may support narrower inferences or hypotheses about vote miscount or other important political phenomena.

Do response rates infirm the non-response bias hypothesis?

US Count Votes and others have argued that the reported response (completion) rates infirm the Edison/Mitofsky hypothesis that Bush voters participated at a lower rate. Freeman, perhaps preparing in haste, presented two remarkable "illustrations" of this argument in his joint presentation in October 2005. Here is the first:

Figure 3: Freeman illustration of varying response rates

If Bush voters were less likely to participate in the polls, this is the relationship we would expect to see:



Source: Freeman 2005b, 30

In fact, this is far from the relationship we would expect to see. Edison/Mitofsky suggested that overall, the results could be explained by a 56% completion rate among Kerry voters and a 50% rate among Bush voters. The slide above implies that over 60% of Kerry voters, and closer to 45% of Bush voters, completed the survey – upon the crucial assumption that those rates would not vary with vote share (although of course, by definition, the proportions of Kerry and Bush voters would vary). A later slide (Freeman 2005b, 32) simulating state-level results depicts an even more curiously strong relationship between vote share and response rates: it implies Kerry-voter response rates approaching 100% and Bush-voter response rates around 20%.

Illustrations aside, do the observed response rates in fact in firm the hypothesis of non-response bias? The observed response rates are slightly higher in the strong-Bush precincts (those that voted 80%+ for Bush) than in the strong-Kerry precincts: 56% versus 53% (E/M 2005, 37). However, this fact seems inconsequential for several reasons. E/M reports that the difference in response rates is statistically insignificant (which seems evident). Indeed, the precinct-level response rates are statistically consistent with a slight negative slope – which is only to say that the “true” relationship, if any, *might* be slightly positive or slightly negative. It may be worth noting that the reported response rates are not highly reliable: in the case of Cincinnati 4-M, described on page 10 above, the interviewer apparently reported a response rate of 62% (31 out of 50), although by protocol he presumably should have attempted well over 100 interviews.

Finally, and crucially, the very premise of the expected relationship is doubtful. Freeman describes the expectation of a higher response rate in strong-Kerry precincts as “practically a mathematical necessity” (Freeman 2005, 6), but it is not. Freeman here commits the ecological fallacy. It is perfectly plausible that both Kerry voters and Bush voters might participate at higher rates in Republican than in Democratic precincts (suppose, for instance, that the Democratic precincts are disproportionately urban, crowded, and chaotic), while Kerry voters





might participate at higher rates than Bush voters within precincts across the partisan spectrum. All in all, the response rate argument – modestly suggestive when it was first offered in the spring of 2005 – has little force.

What about the excess of 2000 Bush voters?

One popular argument that the exit polls evince fraud hinges on the reported distribution of 2000 votes. In the final results weighted to the official vote totals, 43% of respondents reported that they voted for George W. Bush in 2000, and 37% that they voted for Al Gore. Not only is this result counterintuitive on its face, because Gore received more votes than Bush did, but it appears arithmetically impossible if extrapolated to the entire 2004 electorate. Bush received about 50.5 million votes in 2000, and perhaps 2 million or so of these voters may have died before the 2004 election. Yet 43% of the 2004 electorate would yield 51.7 million people who had voted for Bush in 2000. As Freeman puts it:

...the beauty of numbers is an integrity in the entirety. When an irreconcilable contradiction exists, it is a sure sign that something is amiss.... If we are to believe the NEP figures, one in seven Gore voters through death, disability, or divine intervention disappeared from the electorate; but no such fate befell a single Bush 2000 voter; indeed another million miraculously appeared. (Freeman 2005, 9-10)

Figure 4: tabulation of 2004 by 2000 presidential votes, 2004 exit poll (CNN.com)²⁵

PRESIDENTIAL VOTE IN 2000				
	BUSH	KERRY	NADER	
TOTAL	2004	2000	2004	2004
Did Not Vote (17%)	45%	n/a	54%	1%
Gore (37%)	10%	n/a	90%	0%
Bush (43%)	91%	n/a	9%	0%
Other (3%)	21%	n/a	71%	3%

Although Freeman does not put it quite so baldly, isn't it apparent that the exit pollsters essentially had to invent several million Bush 2000 voters in order to account for Bush's re-election?

Actually not. This excess of self-reported Bush 2000 voters does seem like an unprecedented anomaly – if, that is, we never glance at any other exit polls. If we do examine other exit polls, the precedents are hard to miss. In the 2000 VNS exit poll, 45.6% of respondents reported having voted for Bill Clinton in 1996, only 31.3% for Bob Dole – although Clinton won in the

²⁵ This table is derived from the CNN.com report of “U.S. President / National / Exit Poll” results at <http://www.cnn.com/ELECTION/2004/pages/results/states/US/P/00/epolls.0.html>. To enhance readability, the table is pasted in HTML and then reformatted to approximate the original appearance.

official returns by just 8.5 points.²⁶ Moreover, 45.6% of the 2000 electorate would equal about 48.1 million Clinton voters, although Clinton received only 47.4 million votes in 1996. Did exit pollsters invent millions of Clinton 1996 voters in 2000, in effect concealing evidence of massive vote miscount favoring Al Gore? If so, why hasn't Freeman told us?

But we need not stop in 2000. I examined ten exit polls in eight elections dating back to 1976. In *all ten polls*, the previous winner's reported vote share (among people who reported having voted four years earlier) was larger than his actual vote share had been – about 11 points larger on average. This evidence, and much other evidence, indicates that some respondents falsely report having voted for the incumbent. (An extensive discussion appears in Lindeman 2006b.)

Another way of arguing for fraud from the figure above runs (and falls) as follows. Suppose that Kerry indeed won about the same proportion and number of Bush 2000 voters (9% of them) as Bush won of Gore 2000 voters (10%). Then Kerry's margins among the other two groups – “Other” 2000 voters (mostly Nader voters) and the large group who did not vote in 2000 – should have given him the victory, shouldn't they have? Upon close inspection, this argument likewise assumes that people reported their 2000 votes accurately. I argue (Lindeman 2006b) that in fact, many more Gore 2000 voters defected to Bush than Bush 2000 voters defected to Kerry. However, many of these Gore-to-Bush defectors misreport having voted for Bush in 2000 as well, and therefore do not show up as defectors! The 2000-2004 National Election Study panel survey contains a striking minority of respondents who did just this: they defected from Gore to Bush both in the 2004 election and “retrospectively” in their recalled 2000 vote.

Ironically, the 2000 recalled vote question actually provides weak evidence that the exit poll was indeed biased. The unweighted result would yield a smaller exaggeration of the previous winner's margin (about 2 points) than in any exit poll I examined. Granted, President Bush had low approval ratings in November 2004, but probably not lower than Richard Nixon's in November 1976, when Nixon's retrospective 1972 margin in the exit poll was 3.9 points larger than it had been in the actual returns. Thus, the unweighted results appear to yield too *small* an excess of Bush 2000 voters.

The swing states

Freeman seizes on E/M's observation that precinct-level error (Within Precinct Error) is slightly larger in precincts in the 11 swing states (-7.9 points on average) than in other precincts (-6.1 points) (E/M 2005, 42). E/M hints that the difference might be attributable to exposure to “more paid advertising and media coverage.” Freeman urges an alternative explanation: “The first priority was, of course, to win states. Thus, it would make sense that votes would be most vigorously coveted in the swing states...” (Freeman 2005a, 7).

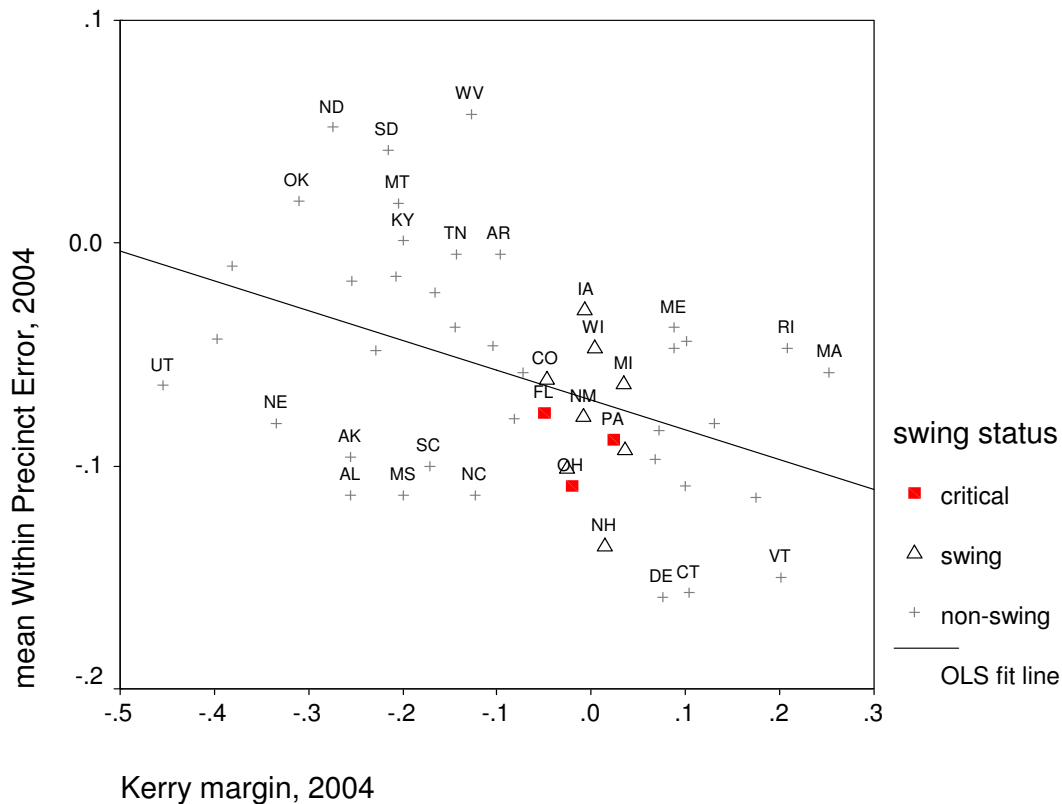
On close inspection, this theoretical expectation is not so compelling. One might expect that votes would be stolen *only* in swing states, but perhaps that would be too obvious. Well, then, if (as Freeman apparently believes) Bush supporters had the wherewithal to steal votes in states all

²⁶ These are weighted percentages; the unweighted percentages are similar (Clinton garners 45.8% of the retrospective vote). Note that among respondents who reported having voted in 1996, the gap is even wider: Clinton retrospectively defeats Dole by 16.4 points.

over the country, why would they not try to steal roughly equal proportions of votes in swing and non-swing states, so as to throw the hounds off their trail? And can one really say that it “would make sense” that the malefactors managed to steal just enough votes in Pennsylvania to lose by 2 points instead of double digits – and to match the pre-election polls (see page 17)?

Any statistical “swing state effect” seems to be far outweighed by a correlation Freeman does not mention: exit poll discrepancies favoring Kerry tended to be larger in states where he did better (not worse!). Controlling for vote share as in Figure 5, the swing states seem generally unremarkable.²⁷ (Ohio is well below trend, but less so than, say, Utah.) Perhaps this correlation merits *more* neglect than it has hitherto received.

Figure 5: Within Precinct Error by Kerry margin



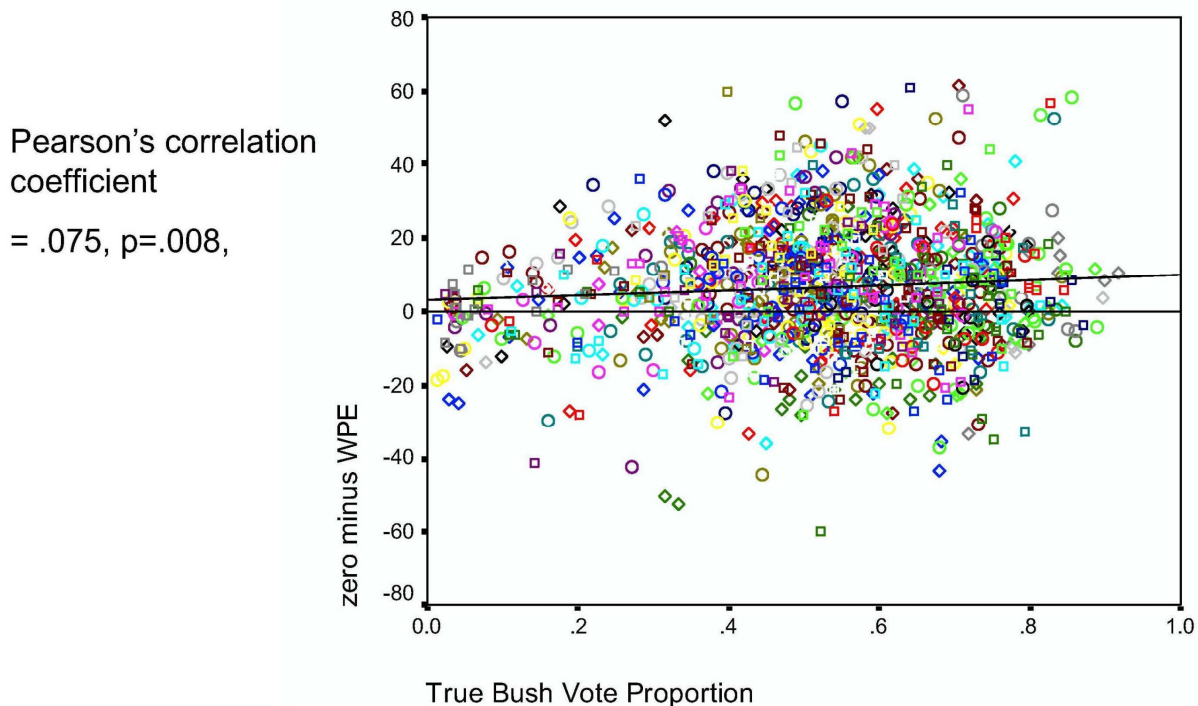
The GOP strongholds

Freeman reproduces a table of mean WPE by range of “precinct partisanship.” He summarizes, “There’s no PLD [WPE] at all in the Kerry strongholds, but the discrepancy is highest on the right side of the spectrum.... In fact, the stronger Bush’s support, the greater the disparity” (Freeman 2005a, 7). Freeman elaborates, “If fraud were afoot, it would make sense that the president’s men would steal votes in GOP strongholds, where they control the machinery of government....”

²⁷ Nine of eleven swing (or “critical”) states are labeled here; Minnesota is near PA, and Nevada near OH.

Well, perhaps. As it happens, the mean red shift is 2.4 points *smaller* (less negative) in what E/M dub the “Moderate Republican” precincts, where Kerry got only 20% to 40% of the vote, than in the “Even” precincts (40% to 60% Kerry). It is true that the mean WPE is close to 0 in the high-Kerry precincts, and that the mean is highest in the high-Bush precincts – although the median in the high-Bush precincts is actually smaller than in the rest of the sample.²⁸ It might be true that the high-Bush precincts are generally controlled by “the president’s men,” although there are only 40 such precincts in the exit poll sample (about 3% of the total) – somewhat meager pickings for fraudsters afoot. One also wonders, again, what to make of the fact that red shift is greater on average in *Kerry* states.

Figure 6: Within Precinct Error by Bush vote share



Do “GOP stronghold” precincts hold the key to interpreting the exit poll data? Consider Figure 6, a variant of which is reproduced in Freeman (2005b, 46). (Note that the vertical axis depicts zero minus WPE, so “red shift” precincts lie above the X axis.) It is apparent in this plot that large red shift values – and, for that matter, large blue shift values – are not concentrated in any particular part of the vote share spectrum. The slight positive correlation appears to owe to the paucity of extreme red shift in precincts with very low Bush vote shares (upper left). This paucity is influenced by mathematical tautology: if Bush’s official vote share is (say) 10%, the exit poll cannot possibly overstate Kerry’s winning margin by more than 20 points. Given that

²⁸ The *mean* WPE in the 40 high-Bush precincts is -10.0. The *median* is -5.8. (Although the overall median is not reported, we can infer from the “cooperation by polling place official” table [E/M 2005, 38] that it was -6.0 or more negative.) It is apparent in Figure 5 that the mean is heavily influenced by four precincts.

high-Kerry precincts considerably outnumber high-Bush precincts, simple arithmetic tends to induce a positive correlation. Liddle (2005) demonstrated that random variation in completion rates *also* tends to induce a positive correlation between Bush vote share and WPE. Mitofsky demonstrated at the 2005 AAPOR meeting that Liddle's alternative measure yields a smaller and insignificant correlation between vote share and red shift ($|r| = 0.034$, $p = 0.23$). The story does not end there, because Liddle's measure proves to be biased in the opposite direction for small samples (Lindeman, Liddle and Brady 2005). But even relying on the original WPE measure, it is hard to imagine how these data support a Men In Strongholds fraud narrative.²⁹

Republican governors

In a similar vein, Freeman hypothesizes that where "Republicans controlled the State government, there might have been greater opportunity to steal votes" (Freeman 2005a, 8). His supporting slide reports that the mean WPE was -6.7 in states with Republican governors, but -5.0 in states with Democratic governors, and "t-test: $p = .04$ " (Freeman 2005b, 40). I have been able to replicate Freeman's means (and medians), but not his t-test result; in fact, using a variety of parametric and non-parametric tests, I have never seen a p value less than 0.10.³⁰

The Republican-governor hypothesis has other problems besides statistical marginality. Once again, the causal reasoning is not so compelling, for rarely do governors control elections. Most often, secretaries of state and/or nonpartisan election boards do, in concert – and/or in conflict – with local elections officials (usually at the county level). This caveat turns out to be non-trivial. Of the five states with the largest red-shift WPE, one (Delaware) had a Democratic governor who appointed the commissioner of elections; three (Connecticut, Vermont, and New Hampshire) had Republican governors but Democratic secretaries of state who administer elections; and the last (New York) had a Republican governor and secretary of state, but elections are administered by a bipartisan board. It seems inapt to construe any of these states as Republican-controlled for purposes of assessing the likelihood of fraud.

Yet if we do not, Freeman's empirical claim seems to collapse altogether. If Connecticut, Vermont, New Hampshire and New York are removed from the analysis (generously, since all except New York apparently should be coded Democratic), the difference in mean WPE narrows to 0.4 points using WPE, or 1.2 points using Freeman's preferred IM WPE – far from statistical significance in either case. There may be offsetting cases of states with Democratic governors but Republican or non-partisan election control, but it is unlikely that they would alter the basic result, or non-result.

African American voters

Freeman notes that African Americans have been disenfranchised in a host of ways, including felony disenfranchisement and voting machine shortages. "And is PLD higher in states with more blacks? Yes, it is.... There is no apparent polling explanation; so one must at least

²⁹ An alternative account would argue that the precincts toward upper right are not "GOP strongholds" at all, but that miscount has increased both their Bush vote share and their red shift. The red shift/"swing" analysis discussed above (page 17) seems well suited to investigating this possibility.

³⁰ More specifically, these means apply to WPE, not IM WPE. Neither WPE nor IM WPE yields statistically significant differences.

consider the possibility that this higher PLD indicates that blacks are being disenfranchised by yet other means as well.” (Freeman 2005a, 8)

Differential disenfranchisement of blacks is a real and distressing fact in 2004 as in other elections (e.g. Klinkner n.d., Mebane 2006), and one hesitates to complain that Freeman’s argument here verges on *non sequitur*. Freeman has not actually proposed any mechanism by which fraud would induce a relationship between African American population and exit poll red shift. He merely cites the anomaly, declares it unexplained, deems it suspicious, and (at least by implication) tallies it in the fraud column. To quote Freeman’s own stern words, “scientists are expected to conduct theory driven analyses, not to fish for findings and then concoct an explanation” (Freeman 2005a, 5) – although Freeman does seem to have omitted the latter step.

What might account for greater red shift in states with large African-American populations? Let us provisionally (and plausibly) assume that this causal mechanism operates by disenfranchising black voters – but how? As Freeman indicates, the mechanisms he enumerates are unlikely to explain the outcome, because blacks who are prevented from casting ballots will rarely if ever participate in the exit poll.

One possible explanation is that black voters disproportionately cast provisional ballots that were ultimately not counted. However, this explanation is unlikely to account for much red shift in most states because the proportions of uncounted provisional ballots are simply too small. For instance, in Florida, some 18,000 provisional ballots went uncounted (Florida Department of State, n.d.), about 0.2% of the presidential votes counted.

Another explanation is that black voters experienced greater ballot spoilage or residual vote (overvote and undervote) rates, as county-level analysis suggests was true for Florida in 2000 (Klinkner n.d.). However, given that residual vote rates decreased substantially in several states after the adoption of electronic voting machines (Stewart 2005), it seems unlikely that residual votes can account for any substantial portion of a large *increase* in exit poll red shift, no matter how racially disproportionate the “new” residual vote pattern might be.

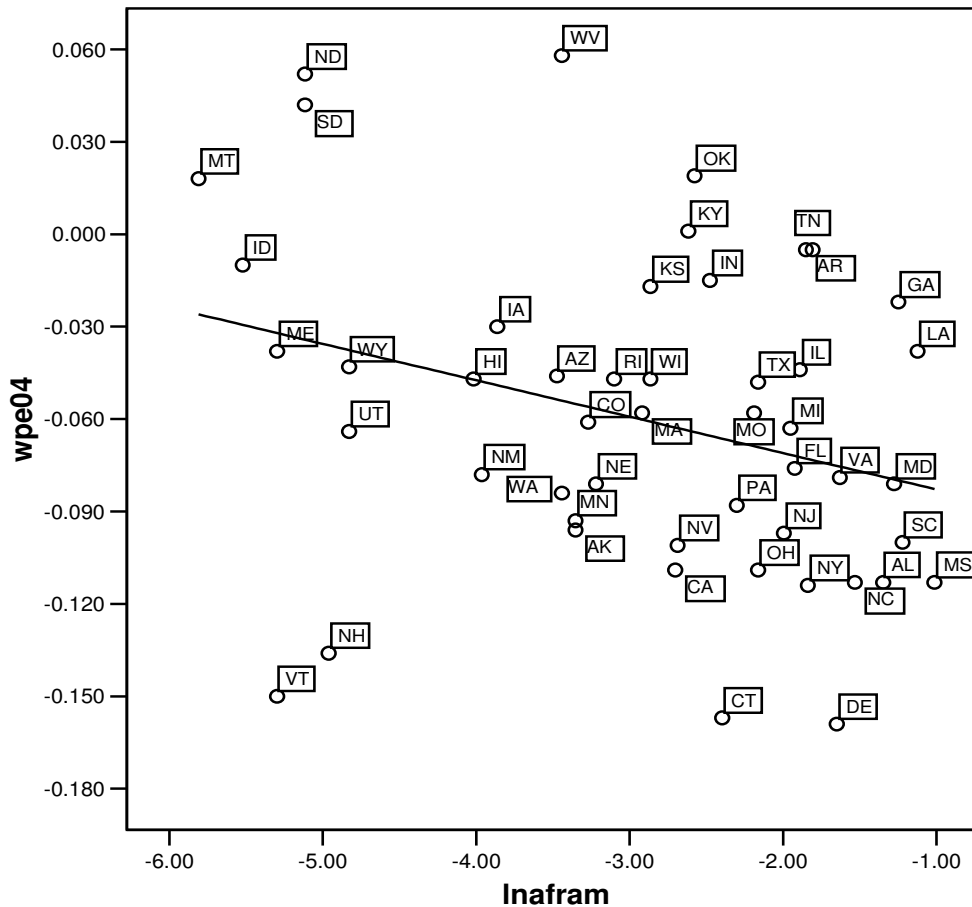
Another possibility, thus, is that the (apparent) race effect is mediated through vote-switching on newly adopted voting technologies, although the causal link to race remains unclear. If so, we might reasonably expect the use of new voting technologies to be associated with higher levels of red shift. However, as I noted above (page 12), available evidence seems to point against this conclusion.

It seems prudent to consider the alternative hypothesis that the correlation is spurious. Figure 7 below depicts the relationship between the logarithm of % African-American population and mean WPE, which is statistically significant ($r = -0.303, p = 0.034$).³¹ Note that six contiguous

³¹ Freeman suggests using the “IM WPE,” which is based on the same precinct-level WPE formula but – unlike the usual state-level measure – does not exclude the top two and bottom two outliers. Here I use WPE because it is more familiar and yields a slightly stronger correlation. Note also that Weighted Least Squares regression, using square root of sample size (number of respondents) as the weight, yields smaller correlations that approach but do not attain statistical significance at the 0.05 level. For exploratory analysis, it seems inappropriate to enforce an arbitrary significance criterion.

states with very low proportions of African-Americans (and small populations) cluster at upper left. If these states are removed from the analysis, the bivariate correlation becomes essentially 0 (actually -0.022). If the three New England states that also have very low proportions of African-Americans (ME, VT, NH) are also removed, then the correlation increases in magnitude again but does not approach statistical significance ($r = -0.192$, $p = 0.23$). I will not proffer an *ad hoc* hypothesis to account for the apparent “mountain state effect.” I will suggest, however, that any phenomenon that depends on small minority populations in small states (North Dakota had on the order of 4,000 blacks in 2000) to be detectable seems unlikely to demonstrate massive, widespread fraud. As in many other cases, analysis with much larger data sets might yield more informative results.

Figure 7: Within Precinct Error by African American population (ln of %)



Election administration problems

Freeman reports a moderate and robust correlation ($r = 0.28$, $p < .01$) between red shift and election day problem reports to an MSNBC toll-free number (MY-VOTE1), measured in calls per million voters. Freeman describes the result as “indicating that those problems did in fact result in corrupted counts” (Freeman 2005b, 9). The inference is bold, but questionable, because many calls concerned issues that seem unlikely to have contributed to corrupted counts. For

instance, in Florida, 60% of calls concerned registration or absentee ballot status.³² While some of these calls may have led to provisional ballots being cast and not counted, likely most were resolved in other ways – successfully or not.

Indeed, the proportions of absentee calls are the strongest predictor of red shift (Lindeman 2005c). Controlling for absentee calls, other problem report types that might seem more likely to correlate with miscount – for instance, reports of mechanical failures – generally did not predict red shift. Moreover, states with high absentee call rates also tended to be states where Kerry fared *best* relative to his pre-election results (although this correlation was not significant). Indeed, the statistical significance of the correlation between red shift and problem reports seems to depend on including states with positive WPEs (blue shift) – again featuring North and South Dakota – that all had low proportions of complaint calls.

My point is not to dismiss problem reports. Problem reports, both in the MYVOTE1 database and in the EIRS (OUR-VOTE) database, surely could convey valuable information about election irregularities, whether fraudulent or not. However, the problem reports do not appear to support the master narrative in which exit polls demonstrate massive, widespread vote miscount.

The smoking gun in Ohio?

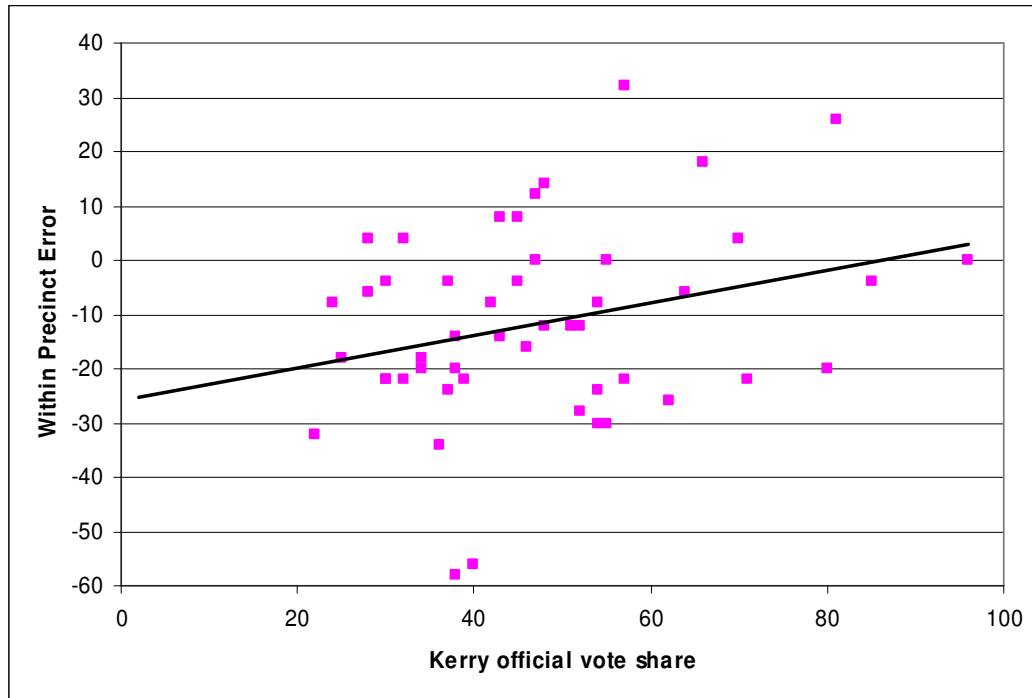
Baiman and Dopp (2006), who assert “Virtually Irrefutable Evidence of Vote Miscount,” appear to offer approximately four distinct arguments in support of their claim.

1. Baiman and Dopp (2006, 3) report that Ohio’s “exit poll discrepancy pattern includes three precincts with virtually impossible outcomes” (2006, 3). That is, these precincts have exit poll outcomes unlikely to occur due to sampling error alone. This result is presumably accurate, albeit uninformative. As I have noted (page 10 above), one of these precincts was Cincinnati 4-M, which is almost a poster child for likely non-response bias – although of course I cannot rule out fraud there or anywhere else. (The presence of multiple precincts at the polling place introduces another source of possible bias.)
2. Baiman and Dopp (2006, 12) report that the observed results are inconsistent with the assumption of “a uniform 59% to 50% (1.18) Kerry-to-Bush voter response ratio.” This result in no way counts as evidence against non-response bias, because non-response bias is expected to vary across precincts. In fact, as I argued above (pages 13ff.), WPE varies across precincts in ways consistent with hypotheses of *non*-uniform non-response bias. Moreover, Baiman and Dopp make no effort to establish that the observed results are consistent with any *specific* vote miscount hypothesis. Instead, vote miscount is (as far as I can tell) invoked as a capacious and amorphous “explanation” that can accommodate any residual variation.
3. Baiman and Dopp (2006, 10) aver, “As Kerry official vote share increases, exit poll discrepancy trends to zero. As Bush vote share increases, exit poll overestimates of Kerry vote share rises [sic].” Baiman and Dopp initially base this assertion upon a rather peculiar comparison of two polynomial (quadratic?) curves. Eventually, they compare

³² Here I draw upon the file <http://www.philly1.com/CARTER-BAKER-PROB-STATE.xls>, which serves as Section 6 of “MYVOTE1: Voice of the Electorate 2004, A National Report,” presented to the Carter-Baker Commission on Federal Election Reform, June 30, 2005 (<http://www.philly1.com/CARTER-BAKER-FINAL-REPORT.pdf>).

vote shares with exit poll discrepancies directly (2006, 16), yielding a plot similar to the figure below. It can be seen that WPE does indeed “trend[] to zero” as Kerry vote share increases – at least, if one disregards the actual data points, which have no clear trend. The correlation is barely statistically significant ($r = 0.282, p = 0.050$), and not at all robust.³³ More to the point, Baiman and Dopp offer no clear account of how the existence or non-existence of this trend bears upon fraud. Incidentally, certainly these results do not support Freeman’s conjecture of fraud in “GOP strongholds” – in fact, by Freeman’s definition, there *were* no “GOP strongholds” in the Ohio sample.

Figure 8: Ohio WPE by Kerry official vote share



4. Baiman and Dopp (2006, 16) comment, “Mathematics (beyond the level of this report) show that a downward slope... of WPD [WPE] plotted by exit poll share is consistent with vote fraud and miscounts.” We need not overthink the mathematics. Since WPE directly depends on exit poll share, larger Kerry exit poll shares naturally tend to coincide with more negative WPE. This result is indeed “consistent with vote fraud and

³³ For instance, if the two precincts with the largest *blue* shift (WPE > 20%) are omitted, the correlation falls far short of significance ($r = 0.189, p = 0.20$). Or, if the two precincts with the largest red shift (WPE < -50%) are moved 10 percentage points to the right, increasing Kerry vote share, then again the correlation falls far short of statistical significance ($r = 0.219, p = 0.13$).

On page 17, Baiman and Dopp assert that “in precincts with less than 57% Kerry vote share, the pattern is obviously nonrandom and is consistent with vote miscounts that favor Bush.” The “pattern” seems to be that WPE tends to be negative in these precincts, although this “obviously nonrandom” result does not precisely leap out of Figure 7. Even if we accept the arbitrary choice of 57% as a criterion, the statistical significance of this distinction is marginal at best, and its substantive significance remains obscure.

miscounts,” but it is *equally* consistent with non-response bias or any other source of WPE.³⁴

Have Baiman and Dopp presented anything approaching “virtually irrefutable evidence of vote miscount” in Cincinnati 4-M? Surely not. Have they identified any telltale patterns evincing miscount as opposed to non-response bias? Not that I can tell.

But what about all the other evidence?

But what about all the other evidence that I have ignored in this paper? The Warren County (OH) lockdown, the punch card switches in Cleveland, the fact that an obscure black judge got more votes than John Kerry in several Ohio counties, the returns in Florida where heavily Democratic counties voted for Bush, the EIRS complaints of vote-flipping, the Lehto-Hoffman analysis of Snohomish County (WA), the divergence between election day and early/absentee voting in North Carolina, the undervotes in New Mexico, the drastic misallocation of voting machines in Franklin County (OH)....

With apologies to readers who have little idea what the preceding sentence was about, I will not attempt to enlighten them here. For what it is worth, I have sifted through quite a bit of evidence about the 2004 election apart from the exit polls. Although many of the anomalies cited do not strike me as robust evidence of anything whatsoever,³⁵ some evince serious flaws in the conduct of the election (for instance, on voting machine allocation in Franklin County, see Mebane 2006; Highton 2006). Some of the evidence could possibly point to fraud. Isn't it irresponsible of me not to present all that evidence? Or rather, some would say, shouldn't I focus on presenting the evidence that raises the most questions about questionable election systems?

I can only appeal to readers' capacity to make basic intellectual distinctions. This paper does not address the possibility of “fraud” in general; it addresses specific arguments that the exit polls evince massive, widespread fraud. Some observers seem to believe that any evidence (however tenuous) for any form of fraud in 2004 constitutes supporting evidence for all other forms of fraud, but I do not. Nor does offering evidence against one form of fraud entail denying the existence of any fraud, misfeasance, or systemic vulnerability. We should not be afraid to think clearly.

Beyond exit poll fundamentalism

Some observers consider exit poll fundamentalism as harmless at worst, certainly not worth opposing. If it helps to motivate people to press for election reform, how bad can it be? Even if academics should not actually embrace useful untruths, surely they can exercise discretion about

³⁴ Baiman and Dopp seem to have withdrawn an earlier claim that the Ohio exit poll data are consistent with a fraud mechanism described in Dopp (2006), wherein 15% of Kerry votes are shifted to Bush in 75% of precincts. This mechanism actually would tend to induce a negative correlation between Kerry official vote share and WPE (greater fraud in Democratic precincts, even after accounting for the fraud), depending on the partisanship distribution.

³⁵ For instance, allegations of suspicious returns in Florida prompted careful attention and, in the view of most observers, persuasive critique (see the overview in Brady et al. 2004, 7-12). Social scientists did not content themselves with taking sides on the issues; they conducted additional empirical tests and revised their judgments in light of the results. This is as it should be.

which ones they oppose. Here my pragmatic judgment coincides with my intellectual judgment: exit poll fundamentalism should end. The conviction – the unwarranted, vehement certainty – that the exit polls prove fraud has inspired startling personal animosity and attacks. It has impeded cooperation between election reform activists and sympathetic academics. It has led journalists and others to associate concerns about election integrity with fringe theories that are rejected or ignored by thousands of domain experts, a practical disadvantage.³⁶ It has evoked unwarranted panic that the death of American democracy is at hand – and “when there is panic in the air, with one crisis tripping over the heels of another, actual dangers mixed with imaginary scares, there is no chance at all for the constructive use of reason” (Lippmann 1922, 260). It has sparked opposition to reforms that would likely improve election integrity. It has distracted attention from indefensible abridgements of voting rights and many other faults in the conduct of elections. In short, it has done far more harm than good.

I see little prospect that the next eighteen months of exit poll hand-wringing could lead to greater knowledge of possible fraud in the 2004 election than the first eighteen months. I understand Baiman and Dopp’s frustration at what they deem the “truly unconscionable” withholding of exit poll data (2006, 26), but in truth, given their apparent lack of interest in actual Ohio returns and even the identity of one of their “virtually impossible” precincts, I cannot imagine how a few more data would help.

Exit poll fundamentalism as a puzzle

As a problem in the sociology of science, the persistence of exit poll fundamentalism raises points of (perhaps morbid) curiosity beyond my competence or the scope of this paper. I have never considered myself a great respecter of academic authority, but I am fascinated and perplexed by the willingness of exit poll fundamentalists to confer authority selectively to analysts operating out of field. Exit poll fundamentalists appeal *generally* to the authority of “prominent statisticians” and “Ph.D. researchers.” *Specifically*, they appeal to the authority of business scholar Steve Freeman, economist Ron Baiman, MA mathematician Kathy Dopp, pseudonymous “Excel expert” TruthIsAll, and various statisticians whose names they generally do not know; they draw further support from English professor Michael Keefer, environmental ethicist Ernest Partridge, geomorphologist Richard Hayes Phillips, and communication professor Mark Crispin Miller. Surely this list includes many intelligent and honorable observers whose arguments should be thoughtfully scrutinized. Still, I find myself wondering, do adherents of these arguments ever genuinely wonder why so few political scientists and survey researchers – indeed, so few social scientists regardless of field – express agreement with their views? Some of these “naysayers” might be explained away as Mitofsky shills, or right-wing stooges, or intellectual naïfs in denial, or cowards afraid for their careers, but all of them?

Again, it is hard to resist analogy to creation science, wherein the “world’s premier refereed creation publication” features neuroscientists writing on genetic mutation, physical chemists on plant death, engineering mathematicians on astrophysics, dentists on human migration routes,

³⁶ I believe, and have argued, that the domain experts are correct. But even if they were wrong, the political cost of arguing against them without conclusive evidence should not be disregarded. Criticisms of electronic voting machines have received much warmer media coverage, in part because recognized domain experts have lent credence to those criticisms.

and former family doctors on “mitochondrial Eve” and ice cores (Baxter 2006). (I assume that these, too, are intelligent and honorable observers, although I doubt that they are right.) But analogy is not explanation. I can observe and describe the closed loop of exit poll fundamentalism, but I cannot truly explain it. It should not be glibly attributed to sour grapes or wishful thinking; its adherents do not appear to enjoy believing it. It is genuinely “weird” (Shermer 2002).

Lessons for survey researchers

Survey researchers and analysts may do well to recognize some of the strange beliefs offered in their name. It is hardly a novel insight that some non-experts are confused by concepts such as “margin of error” – as if sampling error were the only error that matters – although ordinarily the legitimacy of election outcomes is not at issue. Given the many careless criticisms of survey research, it may be flattering to be considered well-nigh infallible. However, the principals of Edison/Mitofsky can testify that in this context, a reputation for infallibility is no boon.

What of future practice regarding exit polls? The 2004 NEP researchers and sponsors could be forgiven for concluding that when it comes to “transparency” and disclosure, no good deed goes unpunished. Nevertheless, disclosure was wise and constructive. For instance, CNN.com posted preliminary tabulations almost immediately as the polls closed (accompanied by a lucid methodological explanation which few people read) – thus setting in train endless accusations of conspiracy. Would it have been better not to release the initial results at all? It might have *delayed* the accusations, but when the tabulations inevitably leaked, the outcome would have been far worse. The January 2005 Edison/Mitofsky evaluation report, which offered an unprecedented level of detail about methods and results, seems to have been mined to support prior assumptions about fraud even as it was criticized for continuing the cover-up. Could E/M have starved the controversy by withholding these results? Surely not: various fraud theorists have proven very resourceful in finding support for their priors. On the contrary, the evaluation report provided useful evidence of non-response bias, as well as many other details of interest to independent researchers.

Given intense scrutiny of election returns, and of exit polls as a *de facto* election audit mechanism, public discourse would likely benefit from making more information available faster – not primarily on election day, but before and after. I will not push the argument for transparency very far: no plausible degree of openness can serve as a universal solvent for suspicion. On the other hand, it is faintly ridiculous that Mark Blumenthal resorted to examining screen shots of a political documentary in order to document bias in the 1992 exit poll results (Blumenthal 2005).³⁷ I do not assert that anyone willfully concealed (or even could have concealed) that information; unfortunately, it was not widely known to people who could have benefited from knowing it, even before the election. Likewise, any careful reader of the Edison/Mitofsky evaluation report can recognize some analytical gaps that could usefully be filled, potentially shedding light well beyond the “exit poll debate.” I will not opine here on

³⁷ Of course, Blumenthal was following upon the figures in the Edison/Mitofsky report, which had just been released (in January 2005). If one revisits Blumenthal’s blog entries from late 2004 and early 2005, one sees a generous effort to piece together fragments of information about exit poll error from various sources. But why was this effort necessary?

what the disclosure policies should be, but formulating and explaining such policies in advance would help to mitigate the perception that data and analytical results were released selectively and capriciously.

I harbor no illusions – certainly not after the last year – that if information is simply made available, everyone will become informed. I do think that some of Warren Mitofsky’s remarks 17 years ago, in his AAPOR presidential address, can be usefully revisited and extended in this context. Invoking Julian Woodward’s 1946 description of polls as a “public utility,” Mitofsky criticized AAPOR’s contemporaneous “Standard for Minimal Disclosure”:

That information is not informative enough for most consumers of survey research, certainly not for most members of the news media and other members of the public. It is barely useful to trained survey researchers. What we have done with our minimal disclosure is to place the burden on our users for making sense of the limitations of our surveys....

That is wrong. It is the responsibility of the researcher to spell out the limitations of his or her own survey when there are limitations....

...Let me be even clearer about what I want to see disclosed. When I say limitations, I want to know how the design affects conclusions based on that survey. Specific statements about what can and cannot be concluded should be part of the disclosure.... [For instance, if] the sampling error is not reported, then the disclosure statement should [explain why].... If there are question biases I want to know what they are. In surveys about racial attitudes, I want to know whether there is an interaction between the race of interviewers and respondents and whether the conclusions are affected by this interaction.

To me, this type of disclosure would come closer to “truth in packaging” of survey research. What we have now does not do it. (Mitofsky 1989, 451-452)

Make no mistake: in many respects Edison/Mitofsky’s and the NEP sponsors’ disclosure has been not minimal but exemplary, and I do not question their ethics – in fact, I have vehemently defended them. In no way do I think that any of these parties oversold the accuracy of the exit poll interviews. Their critics did that, and much else besides. Nevertheless, if we construe the public at large as having some legitimate interest – however misinformed – in the exit poll interview results, I do not believe that we have done enough to explain the limitations.

Better information and understanding of exit polls’ potential pitfalls will benefit activists who are exploring the potential of better exit polls, or independent exit polls, or “parallel elections” in election auditing (see, for instance, studycaliforniaballots.org). I have serious reservations about this line of research, but people who pursue it should be able to benefit from our best knowledge and questions about exit poll research.

And finally

In the course of the exit poll controversy, I have often recalled Walter Lippmann's words in *Public Opinion*:

If [a social scientist] is really critical and saturated in the scientific spirit, he cannot be doctrinaire, and go to Armageddon... for a theory of which he is not sure. If you are going to Armageddon, you have to battle for the Lord, but the political scientist is always a little doubtful whether the Lord called him.
(Lippmann 1922, 234-235)

Some observers have seemed in a great hurry to go to Armageddon. But I will not end with the apocalypse. I have been privileged to talk and to correspond with many people who have legitimate concerns about American electoral processes, and who are working hard and well to address the faults and vulnerabilities that they see. Beyond exit poll fundamentalism lie many puzzles, and arguments, and discoveries, and challenges, and innovations. Let us begin.

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