

# From Medicine to Mobilization: Evidence from a Priming Experiment in Egypt Appendix

July 14, 2016

In this appendix I discuss the sampling methodology and quality control efforts. I also present a comparison of the sample characteristics with the latest iteration of Egypt's census, as well as check on randomization focusing on how key variables are distributed across the treatment and control variables. While the body of the article presents difference-of-means checks across the treatment and control, below I present different graphics of those same comparisons, this time focusing on the substantive differences in the two groups. The penultimate section contains a table used in calculating the post-hoc correction of  $\alpha$ , while this appendix closes with a sensitivity analysis designed to contextualize the mediation analysis in the body of the paper.

The results reported in this article are part of a broader survey of Egyptians' experiences with non-state provision of social services. That full survey included three roughly equal in size groups. The control (Ministry of Health) and treatment (Muslim Brotherhood) groups are discussed here. The third group received a similar prime that discussed a third religious organization. I report those findings in a forthcoming paper.

This survey was initially contracted with a company specializing in face-to-face surveys, and was piloted twice in that form. Concerns about improper sampling procedures, as well as an inability to properly execute the randomization protocols and skip patterns, necessitated a change in survey firm. I eventually selected The Egyptian Center for Public Opinion Research (Baseera) to execute a telephone survey. After two additional pilot attempts, the survey was carried out

over seven days, from May 14 to May 20, 2014. The full sample of 3707 respondents was 107 respondents larger than the targeted sample size of 3600 (three 1200-respondent groups). The control group (Ministry of Health) included 1238 respondents, while the treatment group (the Muslim Brotherhood) included 1245 respondents. The survey response rate was 50.8%.

The survey queried Egyptians aged 18 to 91 over both landline (48.21% of the sample) and mobile phones (51.79%). All respondents were selected via simple random sampling (mobile customers were selected through random digit dialing, while landline customers were selected from a database of all Egyptian landlines). According to Baseera, combining landlines with mobile lines captures over 90% of the Egyptian population, although up-to-date and third-party statistics on coverage are difficult to come by.<sup>1</sup> Face-to-face surveys typically capture a similar proportion of the Egyptian population, but do so by skipping a number of lightly-populated governorates for logistical or security reasons (governorates are roughly akin to American states). In contrast, this telephone survey included respondents from each of Egypt's 27 governorates.

Telephone surveys are well accepted in Egyptian survey research. Especially given my earlier (unsatisfactory) experience with face-to-face surveys, moving to telephone surveys offers a number of advantages germane to this specific project. First, the CATI (computer aided telephone interviewing) software automates randomization as well as adherence to skip patterns. Second, the vagaries of Egyptian law mean that telephone surveys are not subject to Ministry of Interior veto power. Face-to-face surveys, on the other hand, must be pre-cleared by the Ministry. Especially given the sensitivity of this project this was a significant concern. As a final check on the methodology, I contracted a third-party Egyptian researcher and instructed him on proper selection processes, interview techniques, attention to detail, and faithfulness to the questionnaire form. He followed the survey as it was piloted and separately reported back to me. The full survey was executed only after I was satisfied with the survey company's ability to correctly follow the protocol and implement the survey instrument.

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<sup>1</sup>The most recent Egyptian census, from 2006, found that 53.17% of the population owns at least one type of phone.

For purposes of external validity, the below table compares the survey sample against the most recent iteration of the Egyptian census, from 2006. Note that “Urban Governorates” encompasses five governorates: Cairo, Giza, Alexandria, Port Said, and Suez.

Table 1: Survey and Population Characteristics

	Survey Sample (2014)	2006 Census (Age 18 and up)
Mean Age	39.88 years	36.76 years
Percent Female	47.52%	49.14%
Percent Christian	5.51%	5.65%
Percent Unemployed	9.4%	4.48%
Modal Education Level Completed	Secondary/Vocational	None
Percent Living in Urban Governorates	32.34%	28.57%

The categories are either broadly similar (age, sex, religion) or have differences that are likely attributable to the gap between the 2006 census and the 2014 survey (it seems likely that there is more unemployment and more rural-urban migration now). Though the specific modal education level is different, the general distribution of education levels is broadly similar (secondary/vocational is the second most prevalent category in the census, for example). If the survey sample were more educated than the general population, it may potentially *overstate* the on-the-ground effects of the Brotherhood’s social service provision (below I show this, in the test of educational attainment as a proxy for socioeconomic status).

For purposes of internal validity, the Figure 1 shows the differences in means of key population variables, as well as enumerator gender (Benstead 2014), across the treatment and control.<sup>2</sup> Table 2 (following) provides a substantive measure of the same comparisons, along with  $p$  values. Note that to generate the most conservative test I do not apply any statistical correction to the measures.

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<sup>2</sup>Because of differences in scale, age and education are not included in Figure 1. They are included in Table 2.

Figure 1: Difference of Means Tests, Treatment Minus Control

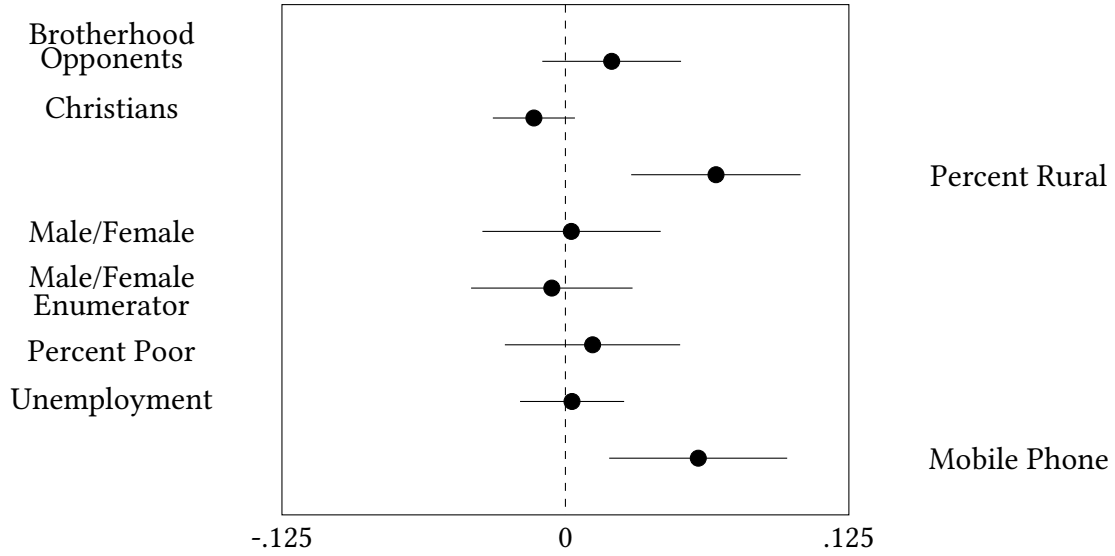


Table 2: Distribution of Key Variables, Treatment vs. Control

Subgroup	$\mu_{Treatment}$	$\mu_{Control}$
Mean Age	39.912 (.401)	39.851 (.409)
Percent Poor	.413 (.014)	.401 (.013)
Percent Rural	.379 (.014)	.312*** (.013)
Percent Female	.477 (.014)	.474 (.014)
Percent Christians	.048 (.006)	.062 (.007)
Percent Unemployed	.095 (.008)	.092 (.092)
Percent Anti-Brotherhood	.195 (.011)	.175 (.011)
Percent Contacted via Mobile	.561 (.014)	.502** (.014)
Mean Years of Formal Schooling	12.737 (.162)	12.799 (.158)
Percent Receiving a Male Enumerator	.282 (.013)	.288 (.013)

Although in substantive terms the difference is small, the treatment group (those receiving

the battery of Muslim Brotherhood questions) were significantly more rural than the control group (those receiving the battery of Ministry of Health questions), and the treatment group were substantially more likely to have been contacted via mobile phone.

As a robustness check on whether or not the unbalance is driving the results, Table 3 models individual responses to the two variables of interest (how likely are you to vote for the Brotherhood (“Vote Choice”) and how you perceive the Brotherhood candidates’ traits (“Likability”). These outcomes are analyzed as a function of the various socioeconomic predictors in Table 2 as well as a dummy variable representing whether or not an individual is assigned to the treatment or the control. If the treatment was not driving the observed differences in vote choice and likability, one indication would be a notable shift in the coefficient on the treatment dummy when all the socioeconomic variables are added to the model. In Table 3 models one and two predict respondent electoral choice and respondent perception of candidate traits solely as a function of assignment to the treatment or the control, respectively. Models three and four also include the relevant socioeconomic variables to examine how this effects the coefficient on the treatment/control dummy.

Table 3: Predictors of Vote Choice and Likability

	(1) Vote Choice	(2) Likability	(3) Vote Choice	(4) Likability
Treatment Dummy	0.0926* (2.56)	0.136*** (3.37)	0.0969** (2.69)	0.136*** (3.44)
Christian Dummy			-0.132 (-1.67)	-0.279** (-3.25)
Urban Dummy			-0.125** (-3.16)	-0.159*** (-3.60)
Female Dummy			-0.0438 (-1.13)	-0.104* (-2.44)
Male Enumerator			-0.00923 (-0.23)	0.0517 (1.17)
SES			-0.0308 (-1.22)	-0.0119 (-0.43)
Unemployment			-0.0670 (-1.05)	-0.155* (-2.22)
Years of School			0.00755* (2.10)	0.0156*** (3.83)
Anti-MB Dummy			-0.359*** (-7.88)	-0.387*** (-7.90)
Mobile Phone Dummy			-0.0104 (-0.26)	-0.0209 (-0.48)
Age			-0.00277* (-2.02)	-0.00531*** (-3.54)
Constant	0.453*** (17.77)	0.682*** (24.22)	0.715*** (7.42)	0.971*** (9.22)
<i>N</i>	2224	1921	2195	1892

*t* statistics in parentheses

\*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$

The lack of a substantive change in the coefficient estimates for the treatment dummy, even when including these other variables, strongly suggests that the differences in underlying sample composition do not substantively influence the analysis- exposure to the treatment remains a solid predictor of both individual vote choice and perception of Brotherhood candidates.

## Experimental Manipulation

The article produced difference-of-means tests for the full sample, as well as the identified subgroups. Figures 2, 3, and 4 offer a different presentation of these same results, in this case focusing on *the substantive differences between the means of the two groups*. In the below plots, dots represent group means while the whiskers represent one standard error. Asterisks denotes a relationship that remains significant following the post-hoc correction of  $\alpha$ . Note that the scales here are truncated.

Figure 2: Propensity to Vote for Brotherhood Candidates

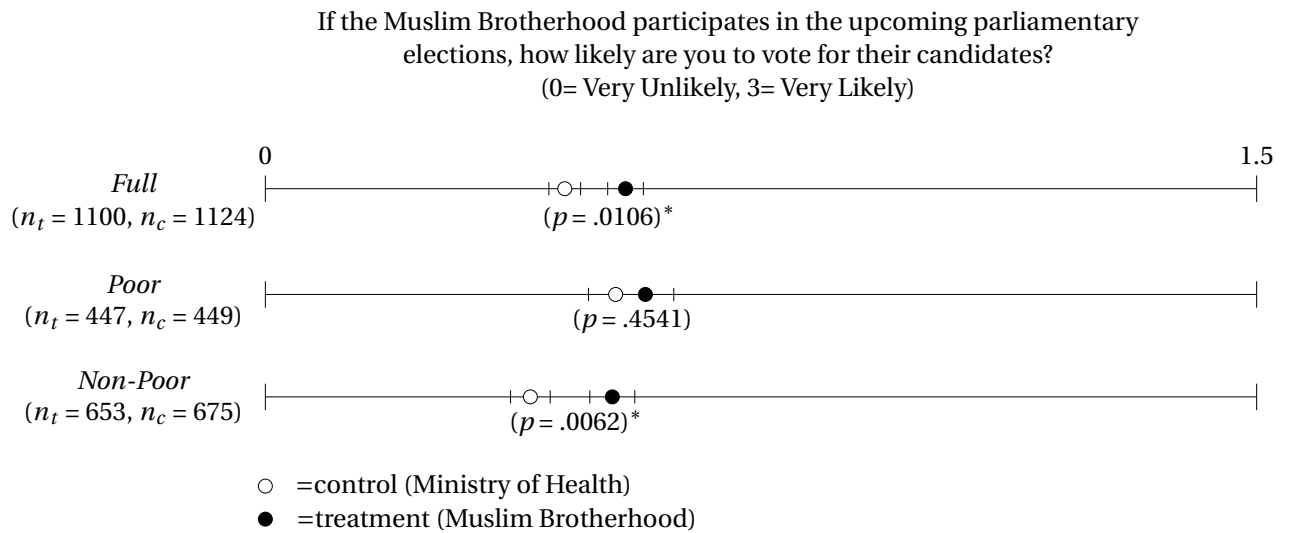


Figure 3: Assessment of Brotherhood Candidates' Traits

I am going to read you a list of terms that some people have used to describe candidates and deputies from the Muslim Brotherhood. Please tell me how much you agree or disagree with each word. (Honest/Capable/Approachable)  
(0= Strongly Disagree, 3= Strongly Agree)

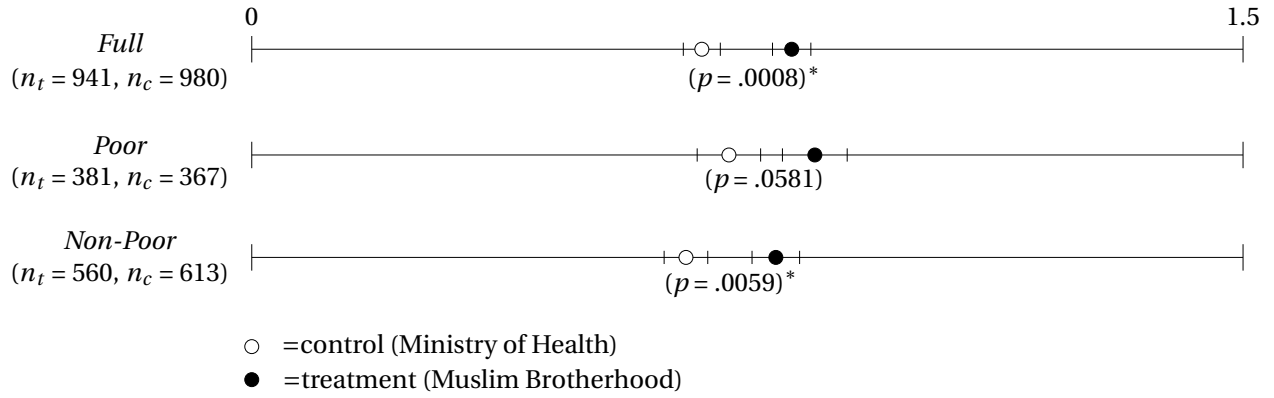
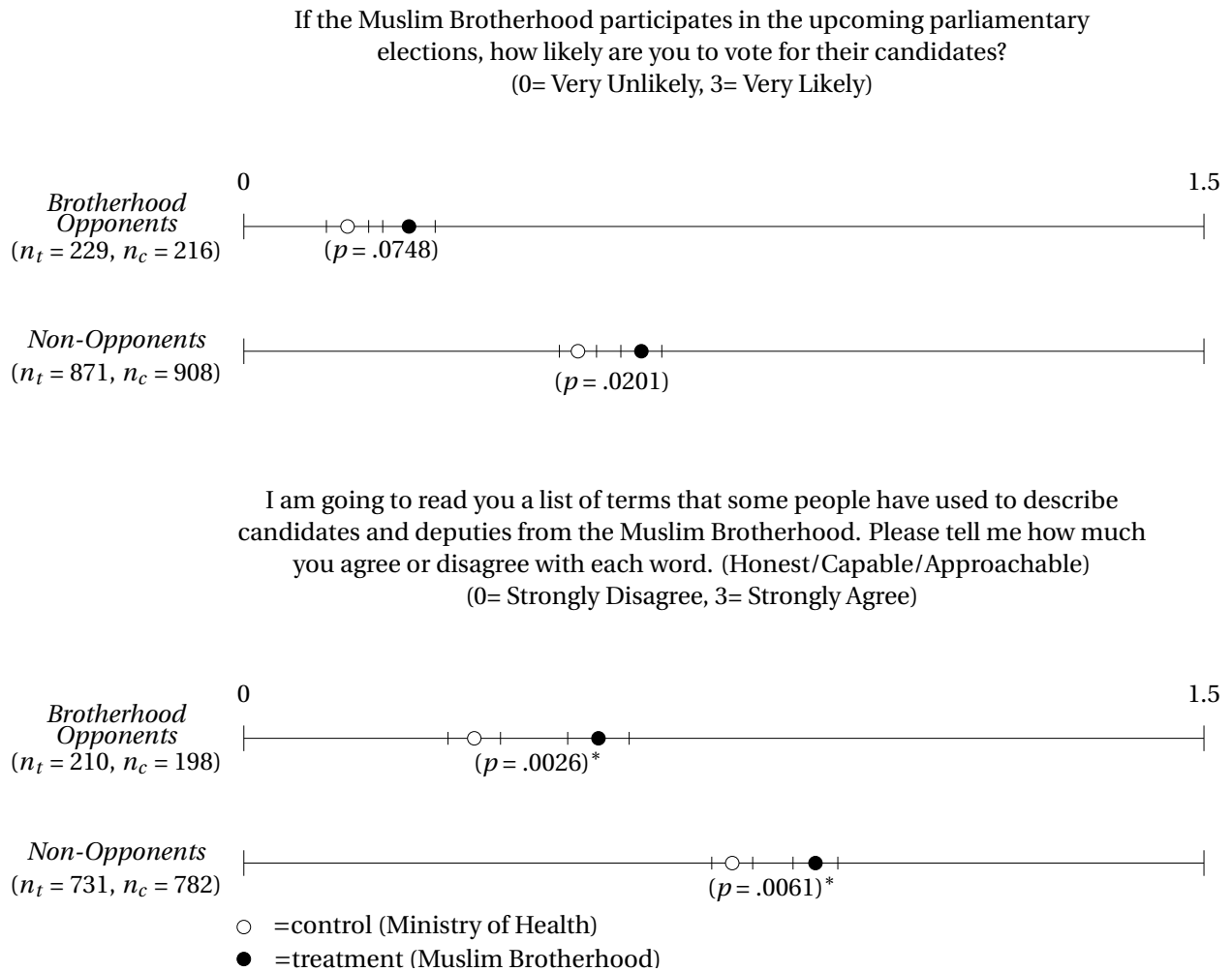




Figure 4: Experimental Manipulation, Brotherhood *Opponents*



One should interpret carefully the rather dismal opinion both the treatment and control groups express about the Muslim Brotherhood and its candidates. This survey was carried out approximately one year after Egypt’s military overthrew elected Muslim Brotherhood president Mohammed Morsi and began to violently repress his supporters, including the killing of over 1,000 protestors at Cairo’s *Raba’a al-Adwiyya* Square.<sup>3</sup> As an Amnesty International official noted in June 2014, “Egyptian authorities have spent the last year engaging in repression on a scale

<sup>3</sup>“Egypt: Security Forces Used Excessive Lethal Force,” *Human Rights Watch*, August 19, 2013. Available online at: <http://www.hrw.org/news/2014/06/09/egypt-new-leader-faces-rights-crisis>.

unprecedented in Egypt's modern history."<sup>4</sup> This environment establishes clear incentives for respondents to strategically *deflate* their opinions of the Muslim Brotherhood in order to mitigate the risk of regime reprisal. This non-trivial possibility of preference falsification means that this study can only weakly make claims about the underlying structure of Egyptian public opinion regarding the Brotherhood. The focus should instead be on the ability of the experimental manipulation to shift these opinions.

## Correction Table

To mitigate the problems of multiple comparisons, the statistical tests in the body of the article report the corrected values for  $\alpha$ , following a Bonferroni (Holm) correction, alongside the uncorrected values (Jaccard and Wan 1996, Holm 1979). Table 4 illustrates this process and notes the results of comparisons following the correction.

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<sup>4</sup>"Egypt: New Leader Faces Rights Crisis," *Human Rights Watch*, June 10, 2014. Available online at: <http://www.hrw.org/news/2014/06/09/egypt-new-leader-faces-rights-crisis>.

Table 4: Correction Tables

Question	Subgroup	$\alpha$ (CI)	$p$ Value	Corrected $\alpha$ (CI)	New Result
Vote Propensity	Non-Poor	.05 (95)	.0062	.01 (99)	Remains Significant
Vote Propensity	Full	.05 (95)	.0106	.0125 (98.75)	Remains Significant
Vote Propensity	Non-Opponents	.05 (95)	.0201	.017 (98.3)	Loses Significance
Vote Propensity	Opponents	.05 (95)	.0748	.025 (97.5)	Not Significant
Vote Propensity	Poor	.05 (95)	.4541	.05 (95)	Not Significant
Likability Index	Full	.05 (95)	.0008	.01 (99)	Remains Significant
Likability Index	Opponents	.05 (95)	.0026	.0125 (98.75)	Remains Significant
Likability Index	Non-Poor	.05 (95)	.0051	.017 (98.3)	Remains Significant
Likability Index	Non-Opponents	.05 (95)	.0061	.025 (97.5)	Remains Significant
Likability Index	Poor	.05 (95)	.0581	.05 (95)	Loses Significance

## Alternative Measure of Socioeconomic Class

The analysis of socioeconomic class-based differential effects in the article was based on an asset index. As a robustness check, in Figures 5 and 6 I reproduce the analysis yet use educational attainment to proxy for socioeconomic class, breaking the sample between those who finished high school and those who did not.

## Sequential Ignorability Analysis

The causal mediation analysis in the text of the article rests on an assumption of sequential ignorability that cannot be directly tested (Imai et al. 2011). However the *mediation* package for Stata also allows iterative violations of the assumption by allowing the error terms of the two OLS regression models (mediator and outcome) to correlate more and more strongly. As this sensitivity parameter ( $\rho$ ) approaches complete correlation (1 or -1), the value of  $\rho$  when the Average Causal Mediation Effect reaches zero provides a metric of the validity of the causal mediation analysis.

Figure 5: Difference in Means, Propensity to Vote for the Brotherhood  
(Treatment minus Control)

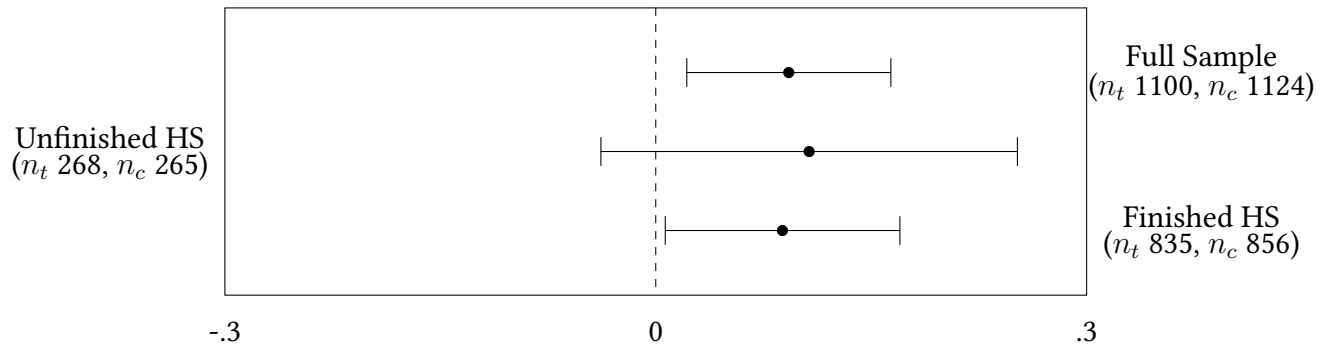
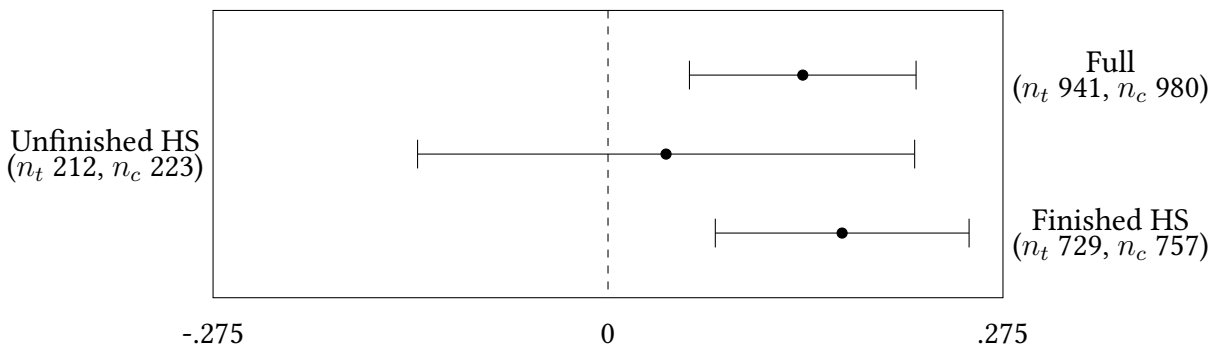
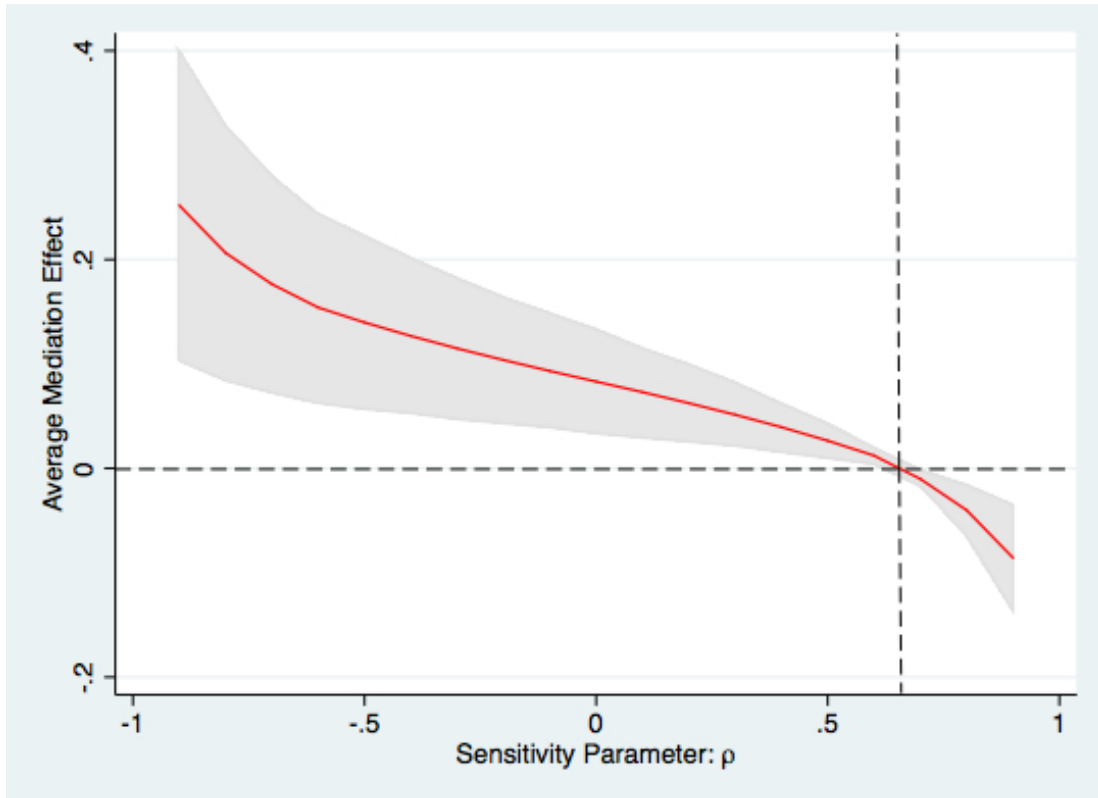


Figure 6: Difference in Means, Likability Index  
(Treatment minus Control)



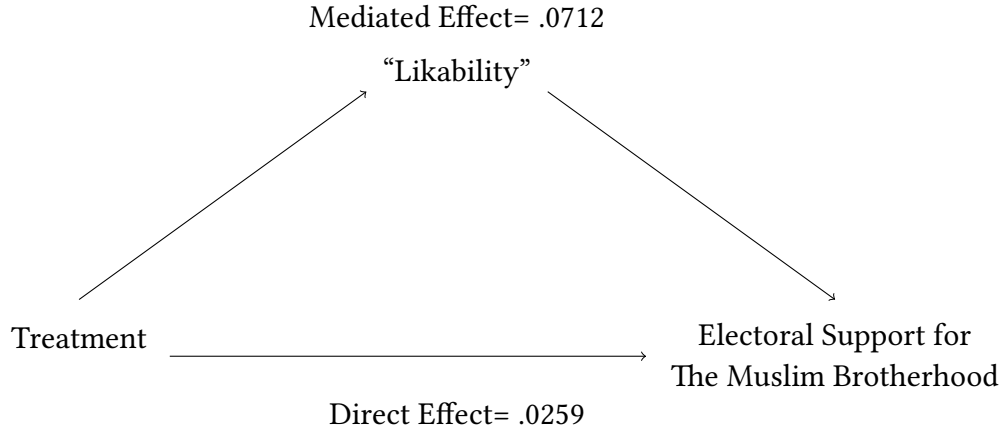
The relationship is presented graphically below.

Figure 7: Sensitivity Analysis



In this case, the ACME reached zero at  $\rho = .6341$ . As Imai et. al. point out, there exists no baseline standard for judging the acceptable value of  $\rho$ . Instead, they suggest comparing the value of  $\rho$  in the present analysis with the value of  $\rho$  as observed in other examples of mediation analysis in the literature (2011, 776). For instance, in a separate article, Imai et. al. (2010) note a value of  $\rho = .48$  for a study of media framing (Nelson and Kinder 1996). Again, while a substantive explanation of the value of  $\rho$  as observed in the above causal mediation analysis, it is possible to say that it is notably stronger than in this example, and robust to fairly significant violations of the sequential ignorability assumption ( $\rho = 0$ ).

Figure 8: Causal Pathway, Brotherhood Opponents



### Mediation Analysis, the Brotherhood’s Opponents

The following Table and Figure re-run the causal mediation analysis limiting the sample *only to those who expressed ex-ante hostility towards the Muslim Brotherhood*.

Table 5: Mediation Results, Effect of “Likability” on Propensity to Vote for Brotherhood Candidates  
Brotherhood Opponents

	Mean	95% Confidence Intervals	
Average Causal Mediation Effect (ACME)	.07118	.01482	.12771
Direct Effect	.02587	-.08208	.12049
Total Effect	.09705	-.02109	.20506

This suggests that, for the subset of the Brotherhood’s opponents, .6914573 of the total effect of the treatment on likeliness to vote for the Brotherhood’s candidates is produced via the proposed reputational pathway.

The value for  $\rho$  at which the ACME = 0 is, for this subset of Brotherhood opponents, is .4512.

## References

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