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Calling Mogadishu: How Reminders of Anarchy Bias Survey Participation

Elaine Denny & Jesse Driscoll

Abstract

How does the fear of anarchy affect telephone survey behaviors? A survey experiment administered to a sample of Mogadishu residents – validated with a natural experiment – is used to assess this question. Randomly-assigned reminders of anarchic violence conditioned differential effects on survey participation depending on subjects' background level of security and welfare. Vulnerable subjects were more likely than non-vulnerable subjects to refuse to provide sensitive survey information after reminders of anarchy.

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[‡]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). The initial 2012 face-to-face survey was conducted cooperatively with Nicholai Lidow and the assistance of enumerators recruited from SUHA Mogadishu. Support for this research was provided by the National Science Foundation (Award No. SES-1216070), as well as the University of California at San Diego's Hellman Fellowship and the

Summary

How does acute fear of anarchic violence affect the survey behavior of subjects residing in active war zones? We answer this question through an examination of telephone survey behaviors for a sample residing in contemporary Mogadishu, the capital city of Somalia.

Mogadishu is, as of this writing, a theater of violent competition for political power. While conducting an apolitical panel telephone survey on citizen well-being, we embedded a survey experiment that varied whether respondents received a survey prime that framed recent events in the city as violent and anarchic, or as state consolidation (or given no prime at all). We then immediately asked our most sensitive survey question: whether respondents would be willing to disclose their clan. We measured non-response in order to make inferences about respondent fear. To increase our confidence in the validity of the finding, we leveraged a natural experiment – a major attack on Somalia’s Parliament – to examine whether the

Program Design and Evaluation Lab. The data, code, and any additional materials required to replicate all analyses in this article are available at the Journal of Experimental Political Science Dataverse within the Harvard Dataverse Network, at: [doi:10.7910/DVN/IS2KTU](https://doi.org/10.7910/DVN/IS2KTU). Madeline Driscoll at Microsoft facilitated permissions for the Skype calls to Mogadishu that began in 2013. The names of the Somali-language survey enumerators that operated the phone bank are Abdulmalik Buul, Ibrahim Warsame, Ahmed Aden, and Abdisalan Haji, Abdikarim Tukri, and Abdiaziz Hussien. Helpful comments on previous drafts were provided by Craig Macintosh, Nahomi Ichino, Chris Fariss, Claire Adida, Lauren Prather, Nico Ravanilla, William Reno, and David Laitin. We also received valuable advice from editors and anonymous reviewers at this journal (and others), co-panelists at the Peace Sciences Annual Meeting, the American Political Science Association Annual Meeting, and various UCSD working groups. The authors have no conflict of interest. Authors’ names are listed alphabetically.

reminders of violent anarchy in the real world caused the same responses as the artificial survey experiment. In both the survey experiment and the natural experiment, the threat of anarchy depressed survey participation disproportionately for the vulnerable.

1 Theory: How Insecurity and Fear Alter Survey Behaviors

Emotions filter incoming information and condition responses in a way that can alter survey behaviors. Anxiety and fear induce risk-averse behaviors (Lerner and Keltner 2001) that correlate with more conservative policy preferences with respect to security (Lerner et al. 2003; Getmansky and Zeitzoff 2014) and lower political engagement (Hassell and Settle 2017). Exposure to violence has many effects other than fear, and recollections of past violence can alter risk preferences even long after the fact (Eckel, El-Gamal, and Wilson 2009; Voors et al. 2012; Callen et al. 2014; Cameron and Shah 2015). Emotionally-induced stress – which can be experimentally manipulated – can disrupt the normal survey ritual.¹ Changes in behavior are most easily observed in the form of non-response when fear primes interact with respondents’ background level of well-being (Huddy et al. 2005). Chronically vulnerable populations are more susceptible to “fight or flight” fear-triggers than their secure counterparts (Levine 2015).

A contemporary Somali sample magnifies the salience of these concerns. Subjects living in war zones are strategic about whether to share information and with whom (Lyall, Shiraito, and Imai 2015). Revealing attitudes on sensitive matters, even over the phone, carries danger (Blair et al. 2013).² As American social scientists taking a close interest in

¹ The seminal treatment of “don’t know” and “refuse-to-answer” survey behaviors is Berinsky (2006). For discussions of fear as a plausible mechanism to explain behavior in post-conflict surveys see Driscoll & Hidalgo (2014). The literature review on enumerator effects in Bush and Prather (2018) is highly valuable.

² A superb discussion of how cellular telephone communication networks change the

the opinions of Mogadishu residents, our motives were interrogated skeptically by both our Somali subjects and our research associates. To the extent we were neutral observers we could be accused of engaging in virtual poverty tourism. To the extent we were something *other than* neutral observers, however, we were potential partisans. In a setting where famine has been used as a weapon for decades, charity cannot be seen as politically neutral. Nor is data collection a pure public good, since conducting the first representative survey in a quarter century, with assistance from the local government, could potentially be viewed as a kind of census with the effect of locking-in de-facto property rights for the war’s winners.³ The decision by subjects to stay on the phone and answer our questions was weighed against the option of hanging up to remain invisible.

Our theoretical expectation was that acute fear, induced by reminders of the surrounding anarchic environment, would decrease survey participation *differentially*, especially for the most vulnerable respondents. The low welfare members of our sample were more likely to lack political protection, connections, and representation. They were, we reasoned, more likely to have an emotional response to reminders of political instability that would leave an imprint on the data. Willingness to share information would be affected by the interaction between perceptions of background levels of threat within the society and one’s social position:

relationship between civilians, insurgents, and counterinsurgent military forces can be found in Shapiro and Weidmann (2015). Their distinction (252) between texts (which cannot be overheard) and voices (which can) is relevant to our study, since respondents were speaking into telephones. Many Somalis regularly admit to a belief that agents of both the Somali government and the U.S. government, as well as agents of the Al Shabaab rebel group, regularly monitor electronic communications. How and whether any of this really mattered remained a source of debate within our research team, as discussed in Appendix Section 5.

³ See Appendix Section 2.

H1: *Vulnerable residents of Mogadishu will be less likely to answer sensitive survey questions after reminders of anarchic violence than secure residents of Mogadishu.*

2 Data

Our sample is generated from a face-to-face, population-representative survey of Mogadishu residents conducted in 2012 (Driscoll & Lidow 2014). Of this initial sample, 252 (39% of the sample) provided phone numbers for follow-up surveys. These numbers were called via Skype by fluent Somali-language enumerators from the San Diego Somali 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). All of our telephone survey enumerators were male.⁴ While our enumerators varied somewhat in their Somali accents and dialects, we could discern no statistically significant differences by enumerator on the outcomes of interest in this study.

Our sample is attractive because it contains high levels of variation in both acute fear and underlying vulnerability. In all waves, the survey instrument avoided explicitly political questions due to the charged situation on the ground and concerns that we could put vulnerable respondents at risk. Instead, the short 24-question survey asked about respondents' current level of services and safety. Our enumerators could discern from vocal characteristics whether or not the respondent was male or female, but little additional information could be gleaned without respondent compliance.⁵

⁴ Benstead (2014) found that survey respondents in Morocco answered more questions – fewer skips and “don’t knows” – when the interviewer was male (377), which may be attributed to “higher authority of males than females in patriarchal societies” (Benstead 2014b: 745), though she could not replicate the finding on a subsequent survey (2014b, 757, fn17).

⁵ For example, none of our enumerators could reliably differentiate subjects by clan by voice characteristics, nor could a Bantu accent be discerned from a “regular” Somali accent.

Why is clan so sensitive in Mogadishu? Somali clans provide a sense of group identity, organize sub-national distributional politics, and govern a host of traditional intra-ethnic social interactions – from social welfare insurance to marriage markets to militia recruitment. Clans have been focal points for conflict in Somalia since at least the 1980s.⁶ There have been clan-targeting pogroms every time the capital switched hands, leading to an exodus of many non-Hawiye civilians from the city. Abgal and Habr Gidr clans cluster together in homogenous neighborhood enclaves where children and assets like electrical generators can be protected. Measured in terms of overall non-response, the most sensitive question in the 2014 survey was whether respondents would be willing to share clan names – a question that had not been asked face-to-face, or in the initial telephone follow-up survey, because it was deemed too sensitive.⁷ Variation in willingness to provide an answer to this question motivated the experimental design that follows.

3 Survey Experiment

Our survey experiment randomly assigned respondents to one of two treatment conditions or to a control group. Randomization occurred prior to placing any calls and the resulting categories are largely balanced on observables (See Table 1, columns 1 through 6). In our small sample, unfortunately, gender and displacement status are not distributed evenly across conditions despite randomization. A larger sample would have been desirable.

Table I About Here

⁶ Appendix Section 1 provides a succinct background summary. Readers seeking more background on the Somali case should begin with Woldemariam (2018), Chapter 6.

⁷ The second most sensitive question, measured by non-response, was also clan related. We had asked whether respondents were in the dominant clan in an area (without being asked to name the clan).

Two treatments primed respondents to consider Somalia’s current government consolidation or persisting anarchy. Both were plausible descriptions of respondents’ political reality in 2014, and both were potential sources of fear. We stated: “In this survey, we are interested in how the existence of [a central Somali government / ongoing lawlessness] is affecting citizens’ quality of life.” A control group, consisting of approximately one third of the survey participants, was given no prime at all.

Directly after the experimental treatment, respondents were asked the question we expected to be our most sensitive: whether they were willing to provide their clan name. Item non-response on this question is the dependent variable – a behavioral indicator of subjects’ participation reticence. Our favored hypothesis is that this reticence was induced by a shift in perception of threat via the mechanism of fear, though we cannot observe this mechanism directly.

Since we theorized that the risks of revealing sensitive information would not be equally acute for the entire sample, varying differentially on underlying vulnerability, statistical analysis examines interaction effects between “fear primes” and respondent vulnerability. Our measure of vulnerability is whether or not a person reports being displaced in the past year.⁸ Violence in the city has driven many from their homes. Internally displaced people (IDPs) live in camps inside Mogadishu or squat in abandoned buildings. Other people displaced by war and famine from the countryside have migrated to Mogadishu for security. Our question does not distinguish between individuals in IDP camps who had fled the rural famine and former urban residents who were relative losers in the ongoing turf war. In either case, the question is a valid proxy for relative social vulnerability. This question also had the advantage of being asked at the very beginning of the survey so had full compliance.⁹

⁸ The question wording was “Are you currently displaced?”. See Appendix Section

⁹ We initially analyzed a model where vulnerability was operationalized as a “vulnerability index” of service provision and reported safety (discussed in Appendix Section 3), though

Figure 1 *Figure I: Survey Experiment Results (Table II Effects)*

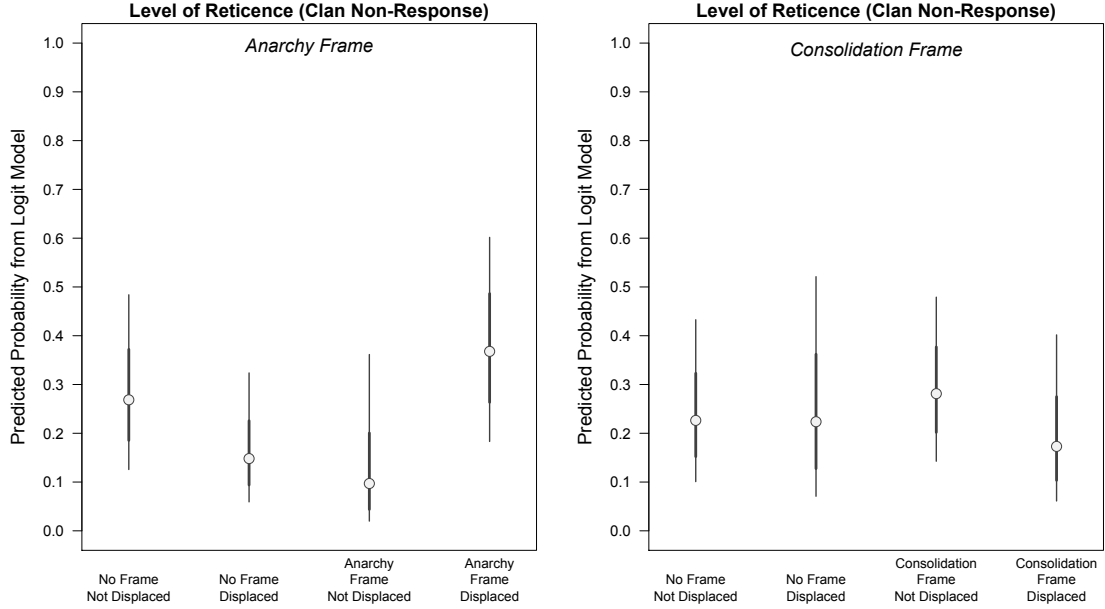


Table 2 presents results of two logistic regressions. Refusal to name one's clan is the dependent variable. Both include enumerator fixed effects. Surprisingly, evidence suggests the anarchy prime reduced reticence somewhat among the non-vulnerable. The interaction between displacement and the anarchy prime is large and statistically significant – evidence of a differential treatment effect. These results hold when we control for the government consolidation prime, which by itself elicits no significant changes in behavior (See Figure 1). The marginal effect of seeing the anarchy prime is a fourfold increase in the probability that a displaced person will refuse to answer the question "What is your clan?" compared to a primed non-displaced subject.

Table II About Here

the sample size was smaller due to some non-response on questions that comprised the index.

In summary: Two experimental treatments were tested against a control in the survey. Only one experiment produced meaningful results. Reminders of lawlessness differentially changed survey behaviors. Reminders of political consolidation did not. The survey experiment provides evidence that the threat of anarchy decreases participation in sensitive survey questions differentially for the vulnerable respondents. We continued to be troubled by concerns about external validity – e.g., the dubious claim that survey primes or vignettes about violence can substitute meaningfully for experiencing real-world violence.¹⁰

4 A Natural Experiment

On 24 May 2014, the last day of Wave 3 of our telephone survey, Al Shabaab militants detonated a car bomb outside of the Somali Parliament compound. A coordinated attack on government security forces followed. The gun battle lasted several hours. At least seven militants and ten members of government forces were killed. Four lawmakers were among the many civilians injured. The public and political nature of the attack differentiated it from background levels of violence that Mogadishu residents experience. Word of the attack spread quickly. We coincidentally placed the last fifth of our survey calls just hours after the attack (after 5:00pm Somali time). Our Somali American enumerator team in San Diego had already heard of the bombing via their social networks by the time they began making survey calls that morning.

From the perspective of our survey sample (i.e., the full set of telephone respondents reached in the third wave) the “Parliament Attack” treatment has all of the relevant characteristics of a natural experiment. The order in which numbers were called (or called back) from our list was randomly assigned. The telephone network continued to function throughout the day. The percentage of respondents agreeing to participate in our survey was unexpectedly higher compared to previous days (97% versus 74%) and subsequent investi-

¹⁰ See Driscoll & Maliniak (2016).

gation of the balance across samples suggests that these “extra” respondents skewed towards the more vulnerable (Table 1).¹¹ Even with this underlying bias in the sample, analysis of the interaction between the treatment effect and respondents’ underlying level of vulnerability can quantify differential effects on vulnerable and non-vulnerable subject pools.

Recall that our theoretical expectation was that the attack would elevate vulnerable residents’ perception of acute threat more than non-vulnerable residents. The attack was a pre-survey treatment, so measuring a behavioral “fear response” was accomplished in three ways: (1) a count of total questions that a respondent refused to answer; (2) a binary variable for whether the respondent refused to answer any of the three most sensitive questions on the survey (as measured by frequency of refusal – two involve clan and one involves a shift in security); and (3) the same outcome measured in the survey experiment: refusal to share clan name (the question that respondents were most reluctant to answer). Table 3 considers the relationship between vulnerability (operationalized once again by displacement), “bomb day,” and our three outcome measures (See Figure 2). A Poisson model is used for the count variable in Column 1, while a logit model is used for the binary variables in Columns 2-3. Non-vulnerable (not-displaced) subjects seem *more* willing to answer questions on bomb day, making interaction between displacement and bomb day substantively large and signed according to expectations in all three models. Models are underpowered due to the small sample size, with p-values ranging between 0.06 and 0.11. The total number of item non-responses probably increased for Mogadishu’s most vulnerable respondents on the day of the Parliament attack.

¹¹ One speculative rationalization for the fact that vulnerable people were more likely to pick up the phone after the Parliament attack is that a higher threat environment and anxiety have been shown to correlate with greater information-seeking in the political arena (Marcus 2002; Merolla and Zechmeister 2009). See Appendix Section 4 for additional discussion.

Figure 2 *Figure II: ‘Natural Experiment’ Results (Table III Effects)*

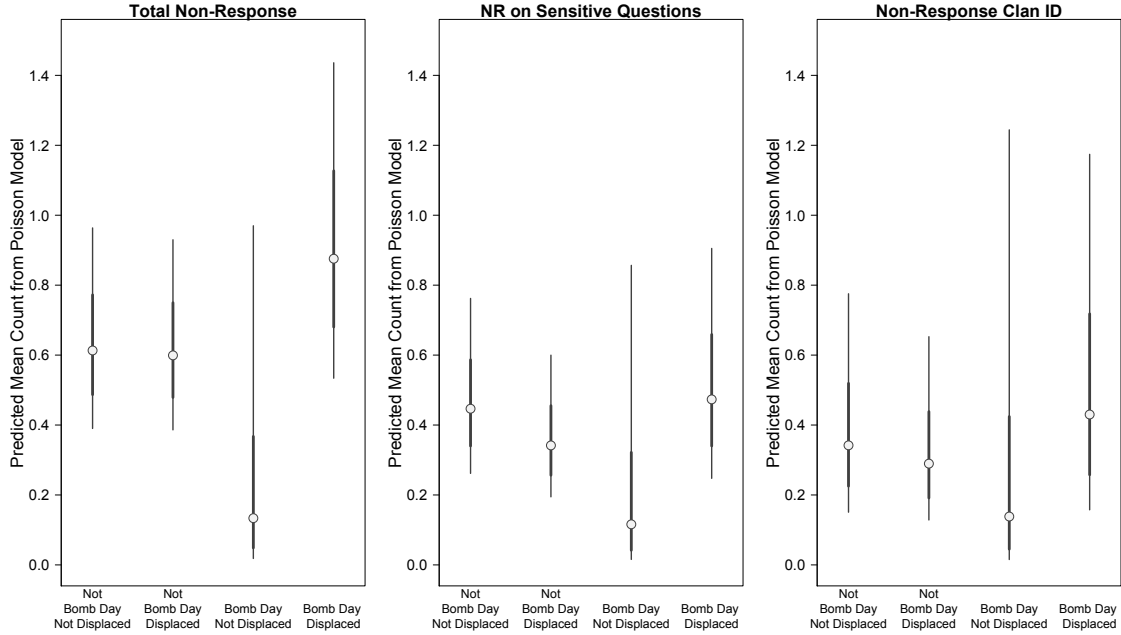


Table III About Here

5 Discussion

Both anarchy primes seem to have made non-displaced respondents somewhat *more* willing to provide sensitive data at the same time they made displaced respondents *less* willing to answer the same question. Our findings serve as a reminder that very small changes in the survey environment – subtle shifts in consent script, question wording, enumerator identity, or changes in local security context – can alter survey participation. While this has long been appreciated by students of survey behavior, our analysis underscores that in war zones the effect on attrition and missing at random (MAR) item response is acute for insecure populations. In general, we draw the following four practical lessons from the study:

- Understanding local context is vital, since “basic demographics” questions can be sensitive in war zones and should be asked near the end of the survey instrument (if they are asked at all). If Somali clan identities had been solicited at the beginning of a survey, all downstream data would have been contaminated.
- University-sponsored scientific research is more likely to generate compliance than research funded by embassies (Corstange 2014).¹² Lengthy IRB-approved solicitation scripts irked some subjects (see Appendix Section 5), but the thorough explanation of our identity and motives probably helped to build rapport and gave subjects opportunities to gauge our sincerity.
- The proliferation of cheap cellular phones to places like Mogadishu make long-distance surveys (like the one in this paper) possible at very low costs. When the research team’s only link to subjects occurs through a telephone, however, contact is more fragile than in face-to-face surveys. It can be severed by subjects with the touch of a button. Given the low marginal costs of adding additional subjects, large initial samples should be recruited in expectation of attrition. Over-sampling vulnerable populations is one strategy to avoid ending up with very small numbers of respondents after attrition.
- If item non-response on sensitive questions is anticipated, and policy-relevant inferences depend on claims of sample representativeness, extra care should be taken to collect the kind of data that will aid imputation of missing data. Context is critical to determining which elements can be solicited, without raising suspicions from subjects, to test “MAR conditional on X” assumptions (with the goal of facilitating defensible, transparent, and reasonable reweighing to regain pre-attrition sample characteristics).

¹² The author’s use of the verb “deter” throughout the writeup implies a theory of fear driving non-response, though the fear mechanism is not explicitly theorized.

6 Conclusion

Vulnerable populations in Mogadishu became less willing to provide sensitive information in a telephone survey when primed to think about ongoing lawlessness. A reminder of real-world anarchy provided an opportunity to probe the external validity of the survey experiment. This behavioral pattern is consistent with the mechanism of fear. Relative winners and relative losers in Somalia’s ongoing war have different incentives to cooperate with scientific data collection. Speculations about the precise mechanism aside, our experimental methodology illuminates that social conditions in conflict zones interact with the survey ritual in ways that can systematically bias the data collected.

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Table I Balance Table of Survey Respondents

	Survey Experiment						Natural Experiment			
	Gov't	sd	Control	sd	Anarchy	sd	Control	sd	Bomb	sd
Female	0.65	0.48	0.47	0.50	0.51	0.51	0.56	0.50	0.42	0.50
Age	32.89	9.78	36.86	13.79	34.81	14.09	35.31	13.50	33.31	10.07
Live in Mogadishu	0.97	0.17	0.93	0.25	0.95	0.22	0.97	0.17	0.88	0.33
Displaced	0.61	0.49	0.48	0.51	0.56	0.50	0.49	0.50	0.73	0.45
Family Size	9.89	4.28	10.75	6.38	11.00	5.67	10.52	5.73	11.12	5.05
Heard Gunfire this Week	0.78	0.42	0.69	0.47	0.78	0.42	0.75	0.44	0.76	0.44
Saw Fight this Week	0.14	0.35	0.26	0.44	0.12	0.33	0.17	0.38	0.19	0.40
Welfare Index Spring 2014	2.44	0.95	2.31	1.13	2.28	0.85	2.39	0.97	2.14	1.01
Security Change (-1 to +1)	0.61	0.73	0.65	0.70	0.68	0.69	0.72	0.65	0.40	0.82
Observations	37		45		42		99		26	

Table II

Level of Reticence (Clan Non-Response)
by Anarchy Prime and Displaced

Refusal to State Clan	(1)	(2)
Anarchy Frame	-1.229 (0.852)	-1.398 (0.891)
Displaced	-0.748 (0.545)	-1.078 (0.793)
Displaced X Anarchy	2.438** (1.029)	2.766** (1.177)
Consolidation Frame		-0.444 (0.741)
Displaced X Consolidation		0.715 (1.098)
Enumerator Fixed Effects	Y	Y
Constant	-1.002** (0.478)	-0.836 (0.543)
Observations	120	120

Standard errors in parentheses

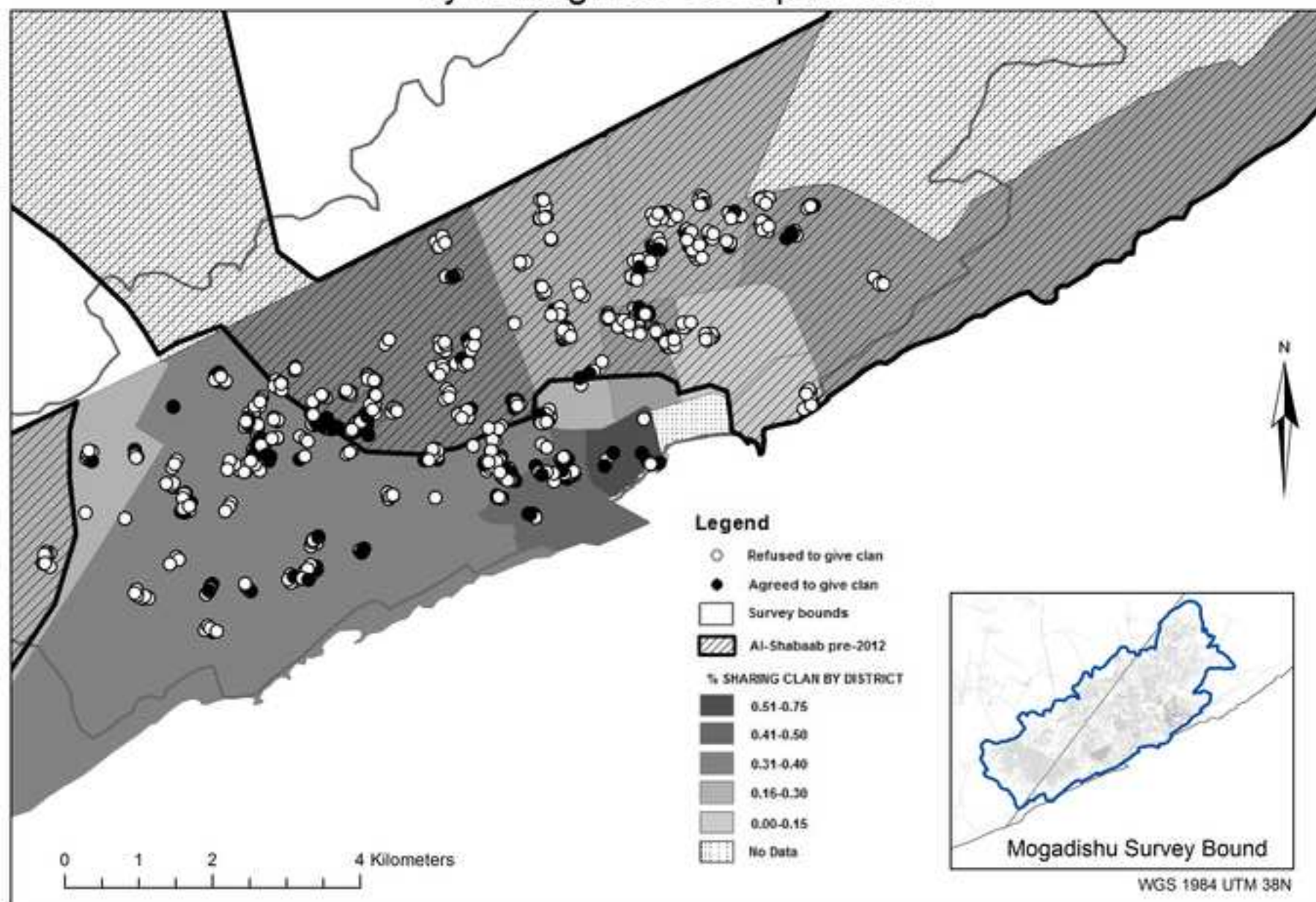
*** p<0.01, ** p<0.05, * p<0.1

Table III

Level of Reticence, by Bomb Day and Displaced Status			
VARIABLES	(1) Total NR	(2) NR on Sensitive Questions	(3) NR Clan ID
Displaced	-0.0231 (0.273)	-0.629 (0.464)	-0.166 (0.484)
Day of Bombing	-1.524 (1.023)	-1.533 (1.143)	-0.905 (1.148)
Displaced X Bomb Day	1.903* (1.063)	1.965 (1.272)	1.301 (1.281)
Enumerator Controls	Y	Y	Y
Constant	-0.487* (0.252)	-0.594 (0.426)	-1.121** (0.460)
Observations	120	120	120

Standard errors in parentheses
 *** p<0.01, ** p<0.05, * p<0.1

Initial Location of Mogadishu Survey Participants, By Willingness to Report Clan



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.

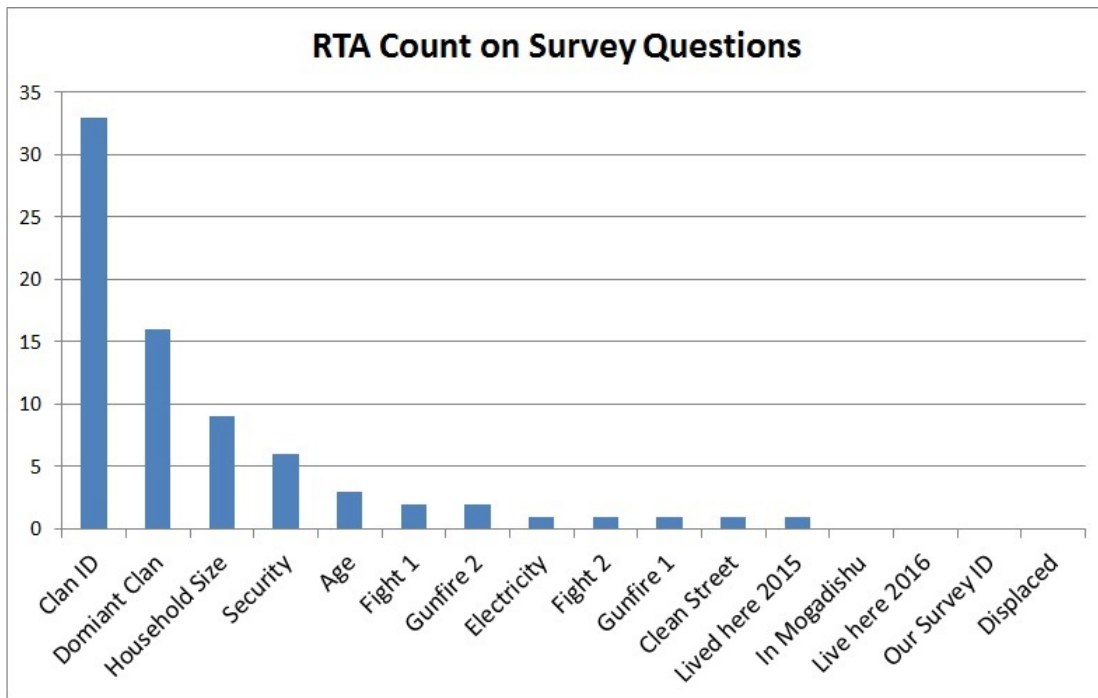
Stephen Krasner once observed dryly: “The phone network works in Mogadishu. And it might be the only thing, maybe, that works in Mogadishu.” The challenge of representation – presenting data solicited from exotic, but highly vulnerable, samples – in a responsible manner is a quickly evolving ethical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

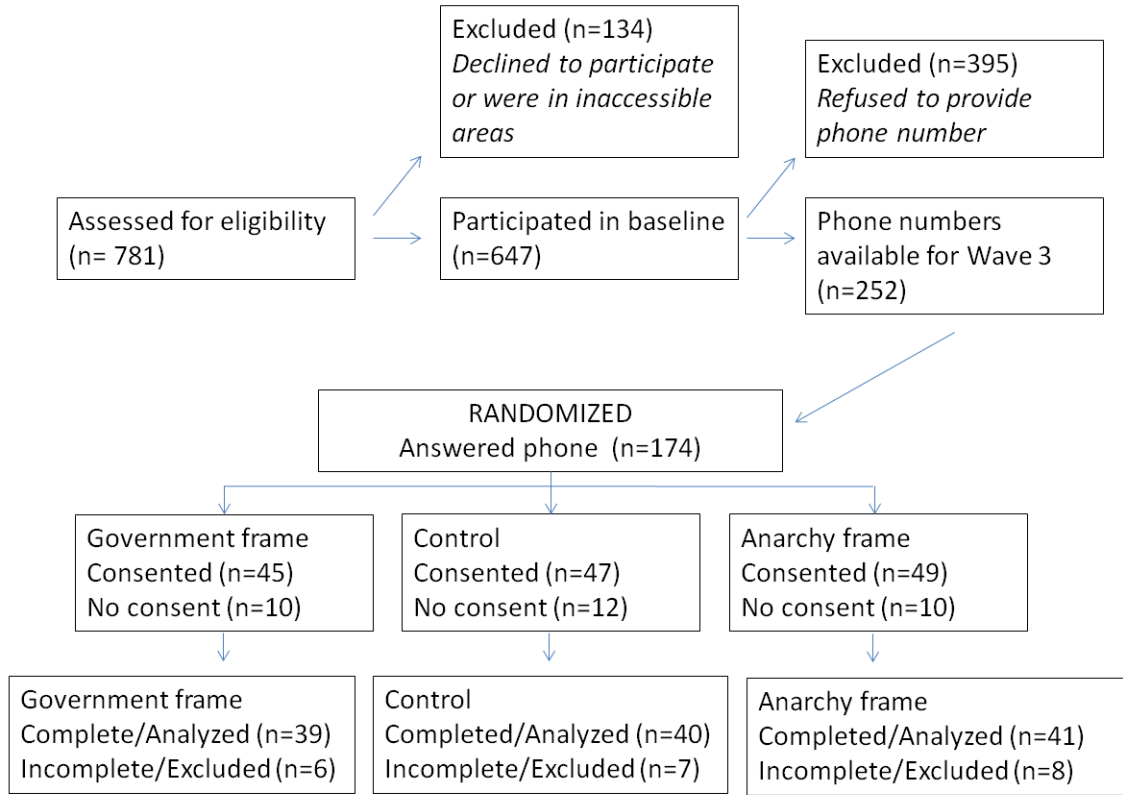
E. Results

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>.