

Testing Legislator Responsiveness to Citizens and Firms in Single-Party Regimes: A Field Experiment in the Vietnamese National Assembly

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We investigate whether communicating constituents' preferences to legislators increases the responsiveness of delegates to the Vietnamese National Assembly (VNA). Using a randomized control trial, we assign legislators to three groups: (1) those briefed on the opinions of their provincial citizenry, (2) those presented with the preferences of local firms, and (3) those receiving only information on the Communist Party's objectives. Because voting data are not public, we collect data on a range of other potentially responsive behaviors during the 2018 session. These include answers to a VNA Library survey about debate readiness; whether delegates spoke in group caucuses, query sessions, and floor debates; and the content of those speeches. We find consistent evidence that citizen-treated delegates were more responsive, via debate preparation and the decision to speak, than control delegates; evidence from speech content is mixed.

An emerging literature explores the responsiveness of authoritarian legislatures to citizens' demands, concluding that officials are receptive to information from citizens (Meng, Pan, and Yang 2017), view themselves as responsive (Manion 2014, 2016), and take policy positions congruent with survey evidence on local preferences (Truex 2016). There is even evidence that despite flawed elections, electoral competition alters government expenditures in patterns consistent with responsiveness (Miller 2015). Despite this enormous progress, analysts have yet to establish a direct causal connection between the articulation of constituency preferences regarding a specific policy debate and the actual behavior of delegates upon learning that information. Establishing this link is critical for shedding light on the varying performance of authoritarian regimes over time (Dimitrov

2013; Gandhi 2008; Gehlbach and Keefer 2011; Magaloni and Kricheli 2010; Wright 2008). In this article, we provide the first test of this link in the theory of authoritarian responsiveness with a randomized control trial providing selected national parliamentarians with information on the opinions of firms and citizens in their province concerning the amendment of the Vietnamese Law on Education (VLOE).

In their seminal discussion of responsiveness, Manin, Przeworski, and Stokes (1999, 9) write that a "government is 'responsive' if it adopts policies that are signaled as preferred by citizens." While responsiveness may follow from the threat of electoral sanctions and while some authoritarian countries do have semicompetitive elections, electoral accountability is not necessary for responsiveness. Governments may also choose to be responsive because of the public spiritedness

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of officials or the checks and balances of different government actors (3–4). In single-party regimes like Vietnam, this latter mechanism manifests as upward accountability to central politicians within the party (Wang 2017). These mechanisms have provided the impetus for research into how legislators in regimes where elections are flawed or absent may nevertheless be responsive (Chen, Pan, and Xu 2016; He and Warren 2011; Malesky and Schuler 2010; Martinez-Bravo, Padró i Miquel, and Qian 2012; Meng and Pan 2015; Meng et al. 2017; Truex 2016). Critically, all three mechanisms for responsiveness (elections, public spiritedness, and upward accountability) hinge on delegates possessing clear information about the preferences of their constituents over policy and then acting on that information (Manin et al. 1999). This insight motivates our field experiment, which aims to lift the informational constraint for treated delegates in order to test how they respond.

Two criteria are necessary for empirically testing whether responsiveness is present. First, there must be an informative signal of aggregate preferences to government actors. Second, responsiveness requires the adoption of policies in line with the signaled preferences. In other words, it necessitates behavioral evidence that the politician moves to enact constituents' objectives. Because of the difficulty of working in authoritarian regimes, scholars have thus far only imperfectly satisfied these criteria. The most well-identified experimental evidence of responsiveness has relied on messages or posts by individual voters (Distelhorst and Hou 2014, 2017), providing an unclear signal about constituents' aggregate preferences. Furthermore, the outcome variable, responsiveness, has been measured via survey experiments and responses, conveying preferences but not behavior. The best behavioral evidence of responsiveness correlates policy proposals by delegates with citizens' preferences from survey data (Truex 2016). However, because information is observational and not experimentally assigned, we ultimately cannot rule out alternative explanations for the alignment of citizens' and politicians' preferences.

In this article, we attempt to improve on previous work through a randomized control trial of Vietnamese National Assembly (VNA) delegates in the debate over amendments to the VLOE during the May 2018 session of the fourteenth VNA. In order to simulate a clear signal, we provided each treated delegate with public opinion data on preferences over education in her home province. We assigned legislators to one of three groups: (1) a control group receiving only baseline information from the VNA Library (the Library) about central party decrees and government documents stating the preferences and goals of the Vietnamese Communist Party (VCP) regarding educational reform, (2) those briefed on the opinions of citizens within their province (citizen treatment) in addition to VCP

recommendations, and (3) those presented with the preferences of local firms (firm treatment) in addition to VCP recommendations. The two key quantities of interest in our experiment are the differences in delegates' behavior between those receiving the informational treatments (groups 2 and 3) and those learning only of government and party demands (group 1).¹ To obtain behavioral outcome measures, we then observed whether delegates (1) declared themselves prepared to debate the law; (2) spoke about the VLOE in group caucus meetings, query sessions with the education minister, or floor debates; (3) mentioned their own province in those debates; and (4) discussed keywords from our infographics.

We find that delegates receiving the citizen treatment appear more responsive than the control group, an important contribution to the debate over authoritarian responsiveness. Delegates in the citizen treatment were at least 9 percentage points more likely to say that they felt prepared for debate, against a control group baseline of 48%. Citizen-treated delegates were also 11 percentage points more likely to speak in caucus meetings, query sessions, or floor debates, against a control group baseline of 41%. Delegates in the firm treatment, however, were not significantly different from the control group on either measure. More speculatively, topic modeling reveals that treated delegates—particularly those in the citizen treatment—discussed keywords from our infographics in legislative forums.

A reasonable criticism of our experiment is that the infographics, novel in Vietnamese legislative debates, may have had an independent effect on delegate behavior. It is important to remember, however, that firm-treated delegates received similarly constructed infographics from the exact same research organization, the Institute of Public Policy and Management (IPPM) at National Economics University, yet did not prove responsive to local business needs. Several alternative explanations for delegates' behavior are therefore unlikely. Our research design makes it clear that what mattered for delegate activity was either the simple fact that citizens' views were conveyed through the infographics or that citizens' preferences were more salient to delegates, not what the infographics looked like or who sent them.

Our findings present three important contributions to the existing political economy and development literatures. In

1. In addition, we employed a saturation design, so that different shares of delegates were treated within each province (Baird et al. 2014; Benjamin-Chung et al. 2017). This allowed us to account for two additional effects. The first, spillover, occurs when a province's nontreated delegates learn of the infographic; the second, reinforcement, happens when delegates encounter peers within their provincial delegation possessing similar information about constituents' preferences.

contrast to much work on authoritarian elections and assemblies, our project emphasizes that authoritarian legislatures may serve goals beyond mere regime resilience, including the delivery of beneficial policies to the citizenry.² Second, our project adds nuance to the principal-agent relationship linking voters to legislators in developing countries by reversing the direction of information transmission (Besley and Burgess 2002). While disclosing records of politicians' past performance remains a popular approach for improving accountability, politicians may not be at fault for poor performance if they lack information concerning the interests of their constituents (Dunning et al. 2019). We show that only when constituents' preferences are first conveyed to legislators can legislative behavior possibly be deemed responsive—regardless of the theoretical mechanism behind responsiveness. Finally, our article offers experimental evidence that legislative strengthening initiatives, which have received heavy investment from development agencies in recent decades, can in fact induce greater legislator responsiveness (Miller, Pelizzo, and Stapenhurst 2004).

THEORIZING RESPONSIVENESS IN SINGLE-PARTY REGIMES

Figure 1 adapts a stylized depiction of a policy-making process proposed by Manin et al. (1999). Constituents have preferences over policies and convey those preferences to politicians via signals, which can include forms of direct political action like opinion polls, letter campaigns, and demonstrations. Individual politicians then work to enact policies consistent with these signaled preferences and, if successful, alter the status quo (outcomes). A politician is *responsive* when she acts in accordance with the signaled preferences of her constituents when working to enact policies. She can be accountable to her constituents only when they have the ability to sanction her for not adhering to their preferences in policy-making decisions. To reiterate, the threat of sanctions is a necessary condition for accountability—not responsiveness. Why, then, might authoritarian legislators be responsive?

Three potential mechanisms have been offered to explain authoritarian responsiveness. First, electoral accountability remains a possibility due to the presence of semicompetitive elections in many authoritarian states. A second potential driver of responsiveness is public spiritedness on the part of legislators. In the terminology of Fearon (1999, 56), some politicians may be “good types . . . who would act on [voters'] behalf independent of reelection incentives.” In lieu of electoral sanctions or good types, separation of powers is a third

mechanism that may also ensure that policies and outcomes eventually conform to the expectations of constituents (Manin 1994). In single-party regimes, this mechanism manifests as upward accountability to central party leaders who express a desire for delegates to represent constituent perspectives (Wang 2017).

These insights have generated a wave of new work, predominantly focused on China, examining authoritarian legislatures for evidence of responsiveness. Manion (2016) surveys members of local Chinese People's Congresses, finding that they see their main function as representing their geographic constituency. Meng et al. (2017) find that Chinese officials articulate a willingness to incorporate citizens' views into their policy choices. Truex (2017) observes a correlation between support for policies in public opinion polls within Chinese People's Congress constituencies and the number of policy proposals made by deputies from those constituencies on those same issues. Distelhorst and Hou (2017) show that Chinese local governments are as likely to answer constituent e-mails as local governments in Western democracies. Building from this, Chen et al. (2016) note that officials are likelier to respond to messages threatening collective action or citizen whistle-blowing. Beyond China, Malesky and Schuler (2010) show that VNA delegates are likelier to speak and criticize in query sessions when they are full-time local delegates or competitively elected but find no compelling evidence that delegates cite their local constituencies or raise local issues. In the only cross-national research to date on authoritarian responsiveness, Miller (2015) observes that authoritarian governments are likelier to spend heavily on education and health care when the ruling party wins despite a poor electoral performance.

While recent empirical work has produced important advances, difficulties inherent to the authoritarian setting have often hampered efforts to pin down the direct causal connection between aggregate voter preferences and legislative behavior outlined in the Manin et al. (1999) definition. In lieu of an informative signal of aggregate voter preferences, the most common approach to testing responsiveness has been to send politicians the individual messages of voters (Chen et al. 2016; Distelhorst and Hou 2017; Meng and Pan 2015; Meng et al. 2017). Yet, only under a restrictive set of assumptions concerning the awareness of politicians, the cost of sending messages, and the correlation between costs and preferences is it possible to believe politicians would see such information as representative of their constituency as a whole.

Responsiveness also requires that politicians adopt—or at least move to enact—policies in line with constituents' signaled preferences. To date, three creative and valuable approaches have tried to measure actual policy behavior, but

2. See Brancati (2014) for a helpful review.

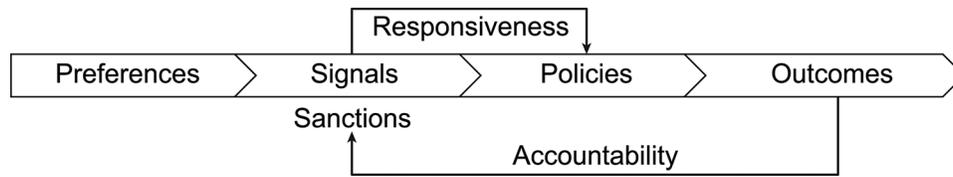


Figure 1. Responsiveness links policy action to constituents' signaled preferences. Policy process adapted from Manin, Przeworski, and Stokes (1999), with black lines representing the actions of politicians.

each suffers from a limitation. First, surveys of politicians on their priorities in office may be prone to social desirability bias, as politicians seek to flaunt their civic-mindedness (Meng and Pan 2015). Second, politicians' responses to randomly assigned constituent e-mails are closer to "performative governance" (Ding 2020) than they are to policy-oriented acts and in fact do not require that the politicians themselves act. Third, researchers have sought to correlate the needs of a constituency, typically identified through survey data, with politicians' preferences (Truex 2017). This comes closer to the classic articulation of responsiveness, but unfortunately we cannot disentangle preference congruence due to responsiveness from that arising out of pure happenstance.

Students of authoritarian regimes have pointed out that politicians may also be responsive to businesses in their communities. Meng and Pan (2015) introduce business interests to the debate. Although directly surveyed local officials claim greater responsiveness to citizens, the authors find that these officials are in fact equally likely to comply with demands of citizens, local businesses, and central officials. These findings are also consistent with the cross-national correlation between authoritarian legislatures and higher levels of domestic investment and gross domestic product growth (Gandhi 2008; Gehlbach and Keefer 2011; Wright 2008). The key insight from the cross-national literature is that authoritarian assemblies are a way for private business interests to defend their property rights, which in turn encourages greater investment. This implies that business interests are entering authoritarian legislative debates either directly through businessmen candidates or indirectly through legislators responding to the demands of businesses in their constituencies (Gehlbach, Sonin, and Zhuravskaya 2010; Szakonyi 2018; Truex 2014). Indirect responsiveness to businesses is encouraged by the fact that in many single-party regimes with quasi-meritocratic promotion, investment and revenue growth are critical for promotion to higher office (Gainsborough 2009; Lü and Landry 2014; Tsai 2007). While these arguments are compelling in explaining the importance of businesses, they simultaneously demonstrate that businesses have a range of different channels for making their views known to government. Consequently, provision of business preferences may be less informative

for authoritarian parliamentarians than data on citizens' preferences.

A critical complication in many states is that legislators have multiple principals (Carey 2008). In addition to acting on the policy preferences of their constituents, they are expected to abide by the mandates of top regime or party leaders and therefore must balance two sets of demands. This cross-pressuring phenomenon occurs in democracies as well (Saiegh 2010), but in an authoritarian setting, responsiveness is not possible when the policy position of regime leaders is both clear and at odds with constituency preferences. According to cooptation and informational theories of authoritarian institutions, however, successful authoritarian regimes are resilient precisely because they are adaptable to pressure from below. Indeed, they argue that a key objective of authoritarian institutions is to convey information to higher-level authorities regarding the preferences of regime outsiders and subnational officials, so that central leaders can adjust their policies accordingly (Gandhi 2008; Reuter and Robertson 2014). Some would argue that this constitutes a mandate for legislator responsiveness through the mechanism of upward accountability.

In the Chinese context, the 2010 and 2015 revisions to the Deputy Law earmark "representation funds" for local Chinese People's Congress deputies, mandate representation training activities, and stipulate that local governments must respond to the proposals, criticisms, and opinions of deputies (Wang 2017). Vietnam's Decree no. 27 endowed VNA delegates with budgets for support and constituency services, while clearly delineating the regime's desired relationship between legislators and constituents. Coupled with the threat of sanctions—be they through control over nomination and vetting procedures or other means—the VNA is structured for responsiveness to constituents via upward accountability to the regime. In this framework, responsiveness to constituents is therefore mediated by central preferences.

Of course, one clear difference between authoritarian regimes and democracies is that the preferences of the authoritarian leadership and constituents often do not align, and authoritarian leaders enact, implement, and enforce policies against the will of constituents and their representatives in parliament.

Recent work has demonstrated various strategies that authoritarian regimes can use to sideline activists and neuter the ability of opposition parties to represent faithfully constituents' interests (Buckles 2019; Gandhi and Przeworski 2007; Kosterina 2017). Consequently, responsiveness is only possible when the authoritarian leadership is divided in its policy preferences or, as is more often the case, in the specific details of how to legislate shared but broad policy goals (Lü, Liu, and Li 2020). Shirk (1993) explains how debates in the Chinese Central Committee became salient when the Politburo was divided over policy or leadership selection. Schuler (2019) applies a similar argument to the VNA, arguing that key institutional changes that have empowered the body—such as the right to query ministers and hold confidence votes—occurred because VCP leaders hoped to use the VNA to check the power of the prime minister and the state bureaucratic apparatus. Thus, from Schuler's perspective, the upward accountability mechanism for responsiveness results directly from elite disagreements and uncertainty over economic policy choices. Empowering constituents and delegates in the VNA is a tool for the VCP to hold the prime minister and his cabinet accountable.

Therefore, four assumptions are necessary to derive the main hypothesis of the responsiveness literature—remaining agnostic as to the particular mechanism at play. First, all delegates are subject to elite leadership messaging about central preferences. Second, there is scope for responsiveness only on the issues for which the authoritarian leadership has uncertain preferences. Third, the average delegate is in fact motivated to be responsive to her constituents. Fourth, such a delegate likely lacks the information concerning her local constituents' preferences that would enable such responsiveness. If citizens' or firms' preferences are already known to delegates through other channels and modes of interaction, informational treatments are unlikely to influence behavior. In other words, a persistent informational gap must prohibit a delegate's inherent receptivity to constituents from blossoming into full-blown responsiveness. Supplying delegates with information on the preferences of constituents should induce responsiveness. Our experiment aims squarely at testing the third and fourth assumptions by eliminating the informational deficit authoritarian legislators frequently face.

RESEARCH DESIGN

The setting for our field experiment is the VNA, whose roughly 500 delegates are directly elected in semicompetitive elections to serve five-year terms and convene biannually to consider draft legislation. Even though campaigning and opposition parties are forbidden, this electoral connection raises the possibility that voters may leverage their ballots to select

“good types” ex ante or “vote the bums out” ex post (Fearon 1999). Importantly, delegates can be distinguished along two lines. Approximately one-third of delegates are central nominees of the party-state in Hanoi, dispatched to represent a province yet maintaining allegiance to their central employers. The remaining delegates are local nominees, typically local government and party officials or professionals who live in the province and are expected to represent local interests.³ There is also a professionalism gap between full-time and part-time delegates, with fully one-third serving year-round in the VNA Standing Committee or in other committee leadership posts. Sixty-nine locally nominated, full-time delegates operate provincial representative offices, provide constituency services, and receive voters' petitions. Part-time delegates attend two plenary sessions per year but otherwise hold full-time jobs outside the assembly and for this reason likely lack the time and capacity to be responsive.

Every four-week legislative session opens with internal group caucuses, each of which provides the delegations from several provinces the opportunity to consolidate viewpoints, determine local priorities, and organize speaking opportunities during legislative debates (see fig. 2 for the experimental timeline). The following two weeks entail various committee meetings and highly publicized query sessions in which delegates question the premier, his deputies, and cabinet ministers on live television. The session culminates in floor debates, where delegates offer well-researched opinions on draft legislation in the hopes of amending the legislation before votes (Malesky, Schuler, and Tran 2012). The upshot of the VNA meeting schedule is that the integrity of individual-level treatments is likely compromised by the group caucuses before a delegate ever finds an opportunity to express herself publicly, be it in a query session or a floor debate. Two potential forms of contamination are possible. First, in the *spillover effect*, treated delegates may pass along information to untreated delegates, increasing responsiveness in the control group. Second, in the *reinforcement effect*, treated delegates may discuss their infographics with peers in their treatment group, thereby reinforcing the confidence of all in this information.⁴

The goal of this project is to measure the responsiveness of legislators in a single-party context—the VNA. To that end, we designed a field experiment, modeled after that of

3. Two delegates were self-nominated.

4. To address these potential stable unit treatment value assumption (SUTVA) violations directly, we modeled the effect of the spillover with a saturation design recommended by Baird et al. (2014) and Benjamin-Chung et al. (2017). The results of this analysis are presented in apps. 4 (table 4.1 model 3; table 4.2 model 3) and 8 (apps. 1–14 available online).

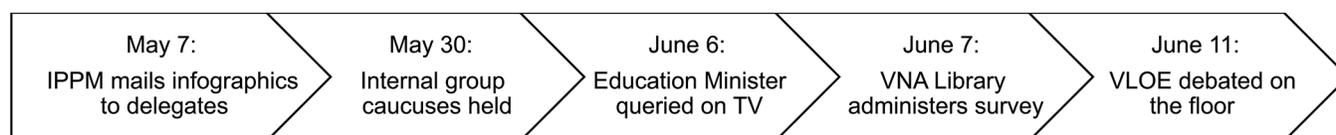


Figure 2. Experimental timeline for May 2018 Vietnamese National Assembly session

Butler and Nickerson (2011), to provide delegates with information on the preferences of their local constituents in the run-up to a legislative session. The first major decision point involved selecting the bill for treatment, subject to three constraints. First, we were restricted to the May 2018 legislative agenda. Second, it was crucial that the bill have high salience both for the citizenry and for local firms, as this would ensure that each constituency held considered opinions on the matter. Finally, we were concerned with the availability of preexisting, high-quality survey data from which to construct the treatments. Application of these criteria led us to select the VLOE, whose current draft consists of 119 articles addressing all aspects of the educational system. The law resulted from a five-year effort at the Ministry of Education to shift Vietnam's curriculum away from knowledge production to developing the capacity for competition in the global economy. The reform effort was thought to "envisage the most drastic and positive changes to education since 1945" (Linh 2015). Before the debate, however, controversy existed about the best teaching methods and curriculum for achieving these goals, while others stressed that corruption and favoritism in education also needed to be addressed by the law (Ho 2017; Le 2017). The VLOE was debated at the May 2018 session, received a vote in the October 2018 session, and ultimately yielded a new National Education Curriculum in December 2018.

Our intervention focused on debates over the substance of the VLOE in May 2018. Topics covered include all educational levels from preschool through vocational and continuing education; the roles of learners, teachers, and the family; finances, tuition, and fees; inspections; and international cooperation. Not only is educational quality of vital importance to parents, but it also affects the business community via labor productivity and training costs. Equally important, these preferences are measured each year via two reputable, nationally representative surveys. We were advised by officials in the VNA that delegates would be wary of making public speeches based on unverified survey information, and therefore they recommended that we use data from well-known instruments rather than tailor-made surveys. The Vietnam Provincial Governance and Public Administration Performance Index (PAPI), conducted annually since 2010 with support from the UN Development Program and others, records citizens' assessments of a host of educational factors, including infrastructure, personnel,

and financial transparency. Similarly, the Vietnam Provincial Competitiveness Index (PCI) has for more than a decade asked Vietnamese firms about the quality of general and vocational education.⁵ Leveraging these individual data and the original survey weights, we constructed for each of Vietnam's 63 provinces a pair of infographics presenting key statistics on the views of citizens and firms regarding the educational system.⁶

Two research design decisions are important to emphasize. First, it was critical that delegates in the firm and citizen treatment groups received similar infographics from the same research institute, the IPPM, as this guaranteed that behavioral differences across the two groups were not attributable to the uniqueness or attractiveness of the infographics, the identity of the sender, or delegates' inferences regarding monitoring of their activity by central authorities. Second, both citizen and firm treatments emphasized topics in which respondents had reasonable knowledge and interest, such as evaluations of teacher quality, school facilities, and educational fees. Other articles in the VLOE emphasize school administration and management, organization of the university system, and integration into international educational networks. Because survey respondents possess little expertise in these areas, they did not feature in the surveys or the infographics. In text analysis of speech content, presented below, we exploit the differences in these two broad topics (national educational system vs. school-level reforms) to identify treatment effects.

Figure 3 displays English translations of the infographics delivered to treated delegates in Nam Dinh province. In recognition of the widely varying educational backgrounds of the delegates themselves, the infographics were kept simple: a title, five percentages with accompanying illustrations and textual explanations, and footnoted source information. Some items reference specific articles scheduled for debate at the VNA session. Citizen and firm bullets 1 reference Articles 27–29 on the goals and quality of general and primary education. Firm

5. Data and survey materials are available at <http://papi.org.vn/eng/> and <http://eng.pci vietnam.org/>. Although the PAPI and PCI reports are provided annually to the VNA by their respective funders, our discussions with VNA officials revealed that legislators perceive the need for processed and relevant information rather than 100-plus-page reports.

6. Provincial-level infographics accord with delegates' mandate to represent their provinces, rather than the subprovincial districts from which they are elected.

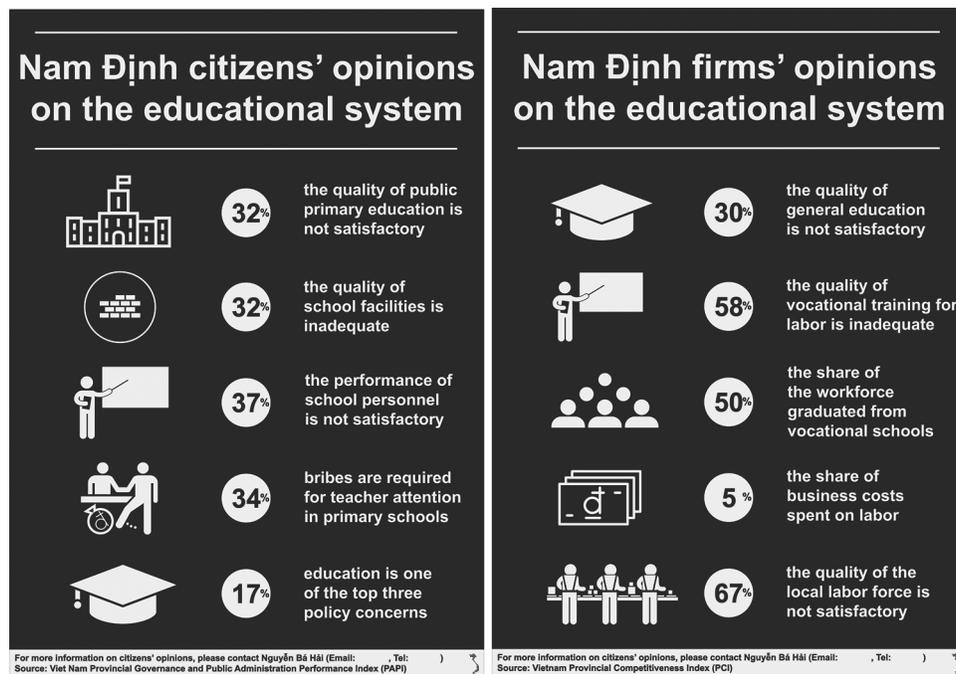


Figure 3. Sample infographics for citizens and firms. Two sets of statistics—one each for citizens and firms—were calculated for each of Vietnam's 63 provinces. Color version available as an online enhancement.

bullets 2–3 provide information regarding Article 31 on vocational education. Citizen bullet 2 provides information on Article 67 concerning school infrastructure funding. Citizen bullets 3–4 reference Article 80 on fostering professional instructors and Article 70 on teacher morality. Citizen bullet 4 also connects to Article 105 on adequate compensation for teachers as a way to stave off informal charges to students. Other items, including citizen bullet 5 and firm bullets 4–5, describe more general perceptions of education in the province and its economic impact, both of which were related to the debate. All of these cards convey preferences for change, which might also be interpreted as grievances over current outcomes.

Printed infographics were delivered in sealed envelopes containing a short explanatory note on letterhead from IPPM. To be clear, the infographics differ by province, as well as treatment arm. Data and analyses in appendix 6, however, examine the relative strengths of the citizen and firm treatments across all 63 provinces, finding little heterogeneity. While the specific numbers for each province differ slightly, overall scores exhibit little variance. The general message of the infographics was designed to be quite clear—large portions of citizens and firms were dissatisfied with the quality of education and personnel in their provinces and were seeking policy improvements to rectify the situation.

Using nationally known surveys came at the cost of perfect symmetry across infographics, as the treatments vary slightly because of the availability of indicators in the PCI and PAPI data sets. One difference is important to highlight when

analyzing the results presented below. The citizen treatment included a statistic indicating the share of citizens who saw education as a top priority, while those receiving the firm infographic did not see a similar measure of issue salience. This means that our average treatment effect cannot disentangle whether delegates are more responsive to the expressed preferences of citizens or to the salience citizens attached to the issue. We return to this difference between the identity of the sender and the salience of the message in the conclusion.

We have qualitative evidence that delegates received the cards, interpreted them as requests for policy improvement, and used them in the debate sessions. Although two delegates directly quoted statistics from the cards in floor and caucus debates, the impact of the cards was generally more subtle and technocratic as delegates requested amendments to specific words or phrases in the law that reflected the preferences of firms and citizens.⁷ We test these behaviors more precisely below.

Because all downstream outcomes are potentially contaminated by group caucusing, we followed Baird et al. (2014)

7. Appendix 11 provides a selection of quotes from delegates in all three treatment groups and legislative forums to illustrate what delegates are saying about the VLOE, paying particular attention to the clauses primed by the cards. The quotes give a sense of the technocratic nature of the debate, illustrating delegates' focus on clause-level details and lack of showboating. Appendix 12 qualitatively illustrates that delegates' suggestions about the draft text correlated to changes in the final law.

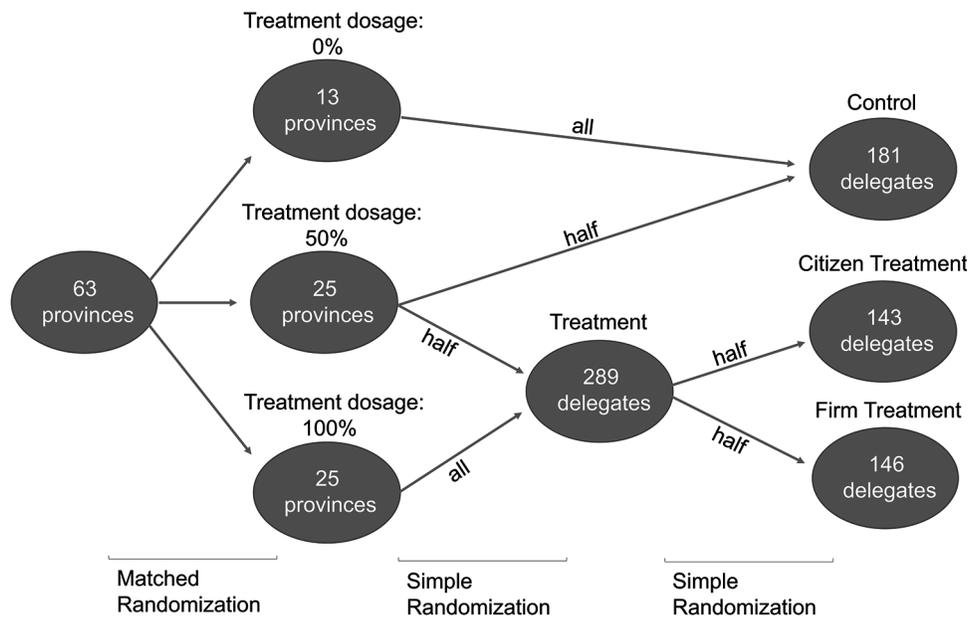


Figure 4. Three-stage randomization procedure. Each province was assigned a dosage, and each delegate to one of three conditions. Color version available as an online enhancement.

in adopting a saturation design with three randomizations across two levels (see fig. 4). In the first stage, we used a genetic matching algorithm to assign each province to one of three treatment dosages: 0%, 50%, or 100%.⁸ All delegates representing provinces in the 100% dosage were assigned to the treatment condition, and all delegates from provinces in the 0% dosage were assigned to the control condition. One simple randomization assigned all delegates from the 50% dosage provinces to one of two conditions, control or treatment. In the third stage, a second simple randomization assigned each delegate in the treatment condition to one of two arms, citizen or firm. After randomization, approximately 40% of delegates were assigned to the control condition, with around 30% assigned to each treatment arm. The approach generates six different treatment groups: control in 0% and 50% provinces, citizen in 50% and 100% provinces, and firm in 50% and 100% provinces.⁹

Because of space constraints, for the main analyses below, we aggregate the six groups to three main treatment arms (control, citizens, and firm). However, the spillover analysis

is discussed in detail in appendix 8. As table 1 confirms, the randomization achieved balance on the three delegate covariates—indicators for full-time and central nomination status and for competitive elections—used in later analyses. Importantly, delegates were also largely balanced across three separate education variables: a dummy variable indicating a career in education as a teacher, professor, or school administrator; a continuous variable marking years of formal education; and a categorical variable classifying delegates by highest level of educational attainment (1 = high school, 2 = bachelor’s, 3 = master’s, 4 = doctorate).

Survey outcomes

In our preanalysis plan (PAP), the key quantities of interest were the average treatment effects comparing citizen-treated to control delegates (hypothesis 1) and firm-treated to control delegates (hypothesis 2). To reiterate:

H1. Citizen: Compared to VNA delegates who learn only of party and government demands, delegates receiving information about citizens’ preferences will be more likely to feel prepared, to speak, and to convey those preferences in legislative settings.

H2. Firm: Compared to delegates in the control group, delegates receiving information about firms’ preferences will be more likely to feel prepared, to speak, and to convey those preferences in legislative settings.

8. See app. 3 for matching covariates.

9. Analysis of these groups in table 7.1 reinforces the finding that the citizen infographic generated significant delegate responsiveness. Because the saturation levels mix both the citizen and firm treatments, the six treatment groups are inappropriate for measuring spillover of a particular treatment. Appendix 8 therefore employs a more direct test using exact treatment shares, finding no evidence of spillover and some evidence of reinforcement.

Table 1. Randomization Achieved Balance across Treatment Conditions

	Control		Citizen		Firm		Control-Citizen		Control-Firm		Citizen-Firm	
	Mean (1)	SD (2)	Mean (3)	SD (4)	Mean (5)	SD (6)	<i>p</i> (7)	<i>t</i> (8)	<i>p</i> (9)	<i>t</i> (10)	<i>p</i> (11)	<i>t</i> (12)
Full time	.343	.476	.343	.476	.336	.474	.998	-.002	.896	.131	.900	.126
Central nominee	.199	.400	.224	.418	.185	.390	.588	-.542	.750	.318	.415	.817
Competitively elected	.552	.499	.510	.502	.507	.502	.454	.750	.413	.820	.951	.062
Educational career	.039	.193	.035	.184	.021	.142	.860	.176	.330	.975	.458	.743
Years of education	11.343	.951	11.273	.965	11.068	1.061	.516	.651	.016	2.431	.088	1.712
Level of education	2.856	.761	2.867	.833	2.829	.825	.904	-.120	.756	.311	.694	.393
Prepared for debate	.481	.502	.709	.457	.576	.497	.001	-3.302	.176	-1.357	.058	1.904
Spoke	.409	.493	.510	.502	.459	.500	.069	-1.825	.366	-.906	.382	.875
Mentioned province	.028	.164	.091	.288	.075	.265	.020	-2.340	.058	-1.902	.633	.478
<i>N</i>	181		143		146							

Note. Columns 1–6 display summary statistics; cols. 7–12 demonstrate balance across treatment arms.

Testing these hypotheses requires a direct behavioral measure of responsiveness. Unfortunately, two ideal outcomes are unavailable in our research setting and indeed are rarely available in authoritarian regimes. First, voting data are only revealed to the public in aggregate, so individual votes cannot be attributed to delegates. Second, only the president, prime minister, and speaker of the VNA are allowed to introduce legislation, so information on bill sponsorship is also unavailable (Lü et al. 2020; Truex 2016). Nonetheless, VNA delegates do engage in a wide range of policy-making activities in the caucuses, floor debates, and query sessions—all observable to the public.

Our initial outcome derives from the delegate survey, which covered three bills from the May 2018 legislative agenda and asked whether the delegate was prepared to debate each bill.¹⁰ Delegates who indicate their readiness to debate the VLOE are coded as being responsive. To understand why, it is important to recall the experimental timeline,

as well as two of the assumptions behind our experimental approach. First, because the survey was administered following the group caucuses and the query session, a delegate's response is not merely a prospective assessment of debate readiness but may also be influenced by her actual experience in two recent legislative settings. In other words, a delegate may indicate preparedness because she has already participated in exchanges regarding the bill. Next, we assume that the average VNA delegate in fact desires to be responsive to her constituents yet likely lacks information on the preferences of those constituents. In other words, an inherent receptivity to constituents is rendered latent by an informational gap. By supplying targeted information and thereby raising the probability that the informational gap is filled, our infographics should, on average, induce responsiveness on the part of delegates. While the survey did not probe whether delegates had decided to vote in accordance with the preferences of their constituents (a question falling outside the Library's official mandate and therefore off-limits), the provision of these preferences should be the only fact distinguishing treated and control delegates. If the provision of this information has indeed caused treated delegates to feel prepared for debate at higher rates, responsiveness to constituents then becomes the most natural interpretation. Finally, by examining whether delegates feel better prepared for debate after receiving a signal of constituents' preferences, we hew more closely to our theoretical framework by probing the assumption that legislators face an informational gap.

To test this claim, we regress a dichotomous indicator for preparedness on three delegate-level covariates and individual treatment assignments. More concretely, we run a linear

10. See app. 1 for the survey instrument, in the original Vietnamese and English translation. Our original PAP called for the survey to be sent out before the group caucuses and its concomitant threat of spillover, but the Library encountered logistical troubles that seriously delayed survey administration. Before any posttreatment data were collected, we addressed this by amending the PAP to incorporate an interaction between individual-level treatment dummies (one each for citizens and firms) and provincial-level treatment shares. Another deviation from the PAP prompted by delayed survey administration is that delegates never received a second treatment (i.e., citizen-treated delegates would have received the firm treatment and vice versa).

probability model with the following specification, where i indexes delegates:

$$\Pr(Y_i = 1) = \beta_0 + \beta_1 \text{Cit}_i + \beta_2 \text{Firm}_i + \gamma \mathbf{X}_i + \varepsilon_i,$$

where \mathbf{X} denotes indicators for full-time, central nomination, and competitive elections, which both theory and prior work suggest may influence responsiveness. Our ordinary least squares (OLS) specifications consist of a baseline with treatment dummies only and a second model adding covariates.¹¹

Behavioral outcomes

While the survey results may provide a useful indicator of an intention to be responsive, a survey response is not a behavioral outcome, which we have argued is critical to identifying actual responsiveness. To supplement the survey, then, we analyze the pooled transcripts from three distinct legislative settings. The first of these, the group caucuses, breaks new ground in the study of authoritarian institutions, for these caucuses constitute previously unstudied internal deliberations. Transcripts from the remaining two sources, query sessions and floor debates, are publicly available and have been productively employed in past work on the VNA (Malesky and Schuler 2010; Malesky et al. 2012). All transcripts were obtained as Word documents, manually skimmed to ensure consistent formatting, and exported to text files. Using standard text-parsing methods implemented in R, we split these files into speaker-speech chunks, concatenated them by speaker, and matched each speaker to other delegate-level data. The first—and most basic—measure of responsiveness derived from delegate remarks is an indicator variable equaling 1 when a delegate spoke at all and 0 otherwise. As previous work has noted, legislators speak infrequently in the Vietnamese context. A treatment effect on delegate speech would therefore indicate that treated delegates have more information concerning the preferences of their constituents to discuss in caucuses, query sessions, or floor debates. We again opt for linear probability models with and without covariates.

If delegate speech is a measure of responsiveness, then closer scrutiny of the content of those remarks should yield more refined measures of responsiveness. Our primary results include an indicator for whether a delegate mentioned her own province, as this would plausibly accompany a discussion of the information contained in the treatments. Appendix 4 presents additional analyses examining constituency synonyms and particular articles from the VLOE (table 4.4). The

final, and most speculative, of the behavioral analyses applies the structural topic model (Roberts et al. 2014) to estimate the effect of the treatments on the prevalence of infographic-related keywords in delegates' statements. While the topic model was not in our PAP, it facilitates analysis of the content of delegates' speeches, thereby shedding light on whether issues raised in the infographics entered speeches.

Analyzing the content of delegates' remarks at the individual level poses two related problems. First, there is the issue of selection into speaking. Few delegates speak in any one setting, and subsetting the data to exclude those who do not introduces posttreatment bias (Montgomery, Nyhan, and Torres 2018). In addition, speaking delegates do not want to repeat one another's points. Typically, then, only one or two delegates from each province will speak in a particular setting. This means that even if there was no spillover, speaking behavior would be most accurately measured at the provincial level. In order to account for these ceiling effects, and to avoid the posttreatment bias associated with subsetting on speech, we conduct most content analyses with provincial-level indicator variables (not counts). While the topic model results, whose unit of analysis is the delegate-forum speech, may suffer from posttreatment bias, we offer them simply as suggestive evidence that the citizen treatment may have affected speech content.

RESULTS AND ANALYSES

Before presenting and interpreting our results, we address two methodological concerns. In lieu of regression tables with asymptotic p -values, we instead adopt a randomization inference-based approach coupled with graphical presentation of all results (Fisher 1937). RI has been increasingly recommended for analyzing experiments in "low information" settings such as those with complex randomization procedures, binary outcomes, clustering of observations, or a small number of observations (Gerber and Green 2012). Because RI is nonparametric and simply replicates the original randomization procedure, results are not model dependent and are therefore less influenced by the analyst's specification choices (Imbens and Rubin 2015). An additional feature of RI is that is designed to test the more conservative sharp null hypothesis of no treatment effect for all subjects. Young (2019), for instance, shows RI reduced statistically significant results in top economic journals by as much as 22%.

First, we reassigned delegates to treatment and control groups 10,000 times in precise accordance with the three-stage randomization procedure detailed above; covariates and outcomes remained undisturbed. Ideally, all potential randomizations should be realized, but when the combinatorics do not permit complete enumeration a large sample provides

11. Although the PAP called for the addition of provincial fixed effects, this proved impossible because of the provincial-level dosage design. Realizing this belatedly, we opted instead for randomization inference (RI) and, in app. 5, province-clustered standard errors.

a good approximation (Gerber and Green 2012). In the second step, we conducted all analyses on each of the newly randomized data sets. Finally, by comparing our actual experimental estimates to the distribution of rerandomized estimates, we obtained an answer to the question: Under the sharp null hypothesis of no effect, just how unusual are our experimental results? If, for example, an experimental result is smaller than 1 (or exceeds 39) out of every 40 rerandomized results, then it is deemed significant at the .05 level.

We also call attention to the survey nonresponse evident in table 1. Although the causes of this nonresponse are unknown and may vary idiosyncratically, analyses in appendix 10 indicate that survey nonresponse is uncorrelated with our treatments and all but one covariate (central nomination status). Because recent work has shown that when missingness is driven by values of the independent variables list-wise deletion should not bias regression results (Arel-Bundock and Pelc 2018), we consign multiple imputation-based results to a robustness check in table 5.1.

Direct treatment effects on survey outcomes

Did delegates exhibit responsiveness? To answer that question, we now present the results of the delegate survey, first with *t*-tests and then via RI. Columns 1–6 of table 1 provide the unadjusted differences in three individual-level outcome variables. It is clear that citizen-treated delegates are more likely than the control group to say that they were prepared to debate (22.8 percentage points), to speak during the VNA session (10.1 percentage points), and to mention their home province (6.3 percentage points). As columns 7–12 make clear, these differences are statistically significant at the .01, .1, and .05 levels, respectively. By contrast, the firm treatment

group is only marginally different from the control group, and only when considering the propensity to name the home province, but even this result could be influenced by non-random selection into speaking.

Figure 5 displays the direct effects of the infographics that emerge from RI, illustrating two primary specifications (figs. 5A and 5B) and two robustness checks (figs. 5C and 5D). Each panel presents the actual experimental coefficients numerically and via a short vertical segment and circle; also present is a density plot of the rerandomized coefficients. Under the baseline regression (fig. 5A), we find a large, statistically significant direct effect of the citizen treatment on debate preparation, and this effect persists with the addition of covariates (fig. 5B). These coefficients imply that the citizen treatment raised the probability that a delegate was prepared to debate by well over 20 percentage points. Although the coefficients for the firm treatment are similarly stable across specifications, they are consistently small and statistically insignificant. As an initial robustness check (fig. 5C), we exclude centrally nominated delegates, for whom responsiveness is theoretically more attenuated. Here as well we find that both treatment effects remain stable. Figure 5D presents the results of a placebo test using two laws—the Law on Livestock and Law on Cultivation—for which no experimental treatments were administered. Interacting a dummy variable for the VLOE with the treatments, the citizen and firm coefficients measure any Hawthorne effect of the infographics or survey, while the treatment-education interaction coefficients capture the marginal effects of the treatments beyond any Hawthorne effect. In addition to a Hawthorne effect of approximately 14 percentage points, we observe a significant 9.1 percentage point effect of the citizen treatment on debate

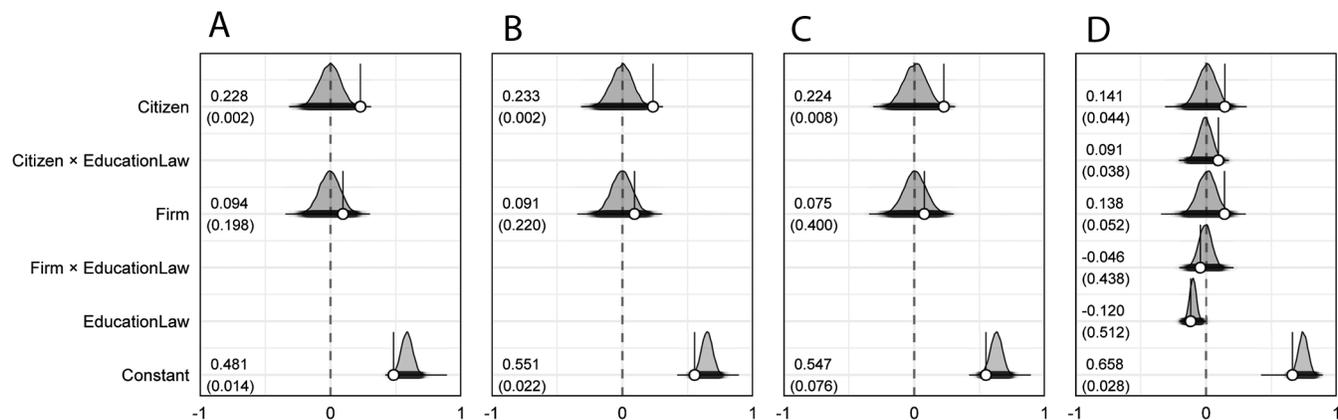


Figure 5. Citizen-treated delegates exhibit greater responsiveness via debate preparation: A, all delegates (*n* = 293); B, all delegates + covariates (*n* = 293); C, no central nominees (*n* = 250); D, placebo test (*n* = 879). Density plots of 10,000 replicate coefficients from randomization inference, with ordinary least squares coefficients and parenthetical *p*-values at left.

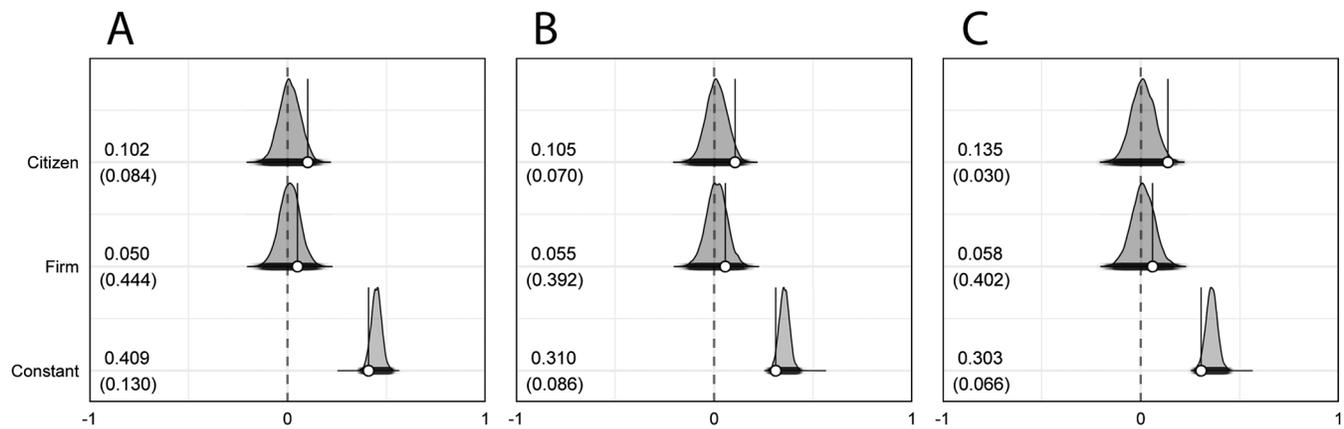


Figure 6. Citizen-treated delegates were more likely to speak in a legislative forum: A, pooled texts, all delegates ($n = 470$); B, pooled texts, all delegates + covariates ($n = 470$); C, pooled texts, no central nominees ($n = 375$). Density plots of replicate coefficients, with ordinary least squares coefficients and parenthetical p -values at left.

preparation—and no effect of the firm treatment. These results clearly indicate that the informational treatment signaling citizens' preferences rendered delegates more prepared for debate and that this effect is not spurious.

Direct treatment effects on behavioral outcomes

We next analyze transcripts from group caucuses, query sessions, and floor debates to obtain further evidence of this responsiveness. We begin with the simple question, were treated delegates more likely to speak on the record, across any of these settings? Figure 6 examines whether the citizen and firm treatments had direct effects on the probabilities with which delegates spoke in any of these contexts. Again, we present a baseline result, add delegate covariates, and remove central nominees. Although substantively weaker and statistically less significant, the direct effects of the citizen treatment remain. Exhibiting stability across specifications, this effect implies a 10–14 percentage point boost in the probability of speaking. Similar to previous results, the firm treatment consistently yields a small null effect.¹²

12. Appendix 13 takes this one step further, examining in greater detail where these direct effects on speaking probability manifest. Group caucuses are internal party-state affairs; query sessions and floor debates occur in the public glare, yet only the latter event is specific to a particular piece of legislation. We find that overall speaking activity is higher in the caucuses, where 26% of control delegates spoke, compared to only 3.4% in the floor debates and 7.5% in the query sessions. The citizen treatment induced delegates to speak primarily in the query sessions with the minister of education—in this setting, the average citizen-treated delegate was 7 percentage points more likely to speak than her firm-treated or untreated peers, a 92% increase over the control. Neither treatment showed an individually significant effect in the floor debate or caucuses, and the firm treatment produced no effect in the query sessions.

Textual outcomes

If the citizen treatment not only causes delegates to feel more prepared for debate but also prompts them to speak at higher rates, can further evidence of responsiveness be gleaned from the contents of their remarks? Appendix 4 presents several provincial-level approaches to this question, examining a delegation's remarks for mentions of their province (table 4.3). This marks attention to local interests, as delegates tend to focus on national-level issues and rarely use their constituencies as examples in their speeches (Malesky and Schuler 2010). We also look at synonyms for citizen and firm constituencies (table 4.4) and specific articles from the debated legislation (Malesky and Schuler 2010). We find some evidence of delegates citing their constituencies but no evidence that delegates named particular clauses. However, searches for the citation of article numbers may be too blunt, missing vital nuance in delegates' speeches.

Thus, our final approach to assessing speech content for treatment effects is the structural topic model, which allows us simultaneously to discover the topics discussed and estimate the effects of the informational treatments on the prevalence of these topics (Roberts et al. 2014). To be clear, we do not intend this as a direct test of our theory, as recent work highlights difficulties in drawing causal inferences from text (Egami et al. 2018). Rather, our analysis is more in the spirit of hypothesis generation, as we hope to discern what, exactly, treated delegates said about the VLOE and whether these statements related in any way to the informational treatments. In our research design, we emphasized that the infographics displayed information concerning issues, such as teacher quality and school fees, in which citizens and firms have reasonable knowledge and interest. By contrast, we did not treat delegates regarding the organization

of the university system or school management. Digging deeper than our preregistered design anticipated, we exploit these differences to explore further whether the infographics influenced the content of delegates' speeches. Because initial analyses revealed that delegates' polite and highly formulaic phrasing produced substantively useless topics, each delegate-forum speech was read and summarized with an open-ended set of keywords by a native Vietnamese speaker informed about neither the treatment conditions nor the purpose of the exercise. We then estimated a two-topic model on these keyword summaries, allowing the relative prevalence of each topic to vary as a function of the treatment assignments, the legislative forum involved, and our standard delegate covariates. Because of nonrandom selection into speaking—which we know to be correlated with our infographics—and the threat of posttreatment bias, the results should be treated with additional caution.

Figure 7 reveals that citizen-treated delegates were 25 percentage points more likely to discuss topic 2, whose most representative keywords include treatment highlights such as “public schools,” “school fees,” and “quality of training.” By contrast, treated delegates were less likely to discuss topic 1, characterized by “school boards,” “rankings,” “decision-making authority,” and so on. These two topics correspond quite closely to the broad categories discussed above in the research design. Topic 1 parallels vocabulary used to discuss the administrative and organizational features of the law that we chose not to treat. Topic 2, however, accurately captures the types of items that the infographics highlight. It is worth noting that the effect of the firm treatment, while statistically significant, is less than half that of and statistically distinguishable from the citizen treatment, a finding consistent with

our other results. Because the representative terms of topic 2 are far more relevant to the infographics we provided, we take this as circumstantial evidence that delegates were responding to the new information.

Alternative explanations

While we argue that higher rates of debate preparation and legislative speech among citizen-treated delegates constitute responsive behavior stimulated by our informational treatments, several plausible alternative explanations for these results remain. Delegates may have felt their activities were being monitored by superiors or inferred that the IPPM was lobbying them to address education. The visually engaging infographics may have heightened the salience of the VLOE, disrupted the status quo and spurred activity, or simply provided delegates with more to say. Yet each of these explanations should hold with equal force for citizen- and firm-treated delegates, and each therefore fails to explain both the significant effects of the citizen treatment and the null effects of the firm treatment in sharp null tests of the hypotheses.

Figure 8 proceeds a step further, presenting the differences between the citizen and firm treatment effects in a RI framework. Following the Keele and Miratrix (2019) approach to generating confidence intervals via RI, we compare the difference in our experimentally obtained coefficients on the citizen and firm treatments to those calculated on our 10,000 permuted data sets. Each figure entry plots the difference between the citizen and firm coefficients, with confidence intervals created by inverting a series of 95% level tests for a grid of treatment differences and retaining the minimal and maximal differences that fail to lead to test rejection. Here the

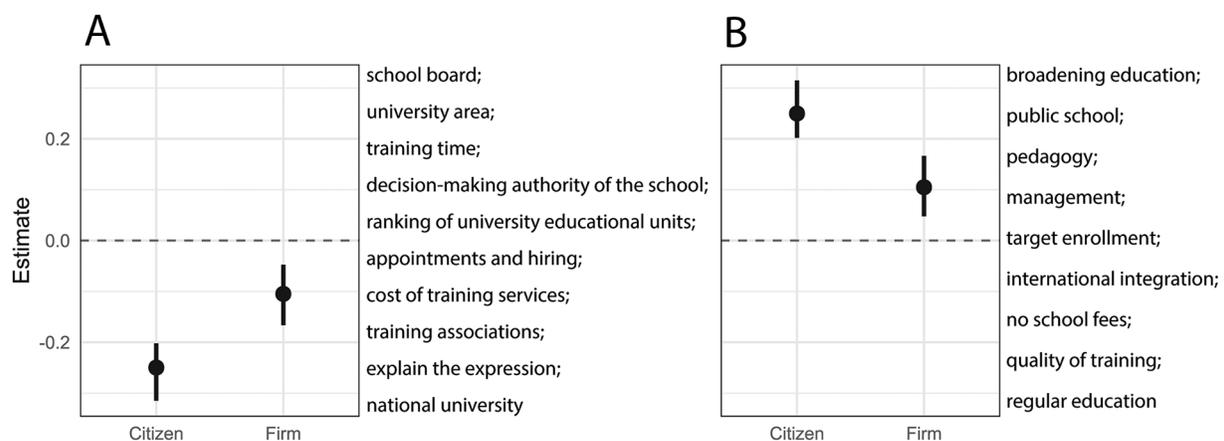


Figure 7. Treated delegates were more likely to discuss keywords associated with the treatments. Marginal effects of treatments on the prevalence of two topics, whose characteristic keywords appear beside each panel. A, Topic 1, national education system. B, Topic 2, school-level reforms. Estimates from the structural topic model (Roberts et al. 2014).

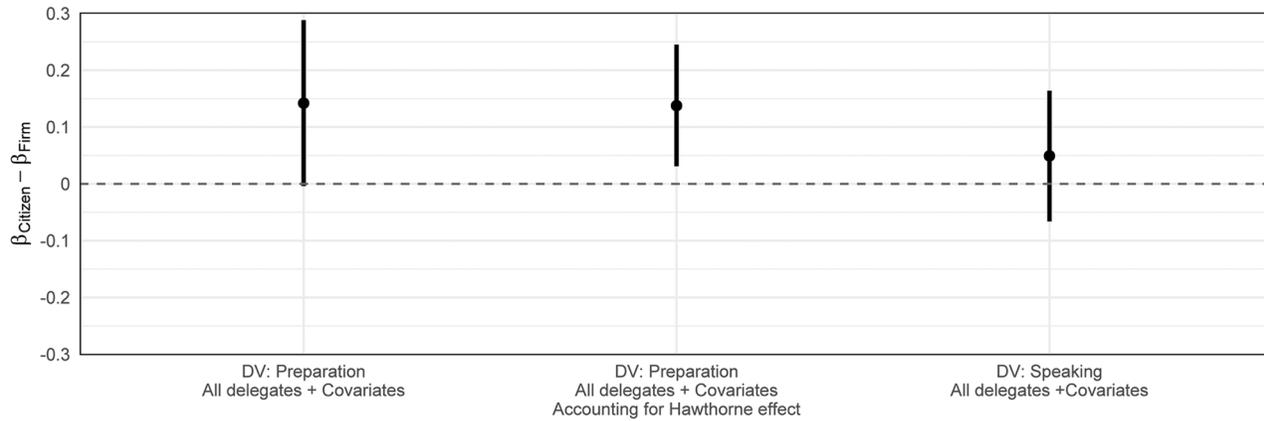


Figure 8. Direct effects of citizen treatment exceed those of firm treatment. Difference in citizen and firm coefficients, with randomization inference–derived confidence intervals.

null distribution is composed of the treatment differences computed for each of the rerandomized data sets. All three differences are positive; the first two entries show that, for debate preparation, the OLS-based citizen ATE is significantly larger than the firm ATE (at the .05 level) after removing the Hawthorne effect and nearly so when this effect is included. Although the speaking outcome does not display a significant difference in treatment effects, the alternative explanations noted above all fail to account for the stronger citizen ATEs associated with debate preparation.¹³

CONCLUSION

Our article presents the first randomized experiment on legislator responsiveness in an authoritarian national assembly, permitting direct testing of the causal link between a national legislator’s knowledge of constituents’ preferences and her consequent legislative behavior. We contribute to the debate over authoritarian responsiveness by answering two questions—are such legislators responsive and, if so, to whom? We find that delegates are indeed responsive but only to signals of citizens’ preferences. We find null effects for legislators treated with information from local firms. Citizen-treated delegates were 23 percentage points more likely than untreated delegates to feel prepared for debate (9 percentage points after accounting for potential Hawthorne effects) and 11 percentage points more likely to speak in caucus meetings, query sessions, or floor debates. Delegates treated with a signal of firms’ preferences, however, did not differ significantly on either measure. Turning to finer-grained measures of responsiveness, we also find that citizen-treated delegates

were 25 percentage points more likely to focus their stated remarks on keywords presented in our infographics.

Does the null effect for the firm treatment mean that delegates were unresponsive to business needs? At this stage in the research program, we cannot be certain. It is also possible that the effects of the firm infographic were smaller because delegates already had substantial prior information regarding firms’ needs or because they were reluctant to advocate publicly for business needs. Further work is necessary to rule out alternative explanations definitively.

The null effects of the firm treatment and statistical tests of the difference between citizen and firm ATEs help to mitigate the threat of several reasonable alternative theories for the observed treatment effects. It is unlikely that responsiveness on the part of the citizen-treated delegates was due to a belief that their activities were being monitored by superior authorities; that the IPPM, another state institution, was particularly focused on education; that the visually engaging treatment increased the salience of the VLOE in the minds of delegates; that the treatment was a shock to the normal patterns of business and consequently spurred activity; or that delegates felt motivated because the infographics provided them with something to say. All of these explanations would have held true for the firm treatment as well, yet we only find evidence for the citizen treatment. Setting aside the citizen treatment and focusing solely on the insignificant effect of the firm treatment, it is clear that all of the above alternatives are inconsistent with the available evidence.

One potential source of confounding may be the fact that only the citizen infographics gauged the salience of education, with no comparable statistic included on the firm infographics. Although one might be tempted to attribute the effect of the citizen treatment to expressed salience as much as to the identity of the constituents, less than 20% of citizens

13. In app. 14, we drop central committee members and find even greater support for differences between the treatments.

ranked education highly, potentially biasing results in the opposite direction of our findings. Nevertheless, if issue salience—not constituent identity—were the most important factor, this would still constitute a theoretically informative measure of responsiveness. When delegates were told an issue mattered to constituents, they acted on it. We cannot disentangle the two in the current project, but we hope to do so in future research by ensuring symmetry across informational treatments.

While these findings move the literature forward, they are limited somewhat by the artificiality of our research approach. First, we detect some evidence of a Hawthorne effect on debate preparation, which should be taken into account when evaluating the substantive effects of the analysis. Second, despite our best efforts, we were unable to mimic exactly how citizens, firms, and other nonstate actors interact with their parliamentary representatives. Institutional, informational, and access barriers insulate delegates from direct interaction with the public they nominally represent. As shown by conducting the experiment, however, overcoming these barriers is possible given significant time, effort, resources, and high-level connections that the average citizen does not possess. Once contacted, delegates do desire information on the preferences of their citizens and even appear willing to act on the information. In the words of Meng et al. (2017), they are indeed “receptive.”

An additional limitation of the current article is that we cannot distinguish between the public spiritedness, upward accountability, and electoral accountability arguments. However, in ongoing work we further test the mechanisms more directly in a debate over the new Labor Code by reminding recipients of the citizen and firm infographics about either the upcoming 2021 election or the VCP’s interest in encouraging responsiveness to constituent concerns.

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