

Calling Mogadishu: Appendix

1 Background

The situation in Somalia is often invoked as an object lesson for the real-world possibility of Hobbesian anarchy. Hundreds of thousands have died since the 1980s. Multiple competing state consolidation projects have unfolded centered on the capital city of Mogadishu. Shifting Somali political coalitions were intermittently assisted by military interventions from a broad coalition of external powers, none of whom wanted to see the Union of Islamic Courts (UIC) ascend to political prominence.¹

A major military turning point was June 2006, when the UIC controlled all of Mogadishu – the first unified actor to do so in 15 years. In response, rapid invasion by the Ethiopian military in December 2006 triggered an implosion of the UIC amidst heavy fighting. Scattered by Ethiopia’s military advance across southern Somalia (with assistance from U.S. airstrikes), UIC forces retreated from the city. With Ethiopian forces providing security, the internationally recognized Transitional Federal Government (TFG) relocated to Mogadishu in January 2007 after spending many years in Baidoa. Security in Mogadishu then deteriorated dramatically. Ethiopian forces faced bombings and ambushes, and eventually withdrew in 2009. By this time, an extreme faction of the UIC, calling itself Al Shabaab,

¹ Between 1995 and 2004, almost all of the cross-clan peace initiatives in the two Shabelle regions, which surround Mogadishu, were led by Islamic figures attempting to use religion to provide services across clan lines. The authors are grateful to Will Reno for drawing our attention to this. For regional background see Woldemariam (2018), especially 250-280.

or “the youth,” had emerged as a powerful military force, controlling more than half of Mogadishu by 2011. The military tide turned on 8 August 2011 when Al Shabaab unexpectedly withdrew from Mogadishu overnight. The TFG suddenly found itself with the opportunity to control a city that had seen only six months of stable control in the past two decades. For the first time in recent history, the United Nations Political Office for Somalia (UNPOS) had a local partner with an opportunity to assert a violence monopoly. The city remained a patchwork of clan-enclaves, however, with frequent clashes among clan militias and a zone of open warfare on the city’s outskirts.

2 Initial Sampling

In 2012 – shortly after the *Al Shabaab* insurgent group stopped holding territory, but before the state apparatus had firmly established control over the city – researchers from the University of California at San Diego conducted the first representative survey of Mogadishu, the capital of Somalia, in 25 years. Certain portions of the city were depopulated (free-fire zones with no static population) at the time that the first survey was conducted. Procedures are described in Driscoll & Lidow (2014). The goal was not a census, but to generate a defensible estimate of the city’s population on the assumption that the government lacked capacity to generate a credible number on its own. The baseline survey wave identified respondents from 781 households, of which 649 agreed to participate in the study – a response rate of 83% conditional on having been contacted and invited to take part.

This exercise would have been impossible if not for explicit permission from, and coordinated cooperation with, the recognized Somali government. This obvious cooperation raised concerns by many study subjects that the exercise was an inaugural census that could function as a *de-facto* land registry and reify contested property claims. The predictable beneficiaries would be members of a certain clan group: The Hawiye clan family.

Who are these Hawiye? Somalis are the dominant ethnic group in Somalia. Somali

Figure 1 Insert Appendix Figure 1 (MAP OF MOGADISHU) About Here

society delineates and understands itself through a segmentary lineage system, known more colloquially as “clan structure.” Clan lineages, not ethnic cleavages, have provided focal points for political conflict.² For this reason, a choice was made by the research team very early in the research process to *not* ask respondents to reveal their clan.

Though the face-to-face wave of the survey did not ask individuals their clan, enumerators did ask the question “Would you be willing to consider telling us your clan in a future survey?”³ Results varied starkly (See Figure 1). In the relatively-secure commercial center of Hamar Weyne, more than 70% of respondents said they would be willing to reveal their clan – more than double the proportion of the next highest district. In most districts once held by insurgents, *none* of those surveyed said they would be willing.

The patterns of non-participation, non-response and refuse-to-answer on the 2012 face-to-face survey track relatively well onto the zone of territorial control by *Al Shabaab*. This is not surprising. Many of these civilians were exposed to government gunfire and shelling during the year prior to the study. Enclaves in this territory would have had no recourse if they were accused of harboring Islamist sympathies as a pretext for driving families from their homes. Our prior was that neither citizens living in territory previously controlled by

² Some blame President Mohamed Siyaad Barre for fostering clan divisions as a divide-and-rule strategy from 1969-1988 to manage the political consequences of economic contraction and social dislocation resulting from war and famine; others credit his nationalist project for attempting to banish clan names as elements of a feudal past. In any case, the final collapse of Barre’s regime in 1991 triggered a scramble for control of the symbols of state power in the capital city of Mogadishu. Clans have mattered a great deal ever since.

³ The exact question asked on this survey was: “We are not going to ask you about your clan, but if another survey asked you, would you be comfortable telling them your clan?”

the Al Shabaab insurgency, nor those living in refugee and displacement camps, would be likely to want to participate in a survey that solicited information about their clan identity.

3 Telephone Contact & Panel Attrition

When asked at the end of the face-to-face survey whether the respondent would be willing to agree to a follow-up survey, 252 (39% of the sample) responded positively and provided their phone numbers.⁴ These numbers were called via Skype by fluent Somali-language enumerators from the San Diego community in the spring of 2013 (Wave 2), in the spring of 2014 (Wave 3), and the finally in the fall of 2014 (Wave 4). Calls were made in collaboration with the Somali Youth League of San Diego. Enumerators were recruited and trained from this group. Each number was tried three times, except for a small portion of numbers in Wave Two, which were attempted only twice.

The goal was to maintain a “virtual panel” to track welfare changes in Mogadishu over two-and-a-half-years of violent state consolidation. The effort was a qualified success. Non-responses on the solicitation of telephone numbers was non-random – missing at random (MAR) but not missing completely at random (MCAR) – so might, in principle, have been “recoverable” via weights. From the beginning, it was clear that the individuals who provided phone numbers were systematically different than the rest of the sample: younger, higher welfare, more likely to be displaced, more likely to report living in an area with clean streets and no fighting, more likely to indicate a willingness to provide information on their clan in a future survey, less likely to be from a homogenous enclave, and less likely to be living in an area controlled by a militia captain who was resisting state consolidation (Table I).

Logistical and technological complications introduced biases that had the effect of

⁴ The exact text of the question was: “Are you willing to participate in a follow-up study that would send you questions via SMS? You would receive compensation such as phone credit.”

Table I Probability in Wave 1 of Providing Phone Number

	(1)	(2)	(3)
Provided Cell Number			
Displaced	0.466** (0.19)	0.505*** (0.18)	0.401** (0.18)
Electricity	-0.217 (0.23)		
Street Cleaned	0.661*** (0.19)		
No Gunfire	-0.351* (0.19)		
No Fight	0.796*** (0.24)		
Pay for water	-0.333 (0.24)	-0.534** (0.22)	-0.616*** (0.21)
Will Tell Clan	0.612*** (0.19)	0.598*** (0.19)	0.595*** (0.19)
Former Al Shabaab area	-0.122 (0.19)	-0.273 (0.18)	-0.334* (0.18)
Militia district	-0.646*** (0.19)	-0.530*** (0.18)	-0.436** (0.17)
Homogeneous district	-0.620*** (0.19)	-0.551*** (0.19)	-0.413** (0.17)
Male	0.005 (0.18)	-0.053 (0.18)	-0.068 (0.18)
Age	-0.024*** (0.01)	-0.024*** (0.01)	-0.024*** (0.01)
Vulnerability Wave 1		-0.227** (0.09)	
Ln(Port Proximity)			0.302* (0.16)
Constant	0.360 (0.43)	1.623*** (0.43)	-1.335 (1.33)
Observations	626	626	647
Pseudo R2	0.095	0.071	0.068

excluding some of the more vulnerable citizens of Mogadishu from our sample. When our first round of Skype calls was placed in Spring of 2013, less than half of the numbers were answered by an individual willing to complete the survey (121/252). The best predictor of not making contact – either because the telephone number did not work or no one answered after three tries – was the respondent being displaced at the time the phone number was provided (Table II). Summary statistics from the first (face-to-face, Spring 2012) and third wave (via Skype, Spring 2014) of the study can be found in Table III.

Strong anonymity protections also had the effect of limiting our ability to guarantee panel validity. There was a clear trade-off between two goods: assuring respondent anonymity on the one hand, and the researchers' desire to know with certainty that they are keeping a panel of the same respondents over the telephone on the other. Ultimately, based on consultations with Somalis and area experts, we privileged the first set of concerns in the initial study design on the assumption that credible guarantees of respondents' anonymity were a prerequisite to enrolling an unbiased sample of study participants in the first place.

With hindsight, we wondered if we should have provided a script that would have allowed respondents to choose an anonymous identifier – like a codeword or a secret number – for validation. The trade-off of using these cloak-and-dagger techniques in a setting like Mogadishu is that many of our Somali enumerators would have come to suspect the motives of the study. It would also not have addressed the ubiquitous fear by Somalis that our phone calls were being monitored by third parties. In any case, because of our anonymity protections, we were unable to know with total confidence how many of the respondents in Waves 2-4 were actually the same individuals that were originally surveyed.

In the absence of unique identifiers for panel respondents, we used two criteria to estimate rates of panel attrition: first, whether the respondent answered that s/he was contacted by us in the previous 12 months, and secondly, whether the gender of the voice on the phone matched the gender of the original respondent. We find that a significant portion of

Table II Probability of Callback Success by Wave 1 Characteristics

	(Wave 2)	(Wave 3)	(Wave 4)
Displaced	-0.458* (0.28)	0.211 (0.30)	0.070 (0.31)
Electricity	0.093 (0.36)	0.469 (0.39)	0.441 (0.40)
Street Cleaned	-0.073 (0.29)	-0.794** (0.33)	0.284 (0.31)
No Gunfire	-0.582* (0.30)	-0.251 (0.33)	-0.798** (0.34)
No Fight	0.046 (0.38)	0.742* (0.41)	0.756* (0.41)
Pay for water	-0.383 (0.35)	0.014 (0.37)	-0.085 (0.38)
Will Tell Clan	0.438 (0.29)	0.116 (0.31)	0.609* (0.32)
Former Al Shabaab area	0.013 (0.29)	0.197 (0.31)	0.173 (0.32)
Militia district	0.142 (0.29)	0.314 (0.31)	-0.164 (0.32)
Homogeneous district	-0.097 (0.30)	-0.060 (0.32)	0.277 (0.33)
Male	-0.217 (0.28)	-0.112 (0.30)	-0.358 (0.31)
Age	0.011 (0.01)	-0.001 (0.01)	0.016 (0.01)
Constant	0.343 (0.65)	0.460 (0.69)	-0.429 (0.71)
Observations	245	245	245
Pseudo R2	0.037	0.040	0.049

Table III Descriptive Statistics of Respondents in Waves 1 and 3

	(Wave 1 Sample)		(Wave 3 Sample)	
	mean	sd	mean	sd
Female	0.61	0.49	0.53	0.50
Age	32.56	20.12	34.67	12.79
Household Size	7.50	9.43	10.61	5.78
Displaced	0.40	0.49	0.55	0.50
Lived Here Last Year	0.55	0.50	0.74	0.44
Hours Power	8.63	10.32	11.04	11.10
Heard Gunfire	0.49	0.50	0.74	0.44
Saw Fight	0.23	0.42	0.18	0.39
Street Cleaned	0.53	0.50	0.73	0.45
Welfare Index	2.18	0.98	2.32	1.00
Security Has Improved	0.82	0.39	0.76	0.43
Observations	781		125	

our self-identified panel respondents do not report consistent demographic information across waves.⁵ Though 68 respondents reported being the same respondent in Waves 1 and 2 of the survey, at least 20 are actually different individuals, as evidenced by the fact that their gender does not match the gender of the Wave 1 respondent.⁶ Using these parameters, our most conservative estimate would be that of the 647 original participants and the 252 respondents who provided their phone numbers, we have defensible panel data for 30 individuals across all four survey waves (less than one in twenty (!) of the initial sample from the original survey). The majority of the 137 individuals that picked up the phone and opted to participate in the

⁵ Based on discussions with our enumerators, it appears that some participants reported having been surveyed before when another household member was actually the initial respondent. We attribute the unusual increase in the participants reporting that they are the same person between waves three and four to the short time between wave three and four compared to the full year that passed between other waves.

⁶ One interpretation of these trends would be that different respondents were from the same household, even though the survey script asked if the initial respondent was available.

fourth survey wave are, therefore, quite obviously not the same people that provided us the initial 252 numbers. Once all the phone passes, SIM-card swaps, and other unpredictable factors are taken into account, however, is this really worse than the sample we would have received from random digit dialing? All we can say for certain is that we were not speaking to a representative subsample of the initial respondents (which was defensibly representative of the 2012 population). If the goal was to sample only the "landed" population of 2015 Mogadishu – the relative winners of the gradual war of attrition, the families that are likely to be the selectorate for future Mogadishu politics – as a referent population, dropping all self-described “displaced” respondents may be informative.

Measured in terms of overall non-response, the most sensitive questions in Wave Three was whether respondents would be willing to share clan name – the justification for our main study write-up. Of the respondents who *were* willing to provide their clan, however, a clear majority, 58 percent, self-identified as Hawiye or a related sub-clan. It is not accurate to say that all Hawiye are relative “winners,” of course. There were self-reporting members of the Hawiye clan family in the sample who were also displaced. The full range of vulnerability was represented in both the Hawiye and non-Hawiye groups.⁷

In summary: Willingness to provide a phone number for follow-up survey rounds was non-random. Survey attrition across waves was also non-random. As such, the data in

⁷ Though it is not relevant to the findings in the main paper, we used a four-variable additive index that includes two measures of direct exposure to violence and acute insecurity (“have you heard gunfire in the last week” and “has there been fighting on your street in the last week”), self-reported access to electricity, and respondent dwelling characteristics (a binary variable coding whether the survey was conducted in a home with intact roof and walls). A t-test comparing average vulnerability between Hawiye and non-Hawiye respondents finds that average vulnerability among non-Hawiye respondents was 0.3 points higher than average vulnerability for Hawiye (significant at 93 percent).

this study is only a convenience sample of Mogadishu residents. For this reason we rely on randomly-assigned treatments for all inferences.

4 Sample Balance on 24 May 2014 & Alternative Mechanism Speculation

The treatment group on the day of the natural experiment overrepresented vulnerable individuals. Table IV shows that our proxy for vulnerability, displacement, is correlated with increased likelihood of being a respondent the day of the Parliament attack ($p < 0.10$). Men were probably also more likely to pick up the phone on that day than women ($p < 0.16$). Other variables related to overall security did not emerge as statistically significant due to small sample size and multicollinearity.

Table IV: Probability of Being Called and Answering on Bomb Day

VARIABLES	(1)	(2)
Female	-0.610 (0.458)	-0.691 (0.482)
Displaced	0.842* (0.502)	0.932* (0.541)
Age	0.00427 (0.00984)	0.00514 (0.0112)
Live in Mogadishu		-1.261 (0.948)
HH Size		-0.000340 (0.00974)
Heard Gunfire		0.0806 (0.580)
Saw Fight		-0.675 (0.638)
Enumerator Controls	Y	Y
Constant	-1.370** (0.615)	-0.0476 (1.204)
Observations	103	101

Standard errors in parentheses

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

While the order in which phone numbers were called was random, the protocol involved calling all numbers three times before removing them from the sample. The May 24th pool of numbers, close to the end of Wave 3, included many “re-call x2” or “re-call x1” subjects that had been attempted on previous days without success. The skewed sample led us to speculate that these initial fails were more vulnerable subjects than the average.

We have no compelling theory, however, for why the day of the Parliament Attack vulnerable people were more likely pick up the phone. One possibility is that that the heightened threat on May 24th made vulnerable people more likely to pick up a phone call from an unexpected source in the hopes of obtaining important information about situational risks. Perhaps on the day of the bombing there were heightened expectations of aid provision

or from an NGO: That subjects picked up the phone, and stayed on the phone, thinking that security or charity assistance was tied to the survey. The suggestive pattern that non-vulnerable residents of Mogadishu were somewhat *more likely* to answer questions on this day is also consistent with this, since secure residents would be best-positioned to benefit from foreign charity. As it gradually became clear that the people on the other end of the phone were going through the motions of a survey that had nothing in particular to do with the day's events, vulnerable respondents may have become less likely to provide information than their non-displaced counterparts.

We could speculate further that, against the backdrop of the Parliament attack, the interaction of bomb day and the research may have generated negative emotions including, but not exclusive to, fear (or disgust, humiliation, acute helplessness, etc.), especially for vulnerable respondents. In this alternative account the priming does not produce an effect quite as straightforward as in the survey experiment (fear of anarchy inducing a fight or flight response). It is rather emotional discomfort by vulnerable subjects living in an active war zone, with much more pressing concerns than taking part in a telephone survey. Many readers will conclude that the survey experiment is the better-identified portion of this study.

5 Additional Thoughts On The Solicitation of ‘Sensitive Information’

Neither the face-to-face survey nor any of the first two survey waves collected data on political beliefs, respondent identity (except via processes described above), or expectations about the future, focusing instead solely on the provision of basic services, observed acts of violence, and perceptions of security and vulnerability compared to the previous year.

We continued to speculate that our approach was over-cautious. In the fourth survey wave, we explicitly tested the hypothesis that respondents would refuse to participate further in our study if we asked for progressively more sensitive identifying information – such as approximate home location and even respondent name. As our third survey question in Wave

4, enumerators asked respondents where in Mogadishu they lived, requesting landmarks and street names to make the information as specific as possible. The enumerators then used Google maps (in real time) to find approximate geotagged coordinates for the home. To our surprise, less than 12 percent of respondents refused to provide location information. None of the respondents dropped out of the survey after being asked to provide this information. Overall, the 126 respondents who opted to participate in Wave 4 on average refused to answer less than one question each (0.6).

At the very end of the survey, we further tested whether asking for identifying information would lead to high item non-response rate. We asked respondents, "Would you be willing to give us your name?", and if they said yes, we subsequently asked for their name. We did not actually record the names, only whether a respondent was willing to provide their full name. Fully 80% of respondents were willing to provide a full name (first and last). While this response rate was much higher than expected, it also illustrates that at least a fifth of the sample would not have participated if forced to provide their names as a condition of participation. This is almost certainly an under-estimate, since it does not include all of the people, in all previous waves, who declined participation at some point.

Rapport developed with certain respondents over time, but it remains unclear to our team is whether the unexpectedly high willingness to share identifying information developed organically (e.g., due to repeated phone contact with the same people), or if it was a result of survey attrition (e.g., we gradually eliminated everyone except for "talkers" from the sample).

Sometimes the answers that we received were jarringly specific. On December 13, 2014, one of our respondents, while answering the question "do you hear gunfire on your street," volunteered that earlier in the week there had been a person murdered next door to her home. She asked the enumerator to repeat what she had said back to her so that she knew that he had heard her correctly and was listening with empathy. He did, and she proceeded to complete the questionnaire. We share this anecdote to draw attention to the fact that

willingness to provide information to strangers over the phone was higher than expected.

In hindsight, random digit dialing could have reached a larger study population at low additional cost. At least some of the people that picked up the phone probably would have answered our questions, even though many respondents did not seem to believe our IRB approved introductory script. As an open-ended question we asked respondents who they thought we were. The modal response, about 40%, was some version of “You are from a university in California, like you said.” The remainder of the responses were mostly split between “I don’t know,” “the U.S. government,” “an NGO.” Initial plans to use cash payments to subjects via mobile money to incentivize ongoing participation within and across panel waves (and more ambitious plans to use behavioral games to measure risk profiles) were all rendered moot by U.S. government restrictions on financial transfers to anonymous subjects in a frontline state in the War on Terror. This will be a reoccurring constraint for samples recruited from certain war zones. Even if payments had been possible and had worked to arrest overall sample attrition, it is unclear whether compensation would have reduced differential item non-response.

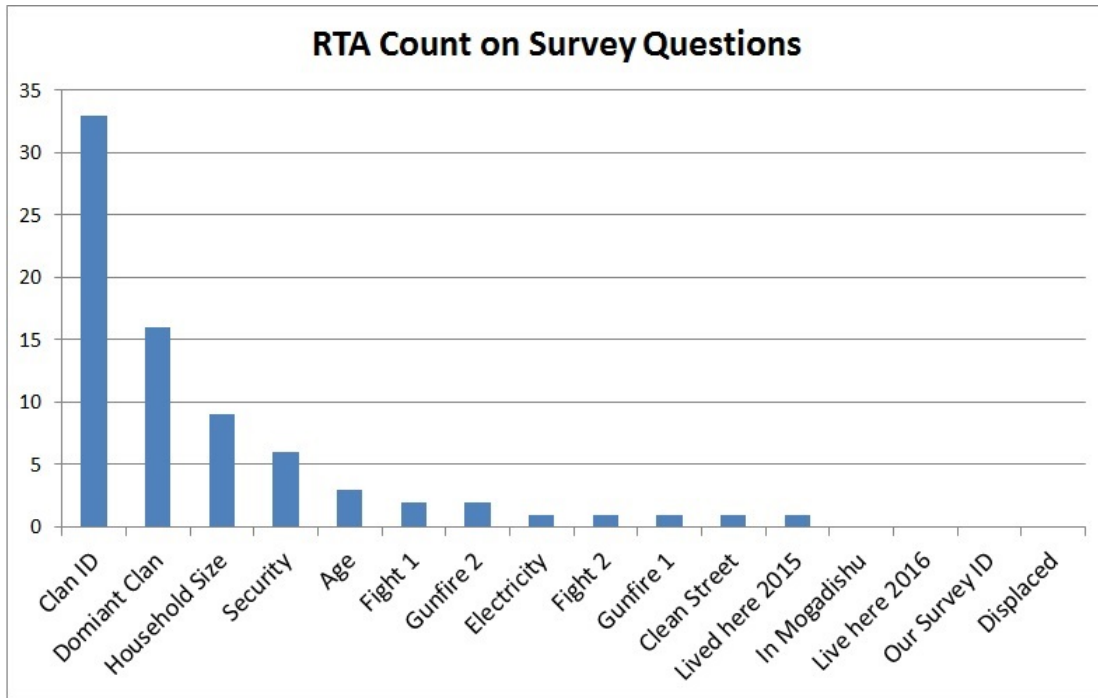
Stephen Krasner once observed dryly: “The phone network works in Mogadishu. And it might be the only thing, maybe, that works in Mogadishu.” The challenge of representation – presenting data solicited from exotic, but highly vulnerable, samples – in a responsible manner is a quickly evolving ethical frontier for students of political behavior.

6 English Translations of Relevant Survey Questions

Appendix Figure 2 illustrates which survey questions had the highest item non-response rates in the third survey round (the data used in the paper’s main analysis). Question language for the most sensitive questions is as follows:

1) Are you comfortable telling us your clan? (If YES:) What is your clan?

Figure 2 Item Non-Response In Wave 3 Of The Survey



- 2) Are you from the dominant clan for this area?
- 3) How many people live in this household?
- 4) Think back to this same time last year. How does the security situation now compare to the situation back then?

The survey prime used in the experiment is as follows:

[ENUMERATOR RANDOMLY SELECTS THREE OF THE FOLLOWING STATEMENTS (a, b, OR c) TO READ TO RESPONDENT:]

- a) In this survey, we are interested in how the existence of a central Somali government is affecting citizens' quality of life.
- b) In this survey, we are interested in how the existence of ongoing lawlessness is affecting citizens' quality of life.
- c. [NO STATEMENT]

7 APSA Recommended Reporting Standards for Experiments

The Experimental Standards Committee of APSA’s Organized Section on Experimental Research has outlined recommended minimum reporting standards for experimental research. In this section of the appendix, we follow the Committee’s reporting format and provide supplemental information as necessary. Where we have already provided relevant research descriptions in the main text, we reference where this information can be found.

A. Hypotheses

See Section 1 (Theory: How Insecurity and Fear Alter Survey Behaviors).

B. Subjects and Context

For eligibility and exclusion criteria, as well as recruitment and survey dates, see Section 2 (Data) and Appendix Section 3 (Telephone Contact and Panel Attrition).

Procedures used to recruit and select participants in the initial survey wave are described in these sections, Appendix Section 2 (Initial Sampling), and further information is available in Driscoll and Lidow (2014).

Survey Wave 3, used for the primary analysis presented here, was conducted via Skype. Enumerators were based in San Diego, and calls were placed from the University of California at San Diego campus. Respondents were located in Somalia. Respondents who reported living

outside of Mogadishu were excluded from the analysis, given our focus on Mogadishu-specific threats and political events.

See Appendix Section 3 (Telephone Contact and Panel Attrition) for a discussion of how our sample of “panel” participants evolved over time. In Wave 3, 20% of the baseline sample (and 54% of the phone numbers provided at baseline) yielded a successful contact when we made the call. Of those respondents who picked up the phone in Wave 3, 88% completed the survey. However, the actual defensible panel we have across survey waves is much smaller, as discussed in the same Appendix section.

C. Allocation Method

See Section 3 (Survey Experiment) for a discussion of survey experiment randomization. Section 4 (A Natural Experiment) also discusses our quasi-experimental approach and presents a balance table to assess whether assignment of conditions is as-if random. Additional discussion can be found in Appendix Section 4 (Sample Balance and Alternative Mechanisms).

Participants were unaware of condition assignments. Enumerators were aware that there were three different conditions and administered the survey accordingly, depending on which condition (question language) had been randomly assigned to that specific respondent. The researchers assessing outcomes were aware of condition assignments but only analyzed them in the aggregate, as they directly related to the study’s hypotheses.

D. Treatments

See Section 3 (Survey Experiment) for a discussion of survey experiment treatment condi-

tions and the control group.

Survey responses were recorded by enumerators using paper survey forms. Surveys were conducted via Skype, with calls placed in San Diego to Somali cell phone respondents in Mogadishu.

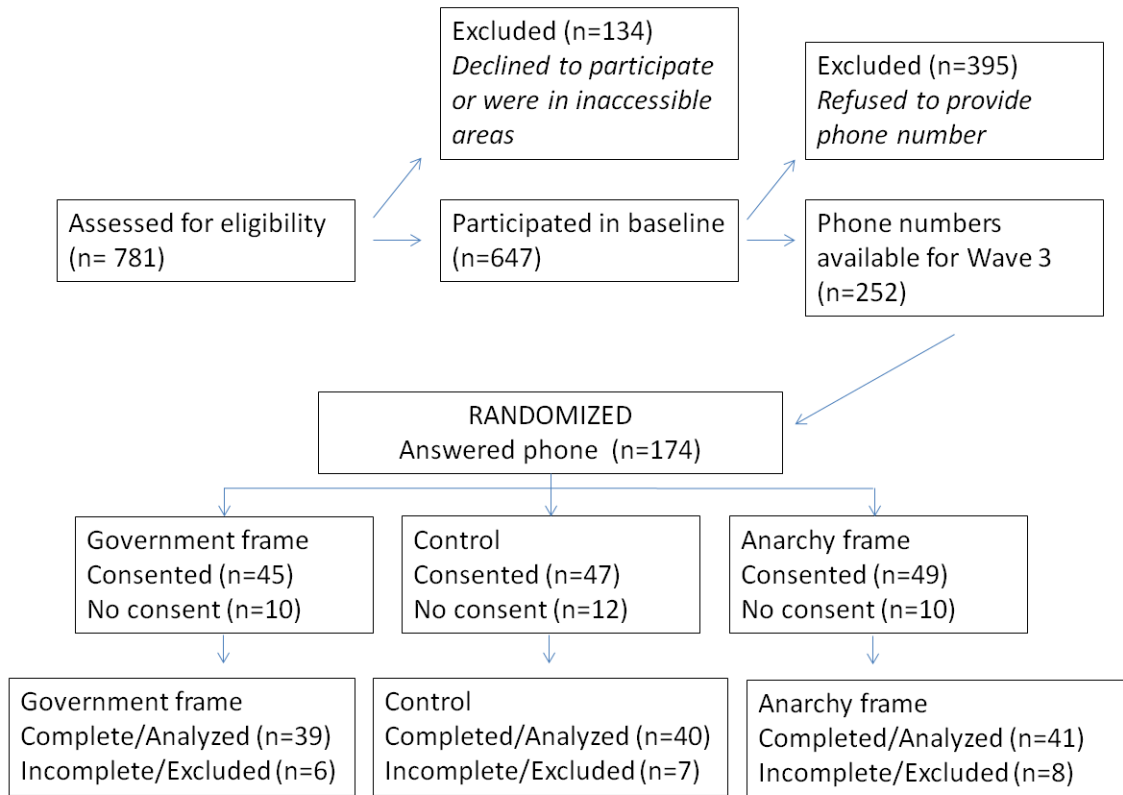
E. Results

1. Outcome Measures and Covariates

Definitions of our measures and covariates are provided in Sections 3 and 4 (Survey Experiment and Natural Experiment).

We designed our Wave 3 survey instrument (and experiment) based on the hypothesis that insecurity would interact with threat (in this case anarchy) to depress survey participation. When this hypothesis was borne out by the data, we looked for other ways to test the robustness of this finding, particularly given our small sample size. We turned to the natural experiment, using the Parliament bombing as another operationalization of anarchy.

2. Participant Flow Diagram



3. Statistical Analysis

See Section 2 (Data) for a discussion of sample attrition and missingness. See Sections 3 and 4 (Survey Experiment and Natural Experiment) for statistical analysis of the results. No weights were used in the analysis of these data.

F. Institutions

All data analyzed in this project were collected according to processes approved by the Human Research Protections Program at the University of California at San Diego (Project #111743 and #131065). See the first footnote for a discussion of funding sources.

Replication data are available at:

<https://dataverse.harvard.edu/dataset.xhtml?persistentId=doi:10.7910/DVN/IS2KTU>.