NOT FOR PUBLICATION

Supplementary Online Appendices

The Social Costs of Public Political Participation:
Evidence from a Petition Experiment in Lebanon

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Appendices

A Petition

This appendix provides more detail on the petition experiment. Figure 1 shows the enumerator instructions for inviting respondents to participate in the public/private petition while Figure 2 presents the actual text of the petition. Both the survey script and the petition text were translated into Lebanese Arabic to ensure that language barriers would not influence the choice to sign the petition. The enumerators were trained to use the appropriate invitation script for the treatment assignment and also to read the complete petition with respondents before allowing them to decide (in private) whether to sign.

A.1 Ethical considerations

In deciding how to make the petitions available to politicians, we took several factors into consideration. On one hand, publicly releasing the petitions could potentially put signatories at risk of social sanctioning or getting cut off from clientelist benefits. On the other hand, we would be deceiving petition signatories if we did not release the petitions as promised. We also wanted to make sure that signatories and politicians would understand the purpose of the private/public variation when the petitions were released so as not to raise questions about the credibility of the exercise.

To accomplish the above, the Lebanese Center for Policy Studies (LCPS) is releasing the petitions in conjunction with a policy report. The policy report discusses the motivation for the petition in Lebanon's political context. It then explains the motivation for incorporating a public/private experiment into its petition activities and explains how to interpret the results for the sample and population. LCPS lists all signatories to the petition as an appendix to this policy report along with whatever information signatories were asked to provide. This means that signatories in the public condition have their names included while those in the private condition do not, consistent with what respondents were told in advance.[1] LCPS is also making available images of the signed petitions (as a pdf document) on its website.[2]

We took two steps to address concerns about the risks of signing in the public condition. First, we made sure that respondents were fully informed about how the petitions would be used so that they could assess the risks. Specifically, as can be seen in the scripts below, all respondents in the public condition were told that the petitions would be presented to political leaders and that their leaders would be able to know that they personally had signed. This enabled respondents to assess for themselves whether they wanted to sign under these conditions. Second, it was emphasized that the decision to sign the petition was completely voluntary. Respondents were asked to make their decisions in private and to seal the petition—whether completed or not—in an envelope before returning it to the enumerator. By inviting respondents to make their decision out-of-sight of even the enumerator, we aimed to reinforce the voluntary nature of the decision and

[1] We contemplated blacking-out names from the 'public' petitions before submitting them to officials but this also seemed like deception since participants acted believing their signatures would be revealed.

[2] LCPS collected additional signatures to the petition through its other activities (all of which were 'public' petitions) and these are also included in the appendix to the policy report and on the website.

minimize the possibility that the enumerator's presence would make the respondent feel obligated to sign. By providing full information and making the decision voluntary, the conditions under which respondents decide to participate resemble those of many petition campaigns organized by civil society organizations or political groups, most of which mandate signatures (La Raja, 2014). Indeed, the very goal of the study was to understand when individuals would be willing to take political action voluntarily given that there are potential associated risks (of which they were aware). We note that this study is covered under IRB PRO15060167 at the University of Pittsburgh.

We also note that in the Lebanon there is little risk of more serious political consequences from petition signing (such as arrest or other forms of political oppression). Lebanon is a strong democracy, as indicated by its score of six in the Polity IV dataset (where scores range from -10 to 10 and 10 indicates full democracy). Turnout in the last parliamentary elections (in 2009) was estimated at about 55 percent.[3] Freedom of the press is relatively strong and protest is not uncommon. Petitions are also a commonly used form of public political participation in Lebanon. Among other causes, Lebanese citizens have used petitions to demand social and political change (Hanafi and Arvanitis, 2016, 293-294), to demand democratic elections (Karam, 2005), to build support for the adoption of a civil marriage law, (Karam, 2005), to advocate for the civil and economic rights of various groups (Bianchi, 2014), and to protest unsustainable or otherwise undesirable infrastructure projects (Meraaby, 2017). Our piloting suggested that individuals were not concerned about political repercussions from petition-signing.

[3] http://www.nytimes.com/2009/06/08/world/middleeast/08lebanon.html

11 Behavioral Outcomes

SCRIPT: *We would now like to give you the option to take two different actions. You can decide to participate in none of them or in as many as you like. We are asking these questions on behalf of LCPS but please base your answer on what you honestly want to do.*

11.1 Invitation to Sign Petition

Enumerator Note: Before continuing, be sure to check which version of the petition you are conducting with this respondent.	**PUBLIC**	**PRIVATE**

SCRIPT:*I am now going to give you a copy of a petition demanding fundamental changes to Lebanon's political system in such a way that confessionalism is phased out and the nation's interests are put ahead of sectarian, regional, and personal interests. LCPS is collecting signatures from citizens all over the country to call upon leaders to put economic priorities for the country ahead of confessional politics.Signing this petition means that you have a chance for your voicing your opinion and take action on an important issue. Please remember that LCPS is an independent, non-partisan, non-governmental organization.*

For the public group only!
*Before making your decision, you should be aware that the petition **does** require you to sign your name.This means there is **some** chance that political leaders will know that you personally signed the petition.* *All petitions will be gathered together and the results will be shared with leaders around the country, including members of government, political parties, your MPs, and your Zaim, so that the voices of those who support this petition can be heard. Just to be clear, signing this petition means that you are in favor of taking a stand on this issue, even if your political leaders do not share the same position.* *In order for your petition to be considered valid, you must complete all information in the bottom portion of the petition.* *Also please rest assured that any information you provide on the petition will not be linked by personal information to the survey you just completed.*

For the private group only!
*Before making your decision, you should be aware that the petition does **not** require you to provide your name. This means there is **no** chance that political leaderswill know that you personally signed the petition.* *All petitions will be gathered together and the results will be shared with leaders around the country, including members of government, political parties, your MPs, and your Zaim, so that the voices of those who support this petition can be heard. Just to be clear, **although your name will not be revealed to your political leaders,** signing this petition means that you are in favor of taking a stand on this issue, even if your political leaders do not share the same position.* *In order for your petition to be considered valid, you must complete all information in the bottom portion of the petition.* *Also please rest assured that any information you provide on the petition will not be linked by personal information to the survey you just completed.*

Enumerator Note:
Hand the correct copy of the assigned PUBLIC or PRIVATE petition to the respondent along with an empty envelope
Allow the respondent to read through the petition so that s/he fully understands the terms.
If the respondent is uncomfortable reading you may read aloud to him/her.

SCRIPT:*You can read the petition here and decide in private whether or not you actually want to sign it. If you do want to sign, please complete ALL of the information requested and put the petition back in the envelope and seal it until it can be opened by LCPS. This way I will not see what you decided to do.*

[RESPONDENT COMPLETES PETITION PROCEDURE]

Figure 1: Script inviting participants to sign the petition

RESPONDENT ID:

__ __ __ __ __ __ __ __ __ __

CROSS-SECTARIAN PETITION FOR SYSTEM CHANGE IN LEBANON

Preamble

The recent protests that sparked mobilization across Lebanon originated with dissatisfaction over trash removal but quickly tapped into a larger sense of prolonged dissatisfaction with public goods and poor service provision in Lebanon as well as the overall inability of the government to ensure the economic welfare of people in the country.

The current structure of the Lebanese state, political system and electoral system has heavily contributed to the spread of corruption, the development of sectarian politics and sect-based parties, and the expansion of clientelistic and mafioso practices. This has resulted in the lack of proper regional or national development and the absence of basic public services as the political elite argues over how to divide state resources and maximize private gain. Since the civil war, we have witnessed time and time again how confessional interests are put ahead of the wider economic needs and priorities for the country.

Petition

We, the undersigned, demand that a constitutive entity be formed and tasked with radically revising the political structure in such a way that confessionalism is phased out and the nation's interests are put ahead of sectarian, regional, and personal interests.

We specifically call for:

1. Abolishing confessional politics in accordance with the Lebanese constitutional amendment, which states that: "The abolition of political confessionalism shall be a basic national goal and shall be achieved according to a staged plan."

2. Dropping the sectarian division of power and positions for the three heads of state as well as eliminating quotas related to the parliament, government positions, public servant jobs, the judiciary and military.

3. Holding accountable, based on sound mechanisms for investigation and fair trial, individuals (including leaders and politicians) who are proven guilty of crimes and mismanagement of public resources.

4. Reducing the influence of sectarian parties and encouraging the emergence of programmatic parties that organize along economic interests and prioritize need-based development.

5. Ensuring that revenue and services from the state - including all future revenue associated with oil and gas –are allocated ***on the basis of need and priorities for economic development and not on the basis of traditional confessional politics.*** This means that no MP, political party, or Zaim should dictate the allocation of public funds nor directly receive any income from public resources.

_____ _____

Name Electoral district

_____ _____

Age Confession

_____

Date

Figure 2: Text of the petition

B Sampling and Randomization

B.1 Sampling

The data in this study comes from a nationally representative sample of 2,496 adult Lebanese citizens (18-65 years of age) fielded from December 2015-February 2016. Respondents were selected through multi-stage cluster sampling. First, primary sampling units (primarily villages in rural areas and cities or neighborhoods in urban areas) were classified on the basis of population size and predominant sect. PSUs that had fewer than 2,000 people were classified as 'small', PSUs with 2,001-35,000 people were classified as 'medium', and PSUs with more than 35,000 people were classified as 'large'. Information on the predominant sect in each PSU was obtained from Information International (II), the professional Beirut-based survey firm that implemented the survey, with assistance from LCPS. We then grouped PSUs into strata on the basis of their district, the PSU population category, the predominant sect, and whether or not the PSU was in the capital of that district.[4] PSUs were randomly sampled within strata using simple random sampling. All in all, the final sample consists of 2,496 respondents in 195 PSUs.

Households (and individuals within households) were randomly sampled within PSUs, with one respondent per household. The number of respondents in each PSU was determined by the PSU population category. We sampled eight households in 'small' PSUs, 16 households in 'medium' PSUs, and 32 households in 'large' PSUs.[5] II sampled households within PSUs following their standard procedures. They first divided each PSU into neighborhoods based on local information and then selected households using systematic random sampling in each selected neighborhood according to the number of buildings in the neighborhood. One challenge with this method is that there is a lack of reliable data in Lebanon on the number of households within each neighborhood, making it difficult to know with confidence the probability that a household was selected within its PSU strata. We address how we deal with this below in Appendix H.1.

Within sampled households respondents were selected using simple random sampling. To achieve a similar number of men and women in the sample, a target sex was set for each household. Upon arriving at a household, enumerators first obtained permission from the head of household to conduct a survey with a randomly chosen member of the household. The enumerator informed the head of household that the purpose of the survey was to understand people's perceptions of economic, political, and sectarian issues in Lebanon (see Figure 3). If consent was provided, the enumerator then proceeded to ascertain how many individuals in the household met the sampling criteria (they were Lebanese citizens, were between the ages of 18-65, and met the definition for household member, see Figure 4). The respondent was randomly sampled from among those household members who met these criteria (and the gender target) by randomly selecting a month of

[4] We excluded PSUs and strata that had fewer than 200 people and that were in insecure areas, which were predominantly Hezbollah controlled areas. In total we excluded 194 PSUs, reducing our eligible PSUs from 1,017 to 823.

[5] These numbers were set based on our block random assignment needs for a different component of this project.

the year and then selecting the person born earliest in that month.[6] If the sampled respondent was at home, the enumerator proceeded with another round of informed consent (see Figure 5). If a sampled household or individual was not at home, one follow-up visit was conducted. Enumerators chose a replacement household if no one was home after two visits or the head of household refused to provide consent by taking the next household to the right upon exiting. If an individual was not at home or refused consent, the next household member on the list (according to the 'birthday' sampling method) was selected to be interviewed. We note that all sampling procedures and recruitment protocols were approved under IRB PRO15060167 at the University of Pittsburgh.

While the results presented in the main text are estimated on the sample, we check whether the results change when we estimate them for the population. This is especially important because the 60 percent unit non-response raises questions about sample selection and the generalizability of the sample estimates. In Appendix H we discuss how we construct population weights using the survey design weights as well as post-survey weighting adjustments. We also show that the sample and population results are similar.

[6] In case no one was born in that month, the field workers moved on to the next month. This 'birthday' method was used after piloting showed that fully enumerating a list of all eligible individuals within a household and sampling from that list drew too much suspicion from respondents.

Survey of Public Opinionin Lebanon

Q1 FILL IN THE SURVEYOR'S INFORMATION	
A. First name, Last name	B. Surveyor ID

Q2RESPONDENTSURVEY ID [GENERATED AUTOMATICALLY]	ID_____

1 Respondent Selection

1.1 Household Consent

ENUMERATOR NOTE: Upon approaching the correct randomly sampled household, please introduce yourself using the following script.

SCRIPT: *Good day. My name is _____. I am from Information International, a surveying organization that is not related in any way to the government or any political party. We are working with independent researchers led by the Lebanese Center for Policy Studies (LCPS) to study ordinary citizen's perceptions of economic, political and sectarian issues in Lebanon. We would like to discuss these issues with a member of your household. Any answers provided will be treated as confidential information; that means that we shall never single you out.*

We are interviewing 2400 households in Lebanon. Your household has been chosen by chance. Every adult in the country has a chance of being included in this study.

We would like tochoose an adult from your household to conduct the interview with them in private.

Q3Do you consent to allow your household to participate?	No	0	→DO NOT MARK SURVEY and Select another household
	Yes	1	

ENUMERATOR NOTE: The person must give his or her informed consent by answering positively. If participation is refused, walk away from the household and record this in the below table on "Reasons for Unsuccessful Visit." Substitute the household using the next numbered household to the right when facing the original household. If consent is secured, proceed to Respondent Selection.

Figure 3: Consent from head of household

1.2 Respondent Selection Procedure

***SCRIPT**: Thank you. Because this is a nationally representative survey, we need to randomly select one person in your household to interview. This person must be:*

- *A Lebanese citizen.*
- *A member of this household, which is a group of people that typically live and eat together and in most cases form a common decision-making unit.*
- *Between the ages of 18-65.*

To help me randomly sample a member of your household, please tell me:

Q4What is the total number of people in this household, defined as a group of people that typically live and eat together and in most-cases form a common-decision making unit?	_____
Q5How many people in your household fit the eligibility criteria (they are citizens and are between 18-65)?	_____
Q6How many of these are male (eligible males)?	_____
Q7 ENUMERATOR: Record the number of ELIGIBLE females here (Q5 minus Q6)	_____

Enumerator Note: Before continuing, be sure to check the target gender of the respondent before proceeding.	MALE	FEMALE

***SCRIPT**: As part of the representative sampling, we are interviewing equal numbers of men and women. We would like to randomly select a [MALE/FEMALE] in your household. To do this, we will randomly select a month out of the year and interview the [MALE/FEMALE] born in that month in your household.*

ENUMERATOR: Ask the member of the household to draw a random card from 1 to 12. Ask whether a [MALE/FEMALE] is born in that month. If yes, that is the person whom you should attempt to interview first. If no, then ask about each subsequent month. If more than one person is born in the same month, select the person whose birthday is earliest in the month.

The person I need to speak to is:			
Q8	Is this person presently at home?	0 Yes (→Q9)	1 No (→Q10)
Q9*If yes to* Q8	May I please interview this person now?	0 Yes (→Section 1.3)	1 No (→Q10)
Q10*If no to* Q8	Will this person return here at any time today?	0 Yes (→)	1 No (→)

If yes to Q10	Please tell this person that I will return for an interview at [insert convenient time]. *If this respondent is not present when you call back, randomly sample another male/female in the household and repeat* Q8-Q10.

If no to Q10	Thank you very much. I will select another [MALE/FEMALE] IN THE HOUSEHOLD. ENUMERATOR: Take the next [MALE/FEMALE] according to birthday order of the target gender. CompleteQ8-Q10 again. If there are no members of the target sex available, then go back to the script beforeQ8and complete the birthday sampling process again with members of the opposite sex using the same randomly selected birth month. If you conduct the interview with the opposite gender from what was assigned, you MUST record this in the Enumerator Questions at the end of the survey.

4

Figure 4: Sampling within household

1.3 Respondent Consent Script

[CHECK TO MAKE SURE THAT ALL OF THE FOLLOWING CONDITIONS ARE MET BEFORE CONTINUING WITH CONSENT]

Are you a member of this household?	0 No	1 Yes	If no: Take next respondent by birth date
Are you between the ages of 18-65?	0 No	1 Yes	If no: Take next respondent by birth date
Are you a citizen of Lebanon?	0 No	1 Yes	If no: Take next respondent by birth date
[DO NOT ASK] Is this individual of sound mind?	0 No	1 Yes	If no: Take next respondent by birth date

My name is _____. I am an enumerator from Information International, a survey firm working with independent researchers from the Lebanese Center for Policy Studies (LCPS).

We are conducting a study with 2400 households in Lebanon. Every adult in the country has a chance of being included, and you and your household have been chosen by chance.

The purpose of the study is to understand people's perceptions of economic, political, and sectarian issues in Lebanon, and we would like to discuss these issues with you. The data collected will be used for academic purposes and we are working independently from government, political parties or companies.

There are no direct benefits or risks in taking part in this study, but we hope that by contributing to this research you will be contributing to improving conditions in this country. There is no penalty for refusing to participate and you can refuse to answer any question if you want to or stop the interview at any time.

This interview will take about 60 minutes to complete and any answers you provide will be completely confidential and combined with the answers from all the other people we are talking to so that it will be impossible to single you out. There are no right answers, so please answer as truthfully as possible.

If you have any questions, I can give you a card containing the contact details of the Lebanese Center for Policy Studies.

Suzanne Massaad
Lebanese Center for Policy Studies
info@lcps-lebanon.org
+961 1 799301

Q11Do you consent to participate?	No	0	→Take the next [male/female] respondent according to birth date and repeat Q8-Q10 with the new respondent.
	Yes	1	

ENUMERATOR NOTE: Complete ONLY if there are one or more unsuccessful attempts to interview the respondent selected using the correct Respondent Selection Procedure:

Q12Reason for unsuccessful attempt to interview the selected respondent:	Resp 1	Resp 2	Resp 3	Resp 4	Resp 5
Head of Household refused to allow interview to be conducted with sampled respondent	1	1	1	1	1
Failed at least one of the consent "checks": (1) not a member of the HH; (2) not 18 to 65 years of age; (3) not a citizen of Lebanon; OR (4) not of "sound mind"	2	2	2	2	2
Not home and will not return any time today	3	3	3	3	3
Refused consent	4	4	4	4	4
Other	50	50	50	50	50

Q13Is this the sampling form corresponding to the person who was actually interviewed under this RESPONDENT ID?	0 No	1 Yes

If Yes to Q13	Enumerator Note: Make sure you staple THIS filled sampling packet to the front of the completed survey that you conducted with this sampled individual under this respondent ID.

5

Figure 5: Consent from main respondent

B.2 Randomization

We implemented the random assignment prior to sampling respondents themselves, following a slightly different procedure for medium/large versus small PSUs. First, within medium and large PSUs we block randomly assigned individuals to public and private treatment conditions. Randomization was implemented in Stata 13.0 on November 7-8, 2015 (we used the date as the randomization seed in YYMMDD format, so 151107). For the randomization we wanted to guarantee optimal balance (orthogonality) with the sex of the respondent given that we were also setting those targets for the survey. For instance, if there were 16 individuals in a medium PSU (block), we wanted to guarantee that eight would be randomly assigned to the private condition (of which four would be women and four would be men) and the other eight would be assigned to the public condition (of which four would be women and four would be men). To accomplish this, we first expanded the PSU level dataset into an individual level dataset and set the target treatment assignment combinations within each PSU (e.g. a target of four 'private' men, four 'private' women, four 'public' men and four 'public' women). Once all targets were prepared, we randomly generated a number (using the command *g master_rand = runiform(0,1)*) and sorted by the random number within each PSU. This determined the treatment combination for each respondent in the order that their household was sampled.

Table 1 provides an example of how the randomization was implemented for the medium-sized PSU of Bachoura in Beirut. Panel B shows how the targets were set for assignment to the private/public treatments (column 3, coded 1/2) and for female/male targets (column 4, coded 0/1). Column 5 shows the number randomly generated in Stata. The rightmost panel shows how respondent order is assigned after sorting on the random number within the PSU. Thus, in this example, the respondent in the first household sampled/visited within a PSU would be female and would receive the public petition. The respondent in the second household sampled/visited would be male and would get the private version, etc.[7]

Randomization was implemented slightly differently for small PSUs in that treatment was assigned at the PSU (cluster) level. This was done because there was some concern that respondents would be more likely to know each other in smaller geographic areas and would discuss the petition before the surveying process was completed. We were concerned that the credibility of the petition would be called into questions if respondents learned that some people had the option to sign in private while others did not. To assign randomly small PSUs to public and private conditions, we created new strata on the basis of Mohafazat (governorate) and majority confession and counted the number of PSUs already sampled in each new strata. If there was an odd number of PSUs we then randomly sampled one additional PSU such that were an even number of PSUs in each of these new strata.[8] We then block randomly assigned PSUs to public and private conditions within

[7] Note that if a household needed to be replaced because no one was home or consent was refused, the replacement household received the original household's treatment assignment.

[8] We followed this procedure because the decision to assign treatment at the cluster level in small PSUs was made relatively late in the implementation process and this caused minimal disruption to II's planning.

these strata.[9] We implemented the cluster level treatment assignment by assigning PSUs to private or public versions of the petition within their blocks before expanding the dataset to the individual level. We then set the gender targets after we expanded the dataset to the individual level. We still randomized the order in which households would be visited (and thus treatment*gender combinations implemented) in small PSUs. We note that because we assigned by cluster for some units we cluster standard errors at the level of the PSU in our analysis, which is a conservative approach (see Appendix D).

Prior to implementing the study we ran basic power calculations to determine the minimal effect size that we would be able to detect given our sample size and research design (assuming .80 power and a 95 percent confidence level). Making different assumptions (on variance, intra-cluster correlation, and the amount of variance explained by blocking) we determined that the smallest effect we would be able to detect would be in the range from .10 to .30 standard deviations (the SD was about .45 for the control group). Our main results indicate that statistical power was not a problem in this project.

[9]Overall treatment was assigned at the cluster level for 97 (of 193) PSUs (or 776 respondents).

LOCATION		RAND BEFORE SORTING			RAND AFTER SORTING			
district (1)	psu (2)	Public (3)	Sex (4)	Random (5)	Public (6)	Sex (7)	Random (8)	Order (9)
Beirut	Bachoura	1	0	0.42192	2	0	0.95123	1
Beirut	Bachoura	1	0	0.57987	1	1	0.94629	2
Beirut	Bachoura	1	0	0.80073	1	0	0.80073	3
Beirut	Bachoura	1	0	0.58571	2	1	0.77955	4
Beirut	Bachoura	1	1	0.50200	2	0	0.73748	5
Beirut	Bachoura	1	1	0.69410	2	1	0.70746	6
Beirut	Bachoura	1	1	0.94629	1	1	0.69410	7
Beirut	Bachoura	1	1	0.28673	1	0	0.58571	8
Beirut	Bachoura	2	0	0.08569	1	0	0.57987	9
Beirut	Bachoura	2	0	0.73748	1	1	0.50200	10
Beirut	Bachoura	2	0	0.35333	1	0	0.42192	11
Beirut	Bachoura	2	0	0.95123	2	1	0.39517	12
Beirut	Bachoura	2	1	0.70746	2	0	0.35333	13
Beirut	Bachoura	2	1	0.77955	1	1	0.28673	14
Beirut	Bachoura	2	1	0.39517	2	1	0.08649	15
Beirut	Bachoura	2	1	0.08649	2	0	0.08569	16

In columns 3 and 6, two (2) indicates assigned to the public condition and one (1) indicates assigned to the private condition.

Table 1: Randomization snapshot

C Balance Check and Controls

The survey contains a large number of pre-treatment covariates that can be used to check balance. Table 2 shows the results of the balance test. We check balance using 60 pre-treatment covariates specified in the pre-analysis plan.[10] While we check balance using the individual covariates, we also create indices for measures that capture a common underlying concept using inverse covariance weighting, as in Anderson (2008).[11] For instance, Table 2 shows that we have five different survey measures of sectarian-based prejudice, which we then use to create a 'prejudice' index. In Table 2 we present results of the balance tests for the individual covariates as well as for the indices, but note that if there is an imbalance in an index component there is likely to be imbalance in the index itself.

We test for balance by running an ordinary least squares regression of each covariate on the treatment assignment indicator, controlling for blocking strata fixed effects (see the first equation in Appendix D). The results of the randomization check are reported in Table 2 for the sample. The first column reports the mean in the private condition; the second column the coefficient on the treatment indicator; and the third column the p-value. As can be seen from the table, randomization was successful in achieving balance on pre-treatment controls for respondents in the public and private petition conditions. For the sample, only two of the 60 individual covariates are significant at the 95 percent confidence level in the sample, which is well within what we would expect to observe by chance. Also as expected, the coefficients are close to zero for each covariate.

[10] We add an indicator for whether a respondent was interviewed by a co-ethnic or not, based on the findings in (Adida et al., 2016).

[11] We pre-specified that we would use inverse covariance weighting to create all indices. Inverse covariance weighting assumes one latent trait of interest—which is consistent with how we pre-specified indices—and constructs an optimal weighted average by weighting-up index components that have lower covariance (and thus provide more 'new' information).

Table 2: Balance Check

	Sample		
	cont.	coeff.	pval
Panel A: Demographics			
Age	38.41	-0.63	0.236
Highest level of education completed (1-8)	4.71	0.00	0.991
Head of the household (0/1)	0.36	0.00	0.912
Married (0/1)	0.62	-0.01	0.707
Female (0/1)	0.50	-0.01	0.194
Maronite (0/1)	0.27	0.00	0.937
Shia (0/1)	0.24	0.02	0.064
Sunni (0/1)	0.26	0.00	0.807
Panel B: Sectarian Variables			
Strength of sectarian identity index (z-score)	-0.01	0.01	0.768
Strong sectarian identity (compared to Lebanese identity) (1-5)	2.04	0.06	0.109
Identify by sectarian group (1-4)	2.16	-0.04	0.297
Sectarian heterogeneity of social network (1-5)	3.08	-0.02	0.544
Sect-based prejudice index (z-score)	0.00	0.00	0.994
Not comfortable having non co-sect. neighbors (1-4)	1.83	-0.01	0.777
Not comfortable being supervised by non co-sect. (1-4)	1.92	0.01	0.732
Not comfortable having non co-sect. friends (1-4)	1.86	0.02	0.563
Not comfortable setting up a business with non co-sect. (1-4)	2.12	0.03	0.490
Not comfortable marrying non co-sect. (1-4)	2.96	-0.02	0.706
Benefits from sectarian system index (z-score)	0.01	-0.01	0.756
Sect. imp. for access to jobs/benefits for HH (1-4)	2.38	0.01	0.781
Sect. imp. for econ development of Lebanon (1-4)	2.37	-0.05	0.199
Sect. imp. for local development of your region (1-4)	2.39	-0.01	0.731
Sect. imp. for responsive/representative govt (1-4)	2.31	-0.02	0.590
Sect. imp. for protection of Lebanon from intl threats (1-4)	2.36	0.00	0.947
Sect. imp. for protection of sect from intl threats (1-4)	2.40	0.03	0.521
Sect. imp. for protection of sect within Lebanon (1-4)	2.47	0.04	0.335
Sect. imp. for stability/avoiding war (1-4)	2.41	0.03	0.450
Not difficult to get help from Zaim/politician (1-4)	2.00	0.02	0.646
Satisfied with MPs that represent sect (1-4)	2.17	-0.07	0.064
Thinks sectarian politics is important for some things (0/1)	0.70	0.01	0.619
Strength of Lebanese identity index (z-score)	0.00	-0.01	0.882
Strong Lebanese identity (compared to sect identity) (1-5)	3.18	0.01	0.523
Identify as Lebanese (1-5)	3.41	-0.02	0.384
Part of the majority sect in the PSU (0/1)	0.60	-0.02	0.197
Interviewed by a co-sectarian (0/1)	0.43	-0.02	0.357
Panel C: Economic Variables			
Class heterogeneity of social network (1-5)	3.14	-0.05	0.188
Attachment to economic identity index (z-score)	0.02	-0.03	0.378
Identifies by profession/occupation (1-4)	1.94	-0.04	0.311
Identifies by economic class (1-4)	1.85	-0.02	0.631
Upper-class economic status index (z-score)	0.01	-0.03	0.536
Total TVs owned	1.64	0.03	0.316
Total desktop computers owned	0.28	0.00	0.840

Continued on next page

Table 2: Balance Check

	Sample		
	cont.	coeff.	pval
Total mobile phones owned	3.06	0.06	0.327
Total laptop computers owned	0.68	-0.03	0.387
Total houses/apartments owned	1.01	-0.03	0.134
Total cars owned	1.40	-0.01	0.757
Household's total net income in a typical month (1-10)	5.43	0.04	0.327
Total income earners with white collar jobs (up to 2)	0.70	-0.03	0.278
Views household as upper-class	2.68	0.00	0.862
Cross-sect economic group distance index (z-score)	-0.05	0.10	0.006
Social distance: poor Christians to poor Muslims (1-7)	4.27	0.15	0.028
Social distance: rich Christians to rich Muslims (1-7)	4.72	0.19	0.004
Same-sect economic group distance index	-0.02	0.04	0.327
Social distance: poor Christians to rich Christians (1-7)	3.74	0.05	0.430
Social distance: poor Muslims to rich Muslims (1-7)	3.54	0.08	0.276
Panel D: Political Action Variables			
Political efficacy index (z-score)	0.01	-0.03	0.529
There are many legal ways for citizens to influence govt (1-4)	3.00	-0.04	0.287
Disagree there is no way to make public officials listen to citizens (1-4)	2.85	0.00	0.913
Political action index (z-score)	0.02	-0.03	0.302
Used social media to take action (0/1)	0.15	-0.01	0.352
Talked to party members/MPs/zaim (0/1)	0.10	-0.01	0.235
Signed a petition (0/1)	0.07	0.00	0.800
Attended a demonstration or protest march (0/1)	0.13	0.00	0.818
Joined an NGO that advocates for a policy issue (0/1)	0.05	-0.01	0.331
Cross-pressure index (z-score)	0.01	-0.02	0.546
Reluctant to act because it creates enemies (1-4)	2.55	-0.02	0.604
Reluctant to act because worry what people would think (1-4)	2.22	0.00	0.954
Reluctant to act because don't know where I stand on issues (1-4)	2.21	-0.04	0.344
Panel E: Fear of Social Sanctioning			
Fear of sanctioning index (z-score)	0.01	-0.02	0.521
Difficult to go against opinion of political leaders (1-4)	2.16	-0.04	0.247
Difficult to go against opinion of family, friends, neighbors (1-4)	2.30	-0.01	0.774
Difficult to go against opinion of sectarian community (1-4)	2.33	0.02	0.595

D Estimation

To identify the effects of public disclosure in the petition experiment we estimate:

$$Y_{ij} = \alpha + \beta T_{ij} + \gamma_j + \epsilon_{ij}$$

where Y_{ij} is the binary outcome for returning a completed petition for individual i in sampling and blocking strata j, β is the coefficient on the binary treatment indicator, γ_j is strata fixed effects and ϵ_{ij} is the individual error term. Errors are clustered at the level of primary sampling unit. We cluster standard errors at this level because treatment was assigned at the cluster (PSU) level rather than the individual level within 'small' strata only, as described in Appendix B.1. By clustering all standard errors at the PSU level we are taking a conservative approach. We also check the robustness of all results to the inclusion of a vector of pre-treatment controls X'_{ij} (as specified in Appendix C).

For heterogeneous treatment effects we estimate:

$$Y_{ij} = \alpha + \beta_1 T_{ij} + \beta_2 C_{ij} + \beta_3 T_{ij} * C_{ij} + \gamma_j + \epsilon_{ij}$$

where C_{ij} refers to a binary social cost indicator and β_3 is the coefficient on the interaction between the treatment and cost indicator. For the heterogeneous effects analysis, we are primarily interested in the coefficient β_3, which tells us whether the effects of public disclosure vary significantly for the subgroups defined by C_{ij}. As above, in the main text and appendices we check the robustness of all results to the inclusion of a vector or pre-treatment controls X'_{ij} (that excludes C_{ij}).

Before estimating results, we take the (pre-registered) step of correcting for a small amount of item non-response by performing 10 rounds of multiple imputation using all pre-treatment covariates from the survey, with imputations produced via predictive mean matching in the ice package in Stata (Royston, 2004). This is mostly relevant for our regressions with controls where a small amount of missingness can nonetheless lead dropping a significant number of observations from the analysis.

E Extended Discussion of Inferential Risks and Results

E.1 Inferential risks

We consider two potential inferential risks associated with our experimental design. One possible risk is that treatment effects could be due to different *effort* costs rather than *social* costs. This could be the case because individuals in the public condition had to fill out one more line on the petition than individuals in the private condition. While it is possible that this entailed additional—albeit likely very minimal—effort, we were more concerned that the potential solutions to address this posed a bigger risk to inference than the effort differential itself. For instance, one option would have been to have everyone sign their names but inform a random half of the sample that their names would not actually be publicly revealed. We were concerned, however, that this promise would not seem credible and would undermine the confidentiality of the private condition that we were emphasizing. Another option would have been to use a placebo line on the petition, such as sex, education level or sector of employment, but we were concerned that this might have other effects that would be hard to observe (for instance perhaps those with low education or who were unemployed would feel ashamed and that would make them less willing to sign in private). In the end, while we cannot exclude the possibility that there was some effort differential, the act of signing one's name in practice requires minimal effort. We note, however, that such effort costs might be one reason we still observe a negative effect of public disclosure on participation for 'low cost' individuals in the heterogeneous effects analysis. The fact that public disclosure has a significantly bigger effect for those with greater fears of social sanctioning supports the interpretation that social costs are nonetheless playing a role.

We also note that there is little possibility that the effects of public exposure are due to potential political costs. In Lebanon there is little risk of more serious political consequences from petition signing (such as arrest or other forms of political oppression). Lebanon is a strong democracy, as indicated by its score of six in the Polity IV dataset (where scores range from -10 to 10 and 10 indicates full democracy). Turnout in the last parliamentary elections (in 2009) was estimated at about 55 percent.[12] Freedom of the press is relatively strong and protest is not uncommon. Petition-signing also has a long history in Lebanon and petitions have been a common form of political action dating back to the early 20th century under the French Mandate period (Darling, 2013, 186). Among other causes, Lebanese citizens have used petitions to demand social and political change (Hanafi and Arvanitis, 2016, 293-294), to demand democratic elections (Karam, 2005), to build support for the adoption of a civil marriage law, (Karam, 2005), to advocate for the civil and economic rights of various groups (Bianchi, 2014), and to protest unsustainable or otherwise undesirable infrastructure projects (Meraaby, 2017). Our piloting indicated that people did not believe that there were political repercussions to petition-signing.

Another possible inferential risk arising from the design is that signing in the private condition does not reflect the true level of private support for sectarian system reform. For instance, it could

[12] http://www.nytimes.com/2009/06/08/world/middleeast/08lebanon.html

be that signing in the private condition over-estimates the true level of private support. This would be the case if, for example, respondents do not truly support reform but felt compelled to sign the petition due to social desirability bias (perhaps related to enumerator observation). It was precisely for this reason that we aimed to minimize social desirability bias by making sure that the enumerator was not present to observe the respondent's decision to sign the petition. Alternatively, it could be that signing the private petition *under*-estimates support for sectarian system reform. This would occur, for example, if an individual truly supported reform but remained worried about the confidentiality of that decision in the private condition despite our assurances. Signing in the private condition could also under-estimate support for reform if an individual truly wanted change but simply did not obtain expressive benefits from taking political action.

Our approach in addressing this concern was to design and implement the experiment in a way that would minimize the influence of factors—like social desirability bias and confidentiality concerns—that could cause private political behavior to diverge from true private beliefs. Another possible option would have been to use a list experiment or other indirect question technique to try to obtain a 'baseline' measure of true support for sectarian system reform. We chose not to do this, however, because it was difficult to construct a survey measure that could be compared to private petition behavior. Say, for instance, we used a list experiment that asked "How many of the following issues to you support" where the sensitive item was "reforming the sectarian political system." If support for the issue appeared to be greater as measured by the list experiment than by private petition signing it would be hard to know if this was because the list experiment was better capturing true beliefs; was simply more vague and thus less controversial than the actual text of the petition; or was not accounting for willingness to take political action to express that belief (which the private petition does capture). We also considered a list experiment question along the lines of: "If you were given the opportunity to sign petitions on the following issues, how many would you sign..." where the sensitive item was "reforming the sectarian political system." Formulated in this way, we again would not know if differences were due to vaguer language or over-estimations of willingness to take political action. Indeed, list experiments that ask respondents to report behavior that they *would* take are subject to the same concerns about stated versus revealed preference measures that commonly dog surveys. It is for these reasons that we felt that we opted not to use a list experiment to obtain a baseline measure of true support for the types of reform advocated by the petition.

We also note that while there is some risk that private petition behavior does not reflect 'true' beliefs, this does not necessarily bias our treatment effect estimates, which is our main interest in this paper. The treatment effect estimates would be biased only if the factor driving over- or under-estimation of true private support has a differential effect on behavior in the private and public conditions. If behavior were affected by social desirability bias—for instance due to enumerator observation—these effects would plausibly be similar in private and public conditions. If, however, respondents were concerned about violations of confidentiality in the private condition, then this could have had the effect of driving down signing in the private condition and reducing

the size of the treatment effects (since a private treatment without confidentiality is essentially a public treatment). This speaks to why we went to great lengths to reassure participants about the confidentiality of their decision in the private condition.

E.2 Understanding the high levels of petition signing

The levels of petition signing in both private and public conditions are high, which might raise concerns about the credibility (and external validity) of the decision to sign the petition. One possible explanation for the high level of signing is simply that we conducted the project at a time of rare political mobilization in Lebanon in which anti-sectarian sentiments and eagerness to take political action were particularly high.[13] As such, the high levels of signing could reflect genuine willingness to take political action to demand reform to the status quo.[14]

The concern arises, however, that the high levels of signing are not a genuine reflection of these sentiments but rather an artifact of the research design. In this Appendix we discuss three possible explanations—experimental design, invitation method, and sample selection—for the high levels of signing. We underscore in the discussion below that some of these explanations pertain to our estimates of support for sectarian system reform but not our estimates of the treatment effects, which is our main interest in this paper. Since it is difficult for us to adjudicate among these explanations, we discuss them transparently here so that readers can draw their own conclusions.

One possibility is that the high levels of signing are an artifact of the experimental design and that signing (either publicly or privately) does not reflect the true level of private support for sectarian system reform. We address this possibility above in our discussion of inferential risks (see Appendix E.1), noting that we took steps to mitigate the possibility that high signing levels reflect an over-estimation of the true support for sectarian system reform.

A second possible explanation for the high level of signing is that we had a 'captive audience' in that we invited individuals to sign one-on-one at the end of a survey. Indeed, there is evidence that face-to-face methods of mobilization are more effective than indirect methods (Gerber and Green, 2000). It could certainly be the case that overall petition signing rates would have been lower in both private and public conditions had individuals been invited to sign by another method, for instance by an email and a link to an online petition. We note that while the personal method of invitation might have resulted in high levels of signing, it does not bias the treatment effect

[13] In June 2017, partly in response to growing civil society and public pressure over sectarianism, Lebanon's government passed a long awaited electoral reform to introduce proportional representation and scheduled much-delayed parliamentary elections for May 2018. See `http://www.newyorker.com/news/news-desk/is-lebanons-new-electoral-system-a-path-out-of-sectarianism`.

[14] There is little available data on the number of petition signatories as a share of all those approached to sign so it is difficult to assess signing levels in other comparable petitions in Lebanon or elsewhere. We note, however, that petitions are a common form of political action in Lebanon and that they frequently get large numbers of signatories. For instance, one online petition sponsored by Animals Lebanon (to shut down a circus accused of animal abuse) obtained 12,600 signatures within the first two months (Melki and Mallat, 2014). In a study conducted outside of Lebanon, Ratner and Miller (2001) find that 86 percent of their sample was willing to sign a petition opposing Proposition 174 (on school vouchers) in California. While this study also shows a high level of signing, it is difficult to compare to ours given the differences in contexts.

estimates since the invitation method was the same in public and private conditions. We also note that this explanation does not imply that we have biased estimates of the true level of support for sectarian system reform. Rather, this explanation suggests that there are a large number of individuals genuinely support reform of the status quo and that the method of invitation succeeded in mobilizing them to express their preferences.

A third possible explanation for the high level of signing is sample selection. If those who were interested and engaged in politics were more likely to select into the sample then overall levels of petition-signing could be exceptionally high. Sample selection is a concern in this study because, as discussed below in Appendix H.1, approximately 60 percent of those initially sampled to take the survey refused to participate. In Appendix H we go to lengths to try to correct for sample selection and generalize results to the population using different weighting methods. As can be seen in Appendix H.3, however, we observe high levels of signing in the population estimates as well. The limitation of post-survey weighting adjustments is that we can only re-weight the sample based on observable, typically demographic, characteristics common to both the reference dataset and our survey. Thus, we can only account for unobservable characteristics like political interest to the extent that they correlate with these observed measures. In other words, we cannot say with confidence that the population estimates correct for possible determinants of sample selection.

We therefore also implement a simple bounding exercise to assess how sample selection might affect our estimates. Let us assume that we somehow persuaded the 60 percent who refused consent to participate in the survey itself, expanding our dataset to n=3994. Let us also assume that all of those who originally refused to participate were politically disinterested types such that *none* of them chose to sign the petition, regardless of their random assignment to the public or private condition. The results suggest that approximately 44 percent of the sample would have signed in the private condition compared to 31 percent in the public condition—still a 13 percentage point difference. We could consider these estimates lower bounds on overall levels of petition-signing in light of potentially un-addressed concerns about selection into the sample.

F Robustness of Results to Controls

We check the robustness of our main results to the inclusion of the full set of controls described in Appendix C. Including controls is particularly important for the heterogeneous effects analysis because of the observational nature of the conditioning variables. While we cannot be certain that different types of social pressure *cause* the differences in the effects of public disclosure, we get one step closer to understanding the association between these variables and the treatment by controlling for potential confounders. The results in Table 3 are virtually identical to those presented in the main text.

	Main treatment effect	Conditional effects: Fear of Sanctioning			
		Index	By elites	By fam	By commun.
	(1)	(2)	(3)	(4)	(5)
Public petition (versus private)	-0.19***	-0.15***	-0.16***	-0.13***	-0.15***
	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)
Public X Fear sanctioning (index)		-0.08*			
		(0.04)			
Public X Fear elite sanctioning			-0.09*		
			(0.04)		
Public X Fear fam sanctioning				-0.15***	
				(0.04)	
Public X Fear commun. sanctioning					-0.09*
					(0.04)
Fear sanctioning (index)	-0.03*	0.00			
	(0.01)	(0.03)			
Fear elite sanctioning			0.03	-0.01	-0.01
			(0.03)	(0.02)	(0.02)
Fear fam sanctioning			-0.02	0.06*	-0.02
			(0.03)	(0.03)	(0.03)
Fear commun. sanctioning			-0.02	-0.02	0.03
			(0.03)	(0.03)	(0.03)
Constant (signed in private)	0.89	0.90	0.90	0.88	0.89
	(0.09)	(0.09)	(0.09)	(0.09)	(0.09)
N	2496	2496	2496	2496	2496

Notes: ** $p < .05$, ** $p < .01$, *** $p < .001$. P-values are based on a two-tailed test. Clustered standard errors are in parentheses; all results control for block fixed effects.*

Table 3: Effects of public disclosure on petition-signing (with full set of controls)

G Heterogeneous effects

G.1 Fear of social sanctioning (and network homogeneity

)

This paper tests the hypothesis that the effect of public disclosure will be greater for those with more fear of social sanctioning. The pre-registered hypothesis (H3c) was that the divergence between public and private preferences would be greatest for those who "privately dislike sectarianism and are fearful of social sanctioning." We included this hypothesis to confirm that the effect of public disclosure varies for individuals who are more or less susceptible to public pressure.

The pre-analysis plan also specifies that we will test this hypothesis using the three measures described in the main text, namely the pre-treatment survey questions: "How difficult would it be to do something that you wanted to do that did not align with the opinions of [your sectarian or political leader/your family, friends or neighbors/or your confessional community]?"[15] We pre-registered that we would create a 'fear of social sanctioning' index using inverse covariance weighting (Anderson, 2008).[16] In our discussion of data cleaning and preparation in the pre-analysis plan (Section 6.2) we also register our choice to use a binary version of the index (cut at the median) and binary versions of component variables (cut at the midpoint of the scale) in heterogeneous effects analysis for ease of interpretation. In the pre-analysis plan we also discuss the importance of investigating how different sources of social pressure—elites, immediate social network, and broader community—condition the effects of public disclosure. We do not have specific hypotheses related to these, however, and thus treat this analysis as exploratory. Table 4 provides descriptive statistics for the sample using the binary versions of the index components.

	mean	*SD*	*min*	*max*	*% missing*
Social Pressure					
Leaders	0.39	0.49	0	1	0.07
Friends, family	0.44	0.50	0	1	0.01
Community	0.51	0.50	0	1	0.01

Notes:

Table 4: Descriptive statistics for variables used in heterogeneous effects analysis related to social pressure

The results also provide suggestive evidence that fear of sanctioning by friends, family and neighbors has a bigger effect on reducing participation in the public treatment condition than other sources of social pressure. We test the equality of coefficients using the following Z test (Paternoster et al., 1998):

[15] Responses were recorded on a four-point Likert scale.

[16] Inverse covariance weighting assumes one latent trait of interest—which is consistent with how we pre-specified indices—and constructs an optimal weighted average by weighting-up index components that have lower covariance (and thus provide more 'new' information). The results are almost identical when using a mean effects index instead.

$$Z = \frac{\beta_1 - \beta_2}{\sqrt{(SE\beta_1)^2 + (SE\beta_2)^2}} \quad (1)$$

where SEβ is the standard error of β. Testing the equality of coefficients for the interaction on *Public*Fear of sanctioning by family* and *Public*Fear of sanctioning by community*, we obtain $Z = -.88$. This implies that the one-sided p-value on the test of the equality of coefficients is about $p = .1894$ (the two-sided p-value is about .3788), meaning that we cannot reject the null of no difference with a conventional level of confidence. We obtain a similar result when testing the equality of coefficients for the interaction on *Public*Fear of sanctioning by family* and *Public*Fear of sanctioning by leaders*.

We perform an additional hypothesis test related to social pressure. In the pre-analysis plan we also hypothesize that the divergence between public and private preferences will be greatest for those who "privately dislike sectarianism and are in homogeneous sectarian networks."[17] Whereas the analysis in the main text pertains to how fear of social sanctioning by one's sectarian community conditions the effect of public disclosure, this hypothesis considers how the proportion of co-sectarians in one's social network conditions the effect of public disclosure. We anticipated that the effect of public disclosure on reducing participation would be greater for those with more homogeneous sectarian social networks. We allow, however, for the possibility that public disclosure might result in *more* participation in public than in private for individuals in heterogeneous sectarian communities, which we would interpret as evidence that individuals privately support sectarianism but feel pressured by their 'cosmopolitan' social environment to take a stand against it. In retrospect, however, we do not think that this experiment was well designed to test this latter possibility (which is also why we do not focus on it in the paper). A good test would require making it observable to an individual's social network that they had an opportunity to sign but chose not to such that *in*action might have social costs.

Nevertheless, we ran this hypothesis test and present the results here. We test H3d using the (registered) pre-treatment survey question: "Of your closest friends and acquaintances, the people that you interact with on a regular basis, how many of them belong to a different confession than you." Responses were recorded on a five-point scale ranging from 1 = almost all to 5 = almost none. As pre-registered, we dichotomize this variable such that 1 equals those whose who have more homogeneous networks (those who say that 'a few', 'almost none', or only 'some' of their network belongs to another sectarian group) while we code as zero those who have more heterogeneous networks (those who say that 'most' or 'almost all' of their network consists of non co-sectarians). Interestingly, the correlation between the binary measure of homogeneous networks and our binary measures of fear of sanctioning by elites, family, and broader community is relatively low (.20, .16, and .17, respectively). This indicates a relatively low association between those who have more homogeneous networks and those who are relatively more afraid of social sanctioning.

[17] We primarily intended to test this hypothesis with respect to a different experiment but it is also relevant to this paper and we thus discuss it here for completeness. Specifically, we conducted a separate experiment on public/private Facebook discussion groups, and we pre-registered this hypothesis (H3d) as primarily relevant to that experiment.

Table 5 presents results for how the effect of public disclosure varies for those with homogeneous versus heterogeneous sectarian social networks. The results using the binary version of the variable suggest that the sectarian homogeneity of one's social network does *not* condition the effect of public disclosure. In other words, public disclosure reduces participation regardless of the type of social network an individual has. These findings are similar whether we look at results with or without controls.

This result is puzzling given the strong support we find for the 'fear of social sanctioning' results. We therefore dig deeper to explore how the marginal effects of public disclosure vary for the five different levels of homogeneity following the original variable coding. The result in Table 6 are more consistent with the expected pattern. For example, looking at the results with no controls, we see that public disclosure caused a 10 percentage point decrease in participation for those who say that almost all of their friends are non co-sectarians. For those whose networks have moderate levels of homogeneity, public disclosure caused about an 18-21 percentage point decline in participation. For respondents in very homogeneous networks—where almost none of their friends belong to different sectarian groups—the effect of public disclosure was 32 percentage points. The results are very similar when including the full suite of controls. Overall, these findings support the notion that the effect of public disclosure on reducing participation is greatest for those who face higher potential social costs.

	OLS without full set of controls	OLS with full set of controls
Public petition	-0.19***	-0.19***
	(0.02)	(0.02)
Public * Homog. network	-0.02	-0.02
	(0.04)	(0.04)
Homog network	-0.02	0.04
	(0.03)	(0.03)
Constant	0.71	0.87
N	2496	2496
Block fixed effects	Yes	Yes

Notes: * $p < .05$, ** $p < .01$, *** $p < .001$
P-values based on a two-tailed test.
Clustered standard errors in parentheses.

Table 5: Effects of public disclosure on petition signing conditional on the sectarian homogeneity of social networks

	OLS without full set of controls		OLS with full set of controls	
Almost all	-0.11	(0.09)	-0.10	(0.08)
Most	-0.19***	(0.04)	-0.18***	(0.04)
Some	-0.22***	(0.04)	-0.21***	(0.04)
A few	-0.18***	(0.05)	-0.18***	(0.05)
Almost none	-0.32***	(0.07)	-0.30***	(0.07)
N	2496		2496	
Block fixed effects	Yes		Yes	

Notes: * $p < .05$, ** $p < .01$, *** $p < .001$
P-values based on a two-tailed test.
Clustered standard errors in parentheses.

Table 6: Marginal effects of public disclosure conditional on the number of people in a respondent's social network who are co-sectarians

G.2 Social groups

It is likely that the costs of public disclosure also vary for different social groups. To examine this we pre-registered two additional hypotheses: that the effect of public disclosure will be greatest for those who privately dislike sectarianism and are "socially or economically vulnerable (women, the poor)" (H3b) and who are "in sectarian groups with stronger norms of intra-group punishment for inter-group cooperation" (H3e). To test these hypotheses we examine how the effect of public disclosure varies for women, the poor, and members of different sectarian groups. We note that understanding how the effects of making signing public vary by economic status and sectarian group is particularly relevant given the nature of the petition.

Table 7 presents descriptive statistics for the sample for the variables used to test these heterogeneous effects. To examine heterogeneous effects by economic status we first construct an index of wealth. We use a total of nine variables in constructing the index, including six variables that measure asset ownership (television, desktop computer, phone, laptop, house, and car). We also include a measure of income where respondents were asked to select a household income range from a scale of 10 ranges (for details, see the survey instrument, available from the authors). We also include a measure of the number of heads of household (up to two) who work in a white-collar sector. Finally, we asked respondents about their perceived social class, with answers coded on a five-point scale from extremely poor to very wealthy. We aggregate these variables into an index using inverse covariance weighting (Anderson, 2008). We create terciles of this index and code as poor those individuals who fall in the bottom tercile. With respect to sect, Lebanon has three main sectarian groups—Maronite Christians, Sunni Muslims, and Shia Muslims.

	Sample				
	mean	*SD*	*min*	*max*	*% missing*
Panel A: Gender					
Female	0.49	0.50	0	1	0.00
Panel B: Economic Class					
Total TVs owned	1.66	0.68	1	4	0.00
Total desktop computers owned	0.28	0.48	0	2	0.00
Total mobile phones owned	3.09	1.42	1	10	0.00
Total laptop computers owned	0.67	0.73	0	5	0.00
Total houses/apartments owned	1.00	0.42	0	5	0.00
Total cars owned	1.40	0.79	0	9	0.00
HH's total net income in a typical month	5.43	1.00	2	10	0.08
Total HH heads with white collar jobs (up to 2)	0.68	0.63	0	2	0.00
Views household as upper-class	2.68	0.57	1	4	0.01
Panel C: Sectarian Group					
Maronite Christian	0.27	0.45	0	1	0.01
Sunni Muslim	0.26	0.44	0	1	0.01
Shia Muslim	0.26	0.44	0	1	0.01

Notes: *The table presents summary statistics for the sample for the variables used in heterogeneous effects analysis in the main text. Panel B provides details on the variables used in the creation of the wealth index. We aggregate nine measures into a summary index using inverse covariance weighting. The binary measure of lower income used in the main analysis is obtained from dividing this index into terciles and coding those in the lowest tercile as lower-income=1, and 0 otherwise.*

Table 7: Descriptive statistics for variables used in heterogeneous effects by social groups analysis

Table 8 shows the results for how public disclosure varies for different social groups (presenting results for the sample and population, with and without controls). First, we find little evidence of a differential effect of public disclosure for men and women. The coefficient on the interaction of $Public * Female$ is close to zero and statistically insignificant. This is notable because, while there is some evidence that women prefer public goods to clientelism (Wantchekon, 2003), both men and women in this context have similar private preferences and face similar social costs to expressing them.

In contrast, there is some evidence that public disclosure had a bigger effect on reducing participation for lower income groups compared to upper income ones. As can be seen in columns two and seven, public disclosure reduced participation by a further eight percentage points for the poor as compared to the rich—a result that is statistically significant when including the full set of controls. Interestingly, while we might expect the poor would support sectarian politics more because they benefit the most from ethnic-based clientelism, the results in the private condition show that this is not the case in this context. Importantly, these findings indicate that the effects of public disclosure are greatest for a group that could benefit the most from system reform but that is also the most vulnerable to getting cut off from sectarian benefits.

Finally, we look at the effects of public disclosure on members of Lebanon's three main sectarian groups. It is difficult to anticipate *ex ante* the sectarian group for which there might be the greatest social costs of public participation in the petition because of the complicated history of each sectarian group to the sectarian political system overall. The Constitution of 1926 created a parliamentary system with proportional representation along sectarian lines and Article 95 established the sharing of state offices. Following independence in 1943, Christian Maronites and Sunni Muslims struck a National Pact that further enshrined the notion of power-sharing among confessions. The office of the President was reserved for a Christian while the office of Prime Minister was reserved for a Sunni (later it was agreed that the position of Speaker would go to a Shia Muslim). At the time of independence, Christians were the largest and highest status group, followed by Sunnis. Shia constituted a much smaller and more economically disadvantaged group. Since then, the size of the Maronite population has declined while the Shia population share has increased dramatically. Shifting demographics and sectarian conflict contributed to civil war, which that lasted from 1975-1990. The 1989 Ta'if Agreement brought an end to Lebanon's civil war and split representation equally between Christian and Muslim populations. This is the balance that remains to this day despite the relative increase in the size of the Muslim population.

In investigating variation by sectarian group, we were particularly interested in whether public disclosure has a bigger effect on Maronite Christians because they stand to lose the most from sectarian system reform.[18] The results in Table 8 are revealing. We see that, in general, Maronites are 11 percentage points less likely to sign the petition at all (column 3). This is consistent with the notion that system reform would potentially be more harmful to Christian interests. The effects

[18] Maronites retain 50 percent of the seats in the legislature and the council of ministers despite being only 25-30 percent of the population.

of public disclosure, however, seem to be greatest for Sunnis. The effect of public disclosure on reducing participation was 14 percentage points greater for Sunnis than for other sectarian groups and this result is statistically significant. One possible explanation for this pattern is that it is, in fact, Sunnis who are experiencing the greatest threat to their sectarian status at present due to a number of changes in recent years, including a collapse in support from Saudi Arabia, a weakening sectarian party, and a rise in Shia strength (Salloukh et al., 2015). Our results are consistent with a story in which many Sunnis might privately be disaffected with the status quo but remain reluctant to admit so publicly.

	OLS without full set of controls					OLS with full set of controls				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Public petition	-0.21***	-0.18***	-0.20***	-0.17***	-0.21***	-0.21***	-0.17***	-0.20***	-0.16***	-0.20***
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Public * Female	0.02					0.02				
	(0.03)					(0.03)				
Public * Poor		-0.08					-0.08*			
		(0.04)					(0.04)			
Public * Maronite			0.01					0.01		
			(0.05)					(0.04)		
Public * Sunni				-0.14**					-0.13**	
				(0.05)					(0.05)	
Public * Shia					0.02					0.02
					(0.06)					(0.05)
Female	-0.04					-0.02	-0.01	-0.01	-0.01	-0.01
	(0.02)					(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Poor		-0.03				-0.04*	0.00	-0.04*	-0.04	-0.04*
		(0.02)				(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Maronite			-0.11*			-0.09*	-0.09*	-0.10	-0.10*	-0.09*
			(0.05)			(0.04)	(0.04)	(0.05)	(0.04)	(0.04)
Sunni				0.10		-0.01	0.00	-0.01	0.06	-0.01
				(0.05)		(0.06)	(0.06)	(0.06)	(0.06)	(0.06)
Shia					0.02	-0.04	-0.03	-0.04	-0.05	-0.05
					(0.07)	(0.07)	(0.07)	(0.07)	(0.07)	(0.08)
Constant	0.72	0.71	0.73	0.68	0.70	0.91	0.89	0.91	0.90	0.91
N	2496	2496	2496	2496	2496	2496	2496	2496	2496	2496
Block fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table 8: How the effect of public petition disclosure varies by social group

H Results for the Population

H.1 Population weights

Since assignment to public and private petition conditions was done randomly we can obtain unbiased estimates of the treatment effect for the *sample* by comparing outcomes for respondents in the treatment group to outcomes for respondents in the control condition, as reported in the main text. We are also substantively interested, however, in assessing underlying support for sectarian system reform in the population, which requires weighting the sample to reflect the population. This is especially important in our case given that approximately 60 percent of the sampled households or respondents refused to take part in the survey, which we attribute to the length of the survey and the sensitivity of some of the subject matter. While this is still a better response rate than in many public opinion surveys in other countries, it nevertheless suggests that our sample could be systematically different from the population.[19]

One option for weighting would involve using our design weights, specifically weighting by the inverse of the probability of selection (the probability a PSU was selected within a strata $\times$ the probability a household was selected within a PSU $\times$ the probability that an individual was selected within a household). There are two challenges with this approach, however. First, as described in Appendix B, it is very hard to obtain accurate information on the number of households within a PSU or neighborhood because of the population density in many urban PSUs and because reliable official information on populations is not maintained in Lebanon (there has been no official census in Lebanon since 1932). While we construct probabilities based on the PSU population and estimated household size using our data, these estimates should be treated with caution. Second, these design weights do not account for potential selection into the sample due to the high unit non-response rate.

A common procedure in such cases is to use post-survey weight adjustments—such as post-stratification weighting or calibration/raking—to generalize from the sample to the population (Franco et al., 2016). Regardless of the method used, post-survey weight adjustments aim to bring the sample closer to known population totals. The key to post-survey weighting is the existence of a high-quality reference dataset, such as a census or general population survey, with population totals for observable (typically demographic) characteristics. As discussed above, in the case of Lebanon, no census has been conducted since 1932 and (to our knowledge) there are few general population surveys on which to draw. We decided to use data from the Arab Barometer III survey (implemented in Lebanon in July 2013) as our reference. The Arab Barometer III is a nationally representative survey that is widely used by academic and policy researchers to draw population inferences.[20] We note that the Arab Barometer population estimates are based on a survey with a

[19] For instance, Rosenfeld, Imai and Shapiro (2016) report that the American Association for Public Opinion Research's Response Rate was 5.3 percent. Response rates for Pew Research Polls typically range from five to 15 percent in the United States. See `http://www.pewresearch.org/methodology/u-s-survey-research/our-survey-methodology-in-detail/`.

[20] For details, see `http://www.arabbarometer.org/`.

sample size of 1200 and for which there is also some non-negligible item non-response (as with age or income information). This suggests that these population estimates should thus also be treated with some caution, but in taking this step we aim to bring our sample closer to the distribution of demographic characteristics from a well-known and widely used data source.

In generalizing from our sample to the population we employ three different versions of the population weights although we only report results using the entropy balancing weights in the main text. First, we estimate results using our original design weights. Second, we implement a traditional raking method using the *ipfraking* command in Stata and the Arab Barometer III data as the reference dataset. In order to get the raking procedure to run, we had to use only a small set of variables common to both datasets, namely gender, education level, employment status, and sect. Finally, we use entropy balancing to construct post-survey weights. Entropy balancing is increasingly used as a reweighting method to achieve covariate balance between 'treatment' and 'control' groups in observational studies (Hainmueller, 2012). As discussed by Hainmueller (2012), however, entropy balancing can also be used to adjust survey design weights so that sample moments match the known population moments (see also Hainmueller and Xu, 2013; Hartman et al., 2015.) Entropy balancing is comparable to other traditional post-survey weighting schemes, including post-stratification and calibration/raking. The advantages of entropy balancing include the ability to incorporate survey design weights as base weights and to obtain exact balance between sample and population moments for up to three moments and for a large number of covariates (Watson and Elliot, 2016). We balance on the first moment (the mean) using the *ebalance* command in Stata. We prefer the entropy balancing weights because they allow us to incorporate a larger number of covariates but we show below that the results are consistent across all weighting approaches.

Table 9 provides descriptive statistics for our sample (Panel A) and the population using our design weights (Panel B). Panel C shows the population estimates from the Arab Barometer survey. The rows in the table show the full set of comparable variables across our survey and the Arab Barometer survey that we were able to use to generate the entropy balancing weights. Comparing our sample to the population estimates from Arab Barometer in Panel C, we see that respondents in our sample have a higher education and higher levels of unemployment than the Arab Barometer population estimates. These differences are consistent with the high levels of unit non-response in our study. Panels D and E show our estimates of population parameters after post-survey weighting our data using raked weights and entropy balancing weights, respectively.[21]

[21] Before implementing entropy balancing we correct for a small amount of item non-response by performing 10 rounds of multiple imputation using all pre-treatment covariates from the survey, with imputations produced via predictive mean matching in the ice package in Stata (Royston, 2004).

	Panel A Summary stats for the sample		Panel B Sum. stats (pop) Design weights		Panel C Arab Barometer Population estimates			Panel D Sum. stats (pop) Raked weights		Panel E Sum. stats (pop) Entropy weights	
	mean	*SD*	*mean*	*SD*	*mean*	*SD*	*N*	*mean*	*SD*	*mean*	*SD*
Region											
Beirut	0.05	0.22	0.09	0.28	0.11	0.31	1200	0.09	0.29	0.11	0.31
Mout Lebanon	0.26	0.44	0.39	0.49	0.39	0.49	1200	0.36	0.48	0.39	0.49
North	0.22	0.42	0.22	0.41	0.22	0.41	1200	0.21	0.41	0.21	0.41
Beqaa	0.20	0.40	0.14	0.35	0.12	0.33	1200	0.15	0.36	0.12	0.33
South	0.13	0.34	0.11	0.32	0.11	0.31	1200	0.13	0.34	0.11	0.31
Nabatieh	0.13	0.34	0.05	0.21	0.06	0.24	1200	0.06	0.24	0.06	0.24
Demographics											
Age (18-66)	38	14	38	15	39	14	1117	40	14	39	14
Female	0.49	0.50	0.48	0.50	0.51	0.50	1200	0.51	0.50	0.51	0.50
Secondary education (=1)	0.64	0.48	0.65	0.48	0.52	0.50	1200	0.52	0.50	0.52	0.50
Married (=1)	0.62	0.49	0.54	0.50	0.59	0.49	1200	0.60	0.49	0.59	0.49
Confession											
Sunni	0.26	0.44	0.32	0.46	0.27	0.45	1200	0.27	0.45	0.27	0.44
Shia	0.26	0.44	0.21	0.41	0.27	0.44	1200	0.27	0.44	0.27	0.44
Maronite Christian	0.27	0.45	0.31	0.46	0.25	0.44	1200	0.25	0.44	0.26	0.44
Druze	0.10	0.29	0.07	0.25	0.08	0.27	1200	0.08	0.27	0.08	0.27
Employment and wealth											
Income (scale 1-15)	5.43	1.00	5.57	1.01	4.89	1.07	864	5.59	0.98	4.90	0.95
Employed	0.48	0.50	0.47	0.50	0.65	0.48	1196	0.65	0.48	0.65	0.48
HH own computer	0.62	0.48	0.68	0.47	0.78	0.41	1200	0.66	0.47	0.78	0.41
HH owns a car	0.92	0.26	0.93	0.26	0.69	0.46	1200	0.92	0.27	0.69	0.46

Notes: *The first two panels present summary statistics for the sample (Panel A) and for the population using our design weights (Panel B). Panel C provides summary statistics for the population using the Arab Barometer III data for Lebanon. The final two panels show summary statistics for the population after using raked weights (Panel D) and entropy balancing weights (Panel E) on our survey data and using Arab Barometer III as our reference dataset.*

Table 9: Results of Post-Survey Weighting

H.2 Balance checks for population

Table 10 replicates Table 2 for the population. We check balance by implementing a weighted least squares version of the first equation in Appendix D. Here we observe that five of the 60 individual covariates are significant at the 95 percent confidence level. While this is slightly higher than than we might expect to observe by chance, it is within reason. Below we check the robustness of our estimates of treatment effects to the inclusion of imbalanced controls to ensure that these imbalances are not biasing results.

Table 10: Balance Check for Population

	Population		
	cont.	coeff.	pval
Panel A: Demographics			
Age	39.88	-1.76	0.083
Highest level of education completed (1-8)	4.54	-0.17	0.224
Head of the household (0/1)	0.45	-0.04	0.164
Married (0/1)	0.58	0.02	0.605
Female (0/1)	0.53	-0.05	0.103
Maronite (0/1)	0.25	0.02	0.505
Shia (0/1)	0.25	0.04	0.089
Sunni (0/1)	0.27	0.00	0.818
Panel B: Sectarian Variables			
Strength of sectarian identity index (z-score)	-0.34	0.14	0.018
Strong sectarian identity (compared to Lebanese identity) (1-5)	1.75	0.22	0.006
Identify by sectarian group (1-4)	1.88	0.03	0.646
Sectarian heterogeneity of social network (1-5)	2.74	0.07	0.258
Sect-based prejudice index (z-score)	-0.34	0.09	0.267
Not comfortable having non co-sect. neighbors (1-4)	1.57	0.04	0.475
Not comfortable being supervised by non co-sect. (1-4)	1.58	0.09	0.208
Not comfortable having non co-sect. friends (1-4)	1.52	0.02	0.616
Not comfortable setting up a business with non co-sect. (1-4)	1.82	0.07	0.300
Not comfortable marrying non co-sect. (1-4)	2.73	0.13	0.330
Benefits from sectarian system index (z-score)	-0.27	-0.06	0.326
Sect. imp. for access to jobs/benefits for HH (1-4)	2.23	-0.03	0.750
Sect. imp. for econ development of Lebanon (1-4)	2.23	-0.13	0.104
Sect. imp. for local development of your region (1-4)	2.29	-0.14	0.119
Sect. imp. for responsive/representative govt (1-4)	2.10	-0.14	0.093
Sect. imp. for protection of Lebanon from intl threats (1-4)	2.18	0.00	0.986
Sect. imp. for protection of sect from intl threats (1-4)	2.21	-0.04	0.654
Sect. imp. for protection of sect within Lebanon (1-4)	2.36	-0.01	0.933
Sect. imp. for stability/avoiding war (1-4)	2.27	0.02	0.834
Not difficult to get help from Zaim/politician (1-4)	1.71	-0.03	0.674
Satisfied with MPs that represent sect (1-4)	1.90	-0.11	0.107
Thinks sectarian politics is important for some things (0/1)	0.66	0.00	0.956
Strength of Lebanese identity index (z-score)	0.09	-0.01	0.865
Strong Lebanese identity (compared to sect identity) (1-5)	3.21	0.03	0.537
Identify as Lebanese (1-5)	3.48	-0.05	0.156
Part of the majority sect in the PSU (0/1)	0.58	-0.01	0.419
Interviewed by a co-sectarian (0/1)	0.47	-0.09	0.013
Panel C: Economic Variables			
Class heterogeneity of social network (1-5)	2.82	0.01	0.885
Attachment to economic identity index (z-score)	-0.08	-0.13	0.025
Identifies by profession/occupation (1-4)	1.85	-0.11	0.068
Identifies by economic class (1-4)	1.76	-0.13	0.025
Upper-class economic status index (z-score)	-0.10	-0.04	0.585
Total TVs owned	1.55	0.04	0.319

Continued on next page

Table 10: Balance Check for Population

	Population		
	cont.	coeff.	pval
Total desktop computers owned	0.35	0.01	0.811
Total mobile phones owned	2.83	0.30	0.018
Total laptop computers owned	0.76	-0.02	0.643
Total houses/apartments owned	0.90	-0.08	0.075
Total cars owned	1.01	-0.05	0.289
Household's total net income in a typical month (1-10)	4.91	-0.03	0.687
Total income earners with white collar jobs (up to 2)	0.70	-0.03	0.557
Views household as upper-class	2.42	-0.04	0.460
Cross-sect economic group distance index (z-score)	-0.05	0.08	0.189
Social distance: poor Christians to poor Muslims (1-7)	4.14	-0.03	0.813
Social distance: rich Christians to rich Muslims (1-7)	4.85	0.34	0.012
Same-sect economic group distance index	-0.25	0.07	0.354
Social distance: poor Christians to rich Christians (1-7)	3.36	0.19	0.261
Social distance: poor Muslims to rich Muslims (1-7)	3.08	0.06	0.656
Panel D: Political Action Variables			
Political efficacy index (z-score)	0.13	-0.06	0.378
There are many legal ways for citizens to influence govt (1-4)	3.12	-0.08	0.204
There is no way to make public officials listen to citizens (1-4)	2.94	-0.02	0.701
Political action index (z-score)	0.09	-0.05	0.543
Used social media to take action (0/1)	0.13	0.01	0.556
Talked to party members/MPs/zaim (0/1)	0.09	-0.01	0.717
Signed a petition (0/1)	0.09	-0.02	0.372
Attended a demonstration or protest march (0/1)	0.22	0.00	0.948
Joined an NGO that advocates for a policy issue (0/1)	0.07	-0.02	0.183
Cross-pressure index (z-score)	-0.12	0.01	0.837
Reluctant to act because it creates enemies (1-4)	2.63	0.06	0.568
Reluctant to act because worry what people would think (1-4)	1.96	-0.03	0.651
Reluctant to act because don't know where I stand on issues (1-4)	1.90	-0.01	0.878
Panel E: Fear of Social Sanctioning			
Fear of sanctioning index (z-score)	-0.21	-0.03	0.680
Difficult to go against opinion of political leaders (1-4)	2.03	-0.09	0.272
Difficult to go against opinion of family, friends, neighbors (1-4)	2.07	-0.01	0.919
Difficult to go against opinion of sectarian community (1-4)	2.07	0.07	0.358

H.3 Main results for the population

We first replicate our main results with and without controls using our three population weights: the design weights, the raked weights, and the entropy balancing weights. We estimate treatment effects using a weighted least squares version of the equations in Appendix D. Tables 11, 12, and 13 present results for the population replicating the table in the main text (and also presenting results with controls).

Overall, the results for the population are very similar in magnitude and significant to the results for the sample. While the estimates are noisier (which is generally always true when weights are used) the magnitudes are similar to those observed for the sample.[22] The results are also consistent across the different weighting approaches. Specifically, the results suggest that public disclosure had the greatest effect on reducing participation for those afraid of disagreeing with their friends, family, and neighbors (this effect is significant in some estimations). The coefficient on the interaction of *Public*Fear of elite sanctioning* is similar in the sample and the population estimates, although less precisely estimated in the latter. The only coefficient that appears to differ across the sample and population (regardless of weighting approach) is that on *Public*Fear of community sanctioning*. The results for the population suggest that the effects of disclosure might not be conditional on fear of sanctioning by the broader community. As a result of this greater variation in the conditional effects of different sources of social pressure (and lower levels of precision in the estimates) the interaction of *Public*Fear of sanctioning (index)* is not statistically significant. Nevertheless, the population results support our main findings about the effects of public disclosure and reinforce the possibility that the effect of public disclosure is greatest for those who are afraid of sanctioning by their immediate social networks.

[22] While weighting introduces precision loss, the similar sample and population results help to reassure that weighting did not introduce other potential sources of bias (Miratrix et al., 2017).

	Main effects	Conditional effects: Fear of Sanctioning				Main effects	Conditional effects: Fear of Sanctioning			
		Index	By elites	By fam	By commun.		Index	By elites	By fam	By commun.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Public petition (versus private)	-0.20***	-0.18***	-0.17***	-0.16***	-0.19***	-0.18***	-0.17***	-0.17***	-0.15***	-0.17***
	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)
Public X Fear sanctioning (index)		-0.04					-0.05			
		(0.06)					(0.06)			
Public X Fear elite sanctioning			-0.09					-0.08		
			(0.07)					(0.07)		
Public X Fear fam sanctioning				-0.11					-0.12	
				(0.06)					(0.06)	
Public X Fear commun. sanctioning					-0.02					-0.05
					(0.05)					(0.05)
Fear sanctioning (index)		0.04					0.02			
		(0.05)					(0.05)			
Fear elite sanctioning			0.11*	0.06	0.07			0.10*	0.06	0.06
			(0.05)	(0.03)	(0.04)			(0.04)	(0.03)	(0.03)
Fear fam sanctioning			-0.07*	-0.02	-0.07			-0.06	-0.01	-0.06
			(0.04)	(0.05)	(0.04)			(0.04)	(0.05)	(0.04)
Fear commun. sanctioning			0.01	0.01	0.02			-0.01	-0.01	0.01
			(0.04)	(0.03)	(0.03)			(0.04)	(0.03)	(0.03)
Constant (signed in private)	0.67	0.65	0.66	0.65	0.67	0.82	0.78	0.80	0.79	0.80
N	2496	2496	2496	2496	2496	2496	2496	2496	2496	2496
Full set of controls	No	No	No	No	No	Yes	Yes	Yes	Yes	Yes

Notes: * $p < .05$, ** $p < .01$, *** $p < .001$
P-values based on a two-tailed test.
Clustered standard errors in parentheses.

Table 11: Effects of public disclosure on petition signing: Population estimates using **design weights**

	Main effects	Conditional effects: Fear of Sanctioning				Main effects	Conditional effects: Fear of Sanctioning			
		Index	By elites	By fam	By commun.		Index	By elites	By fam	By commun.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Public petition (versus private)	-0.20***	-0.18***	-0.16***	-0.15***	-0.18***	-0.15***	-0.17***	-0.15***	-0.14***	-0.16***
	(0.02)	(0.03)	(0.03)	(0.03)	(0.04)	(0.04)	(0.03)	(0.03)	(0.03)	(0.03)
Public X Fear sanctioning (index)		-0.02					-0.03			
		(0.07)					(0.07)			
Public X Fear elite sanctioning			-0.09					-0.08		
			(0.08)					(0.07)		
Public X Fear fam sanctioning				-0.11					-0.13*	
				(0.06)					(0.06)	
Public X Fear commun. sanctioning					-0.02					-0.05
					(0.06)					(0.05)
Fear sanctioning (index)		0.03					0.01			
		(0.05)					(0.05)			
Fear elite sanctioning			0.10*	0.06	0.06			0.09	0.04	0.04
			(0.05)	(0.04)	(0.04)			(0.05)	(0.04)	(0.04)
Fear fam sanctioning			-0.09**	-0.03	-0.09**			-0.07*	0.00	-0.07*
			(0.03)	(0.04)	(0.03)			(0.03)	(0.05)	(0.03)
Fear commun. sanctioning			0.02	0.02	0.03			0.00	0.00	0.03
			(0.05)	(0.05)	(0.04)			(0.05)	(0.05)	(0.04)
Constant (signed in private)	0.78	0.69	0.69	0.69	0.70	0.72	0.82	0.83	0.83	0.83
N	2496	2496	2496	2496	2496	2496	2496	2496	2496	2496
Full set of controls	No	No	No	No	No	Yes	Yes	Yes	Yes	Yes

Notes: * $p < .05$, ** $p < .01$, *** $p < .001$
P-values based on a two-tailed test.
Clustered standard errors in parentheses.

Table 12: Effects of public disclosure on petition signing: Population estimates using **raked weights**

	Main effects	Conditional effects: Fear of Sanctioning				Main effects	Conditional effects: Fear of Sanctioning			
		Index	By elites	By fam	By commun.		Index	By elites	By fam	By commun.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Public petition (versus private)	-0.19***	-0.12**	-0.12**	-0.08*	-0.13**	-0.12***	-0.10**	-0.10**	-0.06	-0.10**
	(0.02)	(0.04)	(0.04)	(0.04)	(0.05)	(0.04)	(0.03)	(0.03)	(0.03)	(0.04)
Public X Fear sanctioning (index)		-0.07					-0.05			
		(0.07)					(0.07)			
Public X Fear elite sanctioning			-0.09					-0.07		
			(0.07)					(0.08)		
Public X Fear fam sanctioning				-0.17*					-0.17*	
				(0.07)					(0.08)	
Public X Fear commun. sanctioning					-0.04					-0.05
					(0.06)					(0.06)
Fear sanctioning (index)		-0.05					-0.03			
		(0.05)					(0.04)			
Fear elite sanctioning			0.04	-0.01	0.00			0.00	-0.04	-0.03
			(0.06)	(0.04)	(0.04)			(0.06)	(0.04)	(0.04)
Fear fam sanctioning			-0.09	0.00	-0.09			-0.06	0.03	-0.06
			(0.06)	(0.06)	(0.06)			(0.05)	(0.06)	(0.05)
Fear commun. sanctioning			-0.01	-0.02	0.00			0.01	0.00	0.03
			(0.06)	(0.06)	(0.07)			(0.06)	(0.05)	(0.06)
Constant (signed in private)	0.70	0.74	0.74	0.73	0.75	0.86	0.89	0.89	0.86	0.88
N	2496	2496	2496	2496	2496	2496	2496	2496	2496	2496
Full set of controls	No	No	No	No	No	Yes	Yes	Yes	Yes	Yes

Notes: * $p < .05$, ** $p < .01$, *** $p < .001$
P-values based on a two-tailed test.
Clustered standard errors in parentheses.

Table 13: Effects of public disclosure on petition signing: Population estimates using **entropy balancing weights**

I Multiple Hypothesis Testing Adjustment

In this study we test one main hypothesis and five heterogeneous effects hypotheses (for a summary, see Appendix J). Our conditional effects hypotheses involve five pairwise comparisons: (1) individuals with low and high fears of sanctioning (main text), (2) with homogeneous and heterogeneous sectarian social networks (see Appendix G.1), (3) men and women (Appendix G.2), (4) poor and rich (Appendix G.2), and (5) Sunnis versus other sectarian groups (Appendix G.2). To account for these multiple hypothesis tests, we implement a (pre-registered) sharpened false discovery rate (FDR) q-value adjustment following on Anderson (2008).[23] We implement the correction using the p-values from the results for the sample (without controls). Additionally, we implement—separately—a correction for the pairwise comparisons involving the three sources of social pressure: (1) low and high fears of sanctioning by elites, (2) by one's immediate social network, and (3) by the broader sectarian community. We correct for this separately because of the exploratory nature of the analysis.

Table 14 shows the results. With the correction, the adjusted p-value on our main interaction of interest—the interaction between the fear of sanctioning index and the treatment—is no longer significant at the 95 percent confidence level (our pre-registered significance level), although it is still significant at the 90 percent confident level. The separately corrected p-values for the index components all remain highly statistically significant.

	Original P-value	Sharpened Q-value
Panel A: Corrections for conditional effects hypotheses		
Public * Fear of sanctioning (index)	0.043	0.095
Public * Sectarian network homog.	0.591	0.310
Public * Female	0.496	0.310
Public * Poor	0.069	0.102
Public * Sunni	0.007	0.037
Panel B: Corrections for sources of social pressure		
Public * Fear of sanctioning by elites	0.022	0.017
Public * Fear of sanctioning by fam, friends	0.001	0.004
Public * Fear of sanctioning by community	0.024	0.017

The rows report the p-values from a two-sided test of the treatment indicator interacted with a binary conditional effects variable (see the first equation in Appendix D).

Table 14: Multiple hypothesis correction

[23] The FDR correction controls for the expected proportion of false discoveries among all discoveries.

J Comparing the Paper to the Pre-analysis Plan

The pre-analysis plan for this paper was registered with the Evidence in Governance and Politics (EGAP) network (`egap.org/registration/1984`). This Appendix discusses the ways in which the paper does (and does not) adhere to the original pre-analysis plan. Pre-analysis plans have become more common in political science in recent years as a means to prevent publication bias and to draw a bright line between confirmatory hypothesis testing and exploratory analysis. We note that the purpose of a pre-analysis plan is not to tie researchers' hands and prevent any deviations during the natural course of research but rather to encourage researchers to be transparent about those deviations and provide justifications (Humphreys, de la Sierra and van der Windt, 2013; Monogan III, 2015; Olken, 2015).

J.1 Hypotheses

We pre-registered one main hypothesis and five heterogeneous effects hypotheses for this paper. Table 15 summarizes these hypotheses as they were originally specified in the pre-analysis plan and refers readers to where they are tested in the main paper and appendix. There was one hypothesis—H3a—that we ultimately decided not to test.[24] We originally planned to test H3a using survey measures of dislike for sectarianism but we did not use any sensitive question techniques in asking these questions. After reading Adida et al. (2016) and seeing the big treatment effects in our own study—which suggest that subjects are much more likely to support sectarianism in public than in private—we did not feel confident that our pre-registered survey measures reliably capture 'private' dislike for sectarianism.

We discuss in the pre-analysis plan the importance of considering different sources of social pressure, whether from elites, one's immediate social network, or the broader sectarian community. We do not specify hypotheses for these individual sources. We therefore treat this analysis as exploratory.

[24] The pre-analysis plan also specifies an H2 but that is for a separate experiment on the effects of participation in public/private Facebook discussion groups that we will analyze in a different paper.

Pre-registered Hypotheses		Main text	Appendix
Main hypothesis			
H1	Private preferences will diverge from public preferences when those preferences are made known to political elites	Yes	Yes
Heterogeneous effects hypotheses			
H3a	Divergence between public and private preferences will be greater for those who privately dislike sectarianism	Not tested‡	
H3b	Divergence will be greatest for those who are socially and economically vulnerable (women, the poor)	No	F.2
H3c	Divergence will be greatest for those who are fearful of social sanctioning	Yes	F.1
H3d	Divergence will be greatest for those who are in homogeneous sectarian networks	No	F.1
H3e	Divergence will be greater for those who are in sectarian groups with stronger intra-group punishment	No	F.2

‡ *This hypothesis is not tested in the paper.*

Table 15: Comparison of hypotheses in the pre-analysis plan and paper

J.2 Data and measurement

We stick closely to the pre-analysis plan in terms of which variables are used in the analysis and how they are coded. Section 6.2 of the pre-analysis plan provides details on our plans to address a small amount of item-level missingness through chained imputation; to create indices out of variables that measure the same latent construct using inverse covariance weighting; and to use binary versions of indices (cut at the median) and component variables (cut at the midpoint of the scale) when doing heterogeneous effects analysis for ease of interpretation. We also specify in Appendix F of the PAP all variables that were to be used to test the main and heterogeneous effects as well as what variables/indices would be created and used as controls and to check balance. The only real change to this analysis was the addition of two control variables: one measuring whether a respondent was interviewed by a co-sectarian and one capturing whether a respondent belongs to the majority sectarian group in their primary sampling unit. These changes are not consequential as the results are highly robust to the exclusion/inclusion of controls.

J.3 Estimation

We also stick closely to the pre-registered estimation strategy. We pre-specify including a vector of controls in our main estimation and present all results with and without controls. We also pre-specify our focus on sample average treatment effects. We do not pre-register looking at population-level results due to the challenges with obtaining reliable weights (discussed in Appendix H). We decided to present population results in the Appendix primarily based on feedback received on earlier versions of the paper. Early readers wanted to see some effort to try to generalize the sample results to the population given the relatively high level of unit non-response in the study and the substantive interest of trying to understand the true level of support for sectarian system reform in the Lebanese population. Finally, the pre-specified multiple hypothesis correction for the conditional effects hypotheses is implemented in Appendix I.

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