Protests as Strategic Games: Experimental Evidence from Hong Kong's Anti-Authoritarian Movement

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Abstract

Social scientists have long viewed the decision to protest as strategic, with an individual's participation a function of her beliefs about others' turnout. We conduct a framed field experiment that re-calibrates individuals' beliefs about others' protest participation, in the context of Hong Kong's ongoing anti-authoritarian movement. We elicit subjects' planned participation in an upcoming protest and their prior beliefs about others' participation, in an incentivized manner. One day before the protest, we randomly provide a subset of subjects with truthful information about others' protest plans, and elicit posterior beliefs about protest turnout, again in an incentivized manner. After the protest, we elicit subjects' actual participation. This allows us to identify the causal effects of positively and negatively updated beliefs about others' protest participation on subjects' own turnout. In contrast with the assumptions of many recent models of protest participation, we consistently find evidence of strategic *substitutability*. We provide guidance regarding plausible sources of strategic substitutability that can be incorporated into theoretical models of protests.

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1 Introduction

Mass political movements have long demanded fundamental political rights. Citizens have taken to the streets from Tiananmen Square to Tahrir Square, from the Women's Suffrage Movement to the Civil Rights Movement, from the Velvet Revolution to Hong Kong's Umbrella Revolution. What drives individuals' decisions to participate in political protests such as these?

Strategic considerations have long been seen as crucial, with an individual's participation shaped by her beliefs about the participation of others. On the one hand, protests are a classic example of a political collective action problem: individuals have an incentive to free-ride on the costly participation of others, and may thus be less willing to turn out when they believe more others will do so, thus producing a game of strategic *substitutes* (Olson, 1965; Tullock, 1971; Palfrey and Rosenthal, 1984). On the other hand, much recent theoretical work assumes strategic *complementarity*: this might arise because the cost of participation is anticipated to be lower when a protest is larger; because participatory utility is greater in a larger, more successful protest; or, because one's utility under a post-revolution regime will be greater if one was a protest participant (see, for example, Bueno de Mesquita, 2010; Edmond, 2013; Barberà and Jackson, 2017; Passarelli and Tabellini, 2017). Indeed, in a recent review article, Gehlbach et al. (2016, p. 579) go so far as to affirm that strategic complementarity "*characterizes* mass protests."[1]

In this paper, we identify the causal effect of beliefs about other individuals' protest turnout on one's own, conducting a framed field experiment with potential participants in an anti-authoritarian protest in Hong Kong. We study participation in a July 1 March, a yearly protest that represents an important component of Hong Kong's ongoing anti-authoritarian movement, epitomized by the recent Umbrella Revolution.[2] The July 1 March shares essential characteristics with anti-authoritarian protests across time and space: participants aim to achieve policy concessions from an authoritarian regime by turning out in large numbers, facing the threat of government crackdown. In this context, we experimentally re-calibrate individuals' beliefs about others' protest participation, and study how these beliefs affect one's own participation. We find consistent evidence of strategic *substitutability* in the decision to protest, challenging many recent models of protest participation that assume strategic complementarity.

While much theoretical work has been done on the strategic element of the protest decision, empirical evidence on the causal effect of beliefs regarding others' protest turnout on one's own is extremely limited. Several recent articles have provided causal evidence on a bundle of "social" influences on protest participation: Enikolopov et al. (2016) present evidence that the diffusion of

[1] Emphasis added by the authors. See also Granovetter (1978); Kuran (1989, 1991, 1997); Chwe (2000); Fearon (2011); Kricheli et al. (2011); Egorov and Sonin (2018) for models of protests in which participation is a game of strategic complements. Some recent theoretical work allows for the possibility of strategic substitutability in the context of the same protest game: see Shadmehr and Bernhardt (2011); Shadmehr (2018).

[2] In Cantoni et al. (2016), we provide a complementary, descriptive study of the characteristics of supporters of Hong Kong's anti-authoritarian movement.

an online social network increased protest turnout in Russia; González (2016) provides evidence that peers' participation in Chilean student protests increased one's own; and, Manacorda and Tesei (2016) provide evidence that mobile phones' diffusion increased protest turnout in Africa. However, these analyses are unable to separately identify the effects of beliefs about the protest participation of others on one's own participation, instead estimating the combined effects of (*i*) learning about a protest's logistics (e.g., time and place); (*ii*) learning about the state of the world; and, (*iii*) learning about others' protest turnout.[3]

Credibly testing for a causal effect of beliefs about others' turnout in the decision to protest against an authoritarian regime has been hindered by two empirical obstacles. First, anti-authoritarian political movements have typically been studied *ex post* (e.g., Kuran, 1989, 1991, 1997; Opp and Gern, 1993; Lohmann, 1994). This not only generates selection issues — movements are generally studied after they have become large and successful — but also makes the prospective study of beliefs nearly impossible: belief elicitation would necessarily be retrospective and likely distorted by the realization of the political outcomes of interest.

Second, even when measured in real time, it is extremely difficult to exploit variation in beliefs to identify causal effects. Naturally-occurring variation is very likely to be endogenous with respect to behaviors of interest. Experimental variation, e.g., arising from an information treatment, runs into challenges from heterogeneous priors, which imply that the same information treatment can generate positive belief updating among one subset of the sample (i.e., those whose priors are below the information provided) and negative updating among another subset.[4] This means, for example, that even an effective intervention may produce average treatment effects on beliefs or on behavior that spuriously appear to be null results. The average effects would simply reflect offsetting heterogeneous treatment effects of opposite signs. Thus, experimental interventions aimed at manipulating beliefs require carefully measured priors (and ideally posteriors as well) to determine exactly how the treatment affects particular individuals' beliefs, and through beliefs, behavior.

We overcome each of these obstacles, as follows. First, we study participation within an ongoing, high-stakes political movement: Hong Kong's anti-authoritarian movement.[5] Because Hong Kong's democrats traditionally protest the rule of the Chinese Communist Party (CCP) each July 1, there exists a known protest about which we can elicit beliefs prospectively, in real time. Second, using a three-part online experiment we conducted at the Hong Kong University of Science

[3]Other recent empirical work on the causes and consequences of mass political movements includes Madestam et al. (2013); Yanagizawa-Drott (2014); DellaVigna et al. (2014); Acemoglu et al. (2014).

[4]See Coffman et al. (2015) for a theoretical analysis of the effects of information nudges.

[5]The eventual success or failure of the movement is likely to have repercussions throughout "Greater China" (and thus around the world) given concerns in Hong Kong, mainland China, as well as in Taiwan, over the increasingly authoritarian and nationalistic policies undertaken by the Chinese Communist Party. Our work contributes to a growing empirical literature on the political economy of the region: for example, Lorentzen (2013) highlights the central government's tolerance of certain types of protests; King et al. (2013) study information control policies that aim at suppressing collective actions.

and Technology (HKUST), we are able to elicit the prior beliefs of over 1,200 university students regarding the protest turnout of their classmates in the upcoming July 1 March (in an incentivized manner); we are then able to provide an information treatment to a random subset and elicit posterior beliefs (again in an incentivized manner); finally, we are able to elicit the students' own protest participation.

The goal of our experimental design is to isolate the causal effect of variation in beliefs regarding others' protest participation on one's own protest participation. To do so, we provide a random subset of individuals in our sample truthful information intended to shift beliefs regarding others' protest participation. A challenge we face is that such information must be provided *prior* to the protest itself — before we know the actual protest decisions of others. To solve this problem, one week before the protest, we collect information on individuals' own planned protest turnout, on individuals' beliefs about others' *planned* turnout, as well as individuals' beliefs about others' future *actual* turnout at the protest. This allows us to provide truthful information regarding others' *planned* participation, plausibly affecting beliefs regarding others' *actual* protest participation.

A day before the protest, we provide a random subset of individuals in our sample truthful information about the planned participation of their classmates. We estimate the "first stage" effect of information regarding others' planned participation on individuals' (posterior) beliefs regarding others' actual participation. Next, we estimate the "reduced form" effect of information regarding others' planned participation on individuals' own actual protest participation. Importantly, we split our analysis into two subsamples: those whose prior beliefs were below the true level of planned participation (whose beliefs regarding actual turnout, we expect, should be positively affected) and those whose prior beliefs were above the true level of planned participation (whose beliefs regarding actual turnout, we expect, should be negatively affected).[6]

Our findings consistently point to our sample of Hong Kong students viewing the strategic component of their protest decision as being a game of strategic substitutes. Among subjects whose prior beliefs regarding others' planned participation were below the truth, the experimental provision of information regarding the true level of other subjects' planned participation has a significant, positive effect on beliefs about actual participation in the protest, and a significant *negative* effect on subjects' own turnout. Among subjects whose prior beliefs regarding others' planned participation were above the truth, the experimental provision of information regarding the true level of other subjects' planned participation has a significant, negative effect on beliefs about actual participation in the protest, and a significant *positive* effect on subjects' own turnout.

We are able to address several concerns about our analysis. First, using list experiments, we

[6]Note that in addition to providing evidence on balance between treatment and control groups in the full experimental sample, we present evidence of balance within each of these subsamples (see Section 3). This sample split relies on our elicitation of beliefs about both others' planned participation and others' actual participation. The former gives us a measure of where priors stood relative to the experimental information we are able to provide to subjects prior to the protest itself (i.e., on *planned* participation). The latter gives us a measure of the priors we care about when examining belief updating in the first stage analysis (i.e., changes in beliefs regarding *actual* participation).

provide evidence that our experimental subjects are willing to truthfully report on potentially sensitive political attitudes related to their participation in the July 1 protest; this helps assuage concerns regarding our reliance on a self-reported measure of protest turnout (see Section 3.3).[7] Second, we can rule out a major threat to internal validity: the possibility that information about other subjects' turnout affected not only beliefs about others' protest participation, but also beliefs about the "quality" of the political movement itself. Such a confounding "social learning" effect, however, would produce the appearance of strategic complementarity, not the strategic substitutability that we find. Third, we can address concerns regarding experimenter demand effects following a similar logic: typically, an experimenter's implied endorsement of an action (by indicating its popularity) would produce the appearance of strategic complementarity (see Section 4.4).

We find suggestive evidence of three sources of strategic substitutability in our context (see Section 5). First, one aim of Hong Kong's protests is to get a sufficient number of people onto the street — this is a public good (as in Olson, 1965; Tullock, 1971; Palfrey and Rosenthal, 1984) that could have tangible consequences and could serve as a signal of the movement's strength to the CCP and to other citizens. If subjects view attaining a threshold level of protest participation as producing a political public good, this will tend to produce strategic substitutability. Consistent with protest turnout being a public good, we find that more pro-social subjects are more likely to protest, even conditional on ideology. Second, experimental subjects perceive not only a greater likelihood of a protest's success as protest size increases, but also a greater likelihood of government crackdown. If the latter dominates, the result can be strategic substitutability. Third, subjects may have social image concerns, and select into protest participation in part to signal their ideological "type" (as in Bénabou and Tirole, 2011). If participation in a smaller protest sends a strong enough signal, individuals wanting to signal their anti-authoritarian ideology may differentially participate when they anticipate a protest will be *small*, thus producing a game of strategic substitutes. Consistent with this mechanism, we find that individuals who participate in the protests after learning that protests will be smaller than expected have ideologically more extreme friends than other protest participants.

Our results thus indicate that protests are not *generically* games of strategic complements — as assumed in much recent work. Models of protest participation must allow for the possibility of strategic substitutes. Yet it is important to emphasize that not all of the mechanisms we observe at work in Hong Kong will be present in all protests, and even if they are present, they will not always outweigh forces generating strategic complementarity emphasized in other work. As we discuss in the Conclusion (Section 6), we believe that our findings reflect the fact that Hong Kong's protests are part of a long-running movement, and that the Hong Kong government protects ba-

[7] One might be particularly concerned about misreporting in survey data collected in authoritarian settings (see Reny, 2016 for a discussion of challenges facing social scientists in China). Indeed, analyses of political behavior in real time are more common in settings that are already politically free (e.g., Gerber et al., 2011, 2017).

sic rights of association and expression. Strategic substitutability thus seems most likely to appear in protests that are part of larger movements, and protests demanding rights from partially-democratic regimes, while forces pushing toward strategic complementarity may dominate in one-shot protests that will end in the ousting of a dictator or the crushing of a movement.

We next, in Section 2, provide an overview of Hong Kong's ongoing democratic, anti-authoritarian movement and the July 1 March, in particular. Next, in Section 3, we describe our experimental design. In Section 4, we present our main findings and discuss threats to internal validity. In Section 5 we discuss the theoretical implications of our findings and the characteristics of our setting that may generate strategic substitutability in the protest game. Finally, in Section 6, we discuss conditions under which strategic substitutability or complementarity are more likely, and offer concluding thoughts.

2 Hong Kong's anti-authoritarian movement

2.1 Political context

Prior to 1997, Hong Kong was a British colony, with limited democratic political rights, but strong protections of civil liberties and respect for the rule of law. On July 1, 1997, Hong Kong was returned to the People's Republic of China, to be ruled as a Special Administrative Region with its own quasi-constitution — the "Basic Law" — and a promise from China that its institutions would be respected and maintained until 2047, under a policy known as "one country, two systems." The Basic Law left ambiguous several important details that have been bargained and battled over between the so-called "pan-democracy" and "pro-Beijing" camps.

The first ambiguity to generate mass political protests was regarding Article 23 of the Basic Law, which covered the legal regulation of speech and behavior that threatened the government. Under the encouragement of Beijing, a law implementing provisions of Article 23 — the "National Security Bill" — was proposed by the Hong Kong Chief Executive (the head of government) in September 2002, and was seen by many Hong Kong citizens as deeply threatening to their human rights and civil liberties.[8] The proposed legislation catalyzed a massive July 1 March (in 2003) in which an estimated half million people protested. This expression of popular opposition led to the withdrawal of the bill, and no legislation on Article 23 has passed since.

More recently, political conflict has arisen from a second ambiguity in the Basic Law, regarding the method of selection of Hong Kong's Chief Executive. Article 45 of the Basic Law of Hong Kong states the following: "The method for selecting the Chief Executive shall be specified in the light of the actual situation in the Hong Kong Special Administrative Region ... The ultimate aim is the selection of the Chief Executive by universal suffrage upon nomination by a broadly repre-

[8]For a discussion of these concerns, see the University of Hong Kong's Human Rights Portal Page, "Research on Article 23," online at `https://goo.gl/GdNcHY`, last accessed February 28, 2018.

sentative nominating committee in accordance with democratic procedures." While indicating an ultimate aim of universal suffrage, the Basic Law does not state *when* elections will be introduced, nor does it clarify the details of nomination. From Hong Kong's return to China until today, the Chief Executive has been selected by an Election Committee, rather than by universal suffrage; currently, the Committee is composed of 1,200 members, and is widely seen as pro-Beijing.

In 2014, the Twelfth National People's Congress proposed an election mode that would have allowed the citizens of Hong Kong a choice between two or three candidates, but these candidates would be selected by the same pro-Beijing committee as before.[9] In response to this limited expansion of democratic rights, a massive July 1 March was mobilized, with hundreds of thousands of citizens taking to the streets. Further escalation and a police crackdown precipitated the even larger-scale "Umbrella Revolution," named for the ubiquitous umbrellas carried by participants. The Umbrella Revolution persisted for months, being slowly cleared out by police by the end of December 2014. While the movement did not alter the policy proposed by Beijing, it did send a clear signal to the Hong Kong legislature (the "LegCo") that a circumscribed change in institutions was unacceptable to the people of Hong Kong. In June 2015, the LegCo struck down the Chinese proposal led by the opposition of the pan-democratic camp.

Since June 2015, the democratic movement in Hong Kong has both fragmented and radicalized. Recent encroachments on Hong Kong citizens' civil liberties, including the arrest of Hong Kong booksellers by the mainland Chinese government, have deepened some Hong Kong citizens' fear of the CCP and their sense of a Hong Kong identity very much distinct from — even opposed to — that of mainland China. The result is that Hong Kong citizens and political parties are now much more loudly calling for independence or "self determination." "Localist" violence has occasionally flared; new political parties, such as the student-led Demosistō, have formed and won seats in the 2016 LegCo election on platforms explicitly calling for self-determination.[10]

2.2 The July 1 Marches: characteristics and achievements

Marches on the anniversary of Hong Kong's handover to China, held each July 1, have been described as "the spirit of democratic struggle in Hong Kong."[11] The July 1 Marches have played an important role in Hong Kong citizens' political engagement with the Chinese government, and have achieved major policy changes and even constitutional concessions — particularly when large crowds of protesters were mobilized.[12] Each protest March, while part of a broader anti-authoritarian, democratic movement, is organized around a specific set of issues and policy aims. The first notable achievement came as a response to the CCP's September 2002 proposal for an

[9] Refer to `https://goo.gl/0oyNmt`, last accessed February 28, 2018.

[10] The legislators elected on a self-determination platform were since removed from office on various technicalities regarding their oath-taking, foreshadowing future conflict.

[11] "Sixteen Years of July 1st Marches: A Dynamic History of Hong Kong Citizens' Fight for Democracy," *Initium Media*, June 30, 2018. Available online at `https://goo.gl/8bZDrf` (last accessed July 5, 2018).

[12] A time series of turnout in July 1 Marches can be seen in Online Appendix Figure A.1.

anti-subversion bill under Article 23, described above. The July 1, 2003, March included around 500,000 people — the largest political gathering in Hong Kong since the Chinese Democracy movement of 1989. Not only was the proposed law withdrawn, but the March eventually forced the resignation of multiple government officials, including the Chief Executive, Tung Chee-hwa.[13]

Another success followed the 2012 March, which included up to 400,000 people, and was part of a mobilization against a CCP proposal for a mandatory "moral and national curriculum" in Hong Kong schools. This proposal, too, was withdrawn shortly after the March. The 2014 March again saw hundreds of thousands of people demanding the popular nomination of Chief Executive candidates in the 2017 election. Although the March did not achieve citizen nomination of Chief Executive candidates, it did produce the massive Umbrella Revolution and led to the rejection of the CCP's proposal for partial democratic rights.

Like others before it, the July 1, 2016, March studied here was organized around important political aims: first, to denounce the perceived corruption of Beijing-backed Chief Executive CY Leung. Second, to mobilize support for democratic — especially the newly-established localist — political parties in the run-up to the 2016 LegCo Elections. While the protest was smaller than some previous Marches (turnout was under 100,000 participants), it is noteworthy that Leung chose not to run for re-election in 2017, despite being unconstrained by term limits; and, parties opposed to Beijing won 55% of the LegCo vote share, with localist political parties winning nearly 20% of the vote in the 2016 election.[14]

Some characteristics of Hong Kong's July 1 Marches may appear idiosyncratic: they are regularly scheduled events and they are largely tolerated by an authoritarian government. In fact, these characteristics appear in other contexts. First, regularly scheduled protests are utilized by many anti-authoritarian movements, from Russia's "Strategy 31" movement demanding rights of assembly to the "Monday demonstrations" in Leipzig that precipitated the fall of the German Democratic Republic.[15] Second, authoritarian regimes are often surprisingly tolerant of protests, within limits. The "Monday demonstrations" in Leipzig were able to proceed in the late summer and autumn of 1989 despite the obvious feasibility of crackdown.[16] In Russia, protesters recently organized rallies in support of opposition politician Alexei Navalny on Vladimir Putin's 65th birthday, in October 2017, and the *Financial Times* notes that in response to a protest of around

[13]In an opinion piece tellingly titled "July 1st March turnout size is absolutely important," former LegCo member Margaret Ng Ngoi-yee writes,"[T]he turnout at the July 1st Marches is absolutely important. If not for 500,000 people taking to the street in 2003, Article 23 would have been legislated already." *The Stand News*, June 29, 2018. Available online at `https://goo.gl/vgP3WP` (last accessed July 5, 2018).

[14]Even using lower-end estimates of the protest size (30,000), as a percentage of the population this would make the 2016 July 1 March around one-third of the size of the largest protest in US history, The Women's March in 2017.

[15]Strategy 31 is discussed in "The Russian protesters who won't give up," by Luke Harding, *The Guardian*, August 30, 2010. Available online at: `https://goo.gl/vfwZro` (last accessed December 9, 2017). Weeks of modestly-sized, regularly-scheduled protests prior to the massive events that led to the fall of the Berlin Wall can be seen in Online Appendix Figure A.2.

[16]See "A Peaceful Revolution in Leipzig," by Andrew Curry, *Spiegel Online*, October 9, 2009. Available online at: `https://goo.gl/iUakCp` (last accessed December 9, 2017).

1,000 people in Moscow, "police largely left protesters alone."[17] Even in mainland China, the Communist Party tolerates particular protests (Lorentzen, 2013). In each of these settings, there exists a threat of crackdown *ex ante*, and — including in Hong Kong — police do crack down when protests cross the line.

Thus, like other anti-authoritarian protests, Hong Kong's July 1 Marches demand (and occasionally win) fundamental political rights — civil liberties and democratic institutions — from an authoritarian regime. Like other anti-authoritarian protests, turnout is important for success. The importance of protest size can be seen in our survey data: subjects in our experiment believe there is a higher likelihood of protest success if a protest is larger (see Figure 1). It can also be seen in the differences between July 1 March organizers' turnout estimates and the turnout estimates of the Hong Kong police. Organizers consistently exceed independent estimates of July 1 March size (and police estimates consistently fall below), with differences between the two reaching the tens or even hundreds of thousands (see Online Appendix Figure A.1).

Finally, like other anti-authoritarian protests, there is the potential for a high personal cost to be paid for turnout. Chinese authorities are deeply concerned about political instability in Hong Kong, at least in part because of potential spillovers into mainland China.[18] Thus, beyond the time cost and the experience of heat, humidity, and rain on a Hong Kong summer's day, the concern of the Chinese government implies the potential for high participation costs: the possibility of arrest and forceful police crackdowns using batons and tear gas — which have already occurred — as well as the potential for more violent suppression, particularly by the People's Liberation Army stationed in Hong Kong. A *New York Times* article describes the Umbrella Revolution in frightening terms: "On the first night, and for the next two weeks, rumors rippled through the [protesters'] camp. Protesters were fearful of a bloody crackdown, like what happened in Tiananmen Square." Interestingly, subjects in our experiment believe there is a higher likelihood of a government crackdown if a protest is larger (see Figure 1). Our finding that beliefs about protest success and government crackdown are both increasing in protest size suggest that (much like in other anti-authoritarian protests) there are forces both for strategic complementarity and for strategic substitutability in the July 1 March.[19]

[17] Several dozen protesters were detained then released in St. Petersburg, which saw a protest of over 2,000 people. See "Anti-Putin protests mark Russian president's birthday," by Max Seddon and Henry Foy, *Financial Times*, October 7, 2017. Available online at: `https://goo.gl/4oWQzA` (last accessed December 9, 2017).

[18] The Chinese government blocked Instagram — the last major uncensored social media platform available inside the Great Firewall — when the Umbrella Revolution broke out at the end of September 2014 (Hobbs and Roberts, 2016).

[19] We discuss limitations on the generalizability of the Hong Kong context in the Conclusion (Section 6).

3 Experimental design

3.1 Design overview

Our experiment was conducted online in three parts.[20] The goal of our experimental design is to isolate the causal effect of variation in beliefs regarding others' protest participation on one's own protest participation. To do so, we provide a random subset of individuals in our sample *truthful* information intended to shift beliefs regarding others' protest participation. A challenge we face is that such information must be provided *prior* to the protest itself — before we know the actual protest decisions of others.

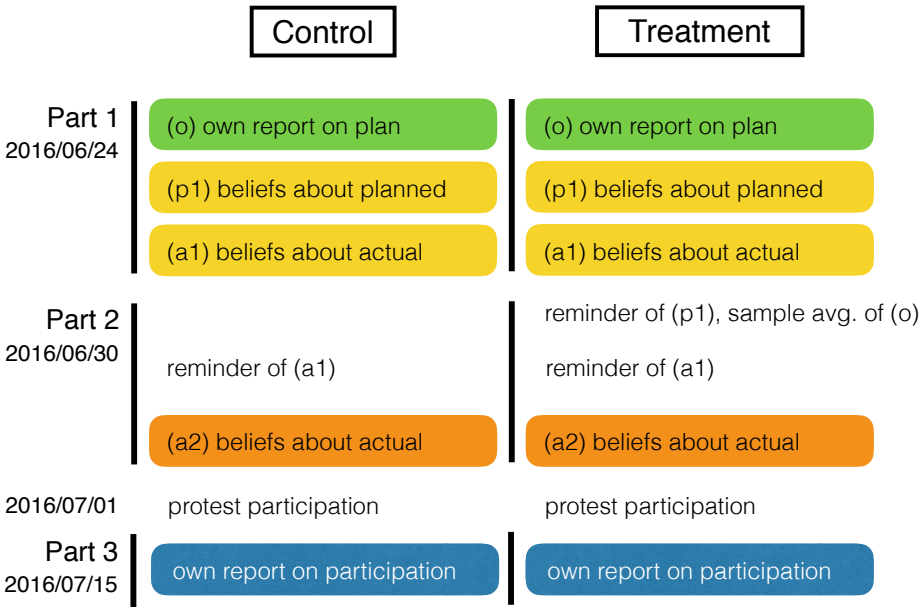
We solve this problem by collecting information on individuals' beliefs about others' *planned* turnout, as well as individuals' beliefs about others' future *actual* turnout at the protest. These should be closely related and, crucially, we are able to elicit planned protest participation (as opposed to actual participation) prior to the protest itself. This allows us to provide truthful information regarding others' *planned* participation, plausibly affecting beliefs regarding others' *actual* protest participation.

We first estimate the "first stage" effect of information regarding others' planned participation on individuals' (posterior) beliefs regarding others' actual participation. Next, we estimate the "reduced form" effect of information treatment regarding others' planned participation on individuals' own actual protest participation. Putting together the first stage and the reduced form, we can estimate the effect of a change in beliefs about others' participation on one's own using two-stage least squares.[21]

The broad outline of the design is as follows:

[20] The experiment described here was preregistered with the AEA along with a second experiment, which varied persuasive messages regarding a democratic political party and examined the effects of these messages on contributions to that party as well as on political attitudes and beliefs. The "persuasion" experiment was completed in Part 1 of the study, and was cross-randomized with the intervention studied here; importantly, all of the data collection for the other experiment occurred *prior* to the experimental intervention studied here (which was implemented in Part 2 of the study). Reflecting the cross-randomization of the two experiments, in Online Appendix Table A.1, we show that the variables collected in Part 1 are generally balanced between the treatment and control groups in this study. In Section 4, we examine the impact of the unbalanced "Part 1" variables on our treatment effects, and find that they have almost no effect on our treatment effect estimates (results are reported in Online Appendix Table A.2). It is important to emphasize that the outcome variables considered in this paper's analysis — posterior beliefs about the participation of others and subjects' own protest participation (collected in Part 3 of the study) — were the *only* outcome variables we collected following the experimental intervention we study here. We provide the full set of survey questions asked in Part 1 of the experiment (reformatted for brevity and organized thematically) in Online Appendix A.1. The full text of Parts 2 and 3 of the study are reproduced in Online Appendices A.2 and A.3, respectively.

[21] We discuss this two-stage estimate, particularly the implied exclusion restriction, in more detail below.



1. **Part 1:** On June 24, 2016, we elicited subjects' own planned participation in the upcoming July 1 March. We also elicited subjects' beliefs regarding other subjects' planned protest participation. We refer to these as elicited *priors* regarding other subjects' *planned* participation. In the same survey, we elicited subjects' beliefs regarding other subjects' actual protest participation on July 1, 2016. We refer to these as elicited *priors* regarding other subjects' *actual* participation. Finally, we elicited subjects' beliefs regarding the total protest participation among all Hong Kong citizens on July 1, 2016. We refer to these as elicited *priors* regarding total *actual* turnout among all HK citizens.

2. **Part 2:** On June 30, 2016, we provided a random subset of our experimental sample with a reminder of their *prior* beliefs regarding other subjects' *planned* participation, as well as information regarding the true level of *planned* protest participation in the experimental sample.[22] For both the information treatment group and the control group, we again elicited beliefs regarding other subjects' actual protest participation on July 1, 2016. We refer to these as elicited *posteriors* regarding other subjects' *actual* participation. Comparing posteriors between the treatment and control groups provides an estimate of the "first stage" relationship. We also elicited subjects' beliefs regarding the total protest participation among all Hong Kong citizens on July 1, 2016. We refer to these as *posteriors* regarding total *actual* turnout among HK citizens.

3. **Part 3:** On July 15, 2016, we elicited subjects' participation in the July 1 protest. Comparing

[22] We provide the reminder of subjects' priors in order to make the information treatment more salient, thus potentially increasing the power of our intervention. Of course, the reminder might serve as an anchor for subjects' responses, which could attenuate the treatment's effects.

participation rates between the treatment and control groups provides an estimate of the "reduced form" relationship of interest. Self-reported July 1 protest participation is also the outcome in our two-stage estimates of the effects of beliefs regarding others' protest participation on one's own.

3.2 Experimental sample

Our sample of experimental subjects is drawn from the population of students at the Hong Kong University of Science and Technology (HKUST). Studying a sample of students to understand protest participation is ideal given students' importance in Hong Kong's democratic movement and in the localist political parties pursuing self-determination. In Part 1 of the study, we recruited participants on June 24, 2016, sending an email to the entire undergraduate population of HKUST.[23] We received 1,744 completed surveys, achieving a response rate of 19.1%. Among these, we focus on the 1,576 students who were either born in Hong Kong or moved there prior to high school (Hong Kong "natives"). Part 1 of the experiment elicited students' political preferences, beliefs, attitudes, and planned and past political protest behavior. Because protests occur every year on Handover Day, July 1, we asked a series of questions specifically eliciting planned participation in the upcoming July 1 protest, as well as (prior) beliefs about turnout at the protest.

We paid students for their participation, and also provided additional payments as a function of their choices in incentivized games and in incentivized belief elicitations. On average, respondents received HKD 205, approximately US$ 25, for completing this first survey. Our experimental intervention was conducted in Part 2 of the study, a very short online survey sent in an email on June 30, 2016, and completed by 1,303 Hong Kong native students. Along with the experimental intervention of interest, this second survey elicited (posterior) beliefs about turnout in the following day's protest. Students received a payment of HKD 25 for completing the survey. Finally, in Part 3 of the study, we elicited students' participation in the July 1 protest of 2016 in a third online survey sent via email on July 15, 2016, and completed by 1,234 Hong Kong native students. Students who completed Part 3 of the study received an additional payment of HKD 25. We present summary statistics for the observable characteristics of the experimental sample — those subjects who completed all three parts of the study — in Table 1, columns 1 and 2.[24]

[23] Our recruitment email informed students that we were researchers at HKUST, UC-Berkeley, Stanford, and the University of Munich interested in understanding attitudes and preferences among college students in Hong Kong. The initial email did not explicitly mention our interest in political attitudes. All experimental materials were provided in English, the primary language of instruction at HKUST. Some bilingual support (i.e., materials provided in Chinese characters) was provided to clarify key terms.

[24] In Online Appendix Table A.3 we present summary statistics for the 1,576 Hong Kong native students who completed Part 1 of the experiment, and for the 1,234 students in our experimental sample. One can see that the two groups are extremely similar. The experimental sample of students is also similar to — though not precisely representative of — the broader HKUST student body on the dimensions of school of enrollment (i.e., students' broad academic area), gender, and cohort (see Online Appendix Table A.4). Note that all of our findings are robust to re-weighting our experimental sample to match the composition of the HKUST student body or to match the composition of the 1,576 Hong Kong native students who completed Part 1 of the experiment (see Section 4.4, below).

3.3 Elicitation of plans, beliefs, and actual protest participation

Part 1: Subjects' planned participation and prior beliefs

In Part 1 of the study, on June 24, 2016, we elicited subjects' own planned participation in the upcoming July 1, 2016, anti-authoritarian protest, asking:

> Are you planning to participate in the July 1st March in 2016?
>
> 1 Yes
> 2 Not sure yet, but more likely than not
> 3 Not sure yet, but more unlikely than yes
> 4 No

We next elicited subjects' beliefs regarding other subjects' planned protest participation (i.e., elicited priors regarding *planned* participation). This elicitation, like all other belief elicitation in this study, was conducted in an incentivized manner.[25] Specifically, we asked:

> Please guess what percentage of the participants from HKUST of this study plan to participate in the July 1st March in 2016 (answer either "Yes" or "Not sure yet, but more likely than not" to the above question on July 1st March in 2016).
>
> If your guess is within 2 percentage points of the percent of students who actually answer either "Yes" or "Not sure yet, but more likely than not," you will earn a bonus payment of HKD 10.

In the same survey, we elicited subjects' beliefs regarding other subjects' actual protest participation on July 1, 2016 (i.e., elicited priors regarding *actual* participation). We asked:

> Please guess what percentage of the participants from HKUST of this study will participate in the July 1st March in 2016.
>
> If your guess is within 2 percentage points of the percent of students who actually participate, you will earn a bonus payment of HKD 10.

Responses in Part 1 indicated that 16.9% of all subjects (including non-native Hong Kong students) planned to participate in the July 1 protest of 2016 (i.e., answered either "Yes" or "Not sure yet, but more likely than not" to the question regarding their plans for the upcoming July 1 protest). This is the number we use to provide "true" information regarding planned participa-

[25] The survey and the incentives are necessarily coarse: we elicit respondents' beliefs on how many other subjects answered "Yes" or "Not sure yet, but more likely than not", not the entire distribution of beliefs. This corresponds to providing an incentive for subjects to report their belief regarding the modal outcome, rather than the mean (though these will correspond if subjects' distributions of beliefs are are symmetric and single-peaked). We find that the distribution of prior beliefs regarding other students' planned participation has a mean and a mode very close to the true level of planned participation in the sample. This is consistent with the incentives we provided generating thoughtful, truthful responses.

tion to experimental subjects (rounding to 17%). The experimental sample's average prior belief regarding planned protest participation was quite close to the truth, at 15.8%, but there was a great deal of variation around the truth (the standard deviation was also 15.8 percentage points).

The sample's average prior belief regarding others' actual participation in the July 1 protest was 13.8%, slightly below the average prior belief about others' planned participation. Prior beliefs about others' planned and actual participation are strongly associated, as expected (the correlation is 0.83).[26]

Finally, we elicited subjects' prior beliefs regarding the total actual turnout at the July 1 March. We asked:

> How many people in total do you think will participate in the
> July 1st March in 2016?
> If your guess is within 10% of what will be reported by the HKUPOP
> after the July 1st March in 2016, then you will earn a bonus payment of HKD 10.
>
> To give you a sense, according to HKUPOP's report, among the July 1st March
> that took place between 2003 and 2015:
>
> The lowest attendance in a given year is: 17,000 (in 2008);
> The highest attendance in a given year is: 462,000 (in 2003).

The experimental sample's average prior belief regarding total turnout in the July 1 protest was 155,153.

Part 2: The experimental intervention and posterior beliefs

In Part 2 of the study, on June 30, 2016, we implemented the experimental intervention, randomly assigning two-thirds of subjects to the treatment group and one-third to the control group.[27] In Table 1, columns 1–3, we present data on the background characteristics, economic status, protest plans, and prior beliefs of the treatment and control groups, and test for balance between them. One can see that the treatment and control groups are very similar on these margins. As discussed above, the impact of an information shock on beliefs, and thus behavior, should differ (having effects of opposite sign) depending on whether the information provided was above or below individuals' prior beliefs. We will conduct much of our analysis separately examining individuals with priors above and below the information treatment, or pooling all subjects, but coding the

[26] In Online Appendix Figure A.3, we present the distributions of subjects' prior beliefs regarding others' planned participation and regarding others' actual participation. One can see in the figure that the distribution of priors regarding actual participation is shifted slightly to the left of the distribution of beliefs regarding planned participation.

[27] The decision to assign more individuals to the treatment group was made anticipating the possibility that some subjects may have ignored Part 2 of the study, and thus effectively ended up in the control condition. Under such a scenario, we could have examined protest behavior among individuals who were actually treated and among individuals who were assigned to the control condition or who did not complete the survey in Part 2 of the study. In practice, the vast majority of subjects completed all three parts of the study, so the additional individuals in the treatment group were not strictly necessary.

treatment indicator as being equal to -1 for individuals with prior beliefs above the information treatment in order to make the treatment effect monotonic.[28] It is thus important to check for balance within the two subsamples of interest — subjects with priors above and below the true value of planned participation of 17%. One can see in Table 1, columns 4–9, that treatment and control groups within each subsample are well balanced.[29]

Individuals in the treatment group — but not the control group — were reminded of their responses from Part 1 regarding other subjects' planned participation in the July 1 protest of 2016, and then told the actual level of other subjects' planned participation, as follows:

Recall that you guessed that *[Part 1 response]*% of HKUST survey participants would plan to attend the July 1 March.

Based on last week's survey, the true percentage of survey participants who plan to attend the July 1 March is **17%**.

All subjects (both treatment and control) were reminded of their responses from Part 1 regarding actual participation in the July 1 protest of 2016, as follows:

Remember that we offered you:
1 — A HKD 10 bonus payment for accurately guessing the percentage of HKUST survey participants who would actually attend this July 1 March;
2 — An additional HKD 10 bonus payment for accurately guessing the total number of Hong Kong citizens would actually attend this July 1 March.

In last week's survey, you guessed that:
1 — *[Part 1 response]*% of HKUST survey participants would attend this July 1 March;
2 — A total of *[Part 1 response]* Hong Kong citizens would attend this July 1 March.

All subjects were then given an opportunity to update their responses from Part 1:

Perhaps since then your views have changed.

We now ask you again to provide guesses about actual attendance of the July 1 March.

---

[28] It is important to note that variation in individuals' prior beliefs was not experimentally induced. In Online Appendix Table A.5, we present both predictors of individuals' own self-reported plans to participate in the protest as well as predictors of individuals' prior beliefs regarding other subjects' planned participation. We discuss the endogeneity of priors further in Section 4.

[29] As noted above, the survey in Part 1 elicited a broad range of subject characteristics: political attitudes and beliefs; personality traits; and, preferences (among others). We present a comprehensive set of balance tests (for the entire experimental sample and for the two subsamples of interest) for 49 different variables in Online Appendix Table A.1, and find statistically significant differences (at the 10% level) for 8 of the 49. In Section 4, we examine the impact of the unbalanced "Part 1" variables on our treatment effects, and find that they have almost no effect on our treatment effect estimates (results are reported in Online Appendix Table A.2).

Instead of your guesses in the previous survey, we will use today's guesses to determine your bonus payment.[30]

1 How many people in total do you think will participate in the July 1st March in 2016?
If your guess is within 10% of what will be reported by the HKUPOP after the July 1st March in 2016, then you will earn a bonus payment of HKD 10.

To give you a sense, according to HKUPOP's report, among the July 1st March that took place between 2003 and 2015:
The lowest attendance in a given year is: 17,000 (in 2008);
The highest attendance in a given year is: 462,000 (in 2003).

2 Please guess what percentage of the participants from HKUST of this study will participate in the July 1st March in 2016?
If your guess is within 2 percentage points of the percent of students who actually participate, you will earn a bonus payment of HKD 10.

The experimental sample's average posterior belief regarding the percentage of other subjects who would actually participate in the July 1 protest was 14.5%; the average posterior belief regarding total actual turnout among HK citizens was 143,856. In fact, the July 1 protest of 2016 was smaller than subjects expected: the protest was attended by 3% of experimental subjects, and only 26,000 people overall.[31]

Part 3: Measuring protest participation

In Part 3 of the study, on July 15, 2016, we elicited subjects' participation in the July 1 protest of 2016.[32] We asked subjects:

Did you attend the July 1 2016 March?

A response of "yes" to this question is our measure of individuals' protest participation.[33]

[30] We chose to pay subjects based on their responses in Part 2 (rather than using responses in both Part 1 and Part 2) in order to minimize any income effects or strategic incentives (e.g., hedging). By reminding subjects of their responses in Part 1 and allowing them to hold the same posteriors as priors, subjects were still able to be paid based on their Part 1 responses.

[31] The smaller than expected protest was perhaps the result of an announcement on the morning of July 1 that the arrested and returned Hong Kong bookseller, Lam Wing-kee, would not lead the protest, as had been expected, out of fear for his safety. The Hong Kong Free Press headlined an article on June 28, "Organisers expect 100,000 to attend July 1 democracy rally led by bookseller and ex-prisoners of conscience" (`https://goo.gl/kgDYr1`, last accessed December 19, 2016). The same source headlined an article on July 1, "Returned HK bookseller will not lead July 1 democracy march after 'serious threat' to safety" (`https://goo.gl/dHcR9b`, last accessed December 19, 2016).

[32] We waited two weeks before collecting information on March attendance as this gave us time to gauge whether subjects would be comfortable responding to a direct question about attendance. Because there had been no arrests in the weeks after the March, we felt comfortable asking subjects directly about their participation.

[33] We also ask those who attended the March a small number of follow-up questions. First, we asked them to indicate

An important concern regarding our measure of protest participation is that experimental subjects may not report on their participation truthfully. This concern is particularly relevant in the context of an ongoing anti-authoritarian movement. However, there are several reasons to believe that self-reported protest turnout is a good measure in our context.[34] First, the particular protest that we study remained peaceful. While subjects faced a risk of government crackdown on the protest *ex ante*, there was no concern regarding legal sanctions on participants two weeks after the protest, when subjects' protest participation was elicited. Second, for fear of government sanction to produce measurement error, it would need to be the case that subjects were willing to take the risk of attending a (very public) protest, but unwilling to tell us in a private survey that they did so. While this is possible (they may misperceive the observability of their protest choice and fear putting their behavior on the record), it strikes us as unlikely.

As a more direct test of our experimental subjects' willingness to truthfully respond to politically-sensitive survey questions, in Part 1 of the study we elicited several key dimensions of political ideology that may be considered sensitive using "list experiments" (or, the "Item Count Technique"; Raghavarao and Federer, 1979). The list experiment provides "cover" for the expression of possibly stigmatized attitudes at the individual level, but allows the researcher to estimate the prevalence of these attitudes at the population level. We adopt a modified version of the standard list experiment (Coffman et al., 2017) in which we also directly elicit the potentially stigmatized attitudes from the control group. Thus, for each potentially sensitive political attitude, we are able to compare the study sample's estimated adherence to that attitude when "cover" is provided (based on our list experiment) with the expressed adherence to that attitude in response to a direct question (from the control group). We can then test whether there exists a significant distortion in the expression of a particular attitude in response to a direct question (e.g., due to stigma or fear).

In Table 2, we present the fraction of our sample expressing support for Hong Kong independence; who consider themselves Hong Kongese rather than Chinese; who have a favorable view of the ruling CCP; and, who support the use of violence in pursuit of Hong Kong's political rights (these estimates are based on subjects completing Part 1 of the study). In the left-hand column, we simply present the population estimate of adherence to a political attitude based on direct questions. In the right-hand column, we show the difference between the estimate based on direct questions and the estimate based on the list experiment. One can see that for three of the political attitudes, there is no significant effect of providing respondents with "cover" for expressing their views: this is true even for self-reported support for Hong Kong's independence — a much more extreme political position than simply attending a July 1 March.

which of 28 groups' crowds they joined at the protest (we also gave them the option of "Others"). Next, we asked for general impressions of the protest in an open-ended manner, subject to a 300 word maximum. Finally, we asked about the number of their friends who attended the protest. Because these questions were only asked of individuals who attended the March, we do not consider them outcome variables in this analysis.

[34] This discussion of Hong Kong students' willingness to report their political attitudes and behavior truthfully closely follows Cantoni et al. (2016).

Observing that the provision of "cover" by the list experiment does not affect estimated support for several sensitive attitudes, one might have been concerned that the "cover" provided by the list experiment was insufficient to elicit truthful reporting. However, Table 2 does show one significant difference between direct elicitation and list experiments: many students in our sample support the use of violence in order to achieve Hong Kong's political rights, but are afraid to say so when directly asked. Finding a significant gap between direct questions and the list experiment on this dimension suggests that subjects *do* value the cover provided by the list experiment when it is needed — but it is not needed in response to political questions within the range of non-violent opposition to the CCP. Because participation in the July 1 March falls within this non-violent range, we are confident that subjects are willing to respond truthfully in response to direct questions about their participation.

4 Main results

4.1 The first stage: effects on posterior beliefs

We begin by presenting the effects of the information treatment on individuals' beliefs regarding actual participation in the July 1 March — the "first stage." Our focus is on posterior beliefs regarding the percentage of other experimental subjects who would actually participate in the July 1 March, rather than on beliefs regarding the total number of participants in the Hong Kong population. The former beliefs are more directly linked to the information provided regarding subjects' planned participation. We will also present some evidence on posterior beliefs regarding total turnout among HK citizens.

The effect of the information treatment — reminding treatment group subjects of their prior beliefs regarding other subjects' planned participation and informing them that 17% of experimental subjects planned to attend the protest — can be seen in the distributions of beliefs regarding subjects' *actual* participation, presented in Figure 2. Given that the information we provided to subjects was above the prior beliefs of some (regarding planned participation) and below the prior beliefs of others, if subjects believed that the information provided was truthful, and updated their priors regarding actual participation in the direction of the new information, one would expect to see a more compressed distribution of posteriors in the treatment group than in the control group. Indeed, this is precisely what one observes in Figure 2: one can see in the figure that the treatment group's posteriors are distributed much more tightly between 10% and 20%.[35]

We next more closely examine the anticipated heterogeneous effects of the information treatment depending on subjects' prior beliefs. In Figure 3, we present a binned scatter plot of the change in beliefs (posteriors minus priors) regarding other subjects' *actual* participation against

[35] A Kolmogorov-Smirnov test of equality of posterior distributions between the treatment and control groups strongly rejects the null ($p < 0.001$).

subjects' priors regarding other subjects' *planned* participation. In the left-hand panel, one can see that, as predicted, subjects in the treatment group with priors regarding planned participation below (above) the information provided consistently updated their beliefs regarding other subjects' actual participation positively (negatively). Subjects in the treatment group with priors more distant from the information provided updated their beliefs more than those with priors closer to the information provided. Individuals in the control group with lower priors tended to update their beliefs positively, and vice versa, but the changes in beliefs are tiny compared to those observed in the treatment group (see the right-hand panel of Figure 3).[36]

Observing that belief updating in the treatment group systematically differs in sign between subjects with priors above and below the information provided, we now present, in Figure 4, the prior and posterior beliefs regarding other subjects' actual participation for the treatment and control groups, split by priors regarding planned participation above and below the information treatment. Recall that treatment and control groups are generally balanced on observable characteristics within each of these subsets (see Table 1, columns 4–9). One can see in the figure that among individuals with priors regarding other subjects' planned participation below (above) the true level, there is a significantly greater increase (decrease) in posteriors among the treatment group than among the control group.

We next estimate regression models predicting posterior beliefs regarding other subjects' actual participation as a function of treatment status, controlling for individuals' prior beliefs regarding others' actual participation.[37] To begin, we pool all subjects, but we code the treatment variable as being equal to -1 for individuals whose prior beliefs regarding the *planned* participation of other subjects were above the truth, in order to make the treatment effect monotonic. The coding of the treatment variable reflects our strong priors, as well as the evidence presented in Figure 3, that individuals in the treatment group update their beliefs about the *actual* participation of others in opposite directions depending on whether their prior beliefs about the *planned* participation of others were above or below the information provided. One can see in Table 3, Panel A, column 1, that the experimental treatment statistically significantly moved beliefs, by just over 3 percentage points.[38]

We next split the experimental sample into groups with priors regarding planned participation

[36]The updating of beliefs among the control group may result from newly acquired information from outside the study, or from information spilling over from the treatment group; such information spillovers would tend to bias estimated effects (in both the "first stage" and the "reduced form") toward zero.

[37]In Online Appendix Table A.6 we present an alternative specification, in which the outcome variable is a subject's change in beliefs (posteriors minus priors) regarding other subjects' actual participation. We focus on belief levels in the main text as this specification is less restrictive on the coefficient on prior beliefs and because models of protest participation typically focus on belief levels, rather than belief changes, as the drivers of protest participation.

[38]We explore alternative "switching points" for the treatment variable (i.e., different levels of priors above which we code treatment equal to -1) in Online Appendix Figure A.4. Specifically, we estimate the specification presented in Table 3, Panel A, column 1, and plot the point estimate (and 95% confidence interval) for integer "switching points" from 0 to 100. One can see in the left-hand panel of Online Appendix Figure A.4 that we find the strongest first stage effects when the coding of the treatment indicator switches at 17%.

above and below the true level of planned participation of 17%. In Table 3, Panel A, columns 2 and 3, one can see statistically significant belief updating in each subsample. Among individuals with prior beliefs regarding planned participation of other subjects below (above) the truth, treatment increases (decreases) beliefs regarding other subjects' actual turnout by around 4.5 (9.5) percentage points. A test of equality of the coefficients estimated in Table 3, Panel A, columns 2 and 3, is rejected with $p < 0.001$.

We explore the robustness of the first stage estimates along two margins in Table 3. First, we examine whether controlling for subject characteristics affects the estimated treatment effects. One can see in Table 3, Panel B, columns 1–3, that controlling for subjects' background characteristics and economic status (variables presented in Table 1) does not meaningfully affect the estimated treatment effects in either the pooled regression or the split sample regressions. As a second robustness exercise, we consider the possibility that our results are strongly influenced by individuals with extreme priors; we thus drop from our sample the 5% of subjects with the lowest prior beliefs and the 5% of subjects with the highest prior beliefs regarding the planned participation of others. One can see in Table 3, Panel C, columns 1–3, that dropping individuals with extreme priors does not greatly affect our results (though the coefficient estimated from the sample with priors above 17% is somewhat attenuated).

An additional important consideration is whether our statistical inferences based on traditional standard errors are sound.[39] As an alternative, we randomly assign (fictional) treatment status (in the same 2/3 treatment, 1/3 control ratio used in the actual experiment) and estimate first stage treatment effects 10,000 times each for the subsample of subjects with prior beliefs regarding others' planned participation below 17% and the subsample with prior beliefs regarding others' planned participation above 17%. We can then compare the t-statistics from the estimated treatment effects from the fictional treatment assignments to the t-statistics from the *actual* treatment assignment (the actual estimates are those in Table 3, Panel A, columns 2–3). We find that our p-values using randomization inference, based on two-sided tests, are very similar to those using standard inference (see Figure 5).

Experimental subjects who updated their beliefs regarding other subjects' turnout at the protest also may have updated their beliefs regarding the turnout of Hong Kong citizens more generally. We thus next examine the effect of the treatment on subjects' beliefs regarding protest turnout among the entire Hong Kong population. We replicate the specifications in Table 3, columns 1–3, but using as our outcome the posterior beliefs regarding the total turnout in the July 1 protest of 2016 (and controlling for prior beliefs regarding the total turnout at the protest). In Table 3, columns 4–6, one can see suggestive evidence that the treatment affected beliefs regarding total protest size in the same direction as it affected beliefs regarding other subjects' turnout.

[39] See Deaton and Cartwright (2016) for a discussion of challenges to statistical inference in randomized control trials.

4.2 The reduced form: effects on protest turnout

We now turn to examining the effects of the information treatment on individuals' own protest participation. As we did in the analysis of the first stage, we split the experimental sample into two groups: first, subjects whose prior beliefs regarding other subjects' planned turnout were below the truth; and, second, subjects whose prior beliefs regarding other subjects' planned turnout were above the truth. In the previous section we saw that in the former group, the treatment increased beliefs regarding other subjects' turnout, while in the latter group, the treatment reduced beliefs regarding other subjects' turnout.

In Figure 6, we present turnout levels among subjects in the treatment and control groups in the two subsamples split according to priors. One can see in the figure that in the subsample whose priors were below the truth, the information treatment caused a statistically significant fall in turnout; in the subsample whose priors were above the truth, the information treatment caused a statistically significant increase in turnout. In other words, we find evidence that the protest decision is a *negative* function of beliefs regarding the turnout of others. It is worth emphasizing that this relationship is found in independent tests on two distinct subsamples: both the subsample with prior beliefs below the information provided, and the subsample with prior beliefs above. The protest game in this setting is one of *strategic substitutes*.

We next turn to regression analysis of the reduced form relationship between treatment and protest participation. As in the first stage analysis, we begin by pooling all subjects, coding the treatment variable as being equal to -1 for individuals whose prior beliefs regarding the planned participation of other subjects were above the truth, in order to make the treatment effect monotonic. In Table 4, Panel A, column 1, one can see that the treatment causes a statistically significant 2.7 percentage point change in turnout in the opposite direction of the change in beliefs.[40] We then, in Table 4, Panel A, columns 2–3, split the experimental sample into groups with priors regarding planned participation above and below the true level of planned participation of 17%, and find significant effects in each subsample, matching the results shown in Figure 6 (a test of equality of the coefficients estimated in Table 4, Panel A, columns 2 and 3, is rejected with $p < 0.001$).

As in the first stage analysis, we address concerns about statistical inferences based on traditional standard errors in our study of the reduced form effects. We again use randomization inference as an alternative, randomly assigning (fictional) treatment status and estimating reduced form treatment effects 10,000 times each for the subsample of subjects with prior beliefs regarding others' planned participation below 17% and the subsample with prior beliefs regarding others' planned participation above 17%. We then compare the t-statistics from the estimated treatment effects from the fictional treatment assignments to the t-statistics from the *actual* treatment assign-

[40] As was done in the first stage analysis, we explore alternative "switching points" for the treatment variable (i.e., different levels of priors above which we code treatment equal to -1) in Online Appendix Figure A.4. Specifically, we estimate the specification presented in Table 4, Panel A, column 1, and plot the point estimate (and 95% confidence interval) for integer "switching points" from 0 to 100. One can see in the right-hand panel of the figure that we find the strongest reduced form effects when the coding of treatment switches in the range between 15% and 30%.

ment (the actual estimates are those in Table 4, Panel A, columns 2–3). We again find that our p-values using randomization inference, based on two-sided tests, are very similar to those using standard inference (see Figure 7).

The estimated reduced form treatment effects in Table 4, Panel A, are not only statistically significant, but also indicate a substantively significant effect of the information treatment on political behavior. One can see this by comparing the variation in protest participation explained by the treatment to the variation explained by the rich set of additional individual covariates we collected in Part 1 of the experiment. In Online Appendix Table A.7, one can see that the treatment actually has *greater* explanatory power than economic preferences, personality traits, cognitive ability, background characteristics, or economic status.

The magnitude of the treatment effect can also be benchmarked against estimated political mobilization effects in the existing literature in economics and political science. We follow DellaVigna and Gentzkow (2010) in calculating the persuasion rate implied by our treatment: the fraction of individuals who participate in the protest when treated, but who would not have turned out to protest in the absence of the treatment. Among the entire sample, we find a persuasion rate of nearly 3%; among the subsample of individuals with priors above the information we provided — whom the treatment moved in the direction of greater participation — we find a persuasion rate of 6.5%. DellaVigna and Gentzkow (2010) calculate persuasion rates for 7 interventions that stimulate turnout, from get out the vote campaigns to media content; our treatment, though it did not directly encourage protest turnout, has effects that exceed most of the impersonal persuasive messages, for example, TV content or mass mailings encouraging voter turnout.[41]

In Table 4, Panels B and C, we conduct robustness exercises analogous to those considered in the first stage analysis. One can see that neither including controls for subjects' prior beliefs regarding other subjects' planned participation, subjects' background characteristics, and economic status (Panel B), nor trimming the 5% of subjects with the lowest and the 5% of subjects with the highest prior beliefs regarding planned participation (Panel C), greatly affects the estimated reduced form treatment effects.

We explore two additional dimensions of robustness in Online Appendix Table A.8. First, we consider sample restrictions depending on subjects' own plans to participate in the July 1 March. One might believe that individuals who reported, without any uncertainty, a plan *not* to attend the March (in Part 1 of the experiment) would be unlikely to have had their protest decision affected on the margin by the experimental treatment. Consistent with this expectation, dropping this group from our analysis results in larger estimated treatment effects than in the baseline specification. In contrast, one might expect individuals who reported, without any uncertainty, a plan

[41] The information treatment is *not* as effective as more personal interventions (e.g., door-to-door canvassing). For comparison, the findings in Gerber and Green (2000) imply persuasion rates of 1% for impersonal get out the vote mailings and 15.6% for door-to-door visits. The magnitude of the treatment effect is sensible given the nature of our interactions with the students in our sample: our repeated interactions were more personal than a mass mailing or mass media campaign, but were less personal than a face-to-face interaction.

to attend the March would most likely to be closer to the margin — either confirmed in their plans or deterred from them by the information treatment. Consistent with this logic, when we drop subjects who planned to attend the March, we find attenuated (though still statistically significant) treatment effects. A second exercise considers an alternative construction of the outcome variable: an indicator of a change from plans in Part 1 of the experiment (no certain plan to attend) to behavior in Part 3 (actual attendance at the March). We find qualitatively similar results when we consider "changed plans" as the outcome variable.

We next examine heterogeneity in the reduced form treatment effect associated with subjects' priors regarding the planned participation of others. In Figure 3 one could see that the largest first stage effects were produced among individuals with prior beliefs far from the information provided by the experimental treatment. We assess whether the largest reduced form effects are also seen among those subjects whose prior beliefs were far from the true level of 17%. To do so, we regress protest participation on the interaction between a treatment group dummy variable and 5-percentage-point bins of priors regarding other subjects' planned participation (as well as lower-order terms). In Figure 8, one can see the estimated coefficients on the interaction terms as well as their 95% confidence intervals. The effect of the treatment on protest turnout was, indeed, greatest among individuals whose priors were furthest from the information provided. These findings provide reassuring evidence of consistency between the first stage effects and the reduced form.[42]

Of course, the variation across subjects in their prior beliefs regarding others' planned participation is not experimentally induced, and is correlated with other variables that also shape the decision to protest (recall that predictors of prior beliefs can be seen in Online Appendix Table A.5). We next explore the extent to which variation in individual characteristics — rather than variation in prior beliefs — is likely to be behind the heterogeneous treatment effects observed in Figures 3 and 8. We begin, in Table 5, Panel A, column 1, by estimating a linear model of heterogeneous *first stage* treatment effects associated with subjects' prior beliefs regarding other subjects' planned participation (roughly, fitting a line through Figure 3). As expected, we estimate a significant, positive y-intercept, and a significant, negative coefficient on the interaction between the treatment dummy and prior beliefs. In Table 5, Panel B, column 1, we estimate a linear model of heterogeneous *reduced form* treatment effects associated with subjects' prior beliefs regarding other subjects' planned participation (roughly, fitting a line through Figure 8). As expected, we estimate a significant, negative y-intercept, and a significant, positive coefficient on the interaction between the treatment dummy and prior beliefs.

We next exploit our rich information about subjects to predict their priors using a linear model including all 47 factors listed in Online Appendix Table A.5. These *predicted* priors represent "prob-

[42] A less parametric analysis of heterogeneity in the treatment effect associated with subjects' priors — locally-weighted regression estimates of the treatment effect across subjects' priors — can be seen in Online Appendix Figure A.5. The patterns are broadly similar to those in Figure 8.

lematic" variation in beliefs: heterogeneity in treatment effects associated with predicted priors may be driven by subjects' observable characteristics rather than by beliefs. Having calculated for each individual their predicted priors, we also are left with their *residual* priors — the component of prior beliefs unexplained by the large set of observable characteristics.[43] In Table 5, Panels A and B, column 2, we estimate linear models of heterogeneous first stage and reduced form treatment effects associated with subjects' *predicted* prior beliefs; these estimates are of the same sign as, but are somewhat less precise than, estimates using actual priors. In Table 5, Panels A and B, column 3, we estimate linear models of heterogeneous first stage and reduced form treatment effects associated with *both* subjects' predicted prior beliefs and residual priors. One can see that while there exists statistically significant heterogeneity in the treatment effect associated with the "problematic" variation in predicted priors, there also exists statistically significant heterogeneity in the treatment effect associated with just the *residual* priors. This provides reassurance that the heterogeneous treatment effects we observe are indeed driven by variation in beliefs.

4.3 Two-stage estimates: the effects of beliefs on turnout

Thus far we have shown that providing information regarding the true level of planned protest turnout among our experimental sample caused: (i) beliefs regarding actual turnout to change; and, (ii) subjects' own turnout to change, with beliefs and turnout moving in opposite directions. We next combine the two effects — first stage and reduced form — in a two-stage analysis that allows us to estimate the effect of beliefs regarding others' turnout on one's own turnout. It is worth emphasizing that one should not interpret the two-stage estimates too literally: we have already shown that the information treatment affected beliefs regarding the turnout of both other experimental subjects and Hong Kong citizens more generally, so we cannot estimate the "pure" casual effect of a one percentage point change in beliefs regarding other subjects' turnout.[44] With this caveat in mind, we still believe this exercise is instructive. Specifically, we predict individuals' protest turnout using their posterior beliefs regarding other experimental subjects' actual participation, instrumenting for the latter with the treatment dummy (when examining the pooled sample, we instrument for posteriors setting the treatment equal to -1 for individuals with prior

[43] Note that it is not the case that heterogeneity in treatment effects associated with predicted priors is necessarily driven by observables, rather than by beliefs, but simply that this cannot be ruled out. Note, too, that heterogeneity in treatment effects associated with residual priors is not necessarily driven by beliefs alone, as it might be driven by unobserved characteristics. Our rich data collection makes the latter less of a concern, but does not eliminate it.

[44] Another important "reference population" whose turnout might affect subjects' own is subjects' close friends. While we do not observe prior and posterior beliefs regarding close friends' turnout, it is worth noting that we do not expect these beliefs to have been affected by our experimental intervention: students likely knew well whether their close friends would attend the July 1 March regardless of their treatment status. We explore heterogeneity in the first stage and reduced form treatment effects depending on whether subjects reported in Part 1 of the study the expectation that a close friend would participate. We find that first stage treatment effects are not significantly different depending on close friends' attendance, but the reduced form treatment effects are larger among individuals' who expected a close friend to attend (see Online Appendix Table A.9). This reflects the fact that individuals on the margin of attendance (and so more responsive to the treatment) were differentially those who expected their friends to attend the July 1 March as well.

beliefs regarding the planned participation of others greater than 17%).

We begin by estimating the two-stage model using the pooled sample, controlling only for subjects' prior beliefs regarding other subjects' actual turnout. In Table 6, Panel A, column 1, one can see that we estimate just under a one-half percentage point decrease in one's own turnout for every percentage point increase in one's posterior beliefs regarding the turnout of others. Table 6, Panel A, columns 2–3, we split the experimental sample into groups with priors regarding planned participation above and below the true level of planned participation of 17%, and find significant effects in each subsample. It is worth noting that while we had found larger first stage and reduced form treatment effects in the subsample with priors above 17% (see Tables 3 and 4), in Table 6, Panel A, columns 2–3, we find very similar estimated effects of beliefs about others' turnout on one's own turnout in the two subsamples — around half a percentage point effect (a test of equality of the coefficients estimated in Table 4, Panel A, columns 2 and 3, fails to reject the null, with $p = 0.535$).

In Table 6, Panels B and C, we conduct robustness exercises analogous to those considered in the first stage and reduced form analyses above. One can see that neither including controls for subjects' background characteristics and economic status (Panel B), nor trimming the 5% of subjects with the lowest and the 5% of subjects with the highest prior beliefs regarding planned participation (Panel C), greatly affects the estimated effects of beliefs on protest turnout. Finally, in Online Appendix Table A.10, we replicate Table 6, but consider belief *changes*, rather than levels, as the endogenous regressor of interest (for which we use treatment dummy as an instrument). We find similar results using this alternative specification.

4.4 Internal validity

We next address several concerns regarding the internal validity of our estimates. First, as noted above, we found that our treatment and control groups were unbalanced on 8 of 49 subject characteristics elicited in Part 1 of the study (summary statistics for these variables and t-tests of equality of means between treatment and control groups are presented in Online Appendix Table A.1). To determine whether observable differences between treatment and control groups might drive our results, we estimate our baseline first stage and reduced form specifications (shown in Table 3, Panel A, column 1 and Table 4, Panel A, column 1, respectively; replicated in Online Appendix Table A.2, column 1), but controlling for the interaction between each unbalanced characteristic and the treatment dummy. One can see in Online Appendix Table A.2, columns 2–9, that controlling for these interactions leaves our treatment effect estimates practically unaffected.

A second concern is that selective attrition between Part 1 of our experiment and Part 3 might affect our estimates (though our finding of insignificant differences between the Part 1 sample's characteristics and the Part 3 sample's, in Online Appendix Table A.3 is reassuring). To address concerns about selective attrition, we re-weight the sample of individuals who completed Part 3 of the study to match the sample of individuals who completed Part 1 (but not necessarily the rest)

of the study. We match the Part 1 sample on subjects' gender, birth year, childhood environment, religiosity, household income, and economic status. In Online Appendix Table A.11, one can see that our estimated first stage and reduced form treatment effects are largely unaffected by this re-weighting exercise.[45]

Another possibility is that information regarding other subjects' protest plans not only affected beliefs about others' participation, but also affected beliefs about the political movement itself — as if the protest were a consumption good with uncertain quality. However, in this case, the standard social learning logic would suggest that positive updating of beliefs regarding the number of other subjects joining a protest should lead students to update *positively* about protest "quality." This would produce the appearance of strategic *complementarity*, not the strategic substitutability that we observe.

Standard experimenter demand effects, too, would likely produce the appearance of strategic complementarity — when the experimenter tells a subject that a behavior is more prevalent than the subject expected, the subject seems likely to assume that the experimenter is trying to induce that behavior, and so be more likely to report it. Further arguing against experimenter demand effects (or other unintended consequences of the experimental treatment) driving our findings, we also find evidence consistent with strategic substitutability in the protest decision even in the *absence* of any experimental treatment. Simply examining naturally-occurring variation in beliefs among our control group subjects one sees a negative relationship between posterior beliefs regarding the actual participation of others and subjects' own participation in the July 1 March (see Online Appendix Figure A.6).[46]

5 Discussion

Our experiment estimates the causal effect of beliefs about others' protest participation on one's own, exploiting variation in beliefs driven by a specific level of planned turnout, from a single protest, in a particular setting. One naturally wonders how to generalize from such an estimate. In Section 2.2, we discussed several important dimensions along which Hong Kong's July 1 Marches share similarities with other anti-authoritarian protests: as in other protests, the Hong

[45] In Online Appendix Table A.11 we also replicate our main results, but weighting observations to match the HKUST student population. We again find that our results are essentially unchanged. It is important to note that our re-weighting exercises can only provide evidence of similar observable characteristics between our experimental sample and the reference samples: the HKUST student population or the sample of individuals who completed Part 1 of the study. We cannot rule out unobservable differences between these groups, though the similarity of observable characteristics is reassuring.

[46] We also observe a negative relationship between subjects' self-reported past protest participation and their beliefs about other subjects' support for the anti-authoritarian movement, in responses recorded in Part 1 of the study (see Online Appendix Figure A.7). It is important to emphasize that, while suggestive, the correlations observed in Online Appendix Figures A.6 and A.7 must be interpreted with caution. Among other concerns, the levels of posterior beliefs in the control group are quite strongly correlated with their accuracy, and subjects with more accurate beliefs may turnout more than others for reasons other than strategic considerations. Past protest participation may shape beliefs about others' ideology, in addition to being shaped by them.

Kong Marches aim to achieve concrete political changes by mobilizing large numbers of individuals, with these individuals facing a potential government crackdown. Thus, on its face, there is reason to expect that protest behavior in the July 1 March will have some degree of external validity to other anti-authoritarian protests.

Yet, it must be acknowledged that even in the same context we study, even in the specific protest we study, it is possible that at a different level of protest participation the causal effect of beliefs about others' turnout on one's own could differ — indeed, it may even flip signs. Given the possible heterogeneity in the treatment effect we estimate, an important question is to what extent does our single empirical parameter estimate bound the set of theoretical models of protest participation?

Although our estimate does not pin down a single model of protest participation, we argue that it does provide important bounds on the set of models consistent with our estimate: much recent theoretical work on protest participation assumes *only* the possibility of strategic complementarity in the protest decision. This assumption has become so widespread that we treat it as a "benchmark" model in what follows. Our findings reject this benchmark model. Although we cannot claim that a specific alternative model produces our findings of strategic substitutability, we provide evidence consistent with three plausible mechanisms that could generate strategic substitutability in our setting, and that could easily be incorporated into the benchmark model to allow for the strategic substitutability that we find.

5.1 The benchmark model

The recent political economy literature has typically modeled protest participation as a global game (or similar), with the stage game featuring strategic complementarity, and by assumption ruling out the possibility of strategic substitutes. Here we present a simple, but general, version of the "benchmark" model of the stage game, and relate it to several recent papers that emphasize different underlying sources of strategic complementarity.

In the benchmark model, individual i's' utility from protest participation can be written as follows:

$$U_i = \mathbb{1}_{P_i=1}\Big(V_i\Big(P_{-i}, S(P_{-i})\Big) - C_i\Big(P_{-i}, S(P_{-i})\Big)\Big),$$

where $\mathbb{1}_{P_i=1}$ is an indicator that individual i participated in the protest and the utility from participating (U_i) is a function of the benefits from participation (V_i) and the costs (C_i). Costs and benefits, in turn, are assumed to depend directly on the participation of other individuals (the level of which is denoted P_{-i}), as well as on the success of a protest (an indicator denoted S), which itself may depend on the participation of others.

The benchmark model first follows the recent literature in assuming that protests are more likely to succeed when they are larger, and that individuals derive differential utility from par-

ticipating in protests that succeed (or that are more likely to succeed). Bueno de Mesquita (2010, p. 449), for example, assumes that "there is some portion ... of the payoffs from regime change that can only be accessed by those who participate ... Substantively, this could be because those who actively participate in revolution gain privileged status after regime change occurs or because there are expressive benefits to having participated in a victorious uprising." Edmond (2013, p. 1425) writes simply that "the more citizens participate in [a protest], the more likely it is that the regime is overthrown and so the more likely it is that any individual also participates." Passarelli and Tabellini (2017, p. 910) write that individuals may derive greater utility from the "feeling of contributing to a more meaningful event with a greater chance of success." Barberà and Jackson (2017, pp. 7–8) assume that individuals attain utility "from knowing or being able to say that they participated in a revolution that was successful: from having been one of those who stormed the Bastille, participated in the Salt March, or protests in Tunisia, etc."

Formally, the benchmark model assumes:

$$\frac{\partial S}{\partial P_{-i}} > 0 \qquad \text{and} \qquad \frac{\partial V}{\partial S} > 0.$$

It is worth noting that the crucial assumption that a protest is perceived to be more likely to succeed when more people turn out (i.e., $\frac{\partial S}{\partial P_{-i}} > 0$) receives empirical support in our experimental context. We elicit experimental subjects' beliefs about the likelihood that a Hong Kong pro-democracy protest would succeed as a function of protest turnout, and find that these are strongly increasing (see Figure 1).

The benchmark model also assumes that the benefits from participating in a protest are larger when it is attended by more people — even if the increased participation does not affect the protest's success. This may be, for example, because participation in a larger protest increases the expressive utility a participant attains. In a model of citizen unrest in response to perceived unfair government policy, Passarelli and Tabellini (2017, p. 910) write that "the psychological benefit of a public display of anger is stronger if the emotion is more widely shared."

Thus, whether because of direct effects, or because of indirect effects working through protest success, the benchmark model assumes that the benefits of protest attendance increase with the attendance of others — thus generating strategic complementarity. Formally:

$$\frac{\mathrm{d}\, V}{\mathrm{d}\, P_{-i}} > 0.$$

The benchmark model assumes similar effects of others' turnout arising from the costs side. The cost of protest attendance may be lower when protests are successful — perhaps because in a successful protest the regime concedes, rather than cracks down. If, as assumed above, protest success is an increasing function of turnout, then greater turnout will lower protest costs, alongside increasing benefits. The cost of attendance may also be falling in protest size independent of success if costs per person are lower in a larger crowd. For example, Passarelli and Tabellini (2017,

p. 910) write that "[strategic] complementarity could also be on the cost side: the probability of being arrested is smaller in a larger crowd."

Thus, the benchmark model assumes:

$$\frac{\partial C}{\partial S} < 0.$$

This assumption, along with the above assumption that $\frac{\partial S}{\partial P_{-i}} > 0$, as well as direct reductions of costs arising from larger turnout imply another force generating strategic complementarity:

$$\frac{\mathrm{d}\, C}{\mathrm{d}\, P_{-i}} < 0.$$

In sum, the benchmark model — synthesizing several recent papers — allows for different underlying sources of strategic complementarity.[47] All of these generate a common prediction: if an individual believes that the turnout of others will be greater, she is unambiguously more likely to turn out to a protest herself.

5.2 Sources of strategic substitutability in the protest decision

Our findings reject the benchmark model of protest turnout that assumes only the possibility of strategic complementarity in the decision to protest. It is important to emphasize that we do not claim that protests are always games of strategic substitutes, but rather that models of protest turnout must allow for the possibility of strategic substitutability. We propose adding to the benchmark model's stage game utility function a term that allows for strategic substitutability: specifically, a term whose first derivative with respect to protest size is negative, at least over some range.

While we cannot conclusively identify specific mechanisms that produce the pattern of strategic substitutability we observe, we are able to provide suggestive evidence of three plausible drivers: first, that protests produce a public good from attaining a threshold level of participation; second, that an individual's expected costs of protest turnout increase with protest size; and, third, that an individual's benefits from signaling her anti-authoritarian type decrease with protest size.

Threshold public goods as a function of turnout

As described above, the benchmark model assumed $\frac{\mathrm{d}\, V}{\mathrm{d}\, P_{-i}} > 0$, arising from several sources of differential utility experienced when participating in larger, more successful protests. However, there may be benefits from one's own protest turnout that over some range are *decreasing* in the turnout of others. A classic argument in the political economy literature (Olson, 1965; Tullock,

[47] This literature (Bueno de Mesquita, 2010; Edmond, 2013; Barberà and Jackson, 2017; Passarelli and Tabellini, 2017) is discussed in more detail in Online Appendix B.

1971; Palfrey and Rosenthal, 1984) is that protest participation produces a political public good. This good might be tangible: attaining a sufficient turnout level might send the regime a signal that directly affects policy (for example, in the 2017 March, significant turnout might have persuaded CY Leung not to run for a second term as Chief Executive). The public good produced by a protest might also be symbolic: sufficient turnout might signal the existence of a "critical mass" of protesters, which sustains the political movement; note that this signal is not inconsequential, as it shapes subsequent protest turnout in the political movement.[48]

We find evidence consistent with protest turnout being a public good in our setting. We construct an index of subjects' pro-sociality (altruism and reciprocity) using their responses in Part 1 of the study. We find that this pro-sociality index is significantly correlated with protest turnout unconditionally, as well as conditional on treatment status, posterior beliefs about others' turnout, and on subjects' own anti-authoritarian ideology (see Online Appendix Table A.12). This is consistent with a theoretical and empirical literature linking pro-sociality to public goods contributions (see, e.g., Croson, 2007).

We can incorporate the production of a public good — either an intermediate, tangible public good (something meaningful but short of democratization) or a symbolic public good — from sufficient protest turnout into our model by adding a term, $\phi(P_{-i})$, to the benefits function:

$$V_i = V_i\Big(S(P_{-i}), \phi(P_{-i})\Big)$$

In this case, the benefits from turnout are a function of: *(i)* the success of the protest (i.e., achieving democratization) $S(P_{-i})$, which is monotonically increasing in P_{-i} and will tend to produce strategic complementarity; and, *(ii)* the production of an intermediate tangible public good or a symbolic public good $\phi(P_{-i})$, which could be increasing in P_{-i} over some range (i.e., below the level at which the public good is completely produced), but decreasing in P_{-i} around the point at which individuals believe their turnout is no longer needed to produce the public good.

Over some range, particularly in a protest in which the production of the public good (e.g., signaling the movement's strength) is more relevant than the achievement of the ultimate success of the movement (e.g., achieving democratization), the production of a public good from protest participation may generate strategic substitutability.[49] Thus, in this case, $\frac{\mathrm{d}V}{\mathrm{d}P_{-i}}$ may be positive or negative, thus allowing for strategic complementarity or substitutability.

Increasing costs as turnout rises

The benchmark model assumed that an individual's expected cost of protest attendance was falling with the expected turnout of others. The logic was that the costs of government crack-

[48] Barberà and Jackson (2017) present a model in which protest turnout in one period affects beliefs about eventual movement success, and thus turnout, in subsequent periods.

[49] Note that a public goods game may not be a game of strategic substitutes in all settings: subjects may play a strategy of conditional cooperation, which generates strategic complementarity (see, e.g., Fischbacher et al., 2001).

down would be diffused across more people, thus reducing an individual's own cost. However, this logic ignores the possibility that the likelihood of a government crackdown is increasing in protest size, at least over some range. Consistent with this proposed mechanism behind strategic substitutability in our setting, one can see in Figure 1 that our experimental subjects believe that government crackdown is an increasing function of protest size.

This source of strategic substitutability can easily be incorporated into the benchmark model. Suppose the cost of turnout is a function of: *(i)* the cost borne by a protester conditional on crackdown, $c_i(P_{-i})$; *(ii)* the success of the protest, $S(P_{-i})$; and *(iii)* the probability of crackdown, $\pi(P_{-i})$. Then, the cost function would be:

$$C_i = C_i\Big(\pi(P_{-i}) \times c_i(P_{-i}), S(P_{-i})\Big)$$

with $\frac{\partial c_i}{\partial P_{-i}} < 0$ (following the logic of the benchmark model), $\frac{\partial S}{\partial P_{-i}} > 0$ (again, following the benchmark model), and $\frac{\partial \pi}{\partial P_{-i}} > 0$. In this case, $\frac{\mathrm{d}\, C}{\mathrm{d}\, P_{-i}}$ may be positive *or* negative, allowing for strategic complementarity or substitutability.

Decreasing signaling value of participation as turnout rises

We finally propose an additional element of the benefits of protest participation, which may be greater when participating in smaller protests: if one cares about signaling one's anti-authoritarian type, the value of that signal, $\theta_i(P_{-i})$, may be decreasing in protest size, at least over some range.[50]

We find several pieces of suggestive evidence consistent with individuals' protest participation being shaped by their political identities (i.e., "types"), and the signaling of those identities. First, we find that students' anti-authoritarian identities (both self-reported and revealed in incentivized lab experiments) are strongly associated with their protest participation plans (see Online Appendix Table A.5). In a subsequent survey of participants in the July 1, 2017, March in which we directly asked protest participants why they attended the March, 45% of participants responded, "Being politically active is an important component of my identity."[51]

[50] We follow the logic of Bénabou and Tirole (2011), who present a model in which an individual trades off honor from taking a costly, meritorious action against stigma from not taking that action. When fewer individuals undertake the costly action, it increases the honor associated with taking the action (because the signal of one's type conditional on taking the action becomes more positive), but also reduces the stigma associated with not taking the action (because the signal of one's type conditional on not taking the action becomes less negative). Under a small set of assumptions, the honor associated with taking a less common action increases more than the stigma from not taking the action falls when the base rate of taking the action is rare. The logic is as follows: when a meritorious action is taken by a small minority in the tail of a distribution, the conditional mean moving further into the tail changes more in the minority tail of the distribution than in the majority. This model produces strategic substitutes in taking the meritorious action when starting from a low base, but strategic complements when starting from a high base. Enikolopov et al. (2018) present a dynamic model in which social image considerations motivate persistent protest participation over time.

[51] The participants in the 2017 survey included some individuals surveyed as part of our experiment conducted in 2016 as well as additional respondents. Among the 2017 survey sample, we directly elicited all 59 protest participants' reasons for attending the 2017 March. Individuals' identity was the second most frequent response, with 27 individuals reporting it. The most frequent response was, "I believed the March would produce political change," reported by 28 individuals.

If subjects selected into smaller protests in response to the perceived signaling value, one would expect individuals with very anti-authoritarian friends — to whom signaling one's type would be especially valuable — to have differentially attended the July 1 March after updating their beliefs about total protest size downward. Analogously, one would expect that individuals who attend a protest specifically to signal their type to their anti-authoritarian friends to select out of attendance after updating their beliefs about total protest size upward. Consistent with this prediction, we find that treatment group protest participants with high priors regarding other subjects' planned participation — a group who turned out to protest after updating their beliefs regarding protest turnout downward — have more anti-authoritarian friends than protest participants in the control group (see Online Appendix Figure A.8). We find analogous results among individuals selecting out of protests when updating their beliefs about turnout upward. This suggests that subjects who had the strongest social image reasons to signal their anti-authoritarian identities differentially selected into protests they believed would be smaller, and selected out of protests they believed would be larger.

We can incorporate the signaling value of protest attendance into a benefits function alongside a threshold public good. Suppose the benefits from turnout are a function of: *(i)* the success of the protest, $S(P_{-i})$, discussed above; *(ii)* the production of a public good, $\phi(P_{-i})$, also discussed above; and, *(iii)* the value of the signal of one's type, $\theta_i(P_{-i})$. Then, the benefits function would be:

$$V_i = V_i\Big(S(P_{-i}), \phi(P_{-i}), \theta_i(P_{-i})\Big)$$

with $\frac{\partial V}{\partial S} > 0$ (as in the benchmark model), $\frac{\partial \phi}{\partial P_{-i}} < 0$ (over some range), and $\frac{\partial \theta}{\partial P_{-i}} < 0$ (when P_{-i} is small enough). In this case, $\frac{\mathrm{d}\, V}{\mathrm{d}\, P_{-i}}$ may be positive or negative, again allowing for strategic complementarity or substitutability.

Thus, while only suggestive, the patterns in our data are consistent with plausible mechanisms underlying strategic substitutability in our setting. Importantly, these mechanisms can easily be incorporated into the benchmark model to allow for either strategic complementarity or substitutability in the protest decision, making it consistent with our main findings.[52]

[52]There exist other potential mechanisms that could generate strategic substitutability for which we do not find evidence in our setting (though they may be present in other protests). Shadmehr and Bernhardt (2011) propose a model in which moderates update their beliefs about the gains from protest negatively if they believe other agents' thresholds for turnout are too low. This makes moderates select out of protesting when they update their beliefs positively — the opposite of what we find in Online Appendix Figure A.8. Myatt (2017) proposes a model in which protest voters may moderate their expression if they believe that extremists are too popular. Again, this model would suggest that moderates select out of protests when they update their beliefs positively, in contrast to our findings. Finally, Shadmehr (2018) proposes protester perceptions of pivotality as potential drivers of strategic substitutability; we did not ask subjects about these perceptions.

6 Conclusion

According to the human rights NGO Freedom House, as of 2016, 26% of the world's population — nearly two billion people — live in states classified as "not free". Recent protests from the Arab Spring to Russia or Venezuela provide reminders that citizens of unfree states today, as in the past, continually rise up and demand political rights.[53] Given the prevalence of authoritarian regimes, it is important to understand individuals' decisions to participate in such protest movements.[54]

We conduct an experiment to study one dimension of individuals' protest decision: how one's turnout is affected by beliefs about the turnout of others. We find consistent evidence indicating that Hong Kong students considering participation in the July 1 protest of 2016 viewed the strategic element of their decision as a game of strategic substitutes. Individuals in our sample who were induced by the experiment to positively update their beliefs about others' turnout became less likely to participate themselves; and, individuals who were induced by the experiment to negatively update their beliefs about others' turnout became more likely to participate themselves. Although our finding of strategic substitutability in the protest decision is estimated from a particular context, the result provides guidance regarding models of protest turnout: specifically, our findings reject many recent models that assume *only* the possibility of strategic complementarity in the protest decision.

While shedding light on individuals' decisions to participate in an anti-authoritarian protest, our experiment raises the question, how general is our finding of strategic substitutability likely to be? It is important to emphasize that the mechanisms we observe at work in Hong Kong will not all be present in all protests, and even if they are present, they will not always outweigh forces generating strategic complementarity emphasized in other work.

Among the mechanisms we highlight, we believe that the existence of a political public good produced from attaining a threshold level of participation is likely to be particularly important in long-lasting political movements, for example, the Women's Suffrage Movements in the early 20th century United States and Britain; the Civil Rights Movement in the 1960s United States; and, political movements in unconsolidated democracies, like Russia's ongoing anti-corruption protests, protests in Venezuela, etc. In all of these settings, getting people on the street may in itself be an aim of the protest: achieving target protest sizes could directly achieve policy concessions, and also signal the strength of a movement to potential future participants. Strategic substitutability is

[53] Data come from Freedom House's (2016) "Freedom in the World" report, available online at `https://freedomhouse.org/sites/default/files/FH_FITW_Report_2016.pdf`, last accessed August 5, 2016.

[54] Research on authoritarian regimes, movements opposed to them, and the consequences of constraints on rulers, typically considers aggregate behavior, rather than individual behavior, as we do. A large literature has studied the consequences of political constraints for economic growth (e.g., DeLong and Shleifer, 1993; Przeworski and Limongi, 1993; Przeworski et al., 2000; Gerring et al., 2005; Rodrik and Wacziarg, 2005; Persson and Tabellini, 2006, 2008; Papaioannou and Siourounis, 2008; Acemoglu and Robinson, 2012; Bates et al., 2012; Meyersson, 2016; Acemoglu et al., 2015). Relatedly, a growing theoretical and empirical literature studies the extension of the franchise (e.g., Acemoglu and Robinson, 2000; Lizzeri and Persico, 2004; Llavador and Oxoby, 2005; Acemoglu and Robinson, 2006; Aidt and Franck, 2012, 2015).

most likely during stages of a political movement when the public goods component of a protest looms large relative to a movement's ultimate success. This does not make the protest inconsequential: tangible achievements are possible even in the absence of complete success; movement survival and growth in the future may depend on turnout in the present. It is important to emphasize, however, that the dynamic signaling element of this mechanism will likely *not* be relevant in one-shot mass events, like the Arab Spring protests, which will either topple a regime or be crushed. One-shot protests are thus more likely to exhibit strategic complementarity.

The second force for strategic substitutability in our setting is subjects' perception of an increase in the probability of government crackdown for larger protests. This force seems most likely to exist in protests demanding political rights from regimes that are not totalitarian (again, the Women's Suffrage Movements, the Civil Rights Movement, and protests in partial democracies). Many regimes, even authoritarian ones, allow some freedom of expression and association; thus, small protests are likely to be tolerated. However, these regimes may not tolerate destabilizing large events, and so may be perceived as being more likely to crack down on larger protests. This mechanism producing strategic substitutability is *unlikely* to be present in settings where protests are patently illegal, such as the Soviet Union and much of Eastern Europe prior to 1989, North Korea, or in much of the Arab World. In these cases, government crackdown is essentially guaranteed, so the probability of bearing a private cost conditional on crackdown becomes the only consideration. In such settings, larger protests are likely to be perceived as lower cost, thus pushing toward strategic complementarity.

Finally, the Bénabou and Tirole (2011) logic of greater individual signaling value of attending a smaller protest seems likely to be quite general: political radicals will be able to more strongly signal their type when a protest is small. It is important to note, however, that the magnitude of this effect may differ substantially across types of protesters and across protests. Again, this force toward substitutability may be especially pronounced in ongoing movements, in which participation becomes part of one's identity, and may be less relevant in one-shot events aimed at toppling a dictator.

Future work thus should shed more light on the mechanisms that underlie the strategic interactions in the protest game, ideally exploring their heterogeneity across varying political settings. It should also engage in the empirical analysis of protests that are linked dynamically as part of larger political movements. In our setting, a large July 1 March not only affects policy immediately, but also shapes the beliefs and behavior of citizens in the anti-authoritarian movement more broadly. This means there are dynamic considerations among protesters, learning among protest participants and non-participants, and potentially important cross-protest spillovers. Given that many protests are not solitary events, understanding these dynamic processes is an important area for future research.

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Figures and tables

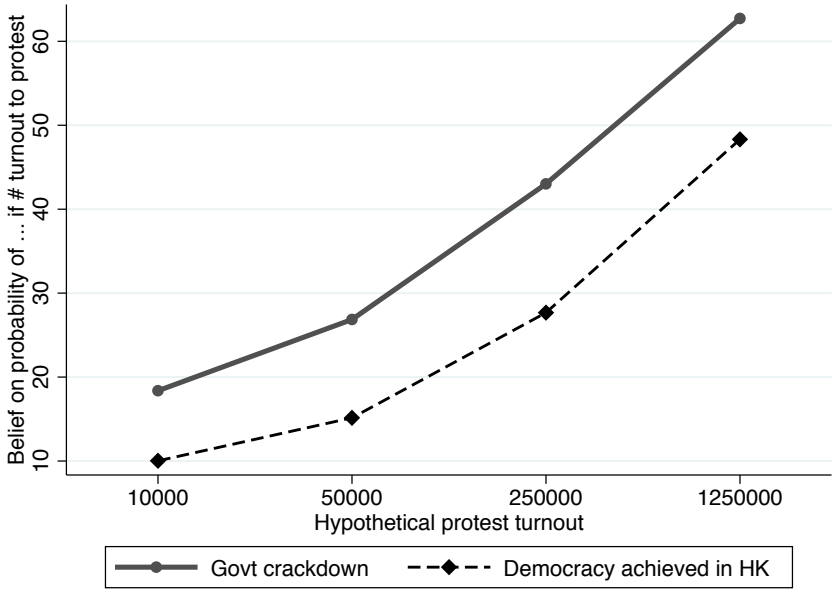

Figure 1: Students' beliefs regarding the benefits (the chance of achieving democratic institutions in Hong Kong) and costs (the chance of a violent government crackdown) for hypothetical protests with different turnout levels, ranging from 10,000 to 1,250,000 participants.

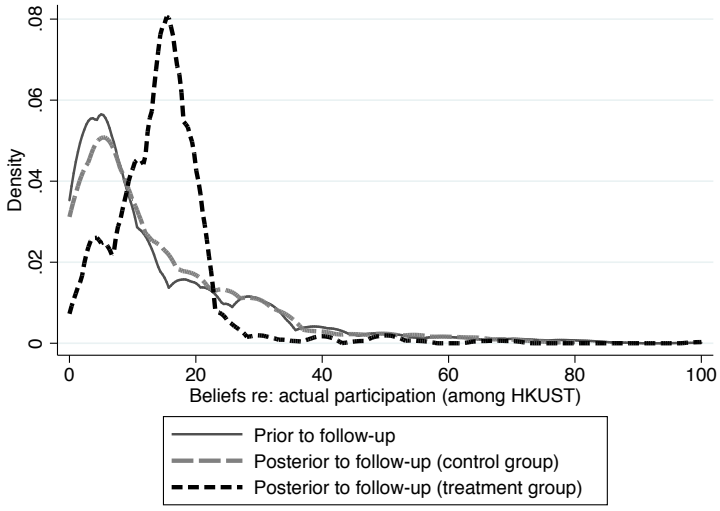

Figure 2: Distribution of prior beliefs and posterior beliefs regarding the actual protest participation of HKUST survey participants.

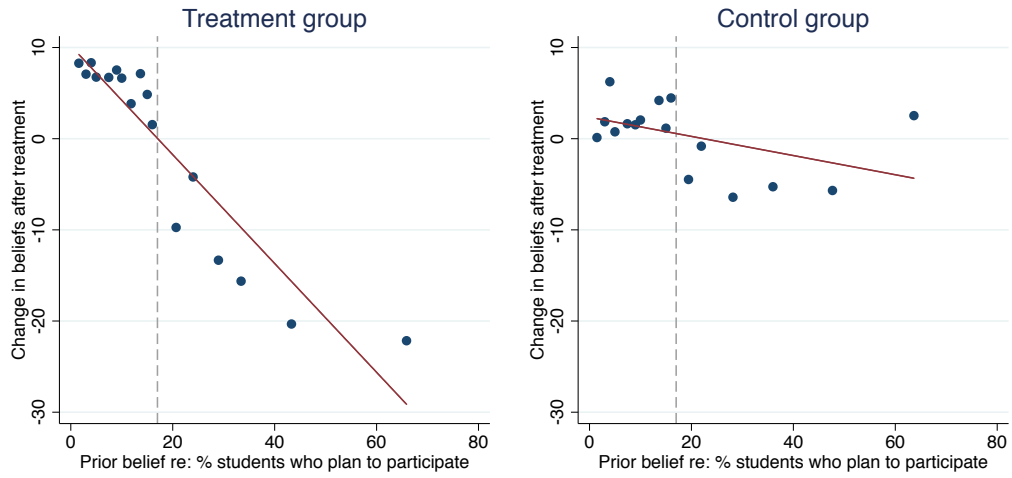

Figure 3: Changes in beliefs (posteriors minus priors) regarding the actual protest participation of HKUST survey participants, presented separately by subjects' treatment status.

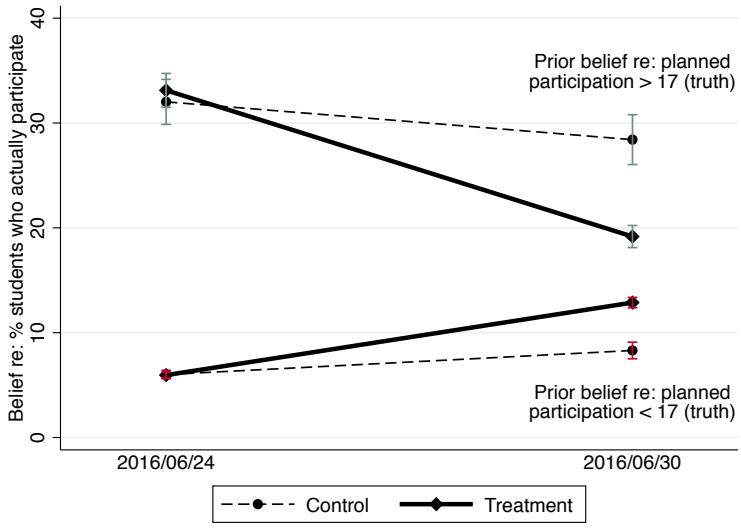

Figure 4: Treatment effect on beliefs ("first stage"). Graph shows prior (measured 6/24/2016) and posterior (measured 6/30/2016) beliefs regarding the actual protest participation of HKUST survey participants, split according to subjects' treatment status and according to prior beliefs about other subjects' planned participation. Subsamples of subjects are divided according to whether beliefs regarding the planned protest participation of HKUST survey participants were above or below the true level of 17%.

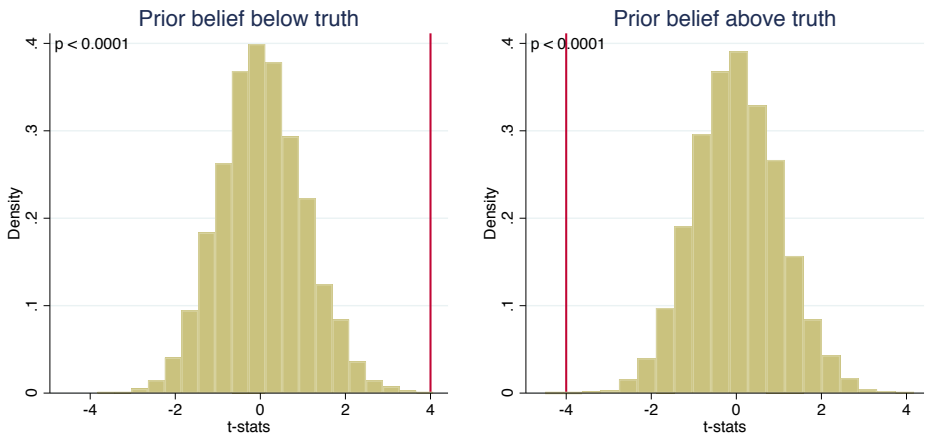

Figure 5: Distribution of t-statistics from estimating the treatment effect on posterior beliefs (1st stage) based on 10,000 *random* assignments of treatment status (2/3 treatment, 1/3 control) to study participants. Subsamples of subjects are divided according to whether beliefs regarding the planned protest participation of HKUST survey participants were above or below the true level of 17%. Red vertical lines indicate the t-statistics from the *actual* treatment assignment (drawn at 4 and -4, rather than their true values, for ease of visualization), with indicated p-values from two-sided tests.

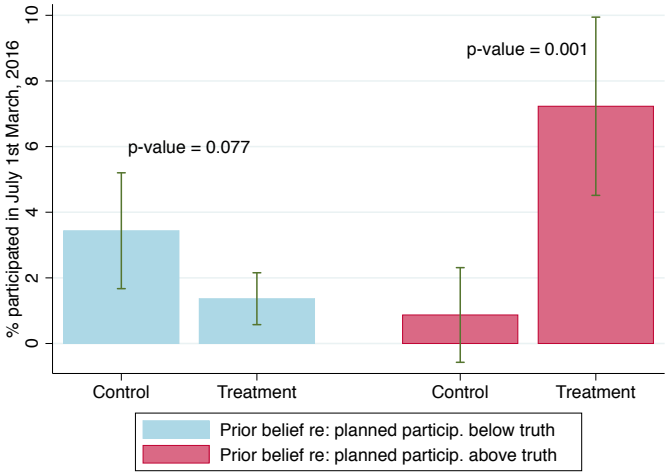

Figure 6: Treatment effect on protest participation ("reduced form"). Graph shows self-reported participation in the July 1 protest of 2016, split according to subjects' treatment status and according to prior beliefs about other subjects' planned participation. Subsamples of subjects are divided according to whether beliefs regarding the planned protest participation of HKUST survey participants were above or below the true level of 17%.

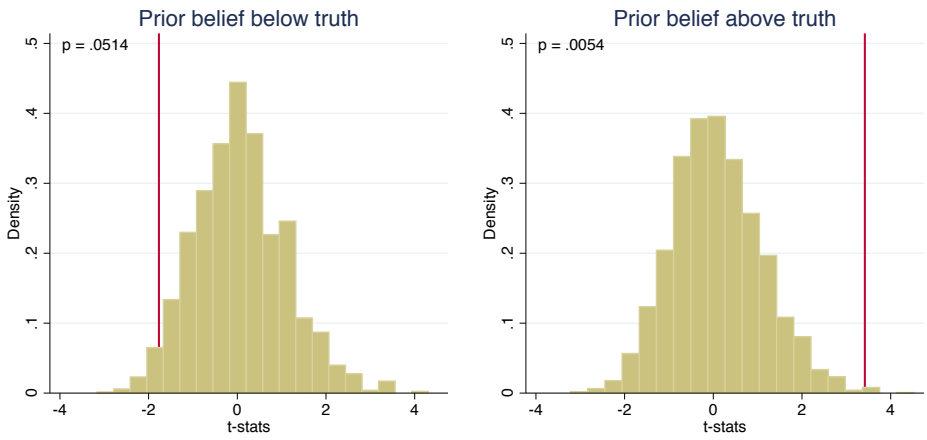

Figure 7: Distribution of t-statistics from estimating the reduced form effect of treatment on protest participation based on 10,000 *random* assignments of treatment status (2/3 treatment, 1/3 control) to study participants. Subsamples of subjects are divided according to whether beliefs regarding the planned protest participation of HKUST survey participants were above or below the true level of 17%. Red vertical lines indicate the t-statistics from the *actual* treatment assignment, with indicated p-values from two-sided tests.

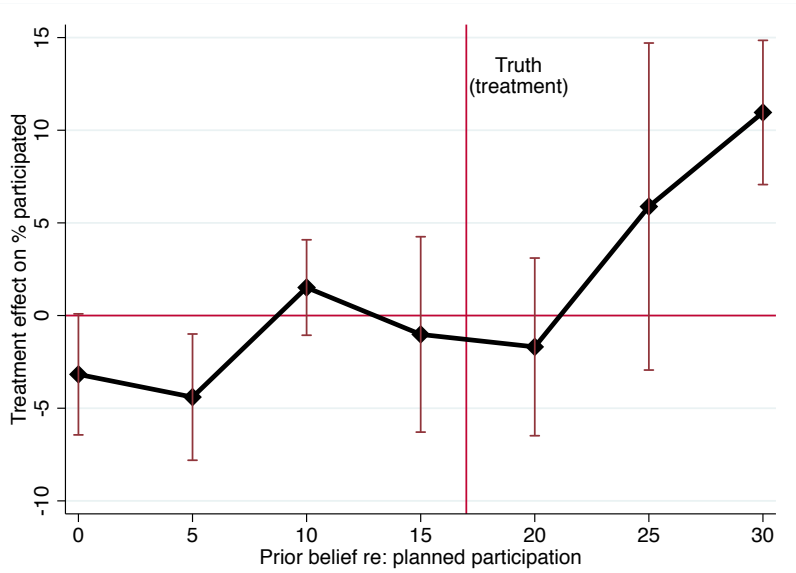

Figure 8: Treatment effect on protest participation ("reduced form"), disaggregated by prior beliefs regarding the planned participation of HKUST survey participants (bins are 5 percentage points wide).

Table 1: Summary statistics and balance check

	Experimental sample			Prior belief on planned particip. below truth			Prior belief on planned particip. above truth		
	Control mean	Treatment mean	p-value	Control mean	Treatment mean	p-value	Control mean	Treatment mean	p-value
Variables:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Male	0.554	0.535	0.548	0.566	0.523	0.233	0.522	0.565	0.454
Birth year	1995.6	1995.6	0.975	1995.8	1995.7	0.365	1995.4	1995.6	0.162
HK-oriented childhood env.	0.090	0.103	0.811	0.071	0.091	0.772	0.139	0.134	0.963
Non-religious	0.783	0.770	0.595	0.809	0.771	0.199	0.717	0.766	0.317
HH economic & social status	0.027	-0.031	0.324	0.019	-0.017	0.623	0.055	-0.065	0.279
Own projected economic status	0.001	-0.056	0.348	0.037	-0.025	0.400	-0.090	-0.130	0.714
Planned to participate in protest	17.0	17.9	0.688	13.9	14.2	0.905	24.8	26.6	0.714
Prior belief re: % others' planned particip.	15.7	15.8	0.902	7.8	7.6	0.501	36.0	35.4	0.743
Prior belief re: % others' actual particip. (HKUST)	13.4	14.0	0.470	6.0	5.9	0.772	32.1	33.2	0.546
Prior belief re: others' actual particip. (all HK)	158243	153665	0.628	145996	140858	0.635	189458	183875	0.763
# of obs.	401	833		288	585		113	248	

Note: Table tests for balance on observable characteristics (treatment versus control) for: the full sample (columns 1–3); the subsample of subjects whose prior beliefs regarding others' planned participation were below the true value of 17% (columns 4–6); and, the subsample of subjects whose prior beliefs regarding others' planned participation were above the true value of 17% (columns 7–9). "HK-oriented childhood env." is a z-score index constructed from an indicator of whether the subject completed high school with English as the formal language of instruction (as opposed to Chinese), and the number of generations a subject's family has lived in Hong Kong. "HH economic & social status" is a z-score index constructed from self-reported total income earned by both parents (including sources of income such as dividends and rents), the number of real estate properties owned by a subject's parents/household in Hong Kong at the time of the survey, and whether a subject's father's and mother's highest educational attainment are above high school, respectively. "Own projected economic status" is a z-score index constructed from the projected median income of HKUST graduates in a subject's major/program, and self-reported expectations of relative income compared to classmates at HKUST at age 40. All z-score indices are weighted by the inverse covariance of the standardized outcomes, computed following Anderson (2008).

Table 2: Item count experiments and willingness to respond to direct questions

Attitudes:	"Yes" in direct question	Δ when cover is provided
Unfavorable view of CCP	0.923	0.020 [0.059]
Consider self as Hong Kongese	0.879	-0.063 [0.051]
Support for HK independence	0.465	0.054 [0.057]
Support violence in pursuit of HK's political rights	0.217	0.169*** [0.050]

Note: The left hand column presents the fraction of 790 Hong Kong local students who expressed the corresponding attitude in response to a direct question. The right hand column presents the difference between that fraction and the fraction estimated to support the attitude using the item count technique ("list experiment"). The 790 students asked the direct questions also represent the control group for the list experiment; the remaining 786 Hong Kong local students represent the treatment group. Assignment to "direct question" and "list experiment" conditions was random among individuals who completed Part 1 of the study. *: Significant at 10%; **: 5%; ***: 1%. This table also appears in Cantoni et al. (2016).

Table 3: Treatment effect on posterior beliefs

Sample:	Posterior belief on participation among HKUST students (%)			Posterior belief on total participation among HK population (#)		
	All subjects	Prior *below* truth	Prior *above* truth	All subjects	Prior *below* truth	Prior *above* truth
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: *baseline*						
Treatment	5.891***			11423.9*		
(direction adj.)	[0.567]			[5988.6]		
Treatment		4.457***	-9.459***		13198.0**	-7013.5
		[0.545]	[1.413]		[6541.3]	[13108.6]
Panel B: *with controls*						
Treatment	6.130***			14236.0**		
(direction adj.)	[0.571]			[6125.8]		
Treatment		4.630***	-10.203***		15181.1**	-11229.0
		[0.531]	[1.452]		[6679.9]	[13550.3]
Panel C: *trimmed*						
Treatment	5.368***			15675.6**		
(direction adj.)	[0.555]			[6394.5]		
Treatment		4.501***	-8.203***		18033.5***	-9788.6
		[0.551]	[1.401]		[6625.7]	[15625.8]
Observations	1234	873	361	1234	873	361
DV mean (control grp.)	14.04	8.44	28.30	139878	128084	169940
DV std. dev. (control grp.)	14.10	8.46	15.54	155482	147528	171162
DV mean (all)	14.50	11.40	22.02	142684	134454	162586
DV std. dev. (all)	10.83	7.99	12.92	142685	139385	148689

Note: Table shows "first stage" effects: the effects of the experimental treatment on subjects' posterior beliefs regarding others' actual protest participation, conditional on subjects' prior beliefs regarding others' actual participation. Columns 1–3 show effects on posterior beliefs regarding other experimental subjects' actual participation, while columns 4–6 show effects on posterior beliefs regarding the total turnout at the protest by all Hong Kong citizens. In columns 1 and 4, all subjects are pooled and treatment is coded as "-1" for subjects whose priors regarding others' planned participation were above the true value of 17%; pooled regressions also control for an indicator of whether subjects' priors regarding others' planned participation were above true value and its interaction with subjects' prior beliefs regarding others' actual participation. Columns 2 and 5 (3 and 6) show effects on posterior beliefs for the subsample of subjects whose priors regarding others' planned participation were below (above) the true value of 17%. Panel A is estimated without any additional controls beyond the corresponding levels of prior beliefs; Panel B replicates the analysis in Panel A, but adds controls for subjects' background characteristics and economic status (gender, year of birth, a z-score index measuring whether subjects were raised in a Hong Kong-oriented environment, whether subjects were raised in a religious household, the economic status of the household, and own projected economic status); Panel C estimates the baseline specification of Panel A, but excluding those individuals in the experimental sample with the 5% lowest and the 5% highest prior beliefs regarding other subjects' planned participation. Number of observations refers to that in the baseline specification. *: Significant at 10%; **: 5%; ***: 1%.

Table 4: Treatment effect on protest participation

	Participated in 2016 July 1st March		
Sample:	All subjects	Prior *below* truth	Prior *above* truth
	(1)	(2)	(3)
Panel A: *baseline*			
Treatment	-2.713***		
(direction adj.)	[0.765]		
Treatment		-2.105*	6.373***
		[1.182]	[1.873]
Panel B: *with controls*			
Treatment	-2.143**		
(direction adj.)	[0.913]		
Treatment		-2.056*	7.131***
		[1.213]	[2.063]
Panel C: *trimmed*			
Treatment	-2.467***		
(direction adj.)	[0.797]		
Treatment		-2.230*	5.711***
		[1.250]	[2.042]
Observations	1234	873	361
DV mean (control grp.)	2.743	3.472	0.885
DV std. dev. (control grp.)	16.35	18.34	9.41
DV mean (all)	2.998	2.062	5.263
DV std. dev. (all)	17.06	14.22	22.36

Note: Table shows "reduced form" estimates, reporting the effects of the experimental treatment on subjects' own protest participation. In columns 1 and 4, all subjects are pooled and treatment is coded as "-1" for subjects whose priors regarding others' planned participation was above the true value of 17%. Column 2 (3) shows effects on posterior beliefs for the subsample of subjects whose priors regarding others' planned participation were below (above) the true value of 17%. Panel A is estimated without any controls. Panel B replicates the analysis in Panel A, but adds controls for subjects' background characteristics and economic status (gender, year of birth, a z-score index measuring whether subjects were raised in a Hong Kong-oriented environment, whether subjects were raised in a religious household, the economic status of the household, and own projected economic status), and for subjects' prior beliefs regarding HKUST students' planned participation in the July 1st March. Panel C estimates the baseline specification of Panel A, but excluding those individuals in the experimental sample with the 5% lowest and the 5% highest prior beliefs regarding other subjects' planned participation. Number of observations refers to that in the baseline specification. *: Significant at 10%; **: 5%; ***: 1%.

Table 5: Heterogeneous treatment effects by prior beliefs

	(1)	(2)	(3)
Panel A: *treatment effect on posterior beliefs (1st stage)*			
Treatment	7.417***	3.172**	6.693***
	[0.827]	[3.254]	[2.301]
Treatment × prior beliefs	-0.643***		
	[0.050]		
Treatment × *predicted* prior beliefs		-0.176	-0.388**
		[0.207]	[0.152]
Treatment × *residual* prior beliefs			-0.448***
			[0.062]
Panel B: *treatment effect on protest participation (reduced form)*			
Treatment	-4.112***	-11.175**	-11.341**
	[1.444]	[4.873]	[4.730]
Treatment × prior beliefs	0.284***		
	[0.068]		
Treatment × *predicted* prior beliefs		0.706**	0.717**
		[0.344]	[0.333]
Treatment × *residual* prior beliefs			0.246***
			[0.068]
Observations	1234	1175	1175
1st stage DV mean (control grp.)	14.04	14.10	14.10
1st stage DV std. dev. (control grp.)	14.10	14.24	14.24
1st stage DV mean (all)	14.50	14.47	14.47
1st stage DV std. dev. (all)	10.83	10.80	10.80
2nd stage DV mean (control grp.)	2.709	2.880	2.880
2nd stage DV std. dev. (control grp.)	16.26	16.75	16.75
2nd stage DV mean (all)	2.981	2.979	2.979
2nd stage DV std. dev. (all)	17.01	17.01	17.01

Note: Table shows "first stage" (Panel A) and "reduced form" (Panel B) estimates, reporting the effects of the experimental treatment on subjects' posterior beliefs about others' participation and on subjects' own protest participation. In column 1, the model allows treatment effects to vary with subjects' prior beliefs regarding the planned protest participation of other experimental subjects. In column 2, the model allows treatment effects to vary with subjects' *predicted* prior beliefs based on observables (the predicted levels of priors regarding others' planned participation are estimated using a linear model including all 47 factors listed in Online Appendix Table A.5). In column 3, the model allows treatment effects to vary with subjects' *residual* prior beliefs, the component of beliefs not predicted by observables, as well as with subjects' *predicted* priors. All regressions include the relevant lower-order term for prior beliefs. *: Significant at 10%; **: 5%; ***: 1%.

Table 6: Two stage estimates of protest participation

Sample:	Participated in 2016 July 1st March		
	All subjects	Prior *below* truth	Prior *above* truth
	(1)	(2)	(3)
Panel A: *baseline*			
Posterior belief	-0.553***	-0.468**	-0.654**
	[0.177]	[0.236]	[0.264]
Panel B: *with controls*			
Posterior belief	-0.550***	-0.445*	-0.657***
	[0.174]	[0.231]	[0.252]
Panel C: *trimmed*			
Posterior belief	-0.607***	-0.514**	-0.754**
	[0.212]	[0.261]	[0.354]
Observations	1234	873	361
1st stage DV mean (control grp.)	14.04	8.44	28.30
1st stage DV std. dev. (control grp.)	14.10	8.46	15.54
1st stage DV mean (all)	14.50	11.40	22.02
1st stage DV std. dev. (all)	10.83	7.99	12.92
2nd stage DV mean (control grp.)	2.743	3.472	0.885
2nd stage DV std. dev. (control grp.)	16.35	18.34	9.41
2nd stage DV mean (all)	2.998	2.062	5.263
2nd stage DV std. dev. (all)	17.06	14.22	22.36

Note: Table shows two-stage estimates of the effects of beliefs about other subjects' turnout in the protest on one's own turnout. The first stage estimates the effects of the experimental treatment on subjects' posterior beliefs regarding other subjects' actual protest participation, conditional on the corresponding prior beliefs. The second stage exploits variation in beliefs regarding other subjects' participation induced by the experimental treatment to estimate the effect of beliefs about others' protest turnout on one's own turnout. In column 1, all subjects are pooled and treatment is coded as "-1" for subjects whose priors regarding others' planned participation were above the true value of 17%; pooled regression also controls for an indicator of whether subjects' priors regarding others' planned participation were above true value and its interaction with subjects' prior beliefs regarding others' actual participation. Column 2 (3) shows estimates for the subsample of subjects whose priors regarding others' planned participation were below (above) the true value of 17%. Panel A is estimated without any controls beyond the corresponding prior beliefs. Panel B replicates the analysis in Panel A, but adds controls for subjects' background characteristics and economic status (gender, year of birth, a z-score index measuring whether subjects were raised in a Hong Kong-oriented environment, whether subjects were raised in a religious household, the economic status of the household, and own projected economic status). Panel C estimates the baseline specification of Panel A, but excluding those individuals in the experimental sample with the 5% lowest and the 5% highest prior beliefs regarding other subjects' planned participation. Number of observations refers to that in the baseline specification. *: Significant at 10%; **: 5%; ***: 1%.

ONLINE APPENDIX, NOT FOR PUBLICATION

Appendix A Experimental materials

A.1 Baseline survey module (Part 1)

ANTI-AUTHORITARIANISM	
Panel A: Responses to direct questions	
Category A.1: *Support for democracy*	
A.1.1	How important is it for you to live in a country that is governed democratically, even if democracy makes no significant difference in the socioeconomic status of you, your family, or the country as a whole? (0 = not at all important; 10 = absolutely important)
A.1.2	Do you think that universal and truly democratic elections play an important role in determining whether you and your family are able to make a better living? (0 = not at all important; 10 = extremely important)
A.1.3	Do you think that universal and truly democratic elections are an important factor in whether or not a country's economy can develop successfully? (0 = not at all important; 10 = extremely important)
A.1.4	Where do you stand in terms of your political attitudes? (0 = pro-establishment / pro-Beijing; 10 = pro-Democracy)
A.1.5	Where do you stand in terms of the following two statements? (0 = I think that only those who demonstrate patriotism towards Beijing should be allowed to become candidates for the Chief Executive; 10 = I think that no restriction should be imposed in terms of who are allowed to become candidates during the Chief Executive election)
Category A.2: *Support for HK independence*	
A.2.1	Where do you stand in terms of the following two statements? (0 = I would like to see Hong Kong be fully integrated with the political institutions of Mainland China; 10 = I would like Hong Kong to be separate and have its own political institutions)
A.2.2	Where do you stand in terms of the following two statements? (0 = I would like to see Hong Kong be fully integrated with the economic institutions of Mainland China; 10 = I would like Hong Kong to be separate and have its own economic institutions)
A.2.3	As it is now, is the Chinese Communist Party legitimate in ruling over Hong Kong? (0 = completely legitimate; 10 = not at all legitimate)
A.2.4	If the Chinese Communist Party undergoes significant reform and Mainland China adopts truly democratic political institutions, do you think the Chinese central government can be a legitimate ruling government over Hong Kong? (0 = completely legitimate; 10 = not at all legitimate)
A.2.5	To what extent do you think Hong Kong should be an independent nation? (0 = HK should not be independent at all; 10 = HK should definitely be independent)
A.2.6	To what extent do you think Hong Kong society should discuss and debate the potential prospect of its independence? (0 = independence should not be discussed at all; 10 = important and beneficial to have open discussion on independence)
Category A.3: *HK identity: self-reported*	
A.3.1	Where do you stand in terms of your national identity? (0 = Chinese; 10 = Hong Kongese)
A.3.2	Where do you stand in terms of your cultural identity? (0 = Chinese; 10 = Hong Kongese)

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A.3.3	How important is being a Hong Kongese citizen to you? (0 = not at all important; 10 = extremely important)
A.3.4	How important is being a Chinese citizen to you? (0 = extremely important; 10 = not at all important)
Category A.4: *Unhappiness with political status quo*	
A.4.1	How democratically is Hong Kong being governed today? (0 = completely democratic; 10 = not at all democratic)
A.4.2	How would you rate the political system in Hong Kong between 1997 and 2012, relative to that prior to 1997? (0 = extremely good; 10 = extremely bad)
A.4.3	How would you rate the political system in Hong Kong today, relative to that prior to 1997? (0 = extremely good; 10 = extremely bad)
A.4.4	All things considered, how satisfied are you with your life as a whole these days? (0 = completely satisfied; 10 = completely dissatisfied)
Category A.5: *Anti-CCP views on current events*	
A.5.1	To what degree do believe that the electoral reform package proposed by Mainland China is democratic? (0 = completely democratic; 10 = completely undemocratic)
A.5.2	Do you support the Legislative Council's veto decision? (0 = completely against Legco's decision; 10 = completely support Legco's decision)
A.5.3	Between October and December 2015, multiple booksellers from Causeway Bay Books have gone missing. Many suspect that the mainland Chinese government was involved. If this is true, what do you think of mainland Chinese government's action? (0 = completely legitimate, in accordance with Basic Law; 10 = completely illegitimate, violation against Basic Law)
Panel B: Self-reported behavior and real-stakes decisions	
B.1	Have you participated in the Occupy Central / Umbrella Revolution during September - December 2014?
B.2	Which party are you are you planning to vote for, during the 2016 Hong Kong Legislative Council Election? (0 = pro-Beijing parties; 1 = pro-democracy parties)
B.3	Are you planning to participate in the July 1st March in 2016? (0 = no, or not sure yet but more unlikely than yes; 1 = yes, or not sure yet but more likely than not)
B.4.1-4	Average amount allocated to HK local partner in national identity games, relative to the amount allocated to Mainland Chinese
B.5	How much money from your participation fee do you want to contribute to Demosisto? (0 = none; 1 = positive amount)
FUNDAMENTAL FACTORS	
Panel C: Economic preferences	
Category C.1: *Risk tolerance*	
C.1.1	Please tell me, in general, how willing or unwilling you are to take risks? (0 = completely unwilling to take risks; 10 = very willing to take risks)
C.1.2	Certainty equivalent from step-wise lottery choices (what would you prefer: a draw with 50 percent chance of receiving 300 HKD, and the same 50 percent chance of receiving nothing, or the amount of xxx HKD as a sure payment?)
C.1.3	Eckel and Grossman (2002) lottery decisions: for the following lottery options, please choose one that you like the most? *[incentivized]*

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Category C.2: *Patience*	
C.2.1	How willing are you to give up something that is beneficial for you today in order to benefit more from that in the future? (0 = completely unwilling; 10 = very willing)
C.2.2	I tend to postpone tasks even if I know it would be better to do them right away (0 = describes me perfectly; 10 = does not describe me at all)
C.2.3	Patience index from a step-wise intertemporal choices (would you rather receive 100 HKD today or xxx HKD in 12 months?)
Category C.3: *Altruism*	
C.3.1	How willing are you to give to good causes without expecting anything in return? (0 = completely unwilling; 10 = very willing)
C.3.2	Today you unexpectedly received 10,000 HKD. How much of this amount would you donate to a good cause? (value between 0 and 10,000)
Category C.4: *Reciprocity*	
C.4.1	When someone does me a favor I am willing to return it (0 = describes me perfectly; 10 = does not describe me at all)
C.4.2	I assume that people have only the best intentions (0 = does not describe me at all; 10 = describes me perfectly)
C.4.3	When a stranger helps you, would you be willing to give one of the following presents to the stranger as a thank-you gift?
C.4.4	How willing are you to punish someone who treats you unfairly, even if there may be costs for you? (0 = completely unwilling; 10 = very willing)
C.4.5	How willing are you to punish someone who treats others unfairly, even if there may be costs for you? (0 = completely unwilling; 10 = very willing)
C.4.6	If I am treated very unjustly, I will take revenge at the first occasion, even if there is a cost to do so (0 = describes me perfectly; 10 = does not describe me at all)
Category C.5: *Preference for redistribution*	
C.5.1-11	Average amount of money allocated to a fellow HK local partner in a series of dictator games *[incentivized]*
Panel D: Personality traits	
Category D.1: *Big 5 - openness*	
D.1.1-5	On each numerical scale that follows, indicate which point is generally more descriptive of you:
D.1.1	1 = no-nonsense; 5 = a dreamer
D.1.2	1 = practical; 5 = theoretical
D.1.3	1 = following authority; 5 = following imagination
D.1.4	1 = seek routine; 5 = seek novelty
D.1.5	1 = prefer things clear-cut; 5 = comfortable with ambiguity
Category D.2: *Big 5 - agreeableness*	
D.2.1-5	On each numerical scale that follows, indicate which point is generally more descriptive of you:
D.2.1	1 = abrupt; 5 = courteous
D.2.2	1 = selfish; 5 = generous
D.2.3	1 = cold; 5 = warm
D.2.4	1 = independent; 5 = team player
D.2.5	1 = skeptical; 5 = trusting
Category D.3: *Big 5 - conscientiousness*	

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D.3.1-5	On each numerical scale that follows, indicate which point is generally more descriptive of you:
D.3.1	1 = messy; 5 = neat
D.3.2	1 = open-minded; 5 = decisive
D.3.3	1 = easily distracted; 5 = stay focused
D.3.4	1 = comfortable with chaos; 5 = a preference for order
D.3.5	1 = procrastinate; 5 = on time
Category D.4: *Big 5 - neuroticism*	
D.4.1-5	On each numerical scale that follows, indicate which point is generally more descriptive of you:
D.4.1	1 = calm; 5 = eager
D.4.2	1 = confident; 5 = cautious
D.4.3	1 = upbeat; 5 = discouraged
D.4.4	1 = don't give a darn; 5 = easily embarrassed
D.4.5	1 = unflappable; 5 = distractible
Category D.5: *Big 5 - extraversion*	
D.5.1-5	On each numerical scale that follows, indicate which point is generally more descriptive of you:
D.5.1	1 = prefer being alone; 5 = prefer being with others
D.5.2	1 = pessimistic; 5 = optimistic
D.5.3	1 = private; 5 = exhibitionist
D.5.4	1 = cool; 5 = outgoing
D.5.5	1 = thoughtful; 5 = conversational
Panel E: Cognitive ability	
Category E.1: *Cognitive reflection test*	
E.1.1	A bat and a ball cost $1.10 in total. The bat costs $1.00 more than the ball. How much does the ball cost?
E.1.2	If it takes 5 machines 5 minutes to make 5 widgets, how long would it take 100 machines to make 100 widgets?
E.1.3	In a lake, there is a patch of lily pads. Every day, the patch doubles in size. If it takes 48 days for the patch to cover the entire lake, how long would it take for the patch to cover half of the lake?
Category E.2: *University GPA*	
E.2.1	GPA at HKUST, demeaned by major/program
Panel F: Economic status	
Category F.1: *Household economic & social status*	
F.1.1	During the past 12 months, what's the average monthly income of your family?
F.1.2	How many properties in HK do your parents currently own in total?
F.1.3	Father's highest educational attainment is above high school
F.1.4	Mother's highest educational attainment is above high school
Category F.2: *Student's projected economic status*	
F.2.1	Median income of HKUST graduates in same major/program (as of 2014)
F.2.2	At age 40, where do you see yourself financially, relative to your classmates at HKUST? (1 = at the very bottom; 7 = at the very top)
Panel G: Background characteristics	
G.1	Gender (0 = female; 1 = male)

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G.2	Birth year
Category G.3: *HK-oriented childhood environment*	
G.3.1	Generations since family migrated to HK (1 = self-migrated; 4 = great grandparents migrated)
G.3.2	Attended HK high school using English as language of instruction
Category G.4: *Religiosity*	
G.4.1	Religiosity (0 = atheist; 1 = religious)

SIMULTANEOUSLY DETERMINED VARIABLES

Panel H: Beliefs about politics	
Category H.1: *Beliefs about future institutions*	
H.1.1	Optimistic about HK's political institutions in 2025 (believe that Hong Kong will have separate and completely different political institutions from those of Mainland China by 2025, with high certainty)
H.1.2	Optimistic about HK's political institutions in 2050 (believe that Hong Kong will have separate and completely different political institutions from those of Mainland China by 2050, with high certainty)
Category H.2: *Beliefs about protest efficacy*	
H.2.1	Probability of achieving democratic institutions in HK if protests occur, relative to the probability if no protest occurs (based on separate elicitation of probability of various protest scenarios and conditional probabilities of democratic institutions under these scenarios)
Panel I: Beliefs about HKUST students	
Category I.1: *Beliefs about HKUST students: support for democracy*	
I.1.1-2	What is the average answer that other participants from HKUST in this study have chosen?
I.1.1	Corresponding question: A.1.4
I.1.2	Corresponding question: A.1.5
Category I.2: *Beliefs about HKUST students: support for HK independence*	
I.2.1-3	What is the average answer that other participants from HKUST in this study have chosen?
I.2.1	Corresponding question: A.2.1
I.2.2	Corresponding question: A.2.2
I.2.3	Corresponding question: A.2.5
Category I.3: *Beliefs about HKUST students: HK identity*	
I.3.1-2	What is the average answer that other participants from HKUST in this study have chosen?
I.3.1	Corresponding question: A.3.1
I.3.2	Corresponding question: A.3.2
Category I.4: *Beliefs about HKUST students: unhappiness with political status quo*	
I.4.1-2	What is the average answer that other participants from HKUST in this study have chosen?
I.4.1	Corresponding question: A.4.1
I.4.2	Corresponding question: A.4.4
Category I.5: *Beliefs about HKUST students: aggressive pursuit of political rights*	

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I.5.1	What is the average answer that other participants from HKUST in this study have chosen? Corresponding question: A.6.2
Panel J: Social life	
Category J.1: *Political social network*	
J.1.1	When you get together with your friends, would you say you discuss political matters frequently, occasionally, or never? (0 = never; 10 = frequently)
J.1.2	When you, yourself, hold a strong opinion, do you ever find yourself persuading your friends, relatives or fellow schoolmates to share your views or not? If so, does this happen often, from time to time, or rarely? (0 = never; 10 = always)
J.1.3	Do you know any direct relative who has participated in the Occupy Central movement in 2014?
J.1.4	Do you know any schoolmate who has participated in the Occupy Central movement in 2014?
J.1.5	Do you know any friend outside of school who has participated in the Occupy Central movement in 2014?
J.1.6	Has any of your direct relatives, schoolmates, or friends outside of school persuaded you to support Occupy Central (or anti-Occupy Central)?
J.1.7	How much do you know, on average, about your direct relatives' political orientation? (0 = do not know at all; 10 = very familiar and certain)
J.1.8	How much do you know, on average, about your schoolmates' political orientation? (0 = do not know at all; 10 = very familiar and certain)
J.1.9	How much do you know, on average, about your friends' political orientation? (0 = do not know at all; 10 = very familiar and certain)
Category J.2: *Sociability*	
J.2.1	Total number of friends at HKUST elicited (Please list the names of your friends at HKUST, in the order from those whom you interact with most frequently, to those whom you interact with less frequently. Please list as many names as you want – there is no space limit)
J.2.2	Current relationship status is non-single
Panel K: Beliefs about close friends	
Category K.1: *Beliefs about close friends: support for democracy*	
K.1.1-2	What is the average answer that 5 of your closest friends at HKUST would have chosen?
K.1.1	Corresponding question: A.1.4
K.1.2	Corresponding question: A.1.5
Category K.2: *Beliefs about close friends: support for HK independence*	
K.2.1-3	What is the average answer that 5 of your closest friends at HKUST would have chosen?
K.2.1	Corresponding question: A.2.1
K.2.2	Corresponding question: A.2.2
K.2.3	Corresponding question: A.2.5
Category K.3: *Beliefs about close friends: HK identity*	
K.3.1-2	What is the average answer that 5 of your closest friends at HKUST would have chosen?
K.3.1	Corresponding question: A.3.1
K.3.2	Corresponding question: A.3.2
Category K.4: *Beliefs about close friends: unhappiness with political status quo*	
K.4.1-2	What is the average answer that 5 of your closest friends at HKUST would have chosen?
K.4.1	Corresponding question: A.4.1

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K.4.2	Corresponding question: A.4.4
Category K.5: *Beliefs about close friends: aggressive pursuit of political rights*	
K.5.1	What is the average answer that 5 of your closest friends at HKUST would have chosen? Corresponding question: A.6.2
Panel L: Media consumption	
Category L.1: *Frequency of news consumption*	
L.1.1	How often do you browse the internet to read about news and current events? (1 = never; 6 = multiple times a day)
Category L.2: *Pro-democratic source of media*	
L.2.1	What are the top 3 internet websites that you regularly browse to consume information? (Select HK local websites among the top 2 choices)
L.2.2	What are the top 3 news outlets that you regularly read for news (including the website, hard-copies of the newspaper, etc.)? (Select pro-democracy news outlets in HK among the top 2 choices)
Panel M: Political interest and knowledge	
Category M.1: *Political interest*	
M.1.1	How interested would you say you are in politics? (0 = not at all interested; 10 = extremely interested)
Category M.2: *Political knowledge*	
M.2.1-4	Able to answer the following questions correctly:
M.2.1	Which of the following is a Democratic Party Legco member?
M.2.2	Which of the following is a pro-Beijing Legco member?
M.2.3	Which of the following is a leader of a newly founded party in HK that focuses on self-determination?
M.2.4	Which of the following is a leader of a newly founded party in HK that focuses on independence?

ADDITIONAL OUTCOME VARIABLES

Panel N: Intensity of political support	
Category N.1: *Aggressive pursuit of political rights*	
N.1.1	What do you think is the consequence of this veto decision, in terms of Hong Kong adopting fully democratic political institutions in the future? (0 = the veto decision is extremely harmful in leading Hong Kong to fully democratic institutions in the future; 10 = the veto decision is extremely beneficial in leading Hong Kong to fully democratic institutions in the future)
N.1.2	Some people support the use of violence to fight for Hong Kong citizens' political rights, while others oppose the use of violence. Where do you stand on this question? (0 = violence can never be justified; 10 = violence is currently justified)

A.2 Pre-protest module (Part 2)

Version A: Control Group

[Screen 1]
Welcome screen: thank you for participating in this follow-up survey.

[Screen 2]
Remember that we offered you:

1. A HKD 10 bonus payment for accurately guessing the percentage of HKUST survey participants who would actually attend this July 1 March (七一遊行);
2. An additional HKD 10 bonus payment for accurately guessing the total number of Hong Kong citizens would actually attend this July 1 March (七一遊行).

In last week's survey, you guessed that:

1. *[embedded individual value]* % of HKUST survey participants would attend this July 1 March;
2. A total of *[embedded individual value]* Hong Kong citizens would attend this July 1 March.

Perhaps since then your views have changed.

We now ask you again to provide guesses about actual attendance of the July 1 March.
Instead of your guesses in the previous survey, we will use today's guesses to determine your bonus payment.

1. How many people in total do you think will *participate* in the July 1st March (七一大遊行) in *2016*?
   - If your guess is within *10%* of what will be reported by the *HKUPOP* (香港大學民意研究計劃) after the July 1st March in 2016, then you will earn a bonus payment of HKD 10.
   - To give you a sense, according to HKUPOP's report, among the July 1st March that took place between 2003 and 2015:
     - The lowest attendance in a given year is: 17,000 (in 2008);
     - The highest attendance in a given year is: 462,000 (in 2003).

   [Slider bar, ranging from 0 to 1,000,000]

2. Please guess what percentage of the participants from HKUST of this study will *participate* in the July 1st March (七一大遊行) in *2016*?
   - If your guess is within 2 percentage points of the percent of students who actually participate, you will earn a bonus payment of HKD 10.

   _____ %
   [Fill in the number: between 0-100]

[Screen 3]
You have now finished the follow-up survey module.

Thank you very much for your participation. We will inform you about the total payment you have earned from last week and today's surveys – the payment will be deposited to your bank account via the *HKUST Student Information System (SIS)*.

We will also inform you about future study opportunities, and we look forward to seeing you again soon!

Feel free to contact us at jzproject@ust.hk if you have questions and/or concerns regarding this study.

Version B: Treatment Group

[Screen 1]
Welcome screen: thank you for participating in this follow-up survey.

[Screen 2]
In this follow-up survey, we wish to update you on your fellow HKUST survey participants' planned participation in this year's July 1 March (七一遊行).

Recall that you guessed that *[embedded individual value]* % of HKUST survey participants would plan to attend the July 1 March.

Based on last week's survey, the true percentage of survey participants who plan to attend the July 1 March is 17%.

Remember that we offered you:

1. A HKD 10 bonus payment for accurately guessing the percentage of HKUST survey participants who would actually attend this July 1 March (七一遊行);
2. An additional HKD 10 bonus payment for accurately guessing the total number of Hong Kong citizens would actually attend this July 1 March (七一遊行).

In last week's survey, you guessed that:

1. *[embedded individual value]* % of HKUST survey participants would attend this July 1 March;
2. A total of *[embedded individual value]* Hong Kong citizens would attend this July 1 March.

Perhaps since then your views have changed.

We now ask you again to provide guesses about actual attendance of the July 1 March.

1. How many people in total do you think will *participate* in the July 1st March (七一大遊行) in *2016*?
   - If your guess is within *10%* of what will be reported by the *HKUPOP* (香港大學民意研究計劃) after the July 1st March in 2016, then you will earn a bonus payment of HKD 10.
   - To give you a sense, according to HKUPOP's report, among the July 1st March that took place between 2003 and 2015:
     - The lowest attendance in a given year is: 17,000 (in 2008);
     - The highest attendance in a given year is: 462,000 (in 2003).

   [Slider bar, ranging from 0 to 1,000,000]

2. Please guess what percentage of the participants from HKUST of this study will *participate* in the July 1st March (七一大遊行) in *2016*?
   - If your guess is within 2 percentage points of the percent of students who actually participate, you will earn a bonus payment of HKD 10.

   _____ %
   [Fill in the number: between 0-100]

[Screen 3]
You have now finished the follow-up survey module.

Thank you very much for your participation. We will inform you about the total payment you have earned from last week and today's surveys – the payment will be deposited to your bank account via the *HKUST Student Information System (SIS)*.

We will also inform you about future study opportunities, and we look forward to seeing you again soon!

Feel free to contact us at jzproject@ust.hk if you have questions and/or concerns regarding this study.

A.3 Post-protest module (Part 3)

[Screen 1]
Welcome screen: thank you for participating in this follow-up survey.

[Screen 2]

1. Did you attend the July 1 2016 March?

   {IF "NO", skip to Screen 3.}

2. Which political group's crowd did you join during the March?
   (please choose all that apply)

   公民黨 Civic Party
   民主黨 Democratic Party
   人民力量 People Power
   工黨 Labour Party
   社民連 League of Social Democrats
   街工 Neighbourhood and Worker Service Centre
   新民主同盟 Neo Democrats
   青年新政 Youngspiration
   香港眾志 Demosisto
   熱血公民 Civic Passion
   本土民主前線 Hong Kong Indigenous

   科大學生會 HKUST Student Union
   科大行動 ProgressUST
   學聯 HKFS

   普羅政治學院 Proletariat Political Institute
   香港花生 HKpeanut
   D100 民間電台 D100 Radio
   職工盟 Hong Kong Confederation of Trade Unions
   雷動計劃 ThunderGo
   小麗民主教室 Siu Lai Democracy Groundwork
   土地正義聯盟 Land Justice League
   法輪功 Falun Gong
   大陸朱 Chu Siu-hung
   爭取全民退休保障聯席 Alliance for Universal Pension
   懇請政府重訂屋宇飼養犬隻條例聯盟 Give Dogs a Home
   旺角鳩鳴團 Mong Kok Shopping Revolution

良心之友 Friends of Conscience
撐傘落區 Umbrella Blossom
其他 Others

3. What was your general impression of the March (300 words or less)?

   {Open-ended}

4. Recall that in the previous survey rounds, we asked about your friends at HKUST. To the best of your knowledge, how many of the friends you named (listed below) attended?

   {Multiple round}

[Screen 3]
So far, you have earned: HKD {*inserted individual payment value*} in total from participating in our study.

The payment will be ***deposited directly to your bank account*** via the ***HKUST Student Information System (SIS)***, in approximately 5-8 weeks.

Feel free to contact us at jzproject@ust.hk if you have questions and/or concerns regarding this study.

Appendix B Discussion of theoretical literature

Here we provide brief overviews of the sources of strategic complementarity assumed in several recent papers cited in the main text.

Bueno de Mesquita (2010)

In his paper on political mobilization and revolution, Bueno de Mesquita (2010, p. 449) develops the following model of individuals' decisions to participate in a revolution:

> The "number" of people who join the revolution is N. The regime is replaced if and only if N is greater than or equal to a threshold $T \in (0,1)$, which is commonly known. A member of the population, i, takes an action $a_i \in \{0,1\}$, where $a_i = 1$ is the decision to participate. A person's type, i, determines how much he or she values regime change. He or she derives a portion, $\gamma \in (0,1]$, of that value only if the revolution succeeds and he or she participated. The other portion, $1-\gamma$, is realized by all players, if the revolution succeeds, whether that player personally participated in the revolution. Participating imposes a cost $k > 0$ on the individual. The payoff to a failed revolution is 0.

Bueno de Mesquita (2010, p. 449) then highlights a key assumption used to generate strategic complementarity in the protest decision, writing:

> Several assumptions merit further comment. First, there is some portion (γ) of the payoffs from regime change that can only be accessed by those who participate in the revolution. (This relaxes the standard collective action problem.) Substantively, this could be because those who actively participate in revolution gain privileged status after regime change occurs or because there are expressive benefits to having participated in a victorious uprising.

Thus, strategic complementarity in Bueno de Mesquita (2010) is built on two elements in the model: first, a protest is only successful if total participation is "large enough" (N must be greater than T); second, an individual derives differential utility (γ) from participating in a protest that is successful.

Edmond (2013)

In his paper on protests and regime change (primarily focused on the manipulation of information by a regime), Edmond (2013, p. 1425) develops the following model of an individual's decision to participate in a movement aiming to overthrow a regime:

> There is a unit mass of *ex ante* identical citizens, indexed by $i \in [0,1]$. The citizens face a regime that seeks to preserve the status quo. Each citizen decides whether to participate in an attack on the regime, $s_i = 1$, or not, $s_i = 0$. The aggregate attack is $S = \int_0^1 s_i di$. The type of a regime θ is its private information. The regime is overthrown if and only if $\theta < S$.
>
> ...
>
> The payoff to a citizen is given by:

$$u(s_i, S, \theta) = (\mathbb{1}\{\theta < S\} - p) \times s_i$$

where $\mathbb{1}\{\}$ denotes the indicator function and where $p > 0$ is the opportunity cost of participating in an attack. A citizen will participate, $s_i = 1$, whenever they expect regime change to occur with more than probability p. To focus on scenarios where individual decisions are not trivial, I also assume that $p < 1$. Otherwise, if $p \geq 1$, it is optimal for an individual to not participate independent of θ and S. With $p < 1$ the individual s_i and aggregate S are strategic complements: the more citizens participate in an attack, the more likely it is that the regime is overthrown and so the more likely it is that any individual also participates.

The Edmond (2013) model thus builds the assumption of strategic complementarity on two elements: first, the assumption that a protest is more likely to be successful when it is larger; and, second, that an individual will derive greater utility from participating in a successful protest.

Passarelli and Tabellini (2017)

In a model of citizen unrest in response to perceived unfair government policy, Passarelli and Tabellini (2017, pp. 909–910) model participation in an anti-government riot as follows:

> Individuals unilaterally decide whether to participate in a riot, trading off the costs and benefits. The benefit is purely emotional: it is the psychological reward of joining other group members in a public display of the frustration caused by the policy or of contributing to take revenge on an unfair government. We refer to the psychological benefit of rioting, denoted a^i, as the aggrievement caused by the policy to members of group i, because we assume that this benefit is related to the emotion of being the victim of unfair treatment.
>
> ...
>
> Joining a riot also entails costs, in terms of time or risk of being arrested or injured. We model these costs as the sum of two components: $\mu + \epsilon^{ij}$. The parameter $\mu > 0$ is known and common to all and reflects external conditions such as the risk of violent repression. The term ϵ^{ij} is a random variable that captures idiosyncratic components of the cost or benefit of participation (the superscript ij refers to individual j in group i).
>
> ...
>
> Finally, we assume that there is a complementarity: the benefit of participation grows proportionately with the number of other group members also participating in the riot, $p^i \lambda^i$. As explained below, aggrievement is an individual emotion, but it is related to the feeling that the group is treated unfairly. Hence, the psychological benefit of a public display of anger is stronger if the emotion is more widely shared. Participation could also proxy for the probability of reaching a critical threshold that triggers a political crisis; in this interpretation, the complementarity reflects the feeling of contributing to a more meaningful event with a greater chance of success. Equivalently, the complementarity could also be on the cost side: the probability of being arrested is smaller in a larger crowd.

Passarelli and Tabellini (2017) thus offer multiple interpretations of their assumption of strategic complementarity: it may arise from greater expressive utility when participating in a larger protest; from greater utility when participating in a protest more likely to succeed; or from lower anticipated costs of participation when a protest is larger.

Barbera and Jackson (2017)

In their paper studying revolution under uncertainty, Barberà and Jackson (2017, pp. 6–8) sketch a simple, baseline model of individuals' protest decision:

> A continuum of citizens of mass 1 are indexed by $i \in [0,1]$. They have a choice to participate in a revolution. Again, we use the term 'revolt' but the model obviously has many applications. The revolution is successful if at least a fraction $q \in (0,1]$ of the population participates. If fewer than q participate, then the revolution fails.
>
> ...
>
> An agent gets a value from the the revolution as a function of whether it is successful or not and whether the agent participates or not. All of these payoffs ... are given by the following table.[1]

	Success	Failure
Participate	$a_i + V_i$	$b_i - C_i$
Not Participate	a_i	b_i

> Here, a_i is the value that an agent gets if the revolution is successful, regardless of whether the agent participates or not, and this can depend on the agent's type and the state. Similarly, b_i is the value that an agent gets if the revolution fails, regardless of whether the agent participates or not, and this can depend on the agent's type and the state. The values, V_i and C_i then are the additional value and cost that an agent gets from participating in the revolution as a function of whether it is successful or fails.
>
> ...
>
> [W]e follow the usual approach that has been followed in the voting literature and have V_i be the marginal utility for having participated, conditional upon success. So this is not the payoff that an agent gets in terms of expecting to be pivotal, but instead the utility they get from knowing or being able to say that they participated in a revolution that was successful: from having been one of those who stormed the Bastille, participated in the Salt March, or protests in Tunisia, etc. It will almost never be the case that one more or one fewer revolutionary would make the difference. People react to some more basic utility from the action itself, which can be motivated in many different ways.

Thus, much like Bueno de Mesquita (2010) and Edmond (2013), the Barberà and Jackson (2017) model builds the assumption of strategic complementarity on two elements: first, a protest is only successful if total participation is "large enough" (the participation level must exceed the threshold q); second, an individual derives differential utility (V_i) from participating in a protest that is successful.

[1] Note that the table's notation is slightly simplified for purposes of exposition.

Appendix C Appendix figures and tables

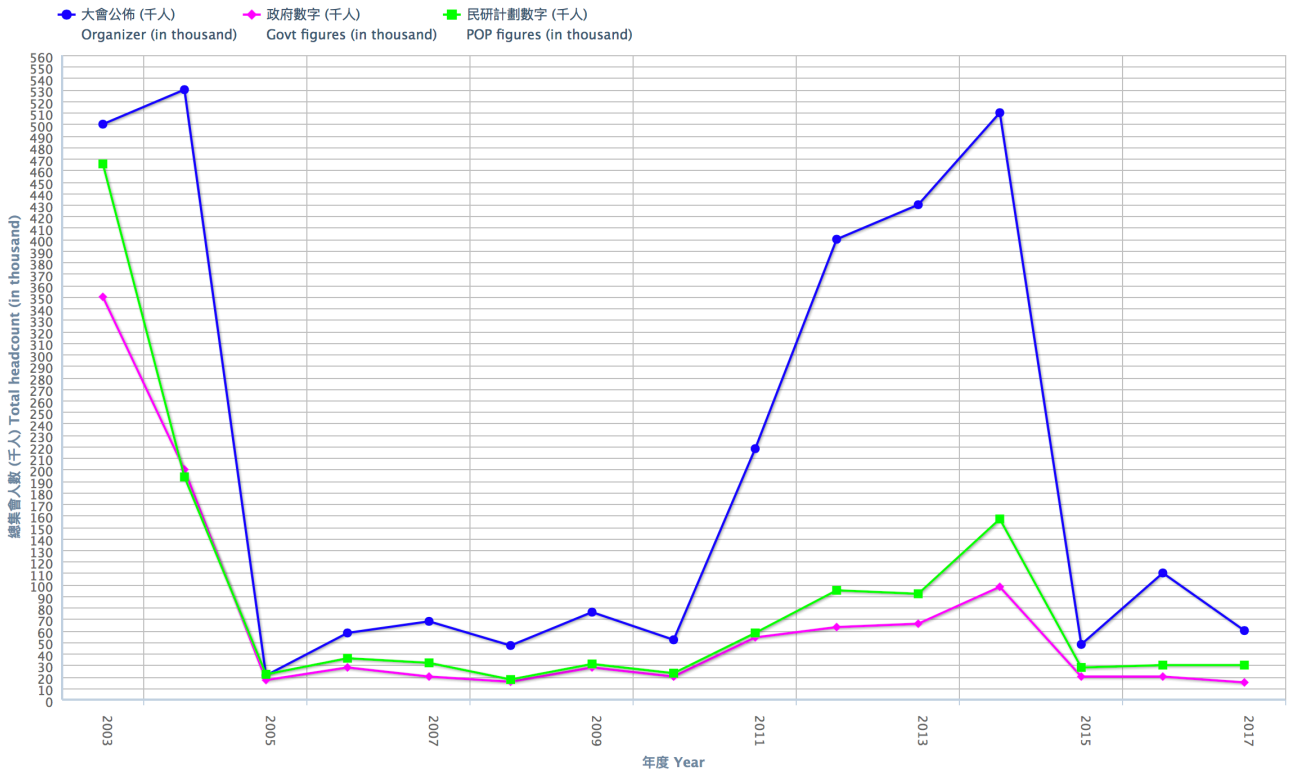

Figure A.1: Turnout at July 1st Marches from 2003 to 2017, as counted by the organizers, as reported in government announcements, and as estimated by the Public Opinion Programme at the University of Hong Kong (all in thousands). Reproduced from the Public Opinion Programme, the University of Hong Kong. Source: `https://www.hkupop.hku.hk/english/features/july1/index.html`, last accessed on December 26, 2017.

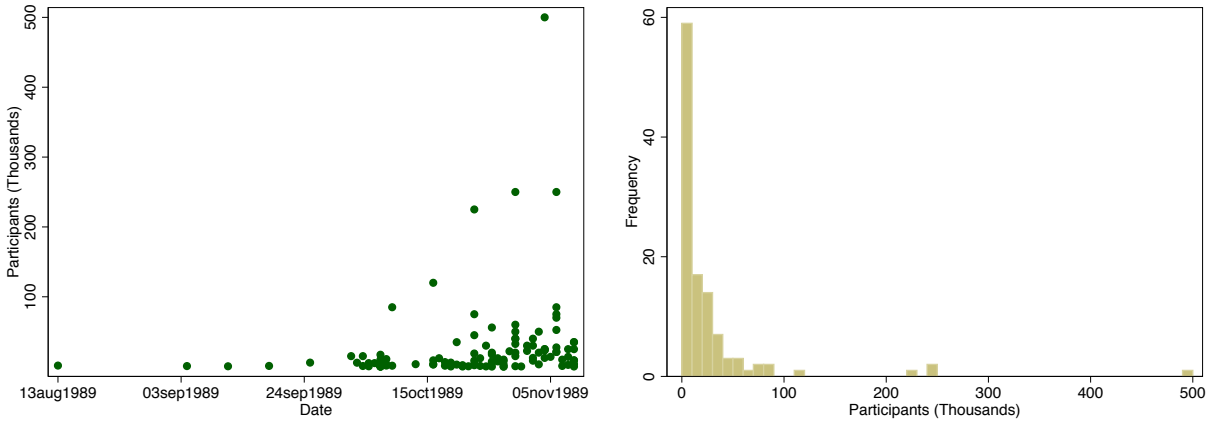

Figure A.2: Protest events in 13 East German district capitals in summer and fall 1989, through November 9, 1989 (when the Berlin Wall fell). Left panel plots individual protests' sizes by date; right panel shows a histogram of protest sizes during the entire time period. When a protest's size is estimated, we take the average of the minimum and maximum estimates. Data come from the Archiv Bürgerbewegung Leipzig.

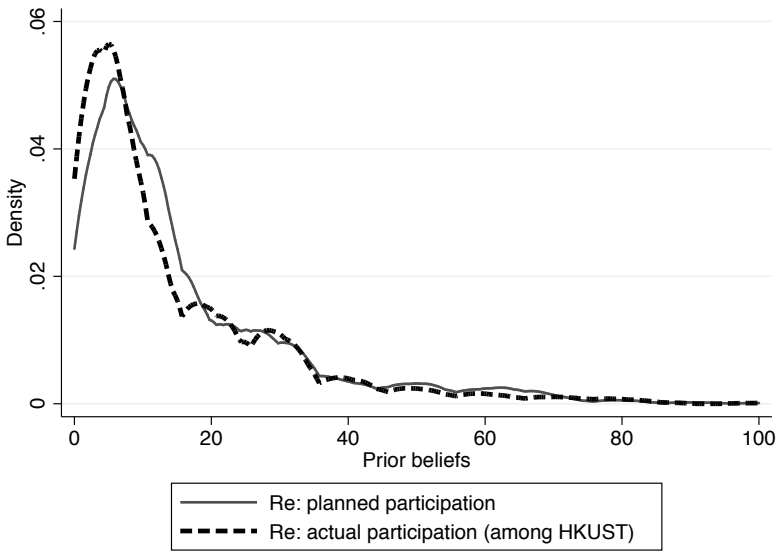

Figure A.3: Distribution of prior beliefs regarding the planned protest participation and actual protest participation of HKUST survey participants.

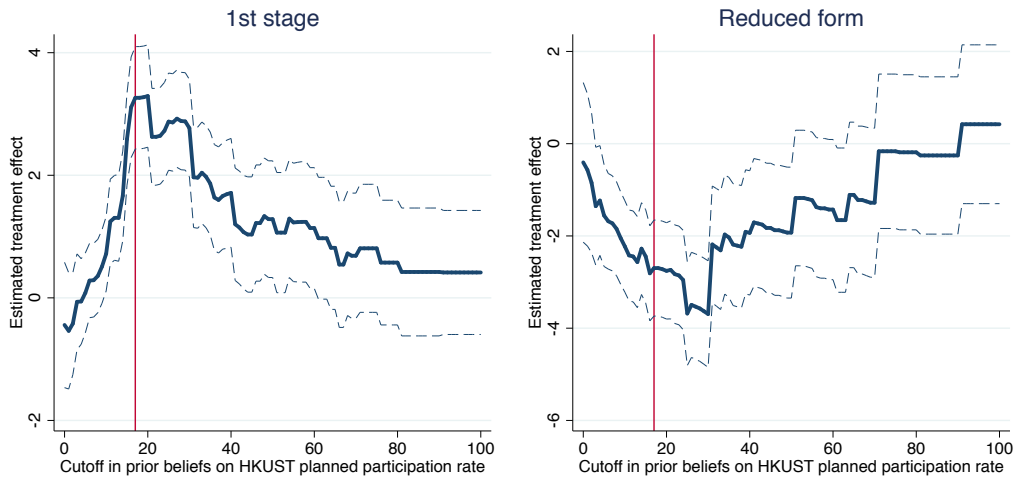

Figure A.4: Estimated treatment effects on posterior beliefs regarding actual participation (first stage, left panel), and on subjects' own protest participation (reduced form, right panel), imposing alternative "switching points" for the treatment variable (i.e., different levels of priors regarding other subjects' planned participation above which we code treatment equal to -1). Figures plot the point estimate (and 95% confidence interval) for integer "switching points" from 0 to 100, with red vertical lines indicating the switching point corresponding to the true mean of HKUST students' planned participation (17%), which is provided to the treated subjects.

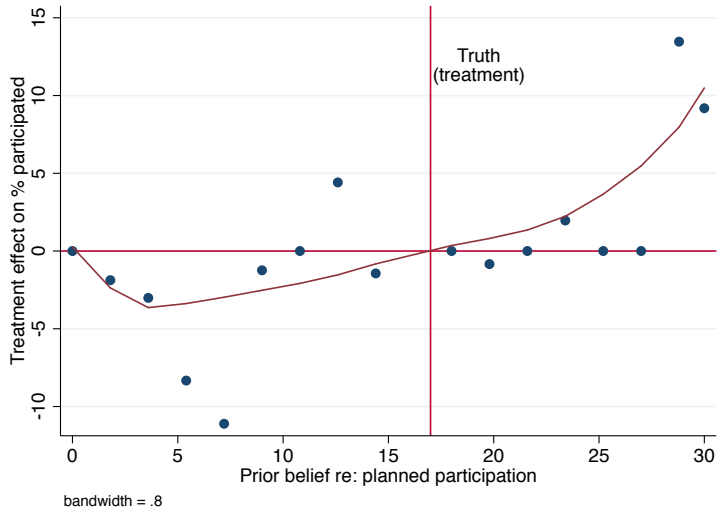

Figure A.5: Non-parametric treatment effect on protest participation ("reduced form"), across prior beliefs regarding the planned participation of HKUST survey participants, smoothed using a lowess kernel at bandwidth = 0.8.

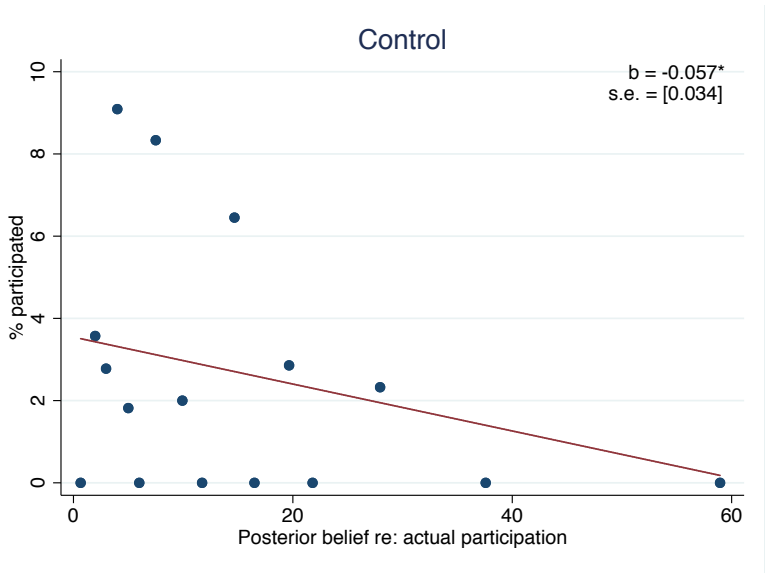

Figure A.6: Binned scatter plot showing the percentage of subjects in the control group who participated in the protest plotted against subjects' posterior beliefs regarding the actual protest participation of HKUST survey participants. Coefficient estimate and standard error are shown from a regression of a participation dummy variable on posterior beliefs.

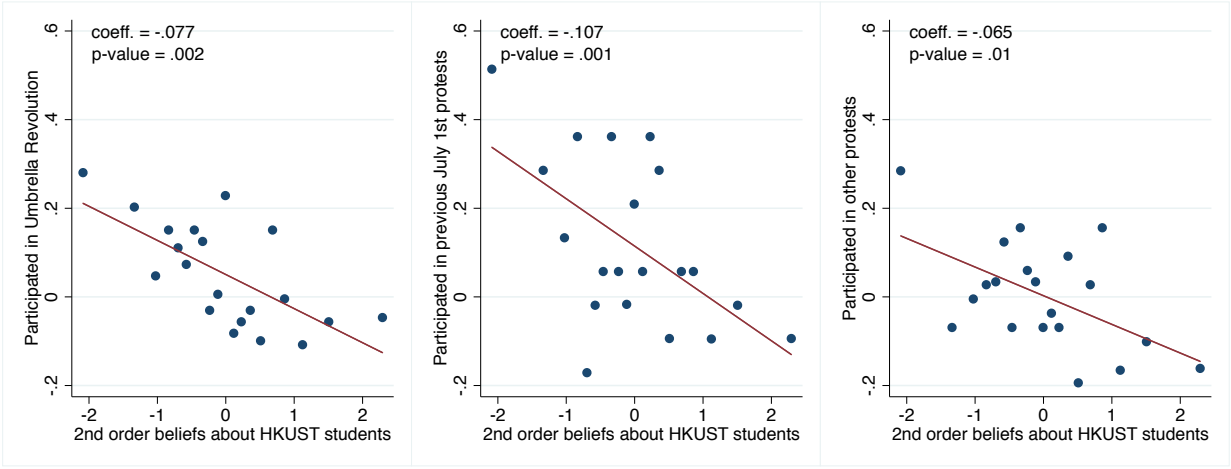

Figure A.7: Binned scatter plots showing past protest participation (standardized) plotted against beliefs about other HKUST students' anti-authoritarian ideology (z-score index constructed using questions from Part 1 of the study). Beliefs about other subjects are residuals, conditional on one's own reported anti-authoritarian ideology.

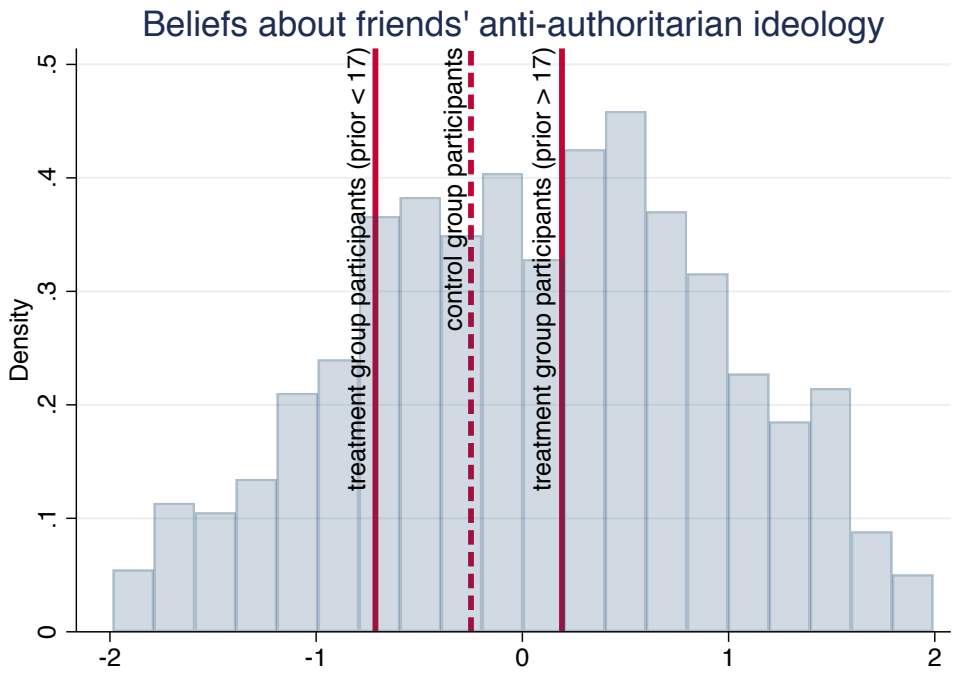

Figure A.8: Distribution of experimental subjects' beliefs about their close friends' anti-authoritarian ideology. Vertical lines indicate the mean level among: (*i*) protest participants in the control group; (*ii*) protest participants in the treatment group whose priors regarding others' planned participation were below the true value (17%); and, (*iii*) protest participants in the treatment group whose priors regarding others' planned participation were above the true value (17%), respectively.

Table A.1: Balance with respect to Part 1 survey items

	Experimental sample			Prior belief on planned particip. below truth			Prior belief on planned particip. above truth		
	Control mean	Treatment mean	p-value	Control mean	Treatment mean	p-value	Control mean	Treatment mean	p-value
Variables:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
A: Anti-authoritarian attitudes									
A.1: Support for democracy	0.100	0.059	0.482	0.094	0.045	0.485	0.114	0.090	0.820
A.2: Support for HK independence	0.107	0.081	0.642	0.107	0.071	0.595	0.108	0.103	0.968
A.3: HK identity (self-reported)	0.172	0.143	0.572	0.161	0.132	0.634	0.202	0.171	0.746
A.4: Unhappiness with political status quo	0.068	0.060	0.897	0.039	0.059	0.769	0.143	0.065	0.474
A.5: Anti-CCP views on current events	0.124	0.053	0.234	0.150	0.056	0.189	0.059	0.045	0.898
A.6: Aggressive pursuit of political rights	-0.008	0.034	0.482	0.026	0.033	0.928	-0.097	0.037	0.232
B: Anti-authoritarian behaviors									
B.1: Participated in Umbrella Revolution	0.401	0.419	0.553	0.402	0.392	0.785	0.400	0.482	0.145
B.2: Vote for pro-democracy party	0.796	0.808	0.594	0.801	0.805	0.867	0.783	0.815	0.466
B.3: Plan to participate in July 1st protest	0.172	0.178	0.794	0.137	0.142	0.867	0.261	0.265	0.933
B.4: HK identity (revealed in identity game)	-0.060	0.015	0.205	-0.079	-0.032	0.481	-0.012	0.128	0.265
B.5: Donation to Demosisto	0.101	0.137	0.076	0.096	0.121	0.272	0.113	0.173	0.143
C: Economic preferences									
C.1: Risk tolerance	-0.025	-0.051	0.663	-0.005	-0.082	0.280	-0.076	0.024	0.346
C.2: Patience	-0.005	0.021	0.656	0.007	0.008	0.995	-0.036	0.053	0.411
C.3: Altruism	0.059	-0.074	0.026	0.084	-0.108	0.008	-0.003	0.004	0.949
C.4: Reciprocity	0.045	-0.094	0.019	0.088	-0.117	0.003	-0.066	-0.038	0.807
C.5: Preference for redistribution	0.002	-0.063	0.279	-0.030	-0.102	0.316	0.085	0.029	0.617
D: Personality traits									
D.1: Big 5 - openness	-0.016	-0.065	0.415	0.010	-0.094	0.139	-0.082	0.004	0.456
D.2: Big 5 - agreeableness	0.047	-0.071	0.045	0.082	-0.112	0.006	-0.042	0.026	0.521
D.3: Big 5 - conscientiousness	-0.015	0.003	0.763	-0.028	0.033	0.386	0.015	-0.069	0.465
D.4: Big 5 - neuroticism	-0.004	0.025	0.622	0.016	0.015	0.983	-0.056	0.049	0.324
D.5: Big 5 - extraversion	0.063	-0.071	0.026	0.118	-0.135	0.000	-0.076	0.080	0.148
E: Cognitive ability									
E.1: Cognitive reflection test	0.052	-0.086	0.023	0.058	-0.089	0.044	0.037	-0.079	0.294

Continued on next page

	Experimental sample			Prior belief on planned particip. below truth			Prior belief on planned particip. above truth		
	Control mean	Treatment mean	p-value	Control mean	Treatment mean	p-value	Control mean	Treatment mean	p-value
Variables:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
E.2: University GPA	-0.080	-0.047	0.565	-0.083	-0.003	0.249	-0.072	-0.149	0.458
F: Economic status									
F.1: HH economic & social status	0.022	-0.033	0.367	0.018	-0.018	0.622	0.033	-0.067	0.362
F.2: Own projected economic status	0.001	-0.058	0.336	0.036	-0.027	0.386	-0.088	-0.129	0.706
G: Background characteristics									
G.1: Gender	0.557	0.535	0.480	0.567	0.522	0.210	0.530	0.566	0.524
G.2: Birth year	1995.6	1995.6	0.969	1995.8	1995.7	0.406	1995.4	1995.6	0.160
G.3: HK-oriented childhood env.	0.080	0.104	0.683	0.065	0.091	0.699	0.120	0.133	0.901
G.4: Non-religious	0.778	0.770	0.745	0.804	0.771	0.434	0.713	0.767	0.270
H: Beliefs about politics									
H.1: Beliefs about future institutions	-0.039	-0.014	0.679	0.010	-0.055	0.357	-0.162	0.083	0.032
H.2: Beliefs about protest efficacy	-0.068	0.037	0.076	-0.068	-0.003	0.365	-0.068	0.133	0.053
I: Beliefs about HKUST students									
I.1: Support for democracy	-0.062	0.004	0.282	-0.057	0.000	0.413	-0.072	0.014	0.482
I.2: Support for HK independence	0.043	0.002	0.499	0.019	-0.010	0.682	0.102	0.031	0.515
I.3: HK identity	-0.001	-0.006	0.923	0.035	-0.007	0.543	-0.092	-0.005	0.427
I.4: Unhappiness with political status quo	-0.035	-0.005	0.603	-0.064	0.006	0.314	0.039	-0.030	0.519
I.5: Aggressive pursuit of political rights	0.066	0.034	0.587	0.065	-0.005	0.323	0.068	0.124	0.600
J: Social life									
J.1: Political social network	0.081	0.064	0.784	0.075	0.018	0.426	0.095	0.173	0.476
J.2: Sociability	-0.044	0.008	0.386	0.007	-0.021	0.690	-0.172	0.077	0.028
K: Beliefs about close friends									
K.1: Support for democracy	-0.028	0.044	0.239	-0.073	0.047	0.087	0.084	0.038	0.714
K.2: Support for HK independence	0.121	0.042	0.167	0.104	0.048	0.413	0.166	0.027	0.197
K.3: HK identity	0.054	0.067	0.816	0.061	0.053	0.901	0.036	0.101	0.561
K.4: Unhappiness with political status quo	-0.038	0.028	0.249	-0.077	0.021	0.151	0.060	0.045	0.884
K.5: Aggressive pursuit of political rights	0.098	0.028	0.230	0.078	-0.014	0.186	0.150	0.128	0.835
L: Media consumption									

Continued on next page

	Experimental sample			Prior belief on planned particip. below truth			Prior belief on planned particip. above truth		
	Control mean	Treatment mean	p-value	Control mean	Treatment mean	p-value	Control mean	Treatment mean	p-value
Variables:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
L.1: Frequency of news consumption	0.097	-0.033	0.031	0.115	-0.003	0.101	0.052	-0.103	0.160
L.2: Pro-democratic source of media	0.121	0.111	0.857	0.127	0.126	0.990	0.107	0.076	0.776
M: Political interest and knowledge									
M.1: Political interest	0.094	0.017	0.205	0.132	0.008	0.084	-0.001	0.039	0.721
M.2: Political knowledge	0.070	0.058	0.847	0.097	0.105	0.914	0.001	-0.053	0.638
# of obs.	406	835		291	586		115	249	

Note: Table tests for balance on outcomes elicited in Part 1 of the study (treatment versus control) for: the full sample (columns 1–3); the subsample of subjects whose priors regarding others' planned participation were below the true value of 17% (columns 4–6); and, the subsample of subjects whose priors regarding others' planned participation were above the true value of 17% (columns 7–9).

Table A.2: Robustness to treatment interacting with unbalanced characteristics

Unbalanced characteristics:	Baseline	Donation to Demosisto	Altruism	Reciprocity	Big 5 - agreeableness	Big 5 - extraversion	Cognitive reflection test	Beliefs about protest efficacy	Frequency of news consumption
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: *treatment effect on posterior beliefs (1st stage)*									
Treatment	5.891***	6.108***	5.951***	5.884***	5.912***	5.943***	5.872***	5.826***	5.935***
(direction adj.)	[0.567]	[0.592]	[0.574]	[0.575]	[0.574]	[0.571]	[0.567]	[0.575]	[0.577]
Panel B: *treatment effect on protest participation (reduced form)*									
Treatment	-2.713***	-2.726***	-2.657***	-2.666***	-2.712***	-2.780***	-2.710***	-2.728***	-2.742***
(direction adj.)	[0.765]	[0.771]	[0.763]	[0.765]	[0.763]	[0.782]	[0.760]	[0.766]	[0.769]
Observations	1234	1234	1234	1234	1234	1234	1234	1234	1234

Note: Table shows "first stage" (Panel A) and "reduced form" (Panel B) estimates, reporting the effects of the experimental treatment on subjects' posterior beliefs about others' participation and on subjects' own protest participation. In both panels, the model allows treatment effects to vary depending on the 8 characteristics elicited in Part 1 of the experiment that are not balanced between treatment and control subjects, as seen in Online Appendix Table A.1. Column 1 replicates the baseline results presented in Tables 3 and 4. Columns 2–9 show results allowing for heterogeneous treatment effects, one unbalanced characteristic at a time. All subjects are pooled and treatment is coded as "-1" for subjects whose priors regarding others' planned participation were above the true value of 17%. All regressions include the relevant lower-order term for the unbalanced characteristic. "First stage" pooled regressions in Panel A also control for an indicator of whether subjects' priors regarding others' planned participation were above true value and its interaction with subjects' prior beliefs regarding others' actual participation.*: Significant at 10%; **: 5%; ***: 1%.

Table A.3: Attrition across survey parts

	Completed Part 1		Completed Parts 1,2,3		Comparison	
	Mean	Std.Dev.	Mean	Std.Dev.	t-stat	p-value
Variables:	(1)	(2)	(3)	(4)	(5)	(6)
Male	0.554	0.497	0.541	0.498	0.704	0.481
Birth year	1995.7	1.489	1995.6	1.473	0.767	0.443
HK-oriented childhood env.	0.087	0.943	0.099	0.933	-0.389	0.697
Non-religious	0.772	0.420	0.774	0.418	-0.202	0.840
HH economic & social status	-0.016	1.001	-0.012	1.002	-0.171	0.864
Own projected economic status	-0.039	0.982	-0.038	1.002	-0.055	0.956
Planned to participate in protest	17.6	38.1	17.6	38.1	0.071	0.943
Prior belief re: % others' planned particip.	16.5	16.6	15.8	15.8	1.203	0.229
Prior belief re: % others' actual particip. (HKUST)	13.8	15.4	13.8	15.2	0.102	0.919
Prior belief re: others' actual particip. (all HK)	160954	168466	155153	155344	0.983	0.326
Treatment	–	–	0.675	0.469	–	–
# of obs.	1576		1234			

Note: Columns 1–2 show descriptive statistics for all subjects who completed at least Part 1 of the study. Columns 3–4 show descriptive statistics for all subjects who completed all three parts of the study. Columns 5–6 show t-statistics and p-values from two-sided t-tests of equality of mean levels of characteristics between subjects who completed at least Part 1 of the study and those who completed all three parts of the study.

Table A.4: Sample representativeness

	Population ratio	Sample ratio	T-test p-value
	(1)	(2)	(3)
Male	0.615	0.543	0.000
Entering cohort of 2012	0.240	0.225	0.172
Entering cohort of 2013	0.254	0.228	0.023
Entering cohort of 2014	0.245	0.246	0.946
Entering cohort of 2015	0.261	0.301	0.001
School of Engineering	0.377	0.328	0.000
School of Sciences	0.237	0.237	0.999
School of Business and Management	0.355	0.364	0.475
School of Humanities and Social Sciences	0.024	0.027	0.485
Interdisciplinary Programs	0.007	0.036	0.000

Note: The source for the population ratio is a HKUST undergraduate student profile compiled by the HKUST Student Affairs Office. The sample ratio is calculated for students who completed Parts 1, 2, and 3 of the study. Column 3 presents p-values from tests of whether the population proportion equals the sample proportion.

Table A.5: Predictors of planned participation & prior beliefs

	Plan to participate in protest		Belief on % students planned to participate	
	beta	s.e.	beta	s.e.
	(1)	(2)	(3)	(4)
A: Anti-authoritarian attitudes				
A.1: Support for democracy	0.060***	[0.009]	-0.414	[0.453]
A.2: Support for HK independence	0.049***	[0.010]	0.240	[0.458]
A.3: HK identity (self-reported)	0.045***	[0.011]	0.557	[0.490]
A.4: Unhappiness with political status quo	0.043***	[0.010]	0.399	[0.470]
A.5: Anti-CCP views on current events	0.044***	[0.009]	-0.889**	[0.440]
A.6: Aggressive pursuit of political rights	0.057***	[0.010]	-0.271	[0.422]
B: Anti-authoritarian behaviors				
B.1: Participated in Umbrella Revolution	0.157***	[0.020]	1.282	[0.861]
B.2: Vote for pro-democracy party	0.018	[0.024]	-0.053	[1.051]
B.3: Plan to participate in July 1st protest	–	–	7.216***	[1.327]
B.4: HK identity (revealed in identity game)	0.023**	[0.010]	0.478	[0.438]
B.5: Donation to Demosisto	0.123***	[0.033]	2.720**	[1.351]
C: Economic preferences				
C.1: Risk tolerance	0.033***	[0.009]	0.408	[0.409]
C.2: Patience	-0.002	[0.009]	-0.777*	[0.460]
C.3: Altruism	0.034***	[0.009]	0.405	[0.440]
C.4: Reciprocity	0.029***	[0.011]	0.200	[0.421]
C.5: Preference for redistribution	0.006	[0.010]	0.319	[0.395]
D: Personality traits				
D.1: Big 5 - openness	0.013	[0.010]	0.711	[0.424]
D.2: Big 5 - agreeableness	0.011	[0.009]	0.578	[0.403]
D.3: Big 5 - conscientiousness	0.002	[0.010]	-0.374	[0.419]
D.4: Big 5 - neuroticism	-0.023**	[0.010]	0.139	[0.411]
D.5: Big 5 - extraversion	0.013	[0.009]	0.611	[0.391]
E: Cognitive ability				
E.1: Cognitive reflection test	-0.014	[0.009]	-0.171	[0.431]
E.2: University GPA	-0.035***	[0.010]	-0.677	[0.423]
F: Economic status				
F.1: HH economic & social status	-0.009	[0.010]	0.005	[0.411]
F.2: Own projected economic status	-0.003	[0.010]	-0.509	[0.404]
G: Background characteristics				
G.1: Gender	0.033*	[0.019]	0.761	[0.834]
G.2: Birth year	0.003	[0.007]	-0.766***	[0.297]
G.3: HK-oriented childhood env.	0.016	[0.010]	0.139	[0.439]
G.4: Non-religious	0.041*	[0.022]	-0.147	[1.001]
H: Beliefs about politics				
H.1: Beliefs about future institutions	0.006	[0.010]	-0.098	[0.419]
H.2: Beliefs about protest efficacy	0.017*	[0.010]	0.694*	[0.420]

Continued on next page

	Plan to participate in protest		Belief on % students planned to participate	
	beta	s.e.	beta	s.e.
	(1)	(2)	(3)	(4)
I: Beliefs about HKUST students				
I.1: Support for democracy	-0.027***	[0.010]	0.174	[0.448]
I.2: Support for HK independence	-0.019**	[0.010]	0.432	[0.417]
I.3: HK identity	-0.019*	[0.010]	0.061	[0.437]
I.4: Unhappiness with political status quo	-0.019*	[0.010]	0.082	[0.441]
I.5: Aggressive pursuit of political rights	-0.014	[0.010]	0.993**	[0.406]
J: Social life				
J.1: Political social network	0.059***	[0.010]	0.674	[0.430]
J.2: Sociability	-0.010	[0.010]	-0.593	[0.436]
K: Beliefs about close friends				
K.1: Support for democracy	-0.016	[0.010]	0.246	[0.456]
K.2: Support for HK independence	-0.006	[0.010]	0.132	[0.447]
K.3: HK identity	-0.017*	[0.010]	0.275	[0.471]
K.4: Unhappiness with political status quo	0.008	[0.010]	0.099	[0.466]
K.5: Aggressive pursuit of political rights	0.001	[0.010]	1.052**	[0.462]
L: Media consumption				
L.1: Frequency of news consumption	0.013	[0.009]	-0.046	[0.395]
L.2: Pro-democratic source of media	0.021**	[0.010]	-0.040	[0.476]
M: Political interest and knowledge				
M.1: Political interest	0.065***	[0.009]	-0.200	[0.415]
M.2: Political knowledge	0.007	[0.010]	-1.158***	[0.426]

Note: Table shows the predictors of planned participation and of prior beliefs regarding other survey participants' planned participation. Subjects' own plans, their priors, and the individual characteristics considered as explanatory variables were all elicited in Part 1 of the study. *: Significant at 10%; **: 5%; ***: 1%.

Table A.6: Treatment effect on changes in beliefs

	Change in beliefs on % participation among HKUST students			Change in belief on total # participation among HK population		
Sample:	All subjects	Prior *below* truth	Prior *above* truth	All subjects	Prior *below* truth	Prior *above* truth
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: *baseline*						
Treatment	9.831***			11337.7***		
(direction adj.)	[0.491]			[3656.6]		
Treatment		4.496***	-10.187***		14644.7**	-5120.5
		[0.552]	[1.611]		[7195.7]	[15009.1]
Panel B: *with controls*						
Treatment	6.886***			9393.9**		
(direction adj.)	[0.538]			[4767.1]		
Treatment		4.655***	-11.607***		16566.3**	-8916.5
		[0.536]	[1.641]		[7294.1]	[15705.1]
Panel C: *trimmed*						
Treatment	9.012***			9722.4**		
(direction adj.)	[0.466]			[3834.6]		
Treatment		4.351***	-7.598***		18549.6**	-5320.2
		[0.578]	[1.580]		[7234.0]	[16828.5]
Observations	1234	873	361	1234	873	361
DV mean (control grp.)	0.664	2.417	-3.805	-18364.9	-17912.3	-19518.5
DV std. dev. (control grp.)	9.914	7.744	13.01	115441	102207	144307
DV mean (all)	0.681	5.430	-10.80	-12468.7	-8098.8	-23036.2
DV std. dev. (all)	13.23	7.874	16.19	103798	97847	116412

Note: Table shows "first stage" effects: the effects of the experimental treatment on subjects' changes in beliefs regarding others' actual protest participation (posteriors minus priors). Columns 1–3 show effects on changes in beliefs regarding other experimental subjects' actual participation, while columns 4–6 show effects on changes in beliefs regarding the total turnout at the protest by all Hong Kong citizens. In columns 1 and 4, all subjects are pooled and treatment is coded as "-1" for subjects whose priors regarding others' planned participation were above the true value of 17%. Columns 2 and 5 (3 and 6) show effects on changes in beliefs for the subsample of subjects whose priors regarding others' planned participation were below (above) the true value of 17%. Panel A is estimated without any controls. Panel B replicates the analysis in Panel A, but adds controls for subjects' background characteristics and economic status (gender, year of birth, a z-score index measuring whether subjects were raised in a Hong Kong-oriented environment, whether subjects were raised in a religious household, the economic status of the household, and own projected economic status), and for subjects' prior beliefs regarding HKUST students' planned participation in the July 1st March. Panel C replicates the analysis in Panel A, but excluding those individuals in the experimental sample with the 5% lowest and the 5% highest prior beliefs regarding other subjects' planned participation. Number of observations refers to that in the baseline specification. *: Significant at 10%; **: 5%; ***: 1%.

Table A.7: Variance decomposition of July 1st March participation

Categories	Univariate R^2	Marginal R^2
Treatment	0.015	0.014
C: Economic preferences	0.011	0.008
C.1: Risk tolerance	0.006	0.004
C.2: Patience	0.000	0.001
C.3: Altruism	0.001	0.000
C.4: Reciprocity	0.006	0.003
C.5: Preference for redistribution	0.000	0.001
D: Personality traits	0.004	0.003
D.1: Big 5 - openness	0.000	0.000
D.2: Big 5 - agreeableness	0.001	0.002
D.3: Big 5 - conscientiousness	0.000	0.001
D.4: Big 5 - neuroticism	0.000	0.001
D.5: Big 5 - extraversion	0.000	0.003
E: Cognitive ability	0.008	0.005
E.1: Cognitive reflection test	0.000	0.000
E.2: University GPA	0.007	0.005
F: Economic status	0.005	0.004
F.1: HH economic & social status	0.003	0.004
F.2: Own projected economic status	0.001	0.001
G: Background characteristics	0.006	0.005
G.1: Gender	0.001	0.001
G.2: Birth year	0.000	0.001
G.3: HK-oriented childhood env.	0.003	0.002
G.4: Religiosity	0.001	0.001

Note: Variance decomposition exercise uses 1,234 Hong Kong locals subjects who completed all 3 parts of the study. All subjects are pooled and treatment is coded as "-1" for subjects whose priors regarding others' planned participation were above the true value of 17%. Univariate R^2 is the R-squared from a regression predicting the July 1st March participation using only the factor (or the z-score index) indicated in each row. Marginal R^2 is the incremental R-squared adding the single factor (or z-score index) indicated in a given row to a regression model that already included all of the other factors listed.

Table A.8: Plans to participate and subsequent treatment effect

Sample:	Participated in 2016 July 1st March			Plan changed: participated
	All subjects	Excl. participants w/ plan not to attend	Excl. participants w/ plan to attend	All subjects
	(1)	(2)	(3)	(4)
Panel A: *baseline*				
Treatment	-2.713***	-4.493***	-1.142**	-1.031**
(direction adj.)	[0.765]	[1.475]	[0.455]	[0.422]
Panel B: *with controls*				
Treatment	-2.143**	-3.944**	-1.361**	-1.284**
(direction adj.)	[0.913]	[1.954]	[0.552]	[0.527]
Panel C: *trimmed*				
Treatment	-2.467***	-4.238***	-1.149**	-1.057**
(direction adj.)	[0.797]	[1.565]	[0.479]	[0.449]
Observations	1234	525	1188	1234
DV mean (control grp.)	2.743	6.832	2.545	2.494
DV std. dev. (control grp.)	16.35	25.31	15.77	15.61
DV mean (all)	2.998	7.048	1.431	1.378
DV std. dev. (all)	17.06	25.62	11.88	11.66

Note: Table shows "reduced form" estimates, reporting the effects of the experimental treatment on subjects' own protest participation. All subjects are pooled and treatment is coded as "-1" for subjects whose priors regarding others' planned participation were above the true value of 17%. In columns 1 and 4, the sample is all subjects. Column 2 excludes subjects who indicated with certainty that they planned not to participate in the 2016 July 1st March; column 3 excludes subjects who indicated with certainty that they planned to participate in the 2016 July 1st March. "Plan changed: participated" is an indicator equal to 1 if a subject did not indicate with certainty that s/he planned to participate in the 2016 July 1st March and actually did participate. Panel A is estimated without any controls. Panel B replicates the analysis in Panel A, but adds controls for subjects' background characteristics and economic status (gender, year of birth, a z-score index measuring whether subjects were raised in a Hong Kong-oriented environment, whether subjects were raised in a religious household, the economic status of the household, and own projected economic status), and for subjects' prior beliefs regarding HKUST students' planned participation in the July 1st March. Panel C estimates the baseline specification, but excluding those individuals in the experimental sample with the 5% lowest and the 5% highest prior beliefs regarding other subjects' planned participation. Number of observations refers to that in the baseline specification. *: Significant at 10%; **: 5%; ***: 1%.

Table A.9: Heterogeneous treatment effects by friends' expected participation

Sample:	Prior *below* truth	Prior *above* truth
	(1)	(2)
Panel A: *treatment effect on posterior beliefs (1st stage)*		
Treatment	4.817***	-7.980***
	[0.606]	[1.970]
Friend participating	2.908*	4.670
	[1.488]	[3.040]
Treatment ×	-2.061	-3.549
friend participating	[1.669]	[3.336]
Panel B: *treatment effect on protest participation (reduced form)*		
Treatment	-0.647	0.676
	[0.849]	[1.783]
Friend participating	10.350**	-1.351
	[4.222]	[1.350]
Treatment ×	-7.343	14.32***
friend participating	[4.538]	[4.001]
Observations	873	361
1st stage DV mean (control grp.)	8.44	28.30
1st stage DV std. dev. (control grp.)	8.46	15.54
1st stage DV mean (all)	11.40	22.02
1st stage DV std. dev. (all)	7.99	12.92
2nd stage DV mean (control grp.)	3.472	0.885
2nd stage DV std. dev. (control grp.)	18.34	9.41
2nd stage DV mean (all)	2.062	5.263
2nd stage DV std. dev. (all)	14.22	22.36

Note: Table shows "first stage" (Panel A) and "reduced form" (Panel B) estimates, reporting the effects of the experimental treatment on subjects' posterior beliefs about others' participation and on subjects' own protest participation. In both panels, the model allows treatment effects to vary depending on subjects' beliefs that at least one of their five best friends would participate in the protest. Regressions include the treatment indicator, an indicator that subjects believe that at least one of their five best friends would participate in the protest, and their interaction. Column 1 is estimated for the subsample of subjects whose priors regarding others' planned participation were below the true value of 17%. Column 2 is estimated for the subsample of subjects whose priors regarding others' planned participation were above the true value of 17%. Subjects' beliefs regarding their friends' participation in the protest are elicited before treatment assignment and are balanced between treatment and control group students. 27.2% of the experimental subjects expected that at least one of their five best friends would participate in the upcoming 2016 July 1st March. *: Significant at 10%; **: 5%; ***: 1%.

Table A.10: Two stage estimates on protest participation (changes in beliefs)

	Participated in 2016 July 1st March		
Sample:	All subjects	Prior *below* truth	Prior *above* truth
	(1)	(2)	(3)
Panel A: *baseline*			
Change in belief	-0.276***	-0.468**	-0.626***
	[0.063]	[0.234]	[0.248]
Panel B: *with controls*			
Change in belief	-0.311**	-0.442*	-0.614***
	[0.120]	[0.230]	[0.223]
Panel C: *trimmed*			
Change in belief	-0.274***	-0.512**	-0.752**
	[0.073]	[0.257]	[0.363]
Observations	1234	873	361
1st stage DV mean (control grp.)	0.664	2.417	-3.805
1st stage DV std. dev. (control grp.)	9.914	7.744	13.01
1st stage DV mean (all)	0.681	5.430	-10.80
1st stage DV std. dev. (all)	13.23	7.874	16.19
2nd stage DV mean (control grp.)	2.743	3.472	0.885
2nd stage DV std. dev. (control grp.)	16.35	18.34	9.41
2nd stage DV mean (all)	2.998	2.062	5.263
2nd stage DV std. dev. (all)	17.06	14.22	22.36

Note: Table shows two-stage estimates of the effects of beliefs about other subjects' turnout in the protest on one's own turnout. The first stage estimates the effects of the experimental treatment on subjects' changes in beliefs regarding other subjects' actual protest participation (posteriors minus priors). The second stage exploits variation in beliefs regarding other subjects' participation induced by the experimental treatment to estimate the effect of beliefs about others' protest turnout on one's own turnout. In column 1, all subjects are pooled and treatment is coded as "-1" for subjects whose priors regarding others' planned participation were above the true value of 17%. Column 2 (3) shows estimates for the subsample of subjects whose priors regarding others' planned participation were below (above) the true value of 17%. Panel A is estimated without any controls. Panel B replicates the analysis in Panel A, but adds controls for subjects' background characteristics and economic status (gender, year of birth, a z-score index measuring whether subjects were raised in a Hong Kong-oriented environment, whether subjects were raised in a religious household, the economic status of the household, and own projected economic status), and for subjects' prior beliefs regarding HKUST students' planned participation in the July 1st March. Panel C estimates the baseline specification of Panel A, but excluding those individuals in the experimental sample with the 5% lowest and the 5% highest prior beliefs regarding other subjects' planned participation. Number of observations refers to that in the baseline specification. *: Significant at 10%; **: 5%; ***: 1%.

Table A.11: Robustness – re-weighting the sample

Reweighting:	Baseline	Part 1 subjects	All HKUST
	(1)	(2)	(3)
Panel A: *treatment effect on posterior beliefs (1st stage)*			
Treatment	5.891***	5.854***	5.905***
(direction adj.)	[0.567]	[0.530]	[0.531]
Panel B: *treatment effect on protest participation (reduced form)*			
Treatment	-2.713***	-2.646***	-3.208***
(direction adj.)	[0.765]	[0.752]	[0.848]
Observations	1234	1234	1234
1st stage DV mean (control grp.)	14.04	13.92	14.32
1st stage DV std. dev. (control grp.)	14.10	14.05	14.27
1st stage DV mean (all)	14.50	14.44	14.56
1st stage DV std. dev. (all)	10.83	10.80	10.89
2nd stage DV mean (control grp.)	2.709	2.669	2.712
2nd stage DV std. dev. (control grp.)	16.26	16.14	16.26
2nd stage DV mean (all)	2.981	2.934	3.114
2nd stage DV std. dev. (all)	17.01	16.88	17.38

Note: Table shows "first stage" (Panel A) and "reduced form" (Panel B) estimates, reporting the effects of the experimental treatment on subjects' posterior beliefs about others' participation and on subjects' own protest participation. Column 1 replicates the baseline results presented in Tables 3 and 4. Column 2 re-weights the observations in the experimental sample to match the study participants who completed at least Part 1 of the study (but not necessarily Parts 2 and 3) on gender, birth year, Hong Kong-oriented childhood environment, religiosity, household income & social status, and own projected economic status. Column 3 re-weights the observations in the experimental sample to match the HKUST student population on gender, cohort, and the school of study within the university. *: Significant at 10%; **: 5%; ***: 1%.

Table A.12: Protest participation and pro-sociality

	Participated in 2016 July 1st March				
	(1)	(2)	(3)	(4)	(5)
Pro-sociality *(z-score index)*	1.285***	1.172***	1.285***	1.007**	0.889*
	[0.518]	[0.514]	[0.518]	[0.495]	[0.494]
Observations	1234	1234	1234	1234	1234
Control for treatment status	No	Yes	No	No	Yes
Control for posterior beliefs	No	No	Yes	No	Yes
Control for anti-authoritarian ideology	No	No	No	Yes	Yes
DV mean (control grp.)	2.709	2.709	2.709	2.709	2.709
DV std. dev. (control grp.)	16.26	16.26	16.26	16.26	16.26
DV mean (all)	2.981	2.981	2.981	2.981	2.981
DV std. dev. (all)	17.01	17.01	17.01	17.01	17.01

Note: Table shows regressions predicting participation in 2016 July 1st March using individuals' pro-sociality: a z-score index constructed using data collected in Part 1 of the study on altruism and reciprocity. Column 1 presents the simple regression without added controls. Column 2 controls for treatment status (direction adjusted). Column 3 controls for posterior beliefs about others' participation. Column 4 controls for an anti-authoritarian ideology index using data collected in Part 1 of the study. Finally, Column 5 controls for treatment status, posterior beliefs and individuals' ideology. *: Significant at 10%; **: 5%; ***: 1%.

t:prosociality