Political Persuasion in the Information Age: Results from a Field Experiment

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Abstract

We study the effectiveness of online persuasion via a lab-in-the-field experiment that exposes respondents to short videos on a proposed national security law in China. Exposure to "nationalist" videos supporting the law reduced willingness to provide critical feedback to the government by 4.8% (7.1% for the "emotional" version), while critical videos increased criticism by 6.6%; for both nationalist and critical videos, the effect on providing supportive feedback was the opposite. In a follow-up experiment, we informed subjects of the extent to which others had a negative view of those criticizing the law, which completely erased any effect of the video on willingness to provide feedback. Overall, our findings indicate the potency of online content in its short-run impact on political dissent, and that these effects operate in large part through their impact on second-order beliefs.

Keywords: Social media, TikTok, Short video, Institutional change, The spiral of silence, Nationalism
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1 Introduction

In both electoral democracies and autocratic regimes, political leaders and aspirants vie for the support of citizens through direct outreach and political advertising. This has led to an extensive and growing literature on political persuasion that has focused in large part on television ad campaigns (Fowler *et al.*, 2021). However, the channels of persuasion have shifted online, accompanied by concerns over the influence that even brief video clips or pop-up ads can have over viewers' attitudes and beliefs.

In this paper, we study the effectiveness of online persuasion via a field experiment that emulates as closely as possible the actual consumption of internet content, and employs as outcomes real-stakes decisions to provide feedback to the government. The specific context is a controversial set of amendments to national security laws, proposed by China's parliament in **August** 2023, to criminalize vaguely defined actions that were "harmful to national sentiment." The legal change was subject to a public consultation period, during which time citizens could provide feedback via an online portal. In the 30 days the law was subject to public feedback in September 2023, approximately 100,000 individuals submitted more than 120,000 comments, an indication of the controversy surrounding the proposed changes.[1]

Coincident with the comment period, the social media app TikTok and other platforms were flooded with videos supporting the proposed changes, stressing the importance of defending national sentiment, with seemingly independent videos often employing similar rhetoric.[2]

To evaluate how such videos influence public attitudes, we conducted an online experiment during the law's public consultation period, through an online survey administered to over 2,000 participants from across China. Each subject assigned to one of the treatment groups watched one or two representative one-minute videos sourced from TikTok.

[1] The median number of commenters on government rules was 322 as of 2024. There is a long right tail, however – the mean number of commenters is 5,400; for example, just over 111,000 commenters provided feedback on major changes to the civil code that were proposed in 2018.

[2] The uniformity of language led to speculation that the videos shared a common source, though there is no direct evidence of this.

These videos fell into two broad categories: the first category included "nationalism clips," which supported the new legal provisions by emphasizing the need to protect national sentiment; the second included "critical clips," which encouraged participants to take a stand against the amendments by highlighting concerns about vague language and the potential for unchecked administrative power. In each case, participants saw either a "calm" video or a more "emotional" one. In all cases, the Tiktok clips were sourced from materials that were widely circulating on the internet at the time. Finally, a set of control participants responded to an otherwise identical survey but did not watch any video.[3] After viewing the videos, participants were prompted to express their opinions on the legislation–either in support of or in opposition to the amendments–via the survey, with the assurance that their responses would be submitted anonymously to the government feedback website.

We find that even these brief exposures significantly shifted respondents' attitudes and their willingness to voice dissent. Participants treated only with a critical clip were 6.6% percentage points more likely to submit comments suggesting revisions to the proposed amendments, a very large effect relative to the control group base rate of 11.42%. Participants in the nationalist clip treatments were 4.8% less likely to submit suggestions relative to the control group, and 4.0% more likely to write in support of the amendments (we observe no effect of the critical clip on supportive comments). Interestingly, the latter effects were more pronounced for exposure to the emotional rather than the calm version and, somewhat surprisingly (and counter to our priors), the effect of the critical clip was stronger for the calm version.

These large effects raise the question of why even brief messages are so persuasive. The most immediate explanation is that they provide new information, leading viewers to update their beliefs about the proposed legal changes (and possibly this information is more immediately salient when the message is delivered in a dramatic and emotional manner). Alternatively, the videos may influence viewers' beliefs about the extent of support for the amendments – and relatedly, how others might view dissenters. If these beliefs in turn influence participants' willingness to express a point of view, the nationalism

[3] We discuss in Section 3.1 the potential ethical concerns raised by treating citizens with exposure to these messages.

clip would reduce criticism of what is perceived as a popular policy (and by the same rationale, the critical clip would increase willingness to provide feedback). Finally, the clips may affect willingness to provide feedback due to demand effects, to the extent that they affect what respondents see as the "right" answer, from either the experimenters' or government's perspective.

A further set of analyses suggest that our main results may be driven to a large extent by changes in second-order beliefs, i.e., the effect of videos on beliefs about perceptions of how others perceive the law and those who voice dissent. We first note that in our original experiment, before viewing the video, participants rated their own tolerance of opposition to the law to be higher than the tolerance that they perceived for others.[4] Second, while not obtained through experimental variation, we find that perceived intolerance of others is a very strong predictor of whether a respondent expresses support for or opposition to the law – as strong a predictor of own-intolerance.

Building on these insights, we conducted a supplementary experiment 7-10 days after our initial survey (still within the 30-day window during which the law was open to citizen feedback), to probe the extent to which misperceptions about the tolerance of others causally affects willingness to provide feedback. (We were left with a total of 536 valid responses after the same screening process as was used for the main experiment.) This supplementary experiment differed in two ways from the original one. First, given the smaller sample size, we focused only on the nationalist treatments, exploiting experimental variation between those viewing the calm versus emotional versions. Second, after viewing the experiment, respondents were given information about how past participants (i.e., those in the original experiment) responded to the question on how much expressing negative views of the proposed legal changes reflected a lack of patriotism. In contrast to the main experiment, we find that the emotional treatment is no longer predictive of writing feedback – whether supporting or opposing – on the new law.t

The preceding results also help, at least indirectly, to address the concern that the videos signal *government* opinion on the law, and as a result may affect participants' beliefs

[4] For example, on a 1-10 Likert Scale, participants rated their own belief that opposition to the law reflected "worship of foreign cultures" as 5.1 on average, but estimated others' views as 5.7.

about government retaliation (Madan *et al.*, 2023). More broadly, while we cannot fully rule out a role for social desirability bias, we took steps to minimize such concerns (some of which were also necessary to minimize any actual risk to study participants). These included removing content in the nationalist clip that could link it to government endorsement and assuring participants that no identifying information would be collected.[5] We also explored the possibility that government-related concerns played an important role in driving participants' responses via a follow-up list experiment with 312 new participants. We asked these respondents (via a list design) to assess public perceptions of the extent to which the government would gain from the proposed legal provisions and, more importantly, whether they thought individuals would be targeted for expressing dissenting views. We find that neither concern was significant. [6]

We believe that our paper is the first to capture directly the consequences of short social media clips on political preferences or beliefs. We view our targeted study as complementing large-scale experiments that alter the social media content of study participants. An ambitious pair of studies focused on Facebook and Instagram users during the 2020 U.S. presidential election campaign documents relatively muted impacts of limiting political content (Allcott *et al.*, 2024) or altering the algorithm governing platforms' feeds (Guess *et al.*, 2023). Our results suggest at least the possibility of a short-term impact in a somewhat distinct political context, and also allow us to explore potential channels through which social media exposure can impact political behavior.

[5] For example, we removed the opening segments of some short videos, as they might be mistakenly perceived as coming from officially endorsed channels.

[6] Because we are interested in the longer-term effect of the brief videos we study here, particularly the nationalist ones, we conducted follow-up surveys at 6 months and 12 months after the baseline experiment, collecting a total 333 valid responses, a response rate of 16%. In the follow-up experiments, we presented the same information as in the baseline, including the legal provision, and asked respondents to report their intolerance levels (both self-reported and predicted for others) along with their attitude toward the legal provision. (Participants were not asked to watch the video again, since we are interested in the longer-term effects of the initial viewing, and their willingness to provide feedback was purely hypothetical, as the comment period had long since closed.) It is perhaps unsurprising that after as little as 6 months, most respondents have little or no recall of the content of the proposed legal provision (90%) nor the videos they had watched (82%). Furthermore, the effect of the videos was no longer detectable in stated willingness to provide feedback. Yet from the perspective of influencing short-term beliefs and preferences—which is more relevant for impacting responsiveness during the brief comment window—our main results are likely more relevant. It is also outside the scope of our experimental design to assess the impact of repeated exposure to short video clips that carry a similar message, as would be the "treatment" for many Tiktok users.

Our findings on the role of first- versus second-order beliefs shed light on the mechanisms through which social media exposure can impact preferences. Given the role we find for the social intolerance of others, our work contributes to the growing body research on how perceptions and misperceptions of others can reinforce conformity to a set of social expectations and/or willingness to show disapproval of establishment opinions (Cantoni *et al.*, 2019; Bursztyn *et al.*, 2020). Given our focus on second-order beliefs, our results also show the potential relevance of the spiral of silence (the unwillingness to express opinions that are potentially erroneously believed to be contrary to social consensus; see Noelle-Neumann 1974) for political discourse. On this theme, our work complements the findings of Huang & Ho (2024), which explores how the spiral of silence can be disrupted via an information intervention in a lab setting involving Berkeley undergraduates. While "spiral of silence" is often seen as a social phenomenon that is absent in our context, past work has shown that even in anonymous settings, social norms can have decisive effects on individual behavior; the influential work of Postmes *et al.* (2001) finds that social norms can be even more impactful in anonymous settings.

The particular features of our experiment allow us to consider the interaction between social media exposure and the spiral of silence, and to do so in a setting in which there may be legitimate fear of reprisal for dissenting viewpoints, and in a collectivist society in which concerns of social isolation may be stronger (Postmes *et al.*, 2001; Huang, 2005; Gearhart & Zhang, 2018; Bursztyn & Yang, 2022).

2 Institutional Background

2.1 Public Opinion Feedback in the Chinese Legislative Process

China's central government initiated online public participation in its legislative process in July 2005 when the draft Property Law was published by the *Legislative Affairs Commission of the National People's Congress (NPC) Standing Committee* on the NPC official website for public input.

The feedback period typically extends for one month, allowing citizens to submit

anonymous feedback through the NPC website.[7] Individuals can offer multiple comments, each potentially addressing specific clauses of the draft. While the total comment count as well as the total number of commenters is visible, individual comments remain private unless commenters opt to publish their feedback on other platforms.[8] Once the feedback period concludes, only the draft and the number of submitted comments are displayed.

In April 2008, the NPC Standing Committee's Meeting institutionalized public participation by mandating that all draft laws be published online to collect public input. Since then, it has become standard practice to invite public opinion on proposed legislation. December 31, 2024, the NPC website had recorded over 300 public consultations, many of which involved multiple revisions. Laws that closely impact daily life, such as the Labor Contract Law, the Law on the Protection of Women's and Children's Rights, and the Civil Code[9], have drawn significant public engagement. For example, the 2018 revisions to the Civil Code attracted 440,491 comments from 111,208 participants (as compared to the median number of comments and commenters, which are 949 and 322 respectively).

After the collection of public feedback, the legislative process requires an explanation of which clauses were amended based on public consultation, especially those with substantial comments.

It also mandates justifications for any clauses that were commented on but left unchanged. In practice, however, the feedback details are rarely disclosed with the final legislation, leaving contributors unsure of the extent to which their suggestions were taken under consideration. Balla *et al.* (2025) show that rulemaking is responsive to critical

[7] See Balla *et al.* (2025) for a more detailed discussion of the Notice and Comment process in China, which bears some similarity to the U.S. process. In short, Chinese citizens can submit their opinions on proposed amendments to major laws, such as the "Public Security Administration Punishments Law" in this study, via the NPC website (`www.npc.gov.cn`), while other laws may be handled by specific agencies that oversee them.

[8] Particularly for controversial amendments, some interested citizens and legal experts may choose to post their submitted comments on social media sites like Weibo and TikTok to signal their stance and rally similar feedback from the public.

[9] The *Civil Code of the People' s Republic of China* is the first law named as a code in New China, holding a foundational position in the legal system. At the same time, it serves as a declaration and guarantee of civil rights. If the Constitution focuses on restricting public power, the *Civil Code* emphasizes protecting private rights. Almost all civil activities, from major matters like signing contracts and establishing companies to minor ones like paying property fees or divorcing, can find their basis in this law.

commentary and in salient policy areas, arguing that it serves as a means to gather information on potential dissent.

2.2 The Revision of the "Public Security Administration Punishment Law"

Within China's legal framework, the "Public Security Administration Punishments Law" (abbreviated to Public Security Law below) is of considerable significance, covering a wide range of behaviors similar to those addressed by its Criminal Law, but for less severe offenses. The Public Security Law is also quite recent, passed in 2005 (whereas the original Criminal Law was ratified in 1979). [10] In contrast to criminal statutes, public security law follows a simplified procedure that gives public security agencies impunity to impose penalties, including detention of up to one month, without requiring involvement from a public prosecutor or judiciary. If a penalty is later deemed erroneous, individuals can seek state compensation only after paying their fine and serving their sentence.

The law includes vaguely defined offenses like "picking quarrels and provoking trouble," which enabled authorities to use broad charges to detain individuals for activities such as petitioning, with detentions of up to one month.

In late 2023, seventeen years after its initial enactment, the central government proposed revisions to the Public Security Law, with a Notice and Comment period of September 1-30. During this time, public feedback was collected on the National People's Congress website. Key areas in the draft include amendments on information security and privacy rights, but the most widely discussed change was the addition of Article 34, which aimed to prevent actions that were disrespectful toward national heroes or harmed national sentiments. The more controversial elements included vaguely worded prohibitions against: (1) "wearing clothing or symbols in public places that are detrimental to the spirit of the Chinese people and hurt the feelings of the Chinese people, or forcing others to do so"; and (2) "producing, transmitting, promoting, or disseminating items or speech that is detrimental to the spirit of the Chinese people and hurts the feelings of the

[10] For the primary text of the public security law, see here: https://www.gov.cn/ziliao/flfg/2005-08/29/content_27130.htm; for Criminal Law, see here: http://www.npc.gov.cn/zgrdw/englishnpc/Law/2007-12/13/content_1384075.htm (last accessed 31 Dec 2024).

Chinese people." Violations could lead to detention of five to ten days and fines of up to 5,000 RMB (or 700 USD).

This amendment sparked widespread debate online. Legal professionals cautioned that phrases like "detrimental to the spirit of the Chinese people" were too abstract and vague, leaving them open to subjective interpretation by law enforcement. This ambiguity, they warned, could result in excessive discretion and even abuses of power. At the same time, pro-nationalist videos endorsing these terms were widely circulated. Supporters of these videos label critics as anti-government, accusing them of sowing distrust in law enforcement. As we noted in the introduction, videos endorsing Article 34 outnumbered those critical of it by a factor of 5, so that if they were persuasive to potential critics, the videos could blunt dissent.

3 Experimental Design

From September 1 to September 30, 2023, the NPC solicited public feedback on the draft amendments to the Public Security Administration Punishment Law through its official website. We conducted our experiment during this period on WenJuanXing, China's largest professional online survey platform.[11]

There were three phases to the experiment: a pilot; our main experiment; and a supplementary module that aimed to allow us to refine the interpretation of our main results. The three involved a total of 3,342 participants, and generated a total of 3,083 valid questionnaires (113 in the pilot; 2122 in the main experiment; and 848 in two supplementary experiments). An overview of the various parts to each of the three phases may be seen in Figure 1.

We begin by describing the baseline experiment, which took place September 10-20, 2023. Respondents were randomly assigned to one of four groups, and within the first two, they were further randomly assigned to two subgroups, as described below.[12]

[11] WenJuanXing is the largest market survey company in China. While it has not been extensively used by researchers in economics, it has had over 100 Global Fortune 500 companies as clients.

[12] The detailed steps of the main experiment can be see in the Figure A1. To randomize participants, we presented them with a set of 6 randomly ordered characters, À, Á, Â, Ã, Ä, and Å. Respondents picked a character, which determined their assignment.

In all but the control group, respondents were shown videos related to the proposed changes to the Public Security Law. Note that in all cases, the videos were re-edited to mask the publisher's identity to reduce the chances that they would be directly associated with government-affiliated entities.

Nationalism Treatment: Respondents were shown one of two videos that supported the proposed legal changes, presenting the amendments as a matter of nationalism, and thus discouraging opposition. We selected the videos from among those with over 300,000 "likes" on TikTok, to ensure that the clips selected represented content that TikTok users would likely encounter in reality. Approximately half (52.8%) of respondents were shown a clip that featured an emotional narrator who spoke disparagingly of opponents to the legal change. We refer to this video as the *emotional nationalist clip*. The other half of respondents were randomly assigned to a *calm nationalist clip*, in which a speaker in a more measured tone rebuts concerns expressed by those opposed to the legal change.

Critical Treatments: Respondents in this group watched videos that critiqued the arbitrariness of the National Security Law and the lack of regard for legal principle. The videos further described the proposed provisions as intentionally vague in order to permit broad discretionary power, thus raising concerns about government overreach and potential abuse of authority. Again, we used one video that used more forceful rhetoric to criticize the discretion that the new law would give to officials (*emotional critical clip*, while the other took a more muted tone, explaining how vague legal provisions could leave enforcement guidelines unclear (*calm critical clip*). As with the nationalism treatments, approximately half were assigned to each of the emotional and critical subgroups.

Mixed Treatments: Participants in this group watched both a nationalist video and a empowerment video (again both emotional), with the order of presentation randomized. Since we find no difference throughout on ordering, we simply combine all mixed treatment respondents into a single group.

Control Group: Participants in this group do not watch any related videos.

This set of treatments allowed us to analyze the differential impacts of nationalist

and empowerment video content, as well as the combined effect of both, on respondents' attitudes and behaviors toward the proposed legal provisions. Appendix Table A1 outlines the themes and representative phrases from each video used in various treatments.

Immediately following the treatment, participants were asked about their willingness to provide an opinion on the new law, which is our first and most direct measure of the treatments' impacts. Specifically, we ask: "Do you have any opinions on this newly added regulation?(您对此新增条例有无修改意见?)" While in English translation, the question is quite ambiguous about whether it is positive or negative, "opinion" (意见) tends to reflect oppositional views in Chinese. Consistent with this interpretation, responses to this question were very strongly correlated with ultimately providing oppositional feedback on the law ($\rho = 0.26$), while responses were completely uncorrelated with providing positive feedback.

Respondents were then presented with several anecdotes that highlighted the potential benefits of commenting. This part of the design was motivated by results from our pilot study, which involved 113 participants (see Figure 1). After watching a video (randomly selected from one of the four in the main experiment) pilot respondents were asked, "What do you think are the reasons people are reluctant to provide feedback?" The three primary reasons we identified were as follows: 1) They felt that as long as they complied with the law, the expansion of administrative power would not affect them personally, 2) They could free-ride on the efforts of others, and 3) They believed their opinions would be ignored.

We provided all participants with information that addressed these potential concerns about the benefits of providing feedback, via a set of three examples that illustrated: 1) the potential for misuse of public power; 2) the public's general enthusiasm for providing feedback on proposed laws; and 3) the possibility of changing legislation via public campaign. We describe the specific illustrative examples in more detail in Section 5 and the full text is available in Appendix A.

After each illustrative example was provided, respondents were again queried about their willingness to provide feedback on the new law, to track how the treatment effect

interacted with knowledge about feedback efficacy.

The main experiment ended with an invitation to write down specific suggestions for the proposed changes. We informed respondents that their comments would remain anonymous, and that we would submit their suggestions to the official website on their behalf.[13] If they did not have any suggestions or comments, they could leave the response blank. Finally, we collected basic demographic information, including their hometown, age, gender, education, income, occupation, and attitudes toward money, trust, and cooperation.

We took a number of steps to ensure the reliability of responses. First, the particular setting affords at least some advantages relative to non-China data collection, in that there is a more rigorous linkage of respondents to actual individuals. The WenJuanXing platform requires mobile phone verification for registration, which is again authenticated when an individual joins the sample. The redemption of any payment requires cross-verification via real names and government identification, and the name associated with the bank card linked to WeChat.

At several points during the survey, simple questions were included to screen out bots and inattentive respondents. For example: "Please enter the number 43," and "12+21 =?" and, "What is the capital of China? Please choose the word below that appears in the preceding sentence." with the options Capital, Beijing, Shanghai, and Nanjing. A less attentive reader would choose Beijing, whereas Capital is the correct response.

We further excluded responses from the video-watching group that were completed in under 3 minutes, as recorded by the survey platform (5.3 minutes for the control group), and responses for which participants consistently selected the same option across all questions (e.g., always choosing the first option).

Our baseline experiment began with a sample size of 2,206, resulting in 2,122 valid responses after exclusions.

To refine our interpretation of the results from the main experiment, we conducted two supplementary surveys during 21-30, September, 2023. In Supplementary Experiment

[13] Apart from ensuring the safety of our subjects, we would be unable to verify or record any comments made directly by participants, since all feedback on the website is confidential.

1, we surveyed 536 additional respondents who were all shown one nationalist video. We took this approach for several reasons. Most importantly, given the limited resources to conduct the follow-up, we wanted to limit the experiment to two treatments. And in doing so, we wanted to maintain as much comparability between treatments as possible, which is why we elected to have the calm version serve as a benchmark, given that it showed relatively modest treatment effects in our main experiment. In this follow-up experiment, after viewing the video, all participants were then provided with information on *others'* beliefs about the new law based on responses of participants in the main experiment to the question, "Do you think proposing amendments to this legal provision is an act of being a 'traitor'?" . Specifically, we informed participants in this supplementary survey that, "As of now, the average score for the 2,212 respondents participating in this survey on the question "Do you think proposing amendments to this legal provision is an act of being a "traitor"." is 5.14 (below the midpoint of 5.5 on a scale from 1 to 10, where 1 represents "completely do not think people who raise objections are traitors" and 10 represents "strongly believe")." The purpose of this supplement was to assess the role of second-order beliefs in driving respondents' willingness to provide feedback on the proposed law.

In a second supplementary experiment, we conducted two list experiments with 312 new respondents, who all watched the emotional nationalist video, and were then asked to assess their attitudes toward the government and its potential responses to negative feedback from citizens. Of particular note, among the items in the list were "The government can benefit from this revision," and "The government will punish citizens who voice opposing opinions."

Finally, we conducted follow-ups with a subset of respondents from the baseline experiment six and twelve months after the initial survey; however, as we have already noted in the introduction, the response rate (and recall) for these follow-ups were quite low.

3.1 Discussion of Experimental Ethics

We took very seriously protecting the interests of our subjects in designing and implementing our experiment, which involved considerations that were complementary to our own interests in obtaining valid responses from them.[14]

We identified two primary risks: first and foremost, exposure to direct retaliation from the government from participating in the experiment; and "treating" some subjects in a way that could permanently alter their beliefs – either positively or negatively – toward the government and its policies.

Our approach to managing the first risk was to ensure that all feedback that we submitted to the government website – and indeed all responses to the experiment – were done with complete anonymity. As noted earlier, this was also essential to persuading respondents to comment critically on the proposed law. To eliminate even the residual risk that subjects' identities might be uncovered later on, after the survey was fully completed, we instructed the survey company to destroy the data.[15] At the time of writing, there existed no way of linking survey answers to individual respondents.

The second risk we identified was that participation in the experiment could alter participants' views on the pending National Security Law and the government in general. We saw this risk as minimal ex ante, as the videos we used in our treatment were commonplace on the Internet, and hence represented exposure to material that they might encounter in everyday life. Relatedly, ex post we found that the consequence of exposure was relatively short-lived – while response rates for our longer-term follow-up were limited, among respondents we were able to contact, we found no relationship between exposure to videos – whether nationalist or critical – and attitudes toward the government and/or national security laws.

[14] The experiment was conducted with fast-tracked permission from Renmin University's Academic Commission, the institution's equivalent of an IRB. The third author was not involved with the project until after all data had been collected and given, as we describe below, that all data are completely anonymized, was exempt from IRB approval.

[15] It would be an unprecedented step for the government to try to access data from WenJuanXing. The company itself has no ties to the Chinese government and is in fact majority-owned by a Hong Kong conglomerate that is in turn held by a diverse set of international investors.

3.2 Variable Construction

Our main analysis is built on whether and how a respondent chooses to share an opinion on the National Security Law. This opinion can be supportive, oppositional, or (less commonly) neutral. Of our sample of 2122 participants, 494 (23.3%) wrote at least something in the text box we provided for feedback via the government web portal. To determine whether a comment was positive or negative, we had three research assistants classify each as positive, negative, or neutral. In the vast majority of cases (99.25%), they were uniform agreement. In all such cases, we follow the consensus classification. In the 16 instances in which the RAs disagreed, we classify the comment as neutral (though we found that we generally agreed with the majority view; and as expected, the classification of these 16 Obs does not affect any of our results).

Of the 494 comments, 90 were classified as neutral. These tended to be very short comments (median length of just 6 characters, in practice usually just one or two English words. Casual inspection reveals that these were almost always some variant on "no" (as in I have no comments) or "no opinion" or, for the longer ones, "I have no relevant expertise to comment on this law."

In practice, we thus treat these cases as equivalent to non-responses.

Of the 404 remaining comments, 265 (65.6%) were classified as critical of the new law, which the remaining 139 (34.4%) were classified as supportive. These varied enormously in length, including several one-word endorsements (e.g., "Good") and also some that were many hundreds of characters. In general, critical comments were longer: the median number of characters was 36 and 54 for positive and negative comments respectively. We define indicator variables, *Oppose* and *Support* that reflect these negative and positive comments.

The five covariates of primary interest are indicator variables for each treatment arm (*Control* is the omitted category: $Nat_Emotional$, Nat_Calm, $Crit_Emotional$, $Crit_Calm$, and $Mixed$.

Finally, we include basic sociodemographic controls and province fixed effects, to improve precision (assuming random assignment, these should not affect our point estimates,

which is true in practice).

The controls include basic demographics (categorical variables for age, income, education, and gender); and control for prior knowledge of the proposed reform.

3.3 Sample Description

Table 1 presents descriptive statistics for the full sample, with balance tests shown in Appendix Table A2. The most notable finding is a significant difference in gender distribution between the *Emotional_nationalism* and *Control* groups. (Surprisingly, we find no education effect despite the generally high education-income correlation. However, this may reflect limited variation in educational attainment, as most respondents reported having a bachelor's degree but no graduate education.)

We also map the provincial distribution of respondents, as shown in Figure A2 in Appendix A. The map shows that respondents are drawn from a very broad geography within China.

4 Results from the Main Experiment

We begin by presenting our main results in graphical form in Figure 2, which summarizes the fraction of respondents in each group that provided either supporting or opposing comments. The dotted vertical lines provide the benchmark proportions from the control group; the dark bars to the left represent the proportion of opposing comments and the lighter bars to the right represent the proportion of supporting comments; we also include "whiskers" indicating the 95% confidence intervals for each fraction.

We begin at the top of the figure with the Nationalism treatments, the endpoints of all bars are shifted to the right indicating fewer opposing comments and more supportive ones, relative to the control. This effect is more pronounced for the Emotional version – in fact, there is no difference between the Calm version and the control group for supporting comments.

We observe the opposite effect for Critical treatments, though the effect is much more pronounced for opposing comments – there is essentially no effect of watching critical clips

on willingness to provide supportive comments. Curiously – and counter to our priors – in this case the effect is driven entirely by the Calm, only-the-facts version.

Finally, we observe no treatment effect in the Mixed arm, in which participants saw both Nationalist and Critical clips, suggesting that the two cancel one another out.

Our main results are presented also in Table 2 using a regression framework, which allows us to include fine-grained individual controls. Given that our randomization was largely successful, it is as expected that the patterns are very similar to those that appear in Figure 2. For each of *Oppose* and *Support*, we present three specifications, all based on the following:

$$Outcome_i = \beta_n Nationalism_i + \beta_c Critical_i + \beta_m Mixed_i + X_i + \gamma_{p(i)} + \varepsilon_i \qquad (1)$$

where i indexes the individual respondent; $Outcome$ is either *Oppose* or *Support*; $Nationalism$, $Critical$, and $Mixed$ are indicator variables for the relevant treatments (or as appropriate a disaggregation of these into emotional and calm); X is a set of i characteristics, and γ are fixed effects for i's province of residence $p(i)$. The results are consistent with what we observe in our graphical presentation of the results.

As we explained in Section 3, after viewing the video, we provided several illustrative examples to convey to respondents the extent to which their comments could be impactful. This element to the experiment allowed us to examine how our treatments interact with potential shocks to empowerment (or disempowerment) – a topic we take up in Section 5, but does raise the question of how video exposure would have impacted willingness to provide feedback in the absence of such messages. To provide a "pure" measure of the video treatments' impacts, we asked participants immediately after watching the video about their willingness to provide an opinion on the law (which, recall from the discussion in Section 3, connoted an *oppositional* opinion). We look at responses to this question, which was asked immediately followed the video(s), as an alternative outcome in Appendix Table A3. We observe patterns that are very closely aligned with our analyses that use *Oppose* as the outcome – participants who saw nationalist videos, especially the emotional variant, were less likely to express a willingness to provide feedback; critical videos had

the opposite effect.

4.0.1 The Role of Potential Government Retaliation

The experiment was designed to minimize the concerns of respondents about government retribution, but such "demand effects" remain a concern given the government's own interests in the success of the law.[16] This concern may have been further reinforced by the government's ability to trace individuals online through telephone numbers via "real-name registration" (though it is precisely this registration that gives us greater confidence in the quality of our sample). To the extent that respondents who viewed the nationalism videos as government-endorsed messages could prompt behavioral adjustments due to fear of retaliation.

As noted earlier, we made several adjustments within the experiment to mitigate this concern: we provided assurance of anonymity in any feedback we provided on the new law; and when embedding short-video content into the questionnaire, we removed the publishers' account information, preventing respondents from viewing it as government-endorsed.

To examine whether such fears are, in fact, a major concern in the first place, one of our follow-up surveys presented an additional 312 participants with a list experiment. After completing the baseline questionnaire, participants were asked to participate in two list experiments, designed to capture sensitivity to government retribution via indirect responses. The control and treatment groups were both presented with four descriptions irrelevant to the study, but the treatment group also received two critical statements: "The government can benefit from receiving feedback on revising the law" and "The government will punish citizens who voice opposing opinions on the law." The full list of items can be found in Table A4, with the relevant experimental items highlighted. Latent concerns were calculated by comparing the mean item counts between treatment and control groups.

[16] See, e.g., Chen & Yang (2019); Lu *et al.* (2024). It is worth noting also that the government is invested in minimizing dissent, and as such is motivated to elicit feedback and preempt any substantive concerns that arise. See, e.g., Fisman *et al.* (2021).

As illustrated in Figure A3, treatment respondents demonstrated minimal mean differences compared to the control group, with average disparities of a little over 0.1. T-tests yielded statistically non-significant results in both list experiments. With a 1-sided test, we can reject that the fraction of participants who agreed with each statement is more than 0.2 at the 5% significant level.

Overall, the results of this follow-up suggest that worries about government retribution was unlikely to be a first order concern in governing participants' decisions (and even less likely that it would have a large differential effect on those who saw particular videos).

4.1 Self- versus Social-intolerance

There are two basic explanations for the impact of exposure to videos on willingness to provide feedback on the proposed law. Most obviously, it could change an individual's beliefs about the law's merits. Alternatively, it may alter their beliefs about whether others support the law.

To gain some insight into whether own-beliefs and/or beliefs about others' support were affected, after viewing the video(s) (or at the same point in the survey for the control group), respondents were asked the following pair of questions:

- *Do you think that people who propose amendments to the new legal provision are unpatriotic?*
- *Do you think that others might people who propose amendments to the new legal provision as unpatriotic?*

For both questions, responses were on a 1-10 scale, with higher values indicating greater intolerance toward proposing amendments to the National Security Law. Specifically, for the first question, 1 was labeled as "I do not think so at all" and 10 as "They are very unpatriotic." For the second, 1 was labeled as, "I do not think others would care" and 10 as "Others will believe they are very unpatriotic." [17]

[17] The literal translations of the text is somewhat different and relates to "worship of foreigners." The text here best reflects, in our view, how the questions would be perceived by a Chinese reader.

While responses are not directly comparable (a point we will return to below) respondents generally gave higher values for the social intolerance question – a mean of 5.6 versus 5.1 for self-intolerance. We define $Self_Intolerance$ and $Social_Intolerance$ based on responses to these questions.

To provide some preliminary evidence on the role of "self-intolerance" and "social-intolerance" in driving willingness to provide positive or negative feedback on the proposed law, we proceed in two stages. First, we repeat the analysis in Equation (1), using at the outcomes $Self_Intolerance$ and $Social_Intolerance$. Second, we look at how these two variables correlate with our two outcomes of primary interest, *Oppose* and *Support*. Before proceeding to this pair of results, we first note that, while responses to the two questions are, unsurprisingly, quite correlated, there appears to be some independent variation. For example, the raw pairwise correlation is 0.46.

We start with the link from treatment status to self- and social-intolerance in Table 3. In the first pair of columns, the outcome is self-intolerance, while in the second pair it is social-intolerance. The results are directionally similar for both outcomes, suggesting that nationalist videos increase intolerance of opposition to the proposed law, while critical videos have the opposite effect.

In Table 4 we look at how self- and social-intolerance in turn are correlated with the ultimate outcomes of *Oppose* and *Support*. Focusing first on *Oppose*, in columns (1) and (2) we include each of self- and social-intolerance separately as regressors; both are highly predictive of abstaining from writing opposing comments. When both are included in column (3), we find that, despite their modestly high colinearity, each remains a significant predictor of *Oppose*, though the coefficient on $Self_Intolerance$ falls by nearly 40%. Finally, we perform a sort of mediation analysis in column (4), in which we include the various video treatments as well as the mediating variables $Self_Intolerance$ and $Social_Intolerance$ as regressors. Most notably, the coefficients on $Self_Intolerance$ and especially on $Social_Intolerance$ are essentially unchanged while that of $Emotional_Nationalism$ is reduced by 43%. We observe less of a "mediation effect" in predicting *Support* (though the critical video treatments were never

predictive of *Support* in the first place).

4.1.1 Supplementary Experiment on Second-Order Beliefs

Naturally, the empirical exercise above is somewhat casual and loosely suggestive. In an attempt to assess distinguish more decisively between self- and social-intolerance as predictors of willingness to provide feedback, we ran a follow-up experiment inspired by our initial set of results. As discussed in Section 3, the set-up in this experiment is very similar to the pair of nationalism treatments in the main experiment (for reasons of sample size, we focused on just two treatments, as explained earlier), though with the additional information provided to participants after viewing the video but before being given the chance to provide feedback on the proposed law:

> "The average score for the 2,212 respondents who previously participated in this survey, for the question "Do you think that people who propose amendments to the new legal provision are unpatriotic?" is 5.14 (below the midpoint of 5.5 on a scale from 1 to 10, where 1 represents "I do not think so at all" and 10 represents "They are very unpatriotic."

For the 536 participants in this experiment, we repeat our main analysis of whether treatment impacted willingness to provide feedback on the National Security Law; these results appear in Table 5. In columns (1) and (2) we present a variant on Equation (1) as follows:

$$Outcome_i = \beta_n Emotional_Nationalism_i + X_i + \gamma_{p(i)} + \varepsilon_i \tag{2}$$

with *Oppose* as the outcome. Whether we focus on the bivariate regression (column (1)) or one with controls (column (2)), the coefficient on *Emotional_Nationalism* is very close to zero (though given the imprecision of these estimates we also cannot reject a large effect). This contrasts with the pattern we see in column (3), in which we present a comparable regression from the main experiment, focused on the comparable subsample of participants that viewed one of the Nationalism videos. In this sample, we find a

significantly negative coefficient on $Emotional_Nationalism$ ($p < 0.05$). We pool the two groups in column (4), and include an indicator variable, $Informed$, denoting participants in the later experiment in which subjects were informed of the average responses of others. As expected given the noisiness of our estimates in columns (1) and (2), the coefficient on $Emotional_Nationalism * Informed$ is not statistically significant. Interestingly, the coefficient on $Informed$ is significantly positive (and particularly if we do not include the interaction term, $p < 0.01$), indicating a greater willingness to provide critical feedback in the later experiment. This could be because of the effect of the information treatment, which generally informed participants that social intolerance was lower than anticipated; or it could simply reflect differences in the sample because the informed group was surveyed at a later date.

We repeat these analyses in columns (5)-(8) for $Support$ as the outcome. in columns (5) and (6), the coefficient on $Emotional_Nationalism$ is close to zero, while it is large and significantly positive for the $Informed = 0$ sample in column (7); as observed in column (8), the difference between the two is statistically significant.[18]

Overall, the results in this section suggest that the videos we presented to participants impacted their willingness to provide feedback – both positive and negative – at least in part because of their effect on second-order beliefs. When we informed subjects of others' actual tolerance, the treatment effect disappeared.

5 Additional Channels and Interpretations

In this section, we consider some further interpretations or potential sources of influence over willingness to provide feedback. While we do not believe that these alternatives can fully explain the set of results we have presented thus far, they could potentially play a role in how survey participants responded to some subset of our outcome measures.

[18] We find that the direct effect of $Informed$ also indicates a lower propensity to provide supportive feedback, though in this case the effect is not statistically significant.

5.1 Could the Treatment Affect Beliefs about Others' Willingness to Comment?

In the previous section, we showed that willingness to provide feedback is no longer affected by videos when accompanied by accurate information on others' tolerance for criticism of the law. We interpret this as reflecting the fact that willingness to provide feedback is very much affected by social pressure. Yet information on others' tolerance for such criticism could also be simply because it affects the number of other people they believe will provide feedback. Since free-riding on the efforts of others was highlighted in our pilot survey as a reason for not providing feedback on the law, we included the following question after the video treatment: "What do you think are the reasons why some people are unwilling to voice their opinions?". It was presented as a multiple-choice item, with one option being: "With so many people in our country, it's not my turn to give feedback." This option serves as a behavioral measure for whether respondents perceive opinion-sharing as a free-riding opportunity (i.e., believing others will provide feedback, thus exempting themselves).

We find that responses to this question are completely uncorrelated with treatment status, suggesting that second-order beliefs are unlikely to act through their impact on expectations about others' willingness to provide feedback.

5.2 Could the Treatment Effect of Videos be Affected by Beliefs about Efficacy of Commenting?

A number of factors could mediate the impact of the video treatments on willingness to provide feedback. For example, understanding the potentially harmful effects of vaguely-defined government powers could increase the impact the critical videos. Or perhaps understanding that many others are likely to comment exacerbates the free rider problem and thus makes people less likely to provide feedback – whether positive or negative. Finally, learning that the government may in fact respond to public feedback could make responses *more* likely.

These three specific factors were highlighted to us in the responses to our pilot survey

as primary determinants of willingness to comment on the proposed law. To explore whether (and how) these factors interacted with our main treatment, after viewing the video(s), *all* respondents were then provided with three illustrative examples to highlight each of the three issues mentioned above. After each piece of information, we asked respondents about their willingness to provide (oppositional) feedback on the new law.

Specifically, we informed participants of (1) the number of opinions others have submitted to reduce the perception that their input is unnecessary; (2) examples of instances in which innocent individuals were harmed or even died due to abuses of power, highlighting the potentially widespread and severe consequences of broadly defined legal authority; and (3) the successful example of public feedback altering proposed laws, from the Women's Rights Protection Law, to show that the People's Congress does respond to public sentiment. (see Table A1 for the exact wording of what respondents were told.)

Before providing any of this information (but after watching the video(s)) we asked participants about their willingness to provide an opposing view of the law; they were then asked the same question about willingness to provide feedback after each of the three pieces of information.[19]

Our main interest is whether giving respondents more accurate information about the actual efficacy and value of providing feedback (albeit focused on oppositional comments) affects the link from our video treatments to willingness to comment on the law (naturally, we only wanted to give only a single opportunity to actually comment). To capture this possibility, we look at how treatment status predicts willingness to provide oppositional comments, using specifications that parallel that of Equation 1. The outcome variables we employ are: $Willingness_t$, which captures responses to the question about willingness to provide oppositional feedback at time t, where $t = 0, 1, 2, 3$ indexes the number of informational treatments a participant has experienced. Our overall observation on these patterns is that there are at most marginal mediating effects of information on the videos' effect on willingness to provide oppositional feedback – throughout, nationalist videos has

[19] There is a significant positive correlation between self-reported willingness and the actual submission of opposing opinions (but no robust correlation with supporting opinions, as expected); See Appendix Table A5.

a negative effect (though not always significant) while critical videos have a positive effect. On balance, we find limited evidence that such information plays a mediating role.

We see this finding as underscoring the relevance of our main result – exposure to brief social media content impacts willingness to express dissent, and is not reliant on "complementary" knowledge such as the efficacy of providing feedback.

5.3 Long-term Impacts

To explore possible longer-term impacts, we conducted follow-up surveys with participants in the main experiment 6 and 12 months after the initial study, obtaining 173 and 160 valid responses, respectively. Combining both longer-term surveys, the response rate was approximately 15%.

Perhaps unsurprisingly, there are no discernible long-term effects, with the results of the follow-up survey suggesting that participants had largely forgotten about both the proposed law as well as the videos they watched. As shown in Appendix Table A6, only 32 respondents (9.61%) in the follow-up surveys could clearly recall the specific content of the new provisions in the Public Security Administration law. Of those who watched the videos in our baseline experiment, approximately 61 respondents (18.32%) remembered the main content of the videos, and only 49 respondents (14.71%) expressed ongoing concern about developments related to the legal amendments.

Table A6 display respondents' perceptions of others' tolerance attitudes toward the law across treatment groups. Notably, the belief that expressing opposition is unpatriotic changed in all groups, generally converging toward the responses of the control group, which did not view the video. As shown in Table A7, few respondents in the follow-up survey maintained opinions similar to those in the initial experiment.

It would be surprising if we had found otherwise – our results are noteworthy even for their short-run impact, and it would stretch credulity to imagine that such an intervention would continue to affect beliefs after months or a year. Yet what may be relevant from the perspective of government persuasion is precisely the short-run impact we document which mutes potential opposition at exactly the moment when dissent could coalesce.

Indeed, our results perhaps mirror the fleeting nature of public interest more generally, so that the type of persuasion we study here can be deployed in the short-term to manage public opinion.

5.4 What Might the Impact of TikTok Videos on the Public's Response to the Public Security Law?

Even a rough calculation of the overall impact on comments related to the new law is beyond the scope of what is feasible in the context of our paper. It would require, for example, taking a position on the cumulative rather than individual impacts of video exposure, and accounting for the more focused experience that respondents in our experiment may have had relative to the casual TikTok user. Further, we would need to account for the extent to which TikTok users are more (or less) likely to engage in civic discourse relative to the average Chinese citizen.

Yet we can provide some facts on exposure to the types of videos we deployed which indicate their potential for limiting dissenting views. During the comment period, we canvassed the set of videos with titles or slogans related to the proposed legal changes (e.g., keywords such as "Public Security Administration Punishments Law," "harming national sentiments," "criminalizing clothing," and so forth) on China's two most popular short video platforms Tiktok and Kuaishou. We focused on those receiving at least 1,000 "likes."[20] The 50 most-related videos returned by this search had approximately 27 million views during September 2023. But it was very heavily skewed toward nationalist videos – 30 of the 50 videos were supportive of the law, and nearly 90 percent of total views were of the nationalist videos, 81 percent of which we classified as "emotional" rather than calm (see A8).

Given these figures, combined with our findings that nationalist videos substantially reduced willingness to provide critical comment, we see the emergent technology of short video platforms as an effective means of controlling public dissent. While it is of course

[20] Based on the financial reports from the third quarter of 2024, TikTok's daily active users exceeded 600 million in 2024, while Kuaishou's daily active user count stood at over 400 million. Also note that 1,000 likes is a relatively high number, given that anyone can upload videos on TikTok, yet few videos get much attention, and their is a very long right tail of video popularity.

impossible to assess the link between the ubiquity of nationalist (versus critical) video content online, it is also notable that the National Security Law – including the more ambiguous provisions that may expand government power – were not subject to substantial revision. The revised version of the law, which was released in July 2024, some greater specificity was added, consistent with at least some government responsiveness to critical feedback. For example, "harming the feelings of the Chinese nation" was modified to read, "in public places, wearing, displaying, or promoting symbols or attire that promote past acts of invasion, causing adverse social impact." However, as noted by China Law Translate (2024), the revised clause still lacks precise definitions of prohibited actions, as it continues to use (legally) ill-defined terms such as "promote" and "glorify."

6 Conclusion

We conducted an online experiment with well over 2,000 respondents to study how online content impacts willingness to express dissent or support for government policies. The particular setting is the comment period for a national security law during September 2023. In the main part of our experiment, respondents were exposed to either nationalist or critical videos (in an emotional or calm variant). We find that particularly emotional nationalist videos have a substantial impact on willingness to provide supportive (a 8.1% increase) or oppositional (a 7.1% decrease) feedback on the law via a government portal designed for eliciting public comment. We find that this effect disappears when we provide information on the extent to which others disapprove of those who criticize the law. Overall, our findings suggest that online content impacts the public's willing to criticize government policy, in large part because it shapes their views on how dissent might be viewed by others.

Our findings highlight the potentially important role that social media may play in shaping public policy discourse, a point emphasized by Treisman & Guriev (2023) among others. There are many directions for future work that can build on our findings. In our setting, for example, holding a dissenting view may hold social costs (given the importance of conformity in Chinese culture) but also potential risks given the government's

tight control over dissent. Understanding the relative roles of each of these factors in driving our results is a promising area for future work. Relatedly, social media is prevalent worldwide, across a range of political and economic circumstances. It may be interesting in future work to explore if and how exposure to online content differentially affects respondents in, say, the U.S. relative to China.

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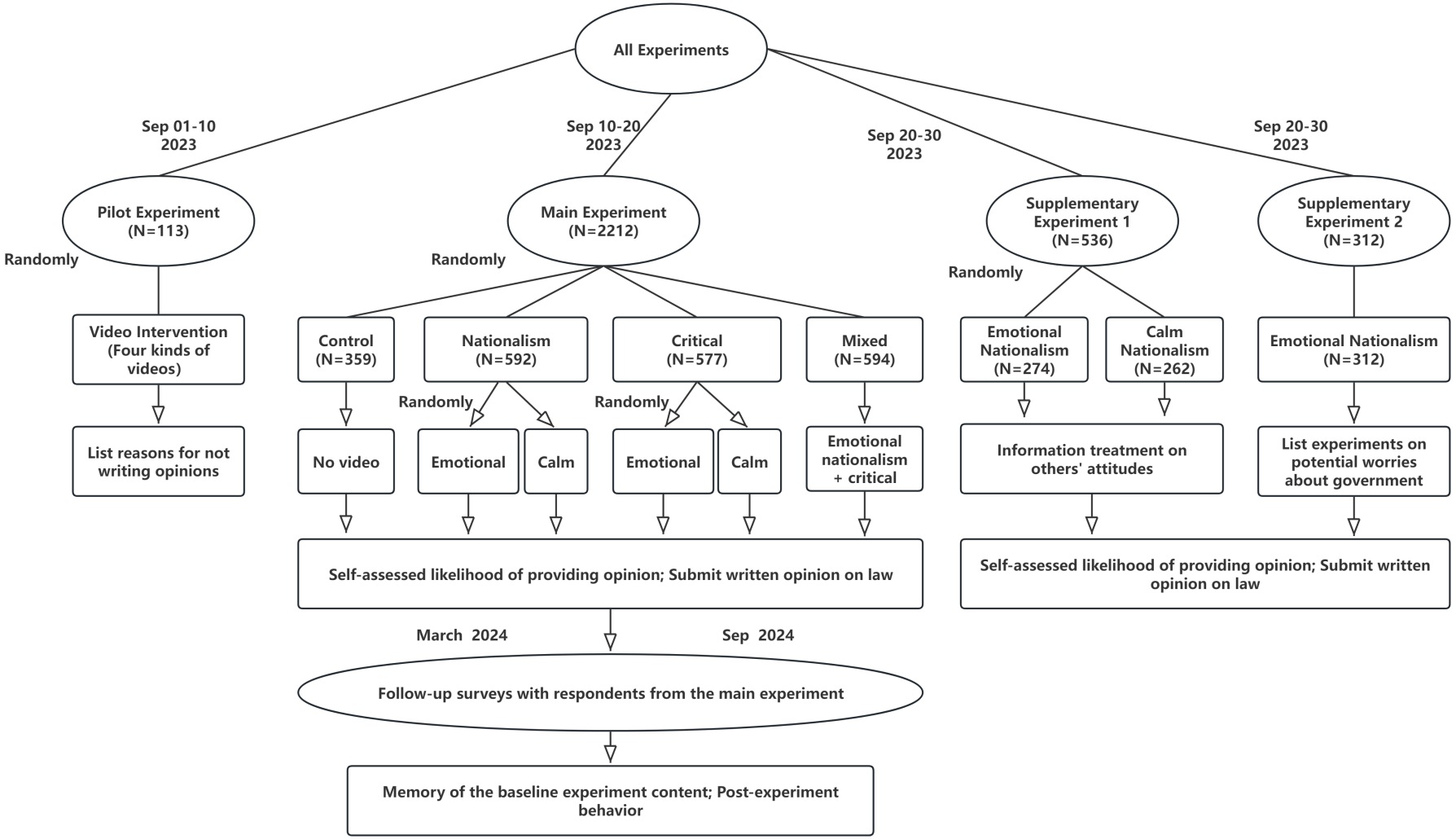
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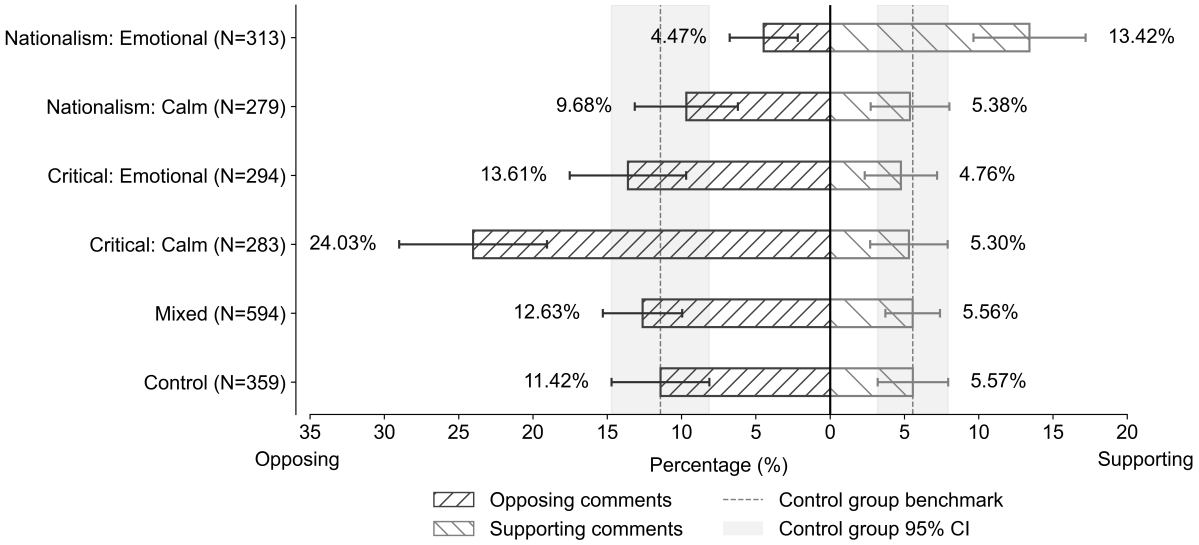
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Figure 1: Experimental Flow



Notes: This figure summarizes the design of our experiments. In both the baseline and supplementary experiments, participants watched one-minute videos. Before the main experiment, a pilot study was conducted to identify the primary three reasons why respondents were reluctant to express their opinions. In both the baseline and supplementary experiments, participants were given targeted textual information addressing the top three reasons for withholding opinions. In supplementary experiments 1 and 2, new participants were treated with extra information and extra two list experiments, respectively. Follow-up surveys were administered six months and one year after the main experiment, repeating the same questions without any video or textual interventions.

Figure 2: The Likelihood of Commenting Across Different Groups



Notes: This figure shows the proportion of respondents that wrote opposing and supporting comments on the proposed provisions of the Public Security Administration Punishment Law. The proportion of opposing comments is the percentage of each group that suggested modifications or expressed dissatisfaction with the newly added provisions. The proportion of supporting comments is the percentage that wrote supportive comments and/or criticized those who opposed it. We label the control group mean and 95% confidence intervals for ease of comparison.

Table 1: Summary of Information (Main Experiment)

	Mean	SE	Min	Max
Basic Characteristics				
Gender	0.56	0.01	0	1
Age	3.40	0.02	1	6
Education	2.83	0.01	1	4
Income (yearly)	2.76	0.02	1	4
Attitude Responses				
Patriotism	1.03	0.01	0	2
Cooperation	5.75	0.06	1	10
Money	7.65	0.04	1	10
Effort	7.51	0.04	1	10
Mistrust	6.40	0.04	1	10
Obs	2,122			

Notes: This table reports the number of Obs, mean, standard error, minimum, and maximum for respondent characteristics in the main experiment. Variable definitions: Gender (0 = Male, 1 = Female); Age (1 = Below 18 years old, 2 = 19–25, 3 = 26–30, 4 = 30–40, 5 = 40–50, 6 = Above 50); Education (1 = High school and below, 2 = Associate degree, 3 = Bachelor's degree, 4 = Master's degree and above); Income (1 = Below 50,000 RMB, 2 = 50,000–130,000 RMB, 3 = 130,000–200,000 RMB, 4 = Above 200,000 RMB). In the "attitude" section, "patriotism" represents the basic patriotic tendency of the public before any intervention (scored 0-2, measured by asking respondents which region in the world they would most like to settle in). The other four items are based on the research of Inglehart *et al.* (2022), which inquired about respondents' fundamental beliefs regarding cooperation, money, effort, and trust, scored on a 1-10 scale.

Table 2: Main Results of Video Intervention on Writing Comments

	𝟙[Oppose]			𝟙[Support]		
	(1)	(2)	(3)	(4)	(5)	(6)
Nationalism	-0.045**	-0.048**		0.041**	0.040**	
	(-2.272)	(-2.377)		(2.366)	(2.277)	
Nat_Emotional			-0.071***			0.081***
			(-3.302)			(3.525)
Nat_Calm			-0.023			-0.005
			(-0.946)			(-0.262)
Critical	0.073***	0.066***		-0.005	-0.004	
	(3.121)	(2.843)		(-0.360)	(-0.290)	
Crit_Emotional			0.020			-0.006
			(0.746)			(-0.332)
Crit_Calm			0.114***			-0.002
			(3.788)			(-0.100)
Mixed	0.012	0.003	0.003	-0.000	0.002	0.002
	(0.557)	(0.152)	(0.157)	(-0.010)	(0.114)	(0.154)
Controls	No	Yes	Yes	No	Yes	Yes
Province FE	No	Yes	Yes	No	Yes	Yes
Obs	2,122	2,121	2,121	2,122	2,121	2,121
R-squared	0.058	0.065	0.072	0.019	0.026	0.034

Notes: This table presents the baseline results of the video intervention's effect on respondents' written feedback, where 𝟙[Oppose] and 𝟙[Support] represent opposing and supporting opinions toward the new regulation respectively. The video interventions consist of three main categories: nationalist videos (*Nationalism*), critical videos (*Critical*), and a mixed condition where participants viewed both video types (*Mixed*). Both nationalist and critical videos were further divided by emotional intensity into emotionally-charged versions (*Nat_Emotional* and *Crit_Emotional*) and calm versions (*Nat_Calm* and *Crit_Calm*). The Mixed group viewed the emotionally-charged versions of both nationalist and critical videos. All regression specifications include province fixed effects and control for demographic characteristics including age, gender, education level, income, and prior attention to the issue. Robust t-statistics are reported in parentheses, *** p<0.01, ** p<0.05, * p<0.10.

Table 3: Outcomes of Video Intervention on Respondents' Self- and Social Intolerance

	Self_intolerance		**Social_intolerance**	
	(1)	(2)	(3)	(4)
Nationalism	0.609***		0.612***	
	(3.553)		(3.283)	
Nat_Emotional		0.839***		1.178***
		(4.270)		(5.625)
Nat_Calm		0.359*		-0.002
		(1.761)		(-0.010)
Critical	-0.181		-0.353*	
	(-1.092)		(-1.861)	
Crit_Emotional		-0.258		-0.431*
		(-1.362)		(-1.946)
Crit_Calm		-0.094		-0.255
		(-0.488)		(-1.125)
Mixed	0.379**	0.384**	0.234	0.243
	(2.215)	(2.240)	(1.269)	(1.320)
Controls	Yes	Yes	Yes	Yes
Province FE	Yes	Yes	Yes	Yes
Obs	2,121	2,121	2,121	2,121
R-squared	0.053	0.056	0.064	0.076

Notes: This table shows the impact of video intervention on respondents' self- and expected social-intolerance. 𝟙[Oppose] and 𝟙[Support] represent opposing and supporting opinions toward the new regulation respectively. The video interventions consist of three main categories: nationalist videos (*Nationalism*), critical videos (*Critical*), and a mixed condition where participants viewed both video types (*Mixed*). Both nationalist and critical videos were further divided by emotional intensity into emotionally-charged versions (*Nat_Emotional* and *Crit_Emotional*) and calm versions (*Nat_Calm* and *Crit_Calm*). The Mixed group viewed the emotionally-charged versions of both nationalist and critical videos. After the video intervention, respondents were asked whether they themselves would perceive someone opposing this new regulation as exhibiting blind worship of foreign things (*Self_intolerance*), and were additionally required to predict whether others would hold such a perception (*Social_intolerance*). All regression specifications include province fixed effects and control for demographic characteristics including age, gender, education level, income, and prior attention to the issue. Robust t-statistics are reported in parentheses, *** p<0.01, ** p<0.05, * p<0.10.

Table 4: The Impact of Intolerance Levels on Writing Comments

	𝟙[Oppose]				𝟙[Support]			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Self_intolerance	-0.023***		-0.014***	-0.013***	0.005**		0.006**	0.006**
	(-8.069)		(-4.341)	(-4.096)	(2.148)		(2.496)	(2.287)
Social_intolerance		-0.024***	-0.019***	-0.017***		-0.000	-0.003	-0.004**
		(-9.445)	(-6.671)	(-6.246)		(-0.180)	(-1.364)	(-2.010)
Nat_Emotional				-0.040*				0.081***
				(-1.857)				(3.489)
Nat_Calm				-0.018				-0.007
				(-0.764)				(-0.373)
Crit_Emotional				0.009				-0.006
				(0.341)				(-0.350)
Crit_Calm				0.109***				-0.002
				(3.696)				(-0.130)
Mixed				0.013				0.001
				(0.589)				(0.076)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Province FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Obs	2,121	2,121	2,121	2,121	2,121	2,121	2,121	2,121
R-squared	0.078	0.090	0.098	0.113	0.023	0.021	0.024	0.037

Notes: This table demonstrates the effects of two types of intolerance on respondents' final decisions to provide written feedback. 𝟙[Oppose] and 𝟙[Support] represent opposing and supporting opinions toward the new regulation, respectively. Following the video intervention, respondents were asked whether they personally would view someone opposing this new regulation as exhibiting blind worship of foreign things (*Self_intolerance*), and were further asked to predict whether others would hold such a perception (*Social_intolerance*). Columns 4 and 8 incorporate video intervention variables, where nationalist and critical videos were further categorized by emotional intensity into emotionally-charged versions (*Nat_Emotional* and *Crit_Emotional*) and neutral versions (*Nat_Calm* and *Crit_Calm*). All regression specifications include province fixed effects and control for demographic characteristics including age, gender, education level, income, and prior attention to the issue. Robust t-statistics are reported in parentheses, *** p<0.01, ** p<0.05, * p<0.10.

Table 5: Supplementary Experiment on Second Beliefs

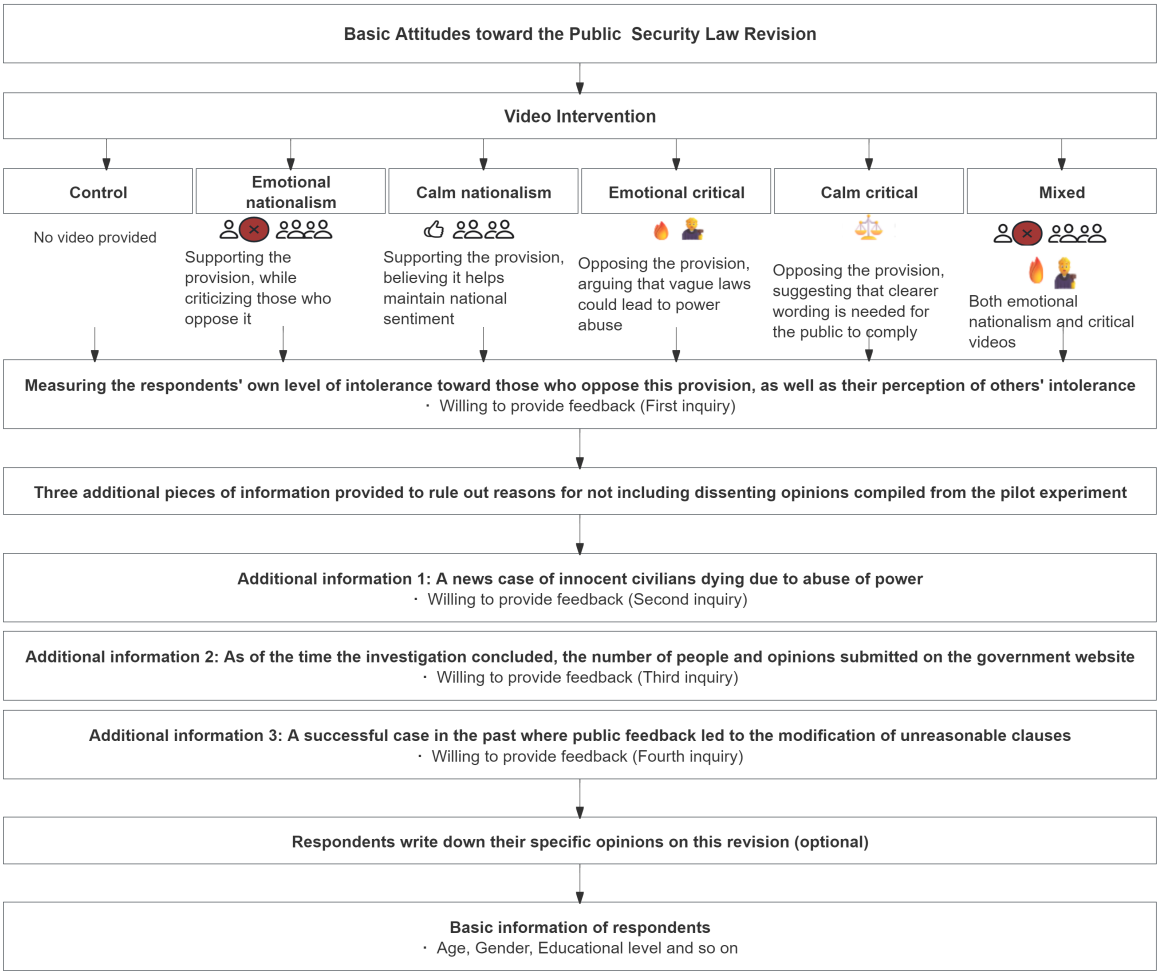
	𝟙[Oppose]				𝟙[Support]			
	Sup-Expt	Sup-Expt	Main-Expt	Pooled	Sup-Expt	Sup-Expt	Main-Expt	Pooled
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Emotional_Nationalism	0.004	-0.012	-0.046**	-0.045**	0.001	-0.004	0.087***	0.084***
	(0.123)	(-0.346)	(-2.043)	(-2.071)	(0.029)	(-0.169)	(3.543)	(3.488)
Informed				0.063**				0.020
				(2.156)				(0.907)
Emotional_Nationalism × Informed				0.047				-0.087***
				(1.174)				(-2.584)
Controls	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Province FE	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Obs	536	534	591	1,126	536	534	591	1,126
R-squared	0.000	0.061	0.086	0.058	0.000	0.056	0.062	0.035

Notes: This table presents the effects of informing respondents about the actual intolerance levels of others (denoted as $Informed$) on their propensity to express opposing (𝟙[Oppose]) or supportive (𝟙[Support]) opinions regarding the new regulation. In this supplementary experiment, respondents were exclusively exposed to nationalist video interventions. The variable *Emotional_Nationalism* represents exposure to emotionally-charged nationalist videos. *Sup-Expt* refers to the nationalist group in the supplementary experiment where participants were informed about others' true intolerance levels, while *Main-Expt* denotes the main experiment. The *Pooled* sample combines nationalist video treatment groups from both experiments. All regression specifications include province fixed effects and control for demographic characteristics including age, gender, education level, income, and prior attention to the issue. Robust t-statistics are reported in parentheses, *** p<0.01, ** p<0.05, * p<0.10.

Appendix

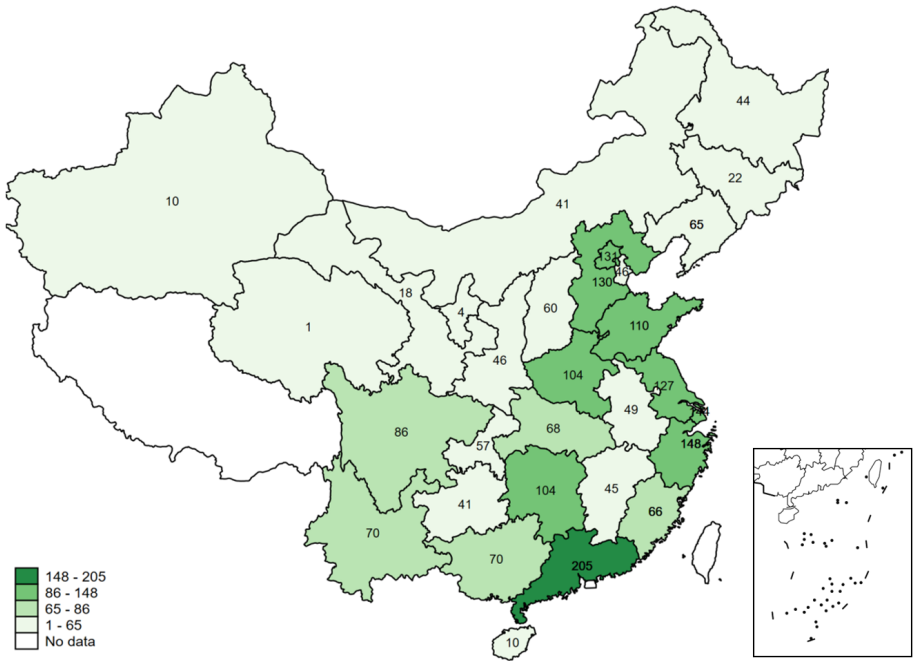
A Additional Information

Figure A1: Experiment Outline of the Main Experiment



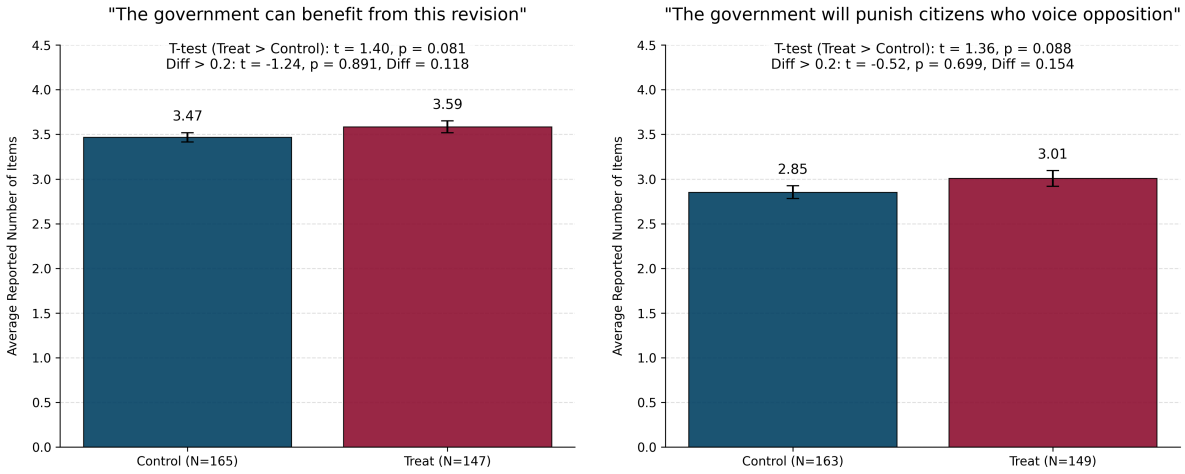
Notes: This figure illustrates the key experimental procedures implemented in the main experiment.

Figure A2: Distribution of Participants



Notes: This figure displays the geographical distribution of participants' home provinces. While there is some variation in the number of respondents across provinces, the majority of provinces are represented in our sample.

Figure A3: List Experiment Results: Sensitivity of Political Expression



Notes: This figure shows the performance of two list experiments. The list experiments measure respondents' latent concerns about expressing political opinions. Results show small differences between Treat and Control groups. At the 5% significance level, we cannot reject the null hypothesis of no significant difference. Using 0.2 as the difference threshold, we can be 95% confident that the true difference does not exceed 0.2. The statistically insignificant differences suggest that respondents' underlying beliefs about the government in these two dimensions were not strong, indicating that our experimental results were not driven by these particular beliefs.

Table A1: The Content of Video Interventions and Information Shocks

Panel A: The Basic Content of Video Interventions		
Type	**Video Piece**	**Theme**
Nationalism	Emotional Nationalism	Uses forceful and emotional language and imagery to connect the legal revision with safeguarding national dignity.
	Calm Nationalism	Uses objective language and descriptions to explain the significance of this legal revision for national sentiment.
Critical	Emotional Critical	Uses intense and provocative language to critique the legal revision, linking it to the expansion and potential abuse of public power, emphasizing unclear and poorly vetted provisions.
	Calm Critical	Uses rational language to explain that legal provisions should be clear to facilitate public understanding and compliance.
Panel B: The Basic Content of After-Video Informational Material Provided to All Respondents		
Information Shocks	**Theme**	**Main Content**
IS1	Legal revisions impact everyone	In 2014, a migrant worker, died during a police intervention in Taiyuan, reported as "migrant worker beaten to death over wage disputes." Excessive authority may harm innocent people; supervising laws is a citizen' s right and duty.
IS2	The actual number of commenter and comments	As of 4:00 PM on September 16, 2023, a total of 93,724 people have submitted 116,463 comments on the revision of this regulation via the official website, with numbers still increasing.
IS3	A precedent where public feedback successfully led to changes in a legal revision	The draft revision of the "Law of the People's Republic of China on the Protection of Women's Rights and Interests" was opened for public comment on December 24, 2021. Over 80,000 people submitted 420,000 comments, which successfully led to the removal of improperly or unreasonably worded provisions in the draft.

Table A2: Balance Test by Each Groups

	Control	Nationalism		Critical		Mixed
		Emotional	Calm	Emotional	Calm	
Basic Characteristics						
Gender	0.54 (0.03)	0.62** (0.03)	0.52 (0.03)	0.56 (0.03)	0.57 (0.03)	0.56 (0.02)
Age	3.39 (0.05)	3.55* (0.06)	3.40 (0.06)	3.48 (0.06)	3.39 (0.06)	3.31 (0.04)
Education	2.84 (0.04)	2.79 (0.04)	2.81 (0.04)	2.79 (0.04)	2.95 (0.03)	2.83 (0.03)
Income	2.86 (0.05)	2.80 (0.06)	2.81 (0.07)	2.75 (0.06)	2.76 (0.06)	2.66 (0.04)
Attitude Responses						
Patriotism	1.00 (0.03)	1.04 (0.03)	1.01 (0.04)	1.10 (0.04)	1.00 (0.04)	1.05 (0.03)
Cooperation	5.80 (0.14)	5.73 (0.15)	5.43 (0.17)	5.98 (0.16)	5.81 (0.15)	5.72 (0.11)
Money	7.47 (0.11)	7.47 (0.10)	7.58 (0.11)	7.55 (0.10)	7.71 (0.10)	7.53 (0.08)
Effort	7.66 (0.10)	7.59 (0.10)	7.67 (0.11)	7.43 (0.12)	7.34 (0.11)	7.80 (0.08)
Mistrust	6.32 (0.09)	6.48 (0.09)	6.42 (0.10)	6.52 (0.09)	6.31 (0.09)	6.38 (0.07)
Obs	359	313	279	294	283	594

Notes: This table presents the mean and standard error (in parentheses) of respondent characteristics for each subgroup. Kruskal-Wallis tests were conducted for continuous and ordinal variables due to non-normal distributions. The test for *attitude towards money* showed significant differences across groups (H=12.305, p=0.0308), but Dunn's post-hoc tests (Bonferroni-corrected) found no significant differences with the control group (all p-adj=1.0). The test for *Age* was marginally non-significant (H=10.122, p=0.0719). Chi-square tests were conducted for *Gender* (binary variable), with a significant difference between the *emotional nationalism* group and the control group (χ^2=4.730, p=0.0297). No other variables showed significant differences across groups (all p$\geq$0.1085). *** p<0.01, ** p<0.05, * p<0.1.

Table A3: Main Results of Video Intervention on Willingness

	$Willing_0$			$Willing_1$			$Willing_2$			$Willing_3$		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Nationalism	-0.127	-0.117		-0.149	-0.126		-0.189	-0.149		-0.155	-0.126	
	(-0.683)	(-0.630)		(-0.843)	(-0.705)		(-1.028)	(-0.803)		(-0.869)	(-0.700)	
Nat_Emotional			-0.423**			-0.200			-0.308			-0.158
			(-2.017)			(-0.984)			(-1.464)			(-0.759)
Nat_Calm			0.213			-0.047			0.022			-0.092
			(0.949)			(-0.217)			(0.098)			(-0.429)
Critical	1.094***	1.139***		0.810***	0.861***		0.926***	0.975***		0.802***	0.851***	
	(5.799)	(6.063)		(4.565)	(4.860)		(4.971)	(5.189)		(4.522)	(4.822)	
Crit_Emotional			0.772***			0.531**			0.686***			0.617***
			(3.510)			(2.529)			(3.112)			(2.982)
Crit_Calm			1.516***			1.205***			1.274***			1.095***
			(6.856)			(5.934)			(5.810)			(5.351)
Mixed	0.457**	0.484**	0.483**	0.371**	0.414**	0.416**	0.433**	0.446**	0.446**	0.331*	0.363**	0.365**
	(2.418)	(2.544)	(2.539)	(2.046)	(2.270)	(2.281)	(2.310)	(2.347)	(2.348)	(1.827)	(2.002)	(2.011)
Controls	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Province FE	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Obs	2,122	2,121	2,121	2,122	2,121	2,121	2,122	2,121	2,121	2,122	2,121	2,121
R-squared	0.029	0.055	0.063	0.020	0.046	0.050	0.024	0.058	0.062	0.019	0.056	0.058

Notes: This table presents the baseline results of the video intervention's effect on respondents' willingness to provide feedback. The video interventions consist of three main categories: nationalist videos (*Nationalism*), critical videos (*Critical*), and a mixed condition where participants viewed both video types (*Mixed*). Both nationalist and critical videos were further divided by emotional intensity into emotionally-charged versions (*Nat_Emotional* and *Crit_Emotional*) and calm versions (*Nat_Calm* and *Crit_Calm*). The Mixed group viewed the emotionally-charged versions of both nationalist and critical videos. We measured respondents' willingness to express opinions after each video intervention and following every textual information intervention. Here, $Willing_n$ represents the respondent's willingness after receiving n rounds of textual interventions, where n=0 corresponds to the measurement taken immediately after the video intervention. All regression specifications include province fixed effects and control for demographic characteristics including age, gender, education level, income, and prior attention to the issue. Robust t-statistics are reported in parentheses, *** p<0.01, ** p<0.05, * p<0.10.

Table A4: The Content of Two List Experiments

List experiment 1: How many items do you support?		**List experiment 2: How many items concern you?**	
Control	**Treat**	**Control**	**Treat**
The government should increase investment in education to reduce educational inequality.	The government should increase investment in education to reduce educational inequality.	The leakage of personal privacy on social media is not effectively regulated by the government.	The leakage of personal privacy on social media is not effectively regulated by the government.
Environmental protection is everyone' s responsibility, and enterprises should take the lead.	Environmental protection is everyone' s responsibility, and enterprises should take the lead.	Risks of online scams and phishing websites are prevalent everywhere.	Risks of online scams and phishing websites are prevalent everywhere.
Smoking should be prohibited in all public places.	Smoking should be prohibited in all public places.	The government has installed excessive surveillance cameras in public spaces.	The government has installed excessive surveillance cameras in public spaces.
The government should raise the minimum wage based on regional characteristics.	The government should raise the minimum wage based on regional characteristics.	Credit card information is stolen during online shopping.	Credit card information is stolen during online shopping.
	The government can benefit from this revision		***The government would punish citizens who voice opposing opinions.***

Notes: This table presents the main content of the list experiments. The descriptions in bold are those seen only by the respondents in the treatment group. In each list experiment, respondents were asked to report the number of descriptions they agreed with, without specifying which ones they agreed with. This approach allows for the measurement of respondents' underlying attitudes.

Table A5: Effects of Willingness Measures on Writing Behavior

	𝟙[Oppose]					𝟙[Support]				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
$Willing_0$	0.032***				0.011***	0.001				-0.005*
	(11.170)				(3.231)	(0.280)				(-1.696)
$Willing_1$		0.033***			-0.002		0.003			-0.001
		(11.423)			(-0.678)		(1.566)			(-0.247)
$Willing_2$			0.037***		0.005			0.005**		0.001
			(12.884)		(1.096)			(2.228)		(0.364)
$Willing_3$				0.044***	0.034***				0.007***	0.010***
				(15.357)	(7.704)				(3.253)	(2.680)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Province FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Obs	2,121	2,121	2,121	2,121	2,121	2,121	2,121	2,121	2,121	2,121
R-squared	0.123	0.118	0.145	0.172	0.179	0.021	0.022	0.023	0.026	0.029

Notes: This table presents the impacts of the respondents' willingness to provide feedback on the actual writing behavior. We measured respondents' willingness to express opinions after each video intervention and following every textual information intervention. Here, $Willing_n$ represents the respondent's willingness after receiving n rounds of textual interventions, where n=0 corresponds to the measurement taken immediately after the video intervention. 𝟙[Oppose] and 𝟙[Support] represent opposing and supporting opinions toward the new regulation respectively. All regression specifications include province fixed effects and control for demographic characteristics including age, gender, education level, income, and prior attention to the issue. Robust t-statistics are reported in parentheses, *** p<0.01, ** p<0.05, * p<0.10.

Table A6: Traced Respondents' psychological performance I

Group	Self intolerance			Social intolerance		
	Baseline	Survey after 6 months	Survey after 12 months	Baseline	Survey after 6 months	Survey after 12 months
Nationalism	5.53	4.06	3.83	6.11	4.58	4.05
Critical	4.71	4.07	3.76	5.21	4.39	4.09
Mixed	5.27	4.12	3.75	5.77	4.76	4.11
Control	4.95	4.09	3.79	5.62	4.72	4.14

Notes: This table presents the changes in two types of intolerance levels among a subset of respondents from the main experiment during their 6-month and 12-month follow-ups. Here, $self_intolerance$ measures whether respondents themselves would perceive opponents of the new regulation as exhibiting blind worship of foreign things, while $social_intolerance$ captures their beliefs about whether others would hold such perceptions.

Table A7: Recall and attitudes for 6 and 12 month follow-up groups

Panel A: Own recall and beliefs about the law

Group	Remember exactly what the new law is about	Remember the video content	Willing to give comments again	Opinion on the law is similar to initial opinion
Nationalism	11.49%	19.54%	16.09%	21.43%
Critical	3.33%	16.67%	16.67%	33.33%
Mixed	13.64%	32.95%	15.91%	7.14%
Control	10.29%	-	17.65%	25.00%

Panel B: Changes in respondents' beliefs of others' attitudes

Group	Belief about the average proportion of the public that supports the new law (%)		Average perception of difference between support and opposition (%)	
	Main Expt	Follow-up	Main Expt	Follow-up
Nationalism	74.08	66.6	12.94	18.05
Critical	61.67	63.4	20.46	19.01
Mixed	65.01	67.58	13.01	14.92
Control	69.15	70.28	13.40	14.24

Notes: The table presents the beliefs and attitudes of respondents who participated in the main experiment, during follow-up surveys at 6 and 12 months. Panel A shows information on respondents' memory of the video content, the legal provisions, and whether their willingness to express an opinion changed. Panel B displays respondents' perceptions of others' attitudes toward the law, compared to the opinions they expressed in the main experiment.

Table A8: The Spread of Video Content and its Influence on Commenting

Panel A: Total Video Views (Thousands), 2023.9.1–2023.9.30

	TikTok		Kuaishou	
Video Type	**Emotional**	**Calm**	**Emotional**	**Calm**
Nationalism	18,885	4,709	900	105
Critical	1,459	936	342	28

Panel B: Reduction in the Number of Opposing Comments Due to Short Video Exposure

		Discount Factor: Real-World Difficulties		
		30%	20%	10%
Discount Factor:	90%	362,013	241,342	120,671
Repeated	80%	321,789	214,526	107,263
Viewers	70%	281,566	187,710	93,855

Notes: This table shows the total views and impacts of the short videos identified. Panel A provides an estimate of the views for videos discussing the legal amendment during the public consultation period on China's top two short video platforms, TikTok and Kuaishou. Since the total view count is unavailable, we use the total number of "likes" and apply a conservative 5% like-to-view ratio to estimate the total views. Panel B estimates the reduction in opposing comments in real-world scenarios, based on our experimental results and accounting for other factors that might influence individuals' willingness to comment. The analysis includes two discount factors: one that reflects real-world difficulties in commenting (Real-World Commenting Difficulties), such as challenges in finding or using the website, and another that accounts for the declining marginal impact of videos on viewers who watch them several times (Repeated Viewers).