**Appendix A: Replicability of Survey Results (Comparison to Afrobarometer)**

The following Appendix presents the questions and (weighted) responses of the 17 Afrobarometer questions that were replicated by the original survey conducted for this article, not including the demographic questions presented in Appendix B. The tables report questions that were asked by both the original survey as well as either (or both) Round 5 and/or Round 6 of the Afrobarometer. Several other questions were originally borrowed from the Afrobarometer, but the question wordings were changed after pre-testing, and therefore these modified questions are not included below. Note that the question ordering is not the same across instruments and that other questions unique to the author’s survey or to the Afrobarometer are not presented below (question numbers for each instrument are included). Such discrepancies in question ordering, along with differences in sampling design (the original survey was not conducted in the three northern regions of the country) and timing of implementation (Afrobarometer Round 5 was conducted in 2013, the author’s survey was conducted in 2014/2015; and Round 6 was conducted in 2015) may account for differences in the findings.

|  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- |
| In this country how free are you to say what you think? | | | | | |
|  | Not at all Free | Not very Free | Somewhat Free | Completely Free | Don’t Know/ Refused |
| **Author’s Survey (Q54)** | 16.00% | 14.87 | 23.24 | 45.04 | 0.84 |
| **Afrobarometer**  **Round 5 (Q17A)** | 10.03% | 20.15 | 33.09 | 33.03 | 3.70 |
| **Afrobarometer**  **Round 6 (Q15A)** | 7.28% | 17.91 | 31.09 | 41.73 | 1.99 |

|  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- |
| In this country how free are you to join any political organization you want? | | | | | |
|  | Not at all Free | Not very Free | Somewhat Free | Completely Free | Don’t Know/ Refused |
| **Author’s Survey (Q55)** | 4.56% | 4.84 | 20.20 | 69.01 | 1.32 |
| **Afrobarometer**  **Round 5 (Q17B)** | 5.67% | 8.40 | 27.16 | 52.68 | 6.09 |
| **Afrobarometer**  **Round 6 (Q15B)** | 4.53% | 6.91 | 24.97 | 59.99 | 3.60 |

|  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- |
| In this country how free are you to choose whom to vote for without feeling pressured? | | | | | |
|  | Not at all Free | Not very Free | Somewhat Free | Completely Free | Don’t Know/ Refused |
| **Author’s Survey (Q56)** | 3.60% | 3.81 | 15.74 | 75.73 | 1.09 |
| **Afrobarometer**  **Round 5 (Q17C)** | 3.21% | 5.71 | 19.76 | 65.64 | 5.68 |
| **Afrobarometer**  **Round 6 (Q15C)** | 2.73% | 5.37 | 20.87 | 68.34 | 2.70 |

|  |  |  |  |
| --- | --- | --- | --- |
| Do you feel close to any particular political party? | | | |
|  | Yes | No | Don’t Know/ Refused |
| **Author’s Survey (Q19)** | 37.80% | 61.99 | 0.15 |
| **Afrobarometer Round 5 (Q89A)** | 41.49% | 53.29 | 5.22 |
| **Afrobarometer Round 6 (Q90A)** | 45.05% | 47.18 | 7.77 |
| [*If respondent replied yes to the previous question*]  **Which political party do you feel close to?** | | | |
|  | RDPC | SDF | Other |
| **Author’s Survey (Q19A)** | 74.54% | 14.36 | 10.08 |
| **Afrobarometer Round 5 (Q89B)** | 69.98% | 9.35 | 12.86 |
| **Afrobarometer Round 6 (Q90B)** | 71.98% | 12.31 | 11.03 |

|  |  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- | --- |
| In general, would you describe the present economic condition of Cameroon as good, bad, or neither good nor bad? | | | | | | |
|  | Very Good | Fairly Good | Neither Good nor Bad | Fairly Bad | Very Bad | Don’t Know/ Refused |
| **Author’s Survey (Q57)** | 2.01% | 14.06 | 25.69 | 21.09 | 34.29 | 2.86 |
| **Afrobarometer**  **Round 5 (Q3A)** | 3.09% | 27.73 | 20.21 | 27.36 | 17.34 | 4.27 |
| **Afrobarometer**  **Round 6 (Q4A)** | 4.34% | 32.19 | 17.01 | 29.75 | 14.71 | 2.01 |

|  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- |
| How much do you trust the President of the Republic? | | | | | |
|  | Not at all | Just a little | Somewhat | A lot | Don’t know / Refused |
| **Author’s Survey (Q43)** | 22.59% | 22.23 | 23.50 | 27.71 | 3.97 |
| **Afrobarometer Round 5 (Q59A)** | 15.93% | 20.62 | 25.28 | 30.70 | 7.48 |
| **Afrobarometer Round 6 (Q52A)** | 10.73% | 18.5 | 27.33 | 39.43 | 4.01 |

|  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- |
| How much do you trust the National Assembly? | | | | | |
|  | Not at all | Just a little | Somewhat | A lot | Don’t know / Refused |
| **Author’s Survey (Q39)** | 32.85% | 28.39 | 22.34 | 10.49 | 5.93 |
| **Afrobarometer Round 5 (Q59B)** | 25.41% | 23.24 | 25.79 | 16.03 | 9.54 |
| **Afrobarometer Round 6 (Q52B)** | 23.73% | 27.38 | 25.96 | 17.59 | 5.34 |

|  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- |
| How much do you trust ELECAM (the electoral commission of Cameroon)? | | | | | |
|  | Not at all | Just a little | Somewhat | A lot | Don’t know / Refused |
| **Author’s Survey (Q40)** | 34.83% | 26.33 | 20.75 | 13.32 | 4.77 |
| **Afrobarometer Round 5 (Q59C)** | 25.32% | 25.79 | 21.41 | 16.37 | 11.10 |
| **Afrobarometer Round 6 (Q52C)** | 26.16% | 27.38 | 25.12 | 15.98 | 5.36 |

|  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- |
| How much do you trust the Ruling Party? | | | | | |
|  | Not at all | Just a little | Somewhat | A lot | Don’t know / Refused |
| **Author’s Survey (Q41)** | 35.96% | 21.89 | 22.42 | 16.21 | 3.52 |
| **Afrobarometer Round 5 (Q59F)** | 36.58% | 24.21 | 14.11 | 13.93 | 11.18 |
| **Afrobarometer Round 6 (Q52F)** | 29.53% | 25.32 | 26.77 | 12.41 | 5.96 |

|  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- |
| How much do you trust the Opposition Political Parties? | | | | | |
|  | Not at all | Just a little | Somewhat | A lot | Don’t know / Refused |
| **Author’s Survey (Q42)** | 52.07% | 23.58 | 11.61 | 7.45 | 5.29 |
| **Afrobarometer Round 5 (Q59G)** | 44.64% | 26.34 | 12.44 | 6.15 | 10.42 |
| **Afrobarometer Round 6 (Q52G)** | 39.75% | 27.03 | 19.83 | 7.52 | 5.87 |

|  |  |
| --- | --- |
| On a scale between 0 and 10, where 0 means completely undemocratic and 10 means completely democratic, where would you place our country today? | |
|  | Sample  Average |
| **Author’s Survey (Q30)** | 4.88 |
| **Afrobarometer Round 5 (Q46A)** | 4.84 |
| **Afrobarometer Round 6 (N/A)** | -- |

|  |  |  |  |
| --- | --- | --- | --- |
| In your opinion, what are the three most important problems facing this country that the government should address? | | | |
|  | **Author’s**  **Survey (Q18)** | **Afrobarometer**  **Round 5 (Q63)** | **Afrobarometer**  **Round 6 (Q60)** |
| Unemployment | 23.38% | 30.00% | 24.53% |
| Poverty | 11.42 | 9.49 | 6.15 |
| Infrastructure / Roads | 10.38 | 6.34 | 6.43 |
| Corruption | 9.67 | 10.02 | 10.03 |
| Security / Crime /  Boko Haram / Political Violence / Civil War | 5.34 | 1.45 | 6.86 |
| Education | 4.96 | 4.04 | 4.39 |
| Hospitals /  Health | 4.38 | 3.81 | 8.45 |
| Management of the Economy | 3.25 | 5.18 | 6.63 |
| Water Supply | 2.71 | 4.84 | 3.06 |
| Electricity Supply | 2.63 | 4.05 | 3.36 |
| Agriculture | 2.54 | 2.17 | 4.06 |
| Wages and Salaries | 2.33 | 8.32 | 3.44 |
| Taxes | 2.17 | 1.23 | 1.40 |
| Economic Inequality / Discrimination /  Tribalism | 3.38 | 0.33 | 0.22 |
| Democracy /  Political Rights | 1.38 | 0.88 | 0.13 |
| Food Shortage | 1.25 | 2.01 | 2.12 |
| Transportation | 1.00 | 0.77 | 1.02 |
| Don’t Know | 0.83 | 0.74 | 0.93 |

|  |  |  |  |
| --- | --- | --- | --- |
| How often do you get news from the radio? | | | |
|  | **Author’s Survey (Q10)** | **Afrobarometer Round 5 (Q13A)** | **Afrobarometer Round 6 (Q12A)** |
| Everyday | 33.74% | 40.66% | 45.52% |
| A few times a week | 29.59 | 26.65 | 25.31 |
| A few times a month | 13.86 | 6.64 | 8.32 |
| Less than once a month | 6.31 | 4.97 | 4.74 |
| Never | 16.50 | 20.94 | 15.93 |
| Don’t Know // Refused | 00.00 | 0.15 | 0.18 |

|  |  |  |  |
| --- | --- | --- | --- |
| How often do you get news from the television? | | | |
|  | **Author’s Survey (Q11)** | **Afrobarometer Round 5 (Q13B)** | **Afrobarometer Round 6 (Q12B)** |
| Everyday | 50.84% | 55.92% | 56.40% |
| A few times a week | 23.71 | 20.81 | 21.48 |
| A few times a month | 9.75 | 5.59 | 6.91 |
| Less than once a month | 4.01 | 1.64 | 2.73 |
| Never | 11.69 | 15.75 | 12.27 |
| Don’t Know // Refused | 00.00 | 0.30 | 0.21 |

|  |  |  |  |
| --- | --- | --- | --- |
| How often do you get news from the newspaper? | | | |
|  | **Author’s Survey (Q12)** | **Afrobarometer Round 5 (Q13C)** | **Afrobarometer Round 6 (Q12C)** |
| Everyday | 7.30% | 6.2% | 5.67% |
| A few times a week | 13.19 | 10.78 | 12.99 |
| A few times a month | 14.61 | 14.01 | 19.83 |
| Less than once a month | 11.69 | 14.90 | 14.36 |
| Never | 53.13 | 53.27 | 46.49 |
| Don’t Know // Refused | 00.00 | 0.84 | 0.67 |

|  |  |  |  |
| --- | --- | --- | --- |
| How often do you get news from the internet? | | | |
|  | **Author’s Survey (Q13)** | **Afrobarometer Round 5 (Q13D)** | **Afrobarometer Round 6 (Q12D)** |
| Everyday | 9.42% | 5.52% | 9.64% |
| A few times a week | 10.09 | 11.13 | 10.12 |
| A few times a month | 9.15 | 7.77 | 11.90 |
| Less than once a month | 4.43 | 7.56 | 8.39 |
| Never | 66.82 | 67.05 | 59.38 |
| Don’t Know // Refused | 00.00 | 0.99 | 0.57 |

**Appendix B: Survey Sample Details**

**Sampling Design**

A total of 2,466 respondents were interviewed during this period, but 67 had to be dropped due to issues with an interviewer in the Mvila department in the South region. Table A presents every district sampled in the survey. I personally implemented the survey in every enumeration area with the aid of one or two assistants. There were five assistants in total, but they worked in different areas.

**Table A: Sampled Areas**

|  |  |  |
| --- | --- | --- |
| Region | Département | Arrondissement |
| Centre | Mfoundi | Yaoundé I, Yaoundé II |
| Nyong et Kellé | Matomb, Bot Makak, Ngog-Mapubi |
| East | Haut Nyong | Abong-Mbang, Angossas, Doumé |
| Littoral | Wouri East | Douala III, Douala V |
| Wouri Centre | Douala I |
| Northwest | Boyo | Belo, Fundong |
| Mezam Centre | Bamenda II, Bamenda III |
| Mezam South | Santa |
| Momo East | Batibo |
| South | Mvila | Ebolowa, Ngoulemakong |
| Océan | Kribi |
| Southwest | Mémé (Kumba Centre) | Kumba I, Kumba II, Kumba III |
| Manyu | Mamfé |
| West | Mifi | Bafoussam I, Bafoussam II, Bafoussam III |
| Noun Centre | Foumban, Foumbot |

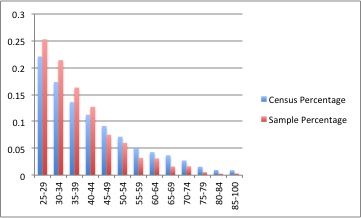
These areas were sampled on two characteristics. First, whether the *département* historically votes for an opposition political party, the ruling party, or “swings” between the two. Second, roughly half of the sampled *départements* within each group type were urban, and half rural. The distribution of *départements* can be seen in Table 2. The survey data have been weighted based on the 2005 Cameroonian census to provide a more representative picture of public opinion in Cameroon. However, the three northern provinces, Adamawa, North, and Far North were not sampled at all in the survey, so all inferences from the survey apply solely to the seven southern regions of Cameroon.

In urban areas, enumerators stopped at every 5th house or business. In rural areas, where population densities were too low, enumerators stopped at every available house or business. Enumerators began at the same randomly chosen location within the enumeration area, walking in opposite directions until sunset. Enumerators interviewed Cameroonian citizens who were twenty-three years or older because the voting age in Cameroon is 20, and I wanted to interview only citizens who would have been eligible to vote in the 2013 parliamentary and legislative elections.

**Survey Sample Characteristics**

Within the weighted sample, 48.79 percent of respondents were male, and 51.21 percent were female. The age distribution of respondents is presented below in Figure A, in comparison to the distribution of age in the general population (Cameroon census data projected to 2015). The majority of respondents were 40 years old or younger.

**Figure A: Distribution of Respondents’ Age**



Education was normally distributed around a mean of ‘some secondary schooling.’ The weighted percentages for level of education within the sample are reported in Table B below.

**Table B: Level of Education in the Sample**

|  |  |
| --- | --- |
| Level of Education | Weighted Proportion of Respondents |
| No formal education | 2.86 |
| Informal education only | 2.58 |
| Some primary schooling | 7.71 |
| Primary school completed | 17.72 |
| Some secondary schooling | 31.73 |
| Secondary school completed | 15.68 |
| Post-secondary school technical diploma | 5.66 |
| Some university | 6.42 |
| University completed | 5.89 |
| Post-graduate degree | 3.75 |

A series of questions were included to assess the level of income of each respondent’s household. Respondents were asked whether they owned various household items, including a radio, television, car, motorcycle, mobile phone, or computer. The average number of items owned is three items, with a normal distribution around this average. The most common combination across the sample was for a respondent to own a television, a radio, and a mobile phone. In addition, about 22 percent of the sample had running water inside of their house, 35 percent only had a well or tap within their household’s compound, and 43 percent had to fetch water outside of their compound (either at a community source or at a nearby river, stream, or lake). Similarly, 28 percent of the sample had a toilet within their house, while 62 percent had a latrine within their compound instead. Ten percent had no toilet or latrine within the house/compound. Finally, the plurality of respondents was self-employed (43.82 percent), for example as a farmer or market trader. Twenty-one percent of the sample was unemployed and looking for work, and 17 percent were formally employed full time. Eleven percent was not looking for work; this included students, housewives, and retired persons.

**Appendix C: Social Sensitivity Bias**

Many steps were taken throughout the period of data collection in order to avoid social sensitivity bias in collecting responses to the survey questions used in this analysis. Three strategies are discussed below: the (unsuccessful) use of a survey experiment to measure vote-buying, the construction of the question used as the dependent variable, and the natural variation of survey enumerators.

Part 1: List Experiment to Measure Vote-Buying

In recent years list experiments have become a common way for researchers to generate responses to sensitive questions in survey research. The structure of the list experiment (also known as the item-count technique) is designed to allow survey respondents to report their socially sensitive actions and opinions without having to reveal outright these thoughts and behaviors to the survey enumerator (Ahart and Sackett 2004; Corstange 2009; Droitcour et al. 1991; Imai 2011; Tsuchiya, et al. 2007). For example, in the American context, they have been used to reveal prejudice against African-Americans and other minority groups (Kane, et al. 2004; Kuklinski, et al. 1997; Redlawsk, et al. 2010; Streb et al. 2008) as well as socially sensitive personal behaviors (Braithwaite and Walsh 2008; Dalton, Wimbush, and Daily 1994; LaBrie and Earleywine 2000). More recently, researchers have also begun to apply the list experiment technique to vote-buying (Gonzalez-Ocantos et al. 2012).

In order to avoid social sensitivity bias, respondents are randomly split into a control group and one or more treatment groups. Each group is read a list of beliefs or activities and asked with how many items they agree. The list is identical for each group, except that the treatment group receives an additional belief or activity that is socially sensitive. For the survey I conducted in Cameroon, I implemented the following list experiment (derived from Gonzalez-Ocantos et al. 2012) in order to reveal the number of respondents who received money or a favor in return for their vote in the most recent election:

**I am now going to read a list of activities concerning the legislative and municipal elections that took place on September 30, 2013. I do not want to know WHICH ones happened during the election, but only the total NUMBER of activities that took place. So when I list each activity, you should count the ones that you saw happen. When I ask at the end how many took place, you should give me a number. Do NOT tell me which ones in particular occurred.**

**1. They put up campaign posters or signs in your community.**

**2. They visited your home.**

**3. They placed campaign advertisements on the television or radio.**

**4. They sent you an email.**

**Treatment: They gave you money or did you a favor.**

The question was designed to avoid both “ceiling” and “floor” effects (Glynn 2013), meaning I included options that would make it highly unusual for a respondent to give the option of ‘zero’ or of ‘four’ (or ‘five’ if in the treatment group), thus revealing their response.

When the researcher compares the two groups’ total averages, the difference in the mean number of choices between treatment and control groups is an estimate of the percentage of respondents in the treatment group that counted the socially sensitive item in their list. Thus, if the average response for the control group if 2.45 while the average for the treatment group is 2.75, then 30 percent of respondents in the treatment group have “reported” that they received a gift or a favor in the previous election.

*Criticism and Concerns with the List Experiment*

Having personally conducted the list experiment, surveying over 340 individuals, I have identified a number of issues with the list experiment technique. Overall, while the list experiment is designed to decrease social sensitivity bias, its cognitive complexity introduces other forms of bias that may be difficult to control for. In a meta-analysis of list experiments in psychology, Tourangeau and Yan (2007) have already voiced some concern about this technique. They find that, taken together, these studies produce a positive treatment effect, but that this overall effect is not significant. However the authors do not discuss the potential reasons for these results.

I highlight several potential sources of bias in the list experiment technique, and argue that the direction of bias cannot always be accounted for. First and foremost, I show that in a face-to-face interviewing context, a large number of respondents simply do not follow the instructions, revealing to the survey enumerator the specific list items that they are counting. This biases the treatment effect downward by undermining the point of the list experiment, and is also potentially correlated with level of education, possibly introducing additional bias depending on the treatment being measured.

In addition, I echo Kramon and Weghorst’s (2012) concerns about the problem of satisficing and agree that a potentially large proportion of a sample may not be providing accurate responses. I posit, further, that if people are satisficing, then *being in the treatment group itself will bias the treatment effect towards 0.50.* Because the treatment group by definition has more options than the control group, its average will be higher when people report random numbers. If the true mean is less than 0.50, then there will be upward between-group bias, and if it is less than 0.50, then there will be downward bias. I argue that satisficing—not lying—produces unpredictable forms of bias. Thus problems with the implementation of the list experiment can introduce different forms of bias in the measure of the sensitive item. If these issues are not fully accounted for, then it is difficult for the researcher to ascertain the true treatment effect of the list experiment. For these reasons, I do not use the results of the list experiment in this article.

*The Difficulty of Following Directions*

The most obvious issue that arose during the implementation of the list experiment was the inability of a sizable number of respondents to correctly follow directions. After discovering this issue, survey enumerators were asked to record whether or not each respondent gave a number in response, or instead listed which items they were counting. *Fully 31 percent of respondents failed to correctly give a numerical option for their response.*

Oftentimes respondents would say “yes” or “no” after each item. Other times they would list the items they agreed with after the list was finished. When respondents began to do this, I instructed the enumerators to stop the respondent and re-read the instructions, allowing the respondent to try to answer the question correctly without revealing all of their list items. However, this option was not foolproof, as respondents would frequently reveal all of their choices before it was possible to stop them, sometimes reacting quickly to the sensitive item which they found surprising. Even when it was possible to stop the respondent, many of them failed to follow the directions multiple times, never being able to figure out what was being asked of them.

For the first few enumeration sites, my assistants and I simply recorded all responses equally—whether or not the directions were correctly followed. This approach is clearly problematic, as respondents who reveal their list items are violating the purpose of the list experiment. By including both types of respondents (those who follow directions and those who do not) in the same category, the researcher biases their treatment effect downward if people who list their items are less likely to include the sensitive item.

This is especially problematic when the ability to follow instructions is correlated with particular demographic characteristics. As seen below in Table C1, in a region-level mixed effects logistic regression, where the dependent variable is the respondent’s ability to follow directions (recorded by the survey enumerator), level of education is significantly related to who could properly follow directions. People with less education are less likely to follow directions.

Thus if a researcher includes the responses of both direction-followers and direction-breakers in the same group, the treatment effect will be further biased if the sensitive item is correlated with the ability to follow directions. In this particular list experiment, if the responses of direction-breakers had not been separated from those of direction-followers, the treatment effect would be doubly diminished for the overall population because less educated citizens, who are more likely to sell their votes, are also less likely to follow directions.

**Table C1: Explaining the Ability to Follow Directions**

|  |  |
| --- | --- |
|  | Respondent Correctly  Followed Directions |
| Rural | 0.380  (0.253) |
| Female | -0.074  (0.162) |
| Age | -0.005  (0.007) |
| Education | 0.254\*\*\*  (0.049) |
| Socioeconomic Status | 0.068  (0.046) |
| Interest in Politics | 0.162\*\*  (0.073) |
| French Speakers  (*baseline = English*) | -0.697\*  (0.383) |
| Pidgin Speakers  (*baseline = English*) | -0.332  (0.216) |
| Constant | -0.048  (0.493) |

\*\*\*p <0.01, \*\*p<0.05, \*p<0.1

*n* = 887

*Satisficing and Bias from the Treatment Effect*

Satisficing can also be a serious issue with list experiments, which require significantly more cognitive effort than straightforward yes/no or multiple-choice questions (Armstrong, et al. 1975). Satisficing occurs when respondents find the task at hand too difficult or taxing, and thus skip one or more steps in the cognitive process required to accurately respond to the question (Krosnick 1991; Tourangeau 2000). If a respondent finds the instructions confusing or the list items too complex, he may not actually count the items on the list that apply to him, but instead report a more-or-less random number that seems to be a reasonable response.

Satisficing is different from not following directions. When someone does not follow the directions, they fail to provide a numerical response to the question. When someone satisfices, they give a numerical response that is not a true reflection of the number of activities or beliefs they agree with. Thus although it is possible to count the number of people who do not follow directions, it is not possible to detect the number of satisficers in a sample. Kramon and Weghorst (2012) have already highlighted some issues with satisficing in the list experiment approach. After implementing a non-sensitive list experiment in Kenya, and then asking respondents outright about these non-sensitive items, they find that more than 40 percent of responses did not match between the two question formats.

Unfortunately this satisficing doesn’t just create noise around the estimate, it also creates bias. The more satisficing there is in a sample, the more the treatment effect is biased towards 0.50 because the control and treatment groups do not have the same number of list items. If everyone reports random numbers, but one group can choose between *n* options, while the second group can choose *n +1* options, then the difference in means between these two groups, by definition, will be 0.50. For example, if the control group has four list items (*average of 4.0* = 2.0) while the treatment list has five items (*average of 5.0* = 2.5), then the average “treatment” effect will necessarily be 50 percent (2.5 minus 2.0 equals 0.5). Thus, the more common satisficing is, the closer the treatment effect will be to 0.50. If the “true” treatment effect is 0.50, then no bias is introduced. But if we do not know what the true treatment effect should be—and if the researcher is using a list experiment, then it is likely that the true treatment effect is not known—it is impossible to know if this bias exists, or if it is artificially pushing the treatment effect upwards or downwards.

Researchers have assumed that if satisficing occurs, it will be randomly distributed across the control and treatment groups, and therefore not create any bias in the treatment effect. This is not always a safe assumption, as the numerical average for the treatment group will be higher than the control group simply because there are more options to choose from in the treatment than in the control. Further, it is unclear what effect this bias will have unless the true treatment effect is already known, or it is possible to somehow identify satisficers. Researchers have tended to assume that the higher their treatment effect is, the better their list experiment must be working (Holbrook and Krosnick 2010), but it is entirely possible that a large treatment effect may instead be the result of extensive satisficing.

*Conclusions*

A number of partial remedies can be used to improve the clarity of the question and reduce satisficing. First and foremost, extensive pre-testing is an important step in working out misunderstandings. In particular, each list item (not just the treatment) should be asked explicitly in order to get a sense of how people understand each one. Additionally, a ‘practice’ list experiment might fix problems involved with following the directions of the actual list experiment. I implemented this technique during the second half of survey implementation and found it to be only partially successful. In total, 552 respondents received a ‘practice’ list experiment before the actual list experiment and 732 received no practice. Roughly 60 percent of respondents who did not get a practice list were able to correctly follow the directions of the ‘real’ list experiment. Sixty-eight percent of respondents who did get practice correctly followed the directions. Thus there was some improvement, but the method was not perfect. In addition, 6.52 percent of respondents who followed directions on the practice list experiment failed to correctly respond in the real experiment! Unfortunately this cannot tell us how practice affects satisficing.

In addition, Kramon and Weghorst (2012) recommend using a “tick” or tabulation system whereby a respondent is given an erasable list of the question items, so that they may check with a pen the items they agree with, and erase their responses at the conclusion of the question. In order to help illiterate respondents, they also recommend the inclusion of pictures or cartoons to help respondents visualize and understand the list options. They find that these techniques have the potential to decrease the number of satisficers and improve the quality of responses.

With all of these recommendations in place, the list experiment is still a risky option for researchers with sensitive survey questions. While asking a sensitive question outright produces response bias, it is clear that this bias will be downward. The list experiment, on the other hand, produces several forms of bias that can be conflicting in direction and difficult to know the direction of. Therefore, caution is needed in interpreting the results of any list experiment. Researchers tend to argue that the larger their treatment effect the better the list experiment must be working, but it critical to keep in mind that upward inflation of the treatment effect may be an artifact of satisficing more than anything else.

Part 2: Measurement of the Dependent Variable

In order to accurately measure the dependent variable, voter turnout, a few steps were taken to minimize response bias. First, after verifying during pre-testing that the question was, indeed, socially sensitive, I moved the question to the very end of the survey. I found that people were far more forthcoming with their responses after they had spent time building a rapport with the survey enumerator. Second, I randomly varied the ordering of the question with another question about voting in the 2011 presidential election. Half of respondents were first asked whether or not they voted in the 2011 election and were *later* asked whether they voted in the 2013 election, while for the other half of respondents, the order was reversed. Differences in responses are recorded in Table C2 below. I also placed several questions in between the two voting questions in order to make people cognitively consider the elections themselves and not just satisfice by responding ‘yes’ simply because they said ‘yes’ to the other election. Note that responses are very similar, regardless of question order.

**Table C2: Responses to Voting Question**

|  |  |  |
| --- | --- | --- |
| 59. With regard to the most recent parliamentary and municipal elections in 2013, that is the elections that were held on September 30 of last year, which statement is true for you? [*Read out options*] | | |
|  | Asked First | Asked Second |
| You were not registered to vote. | 19.4 % | 18.0 % |
| You were registered to vote, but you chose not to vote. | 7.3 | 7.8 |
| You were registered and tried to vote, but were turned away at the polling station. | 0.8 | 0.9 |
| You did vote. | 71.7 | 73.0 |
| Don’t know // Can’t remember | 0.7 | 0.3 |

Finally, instead of simply allowing respondents to answer ‘yes’ or ‘no’ to the question, the survey enumerator read a series of options that gave respondents a number of reasons for not voting. Although all the reasons for not voting were collapsed into one category for the purposes of the analysis, offering respondents a number reasons for not voting relieved some of the social sensitivity of reporting that they did not vote.

Part 3:Interviewer Effects

Another way to investigate social sensitivity bias in response options is to investigate the different ways in which respondents reported their beliefs to different types of survey enumerators. We might expect that different enumerators might elicit more truthful responses from respondents for myriad reasons. For example, some people are naturally more sympathetic than others. Someone who is more similar in demographic characteristics might elicit more truthful answers (e.g. a female respondent might be more truthful with a female enumerator than a male enumerator). Of course, we all introduce bias in countless ways that simply cannot be accounted for. While there is no survey on earth that can claim that no responses are different across enumerators (for this reason, all regression results control for survey enumerator), the hope is that no enumerator introduces systematic bias across a subset of questions. Table C3 below lists all of the survey enumerators and their relevant demographic characteristics.

**Table C3: Survey Enumerators and their Demographic Characteristics**

|  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- |
|  | Gender | Nationality | Primary Language | Number of Surveys | Regions of Work |
| Author | Female | USA | English | 576 | Centre, Est, Littoral, Ouest, Northwest, Southwest, Sud |
| Brenda | Female | Cameroon | English and French | 586 | Centre, Littoral, Northwest, Sud |
| Miguel | Male | Cameroon | French | 294 | Centre, Est, Littoral |
| Evelyn | Female | Cameroon | English | 840 | Est, Northwest, Ouest, Southwest |
| Kwame | Male | Ghana | English | 43 | Northwest, Southwest |
| Shella | Female | Cameroon | English and French | 60 | Sud |

The interviewers vary on the most important attributes, including gender, nationality, and primary language. While, perhaps, the most concerning issue is the author’s nationality, this difference actually offers an opportunity for better understanding the extent of social sensitivity bias in the independent variables. Of course before the start of the survey, it was explained to each respondent why the survey was being conducted, but a common fear for surveys conducted in Africa is that respondents may secretly believe that the government is conducting the survey. This may bias responses away from things the government would disapprove of, for example abstaining from elections, believing voting is not a duty, reporting that the economy is doing poorly, or supporting the opposition. However, we might expect that respondents would find it implausible that an American enumerator was implementing a survey for the government, allowing them to report more truthful responses.

The results in Table C4 do not lend a lot of evidence to this theory. For each model in Table C4, the dependent variable is one of the *independent* variables from the main analysis, and the regressors are each survey enumerator (Shella is the omitted category). In general, it is clear that no one enumerator introduced systematic bias across all (or most) of the questions. Respondents were less likely to report to the author that they believed in the logic of electoral patronage, which may be because they perceive that a Westerner may find such patronage especially distasteful. However, one should note that respondents were even less likely to report this belief to Miguel, a francophone Cameroonian. Respondents were also less likely to tell the author that they believed voting could improve democracy; a finding that is more difficult explain. However, again, respondents were even more likely to tell a francophone Cameroonian—in this case Brenda—that they did not believe that voting could improve democracy.

As a hard test of bias, Model 6 regresses enumerators on the question of whether or not the respondent feels close to an opposition party. We would think that of all the questions in the survey, the most likely to introduce bias based on enumerator would be reporting closeness to the opposition. Respondents should feel especially free to tell an American that they support the opposition. But again, people were not any more or less likely to report opposition sympathy to me than to the Cameroonian enumerators. The only enumerator with an effect is Kwame, but this is likely because all of his surveys were conducted in the anglophone regions, where opposition partisanship is highest. Overall, these results indicate that the enumerators do not appear to be introducing systematic bias, and that fear of reprisals from the government also do not appear to be introducing a unique form of bias.

**Table C4: Relationship between Relevant Independent Variables and Survey Enumerator**

|  |  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- | --- |
|  | Model 1:  Received a Gift or Favor | Model 2:  Believes in Patronage | Model 3:  Evaluation of Economy | Model 4: Civic  Duty | Model 5:  Improve Democracy | Model 6: Opposition Supporter |
| Author | 0.039  (0.040) | -0.709\*\*\*  (0.093) | -0.156  (0.114) | -0.144  (0.139) | -0.206\*\*  (0.082) | 0.030  (0.037) |
| Brenda | -0.017  (0.038) | -0.183  (0.111) | -0.816\*\*\*  (0.108) | -0.050  (0.145) | -0.464\*\*\*  (0.093) | -0.017  (0.035) |
| Miguel | -0.022  (0.044) | -0.792\*\*\*  (0.128) | -0.268\*\*  (0.116) | 0.142  (0.142) | -0.033  (0.070) | -0.029  (0.038) |
| Evelyn | 0.021  (0.044) | -0.171\*  (0.100) | -0.164  (0.107) | -0.029  (0.139) | 0.007  (0.065) | 0.026  (0.033) |
| Kwame | 0.028  (0.054) | 0.017  (0.124) | -0.011  (0.169) | -0.570\*\*\*  (0.157) | -0.181  (0.116) | 0.244\*\*\*  (0.085) |
| Constant | 0.100\*\*  (0.040) | 1.983\*\*\*  (0.052) | 1.611\*\*\*  (0.095) | 1.367\*\*\*  (0.132) | 2.780\*\*\*  (0.058) | 0.083\*\*\*  (0.028) |
| N | 2,381 | 2,222 | 2,337 | 2,384 | 2,337 | 2,399 |
| R-Squared | 0.006 | 0.046 | 0.063 | 0.011 | 0.047 | 0.012 |
| Coefficients are reported. Standard errors are given in parentheses. Standard errors clustered at the sampling unit. ‘Shella’ is the omitted category.  \*p<0.10; \*\*p<0.05; \*\*\*0.01 | | | | | |  |

**Works Cited**

Ahart, Allison M., and Paul R. Sackett. 2004. “A New Method of Examining Relationships Between Individual Difference Measures and Sensitive Behavior Criteria: Evaluating the Unmatched Count Technique.” *Organizational Research Methods* 7(1): 101–14.

Armstrong, J. Scott, William B. Denniston, and Matt M. Gordon. 1975. “The Use of the Decomposition Principle in Making Judgments.” *Organizational Behavior and Human Performance* 14(2): 257–63.

Blair, G., and K. Imai. 2012. “Statistical Analysis of List Experiments.” *Political Analysis* 20(1): 47–77.

Blaydes, Lisa. 2011. *Elections and Distributive Politics in Mubarak’s Egypt*. New York: Cambridge University Press.

Braithwaite, Jeremy, and Jeffrey A. Walsh. 2008. “Self-Reported Alcohol Consumption and Sexual Behavior in Males and Females: Using the Unmatched-Count Technique to Examine Reporting Practices of Socially Sensitive Subjects in a Sample of University Students.” *Journal of Alcohol & Drug Education* 52(2): 49+.

Corstange, Daniel. 2009. “Sensitive Questions, Truthful Answers? Modeling the List Experiment with LISTIT.” *Political Analysis* 17(1): 45–63.

Dalton, Dan R., James C. Wimbush, and Catherine M. Daily. 1994. “Using the Unmatched Count Technique (UCT) to Estimate Base R.” *Personnel Psychology* 47(4): 817.

Droitcour, Judith et al. 1991. “The Item Count Technique as a Method of Indirect Questioning: A Review of Its Development and a Case Study Application.” In *Measurement Errors in Surveys*, eds. Paul P. Biemer et al. John Wiley & Sons, Inc., 185–210. http://onlinelibrary.wiley.com/doi/10.1002/9781118150382.ch11/summary (November 11, 2015).

Glynn, A. N. 2013. “What Can We Learn with Statistical Truth Serum?: Design and Analysis of the List Experiment.” *Public Opinion Quarterly* 77(S1): 159–72.

Gonzalez-Ocantos, Ezequiel et al. 2012. “Vote Buying and Social Desirability Bias: Experimental Evidence from Nicaragua.” *American Journal of Political Science* 56(1): 202–17.

Holbrook, Allyson L., and Jon A. Krosnick. 2010. “Social Desirability Bias in Voter Turnout Reports.” *Public Opinion Quarterly* 74(1): 37–67.

Imai, Kosuke. 2011. “Multivariate Regression Analysis for the Item Count Technique.” *Journal of the American Statistical Association* 106(494): 407–16.

Kane, James G., Stephen C. Craig, and Kenneth D. Wald. 2004. “Religion and Presidential Politics in Florida: A List Experiment\*.” *Social Science Quarterly* 85(2): 281–93.

Kramon, Eric, and Keith Weghorst. 2012. “Measuring Sensitive Attitudes in Developing Countries: Lessons from Implementing the List Experiment.” *The Experimental Political Scientist* 3(2): 14–24.

Krosnick, Jon A. 1991. “Response Strategies for Coping with the Cognitive Demands of Attitude Measures in Surveys.” *Applied Cognitive Psychology* 5(3): 213–36.

Kuklinski, James H., Michael D. Cobb, and Martin Gilens. 1997. “Racial Attitudes and the‘ New South.’” *Journal of Politics*: 323–49.

LaBrie, Joseph W., and Mitchell Earleywine. 2000. “Sexual Risk Behaviors and Alcohol: Higher Base Rates Revealed Using the Unmatched-Count Technique.” *The Journal of Sex Research* 37(4): 321–26.

Redlawsk, David P., Caroline J. Tolbert, and William Franko. 2010. “Voters, Emotions, and Race in 2008: Obama as the First Black President.” *Political Research Quarterly* 63(4): 875–89.

Streb, Matthew J., Barbara Burrell, Brian Frederick, and Michael A. Genovese. 2008. “Social Desirability Effects and Support for a Female American President.” *The Public Opinion Quarterly* 72(1): 76–89.

Tourangeau, Roger. 2000. *The Psychology of Survey Response /*. Cambridge ; Cambridge University Press,.

Tourangeau, Roger, and Ting Yan. 2007. “Sensitive Questions in Surveys.” *Psychological Bulletin* 133(5): 859–83.

Tsuchiya, Takahiro, and Yoko Hirai. 2010. “Elaborate Item Count Questioning : Why Do People Underreport in Item Count Responses?” *Survey Research Methods* 4(3): 139–49.

Tsuchiya, Takahiro, Yoko Hirai, and Shigeru Ono. 2007. “A Study of the Properties of the Item Count Technique.” *The Public Opinion Quarterly* 71(2): 253–72.

Zigerell, L. J. 2011. “You Wouldn’t Like Me When I’m Angry: List Experiment Misreporting\*.” *Social Science Quarterly* 92(2): 552–62.

**Appendix D: Voted in the 2011 Presidential Election**

**Table D1: Dependent Variable is ‘Voted in 2011 Presidential Election’**

|  |  |  |  |
| --- | --- | --- | --- |
| *Reported Voting in the*  *2011 Election* | Model 1 | Model 2 | Model 3 |
| Received a Gift  Or Favor | -0.180  (0.220) | -- | -0.286  (0.230) |
| Expects  Patronage | 0.014  (0.063) | -- | -0.028  (0.067) |
| Per Capita  Budgetary Spending | 0.016  (0.021) | -- | 0.021  (0.022) |
| Evaluation of Economic Performance | -0.072  (0.068) | -- | -0.097  (0.067) |
| Civic Duty | -- | 0.410\*\*\*  (0.088) | 0.400\*\*\*  (0.091) |
| Improve  Democracy | -- | 0.378\*\*\*  (0.091) | 0.381\*\*\*  (0.103) |
| Controls | √ | √ | √ |
| Region Dummies | √ | √ | √ |
| Interviewer  Dummies | √ | √ | √ |
| Constant | -5.083\*\*\*  (2.060) | -6.150\*\*\*  (1.849) | -6.236\*\*\*  (2.142) |
| N | 1,708 | 1,805 | 1,673 |
| Pseudo R-Squared | 0.212 | 0.257 | 0.248 |
| Coefficients are reported. Standard errors are given in parentheses. Standard errors clustered at the sampling unit.  \*p<0.10; \*\*p<0.05; \*\*\*0.01 | | | |

**Appendix E: Full regression results**

**Table E1: Full Regression Results with All Control Variables Reported**

|  |  |  |  |
| --- | --- | --- | --- |
| *Reported Voting in the*  *2013 Elections* | Model 1 | Model 2 | Model 3 |
| Received a Gift  Or Favor | -0.147  (0.364) | -- | -0.164  (0.285) |
| Expects  Patronage | 0.056  (0.073) | -- | 0.000  (0.083) |
| Per Capita  Budgetary Spending | 0.018  (0.024) | -- | 0.018  (0.024) |
| Evaluation of Economic Performance | 0.021  (0.086) | -- | 0.000  (0.088) |
| Civic Duty | -- | 0.218\*\*\*  (0.056) | 0.225\*\*\*  (0.062) |
| Improve  Democracy | -- | 0.412\*\*\*  (0.080) | 0.420\*\*\*  (0.094) |
| Was Threatened | 0.642 | 0.700 | 0.737 |
| Ruling Party Partisan | 1.579\*\*\* | 1.355\*\*\* | 1.469\*\*\* |
| Opposition Partisan | 1.392\*\*\* | 1.333\*\*\* | 1.420\*\*\* |
| Feels Free to Vote | 0.293\*\*\* | 0.232\*\*\* | 0.200\*\* |
| News Consumption | 0.026 | 0.019 | 0.016 |
| Vote Share for Biya | 0.026 | 0.018 | 0.030 |
| Wealth Measure | -0.005 | -0.026 | -0.005 |
| Rural | 0.236 | 0.217 | -0.035 |
| Female | 0.024 | -0.046 | -0.082 |
| Age | 0.061\*\*\* | 0.068\*\*\* | 0.063\*\*\* |
| Education | 0.041 | 0.028 | 0.021 |
| Interviewer: Author | 0.111 | 0.146 | 0.240 |
| Interviewer: Brenda | 0.500\*\* | 0.676\*\* | 0.781\*\* |
| Interviewer: Miguel | 0.027 | -0.247 | -0.004 |
| Interviewer: Evelyn | 1.063\*\* | 0.767 | 0.005\* |
| Interviewer: Kwame | 0.102 | -0.019 | 0.333 |
| Ethnicity: Douala | -0.205 | -0.433 | -0.331 |
| Ethnicity: Makas | -0.057 | 0.333 | 0.183 |
| Ethnicity: Bamiléké | 0.301 | 0.259 | 0.357 |
| Ethnicity: Bamoun | -0.060 | 0.218 | 0.259 |
| Ethnicity: Bassa | 0.714 | 0.734\* | 0.809 |
| Ethnicity: Bayangi | -0.664 | 0.002 | -0.359 |
| Ethnicity: Kom | -0.328 | -0.194 | -0.152 |
| Ethnicity: Mamfe | -0.806 | -0.167 | -0.586 |
| Ethnicity: Moghamo | -0.155 | -0.344 | -0.030 |
| Ethnicity: Other | -0.177 | 0.028 | -0.054 |
| Region: Est | 0.296 | -0.014 | 0.551 |
| Region: Littoral | 0.733 | 0.181 | 0.869 |
| Region: Northwest | 2.611\* | 1.752 | 3.071\*\* |
| Region: Ouest | 2.100\* | 1.386\* | 2.319\*\* |
| Region: Southwest | 1.847\* | 1.115 | 2.090\* |
|  |  |  |  |
| Constant | -6.552\*\*\*  (1.978) | -6.359\*\*\*  (1.637) | -7.913\*\*\*  (2.084) |
| N | 1,712 | 1,810 | 1,677 |
| Pseudo R-Squared | 0.191 | 0.215 | 0.217 |
| Coefficients are reported. Standard errors are given in parentheses. Standard errors clustered at the sampling unit. Reference category for interviewer is ‘Shella.’ Reference category for ethnicity is ‘Beti-Fang.’ Reference category for region is ‘Sud.’  \*p<0.10; \*\*p<0.05; \*\*\*0.01 | | | |

**Appendix F: Socioeconomic Status, Electoral Patronage and Voting**

**Table F1: Socioeconomic Status Interacted with Patronage Voting Measures**

|  |  |  |  |
| --- | --- | --- | --- |
| *Reported Voting in the*  *2011 Election* | Model 1 | Model 2 | Model 3 |
| Received a Gift  or Favor | -0.460  (0.699) | -- | -1.104  (0.0.674) |
| Received a Gift  Or Favor x Wealth | 0.076  (0.132) |  | 0.234\*  (0.138) |
| Expects  Patronage | 0.062  (0.172) | -- | 0.071  (0.186) |
| Expects  Patronage x Wealth | -0.001  (0.031) |  | -0.016  (0.032) |
| Per Capita  Budgetary Spending | -0.000  (0.003) | -- | 0.000  (0.027) |
| Budgetary Spending x Wealth | 0.004  (0.003) |  | 0.004  (0.003) |
| Evaluation of Economic Performance | 0.008  (0.235) | -- | -0.045  (0.204) |
| Evaluation of Economic Performance x Wealth | 0.002  (0.040) |  | 0.010  (0.034) |
| Civic Duty | -- | 0.410\*\*\*  (0.088) | 0.230\*\*\*  (0.060) |
| Improve  Democracy | -- | 0.378\*\*\*  (0.091) | 0.429\*\*\*  (0.092) |
| Controls | √ | √ | √ |
| Region Dummies | √ | √ | √ |
| Interviewer  Dummies | √ | √ | √ |
| Constant | -6.024\*\*\*  (1.970) | -6.150\*\*\*  (1.849) | -7.210\*\*\*  (2.065) |
| N | 1,712 | 1,805 | 1,677 |
| Pseudo R-Squared | 0.192 | 0.257 | 0.220 |
| Coefficients are reported. Standard errors are given in parentheses. Standard errors clustered at the sampling unit.  \*p<0.10; \*\*p<0.05; \*\*\*0.01 | | | |

**Appendix G: Partisanship and Voting**

**Table G1: Partisanship Interacted with the Six Primary Independent Variables**

|  |  |
| --- | --- |
| *Reported Voting in the*  *2011 Election* | Model 1 |
| Ruling Party Partisan x Gift | -1.266\* |
| Opposition Party Partisan x Gift | 0.287 |
| Gift | 0.122 |
| Ruling Party Partisan x Patronage | 0.191 |
| Opposition Party Partisan x Patronage | -0.194 |
| Patronage | -0.017 |
| Ruling Party Partisan x Government Spending | 0.041\*\* |
| Opposition Party Partisan x Government Spending | -0.002 |
| Government Spending | 0.015 |
| Ruling Party Partisan x Evaluation of the Economy | -0.029 |
| Opposition Party Partisan x Evaluation of the Economy | -0.105 |
| Evaluation of the Economy | -0.005 |
| Ruling Party Partisan x Civic Duty | 0.505\*\* |
| Opposition Party Partisan x Civic Duty | -0.251 |
| Civic Duty | 0.175\*\*\* |
| Ruling Party Partisan x Improve Democracy | -0.149 |
| Opposition Party Partisan x Improve Democracy | 0.048 |
| Improve Democracy | 0.447\*\*\* |
| Controls | √ |
| Region Dummies | √ |
| Interviewer  Dummies | √ |
| Constant | -8.326\*\*\*  (1.975) |
| N | 1,677 |
| Pseudo R-Squared | 0.229 |
| Coefficients are reported. Standard errors clustered at the sampling unit.  \*p<0.10; \*\*p<0.05; \*\*\*0.01 | |