

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). Utilizing 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 is 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.<sup>1</sup>

**Keywords**— responsiveness, legislatures, authoritarian institutions, randomized control trial, Vietnam

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<sup>1</sup>Supplementary material for this article is available in the appendix in the online edition. Data and replication materials that support the findings will be made available on the JOP Dataverse (<https://dataverse.harvard.edu/dataverse/jop>) immediately after publication. This article was deemed to be in compliance with the relevant laws for human subjects by Duke University Institutional Review Board (2017-0115-D0671). The pre-analysis plan for the article was registered with Evidence in Governance and Politics (20180610AB) and is available at OSF Registration (<https://osf.io/5qwtx>). Support for this research was provided by the Jameel Poverty Action Lab Governance Initiative (Grant No. 5710004109).

# 1 Introduction

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 (Gandhi, 2008; Wright, 2008; Magaloni and Kricheli, 2010; Gehlbach and Keefer, 2011; Dimitrov, 2013). In this paper, 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 semi-competitive elections, electoral accountability is not necessary for responsiveness. Governments may also choose to be responsive due to the public spiritedness of officials or the checks and balances of different government actors (Manin, Przeworski and Stokes, 1999, 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 (Malesky and Schuler, 2010; He and Warren, 2011; Martinez-Bravo, Padró i Miquel and Qian, 2012; Meng and Pan, 2015; Chen, Pan and Xu, 2016; Truex, 2016; Meng, Pan and

Yang, 2017). Critically, all three mechanisms for responsiveness (elections, public spiritedness, and upward accountability) hinge upon delegates possessing clear information about the preferences of their constituents over policy and then acting upon that information (Manin, Przeworski and Stokes, 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. Due to 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 paper, we attempt to improve upon previous work through a randomized control trial of VNA delegates in the debate over amendments to the VLOE during the May 2018 Session of the 14th 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).<sup>2</sup> 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 nine 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 fora.

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

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<sup>2</sup>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 non-treated delegates learn of the infographic; the second, reinforcement, happens when delegates encounter peers within their provincial delegation possessing similar information about constituents' preferences.

Economics University (NEU), 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 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.<sup>3</sup> 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 paper 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).

## 2 Theorizing Responsiveness in Single-Party Regimes

Figure 1 adapts a stylized depiction of a policymaking process proposed by Manin, Przeworski and Stokes (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

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<sup>3</sup>See Brancati (2014) for a helpful review.

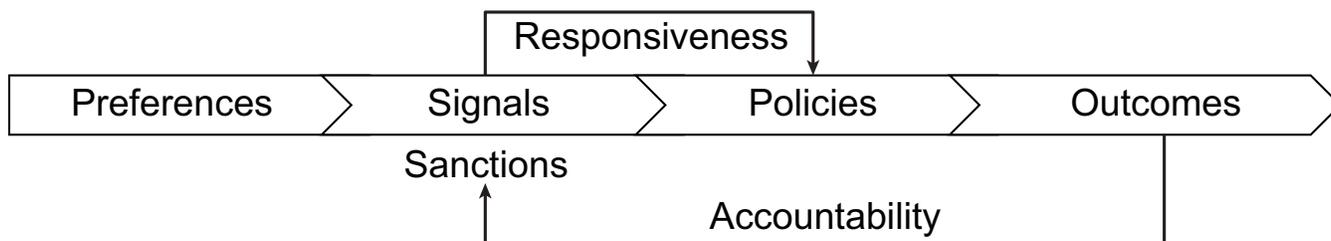


Figure 1: **Responsiveness links policy action to constituents’ signaled preferences.** Figure depicts a schematic of the policy process adapted from (Manin, Przeworski and Stokes, 1999), with thick black lines representing the actions of politicians.

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 policymaking 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 semi-competitive 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 which 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, Pan and Yang (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 emails as local governments in Western democracies. Building from this, Chen, Pan and Xu (2016) note that officials are likelier to respond to messages threatening collective action or citizen whistleblowing. 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, Przeworski and Stokes (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 (Meng and Pan, 2015; Chen, Pan and Xu, 2016; Distelhorst and Hou, 2017; Meng, Pan and Yang, 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 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 emails are closer to “performative governance” (Ding, 2018) than they are to policy-

oriented act, 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 GDP growth (Wright, 2008; Gandhi, 2008; Gehlbach and Keefer, 2011). 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; Truex, 2014; Szakonyi, 2018). 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 (Tsai, 2007; Gainsborough, 2009; Lü and Landry, 2014). 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 also 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 (Saeigh 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 (Gandhi and Przeworski, 2007; Kosterina, 2017; Buckles, 2019). 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, 2018). 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 which 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.

### **3 Research Design**

The setting for our field experiment is the National Assembly of Vietnam (VNA), whose roughly 500 delegates are directly elected in semi-competitive elections to serve five-year terms and con-

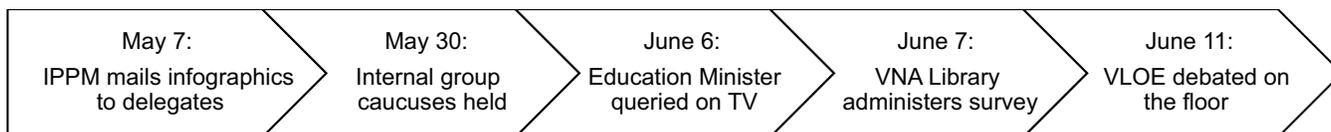


Figure 2: **Experimental timeline for May 2018 VNA session.**

vene 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.<sup>4</sup> 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. 69 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.

Each 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 Figure 2 for 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

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<sup>4</sup>Two delegates were self-nominated.

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.<sup>5</sup>

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 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). Prior to the debate, however, controversy existed about the best teaching methods and curriculum for achieving these goals, while others stressed that corruption and favoritism in the 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

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<sup>5</sup>To address these potential 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 online Appendices 4 (Table 4.1 Model 3, page 4; Table 4.2 Model 3, page 5) and 8 (page 19).

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 United Nations 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.<sup>6</sup> 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.<sup>7</sup>

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'

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<sup>6</sup>Data and survey materials available at <http://papi.org.vn/eng/> and <http://eng.pcivietnam.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 one-hundred-plus page reports.

<sup>7</sup>Provincial-level infographics accord with delegates' mandate to represent their provinces, rather than the sub-provincial districts from which they are elected.

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 versus 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 Bullet 1 reference Articles 27–29 on the goals and quality of general and primary education. Firm 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 online Appendix 6 (page 12), however, examine the relative strengths of the citizen and firm treatments across all 63 provinces, finding little heterogeneity across provinces. 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 based on the availability of indicators in the PCI and PAPI datasets. 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 caucuses 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.<sup>8</sup> We test these behaviors more precisely below.

Because all downstream outcomes are potentially contaminated by group caucusing, we followed Baird et al. (2014) in adopting a saturation design with three randomizations across two levels (see Figure 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%.<sup>9</sup> All delegates representing provinces assigned to

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<sup>8</sup>Online Appendix 11 (page 28) provides a selection of quotes from delegates in all three treatment groups and legislative fora 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. Online Appendix 12 (page 30) qualitatively illustrates that delegates' suggestions about the draft text correlate to changes in the final law.

<sup>9</sup>See online Appendix 3 (page 4) for matching covariates.

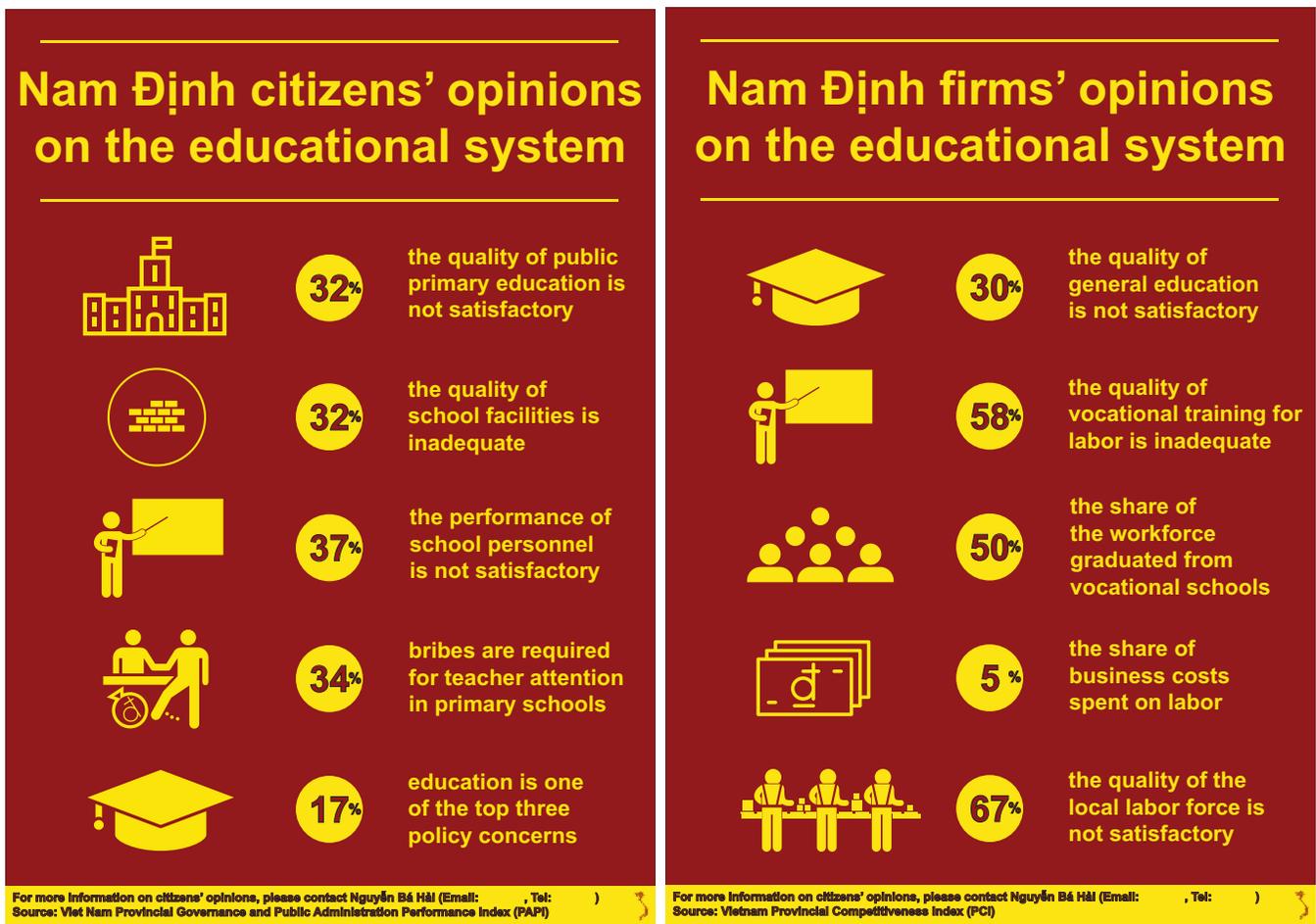


Figure 3: **Sample infographics for citizens and firms.** Two sets of statistics—one each for citizens and firms—were calculated for each of Vietnam’s 63 provinces.

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.<sup>10</sup>

<sup>10</sup>Analysis of these groups in online Appendix Table 7.1 (page 18) 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

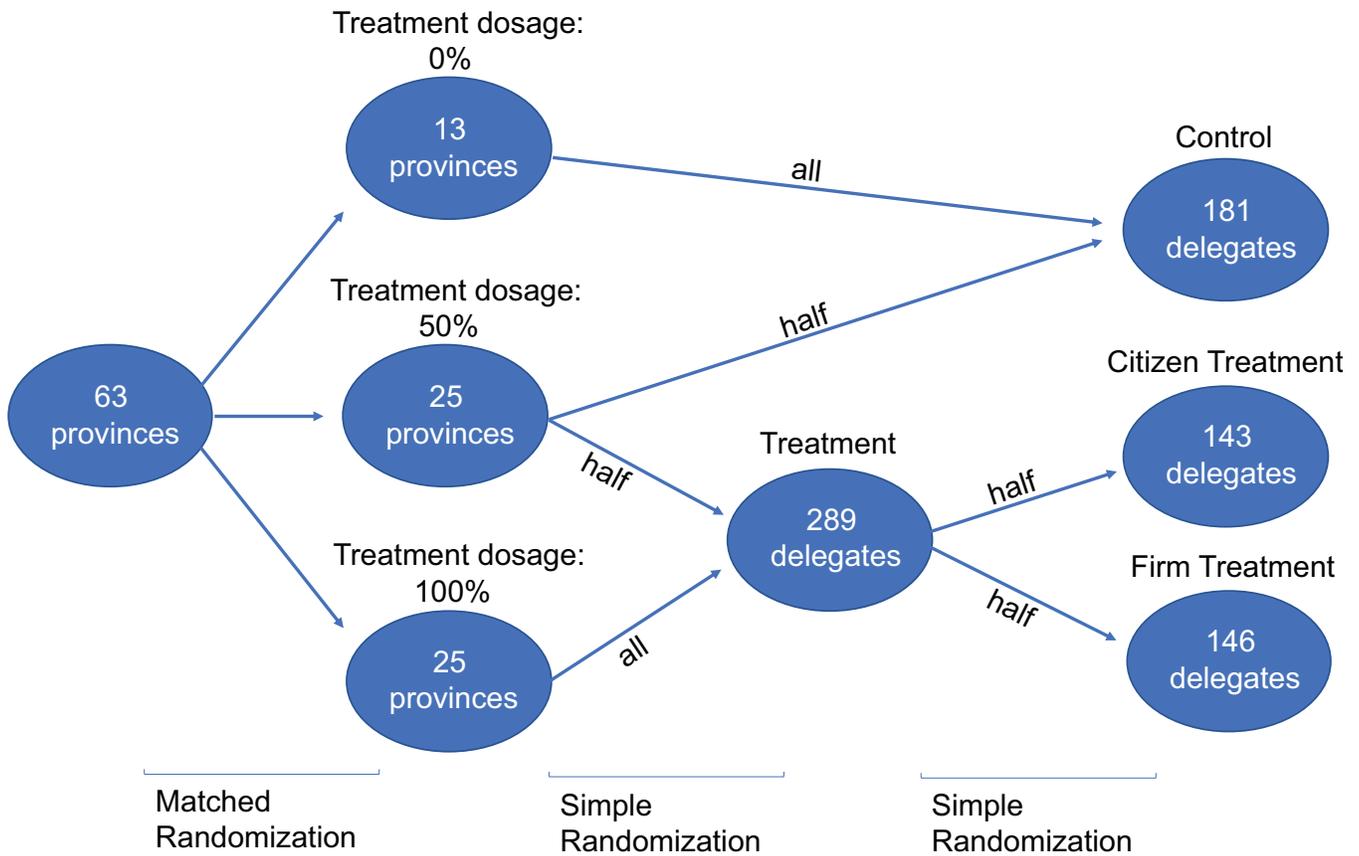


Figure 4: **Three-stage randomization procedure.** Each province was assigned a dosage, and each delegate to one of three conditions.

Due to 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 online Appendix 8 (page 19). 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).

spillover of a particular treatment. Online Appendix 8 (page 19) therefore employs a more direct test using exact treatment shares, finding no evidence of spillover and some evidence of reinforcement.

	Control (N=181)			Citizen (N=143)			Firm (N=146)		
	Mean	SD	NA	Mean	SD	NA	Mean	SD	NA
Full-time	0.343	0.476		0.343	0.476		0.336	0.474	
Central nominee	0.199	0.400		0.224	0.418		0.185	0.390	
Competitively elected	0.552	0.499		0.510	0.502		0.507	0.502	
Educational career	0.039	0.193		0.035	0.184		0.021	0.142	
Years of education	11.343	0.951		11.273	0.965		11.068	1.061	
Level of education	2.856	0.761		2.867	0.833		2.829	0.825	
Prepared for debate	0.481	0.502	73	0.709	0.457	57	0.576	0.497	47
Spoke	0.409	0.493		0.510	0.502		0.459	0.500	
Mentioned province	0.028	0.164		0.091	0.288		0.075	0.265	

	Control-Citizen		Control-Firm		Citizen-Firm	
	<i>p</i> -value	<i>t</i> -stat	<i>p</i> -value	<i>t</i> -stat	<i>p</i> -value	<i>t</i> -stat
Full-time	0.998	-0.002	0.896	0.131	0.900	0.126
Central nominee	0.588	-0.542	0.750	0.318	0.415	0.817
Competitively elected	0.454	0.750	0.413	0.820	0.951	0.062
Educational career	0.860	0.176	0.330	0.975	0.458	0.743
Years of education	0.516	0.651	0.016	2.431	0.088	1.712
Level of education	0.904	-0.120	0.756	0.311	0.694	0.393
Prepared for debate	0.001	-3.302	0.176	-1.357	0.058	1.904
Spoke	0.069	-1.825	0.366	-0.906	0.382	0.875
Mentioned province	0.020	-2.340	0.058	-1.902	0.633	0.478

Table 1: **Randomization achieved balance across treatment conditions.** Top panel displays summary statistics; lower panel demonstrates balance across treatment arms.

### 3.1 Survey Outcomes

In our pre-analysis plan (PAP), the key quantities of interest were the average treatment effects comparing citizen-treated to control delegates ( $H1$ ) and firm-treated to control delegates ( $H2$ ). 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.*

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 is 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 (Truex, 2016; Lü, Liu and Li, 2018). Nonetheless, VNA delegates do engage in a wide range of policymaking 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.<sup>11</sup> 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 their actual experience in two recent legislative settings. In other

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<sup>11</sup>See online Appendix 1 (page 2) for the survey instrument, in the original Vietnamese and English translation. Our original PAP called for the survey to be sent out prior to the group caucuses and its concomitant threat of spillover, but the Library encountered logistical troubles that seriously delayed survey administration. Before any post-treatment 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).

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

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

where  $\mathbf{X}$  denotes indicators for full-time, central nomination, and competitive elections, which both theory and prior work suggest may influence responsiveness. Our OLS specifications consist of a baseline with treatment dummies only and a second model adding covariates.<sup>12</sup>

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<sup>12</sup>Although the PAP called for the addition of provincial fixed effects, this proved impossible due to the provincial-level dosage design. Realizing this belatedly, we opted instead for randomization inference and, in online Appendix 5 (page 9), province-clustered standard errors.

## 3.2 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, Schuler and Tran, 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. Online Appendix 4 presents additional analyses examining constituency synonyms and particular articles from the VLOE (Table 4.4, page 8). 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 post-treatment 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 post-treatment 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 post-treatment bias, we offer them simply as suggestive evidence that the citizen treatment may have affected speech content.

## 4 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). Randomization inference (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. Alwyn Young, for instance, shows RI reduced statistically significant results in top economic journals by as much as 22 percent (Young, 2019).

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 a good approximation (Gerber and Green, 2012). In the second step, we conducted all analyses on each of the newly randomized datasets. Finally, by comparing our actual experimental estimates to the distribution of re-randomized 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 one (or exceeds 39) out of every 40 re-randomized results, then it is deemed significant at the 0.05 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 online Appendix 10 (page 26) 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 listwise deletion should not bias regression results (Arel-Bundock and Pelc, 2018), we consign multiple imputation-based results to a robustness check in online Appendix 5 (Table 5.1, page 10).

## 4.1 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. The top panel of Table 1 provides 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 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 the bottom panel makes clear, these differences are statistically significant at the 0.01, 0.1, and 0.05 levels, respectively. By contrast, the firm treatment group is only marginally

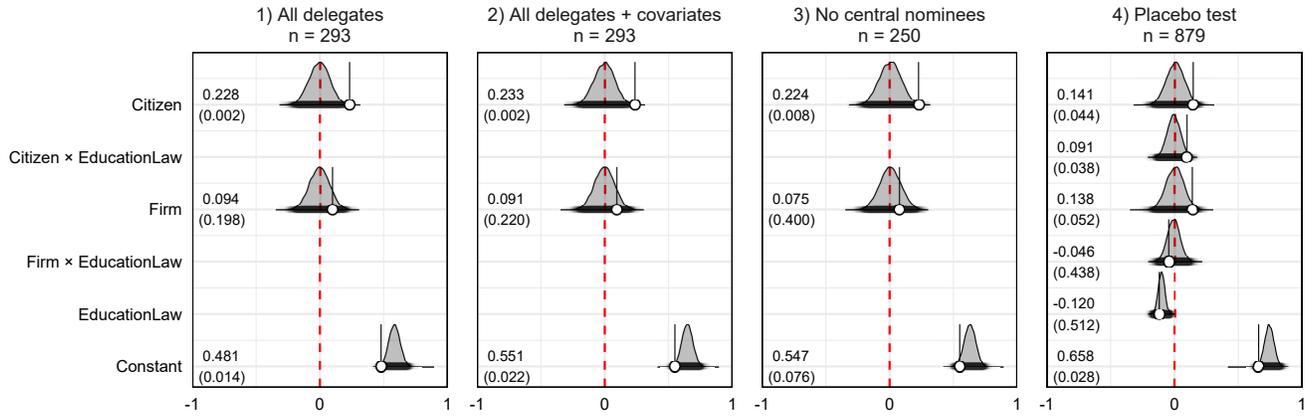


Figure 5: **Citizen-treated delegates exhibit greater responsiveness via debate preparation.** Figure displays density plots of 10,000 replicate coefficients from RI, with OLS coefficients and parenthetical  $p$ -values at left.

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 (left panels) and two robustness checks (right panels). Each panel presents the actual experimental coefficients numerically and via a short vertical segment and circle; also present is a density plot of the re-randomized coefficients. Under the baseline regression (first panel), we find a large, statistically significant direct effect of the citizen treatment on debate preparation, and this effect persists with the addition of covariates (second panel). 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 (third panel), we exclude centrally-nominated delegates, for whom responsiveness is theoretically more attenuated. Here as well we find that both treatment effects remain stable. The final panel 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

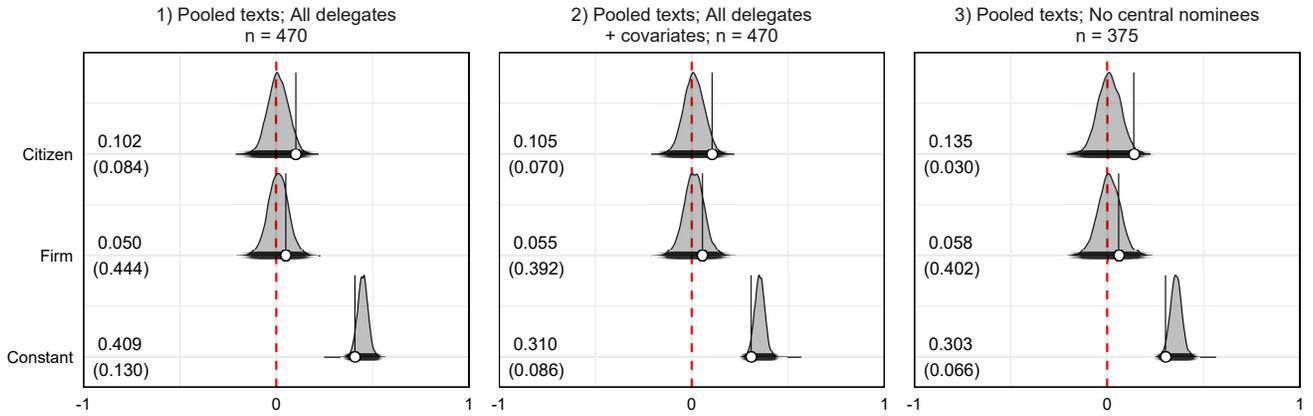


Figure 6: **Citizen-treated delegates were more likely to speak in a legislative forum.** Figure displays density plots of replicate coefficients, with OLS coefficients and parenthetical  $p$ -values at left.

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 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.

## 4.2 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.<sup>13</sup>

### 4.3 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? Online Appendix 4 presents several provincial-level approaches to this question, examining a delegation’s remarks for mentions of their province (Table 4.3, page 7). 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, page 8), and specific articles from the debated legislation (*ibid.*). 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 (STM), 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

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<sup>13</sup>Online Appendix 13 (page 34) 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 seven 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.

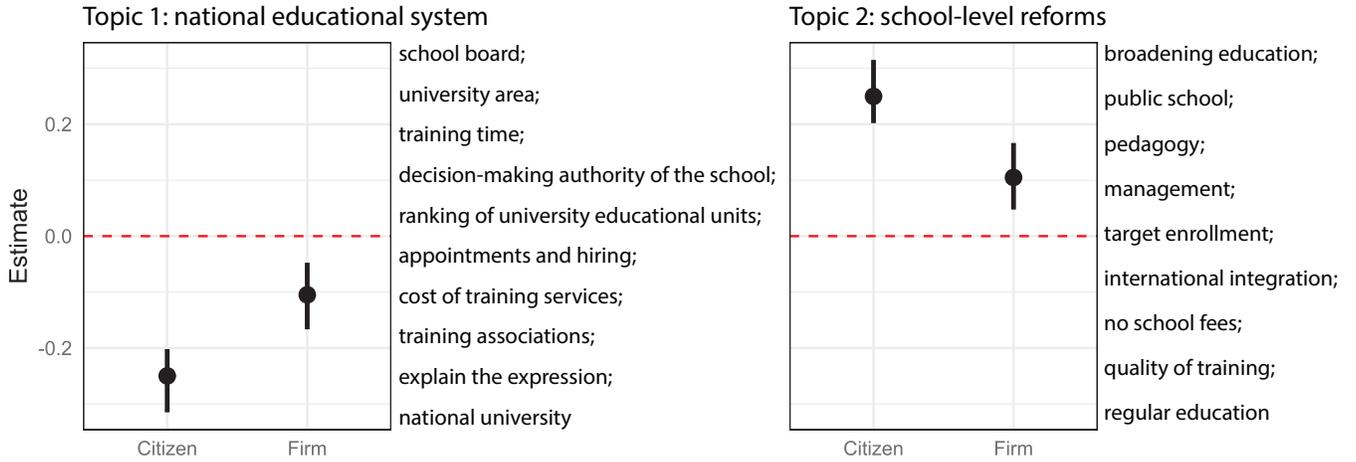


Figure 7: **Treated delegates were more likely to discuss keywords associated with the treatments.** Figure displays marginal effects of treatments on the prevalence of two topics, whose characteristic keywords appear beside each panel. Estimates from the structural topic model (Roberts et al., 2014).

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 pre-registered design anticipated, we exploit these differences to explore further whether the infographics influenced the content of delegates’ speech. 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. Due to nonrandom selection into speaking—which we know to be correlated with our infographics—and the threat of post-treatment bias, the results should be treated with additional caution.

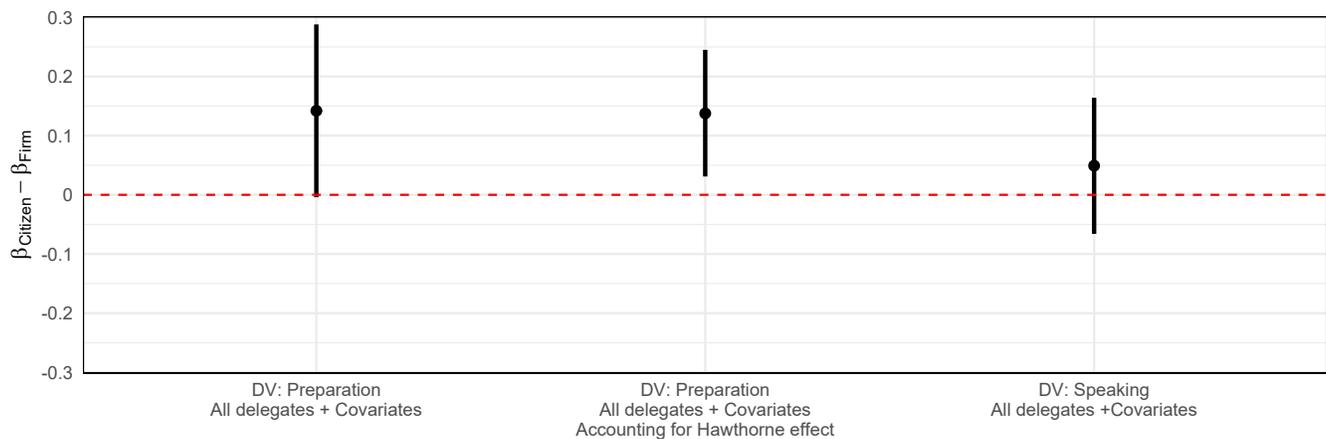


Figure 8: **Direct effects of citizen treatment exceed those of firm treatment.** Figure displays difference in citizen and firm coefficients, with RI-derived confidence intervals.

Figure 7 reveals that citizen-treated delegates were 25 percentage points more likely to discuss topic two, 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 one, characterized by “school boards,” “rankings,” “decision-making authority,” etc. These two topics correspond quite closely to the broad categories discussed above in the research design. Topic one parallels vocabulary used to discuss the administrative and organizational features of the law that we chose not to treat. Topic two, however, accurately captures the types of items which 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 two are far more relevant to the infographics we provided, we take this as circumstantial evidence that delegates were responding to the new information.

#### 4.4 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 (CIs) 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 datasets. Each figure entry plots the difference between the citizen and firm coefficients, with CIs created by inverting a series of 95%-level tests for a grid of treatment differences and retaining the minimal and maximal differences which fail to lead to test rejection. Here the null distribution is comprised of the treatment differences computed for each of the re-randomized datasets. 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 0.05 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.<sup>14</sup>

## 5 Conclusion

Our paper 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

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<sup>14</sup>In online Appendix 14 (page 35), we drop central committee members and find even greater support for differences between the treatments.

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 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 percent of citizens 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 present 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 non-state 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, Pan and Yang (2017), they are indeed “receptive.”

An additional limitation of the present paper 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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## ONLINE APPENDIX

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

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# 1 Delegate Survey Administered by VNA Library

Vietnamese original:

1. Đại biểu thấy còn bản khoản về dự Dự luật Giáo dục sửa đổi?

- Không còn bản khoản
- Còn bản khoản (*Đề nghị Đại biểu trả lời tiếp tại câu 2*)

English translation:

1. Are you [the delegate] prepared to debate the proposed revisions to the Education Law?

- I am prepared to debate.
- I am unprepared to debate (Please answer question two below).

## 2 Transcript Coding and Dictionary

To construct finer grained measures of responsiveness, we applied basic regular expressions and a custom dictionary to code more targeted content outcomes. The first of these is an indicator variable for whether a delegate mentioned her own province. In order to convey to one’s peers the preferences contained in the infographics, it is quite plausible that a delegate might mention her own province. This should also hold for delegates who do not directly discuss their constituents’ preferences but nonetheless act on the infographics more obliquely. Two of the content-specific indicators are constructed from lists of terms which could be used interchangeably with “citizens” or “firms”. The remaining four indicators identify discussion of particular articles within the draft law that are directly relevant to the treatment infographics.

Outcome variables in Table 4.4 (page 8) derived from applying the following Vietnamese regular expressions to the transcripts. To ensure accuracy when character encodings misbehave across operating systems and applications, all Vietnamese regular expressions were first converted to Unicode strings at <https://r12a.github.io/app-conversion/>. For example, string detection for “Điều 31” actually looked for “\u0110i\u1EC1u 31”.

<b>Col</b>	<b>Name</b>	<b>Vietnamese Regex</b>	<b>English Regex</b>
1	Citizens	(ý kiến)?(bầu cử công dân cử tri người bỏ phiếu người dân nhân dân)	(preferences of)?citizens
2	Firms	(ý kiến)?(công ty doanh nghiệp doanh nhân)	(preferences of)?firms
3	Art. 27-29	Điều (27 28 29)	Article (27 28 29)
4	Art. 31	Điều 31	Article 31
5	Art. 70	Điều 70	Article 70
6	Art. 105	Điều 105	Article 105

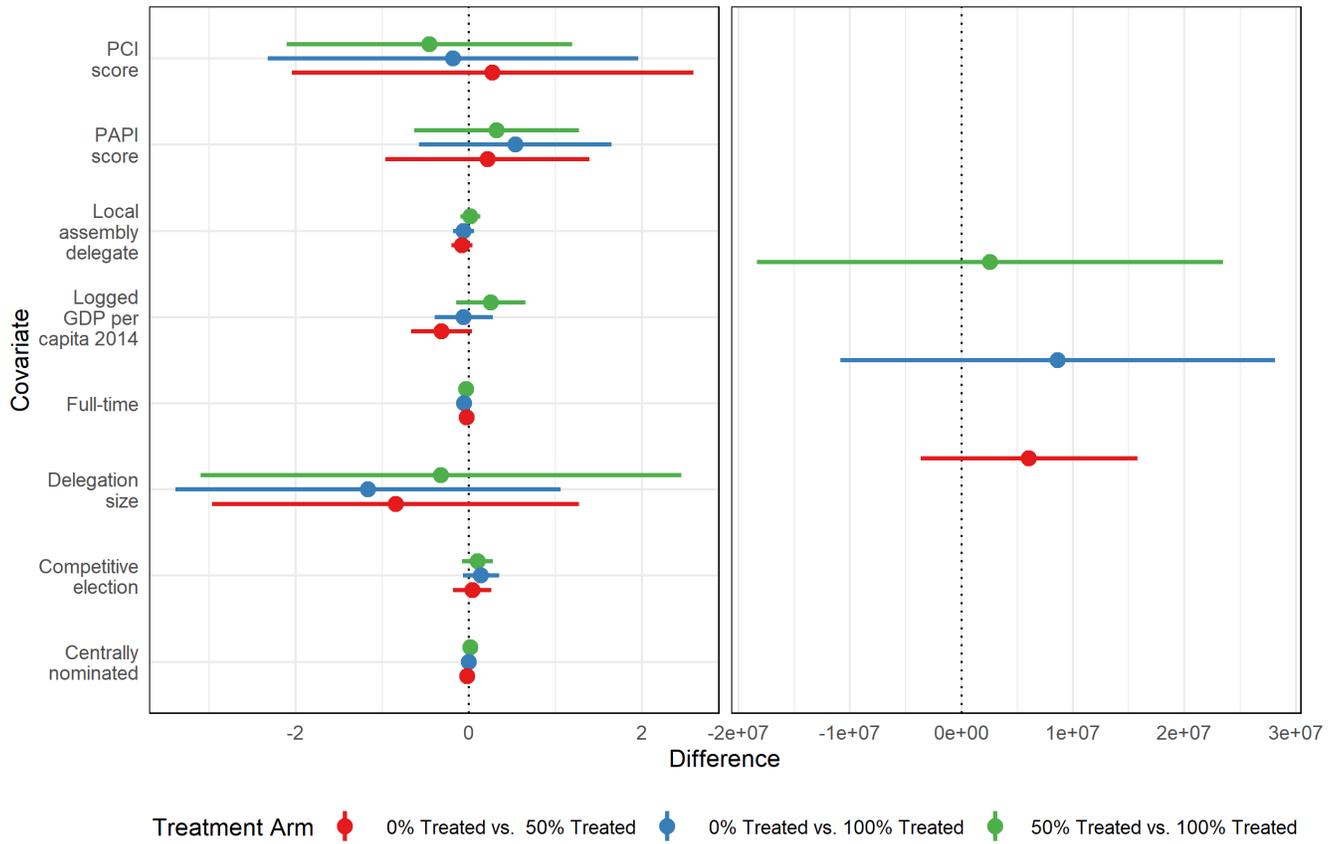


Figure 3.1: **Provincial-level matching achieved balance.** Figure displays balance across three dosages, with the right panel separated for reasons of scale.

### 3 Dosage Matching

Matching was accomplished with the R package `Matching` (Sekhon, 2011) and 11 covariates: (1) % full-time delegates, (2) % centrally-nominated delegates, (3) % competitively elected delegates, (4) % delegates serving in local assemblies, (5) delegation size, (6) 2016 PAPI score, (7) 2016 PCI score, (8) 2014 GDP per capita, and (9) 2016 fiscal transfers. Figure 3.1 demonstrates that provinces were assigned to three dosages (0%, 50%, and 100%) in a way that maintained balance across each of these covariates.

## 4 OLS Results Assessed by Randomization Inference

Tables 4.1 and 4.2 contain the primary regression results used in the paper, with statistical significance assessed by randomization inference. Tables 4.3 and 4.4 contain supplementary regressions with finer-grained content-based measures of responsiveness. The parenthetical quantity underneath each regression coefficient represents that coefficient's quantile as compared to 10,000 coefficients estimated from repeated randomizations. For example, a parenthetical value of 0.999 means that our experimental estimate exceeds 99.9% of the coefficients resulting from re-randomized treatment assignments. The auxiliary statistics in the bottom rows refer to regressions on the observed data.

Table 4.1: Citizen-treated delegates more prepared for debate (Paper Figure 5; Figures 8.1; 8.2).

	All delegates			No central nominees	
	(1) Baseline	(2) Covariates	(3) Saturation	(4) Covariates	(5) Saturation
Citizen	0.228 (0.999)	0.233 (0.999)	0.148 (0.770)	0.224 (0.996)	0.008 (0.510)
% Citizen			-0.343 (0.090)		-0.428 (0.055)
Citizen $\times$ % Citizen			0.441 (0.868)		0.810 (0.967)
Firm	0.094 (0.901)	0.091 (0.890)	0.113 (0.709)	0.075 (0.800)	0.102 (0.677)
% Firm			0.001 (0.508)		-0.072 (0.388)
Firm $\times$ % Firm			0.115 (0.629)		0.196 (0.681)
Full-time		-0.101 (0.402)	-0.12 (0.020)	-0.101 (0.372)	-0.134 (0.003)
Central nominee		0.059 (0.152)	0.074 (0.725)		
Competitive		-0.086 (0.046)	-0.093 (0.062)	-0.064 (0.031)	-0.069 (0.069)
Constant	0.481 (0.007)	0.551 (0.011)	0.598 (0.219)	0.547 (0.038)	0.615 (0.444)
Observations	293	293	293	250	250

Randomization inference based on 10,000 randomizations. Quantile of experimental estimate in parentheses.

Table 4.2: Citizen-treated delegates more likely to speak (Paper Figure 6; Figure 8.3).

Pooled	All delegates			No central nominees
	(1) Baseline	(2) Covariates	(3) Saturation	(4) Covariates
Citizen	0.102 (0.958)	0.105 (0.965)	0.084 (0.743)	0.135 (0.985)
% Citizen			-0.268 (0.042)	
Citizen $\times$ % Citizen			0.231 (0.774)	
Firm	0.050 (0.778)	0.055 (0.804)	0.245 (0.966)	0.058 (0.799)
% Firm			0.023 (0.531)	
Firm $\times$ % Firm			-0.283 (0.131)	
Full-time		0.132 (0.830)	0.129 (0.528)	0.133 (0.882)
Central nominee		0.030 (0.121)	0.032 (0.366)	
Competitive		0.087 (0.847)	0.085 (0.506)	0.080 (0.448)
Constant	0.409 (0.065)	0.310 (0.043)	0.340 (0.373)	0.303 (0.033)
Observations	470	470	470	375

Randomization inference based on 10,000 randomizations. Quantile of experimental estimate in parentheses.

Table 4.3: Delegate mentioned province.

Transcripts:	Pooled		Query	Caucus
	(1) Baseline	(2) Covariates	(3) Covariates	(4) Covariates
% Citizen	0.525 (0.955)	0.581 (0.968)	0.022 (0.502)	0.581 (0.984)
% Firm	0.007 (0.374)	-0.06 (0.268)	-0.045 (0.298)	-0.061 (0.300)
% Full-time		-2.295 (0.760)	0.164 (0.416)	-2.948 (0.771)
% Central nominee		2.309 (0.341)	-0.205 (0.627)	2.992 (0.351)
% Competitive		0.141 (0.991)	0.150 (0.605)	0.034 (0.994)
Constant	0.155 (0.104)	0.428 (0.041)	-0.036 (0.668)	0.541 (0.024)
Observations	63	63	63	63

Randomization inference based on 10,000 randomizations. Quantile of experimental estimate in parentheses. The unit of analysis is the province.

Table 4.4: Speech content analyses.

	(1) Citizens	(2) Firms	(3) Art. 27–29	(4) Art. 31	(5) Art. 70	(6) Art. 105
% Citizen	-0.296 (0.060)	-0.593 (0.026)	0.015 (0.388)	0.063 (0.674)	0.047 (0.415)	0.127 (0.677)
% Firm	0.260 (0.838)	0.299 (0.898)	0.228 (0.706)	0.121 (0.827)	0.176 (0.632)	-0.139 (0.279)
% Full-time	-0.754 (0.786)	0.827 (0.690)	-1.985 (0.828)	0.342 (0.898)	0.574 (0.813)	-0.957 (0.303)
% Central nominee	0.993 (0.116)	-0.907 (0.166)	2.287 (0.124)	-0.496 (0.078)	-1.66 (0.143)	0.697 (0.762)
% Competitive	0.061 (0.052)	-0.129 (0.014)	-0.175 (0.660)	-0.009 (0.888)	-0.121 (0.715)	0.197 (0.740)
Constant	0.856 (0.820)	0.669 (0.875)	0.884 (0.328)	-0.038 (0.060)	0.388 (0.366)	0.387 (0.498)
Observations	63	63	63	63	63	63

Randomization inference based on 10,000 randomizations. Quantile of experimental estimate in parentheses. The unit of analysis is the province.

Table 4.3 presents provincial-level regression results in which the dependent variable is an indicator equaling 1 when any member of a provincial delegation mentions her own province, and the baseline specification includes only the share of delegates receiving each treatment. Pooling the query session and group caucus transcripts, Column 1 displays a strong effect of the citizen treatment, with an increase in saturation from 0% to the observed maximum of 71% yielding an increase of 37 percentage points ( $0.525 \times 0.71$ ). Yet again, we see no evidence for a treatment effect of the firm infographics. Adding covariates (Column 2), included here as provincial shares, only strengthens the effect of the citizen treatment. Although somewhat unexpected given the absence of

floor debate mentions, the marginal effect of increasing the treatment share from 0% to the observed maximum treatment share is approximately 50 percentage points – a considerable effect. Column 3 reveals no effect on the query sessions – understandable given the diffuse focus of these events – while Column 4 presents estimates nearly identical to those of the pooled transcripts. This indicates that the effects in the pooled corpus are driven by delegation behavior in the group caucuses.

A final set of provincial-level results, presented in Table 4.4, examine the floor debate and group caucus transcripts for finer-grained indicators of responsiveness. The first two columns look at terms synonymous (or nearly so) with citizens or firms, and the remaining columns examine particular articles of relevance in the Education Law itself: general education, vocational education, teachers, and school fees. With but one exception, all specifications yield null results. Increased provincial saturation of the citizen treatment significantly reduced the likelihood that a delegation member would mention firms, yet failed to increase the probability of mentioning citizens.

## **5 OLS Results, Province-clustered Standard Errors**

The coefficients presented in Tables 5.1 and 5.2 are identical to those reported the published Appendix; the tables differ in that the parenthetical quantity appearing beneath each coefficient is the more familiar standard error, here clustered on provinces. The only other difference lies in the addition to Table 5.1 of two columns (4 and 8) which replicate analyses (3 and 7) over 100 multiply-imputed datasets.

Table 5.1: Debate preparation. Forms the basis of Figures 5, 8, and 9.

	All delegates				No central nominees			
	(1) Baseline	(2) Covariates	(3) Saturation	(4) MI <sup>a</sup>	(5) Baseline	(6) Covariates	(7) Saturation	(8) MI <sup>a</sup>
Citizen	0.228 (0.070)	0.233 (0.069)	0.148 (0.148)	0.136 (0.149)	0.219 (0.079)	0.224 (0.079)	0.008 (0.168)	0.047 (0.172)
% Citizen			-0.343 (0.249)	-0.272 (0.237)			-0.428 (0.265)	-0.339 (0.248)
Citizen × % Citizen			0.441 (0.350)	0.340 (0.338)			0.810 (0.396)	0.597 (0.390)
Firm	0.094 (0.063)	0.091 (0.062)	0.113 (0.128)	0.116 (0.139)	0.081 (0.073)	0.075 (0.072)	0.102 (0.143)	0.094 (0.153)
% Firm			0.001 (0.230)	0.022 (0.200)			-0.072 (0.266)	-0.012 (0.245)
Firm × % Firm			0.115 (0.321)	0.040 (0.301)			0.196 (0.366)	0.121 (0.355)
Full-time		-0.101 (0.074)	-0.120 (0.074)	-0.091 (0.075)		-0.101 (0.075)	-0.134 (0.073)	-0.105 (0.074)
Central Nominee		0.059 (0.093)	0.074 (0.095)	0.047 (0.095)				
Competitive		-0.086 (0.058)	-0.093 (0.057)	-0.074 (0.054)		-0.064 (0.063)	-0.069 (0.061)	-0.060 (0.061)
Constant	0.481 (0.049)	0.551 (0.059)	0.598 (0.068)	0.575 (0.066)	0.489 (0.056)	0.547 (0.063)	0.615 (0.068)	0.585 (0.069)
Observations	293	293	293	470	250	250	250	470
R <sup>2</sup>	0.035	0.048	0.059	0.053	0.032	0.043	0.066	0.056
RMSE	0.485	0.482	0.479	0.231	0.486	0.483	0.477	0.230

*Note:* Province-clustered standard errors in parentheses. <sup>a</sup>Replicates the preceding column over 100 multiply-imputed datasets.

Table 5.2: Delegate spoke. Forms the basis of Figures 6, 7, and 10.

Transcripts:	Pooled			Floor	Query	Caucus	Pooled (no central nominees)	
	(1) Baseline	(2) Covariates	(3) Saturation	(4) Saturation	(5) Saturation	(6) Saturation	(7) Covariates	(8) Saturation
Citizen	0.102 (0.062)	0.105 (0.063)	0.084 (0.135)	-0.105 (0.045)	0.158 (0.101)	0.073 (0.133)	0.135 (0.065)	0.229 (0.138)
% Citizen			-0.268 (0.204)	-0.164 (0.075)	0.004 (0.125)	-0.192 (0.177)		-0.232 (0.217)
Citizen $\times$ % Citizen			0.231 (0.302)	0.392 (0.128)	-0.222 (0.235)	0.196 (0.270)		-0.045 (0.295)
Firm	0.050 (0.067)	0.055 (0.067)	0.245 (0.160)	0.072 (0.074)	0.020 (0.109)	0.235 (0.129)	0.058 (0.072)	0.230 (0.179)
% Firm			0.023 (0.156)	-0.053 (0.050)	0.043 (0.124)	-0.064 (0.148)		0.022 (0.183)
Firm $\times$ % Firm			-0.283 (0.233)	-0.053 (0.116)	-0.051 (0.215)	-0.163 (0.216)		-0.264 (0.283)
FullTime		0.132 (0.068)	0.129 (0.068)	0.007 (0.039)	-0.012 (0.047)	0.140 (0.067)	0.133 (0.067)	0.137 (0.067)
CentNom		0.030 (0.077)	0.032 (0.075)	-0.016 (0.042)	0.046 (0.062)	0.016 (0.073)		
Competitive		0.087 (0.051)	0.085 (0.051)	0.050 (0.020)	0.073 (0.026)	0.034 (0.047)	0.080 (0.057)	0.076 (0.057)
Constant	0.409 (0.044)	0.310 (0.058)	0.340 (0.065)	0.059 (0.019)	0.071 (0.028)	0.291 (0.059)	0.303 (0.059)	0.329 (0.069)
Observations	470	470	470	470	470	470	375	375
R <sup>2</sup>	0.007	0.033	0.040	0.033	0.023	0.034	0.029	0.037
RMSE	0.496	0.490	0.488	0.229	0.348	0.478	0.488	0.486

*Note:* Province-clustered standard errors in parentheses.

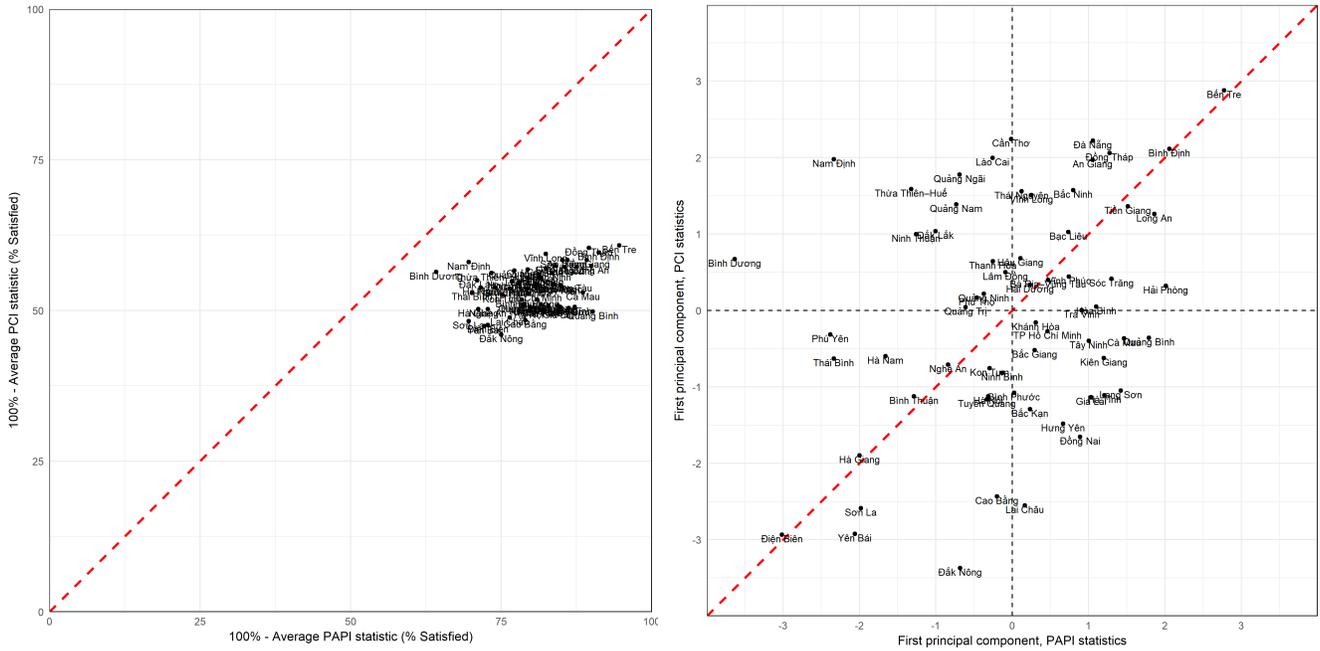


Figure 6.1: **Provincial infographic statistics, raw averages and PCA first components.** Simply averaging over the percentages in each infographic reveals that there is little variation in treatment strength. The overall pattern remains the same, but the purported variation is increased mechanically via PCA.

## 6 Treatment Strength: Heterogeneity by Province

We conducted principal components analysis (PCA) of the provincial-level infographic statistics for each of the two treatments – citizens (from PAPI) and firms (from PCI). Before presenting the PCA results, it is important to put them into context by visualizing the variation in scores on their natural scale. The left panel of Figure 6.1 does this, and clearly the variation is minimal. Here larger numbers imply more satisfied constituents. This, of course, implies that the strength of a particular treatment is approximately equivalent across provinces. Keeping this in mind, we turn to the right panel. Here are plotted all 63 provinces according to their scores on the first normalized principal component. As before, larger numbers imply a more satisfied constituency. Interacting these scores with their respective treatments, we run a regression of the form

$$\Pr(Y_i = 1) = \beta_0 + \beta_1 \text{Cit}_i + \beta_2 \text{PAPI}_p + \beta_3 \text{Cit}_i \text{PAPI}_p + \beta_4 \% \text{Cit}_p + \beta_5 \text{Firm}_i + \beta_6 \text{PCI}_p + \beta_7 \text{Firm}_i \text{PCI}_p + \beta_8 \% \text{Firm}_p + \gamma \mathbf{X}_i + \epsilon_i$$

where  $\mathbf{X}$  includes individual covariates, we obtain the following results displayed in Table 6.1.

Here Column 1 modifies Tables 4.1 and 5.1, Column 3. Interacting treatment assignments with PAPI and PCI scores – essentially inverse treatment strength – yields somewhat divergent results from those obtained with the saturation interaction. Specifically, this interaction implies that citizen-treated delegates in the provinces with the highest observed PAPI scores (the most satisfied constituents) were as much as 35 percentage points more likely to make up their minds than their firm-treated or untreated peers in the same provinces. In low-PAPI provinces, where the treatment is strongest, the marginal effect of citizen infographics is marginally weaker – estimated to equal around 31 percentage points (Column 1). With a slightly positive slope, the effect is that the less satisfied is a delegate’s provincial citizenry, the lower is the likelihood she feels prepared to debate the education bill. The negative coefficient on PAPI, however, implies spillover among those not receiving the citizen treatment: lowering citizen satisfaction from its observed maximum to its observed minimum is associated with a 32 percentage point increase in the probability of debate preparation among firm-treated and untreated delegates. Column 1 also reveals a counterintuitively significant, positive, and increasing marginal effect of the firm treatment as firm satisfaction increases from average levels to higher levels. In other words, firm-treated delegates were more likely to feel prepared for debate in provinces with weaker treatments.

Columns 2-5 modify Tables 4.2 and 5.2, Columns 3-6 by similarly replacing the saturation interactions with (reversed) treatment strength interactions. The first two columns examine speaking proclivities in the pooled transcripts and on the floor. As expected, increased satisfaction on the part of citizens (PAPI) weakens the treatment effect, with clear negative slopes to the marginal effects. Consistent with the saturation effects addressed in the main paper, the effect of the citizen treatment is significant, both statistically and substantively. In fact, a citizen-treated delegate in the province with the lowest PAPI score (the least satisfied constituents) was approximately 50 percentage points likelier to speak than her firm-treated or untreated peers. Comparing this panel to the lower-right panel, addressing floor debates, and comparing Columns 2 through 5, it is clear that this effect is driven not by floor debates but by group caucus sessions.

Table 6.2 modifies the paper’s provincial-level analyses by supplementing treatment saturation with PAPI and PCI controls. As the top two rows make clear, treatment strength has no effect on

any speech content outcomes, with one exception. As a province's PAPI score (citizen satisfaction) increases, it becomes marginally more likely that a member of its VNA delegation mentions Article 70, which concerns teacher quality. Turning to treatment saturations, the probability that some member of a provincial delegation mentions her own province is substantially raised when the provincial share of citizen-treated is increased. The coefficients here, 0.593 for pooled and 0.594 for caucus transcripts, are actually somewhat higher than those found in the paper specifications.

Table 6.1: **PCA-treatment interactions.** Multiplicative interactions between 1) PAPI scores and citizen treatment; and 2) PCI scores and firm treatment.

DV:	<i>Delegate prepared</i>		<i>Delegate spoke</i>		
Data:	(1) Survey	(2) Pooled	(3) Floor	(4) Query	(5) Caucus
Citizen	0.313 (0.077)	0.193 (0.098)	0.038 (0.028)	0.092 (0.066)	0.162 (0.084)
PAPI	-0.050 (0.030)	0.048 (0.024)	0.009 (0.009)	0.020 (0.013)	0.047 (0.024)
Citizen × PAPI	0.008 (0.038)	-0.075 (0.039)	-0.023 (0.012)	-0.046 (0.026)	-0.080 (0.036)
% Citizen	-0.297 (0.184)	-0.150 (0.153)	-0.032 (0.054)	-0.071 (0.105)	-0.107 (0.139)
Firm	0.130 (0.069)	0.154 (0.113)	0.035 (0.036)	0.033 (0.072)	0.195 (0.089)
PCI	0.026 (0.028)	0.032 (0.018)	0.012 (0.008)	0.035 (0.011)	0.006 (0.016)
Firm × PCI	0.092 (0.033)	-0.066 (0.034)	-0.030 (0.014)	-0.038 (0.024)	-0.037 (0.029)
% Firm	0.015 (0.164)	-0.093 (0.150)	-0.089 (0.053)	0.030 (0.088)	-0.144 (0.132)
FullTime	-0.085 (0.073)	0.124 (0.068)	0.014 (0.038)	-0.021 (0.047)	0.135 (0.068)
CentNom	0.026 (0.095)	0.039 (0.076)	-0.022 (0.042)	0.056 (0.060)	0.023 (0.073)
Competitive	-0.094 (0.056)	0.061 (0.051)	0.043 (0.021)	0.057 (0.026)	0.020 (0.048)
Constant	0.595 (0.066)	0.329 (0.062)	0.045 (0.019)	0.079 (0.027)	0.278 (0.060)
N	293	470	470	470	470
R <sup>2</sup>	0.097	0.056	0.027	0.041	0.046
RMSE	0.479	0.490	0.232	0.349	0.481

*Note:* Province-clustered standard errors in parentheses.

DV:	Mentioned own province			Citizens	Firms	Art. 27-29	Art. 31	Art. 70	Art. 105
Data:	(1) Pooled	(2) Query	(3) Caucus	(4) Pooled <sup>a</sup>	(5) Pooled <sup>a</sup>	(6) Pooled <sup>a</sup>	(7) Pooled <sup>a</sup>	(8) Pooled <sup>a</sup>	(9) Pooled <sup>a</sup>
PAPI	0.012 (0.050)	-0.005 (0.024)	0.014 (0.047)	0.025 (0.043)	0.075 (0.052)	-0.059 (0.053)	-0.020 (0.020)	0.088 (0.046)	-0.00002 (0.051)
PCI	-0.012 (0.044)	-0.032 (0.021)	0.002 (0.042)	-0.050 (0.039)	0.014 (0.047)	0.066 (0.047)	-0.002 (0.018)	0.045 (0.041)	-0.042 (0.045)
% Citizen	0.593 (0.249)	0.017 (0.118)	0.594 (0.236)	-0.273 (0.217)	-0.522 (0.263)	-0.041 (0.264)	0.044 (0.098)	0.130 (0.229)	0.127 (0.254)
% Firm	-0.056 (0.268)	-0.071 (0.127)	-0.046 (0.254)	0.251 (0.234)	0.383 (0.284)	0.214 (0.285)	0.100 (0.106)	0.294 (0.247)	-0.167 (0.274)
% FullTime	-2.369 (1.290)	0.017 (0.612)	-2.956 (1.222)	-1.030 (1.125)	0.786 (1.365)	-1.580 (1.369)	0.359 (0.510)	0.666 (1.189)	-1.160 (1.319)
% CentNom	2.382 (1.429)	-0.042 (0.678)	2.991 (1.354)	1.277 (1.246)	-0.916 (1.512)	1.888 (1.516)	-0.500 (0.565)	-1.823 (1.316)	0.919 (1.460)
% Competitive	0.145 (0.215)	0.203 (0.102)	0.015 (0.203)	0.108 (0.187)	-0.236 (0.227)	-0.206 (0.228)	0.017 (0.085)	-0.289 (0.198)	0.260 (0.219)
Constant	0.433 (0.289)	-0.034 (0.137)	0.545 (0.274)	0.869 (0.252)	0.693 (0.306)	0.859 (0.307)	-0.044 (0.114)	0.412 (0.267)	0.391 (0.296)
N	63	63	63	63	63	63	63	63	63
R <sup>2</sup>	0.156	0.092	0.195	0.097	0.132	0.111	0.071	0.181	0.064
RMSE	0.458	0.217	0.434	0.399	0.484	0.486	0.181	0.422	0.468

*Note:* <sup>a</sup>Transcripts from floor debates and group caucuses only. The unit of analysis is the province.

Table 6.2: **Analyzing speech content with PCA.** PAPI and PCI scores have no effect on delegation-level speaking behavior.

## 7 Saturation Results by Original Treatment Conditions

Here we examine treatment effects by provincial-level saturation and individual treatment assignment. We do so by examining raw outcomes across six bins: (1) untreated delegates in untreated provinces, (2) untreated in 50% treated provinces, (3) citizen-treated delegates in 50% treated provinces, (4) citizen-treated delegates in fully treated provinces, (5) firm-treated delegates in 50% treated provinces, and (6) firm-treated delegates in fully treated provinces. Table 7.1 (page 18) displays the results when we regress the debate preparation and speaking outcomes on these simple dummy variables with intercepts suppressed.

Consistent with our primary results using continuous treatment shares, we find that the citizen treatment increases responsiveness relative to untreated delegates in 50% treated provinces, regardless of how responsiveness is measured. Also consistent with earlier results, as we increase a province's treatment saturation from 50% to 100%, we find that citizen-treated delegates are more prepared for debate. We do not, however, observe a similarly-signed difference on the speaking outcome. Because provincial treatment saturations include both the citizen and firm treatments simultaneously, increasing the saturation does not necessarily correspond to increasing a province's share of citizen-treated delegates. We find no evidence of reinforcement effects of the firm treatment across either outcome. Because control delegates do not exhibit increased responsiveness in the 50% treated provinces relative to the untreated provinces, we also find no evidence of spillover effects.

We also conducted formal F-tests for the equality of coefficients to assess direct (Citizen-50% vs. Control-50%; Firm-50% vs. Control-50%), reinforcement (Citizen-100% vs. Citizen-50%; Firm-100% vs. Firm-50%), and spillover (Control-50% vs. Control-0%) effects. As Table 7.2 (page 19) shows, the only statistically significant differences were those between citizen-treated and untreated delegates in 50% treated provinces.

Table 7.1: Examining results by provincial saturation.

	<i>Dependent variable:</i>	
	Prepared	Spoke
Control (0% Saturation)	0.460 (0.324, 0.596)	0.425 (0.320, 0.530)
Control (50% Saturation)	0.500 (0.374, 0.626)	0.394 (0.292, 0.495)
Citizen (50% Saturation)	0.694 (0.557, 0.831)	0.536 (0.429, 0.643)
Citizen (100% Saturation)	0.730 (0.572, 0.888)	0.475 (0.347, 0.602)
Firm (50% Saturation)	0.636 (0.507, 0.766)	0.461 (0.348, 0.573)
Firm (100% Saturation)	0.500 (0.355, 0.645)	0.457 (0.340, 0.574)
N	293	470
R <sup>2</sup>	0.598	0.460
RMSE	0.488	0.499
F-Statistic	71.133	65.880

*Note:* Linear regressions with suppressed intercepts. Coefficient 95% confidence intervals displayed in parentheses.

Table 7.2: Spillover and reinforcement effects not reflected in saturation analysis.

	Prepared		Spoke	
	F-stat	p-value	F-stat	p-value
Direct (50% Citizen vs. 50% Control)	4.192	0.042	3.597	0.059
Direct (50% Firm vs. 50% Control)	2.204	0.139	0.755	0.385
Spillover (100% Control vs. 50% Control)	0.180	0.671	0.182	0.670
Reinforcement (100% Citizen vs. 50% Citizen)	0.114	0.736	0.520	0.471
Reinforcement (100% Firm vs. 50% Firm)	1.909	0.168	0.002	0.967

*Note:* F-tests for equality of coefficients from Table 7.1.

## 8 Saturation Results by Realized Treatment Share

The results in the main text paint a picture of delegates responsive only when presented with the preferences of their local citizens—not when presented with those of their provincial business community. These results, however, ignore the anticipated effects of caucus-induced spillover. By gathering in one or more provincial delegations at the outset of the legislative session, individual delegates have an opportunity to exchange debate-relevant information. Importantly, this includes the provincial-level infographics we provided to randomly selected delegates, and we therefore address the possibility of spillover with a saturation design. By interacting provincial treatment shares with individual treatment assignments, we assume that the greater the proportion of citizen- (firm-) treated delegates in a province, the higher is the probability that information from citizens (firms) will be shared. The quantities of interest here are the conditional effect on control delegates of increasing the provincial share of treated delegates ( $H3$ ) and the conditional effect on treated delegates of increasing the provincial share of similarly-treated delegates ( $H4$ ).

***H3 - Spillover:*** *Control delegates exposed to treated delegates will learn from them and increase*

*responsiveness.*

**H4 - Reinforcement:** *Treated delegates exposed to similarly treated delegates will feel more confident about their information and increase responsiveness.*<sup>1</sup>

To test these claims, we regress a dichotomous indicator for preparedness on three delegate-level covariates, individual treatment assignments, provincial shares of treated delegates, and interactions between treatment assignments and treated shares. We run a linear probability model with the following specification, where  $i$  indexes delegates and  $p$  provinces:

$$\Pr(Y_i = 1) = \beta_0 + \beta_1 \text{Cit}_i + \beta_2 \% \text{Cit}_p + \beta_3 \text{Cit}_i \% \text{Cit}_p + \beta_4 \text{Firm}_i + \beta_5 \% \text{Firm}_p + \beta_6 \text{Firm}_i \% \text{Firm}_p + \gamma \mathbf{X}_i + \epsilon_i$$

where  $\mathbf{X}$  denotes indicators for full-time, central nomination, and competitive elections, which both theory and prior work suggest may influence responsiveness.  $\beta_0$  is the constant, representing the share of untreated delegates in untreated provinces answering that they are prepared for debate.  $\beta_1$  ( $\beta_4$ ) is the marginal change (i.e. the shift in the intercept) due to the citizen (firm) treatment when the provincial share of delegates receiving that treatment is zero. Similarly,  $\beta_2$  ( $\beta_5$ ) represents the marginal effect of increasing the share of citizen- (firm-) treated delegates from 0% to 100% for delegates who did not receive the citizen (firm) treatment. These treatments are indexed by  $p$  because they do not differ among delegates from the same province. A positive coefficient here would signal spillover, in which an untreated delegate grows increasingly likely to feel prepared for debate as the share of treated peers rises. Lastly, positive estimates for  $\beta_3$  ( $\beta_6$ ) would indicate a reinforcement effect, meaning that a rising proportion of similarly treated peers increases the odds that a treated delegate is prepared.

The full results from the regression on preparation to speak and act of speaking can be found above in online Appendices 4 (Table 4.1 Model 3, page 5 and Table 4.2 Model 3, page 6). The first thing to notice is that the coefficients tracking spillover from treatment to control for both citizens  $\beta_2$  and firms  $\beta_5$  are not-significant and even marginally negative in some cases. This indicates that we do not find evidence consistent with spillover from treatment to control (H3). By contrast, we do find significant effects on the interactions terms for citizens  $\beta_3$  and firms  $\beta_6$ . This provides a tentative hint that is consistent with the reinforcement hypothesis (H4).

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<sup>1</sup>H4 was not pre-specified in our PAP and should be treated with greater caution than the others.

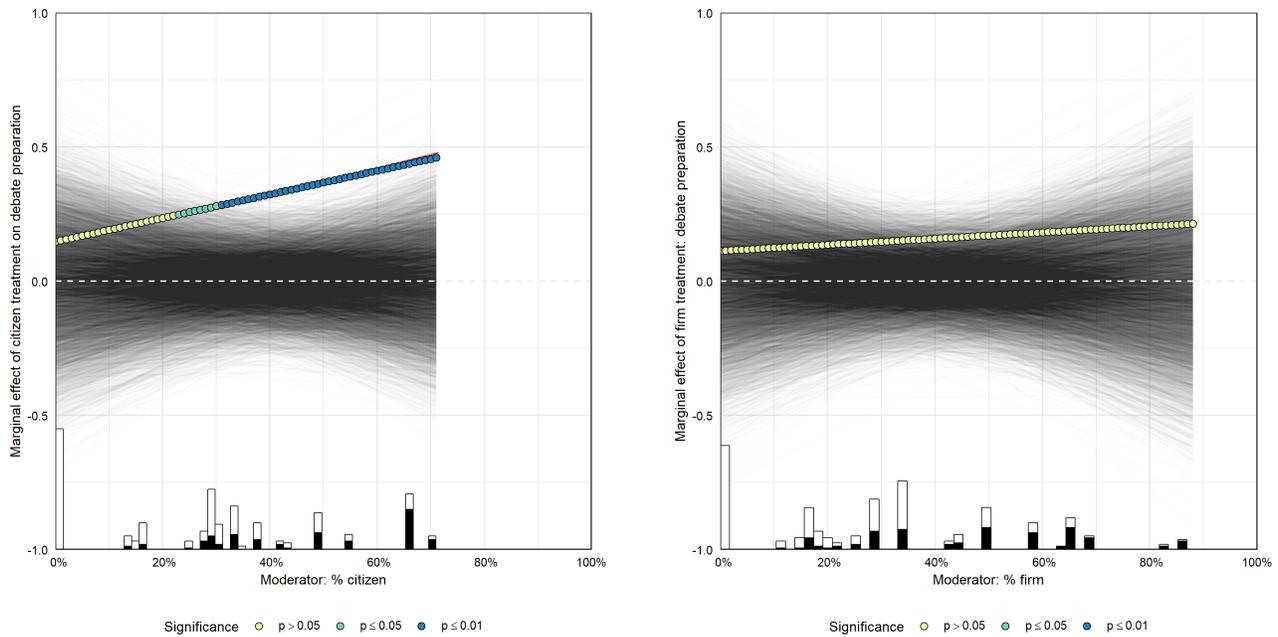


Figure 8.1: **Increasing the share of citizen-treated delegates reinforces the effect of that treatment on debate preparation.** Figure displays marginal treatment effects (colored circles) over 10,000 regression lines from randomization inference. Obscured red line (under circles) plots kernel estimator verifying LIE assumption, and histogram at bottom illustrates support on the moderator.

We explore H4 more directly by plotting the marginal effects of the experiment over the level of saturation. Essentially, we are studying the average treatment effect at different levels of saturation. The left panel of Figure 8.1 displays the marginal effects of the citizen treatment over the observed range of the moderator—the provincial proportion of citizen-treated delegates—with the right panel presenting the equivalent result for the firm treatment. Our experimental estimate is depicted by a line of colored circles, each colored to denote the randomization inference-based significance of the marginal effect. Underneath our estimate—and almost entirely obscured—lies a red curve produced by a kernel estimator (Hainmueller, Mummolo and Xu, 2018). The advantage of this second approach, which flexibly estimates the marginal effect of the citizen treatment at 50 points evenly distributed across the observed range of the %Citizen moderator, is that it permits verification of the assumed linear interaction effect (LIE). The total eclipse of the kernel estimator

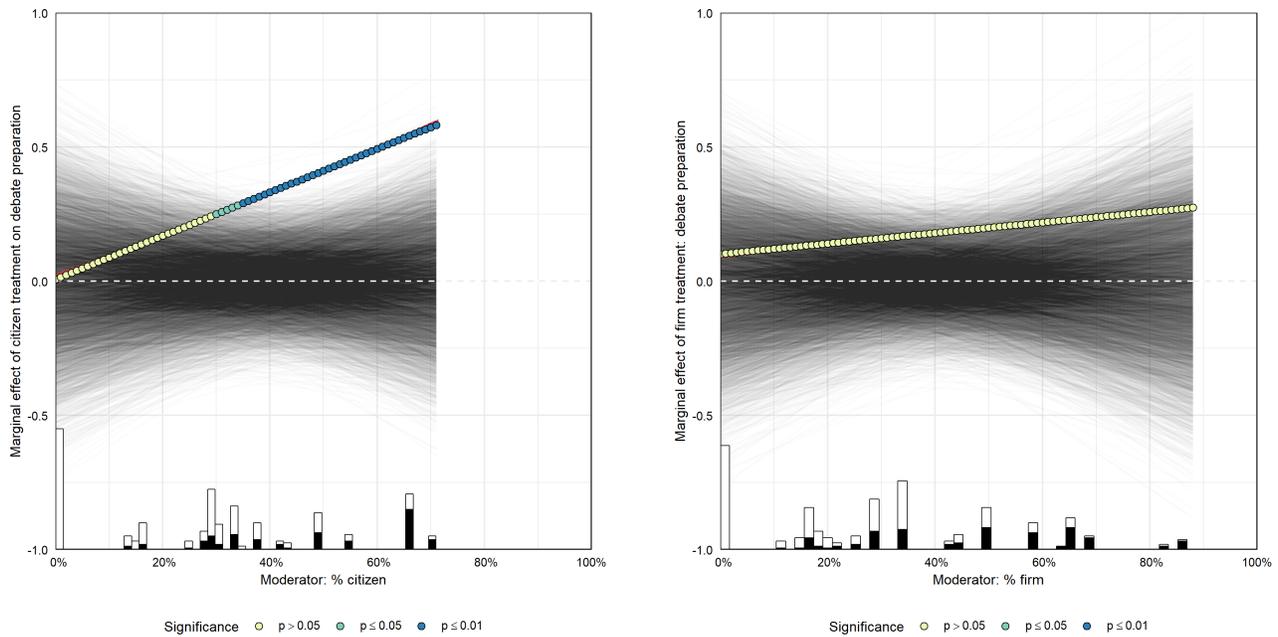


Figure 8.2: **Reinforcement effect of the citizen treatment on debate preparation is stronger among local nominees.** Figure displays marginal treatment effects (colored circles) over 10,000 regression lines from randomization inference. Obscured red line (under circles) plots kernel estimator verifying LIE assumption, and histogram at bottom illustrates support on the moderator.

by our experimental estimate indicates that the LIE assumption is clearly met.<sup>2</sup> Underneath both estimates are the 10,000 partially transparent re-randomized marginal effects, from which we derive the statistical significance of our estimate. At the foot of the panel is a histogram illustrating common support on the moderator (Hainmueller, Mummolo and Xu, 2018). Although common support fails, of necessity, at 0 (the pure control)% saturation, it is otherwise satisfactory.

Substantively, the left panel of Figure 8.1 implies that citizen-treated delegates in the provinces at the highest observed levels of citizen dosage were nearly 50 percentage points likelier to feel prepared for debate than their firm-treated or untreated peers in the same provinces. Even at low dosages, the marginal effect of the citizen treatment is estimated to equal 10-20 percentage points. The positive slope of this marginal effect is indicative of a reinforcement effect: the greater the

<sup>2</sup>The optimal bandwidth selected (using cross-validation) for each figure is full support, meaning that there is always a mix of treated and untreated delegates.

share of citizen-treated delegates in a province, the higher is the likelihood that each feels prepared to debate the VLOE. Regression results presented in Appendix Table 4.1 reveal a negative yet insignificant coefficient on %Citizen, suggesting that spillover among those not receiving the citizen treatment is minimal.

The right panel of Figure 8.1 displays the marginal effects of the firm treatment. Similar to the citizen treatment, the marginal treatment effect is positive and increasing at all dosage levels. This firm effect, however, is substantially smaller and statistically indistinguishable from zero or the citizen effect. Although the LIE assumption holds once again, increasing the firm dosage from 0% to its observed maximum of 88% raises the probability that a firm-treated delegate was prepared for debate by only ten percentage points. At their highest observed treatment shares, the marginal effect of the firm treatment (0.25) is approximately half that of the citizen treatment (0.5) and is statistically indistinguishable from zero. Thus, while we definitively find greater responsiveness to citizens, it cannot be said with confidence that the firm treatment sparks responsiveness. These findings illustrate that delegates exhibit greater responsiveness to citizens than to firms, and that only the citizen treatment induces responsiveness.

Figure 8.2 replicates the analysis of Figure 8.1 on a subset of the data, dropping all central nominees as those least likely to be responsive to local constituencies. These models are substantively similar, but demonstrate a much larger reinforcement effect, particularly for the citizen treatment—the marginal effect is 0.58 at the maximum observed treatment share. This indicates that the reinforcement effect is much more important for the less elite and politically connected local- and self-nominated delegates. As noted in Appendix Table 4.1, the negative spillover effect of the citizen dosage on those not receiving the citizen treatment remains, and is in fact strengthened. As a further robustness check, online Appendix 5 (Table 5.1, page 10) also reports the results of these interactive specifications as applied to 100 multiply-imputed datasets and combined according to the rules laid out in Rubin (2004). These estimates are substantively similar to those obtained via listwise deletion.

Returning to the transcripts from three legislative fora, we begin with the pooled speaking behavior. Figure 8.3 presents the marginal treatment effects on the likelihood of a delegate speaking

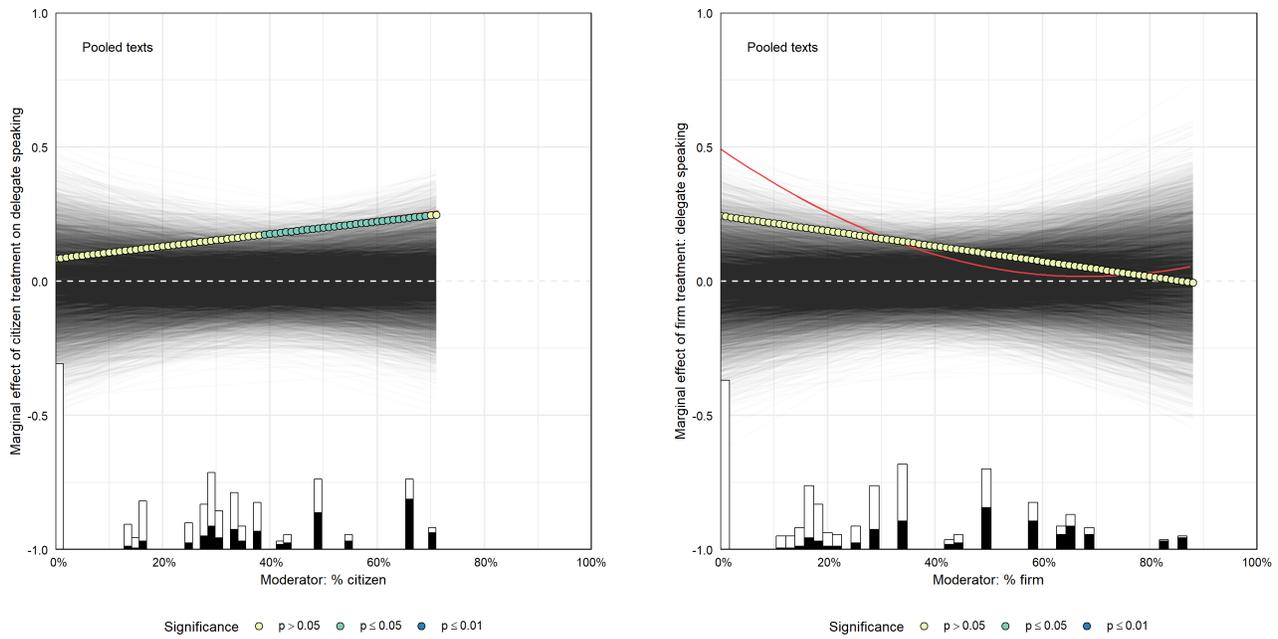


Figure 8.3: **Increasing the share of citizen-treated delegates reinforces the effect of that treatment on speech.** Figure displays marginal treatment effects (colored circles) over 10,000 regression lines from randomization inference. Partially obscured red line (under circles) plots kernel estimator checking LIE assumption, and histogram at bottom illustrates support on the moderator.

in any forum, revealing a number of things. First, the marginal effect of the firm infographic (right panel) is positive at low treatment shares, decreases as the share is increased, and is effectively zero at the observed maximum share.<sup>3</sup> Turning to the citizen treatment, we find positive and linearly increasing marginal effects of the citizen treatment across all treatment shares, and weak negative spillover among the firm-treated and untreated (see online Appendix Table 4.2, page 6, and online Appendix Table 5.2, page 11). Increasing the citizen dosage to its observed maximum triples the marginal effect, from approximately 0.08 to 0.27.

Using a multiplicative interaction to model the threat of spillover, we uncover strong reinforcement effects—and little evidence of spillover—of the citizen treatment. As knowledge of citizens’

<sup>3</sup>The kernel estimator (red curve), however, indicates that the LIE assumption does not hold for the firm treatment-firm dosage interaction. Because the linear model is inappropriate, the randomization inference results are therefore inappropriate to judge the significance of the result at low dosages.

preferences becomes more widespread within a provincial delegation, its delegates exhibit greater confidence in their debate preparation and less hesitation to speak, particularly in floor debates and group caucuses. The flipside of this effect is that as a larger proportion of the delegation is presented with citizens' preferences, those left out—the firm-treated and untreated—are increasingly unsure of how debate may unfold and accordingly more reticent in their speech.

## 9 Placebo test

The docket of the May VNA Session also provides an excellent opportunity to test the results of our experimental finding against a set of placebo laws for which no experimental treatments were administered. In addition to the Education Law, the survey administered by the VNA Library included questions on debate preparation for two additional laws: the Law on Livestock and the Law on Cultivation. While infographic treatments were prepared and delivered on the topic of education, these agricultural laws were not part of the treatment and are wholly unrelated to education. To test these additional laws, we stack the delegate-level data, add a dichotomous variable equaling 1 for responses to the Education Law and 0 for responses to the placebo laws, and interact this variable with the two treatments. Modelled this way, the coefficient on Citizen measures any Hawthorne effect<sup>4</sup> of the survey question among citizen-treated delegates, while the coefficient on the interaction between Citizen and EducationLaw captures the marginal effect of the citizen treatment above and beyond any Hawthorne effect – in other words, the true treatment effect of interest. The effects of the firm treatment are measured analogously.

Interestingly, in Figure 9.1 we observe a Hawthorne effect that hovers around 14 percentage points. It is important to point out that there is no theoretical reason that this effect should differ across treatments, so it is reassuring to find it equal up to two decimal places (0.141 for the citizen treatment and 0.138 for the firm treatment). More importantly, we also capture a strong effect of the citizen treatment above and beyond this Hawthorne effect. Citizen-treated delegates were

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<sup>4</sup>A Hawthorne effect occurs when treated subjects exert extra effort or otherwise alter their behavior simply as a result of their being monitored. Here, treated delegates may have interpreted the Library's survey – in conjunction with the mailed infographics – as an indication that something was afoot.

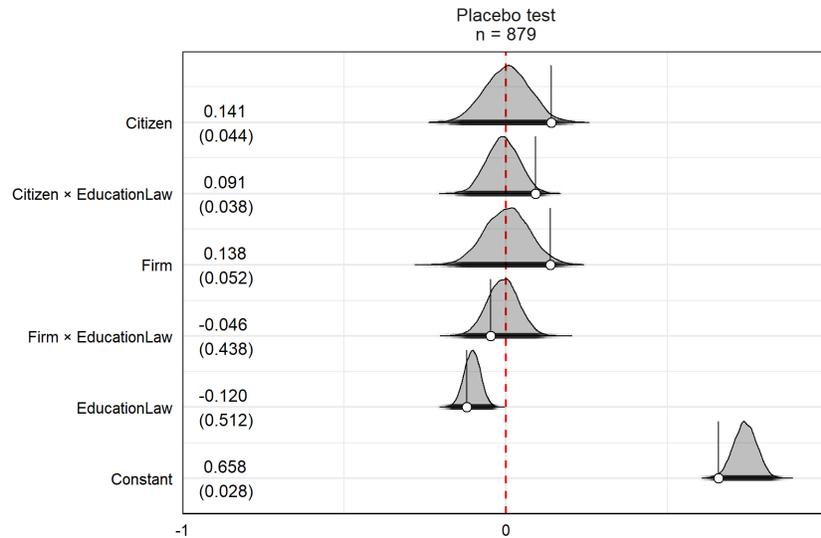


Figure 9.1: **Direct effect of citizen treatment above and beyond Hawthorne effect.** Citizen-treated delegates were nine percentage points more prepared for debate, on top of a consistently measured 14-percentage point Hawthorne effect.

nine percentage points more likely to indicate that they were prepared to debate the Education Law than were their firm-treated or untreated peers – and this effect is statistically significant at the 0.05 level across 10,000 re-randomizations. Consistent with our earlier results, we observe no similar 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.

## 10 Survey nonresponse

Figure 10.1 the relationships between missingness and both treatments and covariates.<sup>5</sup> Each panel displays the distribution of observations across a treatment or covariate (x-axis) and an indicator for survey nonresponse (y-axis). Dashed red lines result from bivariate regressions of nonresponse upon the x-axis variables. In the first two panels,  $p$ -values are the result of randomization inference. The coefficients derived from a regression of survey nonresponse on our experimental treatment

<sup>5</sup>15% of NAs are in fact not missing, but instead cannot be matched to individual delegates due to clerical errors.

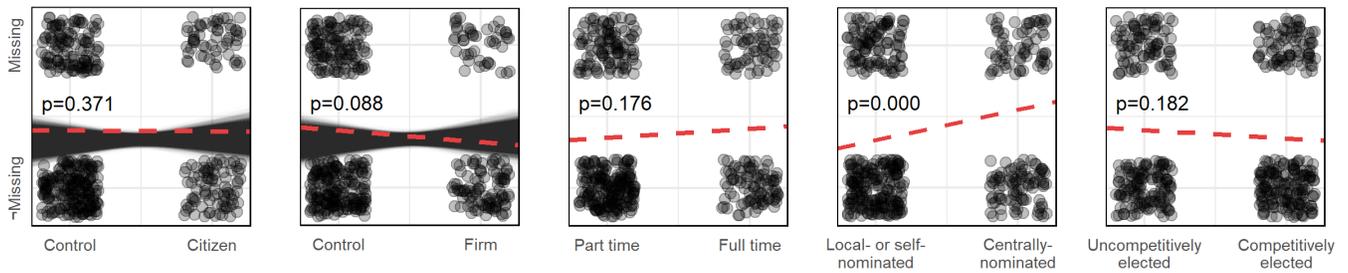


Figure 10.1: **Survey nonresponse uncorrelated with treatments.** Each panel presents the bivariate relationship between a covariate or treatment and survey nonresponse. Statistical significance in panels one and two is assessed by randomization inference.

assignment do not differ significantly from those resulting from 10,000 alternative randomizations, whose lines are plotted with high transparency.

Among the three covariate panels, only one, central nomination status, is significantly correlated with nonresponse. This is reasonable, as central nominees tend to be relatively elite politicians who are therefore harder to access. Recent work has shown that when missingness is driven by values of the independent variables, listwise deletion should not bias regression results (Arel-Bundock and Pelc, 2018). For this reason, we use listwise deletion for our primary results and consign multiple imputation-based results to a robustness check in online Appendix 5 (Table 5.1, page 10).

# 11 Examples of delegates' comments in legislative fora

## Phan Thai Binh, Quang Nam Province, Caucus #15, Citizen Treatment

[Article 77, 91] Through meetings with teacher constituents in Quang Nam province, they expressed objection that teachers may possess bachelor degree in relevant disciplines with a pedagogic training certificate. I also agreed to review the standardized educational qualifications for teachers as well as the reality and unemployment. I am of the view that pedagogical schools would best train teachers.

Qua tiếp xúc cử tri với đội ngũ giáo viên tỉnh Quảng Nam, tất cả nhà giáo đều đề nghị đối với giáo viên sư phạm trong luật có nêu là bỏ đi việc tốt nghiệp ngành khác nhưng được bồi dưỡng chuyên môn sư phạm làm giáo viên. Tôi cũng cho rằng cái này cũng cần đánh giá, khảo sát lại thực trạng và tình trạng thất nghiệp. Theo tôi để chuyên sâu về giáo dục và chất lượng giáo dục thì nên chỉ có các trường sư phạm đào tạo giáo viên là tốt nhất.

## Tran Kim Yen, Ho Chi Minh City, Caucus #2, Citizen Treatment

[Article 31] Voters are of the view that the whole learning process be assessed to decide if a pupil should get an upper secondary school diploma. It is expensive to organize an exam to determine high school graduation qualifications. I propose that upper secondary school students who meet the requirement would receive the diplomas without having to take graduation exams.

[Đ31] Ý kiến cử tri mong muốn phải xem xét quá trình học tập của các em để đánh giá việc có được cấp bằng tốt nghiệp trung học phổ thông không. Việc tổ chức một kỳ thi để xác định tốt nghiệp trung học phổ thông hiện nay rất tốn kém. Tôi đề xuất "có đủ điều kiện", còn điều kiện như thế nào thì có quy định cụ thể thì được cấp bằng, không nhất thiết phải được dự thi.

## Nguyen Phi Long, Binh Dinh Province, Caucus #6, Citizen Treatment

[Article 101] Public pre-schools are overly crowded, ..., non-public schools are massively open without assessment and evaluation, many are not unregistered, teachers are not trained and do not meet standards. Thus, Article 101 should specify the percentage of the budget for pre-school education, especially in remote and border areas, islands of export processing zones. I request the state to prioritize this education level, allocate budget and make it a policy in order to set the ground for investing budget as well as land allocation ... Secondly, there must also be investment in teachers, preschool teachers must have certain status in the education system, in the system of civil servants, officials and managers of the education sector ... It is also necessary to identify critical areas that the State must invest such as industrial parks and export processing zones where a huge number of young workers at the age of reproduction are working, ... as well as remote areas, where socialization of education is not a practical option.

[Đ101] Các trường [mầm non] công lập quá tải, ... trường ngoài công lập mở ò ạt, không có kiểm định đánh giá, nhiều trường tự lập, giáo viên không được đào tạo, không đạt chuẩn. Theo đó, điều 101 cần ghi rõ phần trăm trong ngân sách đầu tư cho giáo dục mầm non đặc biệt ở vùng sâu, vùng xa, biên giới, hải đảo các khu công nghiệp khu chế xuất. Tôi đề nghị Nhà nước ưu tiên đầu tư cho cấp bậc này, giành một nguồn kinh phí nhất định, vì nếu có chính sách thì ngoài điều đầu tư về tiền bạc thì cũng còn phải quy hoạch đất đai... Thứ hai là cũng phải đầu tư về giáo viên, giáo viên mầm non phải có vị trí nhất định trong hệ thống giáo dục, trong hệ thống cán bộ công chức, viên chức của ngành giáo dục, của cán bộ quản lý giáo dục... Tôi đề nghị Nhà nước phải dành kinh phí thích đáng để đầu tư cấp bậc học này. Phải quy định những chỗ trọng điểm là nhà nước phải đầu tư, các khu công nghiệp, khu chế xuất hiện nay, cơ bản là các thanh niên công nhân đều trong độ tuổi xây dựng gia đình... Hay xã hội hóa nhà trẻ mẫu giáo ở vùng sâu vùng xa thì làm sao xã hội hóa được, cái này nhà nước phải dành ngân sách để đầu tư.

## Chau Quang Dao, Kien Giang, Caucus #19, Firm Treatment

We are worried about this number. 67% of firms say that vocational training is low quality and 73% of businesses said that the quality of local labor was not satisfactory. Thus, we have made great efforts in vocational training of rural labor and vocational training. But the businesses cost of labor training accounts for 5% of total costs. As such, we still have many reasons to believe the quality of occupational labor that is not satisfactory.

Con số này chúng ta rất lo. 67% doanh nghiệp cho rằng chất lượng đào tạo nghề còn kém, 73% doanh nghiệp cho rằng chất lượng lao động địa phương chưa đạt yêu cầu. Như vậy chúng ta đã nỗ lực rất nhiều trong việc đào tạo nghề lao động nông thôn rồi đào tạo các ngành nghề. Nhưng mà việc chi phí kinh doanh dành cho đào tạo lao động chiếm 5%. Như vậy, chúng ta còn rất nhiều nguyên nhân dẫn đến chất lượng lao động nghề nghiệp chúng ta chưa đạt yêu cầu.

## Le Quang Huy, Nghe An Province, Caucus #14, Firm Treatment

In practice, companies have broadly applied advance technologies, requiring workers to obtain new skills. For instance, pie manufacturers in Soc Trang, including small businesses, and textile enterprises have replaced manual labor with automatic factory.

một thực tế là gần đây doanh nghiệp đã ứng dụng tiến bộ kỹ thuật rất mạnh mẽ, đòi hỏi người lao động phải có rất nhiều kỹ năng mới như bánh pía Sóc Trăng kể cả doanh nghiệp nhỏ đã vào làm, một loạt doanh nghiệp dệt may đã đưa thiết bị vào và toàn bộ lao động thủ công bị sa thải, phải chuyển đổi sang kỹ năng mới.

**Ly Tiet Hanh, Binh Dinh Province, Caucus #6, Control Group**

[Article 27] From practical experience, we concur that the goals of general education have been reasonably designed; however, it is necessary to ensure the integration and connection between education levels ... **Voters** are also interested in professional orientation for students from general education level ... I suggest that the law emphasizes on and provides specific solutions for this matter.

[Đ27] Thực tế công tác tình hình địa phương, chúng tôi thấy mục tiêu giáo dục phổ thông chúng ta đề ra tương đối sát, tuy nhiên cần bảo đảm có sự liên thông, liên kết giữa cấp học ... **Cử tri** quan tâm vấn đề định hướng nghề nghiệp cho học sinh, trong đây chỉ có đến cấp học trung học phổ thông thì mới đề cập vấn đề kĩ thuật và hướng nghiệp ..., tôi đề nghị trong luật cần nhấn mạnh và có giải pháp cụ thể.

**Ngo Thi Kim Yen, Da Nang City, Caucus #4, Control Group**

[Article 77] Currently there is a circular regulating maximum number of pupils per class at general education levels; however, the average numbers, especially those of big cities, are in fact much higher. It is therefore recommended that the Drafting Board take this factor into account to train a sufficient number of qualified teachers. Besides, it is suggested to consider whether the standardized educational qualifications for teachers in subjects such as art and music at the elementary level must necessarily be at the university level.

[Đ77] Hiện nay có một thông tư quy định sĩ số học sinh ở các cấp giáo dục phổ thông; song thực tế sĩ số trung bình, đặc biệt ở các đô thị lớn, cao hơn nhiều so với mức quy định. Do đó đề nghị Ban soạn thảo tính đến yếu tố này để có lộ trình đào tạo đủ lượng giáo viên có trình độ chuẩn được đào tạo. Bên cạnh đó, đề nghị cần nhắc trình độ chuẩn được đào tạo của giáo viên các môn như mỹ thuật, âm nhạc cấp tiểu học có nhất thiết phải là trình độ đại học không.

**Nguyen Thi Xuan, Dac Lac, Query, Control Group**

Many voters were concerned about Official Letter No. 4612 issued by the Ministry on October 3, 2017, which prohibit teaching of contents beyond textbooks. Is this a ban, and whether such ban on teaching non-textbook content contradicts the spirit of educational innovation, when the Ministry is designing open textbooks for general education?

Nhiều cử tri băn khoăn về Công văn số 4612 do bộ ban hành ngày 03/10/2017, trong đó quy định không được dạy những nội dung ngoài sách giáo khoa. Thưa Bộ trưởng, đây có phải là một lệnh cấm và lệnh cấm dạy những nội dung ngoài sách giáo khoa có mâu thuẫn với tinh thần đổi mới giáo dục, khi chính Bộ đang xây dựng một chương trình sách giáo khoa phổ thông theo hướng mở.

## 12 Relationship between delegates' comments and VLOE changes

Issue in Debate	Comments Made by Delegates	Change from Debated Draft to Final VLOE
<p><b>Budget Prioritization:</b> Delegates requested that the law commit to a specific amount of resources, and prioritize budget to areas in where educational resources were urgent. Article 96 was amended to reflect these requests.</p>	<p><b>Nguyen Phi Long, Binh Duong, Caucus #7, Citizen Treatment:</b> "Public pre-schools are overly crowded, ..., non-public schools are massively open without assessment and evaluation, many are not unregistered, teachers are not trained and do not meet standards. Thus, the law should specify the percentage of the budget for pre-school education, especially in remote and border areas, islands of export processing zones. I request the state to prioritize this education level, allocate budget and make it a policy in order to set the ground for investing budget as well as land allocation ... Secondly, there must also be investment in teachers, preschool teachers must have certain status in the system of civil servants, officials and managers of the education sector ... It is also necessary to identify critical areas that the State must invest such as industrial parks and export processing zones where a huge number of young workers at the age of reproduction are working, ... as well as remote areas, where socialization of education is not a practical option."</p>	<p>Underlined sections added to <i>Article 96, "Article 96. State Budget Invested in Education"</i></p> <ol style="list-style-type: none"> <li>1. The State gives top priority to the allocation of education budget, ensuring that the State budget spent on education and training is at least <b><u>20% of the total state budget expenditure.</u></b></li> <li>2. State budget expenditures on education shall be allocated according to the principles of openness and democracy; Based on the education scale and socio-economic development conditions of each region; <b><u>ensure budgets for the universalization of education and educational development in ethnic minority areas and areas with exceptionally difficult socio-economic conditions. The State is responsible for allocating adequate and timely funding to implement universal education and in accordance with the progress of the school year.</u></b></li> <li>3. Educational management agencies and educational establishments shall have to effectively manage and use the assigned education budget and other revenue sources according to law provisions.</li> </ol>
<p><b>Preschool Education:</b> Delegates and voters raised concerns on critical problems in preschool education and called for the government to prioritize investment and resources into this education level. Article 27 on development policies for preschool education was added</p>	<p>- <b>Truong Thi Bich Hanh, Binh Duong, Caucus #7, Citizen Treatment:</b> There is a serious shortage of preschool education service, especially in industrial provinces that attract workers such as Binh Duong, Dong Nai, Ho Chi Minh City etc. In Binh Duong, an enterprise has opened kindergartens at their premises to look after workers' children. This model is of good service, comparable to public service - teachers' salary is paid by the company, parents only have to pay for children's meals, and the children can stay there in accordance with their parents' shifts. However, government policies to support such models have merely been established, and companies have to arrange all by themselves, from finding teachers, managers etc, not to mention the administrative bureaucracy they face to establish the kindergarten as it is not an area in their business registration. In conclusion, regulations related to preschool education need to be clearer, more specific and address urgent practical problems. If the state is not able to provide preschool education services, then it must have mechanisms to support private sector and promote socialization to ensure preschool institutions meet certain requirements.</p> <p>- <b>Dang Thuan Phong, Ben Tre, Query Control Group, The Education Minister</b> mentioned that our preschool education is highly evaluated by UNICEF, ... but having followed this whole process I would like to reiterate the limitations of our preschool education, which is currently causing a lot of concerns. Firstly, the scale of development is not equal in all regions; the quality of preschool education is not stable; policy network is inconsistent; it receives the lowest investment compared to other education levels; facilities, schools, teachers and management did not meet the demands. The rate of spending on preschool education by the government is only 39%, which means families contribute 61%, the highest contribution compared to other fields of education.</p>	<p><i>New Article 27, "Development Policies for Pre-School Education"</i></p> <p>Policies for early childhood education development</p> <ol style="list-style-type: none"> <li>1. The State adopts policies on investment in preschool education development; give priority to developing preschool education in mountainous areas, islands, ethnic minority areas, areas with extremely difficult socio-economic conditions, and areas with industrial parks.</li> <li>2. The State adopts policies to encourage organizations and individuals to invest in developing preschool education to meet social needs.</li> <li>3. The Government shall detail this Article.</li> </ol>

<p><b>Teacher Qualification:</b> Delegates questioned the need for a pedagogical degree and asked that degrees in other disciplines be recognized for employment in higher level education. Article 27 was amended to provide criteria for recognizing other disciplines.</p>	<p>- <b>Phan Thai Binh, Quang Nam, Caucus #15, Citizen Treatment:</b> Through meetings with teacher constituents in Quang Nam province, they expressed objection that teachers may possess bachelor degree in relevant disciplines with a pedagogic training certificate. I also agreed to review the standardized educational qualifications for teachers as well as the reality and unemployment.</p> <p>- <b>Bui Thanh Tung, Hai Phong, Caucus #6, Firm Treatment:</b> The draft revised law requires that primary and lower secondary teachers without a bachelor's degree in pedagogy training must possess a bachelor's degree of suitable specialization and a certificate of pedagogy training. Many educational constituents, especially those at managerial level, shared that this would not meet the pedagogical requirements ... In my opinion, it is not necessary that university-level lecturers must possess a Master degree, particularly for assisting lecturers, practical teachers or visiting lecturers. There are excellent graduates who are offered teaching assistant positions by their universities; many bachelors, engineers working in private sector with years of working experience. If we can take advantage of this group to be visiting lecturers, we could develop practical experience and knowledge and avoid isolated academic environment</p> <p>- <b>Ngo Thi Kim Yen, Da Nang, Caucus #4, Control Group:</b> Currently there is a circular regulating maximum number of pupils per class at general education levels; however, the average numbers, especially those of big cities, are in fact much higher. It is therefore recommended that the Drafting Board take this factor into account to train a sufficient number of qualified teachers. Besides, it is suggested to consider whether the standardized educational qualifications for teachers in subjects such as art and music at primary schools must necessarily be at the university level.</p>	<p>Amendments made to <i>Article 72, "Standard Qualifications of Teachers' training."</i></p> <p>1. The standard qualifications of teachers' training are prescribed as follows:</p> <p>a) Holding a bachelor's degree <b><u>in pedagogy college or higher for preschool teachers;</u></b></p> <p>b) Holding a bachelor's <b><u>degree in teacher training major or higher for elementary, junior high and high school teachers. If the subject does not have enough teachers with bachelor's degrees in teacher training disciplines, they must have an appropriate bachelor's degree and certificate of pedagogic training;</u></b></p> <p>c) Holding a master's degree for university-level teachers; hold a doctorate degree for teachers who teach, guide master's theses, and doctoral theses;</p> <p>d) The standard qualifications of the teachers who teach at vocational training institutions shall comply with the Law on Vocational Education.</p> <p>2. <b><u>The Government shall prescribe the roadmap for raising the standard level of preschool, elementary and junior secondary teachers prescribed at Points a and b, Clause 1 of this Article. The Minister of Education and Training and the Minister of Labor, War Invalids and Social Affairs shall, within their duties and powers, stipulate the use of teachers in cases where they do not meet the provisions of Clause 1.</u></b></p> <p>New <i>Article 73</i> on "<i>Teacher Training</i>" added to Final LOE.</p> <p>1. The State adopts policies on training and fostering to raise the political, professional and professional qualifications for teachers; Teachers who are sent for training and retraining shall enjoy salaries and allowances according to the Government's regulations.</p> <p>2. Educational institutions have the responsibility to create conditions for teachers to be trained and fostered to meet the standards prescribed by law.</p>
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<p><b>Pedagogical Credit:</b> There was general support for changes of policy for pedagogical students, from tuition exemption to (refundable) subsidies. Many opined that financial subsidies were however, not the key issue to attract talented students into pedagogy. Article 85 was amended to allow for pedagogical credit to attract better teachers.</p>	<p>- <b>Nguyen Thanh Phuong, Can Tho, Floor, Citizen Treatment:</b> Pedagogical credit ... facilitates people who have difficulties in learning conditions as well as free training if they serve in pedagogy in a certain time. However, there will be cases where if pedagogical students do not need to use pedagogical credit but work in education sector after graduation. In my opinion, salary and recruitment policies are the root of the problem as well as of attracting quality people to work in pedagogical field, through which can the quality of the pedagogical training be solved.</p> <p>- <b>Bui Ngoc Chuong, Ca Mau, Floor, Control Group:</b> Subsidies for pedagogical students ... might not deliver the expected impact should graduate students be unemployed, then how they can pay back the loans. Many shared the view that financial subsidies are not the key issue but appealing policies to attract quality pupils into pedagogical institutions as well as quality teachers ... It is necessary to identify demands in order to plan for adequate training of teacher and avoiding underemployment.</p>	<p><i>Article 85. "Scholarships, Social Allowances, Tuition Exemptions, Reductions, Payment of School Fees and Living Expenses,"</i></p> <p>4. Pupils and pedagogical students <b><u>will be supported with tuition fee and living expenses</u></b> for the whole course. Those who are supported with tuition fee and living expenses after 02 years from graduation, if they do not work in education or do not work for a prescribed time, they must reimburse the State-supported expenses. support. <b><u>Maximum time for repayment is equal to the time of training.</u></b></p> <p>Pupils and pedagogical students are entitled to scholarship policies to encourage learning, social allowances, tuition exemption and reduction prescribed in Clauses 1 and 2 of this Article.</p>
<p><b>Certificate of Completion:</b> Delegates called for certifications for learners who complete upper secondary education level at both general education institution or vocational training institutions. With such certificates, they would be recognized and able to proceed to more advanced education when it is possible. Article 34 was amended to provide a pathway for conversion of a certificate to receive more advanced education.</p>	<p>- <b>Tran Kim Yen, HCMC, Caucus #2, Citizen Treatment:</b> Voters are of the view that the whole learning process be assessed to decide if a pupil should get an upper secondary school diploma. It is expensive to organize an exam to determine high school graduation qualifications. I propose that upper secondary school students who meet the requirement would receive the diplomas without having to take graduation exams.</p>	<p><i>Article 34, "Certification of Completion of Elementary, Upper Secondary, and Secondary School Diplomas"</i> Clause 3 was amended and Clause 4 added.</p> <p>3. Students who have completed high school are eligible to take the exam according to the regulations of the Minister of Education and Training but fail to take the exam or fail the exam, they will be granted a certificate of completion by the school principal. general education curriculum. The certificate of completion of the general education program is used to register for the high school graduation exam when the learner needs it or to attend vocational education and use it in specific cases according to provisions of law.</p> <p>4. Students who have a junior high school diploma and attend an intermediate level education in a vocational education institution, after having studied and passed the required high enough level of high school cultural knowledge as prescribed. of the Minister of Education and Training, the head of the educational institution organizing the teaching of a volume of high school cultural knowledge shall grant a certificate of sufficient quantity of high school cultural knowledge.</p> <p>The certificate fully meets the required high school cultural knowledge used to pursue higher education levels and to be used in specific cases as prescribed by law.</p>

<p><b>Tuition Exemptions:</b>  Many representatives expressed their and voters' calls for tuition exemption for kindergarten, primary and lower secondary education. Three new clauses were added to Article 99 to provide for tuition exemptions for poor students.</p>	<p>- <b>Lam Dinh Thang, HCMC, Caucus #2, Firm Treatment:</b> "Regarding tuition exemption for lower secondary education level, it was mentioned in previous versions of the law and was well received by the public. However, this was removed in the last fifth draft. I suggested to keep such idea as in previous drafts with implementation timeline such as from 2020 when the Resolution 29 of nine years of compulsory education takes effect."</p> <p>- <b>Huynh Thanh Phuong, Caucus #4, Tay Ninh, Control:</b> "Constituents expect the National Assembly to consider tuition exemption for lower secondary education. Ministry of Education and Training submitted to the Government but the Government has yet agreed to include it."</p> <p>- <b>Nguyen Van Than, Query, Thai Binh, Firm Treatment:</b> "Private schools, in my opinion, should be allowed to decide their tuitions based on the basic tuition fees and by agreement. The People's Council does not need to decide this."</p>	<p>Sub-Clauses 3 through 5 added to <i>Article 99, "Tuition and Costs of Education and Training Services."</i></p> <p>3. Primary pupils in public education establishments are not required to pay tuition fees; In areas where there are not enough public schools, primary pupils in private educational establishments are supported by the State with school fee payment and the level of support shall be decided by the provincial People's Councils.</p> <p>4. Preschool children of 05 years old in extremely difficult hamlets and villages, ethnic minority areas, deep-lying and remote areas, coastal areas and islands will be exempted from school fees.</p> <p>5. 05-year-old preschool children who are not defined in Clause 4 of this Article and lower secondary students are exempted from school fees according to the Government's prescribed schedule.</p>
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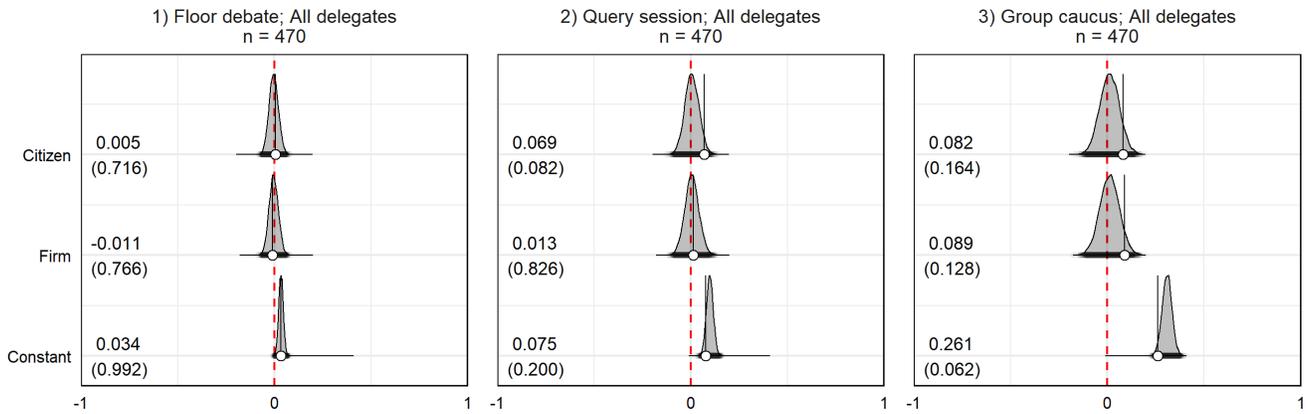


Figure 13.1: **Most speaking occurred in group caucuses.** Figure displays density plots of 10,000 replicate coefficients from RI, with OLS coefficients and parenthetical  $p$ -values at left.

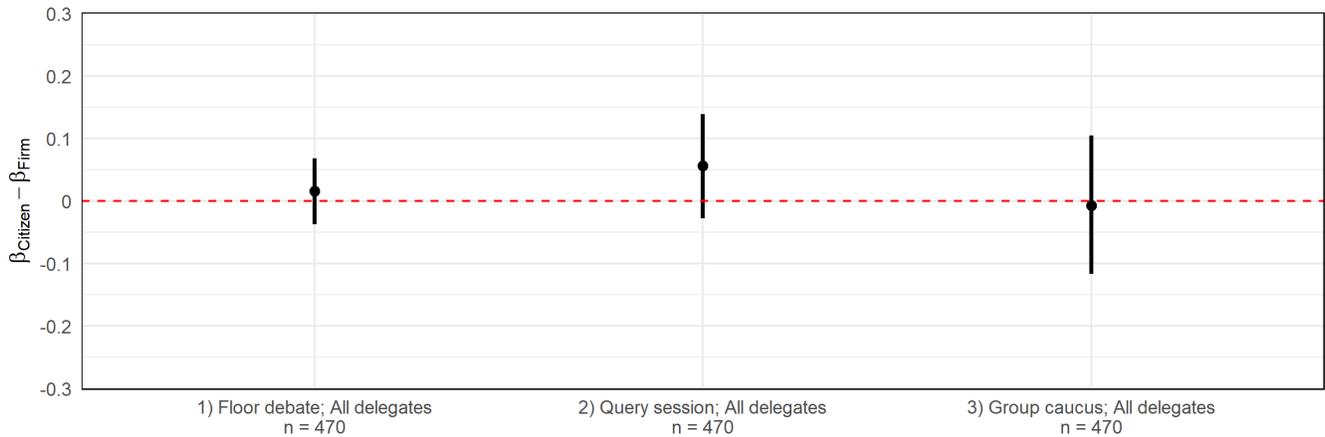


Figure 13.2: **Firm- and citizen-treated delegates spoke at equal rates in group caucuses.** Figure displays difference in citizen and firm coefficients, with RI-derived confidence intervals.

### 13 Speaking results by legislative forum

The main results are based on pooling the transcripts from the group caucuses, query sessions, and floor debates; here we present results within each legislative forum separately. Looking at the regression constants in Figure 13.1, it is clear to see that there was considerably more widespread speaking in the group caucuses. Also notable is the total lack of significant treatment effects at the forum level, with the partial exception of the citizen ATE in the query sessions (significant at  $p=0.10$ ). Although insignificant, the citizen and firm ATEs are largest in the group caucuses, and

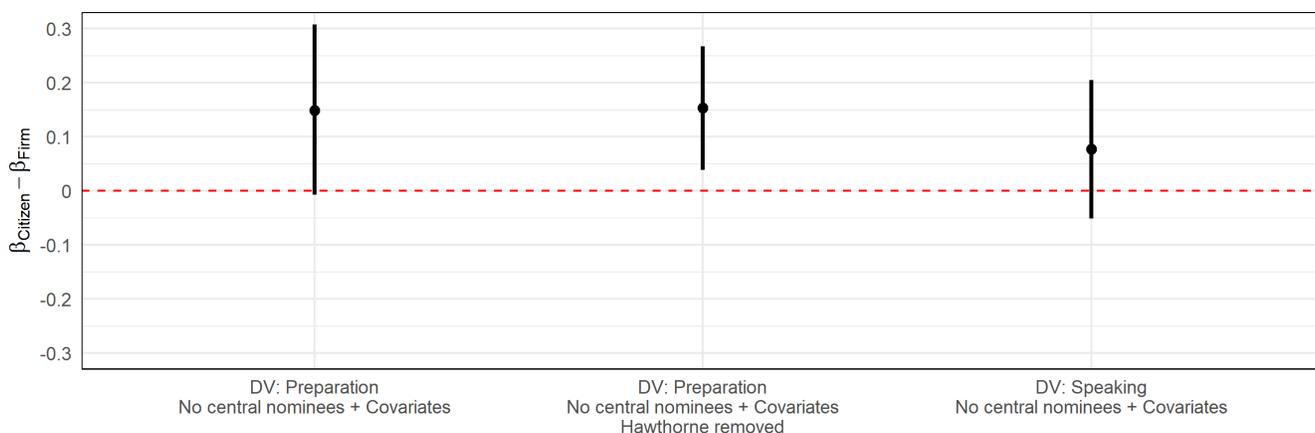


Figure 14.1: **Direct effects of citizen treatment exceed those of firm treatment among local nominees.** Figure displays difference in citizen and firm coefficients, with RI-derived confidence intervals.

appear to be roughly equally-sized at 0.08-0.09.

Figure 13.2 examines the differences in treatment effects across legislative fora. With RI-derived confidence intervals, this figure demonstrates that firm- and citizen-treated delegates spoke at approximately equal rates in the floor debates and group caucuses. The result for treatment effects on speaking in the query session are more ambiguous, but do not attain conventional levels of statistical significance.

## 14 Citizen-firm differences without central nominees

Figure 8 in the paper presents the differences between the citizen and firm treatment effects for three specifications: debate preparation, debate preparation accounting for the Hawthorne effect, and delegate speaking. Here we utilize the same RI-based confidence interval strategy to examine these treatment effect differences in the absence of central nominees, for whom the effects of the citizen treatment are theoretically attenuated.<sup>6</sup> Figure 14.1 presents these findings, which are qualitatively similar to those in paper Figure 8 but slightly stronger. Again, all three differences

<sup>6</sup>While the caption for Figure 14.1 labels the sample as local nominees, it should be recalled that this is a matter of convenience. Technically, the sample also contains two self-nominated delegates.

are positive. Again, the results for debate preparation are nearly significant in the presence of the Hawthorne effect and strongly significant in its absence. Although the difference in speaking outcome treatment effects does not attain statistical significance, it too is a slightly stronger effect than when central nominees are included.

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