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

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January 29, 2019

Abstract

Our project aims to establish whether targeted provision of constituents' preferences increases the responsiveness of delegates to the Vietnamese National Assembly (VNA). Utilizing a randomized control trial (RCT), we assign legislators to one of 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 no informational treatment whatsoever. We also employ a saturation design, applying the treatments to differing shares of delegates across provinces. After the summer 2018 session, we collected behavioral data on delegates from the legislative session, including answers to a VNA Library survey about debate preparation; the identity of speakers in group caucuses, query sessions, and floor debates; and the textual content of those speeches. We find consistent evidence that citizen-treated delegates were more responsive, via debate preparation and the decision to speak; evidence from speech content is more mixed. More speculatively, we find little evidence of spillover from treated to untreated delegates, but substantial evidence of treatment reinforcement. Citizen-treated delegates grew more responsive as more of their peers possessed identical information.

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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 articulate policy positions that are congruent with survey evidence on local preferences (Truex, 2016). There is even evidence that despite highly 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; Dimitrov, 2013; Wilson and Wright, 2017). In this paper, we provide the first test of this link in the theory of authoritarian responsiveness with a randomized control trial (RCT) that provided selected delegates with information on the opinions of firms and citizens in their province concerning the amendment of the Vietnamese Law on Education.

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, electoral accountability is not necessary for responsiveness. Governments may also choose to be responsive due to the public spiritedness of officials, the checks and balances of different government actors (Manin, Przeworski and Stokes, 1999, 3-4), or upward accountability to central politicians. These alternative mechanisms have provided the impetus for research into how regimes without elections or with highly flawed elections may nevertheless be responsive to their constituencies (Weeks, 2008; 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).

Two criteria in the Manin, Przeworski and Stokes (1999) definition, however, are critical for empirical research on authoritarian regimes and provide the impetus for our field experiment. First, there must be an informative signal of aggregate citizen preferences to government actors. This is especially true for scholars employing the "public spiritedness" mechanism, which implies that legislators want and will employ information on their citizens' policy preferences. Second, responsiveness requires the adoption of policies that are in line with the signal of citizen preferences. In other words, it necessitates behavioral evidence that the politician moves to enact citizens' objectives.

Due to the difficulty of researching 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 online posting by individual voters (Distelhorst and Hou, 2014, 2017), not aggregate constituency preferences, providing an unclear signal about the underlying preferences in their constituency. 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 the delegate 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 controlled trial of VNA delegates in the debate over amendments to the Law on Education during the 5th Session (May 2018) of the 14th VNA. In order to simulate a clear voter signal, we provided each treated delegate with rigorous public opinion data on preferences over education in her home province. We assigned legislators to one of three groups: (1) those briefed on the opinions of citizens within their province; (2) those presented with the preferences of local firms; and (3) those receiving no informational treatment whatsoever. Prior to the experiment, however, all three groups of delegates were exposed by the VNA Library to similar baseline information about central party decrees and government documents stating

the preferences and goals of the Vietnamese Communist Party regarding educational reform.

In addition, we employed a saturation design, so that different shares of delegates were treated within each province. This allowed us to measure two additional effects. The first, spillover, occurs when a province's non-treated delegates learn of the informational treatment; the second, reinforcement, happens when delegates encounter peers within their provincial delegation possessing similar information about voter preferences. To obtain behavioral outcome measures, we then observed whether delegates (1) believed they were prepared to debate the law; (2) spoke about the Education Law 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 informational treatments.

We find delegates are responsive to citizens but not firms, an important contribution to the debate over authoritarian responsiveness. First, delegates treated with information on citizens' preferences were 23 percentage points more likely than the control group to say that they felt prepared for debate, and 11 percentage points more likely to speak in caucus meetings, query sessions, or floor debates. Delegates treated with local businesses' preferences, however, were not significantly different from the control group on either measure. Second, we find substantial evidence of reinforcement effects yet no evidence of spillover. Treated delegates felt more prepared and grew more likely to speak as the number of similarly treated delegates in their locality increased. Turning to more fine-grained measures of responsiveness, the higher the provincial share of delegates receiving the citizen treatment, the greater the likelihood a delegation member mentions her province's name in debates over the law. More speculatively, topic modelling reveals that treated delegates discussed keywords from our infographics in legislative fora, an effect that is strongest under the citizen treatment. Finally, analyses in Appendix F. largely eliminate electoral accountability as a potential mechanism for responsiveness.

Our findings present three important contributions to the existing political economy and development literatures. First, the bulk of research on authoritarian elections and

assemblies has prioritized the authoritarian goals of coercion (Slater, 2003), information on potential opposition and regime strength (Geddes, 1999; Magaloni, 2006; Geddes, Wright and Frantz, 2014), coopting opposition (Gandhi, 2008; Wright, 2008; Gandhi and Lust-Okar, 2009), power-sharing among elite politicians (Gehlbach and Keefer, 2011; Svolik, 2012), and identifying talented cadres for promotion (Boix and Svolik, 2013; Malesky and Schuler, 2013).[1] Our project, along with other work in the responsiveness literature, emphasizes goals beyond authoritarian resilience. For one, these institutions may deliver beneficial policies, however imperfectly, to their country's citizens.

Second, beyond the literature on authoritarian institutions, our project adds nuance to the principal-agent relationship linking voters to legislators in developing countries (Besley and Burgess, 2002; Olken, Pande and Dragusanu, 2011). The majority of work on information transmission in developing countries focuses on information provision to voters. A popular approach to improve politicians' accountability to their voters is to disclose and disseminate records of past performance (Ferraz and Finan, 2008; Humphreys and Weinstein, 2012; Chong et al., 2014; Dunning et al., 2019). Work in this vein on the VNA, the site of our RCT, did not find significant evidence of enhanced delegate performance (Malesky, Schuler and Tran, 2012). However, if poor past performance is due to a lack of information regarding constituents' interests, information disclosure programs may not lead to selection of better politicians or improved service delivery (Besley, Pande and Rao, 2005). We show that responsiveness requires that the interests of citizens first be conveyed to legislators, a precondition argued nearly two decades ago yet underappreciated in existing work (Manin, Przeworski and Stokes, 1999). Only then can legislators' actions (or lack thereof) in response to citizens' preferences provide a valid basis for judging their performance.

Finally, our project contributes to the development agenda. A legislature capable of making laws broadly reflective of societal interests is a cornerstone for both development and democracy. Recognizing this fact, development agencies have invested millions in legislative

[1] See Brancati (2014) for a helpful review.

strengthening initiatives around the developing world since the early 1990s (Miller, Pelizzo and Stapenhurst, 2004; Hudson and Wren, 2007). A key component of these programs is to develop the research capacity of the legislature, providing legislators with information relevant to their constituents and to topics of discussion. In our field site of Vietnam, for instance, the US-AID funded Governance for Inclusive Growth project spent a third of its 42 million USD budget on legislative training. The underlying theory behind this intervention is that legislators desire to be responsive to their constituents' interests, but are unsure of those interests or how best to serve them (Butler and Nickerson, 2011; Esaiasson, Gilljam and Persson, 2017).

Our paper offers evidence that providing information increases legislator responsiveness. Currently, most legislative strengthening programs train legislative staffs in their entirety, leaving no valid comparison group, or in self-selected groups, leading to selection bias. Furthermore, training research staff is a step removed from legislators obtaining the necessary information. Indeed, staff may be uncooperative or lack the most relevant information, thus diluting the informational treatment inherent in current programs. Our RCT design solves the first problem by creating valid treatment-control groups. In addition, we strengthen the informational treatment by focusing on education, a hot-button issue that was scheduled for an upcoming legislative debate, by presenting information specific to each legislator's provincial constituency, and by delivering this information directly to legislators via customized infographics. In other words, our informational treatment is timely, tailored, and direct, with information sourced from annual governance surveys of citizens and firms.

The paper proceeds as follows. We first discuss the previous work on legislative responsiveness in authoritarian regimes, before turning to the advantages and impediments to analyzing responsiveness in our specific research context of Vietnam. Third, we detail the research design for our project, explaining why our behavioral measures of responsiveness improve upon previous work. The final sections present the results of our experiment and conclude.

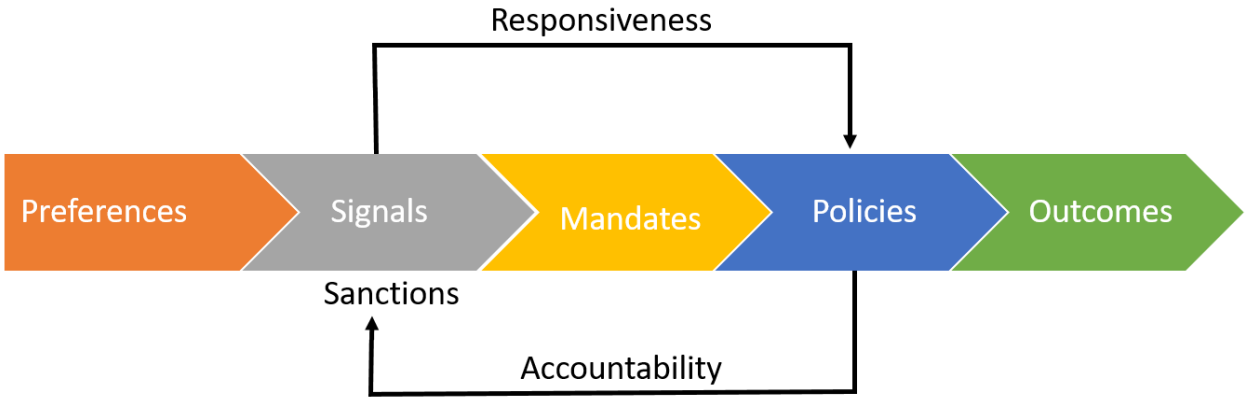

Figure 1: **Policy process.** Black lines represent the actions of politicians in a theory of responsiveness and representation.

2 Theorizing Responsiveness in Authoritarian Regimes

Figure 1 reproduces the stylized depiction of a policy-making process proposed by Manin, Przeworski and Stokes (1999). Constituents have preferences over policies and convey those preferences to politicians through one of two ways. The first method, signals, includes forms of direct political action like opinion polls, letter campaigns, and demonstrations. Mandates are the second way of conveying preferences, and come about when citizens vote for party platforms or for a politician's campaign pledges. Individual politicians then work to enact policies consistent with these preferences and, if successful, alter the status quo. These results are called outcomes. A politician is responsive when she acts in accordance with the signaled or mandated 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.[2] To reiterate, the threat of sanctions is a necessary condition for accountability – not responsiveness. Why, then, might authoritarian legislators be responsive?

One potential driver of responsiveness is simple public spiritedness on the part of legis-

[2]It is important to distinguish responsiveness from the separate concept of representation, which simply entails acting in the best interests of a constituency. Thus, in cases where constituents do not have perfect information on policy choices and their relationship to prospective outcomes, it is still possible for a politician to be representative even when her actions do not align with their demands. In other words, a politician can represent without being responsive.

lators. 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 may also ensure that policies and outcomes eventually conform to the expectations of citizens (Manin, 1994; Persson, Roland and Tabellini, 1997; O'Donnell, 1998). These insights have generated a wave of new work, predominantly focused on China, which examines 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, and that the probability of response is conditioned by social conflict (see also Distelhorst and Hou (2014)). Building off of this finding, Chen, Pan and Xu (2016) note that officials are more likely to respond to messages that threaten collective action or citizen whistleblowing. Beyond China, Malesky and Schuler (2010) show that VNA delegates are more likely to speak and criticize in query sessions when they are full-time local delegates or competitively elected. They 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 more likely to spend heavily on education and health care when the ruling party wins despite a poor electoral performance.

Students of authoritarian regimes have also pointed out that politicians may be responsive to businesses in their communities as well as citizens. Building off a stream of work demonstrating that local businesses are critical for official promotions in single-party regimes (Robison, 1988; Gomez, 1994; Dickson, 2003; Tsai, 2007; Gainsborough, 2009), Meng and

Pan (2015) introduce business interests to the debate. Although when 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; Jensen, Malesky and Weymouth, 2014; Wilson and Wright, 2017). Vocal debate exists about the mechanisms underlying this relationship (Schuler and Westerland, 2018), but all explanations hinge upon the fact that legislatures allow businesses to better protect their institutional and policy interests.

A critical complication in authoritarian regimes is that these 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 leaders and therefore must balance two sets of demands. Indeed, the cooptation and informational theories argue that the main objective of authoritarian institutions is to convey information to higher-level authorities regarding the preferences of regime outsiders, so that central leaders can adjust their policies accordingly (Gandhi, 2008; Gandhi and Lust-Okar, 2009). Some would argue that this constitutes a mandate for legislator responsiveness. Coupled with the threat of sanctions – be they through control over VNA nomination and vetting procedures or other means – the Vietnamese National Assembly is structured for responsiveness to constituents via *upward* accountability to the regime. In this framework, responsiveness to constituents is therefore mediated by central preferences.

Therefore, three assumptions are necessary to derive the main hypothesis of the responsiveness literature. First, all delegates are subject to elite leadership messaging about central preferences and therefore the primary difference between delegates is their access to information on local preferences. Second, the average delegate is in fact incentivized to be responsive to her constituents. Third, such a delegate likely lacks the information concerning her constituents' preferences that would enable such responsiveness. In other words, a

persistent informational gap prohibits a delegate's inherent receptivity to constituents from blossoming into full-blown responsiveness. Resolving this informational gap should induce responsiveness.

2.1 Previous Empirical Studies of Responsiveness

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 legislator behavior outlined in the Manin, Przeworski and Stokes (1999) definition. Specifically, problems frequently lie in three areas: the treatment variable (aggregate information), the outcome variable (policy action), and empirical establishment of causality.

Following the theoretical discussion above, the treatment must be an informative signal of aggregate voter preferences to government actors. Such a signal can include broad information such as public opinion surveys, demonstrations, mail-in campaigns, or publicly expressed support for campaign pledges or platforms (Lupu, 2013; Stokes, 1997), but to date, most work on responsiveness in authoritarian regimes has overlooked this aggregate criterion. The most common approach to testing responsiveness has been to send individual messages of voters or other stakeholders such as firms (Distelhorst and Hou, 2014; Meng and Pan, 2015; Chen, Pan and Xu, 2016; Distelhorst and Hou, 2017; Meng, Pan and Yang, 2017). These individual messages, however, provide information that is too noisy to inform policy choices. Politicians have no way to discern whether a particular email message, constituent letter, or post in a public forum is representative of their entire constituency or simply a particularly aggrieved citizen (Besley and Burgess, 2002). Moreover, this work has yet to consider whether multiple conflicting messages from citizens or other stakeholders may actually inhibit responsiveness by limiting the scope of policies available to politicians.

Responsiveness also requires the adoption of policies that are in line with the signal of citizen preferences. In other words, establishing responsiveness requires observation of

a politician moving to enact citizen preferences. Yet difficulties familiar to any scholar of authoritarian regimes have thus far precluded measurement of actual policy behavior. To date, three creative and valuable approaches have been tried, yet all suffer to some degree from limitations imposed by the challenges inherent to field work in authoritarian settings.

First, researchers have used surveys to poll politicians directly on their priorities in office (Manion, 2016, 2017); however, these answers may be prone to social desirability bias as politicians seek to impress researchers with their civic-mindedness (Meng and Pan, 2015). While embedded list experiments can reduce social desirability bias, they still measure stated preferences rather than actual behavior.

Second, researchers have measured the act of responding to individual emails or letters from constituents (Chen, Pan and Xu, 2016; Distelhorst and Hou, 2017). This captures receptiveness to particular constituents, but again does not entail policy-oriented action. An email is a relatively costless activity that does not require the politician to make trade-offs or take a politically contentious stance. In the words of Ding (2018), an email response is an example of "performative governance," an effort to appease constituents without actually altering the status quo. In fact, an email response does not even require that the politician herself take action, as low-ranking staff may handle correspondence.

Third, researchers have tried to identify correlations between the needs of a constituency, identified through survey data or aggregate economic analysis, and the preferences of politicians (Truex, 2016). These correlations are closer to the classic articulation of responsiveness, but are nonetheless prone to measurement error and omitted variable bias. We cannot disentangle preference congruence due to responsiveness from that arising out of pure happenstance. Miller (2015) on the other hand, finds that electoral shocks enhance greater social welfare spending, but does not demonstrate that citizens actually demanded that spending.

3 Context: The Vietnamese National Assembly

The setting for our field experiment is the National Assembly of Vietnam (VNA), whose roughly 500 delegates serve five-year terms and convene biannually to consider draft legislation. Voters directly elect delegates in reasonably competitive elections at the district level, where block voting entails a 2:1 candidate-to-seat ratio (Malesky and Schuler, 2009). This electoral connection raises the possibility of responsiveness, as voters may be able to leverage their ballots to select "good types" ex ante or "vote the bums out" ex post (Fearon, 1999; Manion, 2017).

Two characteristics make it an ideal empirical setting for generalizable lessons. First, the VNA has assumed a greater role in challenging and checking the executive, despite its authoritarian setting. While individual delegates cannot introduce new legislation, they may propose and vote on amendments during legislative debate and pose critical questions to the cabinet. Many floor comments cite citizens' concerns regarding pressing issues such as food safety, healthcare, and infrastructure.

The role of the body and quality of delegates has changed over time. With each new session, the share of full-time delegates has risen, and research has charted steady increases in educational attainment and professional expertise within the body over time (Malesky and Schuler, 2011, 2013; Schuler, 2018). As a result, the quality of legislative debate and query sessions has improved. Furthermore, elections, while still far from truly free and fair, have become uniquely competitive for single-party regimes. This means that voters enjoy limited but real choice among the candidates nominated and vetted by central institutions or local election commissions (Gainsborough, 2005). In the past three elections, a large number of centrally nominated candidates (those designated for leadership positions) have lost, while an even larger number barely eked out a victory (Malesky and Schuler, 2011). Increasing professionalism and functional expertise have in turn allowed the VNA to take on new responsibilities. These include the query sessions, where delegates grill ministers on their performance and policy choices; legislative debates, where delegates debate draft

laws; and public hearings, where committees (most notably the Economic Committee) solicit expert testimony on policy debates of national import (Schuler, 2018).

Second, the VNA has an underdeveloped research capacity, often leaving legislators bereft of information (Asia Foundation, 2013).[3] During workshops on the importance of information, many legislators have expressed a desire to understand better constituents' preferences – and their inability to do so without support. Importantly, delegates also view this information as a check on the regime's overly rosy official statistics. Taken together, the VNA is representative of many developing-world legislatures: its responsiveness is growing but remains circumscribed by informational deficits and executive branch dominance.

Importantly, there are also organizations within the VNA mandated to improve information provision and responsiveness, giving our project the potential for scale-up. In 2012, for instance, the VNA passed Decree No. 27, stipulating how to better improve debate and contact with local citizens. The decree provided each deputy a small budget for staff support and constituency services in all 63 provinces. The Office of the National Assembly (ONA) was declared responsible for supporting delegates in their legislative activities through its oversight of the Library of the National Assembly, much like the Congressional Research Service in the United States. Operating under the purview of the Standing Committee of the VNA, the Institute for Legislative Studies (ILS) was tasked with improving the quality of VNA proceedings through research endeavors. All of these institutions existed before, but Decree 27 more clearly delineated the relationship between delegates and citizens.

3.1 Barriers to Responsiveness in the VNA

While the VNA's relative competiveness and openness make it an ideal location for observing responsiveness among legislators, other features of the body present serious impediments.

[3]Available in English (https://asiafoundation.org/publication/improving-the-effectiveness-of-information-provision-for-national-assembly-deputies-summary-report-in-english/) and Vietnamese (https://asiafoundation.org/publication/improving-the-effectiveness-of-information-provision-for-national-assembly-deputies-vietnamese/). The Vietnamese version contains delegate remarks supporting the assertions made above; these quotations are not present in the English version.

Some of these obstacles severely hampered our experiment and are worth recounting in detail because they impact both scalability and generalizability. We categorize these impediments under the headings institutional structure and rules, organizational transparency, and formal and informal access.

3.1.1 Institutional Barriers

VNA delegates differ in their mandates and prerogatives, affecting their susceptibility to an informational treatment. First, nominating authority matters. Delegates can be distinguished based on their nomination status. After dropping elite politburo officials from the experiment, 161 of our 470 delegates (34%) are central nominees, meaning they were nominated by government, party, or military institutions in Hanoi, but were sent to provincial electoral districts to run for election. These delegates only nominally represent the interests of the province to which they were sent – some potentially for the first time – and their allegiance lies with their central employers. Central nominees are full-time politicians or high-ranking bureaucrats, and many are designated for leadership positions in the VNA. They are thus placed in easier-to-win districts with lower candidate-to-seat ratios and less competition (Malesky and Schuler, 2011). 307 (65%) delegates are local nominees, typically local officials or professionals nominated by provincial election boards, living in the province, and expected to represent local interests. In our dataset, the two most common categories of local nominees are local politicians and members of mass organizations, such as the Fatherland Front, Women's Union, or Labor Confederation.[4] Second, there is a professionalism gap between full-time and part-time delegates. 160 (34%) VNA members are full-time delegates who serve year-round in the VNA Standing Committee or as Chairs or Deputy Chairs of the VNA's nine committees. 57 percent of full-time delegates are also central nominees; the remaining 69 locally-nominated, full-time delegates maintain provincial representative offices, providing constituency services and receiving petitions from voters. 310 (66 percent) part-

[4]In addition to central and local nominees, two delegates were self-nominated.

time delegates attend the two biannual plenary sessions, but otherwise hold full-time jobs in other fields. Full-time delegates more likely possess the time and capacity to be responsive to citizens, and full-time local delegates should be the most responsive of all. Conversely, we expect lower average treatment effects (ATEs) for central-nominees and part-time delegates.

A second institutional feature which affected our experiment is the pre-debate caucus. Adopted under former VNA Speaker Nguyen Van An, delegates are expected to meet with other provincial representatives to consolidate viewpoints, determine local priorities, and organize speaking opportunities during legislative debates. Query sessions are considered more free-wheeling and so do not require caucusing. Caucuses are typically chaired by the highest-ranking delegate, often the provincial Party Secretary or People's Committee Chairman. No other non-geographic caucusing is allowed. In fact, an attempt to organize a caucus of businessmen and leaders of business associations was rebuffed by the leadership in 2010.[5]

In 2018, the VNA Standing Committee altered the caucusing rules in a way that significantly impedes local responsiveness and affected our experimental design. Rather than convene 63 single-province caucuses, most provinces now meet in groups of three or four with no obvious geographical pattern behind the arrangement. Delegates from Tra Vinh, an ethnically diverse agricultural province in the Mekong Delta, caucused with delegates from Nghe An, a northern province famous for its revolutionary heritage, rich natural resources, and coastline. Delegates from the oil-rich southern beach resort of Ba Ria Vung Tau caucused with delegates from Hoa Binh, a poor, mountainous, bamboo-exporting province in the north. According to the Office of the National Assembly, these provincial groupings are aimed at spurring new conversations and will rotate each session. More skeptical political analysts believe this institutional change was specifically designed to dilute the influence of provincial leaders. Ultimately, these group caucuses impede responsiveness by weakening the ability of provincial delegates to coordinate provincial positions and organize floor activity

[5]Based on personal conversations with those involved in trying to arrange the business caucuses.

toward policy goals.

3.1.2 Opacity and Fluidity

A second issue that limits responsiveness and severely affected our experimental rollout was the constant fluidity and opacity in the topics and timetables of the VNA docket. According to the Vietnamese Law on Promulgation of Legal Normative Documents 2015 (commonly known as the "Law on Laws")[6] all Vietnamese laws and amendments to laws must be subject to legislative debate at two successive sessions. In the first session, National Assembly members debate the specifics of the law, arguing over key ideas and proposing amendments. In the second session, the amended draft law is debated before a final vote. A constituent may learn aggregate vote totals, but not whether her delegates voted for or against the legislation. Consistent with Chapter 3, Section 1 of the Law on Laws, the ONA posts for public comment all draft laws on its official website.[7] This site also publishes dockets for future sessions, listing when proposed laws and other legislative documents will be discussed, debated, and voted upon.

Compared to those of other non-democratic regimes, the procedures are highly transparent, formalized, and professional. However, this transparency is not fully adequate. Changes in the draft legislation, docket, and calendar for legislative debates occur frequently and without notice, and are not promptly reflected on the official website. Because critical debates are often canceled or postponed at the last minute, it is extremely difficult to know with any accuracy when a specific law will be debated. We initially identified several potential candidate laws for our experiment, including the Law on Corruption, the Labor Code, and the Law on Education. We actually identified the Labor Code as the most suitable for our design, because online comments of business and labor organizations signaled strong and conflicting opinions on the amendments. However, debate on the Labor Code was postponed multiple times due to its sensitivity. Consequently, our experiment on the Labor Code was

[6] Available here.
[7] Available here.

first delayed from October 2017 to May 2018, and is presently scheduled for May 2019. Throughout the design stage, we were in direct communication with officials at the VNA, yet despite access to the internal docket we were taken by surprise each time.

With the summer 2018 legislative session fast approaching, we quickly deployed an experiment on the Law on Education as a substitute for the Labor Code. This was only feasible because we already had in hand public opinion data on the law, and because we were assured that another last minute change was unlikely. In thinking about generalizing our experiment, it is important to note how difficult it would be for the average Vietnamese citizen to keep abreast of such high-level policies given the uncertainty and lack of openness in the debate schedule.

3.1.3 Formal and Informal Access

A third obstacle that impairs general responsiveness and impeded our work is the relative inaccessibility of individual delegates. While all 63 provincial delegations run a local VNA office that is designed to interact with citizens and provide constituency services, names and contact information are only available for the 69 full-time, locally-nominated delegates who work in those offices year-round.[8] Contact information for the other 401 delegates is not publicly available, and there is no central repository of email addresses or mailboxes to reach them. To conduct our experiment, therefore, we hired a full-time research assistant to locate contact information for every VNA delegate in the sample. Only after several weeks of investigation were we able to compile a full database of usable, professional addresses.

These access costs are augmented by the fact that VNA delegates appear quite wary of relying on external or unofficial information for debate. In seeking to mail our treatments to delegates, we were advised by VNA officials that delegates would be reluctant to open letters from non-official sources. Upon reaching out to multiple prominent Vietnamese NGOs about potential cooperation on treatment delivery, we received similar advice. They did not

[8]The five national-level cities have more than one provincial office representative.

believe envelopes from their organizations would garner the attention of officials. After a long search, we found a willing and extremely capable partner at the Institute of Public Policy and Management at the National Economic University (NEU). As a government-funded institution, NEU has the official status to warrant delegate attention; as a research university, NEU could mail our treatments as research products that might inform policy without fear that delegates would interpret them as official pronouncements on the part of their functional constituency. The difficulty of access combined with an informal norm whereby VNA delegates only pay attention to official government correspondence makes it extremely difficult for those outside the government system to contact delegates and influence policy.

In sum, the VNA is imperfectly designed to be responsive to those outside the government apparatus. Institutional barriers mean that only a certain class of delegates have an official mandate to represent local interests. Recent changes in caucusing rules create barriers to the consolidation of official provincial policy positions and strategies. Inadequate transparency and frequent changes to the official docket make it difficult to mount an organized effort to influence policy prior to a critical debate. It is nearly impossible to know precisely when debate will commence or even to obtain the final draft of the legislation to be debated. There is no public registry of delegate contact information, requiring significant effort to locate one's representatives. Finally, even if a nongovernmental constituent could contact a VNA delegate, it is unlikely that the delegate would be receptive to communications from unofficial sources.

To combat these obstacles, we put substantial effort into our experimental design, discussed below. We raise them here for two reasons. First, transparency about these obstacles is important for understanding the scope conditions of our findings. We find evidence that delegates want more information on citizen preferences and those that receive it appear to employ it in their work. However, the institutional and informal barriers to the reception of such information imply that the responsiveness we observe will occur only under quite

specialized conditions. Second, our findings imply that removing these obstacles would enhance responsiveness in the VNA. In other words, our experiment points toward clear policy solutions for central authorities interested in responsiveness.

4 Research Design

The goal of this project is to measure the responsiveness of legislators in an authoritarian context – the VNA. To that end, we designed a field experiment 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 which informational treatments would be prepared, subject to three constraints. First, we were restricted to the summer 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 Education Law, whose current draft consists of 119 articles addressing all aspects of the educational system. The Law was debated at the May 2018 session, voted on in the October 2018 session, and eventually led to the launch of the new National Education Curriculum in December 2018.

Our intervention focused on the debates over the substance of the Law in May 2018. Topics covered include all 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 the educational system of vital importance to parents, but it also affects the business community via labor quality and training costs. Equally important, these preferences are measured each year via two reputable, nationally representative 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.[9] 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.[10]

Figure 2 displays English translations of the infographics delivered to treated delegates in Nam Dinh province.[11] In recognition of the widely varying educational backgrounds of the delegates themselves, the infographics were kept simple: a title, five key percentages with accompanying illustrations and brief textual explanations, and footnoted source information. We selected two different types of information. Some items reference specific articles scheduled for debate at the VNA session. Bullet 1 for both citizen and firm cards references Articles 27-29 on the goals and quality of general and primary education. Bullets 2 and 3 on the firm card provide information regarding Article 31 on vocational education. Bullet 2 on the citizen card provides information on Article 67 on the funding of school infrastructure and related assets. Bullets 3 and 4 on the citizen card reference Article 80 on fostering professional instructors and Article 70 on the morality criterion for teachers. Bullet 4 also indirectly connects to Article 105 on adequate compensation for teachers as a way to stave off informal charges to students. Other items describe more general perceptions about education in the province and its economic impact (citizen bullet 5 and firm bullets 4 and 5), both of which were related to the debate. Printed infographics were delivered in sealed envelopes that also contained a short, explanatory note on letterhead from NEU's Institute

[9] 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 the funders of the respective projects, in our discussion with VNA officials regarding the experiment, legislators expressed the need for processed and relevant information rather than one-hundred-plus page reports.

[10] Provincial-level informational treatments accord with delegates' mandate to represent their provinces, rather than the sub-provincial districts from which they are elected.

[11] Appendix F. depicts the relative strengths of the citizen and firm treatments across all 63 provinces.

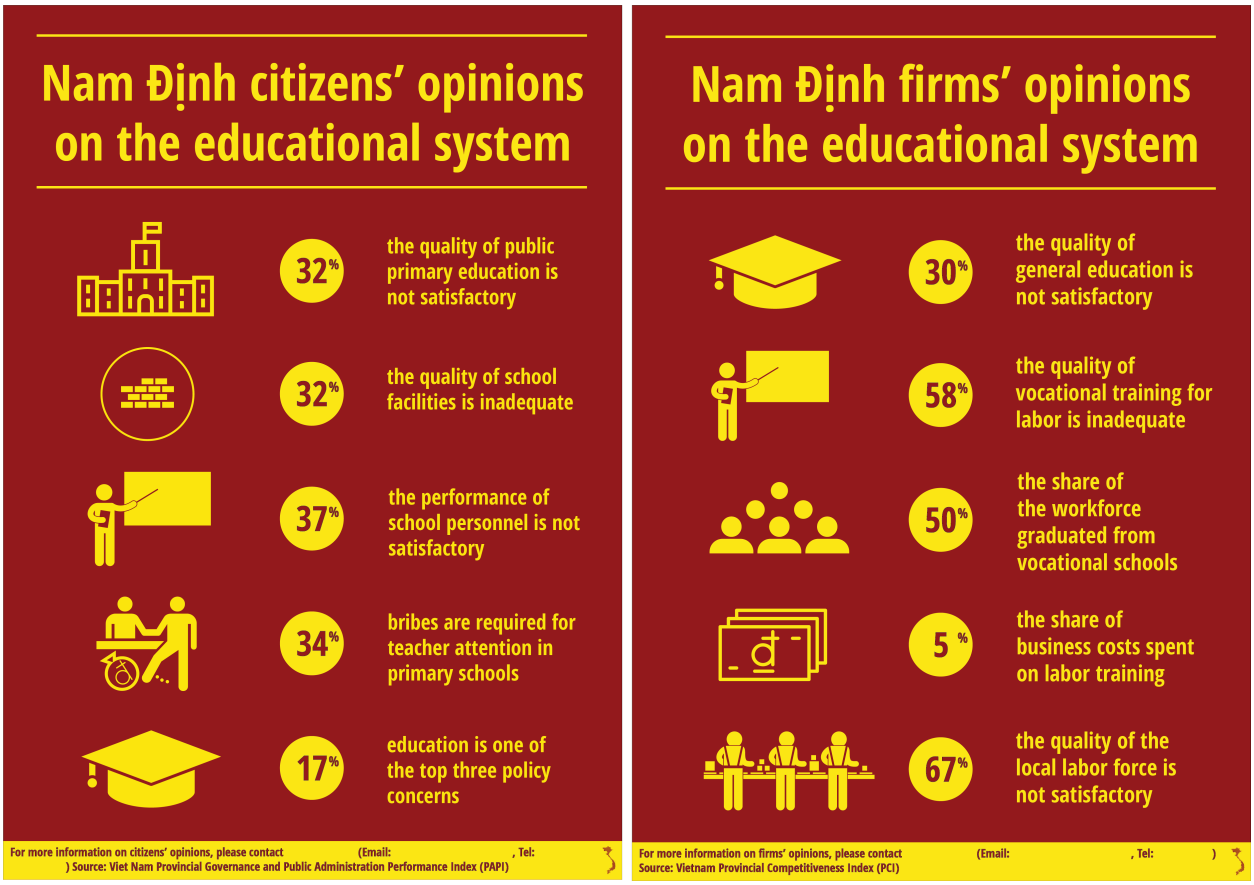

Figure 2: **Sample informational treatments for citizens and firms.** Two sets of statistics were calculated separately for each of Vietnam's 63 provinces.

of Public Policy and Management.

Before discussing the randomization strategy, it will be helpful to sketch out a timetable for the typical VNA session. Each May and October, delegates from across the country convene in Hanoi for a four-week legislative session. They arrive with a pre-arranged legislative docket consisting of one or two dozen bills to be discussed or voted upon. Group caucuses are held the first week, providing delegates from several provinces an opportunity to gather together and take turns voicing opinions on pending legislation.[12] These caucuses are internal affairs, with records ordinarily not released to the public. The following two weeks entail various committee meetings and query sessions in which delegates may question the prime minister, his deputies, or relevant cabinet ministers regarding a particular issue. As

[12]All caucuses involved three or four provinces, except for the two largest provincial-level units (Hanoi and Ho Chi Minh City), which each caucused alone.

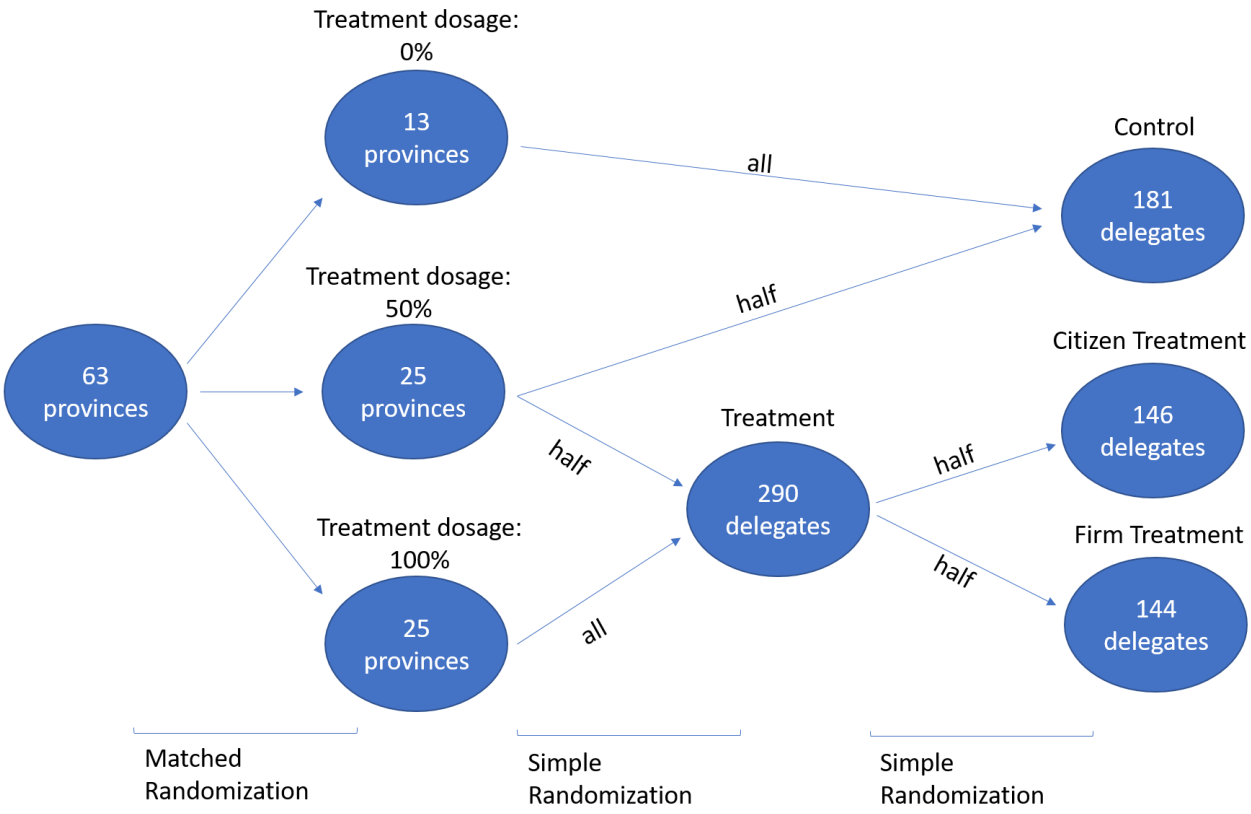

Figure 3: **Schematic representation of the three-stage randomization.** Each province was assigned a dosage, and each delegate to one of three conditions.

previous work has noted, query sessions are highly visible, being broadcast live and recapped on the evening news (Malesky and Schuler, 2010). In the final week of the legislative session, delegates publicly participate in floor debates. Here delegates "with personal expertise. . . are expected to offer their insights on the draft legislation. . . Because of the focus on legislation, speeches in these sessions demand familiarity with the subject and require significant research on the part of the delegate" (Malesky, Schuler and Tran, 2012, 768).

The upshot of this schedule is that the integrity of individual-level treatments is likely compromised by the group caucuses before a delegate ever finds an opportunity to express herself publicly, be it in a query session or a floor debate. Two potential forms of contamination are possible. First, delegates receiving informational treatments may pass along the information to untreated delegates, increasing responsiveness in the control group. We refer to this as a *spillover effect*. Second, treated delegates may discuss their infographics with

other similarly treated delegates, thereby reinforcing the confidence of all in this information. We refer to this as a *reinforcement effect*.[13] Because all downstream outcomes are potentially biased by these dual threats, we adopted a three-stage saturation design for the randomization, shown in Figure 3.

In the first stage, we used a genetic matching algorithm to assign each province to one of three treatment dosages: 0%, 50%, or 100%.[14] All delegates representing provinces assigned to the 100% dosage were thereby 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 of the treatment arms.

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 (EduCareer), a continuous variable marking years of formal education (EduYears), and a categorical variable classifying delegates by highest level of educational attainment (EduLevel: 1=high school, 2=bachelor's, 3=master's, 4=doctorate).

Three weeks prior to the summer 2018 VNA session, NEU mailed each treated delegate an infographic presenting the preferences of citizens or firms within her province. Delegates assigned to the control condition received no information whatsoever other than the basic

[13]We did not anticipate the reinforcement effect in our pre-analysis plan (PAP), and discovered it only after analyzing the results of the saturation design. Readers should treat estimates of the reinforcement effect as a more speculative finding than tests of the spillover effect.

[14]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) logged 2014 GDP, (9) 2014 population, (10) 2014 GDP per capita, and (11) 2016 fiscal transfers.

	Control (N=181)			Citizen (N=143)			Firm (N=146)		
	Mean	SD	NA	Mean	SD	NA	Mean	SD	NA
FullTime	0.343	0.476		0.343	0.476		0.336	0.474	
CentNom	0.199	0.400		0.224	0.418		0.185	0.390	
Competitive	0.552	0.499		0.510	0.502		0.507	0.502	
EduCareer	0.039	0.193		0.035	0.184		0.021	0.142	
EduYears	11.343	0.951		11.273	0.965		11.068	1.061	
EduLevel	2.856	0.761		2.867	0.833		2.829	0.825	
Prepared	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	
Said province	0.028	0.164		0.091	0.288		0.075	0.265	

	Control-Citizen		Control-Firm		Citizen-Firm	
	p-value	t-stat	p-value	t-stat	p-value	t-stat
FullTime	0.998	-0.002	0.896	0.131	0.900	0.126
CentNom	0.588	-0.542	0.750	0.318	0.415	0.817
Competitive	0.454	0.750	0.413	0.820	0.951	0.062
EduCareer	0.860	0.176	0.330	0.975	0.458	0.743
EduYears	0.516	0.651	0.016	2.431	0.088	1.712
EduLevel	0.904	-0.120	0.756	0.311	0.694	0.393
Prepared	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
Said province	0.020	-2.340	0.058	-1.902	0.633	0.478

Table 1: **Summary statistics and balance.** Randomization achieved balance across treatment conditions.

information about VCP objectives for the Law provided to all delegates by the library. In late May, delegates gathered in Hanoi and, on May 30, they attended group caucuses focused on the pending education bill. The following week, on June 6, the Vietnamese populace watched a nationally-televised query session, as delegates engaged the minister of education and a vice premier over the issue of education. The very next day, our implementation partner, the VNA Library, administered a paper-based survey (discussed further below) to all delegates. Finally, on June 11, delegates took to the floor of the assembly to debate proposed revisions

to the Law on Education.

The following subsections introduce and discuss our measures of responsiveness, present regression specifications, and preview the interpretation of later results.

4.1 Survey Outcomes

Our primary outcome derives from the delegate survey, administered by our implementation partner, the VNA Library.[15] The survey covered three bills from the summer 2018 legislative agenda, asking whether the delegate is prepared to debate each bill.[16] Delegates who indicate that they have made up their minds on the education bill are coded as being responsive. To understand why, it is important to recall two of the assumptions behind our experimental approach. First, we assume that the average VNA delegate in fact desires to be responsive to her constituents. Just as important, however, this delegate likely lacks the relevant information regarding 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 informational treatments 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 make up their minds at higher rates, responsiveness to constituents then becomes the most natural interpretation.

To test this claim, we regress a dichotomous indicator for preparedness on three delegate-

[15] See Appendix A. for the survey instrument, in the original Vietnamese and English translation.

[16] Our original pre-analysis plan (PAP) called for the survey to be sent out prior to the group caucuses and its concomitant threat of spillover, but the VNA 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 dosages. 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).

level covariates, individual treatment assignments, provincial shares of treated delegates, and interactions between treatment assignments and treated shares. More concretely, we run a linear probability model[17] with the following specification, where i indexes delegates and p provinces:

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

where $\mathbf{X}$ denotes indicators for full-time, central nomination, and competitive elections, which both theory and prior work suggest may influence responsiveness. β_0 is the constant, representing the share of untreated delegates in untreated provinces answering that they are prepared for debate. β_1 (β_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, β_2 (β_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 a control delegate grows increasingly likely to feel prepared for debate as the share of treated peers rises. Lastly, positive estimates for β_3 (β_6) would indicate a reinforcement effect, meaning that a rising proportion of similarly treated peers increases the odds that a treated delegate is prepared. Our OLS specifications consist of a baseline with treatment dummies only, a second model adding covariates, and a third model introducing the treatment shares and interactions.[18]

4.2 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

[17] We employ the linear probability model for ease of interpretation, and because the marginal effects estimated by OLS, logit, and probit are often very similar (Angrist and Pischke, 2008). As a robustness check, probit results are included in Appendix E.

[18] Although the pre-analysis plan called for the addition of provincial fixed effects, this proved impossible due to the provincial-level dosage design. Realizing this belatedly, we opt instead to cluster the standard errors within each province.

actual responsiveness. To supplement the survey, then, we analyze the pooled transcripts from three distinct legislative settings.[19] 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. 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 without covariates, with covariates, and with treatment share interactions.

If delegate speech is a measure of responsiveness, then closer scrutiny of the content of those remarks should yield more refined measures of responsiveness. Our primary results include an indicator for whether a delegate mentioned her own province, as this would plausibly accompany a discussion of the information contained in the treatments. Appendix C presents additional analyses examining constituency synonyms and particular articles from the Education Law. 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 provides details on the issues that delegates dared to raise during the debate, and can shed light on whether issues raised in the cards entered their speeches.

[19]Similar analyses treating each legislative forum separately appear in Appendix C.

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 sub-setting 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 no information was shared and the threat of spillover was rendered moot, 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.

5 Results and Analyses

Before presenting and interpreting our results, we must 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 is best suited to "low information" settings such as those with complex randomization procedures, binary outcomes, clustering of observations, or a small number of observations (Bowers and Panagopoulos, 2011). Because we face all of these potential obstacles at the same time, we bring randomization inference to bear in assessing the statistical significance of our results.

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

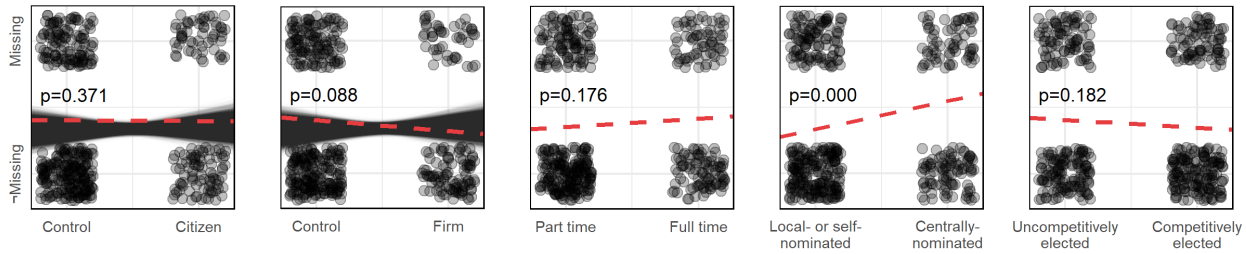

Figure 4: **Investigating survey nonresponse.** Each panel presents the bivariate relationship between a covariate or treatment and survey nonresponse.

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 substantial rate of survey nonresponse evident in Table 1. Although the causes of this nonresponse are unknown and may vary idiosyncratically, we examine in Figure 4 the relationships between missingness and both treatments and covariates.[20] 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 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 have a fundamentally

[20]It should be noted that 15% of NAs are in fact not missing, but instead cannot be matched to individual delegates due to clerical errors.

different mandate and therefore are less interested in representing provincial issues in debates. They also 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 Appendix D.

5.1 Direct Treatment Effects on Survey Outcomes

Did delegates in fact exhibit responsiveness? To answer that question, we now present the results of the delegate survey, first with t-tests and then via randomization inference. The bottom panel of Table 1 provides the unadjusted differences in three individual-level outcome variables. It is clear that delegates in the citizen treatment 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). These are statistically significant at the 0.01, 0.1, and 0.05 levels, respectively. By contrast, the firm treatment group is only marginally different from the control group, and only when considering the propensity to name the home province, but even this result could be influenced by non-random selection into speaking.

Figure 5 displays the direct effects of the informational treatments that emerge from randomization inference, illustrating two primary specifications (top row) and two robustness checks (bottom row). Each panel presents the actual experimental coefficients numerically, via a short vertical segment, and as circles colored to indicate statistical significance; also present is a density plot of the re-randomized coefficients. Under the baseline regression in the upper-left 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 (upper-right 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, the lower-left panel excludes centrally-nominated delegates, for whom responsiveness is theoretically more attenuated. Here as well we find that both treatment effects remain stable.

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[21] 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, 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 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

[21] 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.

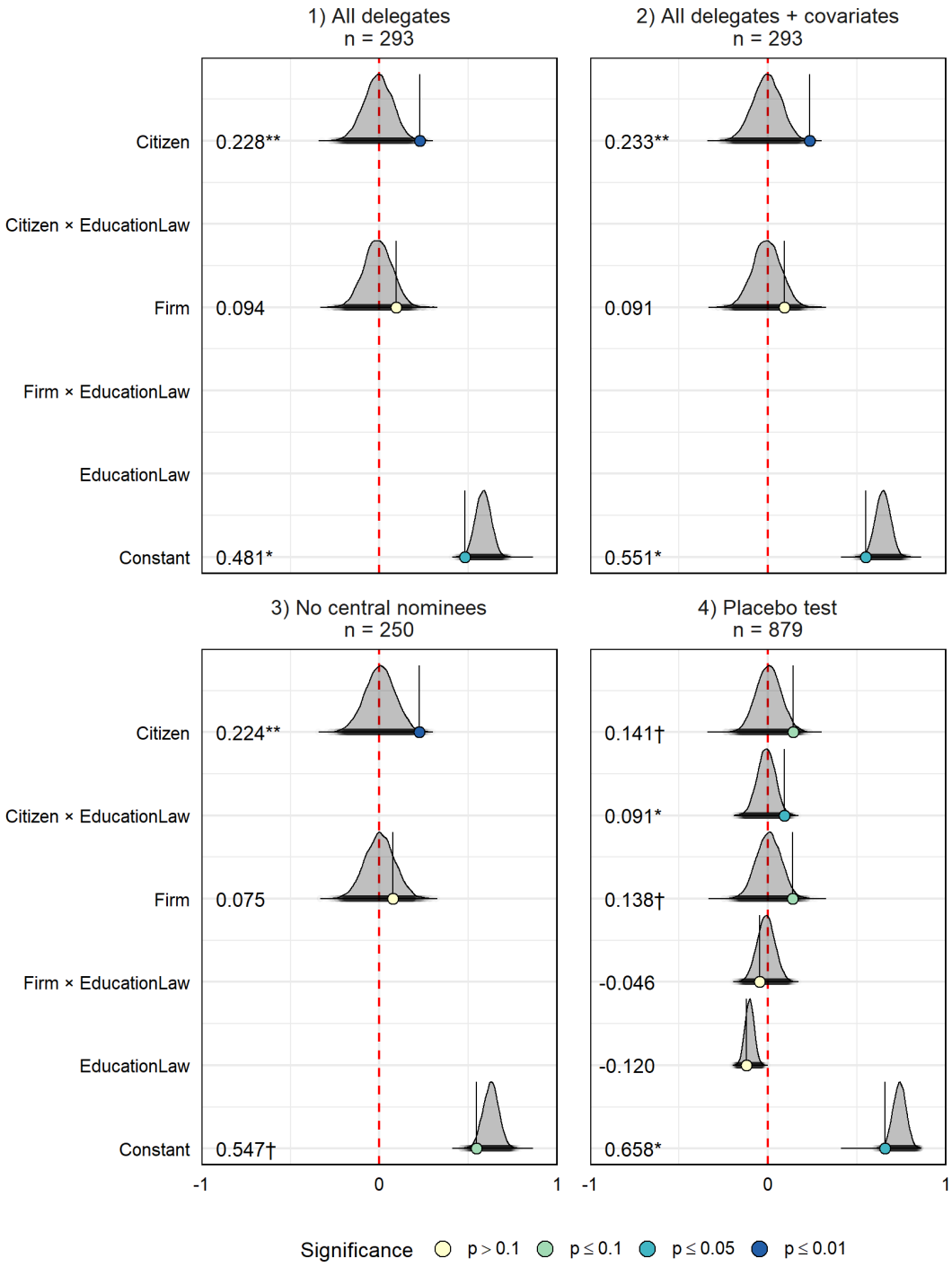

Figure 5: **Direct treatment effects on debate preparation.** Citizen-treated delegates were more likely to feel prepared to debate the Education Law. OLS coefficients appear at the left margin, with statistical significance assessed by 10,000-replicate randomization inference. Density plots display replicate coefficients.

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. To summarize the results thus far, delegates appear responsive to the preferences of their citizens, but not to those of local firms.

5.2 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 three 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.

Figures 5 and 6 make clear that presenting delegates with the preferences of their citizens increased the likelihood of responsiveness, be it through debate preparation or speaking. Figure 7 takes this one step further, examining in greater detail where these direct effects on speaking probability are manifested. Group caucuses are internal party-state affairs; query sessions and floor debate occur in the public glare, yet only the latter event is specific to a particular piece of legislation.

In Figure 7 we find that 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. Neither treatment affected responsiveness in the floor debate or caucuses, and the

firm treatment brought about no effect in the query sessions.

5.3 Testing Spillover and Reinforcement

The results presented thus far paint a picture of delegates apt to respond only when presented with the preferences of their local citizens – not 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. We address the possibility of spillover with a saturation design, instituting a three-stage randomization and a multiplicative interaction modelling approach. 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 on citizens' (firms') will be shared.

We begin exploring these multiplicative interactions by returning to the survey results and debate preparation. The left panel of Figure 8 displays the marginal effects of the citizen

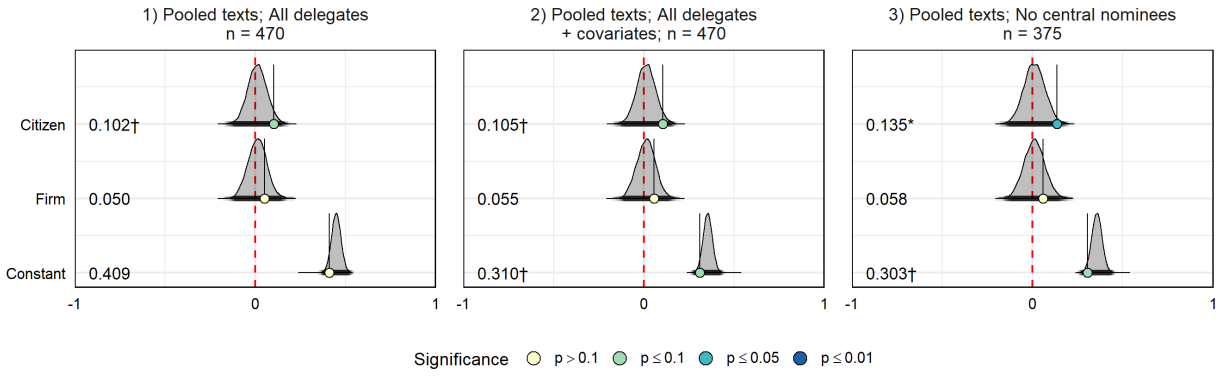

Figure 6: **Direct treatment effects on speech in any forum.** Citizen-treated delegates were more likely to speak in one or more of three legislative settings: group caucuses, query sessions, and floor debate. OLS coefficients appear at the left margin, with statistical significance assessed by 10,000-replicate randomization inference. Density plots display replicate coefficients.

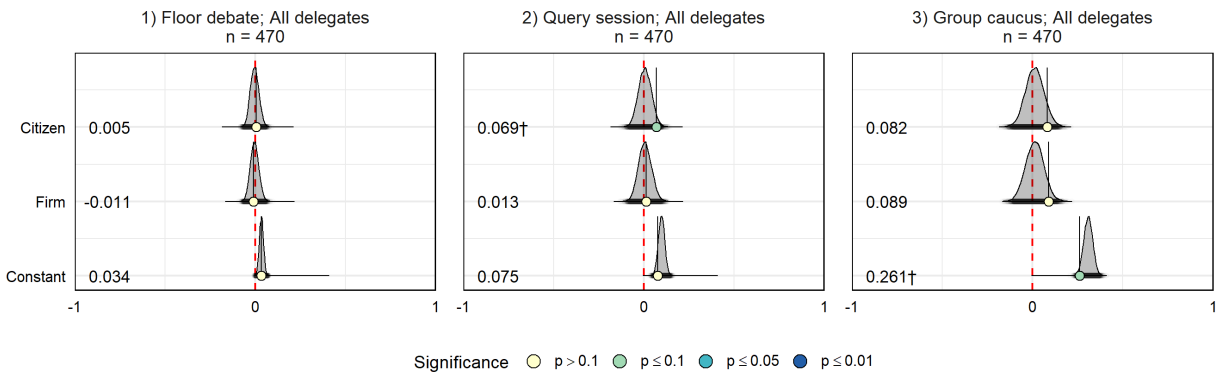

Figure 7: **Direct treatment effects on speech in specific fora.** Citizen-treated delegates were more likely to speak in nationally televised query sessions. OLS coefficients appear at the left margin, with statistical significance assessed by 10,000-replicate randomization inference. Density plots display replicate coefficients.

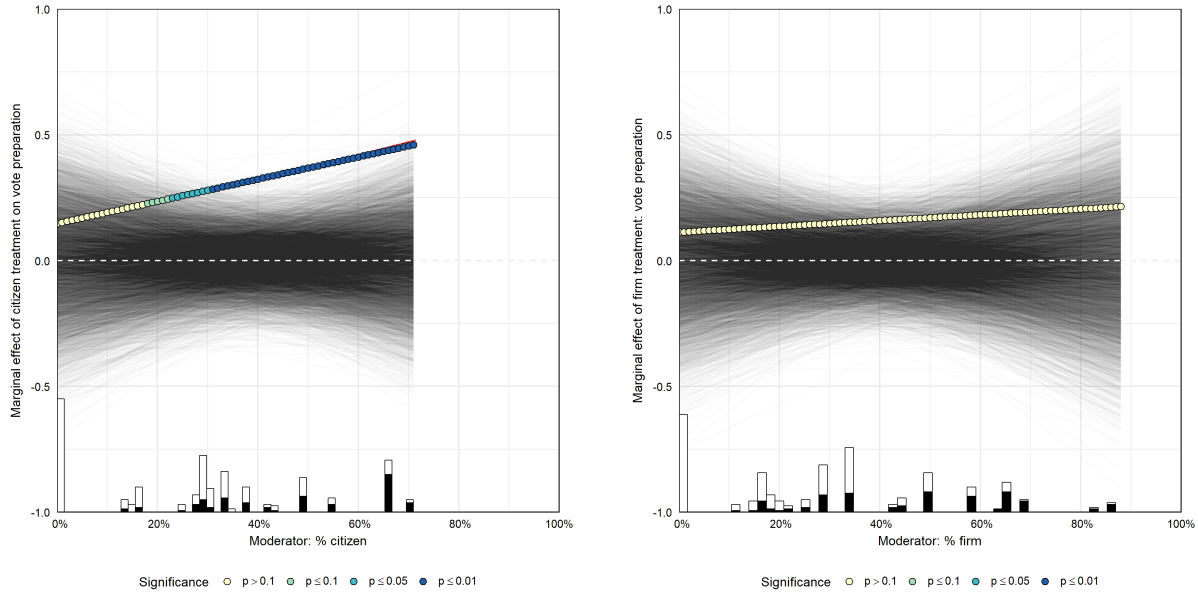

Figure 8: **Marginal treatment effects on debate preparation.** The greater the share of citizen-treated delegates in a province, the higher is the likelihood that each is prepared to debate the Education Law. Obscured kernel estimator (red) confirms assumption of linear interaction effect. Statistical significance assessed by 10,000-replicate randomization inference.

treatment over the observed range of 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 the 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 by our experimental estimate indicates that the LIE assumption is clearly met. 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% saturation, it is otherwise satisfactory.

Substantively, the left panel of Figure 8 implies that citizen-treated delegates in the provinces at the highest observed levels of citizen dosage were nearly 50 percentage points more likely to make up their minds than their firm-treated or untreated peers in the same provinces. Even at low dosages, the marginal effect of citizen infographics is estimated to equal 10-20 percentage points. The positive slope of this marginal effect is indicative of a reinforcement effect: the greater the share of delegates in a province receiving the citizen treatment, the higher is the likelihood that each feels prepared to debate the education bill. Regression results presented in Appendices C. and D. reveal a negative yet insignificant coefficient on %Citizen, suggesting negative spillover among those not receiving the citizen treatment: increasing the provincial share of citizen-treated from 0% to its observed maximum of 71% is associated with a nearly 25 percentage point decline in the probability of debate preparation among firm-treated and untreated delegates.

The right panel of Figure 8 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 appears to hold once again,

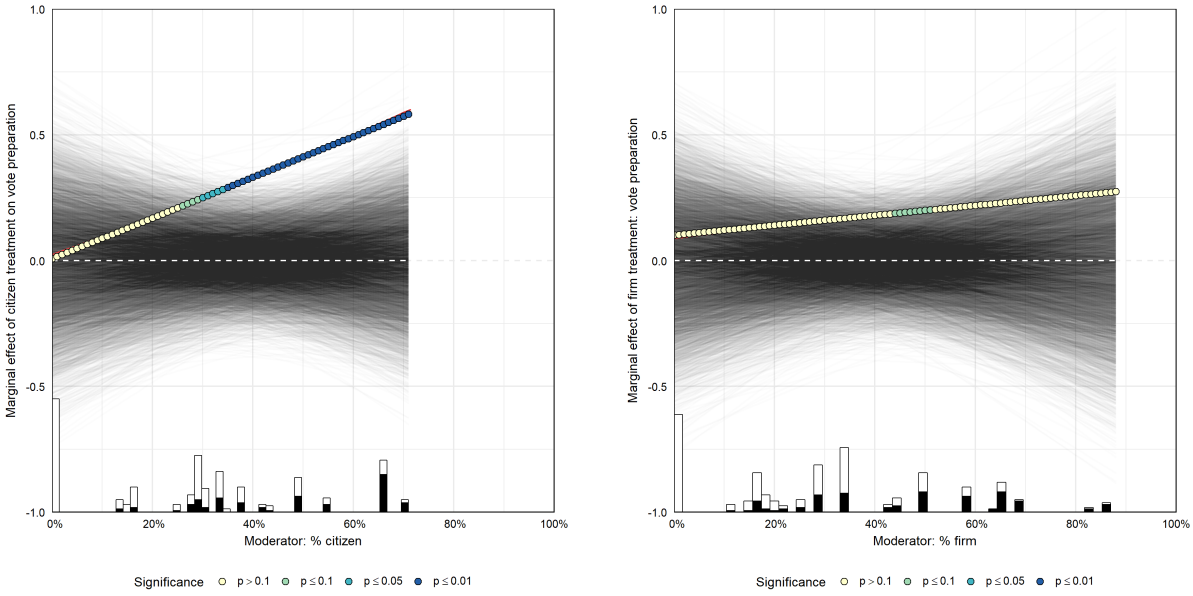

Figure 9: **Marginal treatment effects on debate preparation among local nominees.** The greater the share of citizen-treated delegates in a province, the higher is the likelihood that each is prepared to debate the Education Law. Obscured kernel estimator (red) confirms assumption of linear interaction effect. Statistical significance assessed by 10,000-replicate randomization inference.

increasing the firm dosage from 0% to its observed maximum of 88% raises the probability that a firm-treated delegate was prepared to debate by only ten percentage points. At their highest observed dosages, the marginal effect of 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 will induce responsiveness.

Figure 9 replicates the analysis of Figure 8 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 local- and

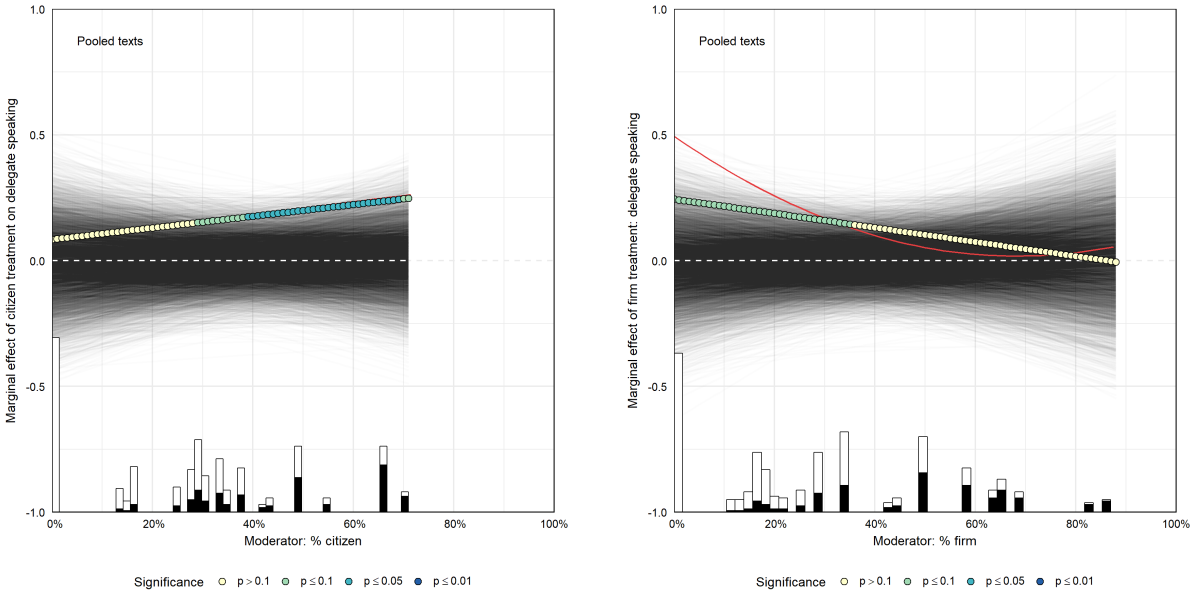

Figure 10: **Marginal treatment effects on speech in any forum.** The greater (smaller) the share of citizen- (firm-) treated delegates in a province, the higher (lower) is the likelihood that each is prepared to debate the Education Law. In the left panel, an obscured kernel estimator (red) confirms assumption of linear interaction effect, while in the right panel the LIE assumption clearly does not hold. Statistical significance assessed by 10,000-replicate randomization inference.

self-nominated delegates. As noted in Appendices C. and D., the negative spillover effect of the citizen dosage on those not receiving the citizen treatment remains, and is in fact strengthened. While we blocked on central nominees during the randomization, excluding them from analyses was not part of our PAP and such inferences should therefore be treated with caution. As a further robustness check, Appendix D. 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.

5.4 Textual Outcomes

Returning to the transcripts from three legislative fora, we begin with the pooled texts. Figure 10 presents the marginal treatment effects on the likelihood of a delegate speaking 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.[22] 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 Appendix C.). Increasing the citizen dosage to its observed maximum essentially triples the marginal effect, from approximately 0.08 to 0.27.

In Appendix C., we drill down to the marginal effects across separate fora. Although the marginal effects of the firm treatment are negligible, the citizen treatment produces a significant and linearly increasing marginal effect once a majority of delegates receive it. This, combined with negligible or null effects in the query session and caucuses, matches our theoretical intuition regarding the publicized and bill-specific focus of floor debates. Regression results reported in the Appendix D reveal negative spillover in floor debates and group caucuses, but not in the query sessions.

If the citizen treatment not only causes delegates to make up their minds at higher rates, but also prompts them to speak at higher rates, can further evidence of responsiveness be gleaned from the contents of their remarks? Appendix C. presents several provincial-level approaches to this question, examining a delegation's remarks for mention of their province, synonyms for citizen and firm constituencies, and specific articles from the debated legislation. We find that increasing a province's citizen treatment share significantly raises the probability that one or more of its delegates mention the province. Increasing the treatment share from 0% to the observed maximum of 71% raises this probability by 41 percentage points (0.581×0.71), a result which holds for the pooled texts and caucus texts. Next, we looked for direct citation of the cards in debates by searching for mentions of articles. In this more precise analysis, both the citizens and firms treatment yielded null

[22]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.

Figure 11: **Structural topic modelling of delegate speech.** Treated delegates were more likely to discuss the topic characterized by treatment-related keywords.

results.

Our final approach to assessing speech content for treatment effects is the structural topic model (STM), which allows us to discover the topics discussed by delegates while simultaneously estimating the effects of the informational treatments on the prevalence of these topics (Roberts et al., 2014). Because initial exploration revealed that delegates' flowery and highly formulaic phrasing produced substantively useless topics, each delegate-forum speech was then read and summarized with an open-ended set of keywords by a native Vietnamese speaker who was not informed of the treatment conditions or purpose of the exercise. We then estimated a two-topic model on these keyword summaries, allowing the relative prevalence of each topic vary as a function of the treatment assignments, the leg-

islative forum involved, and our standard delegate covariates. Due to nonrandom selection into speaking – which we know to be correlated with our informational treatments – and the threat of post-treatment bias, the results should be treated with caution. Figure 11 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", and "decision-making authority". It is worth noting that the effect of the firm treatment, while statistically significant, is less than half that of the citizen treatment, a finding consistent with our other results. Because the representative terms of topic two are far more relevant to the informational treatments we provided, we take this as circumstantial evidence that the treatments affected responsiveness through speech content.

5.5 Discussion

To summarize the results, we find that providing VNA delegates with the preferences of their provincial citizenry significantly improves their debate preparation and likelihood of speaking up, yet similar provision of the preferences of local business communities yields no effects. Using a multiplicative interaction to model the threat of spillover, we uncover strong reinforcement effects – and weak negative spillover – from the citizen treatment. As knowledge of citizens' preferences becomes more widespread within a provincial delegation, its delegates exhibit greater confidence in their debate preparation and ability 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.

There is very little evidence of delegates acquiring new (unprovided) information from one another through spillover. Closer scrutiny of the contents of delegates' remarks yields mixed

evidence of responsiveness to citizens, with almost no significant evidence of responsiveness to firms. Finally, the substantial share of null results on the revelation of precise information highlights the fact that, overall, responsiveness is apparent but relatively weak in the context of the VNA.

6 Conclusion

Our paper represents the first randomized experiment on legislator responsiveness in an authoritarian context, permitting direct testing of the causal link between a national legislator's knowledge of constituents' preferences and her consequent legislative behavior. We contribute to the debate over authoritarian responsiveness by answering two questions – are such legislators responsive and, if so, to whom? We find that delegates are indeed responsive, but only to signals of citizens' preferences – not to firms. Citizen-treated delegates were 23 percentage points more likely than the control group to feel prepared for debate, 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, were not significantly different from the control group on either measure. Second, while we find no evidence of spillover effects among untreated delegates, we do find substantial evidence of reinforcement effects. Treated delegates felt more prepared and were more likely to speak as the number of similarly treated delegates in their locality increased. Turning to more fine-grained measures of responsiveness, we again find that the higher the provincial share of delegates receiving the citizen treatment, the greater the likelihood that a delegation member mentioned the province's name in group caucuses on the law. Finally, we find that citizen-treated delegates were 25 percentage points more likely to focus their stated remarks on keywords presented in our informational treatments. Although we cannot distinguish between the public spiritedness and upward accountability arguments, analyses of heterogeneous treatment effects in Appendix F. offer no support for an electoral accountability mechanism.

While these findings move the literature forward, they are limited somewhat by the artificiality of our research approach. First, we estimate a 14 percentage point Hawthorne effect on self-reported 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 simulate how citizens, firms, and other non-state actors would actually interact with the parliamentary representatives of their provinces. As we documented in Section 3.1 of the paper, institutional, informational, and access barriers insulate delegates from direct interaction with the public they nominally represent. As we show in conducting the experiment, overcoming these barriers is possible, but it requires significant time, effort, resources, and high-level connections that the average citizen does not possess. Once contacted, delegates do appear to 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.

To truly achieve responsiveness under these conditions, however, it is clear that significant reforms in the VNA are necessary. These reforms include: 1) the abolition of central nominees, who have no mandate to be responsive to citizens; 2) the removal of rules restricting caucusing by provincial or other functional groups; 3) adherence to the posted legislative docket and the immediate, public posting of any emergency changes; 4) the publication of delegate contact information; and 5) the cultivation of a social norm that non-official data and analysis are appropriate for parliamentary debates. Whether or not the VNA moves in this direction will reveal much about how it balances its conflicting mandates of providing expertise to key debates, informing central elites about local issues and beliefs about the regime, and responding to the needs of constituents.

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

7 Online Appendix

A. Delegate survey administered by VNA Library

B. Transcript coding and dictionary

C. OLS results assessed by randomization inference

D. OLS results with province-clustered standard errors

E. Probit results with province-clustered standard errors

F. Treatment strength and heterogeneous treatment effects

G. Controlling for education

A. Delegate Survey Administered by VNA Library

Vietnamese original:

1. Đại biểu thấy còn băn khoăn về dự Dự luật Giáo dục sửa đổi?
   - ☐ Không còn băn khoăn
   - ☐ Còn băn khoăn *(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).

B. 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 Figure 14 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".

Col	Name	Vietnamese Regex	English Regex
1	Citizens	(ý kiến)?(bầu cư|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

C. OLS Results Assessed by Randomization Inference

Tables 2, 3, 4, and 5 contain the primary regression results used in the paper, as well as supplementary regressions with finer-grained content-based measures of responsiveness. Rather than standard errors, the parenthetical quantity underneath each regression coefficient represents that coefficient's quantile as compared to 10,000 coefficients estimated from repeated randomizations. In other words, a parenthetical value of, say, 0.999 means that our experimental estimate is larger than 99.9% of the coefficients resulting from new treatment assignments produced by our three-stage randomization procedure. Stars and daggers represent standard levels of statistical significance as applied to these quantiles, and the auxiliary statistics in the bottom rows refer to regressions on the observed data.

The six panels of Figure 12 present the marginal treatment effects on delegate speech separately for each treatment and each legislative forum. Beginning with the firm treatment (bottom row), the marginal effects are negligible. In floor debates (left panel), the effect is substantively small; in query sessions (middle panel), it is indistinguishable from zero; and within group caucuses (right panel), the kernel estimator refutes the LIE assumption and therefore the validity of the result, which is positive yet decreasing in the firm dosage. Turning to the citizen treatment (top row), theory indicates that the greatest effects should be found within the floor debates. Unlike the caucus sessions, debates are conducted publicly and broadcast live; unlike the query sessions, they center on the legislation targeted by the experimental treatments. In fact, we find in floor debates (left panel) a negative effect of the citizen treatment at 0% dosage, but this sharply and linearly increases in the proportion treated, peaking around 0.17 at the maximum observed dosage. We also find a positive and linearly increasing marginal effect in group caucuses, but a weakly decreasing effect in query sessions.

Figure 13 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, the upper-left panel displays a strong effect of the citizen treatment, with an increase in dosage from 0% to the observed maximum of 71% yielding an increase of 37 percentage points (0.525×0.71). Yet again, we see no evidence for a treatment effect of the firm infographics. Adding covariates (upper-right panel), 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. The lower-left panel reveals no effect on the query sessions – understandable given the diffuse focus of these events – while the lower-right panel 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 Figure 14, examine the floor debate and group caucus transcripts for finer-grained indicators of responsiveness. The first two panels look at terms synonymous (or nearly so) with citizens or firms, and the remaining panels 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 dosage of the citizen treatment significantly reduced the likelihood that a delegation member would mention firms, yet failed to increase the probability of mentioning citizens.

	All delegates			No central nominees		
	(1) Baseline	(2) Covariates	(3) Saturation	(5) Baseline	(6) Covariates	(7) Saturation
Citizen	0.228**	0.233**	0.148	0.219**	0.224**	0.008
	(0.999)	(0.999)	(0.773)	(0.996)	(0.996)	(0.513)
% Citizen			−0.343			−0.428
			(0.087)			(0.054)
Citizen × % Citizen			0.441			0.810†
			(0.872)			(0.970)
Firm	0.094	0.091	0.113	0.081	0.075	0.102
	(0.905)	(0.892)	(0.715)	(0.826)	(0.806)	(0.688)
% Firm			0.001			−0.072
			(0.508)			(0.391)
Firm × % Firm			0.115			0.196
			(0.624)			(0.677)
FullTime		−0.101	−0.120*		−0.101	−0.134*
		(0.398)	(0.021)		(0.369)	(0.002)
CentNom		0.059	0.074			
		(0.157)	(0.724)			
Competitive		−0.086†	−0.093		−0.064†	−0.069
		(0.047)	(0.066)		(0.033)	(0.075)
Constant	0.481*	0.551*	0.598	0.489*	0.547†	0.615
	(0.008)	(0.012)	(0.214)	(0.024)	(0.036)	(0.441)
Observations	293	293	293	250	250	250
R^2	0.035	0.048	0.059	0.032	0.043	0.066
RMSE	0.485	0.482	0.479	0.486	0.483	0.477

Note: Randomization inference based on 10,000 randomizations. Quantile of experimental estimate in parentheses. †p<0.1; *p<0.05; **p<0.01.

Table 2: Forms the basis of Figures 5, 8, and 9.

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†	0.105†	0.084	−0.105**	0.158*	0.073	0.135*	0.229†
	(0.958)	(0.963)	(0.701)	(0.002)	(0.991)	(0.694)	(0.986)	(0.972)
% Citizen			−0.268**	−0.164**	0.004	−0.192**		−0.232**
			(0.000)	(0.000)	(0.244)	(0.000)		(0.000)
Citizen × % Citizen			0.231	0.392**	−0.222	0.196		−0.045
			(0.922)	(1.000)	(0.137)	(0.881)		(0.590)
Firm	0.050	0.055	0.245*	0.072	0.020	0.235*	0.058	0.230*
	(0.775)	(0.802)	(0.992)	(0.922)	(0.558)	(0.993)	(0.792)	(0.976)
% Firm			0.023	−0.053	0.043	−0.064		0.022
			(0.311)	(0.401)	(0.427)	(0.060)		(0.240)
Firm × % Firm			−0.283	−0.053	−0.051	−0.163		−0.264
			(0.204)	(0.462)	(0.448)	(0.343)		(0.271)
FullTime		0.132	0.129	0.007**	−0.012*	0.140	0.133	0.137
		(0.823)	(0.482)	(0.003)	(0.983)	(0.250)	(0.876)	(0.905)
CentNom		0.030	0.032	−0.016*	0.046*	0.016		
		(0.130)	(0.351)	(0.992)	(0.012)	(0.744)		
Competitive		0.087	0.085	0.050	0.073	0.034	0.080	0.076
		(0.853)	(0.427)	(0.765)	(0.179)	(0.553)	(0.452)	(0.145)
Constant	0.409	0.310†	0.340	0.059	0.071	0.291	0.303†	0.329
	(0.067)	(0.047)	(0.711)	(0.857)	(0.468)	(0.686)	(0.033)	(0.632)
Observations	470	470	470	470	470	470	375	375
R^2	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: Randomization inference based on 10,000 randomizations. Quantile of experimental estimate in parentheses. †p<0.1; *p<0.05; **p<0.01.

Table 3: Forms the basis of Figures 6, 7, and 10.

Transcripts:	Pooled		Query	Caucus
	(1) Baseline	(2) Covariates	(3) Covariates	(4) Covariates
% Citizen	0.525†	0.581†	0.022	0.581*
	(0.954)	(0.966)	(0.503)	(0.982)
% Firm	0.007	−0.060	−0.045	−0.061
	(0.367)	(0.263)	(0.291)	(0.299)
% FullTime		−2.295	0.164	−2.948
		(0.764)	(0.421)	(0.775)
% CentNom		2.309	−0.205	2.992
		(0.343)	(0.620)	(0.354)
% Competitive		0.141*	0.150	0.034*
		(0.992)	(0.608)	(0.994)
Constant	0.155	0.428†	−0.036	0.541*
	(0.103)	(0.047)	(0.671)	(0.024)
Observations	63	63	63	63
R^2	0.082	0.154	0.049	0.194
RMSE	0.446	0.428	0.208	0.406

Note: Randomization inference based on 10,000 randomizations. Quantile of experimental estimate in parentheses. The unit of analysis is the province. †p<0.1; *p<0.05; **p<0.01.

Table 4: Forms the basis of Figure 13.

	(1) Citizens	(2) Firms	(3) Art. 27–29	(4) Art. 31	(5) Art. 70	(6) Art. 105
% Citizen	−0.296	−0.593*	0.015	0.063	0.047	0.127
	(0.060)	(0.025)	(0.393)	(0.680)	(0.424)	(0.692)
% Firm	0.260	0.299	0.228	0.121	0.176	−0.139
	(0.836)	(0.895)	(0.704)	(0.824)	(0.612)	(0.284)
% FullTime	−0.754	0.827	−1.985	0.342	0.574	−0.957
	(0.785)	(0.685)	(0.816)	(0.892)	(0.812)	(0.296)
% CentNom	0.993	−0.907	2.287	−0.496	−1.660	0.697
	(0.116)	(0.166)	(0.134)	(0.080)	(0.144)	(0.769)
% Competitive	0.061	−0.129*	−0.175	−0.009	−0.121	0.197
	(0.053)	(0.014)	(0.654)	(0.894)	(0.715)	(0.732)
Constant	0.856	0.669	0.884	−0.038	0.388	0.387
	(0.826)	(0.885)	(0.338)	(0.062)	(0.370)	(0.490)
Observations	63	63	63	63	63	63
R^2	0.069	0.093	0.068	0.052	0.088	0.048
RMSE	0.379	0.462	0.465	0.171	0.416	0.441

Note: Randomization inference based on 10,000 randomizations. Quantile of experimental estimate in parentheses. Transcripts from floor debates and group caucuses only. The unit of analysis is the province. †p<0.1; *p<0.05; **p<0.01.

Table 5: Forms the basis of Figure 14.

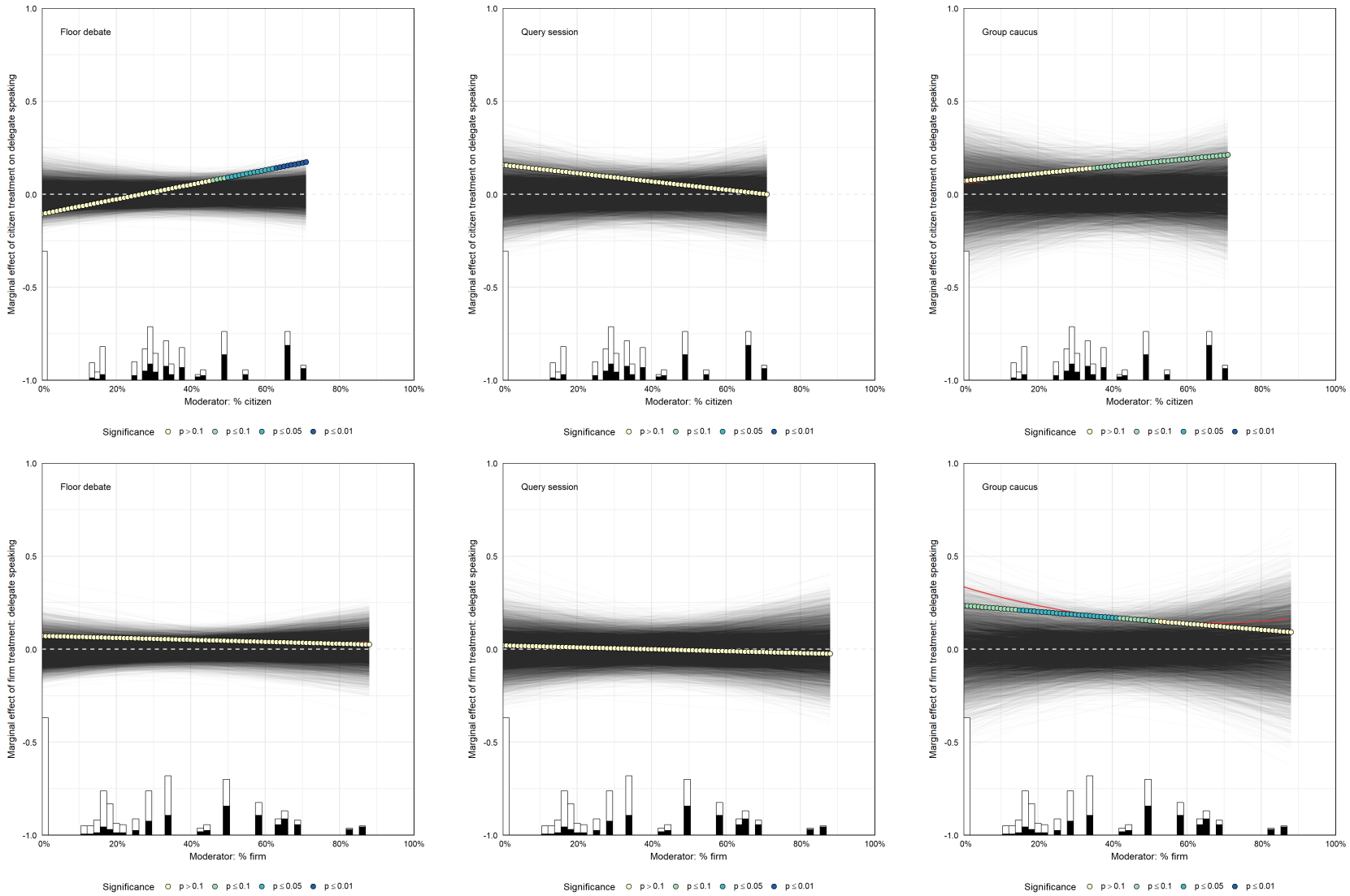

Figure 12: **Marginal treatment effects on speech across fora.** Marginal effects of the citizen treatment (top) increase in the treatment dosage in floor debate (left) and group caucuses (right), but the effect is reversed in query sessions (middle). Marginal effects of the firm treatment are substantively smaller (left), indistinguishable from zero (middle), or incompatible with the LIE assumption (right, red). Statistical significance assessed by 10,000-replicate randomization inference.

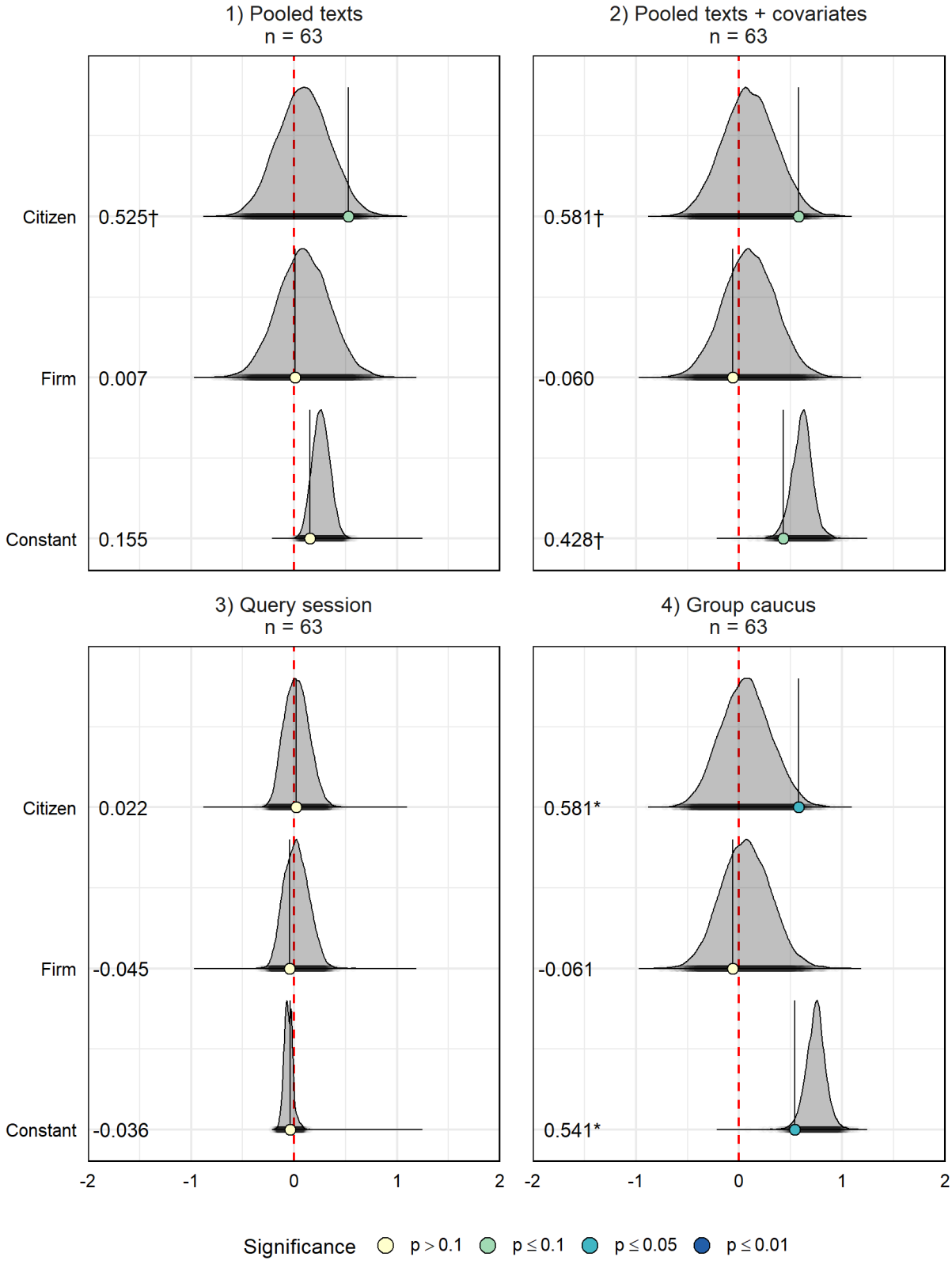

Figure 13: **Citizen treatment increases likelihood that delegation mentions provincial constituency.** Provincial-level regressions of own-province mentions on treatment shares.

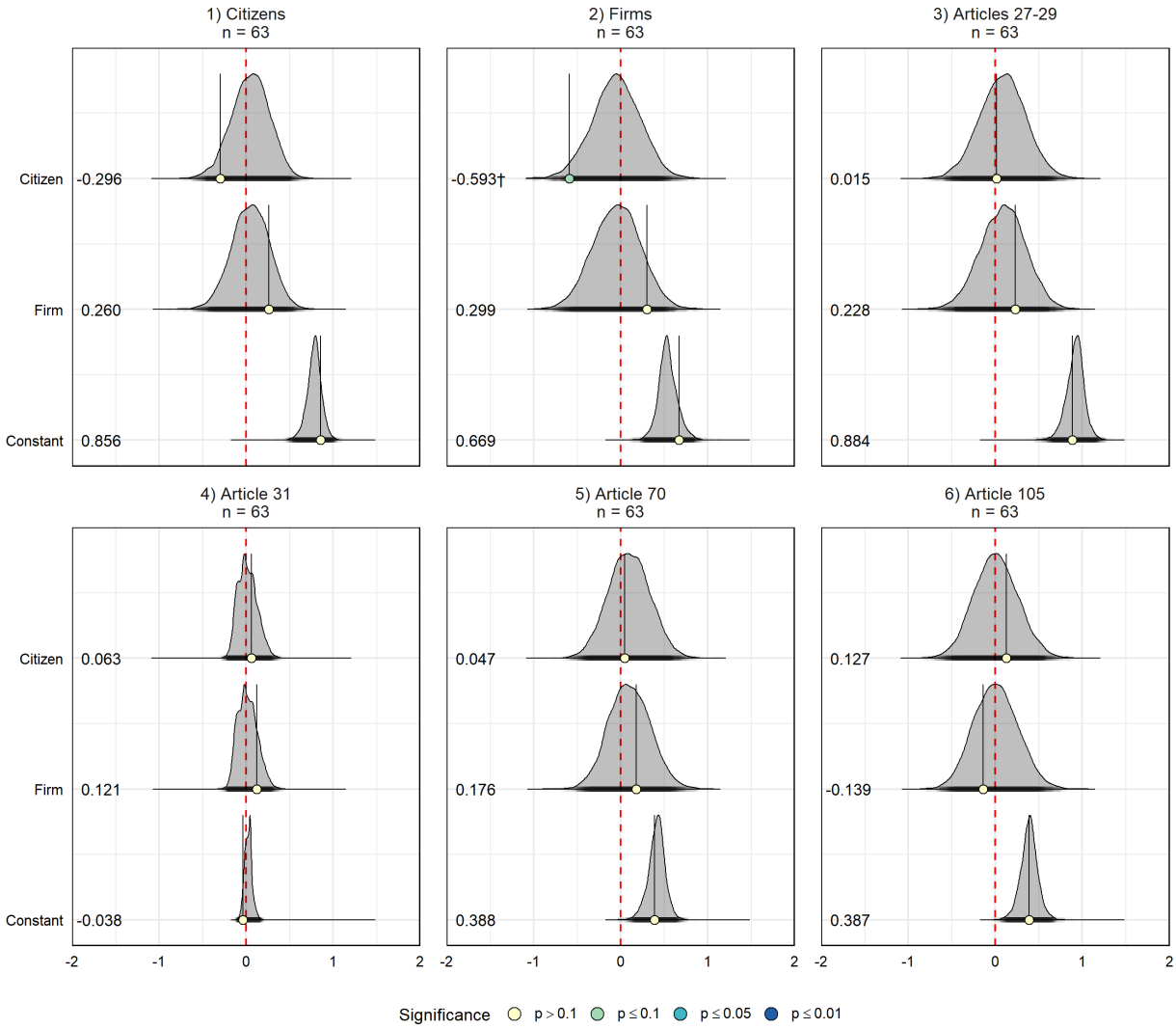

Figure 14: **Almost no detectable treatment effects on speech content.** In provincial-level regressions, increased treatment shares do not impact mentions of citizens, firms, or particular articles from the Education Law.

D. OLS Results with Province-clustered Standard Errors

The coefficients presented in the following four tables are identical to those reported in Appendix C.; the tables differ in that the parenthetical quantity appearing beneath each coefficient is the more familiar standard error, here clustered on provinces. Stars and daggers have also been updated to reflect statistical significance as adjudged by these standard errors. The only other difference lies in the addition in Table 6 of two columns (4 and 8) which replicate analyses (3 and 7) over 100 multiply-imputed datasets.

	All delegates				No central nominees			
	(1) Baseline	(2) Covariates	(3) Saturation	(4) MI[a]	(5) Baseline	(6) Covariates	(7) Saturation	(8) MI[a]
Citizen	0.228**	0.233**	0.148	0.166	0.219**	0.224**	0.008	0.047
	(0.070)	(0.069)	(0.148)	(0.146)	(0.079)	(0.079)	(0.168)	(0.172)
% Citizen			−0.343	−0.266			−0.428	−0.339
			(0.249)	(0.228)			(0.265)	(0.248)
Citizen × % Citizen			0.441	0.289			0.810*	0.597†
			(0.350)	(0.330)			(0.396)	(0.390)
Firm	0.094	0.091	0.113	0.116	0.081	0.075	0.102	0.094
	(0.063)	(0.062)	(0.128)	(0.134)	(0.073)	(0.072)	(0.143)	(0.153)
% Firm			0.001	0.010			−0.072	−0.012
			(0.230)	(0.207)			(0.266)	(0.245)
Firm × % Firm			0.115	0.059			0.196	0.121
			(0.321)	(0.301)			(0.366)	(0.355)
FullTime		−0.101	−0.120	−0.089		−0.101	−0.134†	−0.105†
		(0.074)	(0.074)	(0.076)		(0.075)	(0.073)	(0.074)
CentNom		0.059	0.074	0.051				
		(0.093)	(0.095)	(0.099)				
Competitive		−0.086	−0.093	−0.074		−0.064	−0.069	−0.060
		(0.058)	(0.057)	(0.055)		(0.063)	(0.061)	(0.061)
Constant	0.481**	0.551**	0.598**	0.572**	0.489**	0.547**	0.615**	0.585**
	(0.049)	(0.059)	(0.068)	(0.068)	(0.056)	(0.063)	(0.068)	(0.069)
Observations	293	293	293	470	250	250	250	470
R^2	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. †p<0.1; *p<0.05; **p<0.01. [a]Replicates the preceding column over 100 multiply-imputed datasets.

Table 6: Forms the basis of Figures 5, 8, and 9.

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	0.105†	0.084	−0.105*	0.158	0.073	0.135*	0.229†
	(0.062)	(0.063)	(0.135)	(0.045)	(0.101)	(0.133)	(0.065)	(0.138)
% Citizen			−0.268	−0.164*	0.004	−0.192		−0.232
			(0.204)	(0.075)	(0.125)	(0.177)		(0.217)
Citizen × % Citizen			0.231	0.392**	−0.222	0.196		−0.045
			(0.302)	(0.128)	(0.235)	(0.270)		(0.295)
Firm	0.050	0.055	0.245	0.072	0.020	0.235†	0.058	0.230
	(0.067)	(0.067)	(0.160)	(0.074)	(0.109)	(0.129)	(0.072)	(0.179)
% Firm			0.023	−0.053	0.043	−0.064		0.022
			(0.156)	(0.050)	(0.124)	(0.148)		(0.183)
Firm × % Firm			−0.283	−0.053	−0.051	−0.163		−0.264
			(0.233)	(0.116)	(0.215)	(0.216)		(0.283)
FullTime		0.132†	0.129†	0.007	−0.012	0.140*	0.133*	0.137*
		(0.068)	(0.068)	(0.039)	(0.047)	(0.067)	(0.067)	(0.067)
CentNom		0.030	0.032	−0.016	0.046	0.016		
		(0.077)	(0.075)	(0.042)	(0.062)	(0.073)		
Competitive		0.087†	0.085†	0.050*	0.073**	0.034	0.080	0.076
		(0.051)	(0.051)	(0.020)	(0.026)	(0.047)	(0.057)	(0.057)
Constant	0.409**	0.310**	0.340**	0.059**	0.071*	0.291**	0.303**	0.329**
	(0.044)	(0.058)	(0.065)	(0.019)	(0.028)	(0.059)	(0.059)	(0.069)
Observations	470	470	470	470	470	470	375	375
R^2	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. †p<0.1; *p<0.05; **p<0.01.

Table 7: Forms the basis of Figures 6, 7, 10, and 12.

E. Probit Results with Province-clustered Standard Errors

Table 8 replicates the specifications of Table 2 (and all but the multiple imputation specifications of Table 6), but eschewing OLS for probit regressions. Although the coefficients differ due to modelling differences, the results are substantively similar, as Figure 15 makes clear.

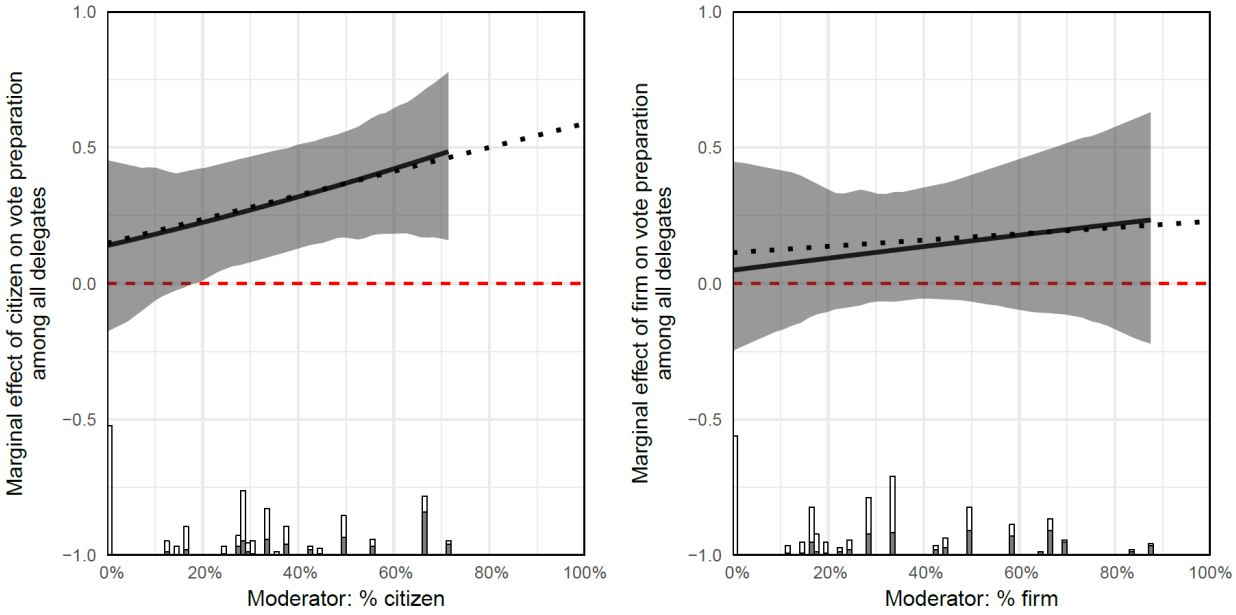

Figure 15: Replicates Figure 8.

	All delegates			No central nominees		
	(1) Baseline	(2) Covariates	(3) Saturation	(4) Baseline	(5) Covariates	(6) Saturation
Citizen	0.598**	0.619**	0.387	0.576**	0.595**	−0.026
	(0.191)	(0.191)	(0.417)	(0.215)	(0.219)	(0.473)
% Citizen			−0.887			−1.113
			(0.660)			(0.708)
Citizen × % Citizen			1.179			2.265†
			(0.991)			(1.164)
Firm	0.237	0.232	0.285	0.203	0.190	0.258
	(0.159)	(0.158)	(0.336)	(0.185)	(0.185)	(0.377)
% Firm			−0.017			−0.208
			(0.626)			(0.727)
Firm × % Firm			0.328			0.537
			(0.856)			(0.977)
FullTime		−0.264	−0.319		−0.264	−0.360†
		(0.195)	(0.196)		(0.196)	(0.194)
CentNom		0.156	0.194			
		(0.244)	(0.250)			
Competitive		−0.229	−0.250		−0.171	−0.189
		(0.156)	(0.156)		(0.168)	(0.167)
Constant	−0.046	0.138	0.265	−0.027	0.125	0.310†
	(0.123)	(0.154)	(0.181)	(0.140)	(0.163)	(0.181)
Observations	293	293	293	250	250	250
Log Likelihood	−194.105	−192.130	−190.402	−165.980	−164.593	−161.510
Akaike Info. Criteria	394.211	396.261	400.803	337.961	339.186	341.020
RMSE	1.151	1.145	1.140	1.152	1.147	1.137

Note: Province-clustered standard errors. †p<0.1; *p<0.05; **p<0.01.

Table 8: Probit replication of Table 6.

F. Treatment Strength and Heterogeneous Treatment Effects

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 16 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

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

where $\mathbf{X}$ includes individual covariates, we obtain the following results displayed in Table 9.

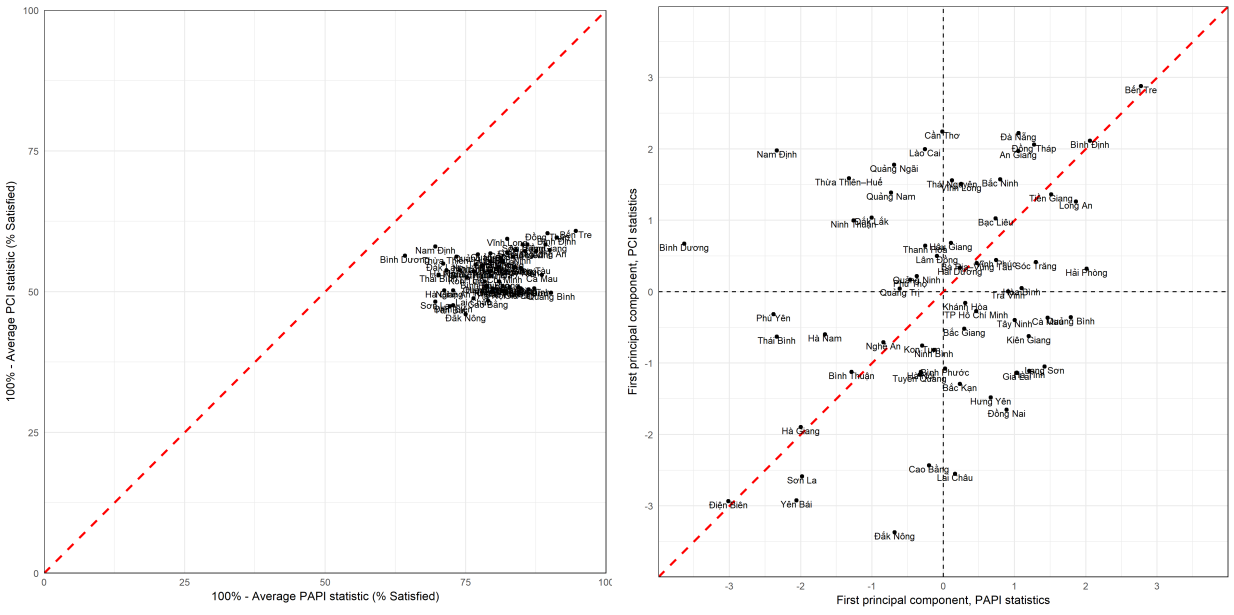

Figure 16: **Provincial infographic statistics, raw averages and PCA first components.** Simply averaging over the percentages in each infographic reveals that there is little variation in treatment strength. The overall pattern remains the same, but the purported variation is increased mechanically via PCA.

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

Note: †p<0.1; *p<0.05; **p<0.01. Province-clustered standard errors.

Table 9: **PCA-treatment interactions.** Multiplicative interactions between 1) PAPI scores and citizen treatment; and 2) PCI scores and firm treatment.

Here Column 1 modifies Table 2/6, Column 3. Interacting treatment assignments with PAPI and PCI scores – essentially inverse treatment strength – yields somewhat divergent results from those obtained with the dosage 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 30 percentage points (upper-left panel, Figure 17). 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 (upper-right panel, Figure 17) 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 Table 3/7, Columns 3-6 by similarly replacing the dosage interactions with (reversed) treatment strength interactions. The first two columns, which examine speaking proclivities in the pooled transcripts and on the floor, are presented graphically in the lower half of the figure. 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 dosage 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 10 modifies the paper's provincial-level analyses by supplementing treatment dosage 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 dosages, 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.

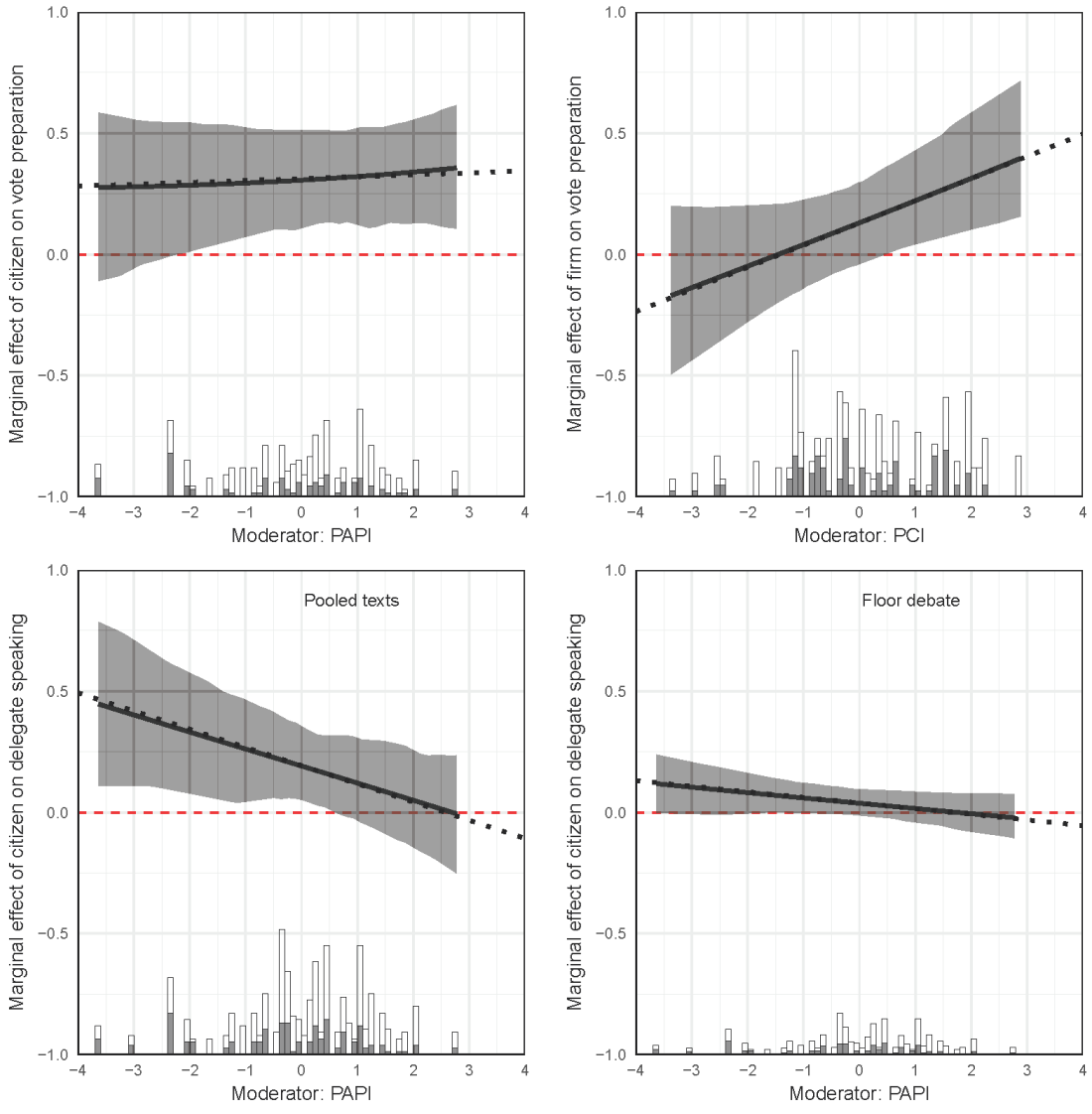

Figure 17: **Increasingly satisfied constituents produce contradictory effects on debate preparation and delegate speaking.** Marginal treatment effects on debate preparation increase as delegates face more satisfied constituents, yet marginal treatment effects on delegate speaking decrease as constituents are more satisfied.

DV:	Mentioned own province			Citizens	Firms	Art. 27-29	Art. 31	Art. 70	Art. 105
Data:	(1) Pooled	(2) Query	(3) Caucus	(4) Pooled[a]	(5) Pooled[a]	(6) Pooled[a]	(7) Pooled[a]	(8) Pooled[a]	(9) Pooled[a]
PAPI	0.012	−0.005	0.014	0.025	0.075	−0.059	−0.020	0.088†	−0.00002
	(0.050)	(0.024)	(0.047)	(0.043)	(0.052)	(0.053)	(0.020)	(0.046)	(0.051)
PCI	−0.012	−0.032	0.002	−0.050	0.014	0.066	−0.002	0.045	−0.042
	(0.044)	(0.021)	(0.042)	(0.039)	(0.047)	(0.047)	(0.018)	(0.041)	(0.045)
% Citizen	0.593*	0.017	0.594*	−0.273	−0.522†	−0.041	0.044	0.130	0.127
	(0.249)	(0.118)	(0.236)	(0.217)	(0.263)	(0.264)	(0.098)	(0.229)	(0.254)
% Firm	−0.056	−0.071	−0.046	0.251	0.383	0.214	0.100	0.294	−0.167
	(0.268)	(0.127)	(0.254)	(0.234)	(0.284)	(0.285)	(0.106)	(0.247)	(0.274)
% FullTime	−2.369†	0.017	−2.956*	−1.030	0.786	−1.580	0.359	0.666	−1.160
	(1.290)	(0.612)	(1.222)	(1.125)	(1.365)	(1.369)	(0.510)	(1.189)	(1.319)
% CentNom	2.382	−0.042	2.991*	1.277	−0.916	1.888	−0.500	−1.823	0.919
	(1.429)	(0.678)	(1.354)	(1.246)	(1.512)	(1.516)	(0.565)	(1.316)	(1.460)
% Competitive	0.145	0.203†	0.015	0.108	−0.236	−0.206	0.017	−0.289	0.260
	(0.215)	(0.102)	(0.203)	(0.187)	(0.227)	(0.228)	(0.085)	(0.198)	(0.219)
Constant	0.433	−0.034	0.545†	0.869**	0.693*	0.859**	−0.044	0.412	0.391
	(0.289)	(0.137)	(0.274)	(0.252)	(0.306)	(0.307)	(0.114)	(0.267)	(0.296)
Observations	63	63	63	63	63	63	63	63	63
R^2	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: †p<0.1; *p<0.05; **p<0.01. [a]Transcripts from floor debates and group caucuses only. The unit of analysis is the province.

Table 10: **Analyzing speech content with PCA.** PAPI and PCI scores have no effect delegation-level speaking behavior.

The next set of results examines whether electoral accountability can explain the evidence of responsiveness that we find. To that end, we regress the debate preparation and delegate spoke indicators on the treatments, delegate covariates, and an interaction between competitiveness and each treatment. In addition, we present these results using both the competitiveness indicator used in the main results, as well as the raw vote shares themselves. All models are presented in tabular and graphical formats, with standard errors clustered at the provincial level.

As can be seen in Table 11 and Figure 18, neither measure of competitiveness yields a significant interaction with the citizen or firm treatment. In fact, both the indicator variable and raw vote shares produce a negative relationship between competitive elections and debate preparation. This evidence does not support an electoral accountability story.

Turning attention to Table 12 and Figure 19, we find that greater levels of electoral competitiveness correspond to an elevated probability that a delegate speaks in one or more legislative fora. While the direction of this effect is consistent with an electoral accountability story, the effects are not significant for any treatment under either measure of competitiveness.

	(1) Dummy	(2) Dummy interaction	(3) Vote share	(4) Vote share interaction
Citizen	0.233**	0.257*	0.236**	0.287
	(0.070)	(0.108)	(0.070)	(0.516)
Firm	0.091	0.096	0.088	0.111
	(0.068)	(0.099)	(0.068)	(0.503)
Competitive	−0.086	−0.071		
	(0.058)	(0.095)		
Citizen × Competitive		−0.043		
		(0.143)		
Firm × Competitive		−0.009		
		(0.137)		
VoteShare			0.595**	0.627
			(0.290)	(0.487)
Citizen × VoteShare				−0.070
				(0.716)
Firm × VoteShare				−0.031
				(0.688)
FullTime	−0.101	−0.102	−0.108	−0.107
	(0.077)	(0.077)	(0.077)	(0.077)
CentNom	0.059	0.058	0.057	0.057
	(0.102)	(0.103)	(0.101)	(0.102)
Constant	0.551**	0.544**	0.079	0.056
	(0.061)	(0.073)	(0.213)	(0.353)

Note: Standard errors clustered by provinces. †p<0.1; *p<0.05; **p<0.01.

Table 11: **HTE by electoral competitiveness.** No evidence of electoral accountability in the debate preparation outcome.

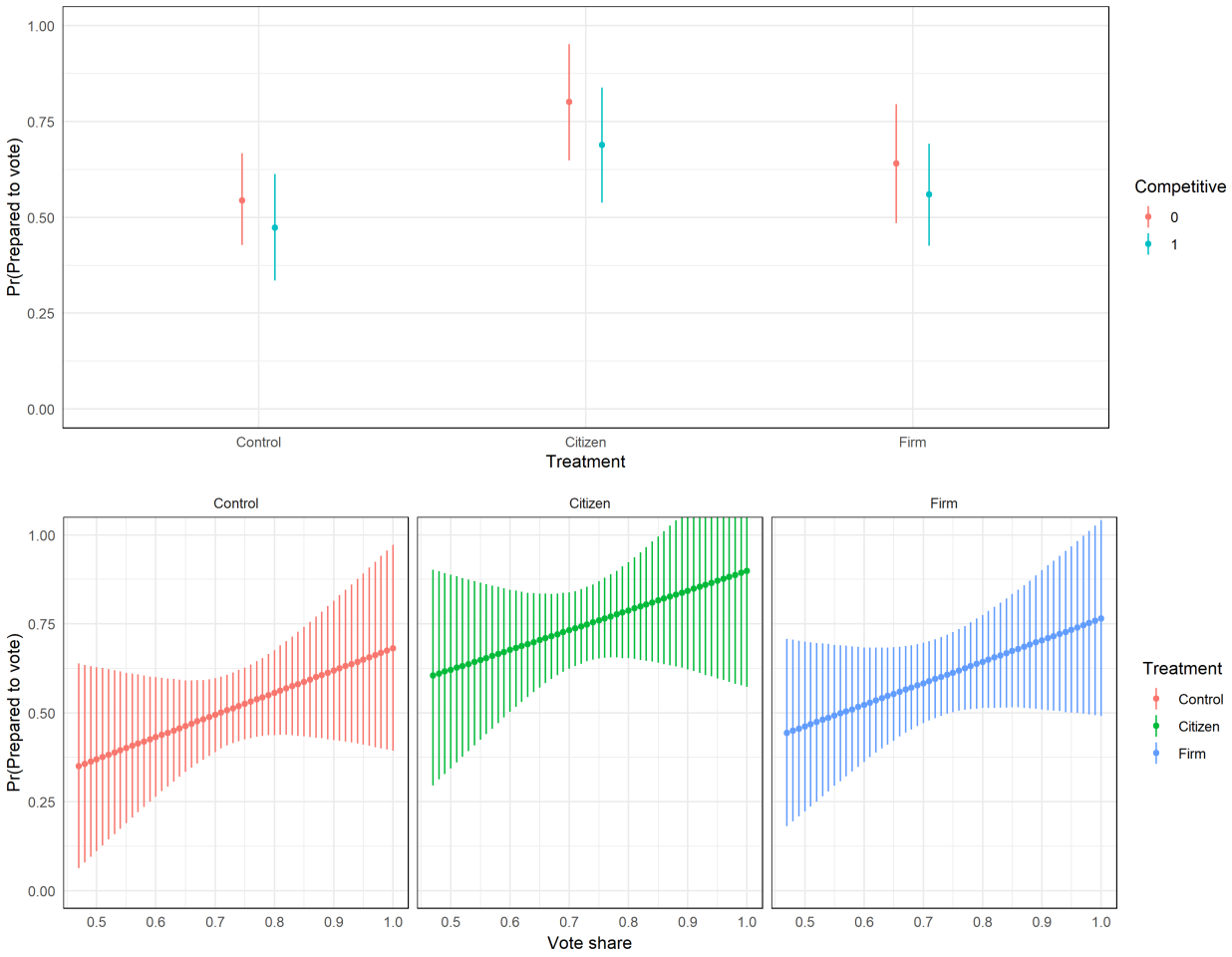

Figure 18: **Predicted probabilities of debate preparation by electoral competitiveness** Regardless of how it is measured, competitiveness does not significantly impact debate preparation.

	(1) Dummy	(2) Dummy interaction	(3) Vote share	(4) Vote share interaction
Citizen	0.105†	0.100	0.103†	0.080
	(0.055)	(0.081)	(0.055)	(0.410)
Firm	0.055	0.029	0.059	0.270
	(0.055)	(0.080)	(0.055)	(0.404)
Competitive	0.087†	0.069		
	(0.046)	(0.074)		
Citizen × Competitive		0.007		
		(0.111)		
Firm × Competitive		0.051		
		(0.110)		
VoteShare			−0.613**	−0.523
			(0.229)	(0.392)
Citizen × VoteShare				0.031
				(0.565)
Firm × VoteShare				−0.291
				(0.553)
FullTime	0.132†	0.132†	0.135*	0.135*
	(0.067)	(0.067)	(0.067)	(0.067)
CentNom	0.030	0.029	0.033	0.032
	(0.080)	(0.080)	(0.079)	(0.080)
Constant	0.310**	0.320**	0.797**	0.732*
	(0.048)	(0.057)	(0.168)	(0.284)
Observations	470	470	470	470
R^2	0.029	0.030	0.032	0.033
RMSE	0.482	0.483	0.481	0.482
F-Statistic	2.815*	2.035*	3.044*	2.226*

Note: Standard errors clustered by provinces. †p<0.1; *p<0.05; **p<0.01.

Table 12: **HTE by electoral competitiveness.** Weak evidence of electoral accountability in inducing delegates to speak.

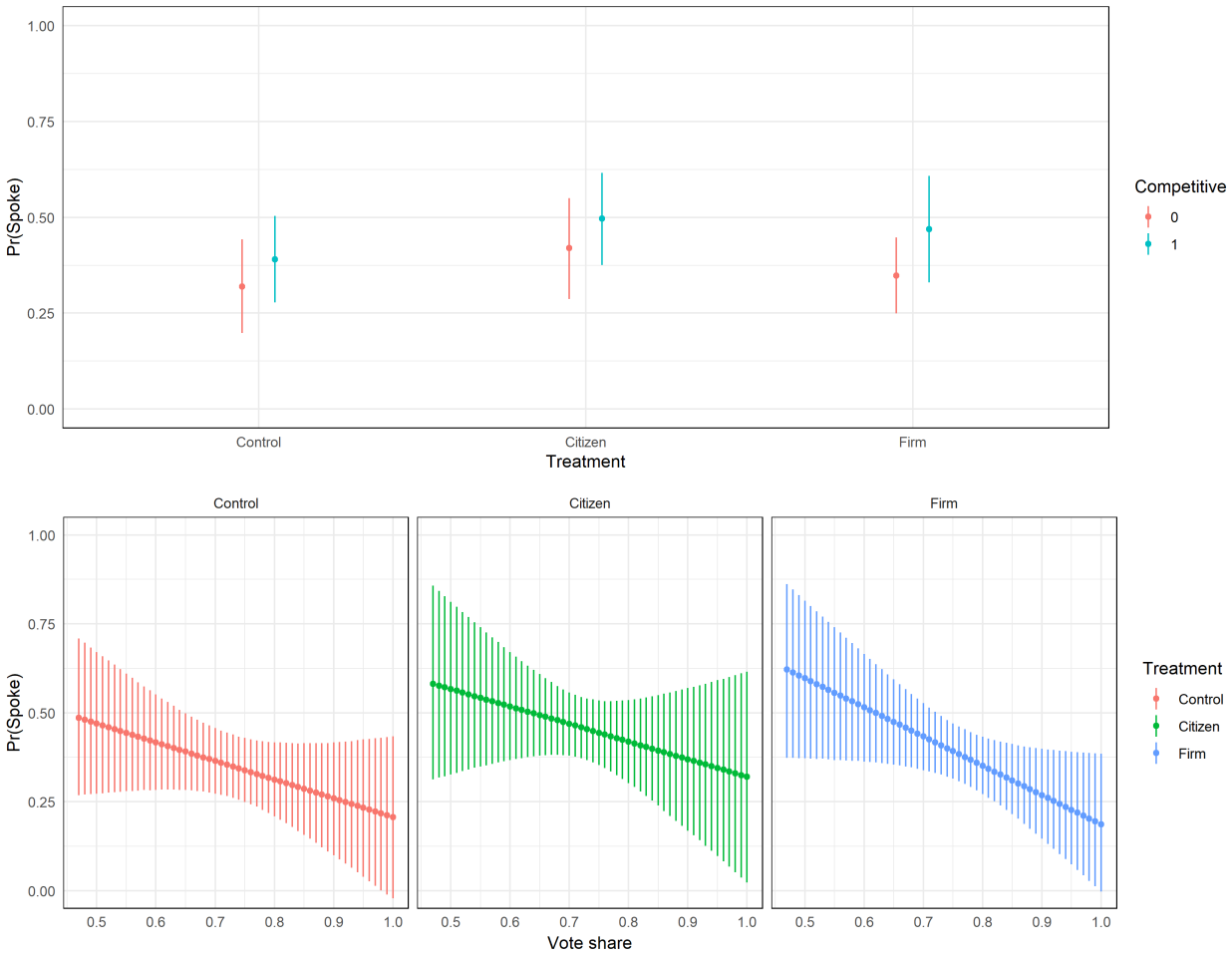

Figure 19: **Predicted probabilities of speaking by electoral competitiveness.** Regardless of how it is measured, competitiveness does not significantly impact speech.

G. Controlling for Education

Although paper Table 1 reveals balance along three education variables – a binary indicator for educational career, a delegate's years of education, and a categorical indicator for highest degree obtained – Table 13 modifies Table 2/6 by introducing these variables as controls in regressions of debate preparation. The substantive results remain essentially unchanged, regardless of which variable is used, although the dummy variable has its own significantly negative effects on debate preparation.

	Baseline	Covariates				Saturation			
	(1) Baseline	(2) No Edu.	(3) EduCareer	(4) EduYears	(5) EduLevel	(6) No Edu.	(7) EduCareer	(8) EduYears	(9) EduLevel
Citizen	0.228**	0.233**	0.259**	0.234**	0.231**	0.148	0.180	0.148	0.183
	(0.070)	(0.069)	(0.067)	(0.069)	(0.069)	(0.148)	(0.142)	(0.149)	(0.145)
% Citizen						−0.343	−0.322	−0.343	−0.365
						(0.249)	(0.243)	(0.253)	(0.250)
Citizen × % Citizen						0.441	0.418	0.440	0.386
						(0.350)	(0.347)	(0.353)	(0.355)
Firm	0.094	0.091	0.110†	0.094	0.093	0.113	0.116	0.113	0.131
	(0.063)	(0.062)	(0.060)	(0.064)	(0.061)	(0.128)	(0.128)	(0.128)	(0.129)
% Firm						0.001	−0.015	0.001	−0.015
						(0.230)	(0.225)	(0.231)	(0.225)
Firm × % Firm						0.115	0.150	0.115	0.104
						(0.321)	(0.314)	(0.322)	(0.322)
FullTime		−0.101	−0.096	−0.100	−0.109	−0.120	−0.115	−0.120	−0.128†
		(0.074)	(0.074)	(0.075)	(0.074)	(0.074)	(0.073)	(0.075)	(0.073)
CentNom		0.059	0.057	0.061	0.095	0.074	0.072	0.074	0.110
		(0.093)	(0.091)	(0.091)	(0.093)	(0.095)	(0.092)	(0.093)	(0.095)
Competitive		−0.086	−0.071	−0.087	−0.088	−0.093	−0.078	−0.093	−0.096†
		(0.058)	(0.055)	(0.059)	(0.058)	(0.057)	(0.055)	(0.058)	(0.057)
EduCareer			−0.232**				−0.230**		
			(0.069)				(0.070)		
EduYears				0.005				0.0003	
				(0.023)				(0.023)	
EduLevel					−0.055				−0.058
					(0.039)				(0.041)
Constant	0.481**	0.551**	0.561**	0.493†	0.704**	0.598**	0.608**	0.595*	0.764**
	(0.049)	(0.059)	(0.058)	(0.269)	(0.126)	(0.068)	(0.066)	(0.272)	(0.136)
Observations	293	293	293	293	293	293	293	293	293
R^2	0.035	0.048	0.074	0.048	0.055	0.059	0.085	0.059	0.067
RMSE	0.487	0.487	0.481	0.487	0.486	0.487	0.481	0.488	0.486

Note: †p<0.1; *p<0.05; **p<0.01. Province-clustered standard errors.

Table 13: **Controlling for delegates' educational backgrounds.** Controlling education does not impact treatment effects.