

Response to A. Makulilo's article on IPSOS' 2013 Voter Intention Survey Performance

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Abstract

This piece critically assesses the article, "Poll-'Pollution'?: The politics of numbers in the 2013 elections in Kenya", by Alexander Makulilo, recently published in the African Review (Vol. 40, No. 2, 2013, p. 1-32). In the article, the author subjects the survey work related to the March, 2013 Kenyan election conducted by the Kenyan branch of the international market research and media-monitoring firm, IPSOS, to harsh criticism, stemming from various purported methodological failures and deliberate bias. This response, by IPSOS-Kenya's chief Research Analyst, comprehensively addresses these criticisms. In doing so, it seeks to reveal the author's insufficient appreciation of such methodology, while raising two critical questions that he conveniently avoids: how credible does he consider the official results, and how close to even these contested results would any final poll – concluded over a week before the election itself – have to be in order to win his approval?

Introduction

I am writing in response to the "Poll-'Pollution'?: The politics of numbers in the 2013 elections in Kenya", by Alexander Makulilo, published in the *African Review* (Vol. 40, No. 2, 2013, p. 1-32). At the outset, it is agreed that its subject matter – public affairs survey research in Africa – is certainly welcome, given its reflection of and potential contribution to the expansion of "political space" across much of the continent over the last several decades. This is especially so, since it has received such paltry scholarly attention, notwithstanding a few exceptions (de Torrente, 2013; Wolf, 2009; Bratton et al, 2005). Moreover, anyone presenting research results should be prepared for serious, sober scrutiny of their work.

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It is thus highly regrettable that this piece, dealing with Kenya's 2013 election, is so highly flawed, a consequence of its voluminous omissions, distortions, and outright falsehoods. As one closely associated with the subject matter and as a former academic myself (Lecturer, Department of Government, University of Nairobi, 1988-94), I therefore sought – and gratefully have been granted – the right-of-reply from *The African Review*. Without identifying them all (which would take even more space than what has been used here), I seek to include enough of them to justify such an assertion, while making clear my deep misgivings that this piece – at least without major revisions/corrections, based at least in part on information from me/IPSOS that was never sought by the author – was ever published.

Examples of Errors and Omissions

Paper Title

The title itself is quite misleading. The author restricts himself to a single survey research company, IPSOS (formerly registered in Kenya as IPSOS-Synovate, formerly Synovate, formerly The Steadman Group; I shall refer to the company simply as 'IPSOS' throughout), and says almost nothing about the actual *politics* of this election; instead, the article aims to undermine the credibility of the voter-intention survey work of this one company (which also has a country-office in his own country, Tanzania). For example, there is no evidence that he attempted to conduct interviews with any political actors about these results, or even with any media professionals and/or executives who published/broadcast/sponsored/interpreted-debated many of them. Rather, his only "grounding" in the reality of this election is a few newspaper quotations from self-serving political actors. Indeed, it was this very sort of distortion of reality by such actors that was the subject of my own 2009 piece (with which the author is familiar).

While rather petty, but also worth noting, the first part of the title ('Poll-Pollution') is either a case of unacknowledged copying, or an unlikely coincidence, this being almost identical to the sub-heading in my published article on the pre-2007 Kenya election surveys ('Poll-ution'; 2009: 290), that the author cites in a different context. Since it is said that imitation is the most desirable form of flattery, perhaps I should withhold any complaint here, however.

Background: IPSOS' Final Survey vs. the Official Results

Before examining the author's article, the survey findings at the centre of his attention should be made clear, given that his use of them is

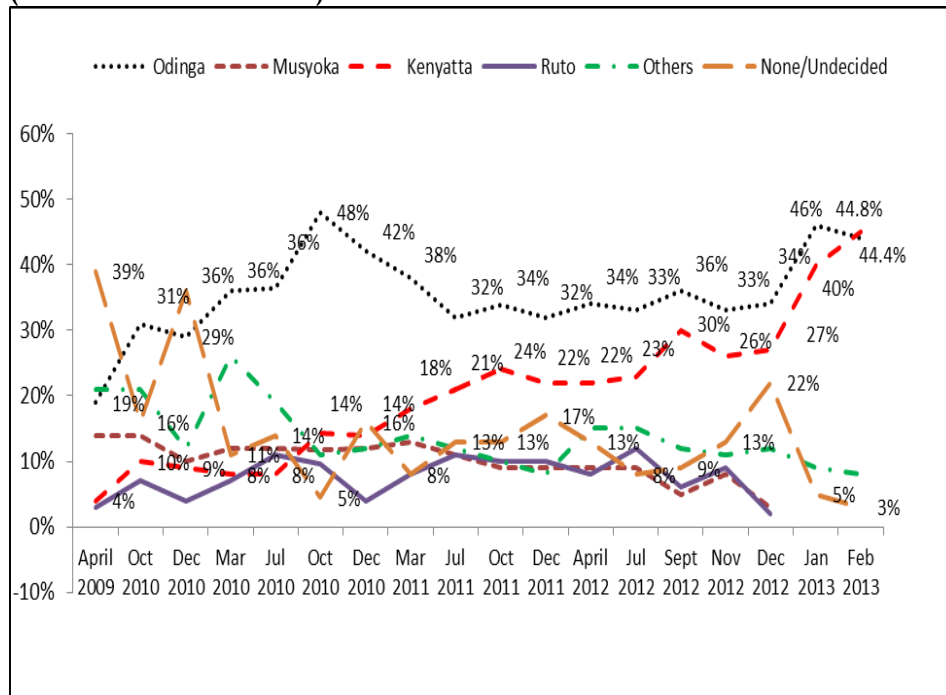
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incorrect/incomplete, and thus quite confusing. Most important here is the contrast between the official election results as announced by Kenya's elections' body (the Independent Election and Boundaries Commission, IEBC) and those of IPSOS' final pre-election survey (discussed in more detail below). Table 1 captures this, showing just how accurate the poll was for all presidential candidates aside from Uhuru Kenyatta (the winner), while Figure 1 shows IPSOS' poll results on stated presidential voting intentions over the previous three and a half years. For reasons that shall become evident, these results underscore just how reliable – and thus credible – the company's work in this area was during the period concerned – as, indeed, it has been in the past.

Table 1: IPSOS' Final (February) Results vs. the Official Kenya 2013 Election Results

Presidential Candidate	IPSOS	IEBC	Variance	Error margin
Kenyatta	44.82%	50.07%	+5.25%	Higher than error margin
Odinga	44.36%	43.31%	-1.05%	Within error margin
Mudavadi	5.18%	3.93%	-1.25%	Within error margin
Kenneth	1.61%	0.59%	-1.02%	Within error margin
Karua	0.84%	0.36%	-0.48%	Within error margin
Dida	0.20%	0.43%	+0.23%	Within error margin
Kiyapi	0.05%	0.33%	+0.28%	Within error margin
Muite	0.00%	0.10%	+0.10%	Within error margin
Rejected Votes (IEBC)	0.00%	0.88%	+0.88%	N/A
Undecided (Polls)	2.95%	0.00%	-2.95%	N/A
<ul style="list-style-type: none"> • <u>Notes:</u> Total positive variance = +6.74% • Total negative variance = -6.75% <p>Source: Ipsos-Synovate, Media Release, 15 March 2013.</p>				

Figure 1: Preferred Potential/Actual Presidential Candidate* - Time Series (March 2009 – Feb. 2013)



Source: Compiled by T. Wolf

*For purposes of visual clarity, only the four most popular candidates are shown with all the rest being combined under “others” aside from those stating they were “undecided” or not prepared to vote for anyone (i.e., “none”).

One other critical fact should be noted here. Removing the 188 respondents who were either “undecided” or would not disclose their preference, the actual results of IPSOS’ February survey are: Kenyatta-Ruto 46.4% and Odinga-Musyoka 45.4%, and keeping in mind this survey’s error-margin of about 1.3%, this puts Jubilee’s total within a range of 45.1%-47.7%. Relevant, too, is Jubilee’s gain of 6% since IPSOS’ January survey – a trend that must be assumed to have continued over the last two weeks of the campaign. (Recall here that the survey’s fieldwork dates were 15-19 February.) The question thus becomes far less one of why the poll was ‘wrong’, and much more one of ‘how close to the IEBC’s figures would IPSOS have been had it been possible to conduct an additional survey a day or two before the election

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itself'? In other words, how 'accurate' must a survey be to win the author's approval?

These survey results, compared with the official results, raise another critical question that the author quite conveniently avoids: how certain is he that the latter are absolutely correct? And to the extent that they may not be, what implication does this have for all the vacuous 'weaknesses' he ascribes to IPSOS' survey research capacity and products? This question should be kept in mind as the following issues are considered.

Methodology Distortion-Interpretation Errors

Beginning with the author's numerous criticisms of IPSOS' methodology, it is helpful to re-visit his own Table that shows the release- and data collection-dates for the four surveys he discusses (p. 15). Much of his misplaced criticism of the surveys' results stems from his failure to identify the contrasting methodologies associated with each one: face-to-face/household surveys, and CATI (mobile phone) instruments. For purposes of clarity, I reproduce his table below (Table 3), which lists four surveys (together with their data-collection and release dates); then, I include my own (Table 4), which covers those listed by the author, together with four earlier ones (21 January - 4 February).

Table 3: IPSOS (Kenya) Pre-Election Polls, 13 February - 22 February*

Survey Release Date	Data Collection Dates
13 Feb.	12 Feb.
18 Feb.	13-15 Feb.
22 Feb.	15-19 Feb.
24 Feb.	15-19 Feb.

Source: * A. Makulilo (2013: 15).

In addition to the dates of data-collection and media release, (my fully factual) Table 4 also indicates: methodology, sample size, error-margin, and results for the two main presidential candidates (Odinga and Kenyatta).

Table 4: IPSOS (Kenya) Pre-Election Surveys, 21 January - 22 February, 2013

Release Dates	Survey Dates	Methodology	Sample Size	Error-Margin	Odinga	Kenyatta
21 Jan.	17-20 Jan.	CATI	2,610	2.00%	40%	36%
25 Jan.	12-20 Jan.	Household	5,895	1.28%	46%	40%
28 Jan.	24-27 Jan.	CATI	2,257	2.06%	45%	41%
4 Feb.	31 Jan.-2 Feb.	CATI	2,227	2.08%	44%	40%
13 Feb.	12 Feb.	CATI	1,074	2.99%	33%	40%
18 Feb.	13-15 Feb.	Household	2,500	1.96%	43%	43%
22 Feb./24 Feb.	15-19 Feb.	Household	5,971	1.27%	44%	45%

Source: Compiled by T. Wolf

*CATI - Computer-Assisted Telephonic Interviews

**All margins-of-error are +/- the percentage shown

Having shown the details of each of these surveys, a few additional points of clarification in response to the author's misinterpretation/confusion may be made. Whereas the author lists separate polls for 22 and 24 February (based on data-collection during 15-19 February), these were actually part of a single survey, though released in two sections (on these dates - and made explicit in these terms to the media houses to which they were presented/sent): one that included presidential-ticket popularity, and another that captured views about the first presidential debate. (The second presidential debate took place on February 25.) The author should thus not find it "surprising" that "two separate polls were conducted at the same time" (thus leading him to mistakenly list these two media releases separately). (The fact that this second release was posted separately on the company's Website may have caused this confusion, yet if so, he could easily have clarified the matter by contacting the company's Nairobi office - which he never did, a most baffling omission for anyone undertaking serious research; indeed he should have directed his incredulity that the "IPSOS experts were not aware of their own polls" (p. 24) at himself. But the reasons why the 22 February survey

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included a section on the (first) presidential debate should be applauded, not criticized: (1) with a sample size about four times larger than the original (CATI) poll – that was limited to those claiming to have listened to/watched it, as noted above – and (2) asking such questions after a substantial period of time had elapsed, would together improve the reliability of the results obtained as compared with the initial, ‘day-after’ (as noted, CATI) poll. As such, the author’s denigration of IPSOS’ methods is again completely misplaced. Other key relevant facts related to this series of IPSOS’ surveys include the following:

- 1) Three of the four CATI polls (21 January, 28 January, and 12 February) were commissioned by Royal Media Services (and included in Citizen-TV evening newscasts). Two other firms were also commissioned to undertake identical surveys so that the results could be compared. (Why is the author silent about this?) The use of CATI methodology was determined by the client, as was the (approximate) sample size.
- 2) The other CATI poll, released on 13 February, was based on the first of two presidential debates (involving all eight candidates). The sampling universe was (as noted above) limited to those contacted who claimed to have “followed” the debate, whether on radio or television. Its results are thus not at all comparable to any of the other polls in this (or in the author’s) Table (and should thus have never been included in his Table, though perhaps mentioned separately).
- 3) Finally, regarding the ‘shifting’ gender balance shown on several of these survey media-releases, the author is correct to note that “women are slightly the majority population in Kenya” (p. 20). However, he fails to recognize that the gender distribution we obtained in our final survey (54% male) is probably a more accurate reflection of the electoral reality, given what is known about the higher rate of voter registration/voter turnout among men (in Kenya, at least; for example, in IPSOS’ June, 2013/post-election survey, 4% more men than women reported having voted in the election). Prior to that, whatever gender ratio was obtained in terms of respondent-capture, the raw data were weighted to bring them into line with the 51%-49% female/male ratio known to be the (census) reality. In any case, what empirical evidence does he possess to prove that men and

women vote differently to any extent in Kenyan (or even in Tanzanian) elections? In other words, like most of his other criticisms, this 'weakness' that he alleges constitutes nothing more than a trivial irrelevance.

Also related to methodology, aside from the specific Kenya survey data he (incorrectly) cites, he commits numerous serious omissions and factual errors. For example, in his reference to the failure of leading U.S. pollsters (Roper, Gallup) to predict Truman's razor-thin victory over Dewey in the 1948 presidential election, he fails to mention that neither firm had employed fully 'scientific'/random sampling of the voting population; rather, they based their sampling on models of participation (turnout) in previous elections, which in this case led to a significant underestimation of certain categories of voters: women, African-Americans, and the poor (discussed at some length by Igo, 2007: 137-8; 186-90). Likewise, the 'Bradley-effect' that explains why B. Obama out-scored H. Clinton in the Democratic Party's New Hampshire primary election in 2008 - yet went on to lose to her in the actual voting (i.e., the 'shyness' of respondents to confess that they were not prepared to vote for someone of colour - of which he makes no specific mention, and which is also ironic given that he inserts six citations of published works about this polling 'failure'!) - was no fault of the pollsters (who, after all, just reported what their respondents told them; for a rigorous exploration of this issue, see Hopkins, 2009; for its consideration in Obama's wider 2008 race, see Issenberg, 2012: 291-7). He also seriously misleads his readers when citing Silver's negative portrayal of the U.S. pollster Zogby in his attempt to fault the credibility of *all* such survey firms. (Silver's review of how such firms performed in the most recent U.S. election is generally laudable, and he ranked IPSOS 6th best out of the 23 polling organizations he assessed; Silver, 2012a). As Silver concluded: "In my view, there will always be an important place for high-quality telephone polls, like those conducted by major news organizations, which place calls to cell-phones. And there may be an increasing role for online polls, which can have an easier time reaching some of the voters, especially younger Americans, that telephone polls are prone to miss" (Silver, 2012b).

Next, and perhaps even more serious for an academic, his questions about the representativeness of IPSOS' sampling frames in terms of demographic characteristics reveal a gross misunderstanding of the process of national random (household) sampling (an issue discussed in more detail below), which is exactly the same as employed in the Afrobarometer surveys, for

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example, of which he (again, quite surprisingly - at least in terms of methodology) makes no mention/reference (despite Tanzania's inclusion in this award-winning research initiative since Round 1 in 1999).

On a closely related matter, the author makes erroneous statements regarding sample size. Specifically, while raising doubts about IPSOS' sample sizes of 2,500 and even nearly 6,000 immediately prior to the election (p. 21), he fails to mention that nearly all U.S. presidential polls, for example - including those praised by Silver - employ (with irrefutable justification) samples of only around 1,500 - and this for an electorate of over 200 million voters! (Afrobarometer surveys generally use samples of 2,400, though in some cases only around 1,400.)

But perhaps the most revealing example of his analytical limitations is his statement that "...opinion polls in a conflict torn society should exhibit some unique methodologies in order to provide an objective projection and analysis of politics in such a society" (p. 13-4). First of all, such voter intention polls only seek to capture such intentions at the time they are conducted, so (unlike exit polls) the issue of "projection" does not arise. Neither do they in themselves constitute an "analysis of politics"; that is left to political analysts (who may or may not be employed/utilized by those conducting/releasing the results of the surveys). At the same time, and far more important, what sort of "unique methodologies" does the author have in mind? My argument is that none exist; in any case, the author offers none. Instead he goes off into diversionary tangential issues such as the financing of such polls, the salience of ethnic identity in candidate-attraction for voters, "mistrust" among Kenyans, the framing of questions, timing, and other (perhaps interesting, but here) completely irrelevant matters.

Given such failings of his own, it is rather ironic for the author to claim that "Normally, pollsters refrain from admitting limitations of their methodology and polling outcome" (p. 2). At the very least, while I have no idea what data-base he used or consulted to determine what 'normal' pollster behaviour is in this regard, it certainly does not apply to the company at the centre of this article, as we/I constantly refer to factors that could result in deviations from our voter-intention survey findings (for example, see reference to Wolf, 2013, below), including my comments about respondents sometimes "lying" to survey interviews on certain questions, cited by the author (p. 22) as 'proof' of the shoddiness of our results, even if, based on my decade of experience in the profession, such deviations are minimal, and

highly specific with regard to question-content and survey type (about which, see more details below).

Factual Omissions/Distortions

The author also commits numerous factual omissions and distortions. First here is his assertion that IPSOS (then Synovate, as noted) failed to include Zanzibar in its 2010 pre-election voter-intention polls in Tanzania. This is incorrect. According to Synovate's Country Manager at the time: "What we refused to do was to divulge the findings for Zanzibar and Pemba. This is because of the stance of the Zanzibar government on [the] publishing of polls" (Personal Interview, Nairobi, 23 January, 2014).

Next, in discussing the record of political polling in Kenya, the author conveniently fails to mention that (as suggested above) at least four research firms conducted and released voter-intention polls before the election, with IPSOS being the most accurate: giving Uhuru Kenyatta (the eventual winner) a (slight) lead in its final poll. Why does he ignore them? Surely if his focus is the *politics* of polling in this Kenyan election, he would at least need to show how IPSOS is situated in this wider (survey) universe. And referring to a point made earlier: whether or not he believes Kenyatta/Ruto did (barely) achieve the outright majority the IEBC declared that they had, does he believe the IPSOS' pre-election survey work was better or worse than these other three firms, and on what empirical facts is his opinion based? Alternatively, if he believes that *all* Kenyan survey firms performed equally badly, let him say so, and substantiate such a claim.

More generally, while the author is quick to conclude that the nature of Kenyan/African society presents "critical challenges (again, apparently discounting the highly acclaimed Afrobarometer surveys) which are not easy to be fixed by pollsters" (p. 23), he is largely silent on the resultant 'failures'. Indeed, he conveniently ignores IPSOS' highly credible past record in measuring (not "projecting" or "predicting"!) voters' intentions prior to the 2005 and 2010 constitutional referenda (i.e., within the margin-of-error in each case) and in connection with which there were no 'rigging' allegations, thus allowing for a more sound assessment of the polls, and before the 2007 presidential election (even though, as noted, he cites my 2009 article that includes all these results).

In this connection, he specifically deceives readers (p. 5) when referring to Cheeseman (2008: 168-9) by first mentioning the four companies that

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conducted polls before the 2007 election, and then quoting his text which refers to “these misleading polls” that “became the subject of great controversy and disagreement” following the declaration of incumbent president Mwai Kibaki as the winner, when in fact Cheeseman had made it clear – in text silently omitted by the author – that he was here referring to the poll results of two *other* firms that had both given Odinga a 10% poll-lead, as opposed to the 2% advantage he received from Steadman (now IPSOS) and one other firm. Indeed, Cheeseman regularly writes pieces for *The Nation* newspaper, making heavy – and explicit – use of IPSOS survey data in doing so. Is the author unaware of this, or is he again just conveniently ignoring facts that refute his distorted arguments? (Also related to the 2007 election, and while not central to his subject matter, he likewise ignores the vote-of-confidence the Kriegler Commission gave to the polls associated with it by rejecting any causal link between them and the 2008 violence that followed it; Republic of Kenya, 2008: 62-3.)

Likewise, the author (strangely) fails to mention the exit polls conducted during both the 2007 and 2013 elections (Gibson and Long, 2008; Long, Kanyinga, et al, 2013), the results of each largely confirming Steadman’s/IPSOS’ final voter intention polls (both – as already noted – conducted about two weeks before the events). As the authors of the latter publication concluded: “Overall, we estimate that Odinga took around 45.3% of the vote and Kenyatta 45.6%, a statistical tie” (Ferree et al, 2014: 15). Note again the above-described re-calculation of IPSOS’ final results: Kenyatta-Ruto, 46.4% and Odinga-Musyoka, 45.4% – that is, statistically identical.

Likewise, he fails to even mention the controversy surrounding the official result of this more recent contest (*Africa Confidential*, 2013; *Africog*, 2013; Carter Center, 2013; Ferree et al, 2014; MARS Group, 2013; Kenya Television Network/KTN, 2014), as well as the voluminous evidence of irregularities presented in the two election petitions brought to challenge the official results. Even if the Supreme Court ultimately dismissed both, the fact that much of the evidence was time-barred and thus never examined by the Court (according to one of Odinga’s lawyers, “about 80% of what we had amassed for the case”; Personal Interview, Nairobi, 23 April, 2014), suggests that the actual votes won by the various candidates is unlikely to ever be known. Indeed, the Court’s decision itself has been held up to highly critical scrutiny (Kegoro, 2013; Maina, 2013; Ongoya, 2013), also all conveniently ignored by the author. Altogether, such gaps are especially puzzling considering his own comment (p. 2) that “polls have a truth benchmark, that is, election

results obtained from a free and fair election.” Yet nowhere in the entire article is the controversy regarding the official results of this 2013 Kenyan election mentioned, especially as this might impede any rigorous interrogation of the accuracy of the polls (another issue that I presciently raised in my pre-election media piece (Wolf, 2013).

(By contrast, the author makes specific reference to the ‘gap’ between ‘official’ and ‘true’ results – including those captured in polls – in his detailed consideration of the pollsters’ performance prior to the 2010 election in Tanzania: “What is challenging is the fact that while polls are fundamentally based on probability sampling the [Tanzanian] electoral field is systematically skewed to favour the ruling party” (Makulilo, 2012: 53). So why does he totally ignore this issue for both the 2007 and 2013 Kenyan elections, even if in this latter case the issue is control of the state machinery, rather than a ‘ruling party’ as such?)

It is also contradictorily ironic that he quotes presidential candidate Peter Kenneth’s rejection of the pre-election polls of mid-January as biased in favour of the two leading candidates, yet IPSOS consistently gave him a higher percentage than he obtained from the Independent Election and Boundaries Commission (IEBC) in the election itself: for example, 1.61% in our final (February) survey, as opposed to just 0.59% from the IEBC. In any case, as a political scientist (or even an ordinary citizen with even a minimum of political savvy), the author should eschew taking such self-serving assertions by politicians at face value (a central theme of my 2009 article, which he is encouraged to revisit). But perhaps he has never undertaken actual field research in Kenya, so that at least some of his naïvete on this issue can be at least understood, if not forgiven. (Though this would assume Tanzanian politicians whom he presumably knows better are cut from altogether different cloth).

This leads to another more general and misleading claim by the author: that such results encourage/result in a ‘band-wagon’ affect by benefiting whoever is shown to be leading in the polls. In his words: “...[P]oll findings are in themselves significant [sic] to influence the actual voting as well as voter turnout. Usually, the electorates take such findings as results” (p. 3). Yet, characteristically, the author offers no evidence in this regard, whether in Kenya or anywhere else, yet much exists to the contrary. In the final (several) polls before the 2007 Kenyan election, for example, Steadman (now IPSOS) gave Kalonzo Musyoka about 10% of the vote, which is exactly what

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he achieved, within the poll's margin-of-error (and Musyoka - unlike Odinga - never questioned the results of that election), yet if the author's assertion were true, he would have attained much less since these polls showed his chances of winning were non-existent (and Steadman's first post-election survey of 2008 revealed that nearly two-thirds of voters had been aware of the pre-election polls). Moreover, how could Odinga have overtaken Musyoka when the latter enjoyed such a firm lead until mid-2006? (And again, why does the author ignore these findings, yet cites my 2009 article containing such relevant data when it suits him to do so?) Even more relevant to the author's article, how could Kenyatta possibly have overtaken Odinga shortly before the 2013 election (as noted: by 0.4% in IPSOS' last poll (44.8% vs. 44.4%) - and then significantly out-score Odinga in the election itself (whatever his 'real' vote-total) if the latter had been leading the former in every poll the company had conducted and released over the previous three years (and in all polls conducted by all other survey firms as well)?

For example, (in IPSOS' surveys as shown in Figure 1) the latter 'led' the former 49% to 14% in October, 2010, by 32% to 22% in April, 2012, and by 46% to 40% in January, 2013. Is he suggesting that if no polls had been conducted/released by any firm Kenyatta would have defeated Odinga by an even greater margin than by the 7% that he did (according to the IEBC)? If so, can he prove this? (For a comparative perspective, looking back on the first four decades of pre-election polls in the US, Monroe could conclude that "public opinion polls seem to have little or no effect on voting behaviour"; Monroe, 1975: 125.) At the same time, evidence exists - in Kenya, at least - that such polls do have a major impact on pre-election party-elite strategy in terms of assessing the viability of candidates, the utility of alliances between particular political parties (representing blocs of the electorate, as opposed to policy-harmony), the selection of running-mates, the key geographic areas to invest in voter turn-out efforts, and so on. But of such practical and proven utility - part of the real 'politics' of the election as related to such polls (as suggested in his paper's title) - the author simply has nothing to say (as also noted above).

Another distortion is contained in a contradictory statement the author offers regarding the 2010 election in Tanzania. After attempting to trash the several polls that were released leading up to that contest, he then surprisingly states that the ruling party was confidentially "fed" the results of two polls conducted in Zanzibar by the Research and Education for Democracy in Tanzania (REDET), enabling it to "strategize its campaigns at the expense of

the opposition” (p. 5). Is the author suggesting that CCM gained an advantage by making use of erroneous poll results? Again, the (il)logic here is simply baffling.

Specific Misrepresentations of IPSOS’ Survey Results Related to the 2013 Election

The author twice suggests that IPSOS’ voter intention results “might” have been falsified “in favour of” one candidate especially in relation to the question posed on several surveys as to whether any respondents had recently changed their voting intentions (p. 16), but fails to indicate which candidate that was, or how the company could have gained by so doing (especially) when the other three companies’ overall results were extremely close to those released by IPSOS. (Or does he believe that the top management of all four companies were so ‘compromised’?) Moreover, if there was any truth to this, how would (all) “politicians, academics and the general public” have “discredited” the polls as partisan? (That is, including even the candidate/party/coalition allegedly ‘favoured’ by IPSOS – presumably, Kenyatta?!) And this statement is itself also false, in that no credible academic dismissed the polls, and they were generally accepted as credible by the public, even if some highly partisan voices stated otherwise: Jubilee, that they would indeed achieve a first round (50%+1) victory, and Odinga (in response to IPSOS’ last poll showing Kenyatta with a slight lead, as noted above), in likewise claiming that he had the ‘numbers’ for a first round victory. But such accusations again underscore that partisan voices are the least likely to render objective judgments on polls results.

Also in this regard, it is regrettable that the author failed to include IPSOS time-series (trend) chart-graph that shows these changes over time, i.e., Slide No. 36. If he had removed his erroneous inclusion of the mobile phone poll (of 13 February) that employed a different sampling frame (to include only those respondents who had heard/watched the two presidential debates, as mentioned above) he would have had no basis for criticizing IPSOS’ work on the basis of “timing” that he spends considerable space doing (p. 10-11). Perhaps more important, that chart-graph (Slide No. 36 in IPSOS’ PowerPoint presentation-release of 22 February, to which he refers) shows that one month earlier – the only previous poll undertaken after the completion of the voter-registration exercise – Odinga enjoyed a 6% lead over Kenyatta (as noted above, 46% vs. 40%). Given that the fieldwork for IPSOS’ final survey was completed on 19 February, and which showed this margin had vanished, that left two full weeks of active campaigning before

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the election itself (4 March). Why, then, does the author not recognize that the Jubilee 'ticket' could very well have continued to gain ground during that remaining period, at least to closely approach (in statistical terms) the official election result? Also relevant here - and which the author likewise conveniently ignores - are two other facts: (1) in this final (February) poll, about 2.5% of respondents claimed to be still "undecided" or refused to answer the question about their voting intentions, and (2) over half of the roughly 8% who expressed an intention to vote for one of the four 'minor' candidates failed to do so, but instead 'invested' in either of the two front-runners (just as I had indicated was likely to happen in my media piece published at this time (Wolf, 2013).

Let me confess one mistake in IPSOS' 22 February media-release which justifiably ruffles the author (p. 17): the question-wording of Slide No. 34: "Apart from President Kibaki, if presidential elections were held now, whom would you vote for if that person was a candidate?" Having examined the interview questionnaire I can assure him that the actual question-wording was: "Which of the following pair of candidates for president and deputy president are you going to vote for in the next general election?", or in Swahili: "*Je, ni nani utawapigia kura kama rais na mwenza wake katika makundi yafuatayo katika uchaguzi mkuu ujao?*" As such, he may dispense with his concerns about (in)appropriate question-wording once the actual ballot-choices had become official (again, concerns that would have been obviated had he requested to see the actual questions which we would have been only too happy to provide).

Next, while correctly emphasizing (and using an appropriate reference) the importance of voter turnout as a factor that can produce actual election results at variance with voter-intention polls, the author asserts that only by "comprehending the level of voter turnout" can a pollster "project" accurate results. Here, the author blurs the distinction between social science and even informed 'fortune-telling' (i.e., non-scientific guess-work).

First of all, prior to the voter registration period (Nov. 18 - Dec. 18), we at IPSOS always made clear that we had no firm basis for guessing/predicting the proportion of Kenyans that would actually register nationally, and among those who did, the variations in these proportions over the political landscape, a reality that would likely help/hurt one pair of candidates/political party or the other. After this exercise was completed, we re-adjusted our sampling frame at the regional level (i.e., new counties =

47) to reflect the IEBC's official registration figures, and which – we calculated on the basis of historical ethnic voting trends – did give a 2-3% boost to Jubilee. Beyond this, and contrary to what the author asserts, there is simply no way to scientifically “project” what these figures are on the ground (even if, in terms of global best-practice, there is nothing wrong with pollsters offering a variety of outcomes based on different turnout scenarios, especially when based on well-documented, uncontentious past data). Indeed, as we stated in our Press Release that accompanied the 25 January media briefing, “Together with such other unknown of factors such as turnout rates in various parts of the country on election Day, there is no solid basis for predicting the actual outcome as of now.”

In this regard, it is unfortunate that the author relied on a misleading (and sloppily edited) report in *The Star* newspaper (2013) which reported me as having said: “Most of these respondents could be lying to us...” (p. 22). Evidently, the journalist in question misunderstood my remarks here, since I had referred to the 91% of respondents who had claimed that they intended to register *and* vote in our previous (November, 2012) survey, that was completed just prior to the start of the one-month voter registration process (Nov. 18 – Dec. 18). By contrast, in that (25 January) presentation to the media, it was indicated at the outset (perhaps before this journalist entered the briefing hall) that only those who (a) claimed to be registered voters, (b) could name the nearest polling station to their residence, and (c) could name the polling station where they had registered/intended to vote (whether or not it was the nearest one – and if not, could give the reason why they had registered elsewhere) were interviewed. This same screening procedure was used in our final (February) survey as well. (Having our interviewers demand to see the actual voter registration slip was considered too invasive.) So whatever ‘lies’ may have been told in November, 2012 were largely ‘corrected’ in our final two surveys.

Second, and perhaps even more important, the author failed to note my subsequent statement (accurately reported by *The Star* in this same story) that “poll companies had generally been proven credible in the performance of the 2005 referendum on the constitution, during the 2007 general election, the 2010 referendum on the constitution and the numerous by-elections held in the country since 2007”, so that any such ‘untrue’ responses’ (on any questions) were “statistically insignificant” (*The Star*, 2013). Once again, this is a case of misleading/selective quotation.

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Whatever the case, it is thus simply incorrect for the author to claim that “Synovate did not ask any question related to voter turnout in all its February 2013 polls” (p. 24), an error which he could have avoided if he had examined the actual questionnaires (which – to repeat – would have been readily provided had he communicated an interest in this issue to us).

As far as actual turnout, whereas in the final (February) IPSOS survey, 98% of those interviewed (all of whom – as noted above – claimed to be registered voters and could name the polling station where they were registered) asserted they would vote, ‘only’ 86% did (according to the IEBC). Relevant here, however (as suggested above), is the fact that a selective examination of election results by several Nairobi-based governance NGOs (e.g., Africog, 2013; Mars Group, 2013), found a number of polling stations where total votes exceeded the number of registered voters; similar discrepancies were discovered by the Kenya Television Network’s “Inside Story”/“*Jicho Pevu*” documentary research team in preparing their three-hour piece on ‘what went wrong’ in the Kenyan election’ (2014), again raising the question as to what ‘true’ election results is the author basing his analysis on?

Yet another false claim by the author is that “Synovate claimed...that Uhuru Kenyatta would be ahead of Odinga” (in the actual election; p. 24). What we reported (as noted above) was that Kenyatta had obtained 44.8% and Odinga 44.4%, and with a margin-of-error of +/- 1.27%, we termed this what it was: a statistical tie. He also states that as part of that release, we “insisted that no one would win the required votes (i.e., at least 50%+1) on the first round.” As already noted, this claim is also blatantly false. We explicitly stated that voter turnout could allow this to happen; indeed, if he had made reference to my piece in the 24 February, 2013 *Sunday Nation* (Wolf, 2013) – which repeated in more detail what I stated to media representatives at the time of our 22 February release about how a first-round victory might occur – he would/should not have made any such statement.

On a related issue, likewise he has no basis for being “surprised” that in our 15 March/post-election analysis-release we allegedly “insisted that it was correct to project Uhuru as the likely winner” (p. 24), as we did no such thing. What we did in our 15 March release was to look (back) at the official/IEBC turnout figures, which showed (among other things) that in the 15 counties where Kenyatta had most support, the turnout was 90%, while in Odinga’s 15 counties of greatest support it was only 84%, which *accounts for* (not “projects”) the official results (again, leaving aside any doubts

subsequently raised in the election petitions and forensic examination of results by several NGOs, as noted above).

The author also complains about the failure of IPSOS to release voters' intentions as expressed in its surveys in terms of specific demographic correlations (technically known as "cross-tabulations"); indeed, he asks why IPSOS chose to "avoid finding out" about the degree of ethnic polarization in its voter-intention surveys (p. 20). Why no survey firm in Kenya (and Tanzania?) releases important political results in terms of ethnic distributions, and why media houses indicate they would refuse to disseminate the same if they did, is an important policy issue over which legitimate disagreements exist. Yet if the author had asked (if he does not know already) he would have been told that such detailed findings (in terms of religion, employment status, education level, and gender, etc., as well as ethnic identity) are available for purchase by anyone, because all the relevant data are collected and available – at a cost. If he (or his academic institution) lacked the required funds, he may deserve some sympathy, and one hopes that in future if he wishes to interrogate such survey results he will be able to source the necessary funding. However, for him to assert that "the Synovate polls did not have even the demographic profile" is yet another blatant falsehood: it did (i.e., Slide No. 8 in the 25 January media release; Slide No. 11 in the 22 February media release – though as suggested, in neither cases were ethnic proportions shown.)

Yet another distortion offered by the author is his assertion that IPSOS had no basis for its post-election (i.e., 15 March) claim "to have projected close to the electoral outcome with a high level of precision at the county level i.e. 91% correct with regards to Kenyatta's counties and 88% correct with regards to Odinga's counties" (p. 23), when the company's final pre-election poll "did not have any single question about preferences by respondents in relation to candidates for presidential or county elections." In fact, he makes two distinct mistakes here. First, this post-election analysis was based entirely on the company's final household survey (as noted above) conducted 15-19 February and released on 22 February (not on the 24 February release) which most certainly did have a question on presidential voting intentions. (Indeed, the author himself elsewhere quotes the figures obtained on this matter!) The second mistake is that no question was asked about county-level election choices; as such, which "candidates...for county elections" is he talking about? When reference was made to "Kenyatta's" vs. "Odinga's counties", this only involved the number of votes each of them

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had received in these particular counties; it had nothing to do with any county elections (i.e., for governor, senator, etc.). (Note, however, that *The Standard Group* had sponsored IPSOS polls on races for various officials in several of the larger counties, including Nairobi and Mombasa, which were all published – and the results quite closely matched the actual election tallies as well, about which the author is once again self-servingly silent. Moreover, such a high level of accuracy that IPSOS achieved at the county level should have been praised by the author, particularly given the higher margins-of-error attached to such sub-sets of the entire sample.) Indeed, some of his (totally misplaced) criticisms might appear more credible if he gave praise where it is clearly unavoidable not to do so; once again, however, he fails on this standard as well.

A final example may be offered to illustrate the author's weak understanding of basic survey analysis. Referring to the 13 February 2013 IPSOS (CATI) poll following the first presidential debate, he states that "Kenya [sic] made a remarkable increase from 36.9% before changing mind [sic] to 39.8%" (p. 16). Note the change: 2.90%. Given that the margin-of-error for this poll (as reported together with the actual findings) was +/- 2.99%, it is therefore erroneous for the author to claim that Kenyatta benefited from "a remarkable increase" according to this poll, since statistically, there was hardly any.

Conclusion

One critical and glaring conclusion that must be drawn from this article is that the author lacks the most basic appreciation of the motivation of survey firms – at least those (such as IPSOS) whose main business is private sector market research – which enter the field of political/election surveys, including those that capture voter-intentions. This is to prove their worth by doing scientific, credible work, and thereby (hopefully) gaining more business from clients of whatever sort. This is so even if, as has been stressed, various factors can affect the accuracy of such polls over which these firms have no control – and which behoove them to identify, when appropriate, both before and after the fact. As such, the author's assertion that "pollsters can manipulate and politicize numbers and yet proceed to defend their positions [one reason being] that a market research company...could lose customers and hence profit" (p. 4) puts the reality precisely upside-down! And any individual within such a firm discovered to have even attempted to so 'rig' the results – nearly impossible given that 3-5 other individuals would know what they are – would therefore soon be among the unemployed.

Given the massive number of errors and omissions (including several obvious ones that, if he had corrected them, would have undermined nearly all of the author's own argument), I submit that this piece utterly fails to contribute to any deeper understanding of voting behaviour in Africa (or anywhere else), or to a better appreciation of the strengths/weaknesses of voter-intention surveys. On the contrary, it serves only to confuse readers as to the fundamental requirements of sound, credible surveys so that they can make up their own minds as to which ones – whether in Africa or elsewhere – actually fulfil them, and which do not, or what additional information they would require in order to be able to assess their worth. And yet, as the author correctly notes, public affairs surveys – including pre-election voter-intention polls – are increasingly common across Africa's more liberalized polities, making it essential that those who publish and consume them understand both their strengths and weaknesses. As such, the author, most regrettably, missed a useful opportunity for such public enlightenment through this article. It is therefore hoped that hopefully now having been fully enlightened, his next such effort can achieve this worthy aim – at least with regard to IPSOS' surveys. In the meantime, on behalf of IPSOS-Kenya, all those interested in the company's survey work are urged to contact us directly whenever questions regarding either methodology or survey results arise. Hopefully, such direct communication will help prevent the sort of misinterpretations and distortions that the above reply has sought to correct.

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