The Adverse Effects of Sunshine

Evidence from a Field Experiment on Legislative Transparency in an Authoritarian Assembly

Edmund Malesky
University of California, San-Diego
Graduate School of International
Relations & Pacific Studies
emalesky@ucsd.edu

Paul Schuler
University of California, San-Diego
Political Science Department
pschuler@ucsd.edu

Anh Tran
Indiana University
School of Public and
Environmental Affairs
trananh@indiana.edu

Abstract: An influential literature has demonstrated that transparency can improve the performance of political officials in democracies, particularly within elected legislature. Although the direct causal link for this relationship runs through the incentives created by voters' responses to new information, recent donor projects have begun to export transparency interventions to authoritarian regimes under the assumption that NGOs and media can substitute for voters in these systems. A countervailing literature, however, argues that transparency can have perverse effects in systems where agents (politicians) understand the relationship between behavior and outcome better than their principals (the voters), exactly the situation we observe in authoritarian polities. In this analysis, we devise a randomized experiment to test the influence of transparency on delegate behavior in query sessions in Vietnam, a single-party, authoritarian regime. We find that transparency has no direct effect on the quantity or quality of delegate behavior in the National Assembly. However, delegates subjected to a high intensity of treatment demonstrate robust evidence of curtailed participation and conformist behavior.

Version 2.1

Prepared for presentation at Princeton Political Economy Conference (Polecon), May 14, 2011.

U.S. Supreme Court Justice Louis Brandeis famously remarked that "sunlight is the best disinfectant" to argue that opening-up the policy-making process to public scrutiny is the best way to remove corruption and restrain self-dealing by politicians. The logic of the argument is straightforward. Increasing transparency, defined as the ability of the principal to observe the agent's behavior and the consequences of the agent's decisions, aligns the interests of the two actors and allows the principal to hold the agent accountable, which reduces corruption and improves public service delivery (Prat 2005).

The *incentive hypothesis*, drawn from theoretical, observational, and experimental analyses in democratic regimes argues that transparency will improve political performance, particularly in elected legislatures, by forcing officials to better respond to the demands of newly informed voters (Brunetti and Weder 2003, Djankov et al. 2003, Besley and Prat 2006, Humphreys and Weinstein 2010). Recently, some have argued that although responsiveness to voters may not exist in authoritarian regimes, transparency of policy-making in national legislators may increase responsiveness to stakeholders such as NGOs or local media, thus leading to the same effect (Kaufman 2004, Smulovitz and Perruzzotti 2000, Bauhr and Grimes 2011, 1).[1]

The logic that transparency initiatives can travel to authoritarian settings, however, assumes that legislators in authoritarian systems serve the same role and have analogous incentives to those do in electoral democracies -- an assumption that contradicts the emerging work on authoritarian institutions showing legislatures in non-democratic systems are primarily a forum for contained exchange between authoritarian leadership and potential opposition. These exchanges may involve direct benefits to particular legislators (Lust-Okar 2006, Blaydes 2006) or a limited say in policy-making (Gandhi 2009,

---

[1] See for example the United Nations Development Program on Governance in the Arab region, which has a substantial legislative transparency component (http://www.undp-pogar.org/index.aspx). The Chinese Open Governance Initiative (OGI), which has been assisted by the Yale Law Center (http://www.law.yale.edu/documents/pdf/Intellectual_Life/Ch_China_Adopts_1st_OGI_Regulations.pdf). The US-AID funded STAR project has a program on transparency in law-making in Vietnam (http://www.usaid.gov/locations/asia/countries/vietnam/).

Wright 2008, Boix and Svolik 2007). Ultimately, however, the idea is to resolve policy disputes without allowing them to spill over into public debate and perhaps inflame popular sentiments.

Under these conditions, legislators, who understand the relationship between their behavior and outcomes better than the citizens who voted for them, may curtail their activities and behave in a more conformist manner under conditions of transparency. This *adverse consequences hypothesis*, developed by Prat (2005), delineates the conditions under which transparency may lead less legislative responsiveness and worse policy outcomes for voters. In many ways, the relationship between the leadership and legislators in authoritarian systems is analogous to the relationship between CEO of Company and his Board of Directors that Prat (2005) analyzes. A CEO may wish to shield the information provided by his advisors from public scrutiny, so that they can offer honest advice or negotiate over strategy without worrying about fallout from their advice among shareholders. Cooptation theorists suggest that authoritarian assemblies may play the same role of supplying information, criticism, and room for negotiation to the leadership without threatening its ultimate survival.

The conflicting *incentive* and *adverse consequences hypotheses* pose a conundrum for domestic reformers and international advocates for increased legislative transparency in authoritarian countries. Transparency will certainly shed light on the dark politics and trade-offs involved in authoritarian institutions, and may help tone down illicit bargains and political self-dealing by exposing these activities to the light of the day. At the same time, however, transparency, in the absence of a strong electoral mechanism to sanction the behavior of legislators, could actually curtail other forms of participation that has been shown to be associated with regime survival, peace, and economic growth (Gandhi 2009, Geddes 2006, Wright 2008). Rolling-out initiatives to increase legislative transparency without considering and testing the magnitude of these adverse effects could lead to self-defeating interventions.

In this study, we address this puzzle with a randomized experiment that tests the effect of transparency on delegate behavior in the National Assembly of the single-party state of Vietnam. As

Malesky and Schuler (2010) demonstrate, the Vietnamese National Assembly offers an ideal forum for such as test because of its biannual query sessions where universally-elected delegates, representing particular geographic regions, are allowed to quiz the Prime Minister and his cabinet on their performance, pending legislation, and other issues of the day. As part of the experiment, Vietnam's highest profile online newspaper, *VietnamNet*, created a new political column called "Know Your Delegates." Under this column, we developed individual websites for 144 randomly selected delegates, where we posted query transcripts and scorecards for each delegate and updated them in real time throughout the 6th query session. This experimental design allowed us to compare the change in participation of treated delegates to a control group of 317 delegates, who did not have their performance posted.

The treatment for the selected delegates was designed to be powerful. A *VietnamNet* reporter met each of the delegates in person and handed an official letter explaining that delegates would be assessed based on the quantity of their query-session participation, as well as how well they represented their provincial constituencies. At the time of the treatment, delegates were well aware of the popularity of *VietnamNet*, which receives about thirty million page views a week. Indeed, during time of 6th query session, our "Your Delegates" column became well known in the country's political media, receiving over 1.3 million page views and 820,000 hits on particular delegates' pages.

It is crucial to note that the proportion of the Vietnamese population who read online newspapers varies dramatically across Vietnam. As a result, our treatment intensity also varies across delegates, as their constituents are located in different provinces with varying levels of internet penetration and therefore varying levels of access to their delegates 'web pages. This variation allows us to observe the effects of different treatment intensities and provides important insight into our understanding of the adverse consequences of legislative transparency in authoritarian parliaments.

We find that when internet penetration increases, treated delegates speak less frequently and less critically about the national government and its policies. The result remains strong after controlling for

structural covariates with internet penetration, such as wealth, population size, good governance, and transfer dependence. In a fully specified model, each additional internet subscription per 100 citizens is associated with a 0.18 reduction in the number of questions asked in the treated group and 1.9% decrease in the percentage of critical queries. Thus, when internet penetration is about 8% (the level observed in Hanoi and Ho Chi Minh City) , we find that treated delegates ask a full question less and reduce their criticism more than 12% below the delegates in the control group. This result also survives when we use alternative measures of treatment intensity such as the proportion of population living in urban areas, or studying in colleges.

Our findings are supportive of the adverse consequences hypothesis and the growing literature on co-optive exchange in authoritarian parliaments. Authoritarian legislatures provide enormous benefits to regime longevity, by allowing leaders to sense popular sentiment, co-opt potential threats, and resolve conflicts peacefully. Because such sessions could fuel popular unrest, however, delegates are encouraged to participate in legislative debate actively only to the extent that their critique to the government does not spill over to the public (Gandhi and Przeworski 2006). In our study, we see evidence of this relationship. As the proportion of the population reading online newspapers increases, the concern for spreading dissatisfaction to the public becomes more serious. As a result, our treatment leads delegates to curtail their participation and representation of their constituencies.

In the following pages we describe the design and results of this experiment. In Section 1, we detail the empirical foundation for the impact of transparency in democratic and authoritarian legislatures, allowing us to develop the two hypotheses our experiment rests upon. In Section 2, we briefly describe the Vietnamese legislature and the role of query sessions. Section 3 details our experimental design, while Section 4 provides results for the direct effects of the transparency treatment. . Section 5 discusses the interaction between the treatment and internet penetration. We conclude by discussing the implications of our experimental findings and the project's next steps to tease out the relationship between transparency and performance in authoritarian settings.

1. Transparency and Political Performance

Going back to Jeremy Bentham (1999) more than 150 years ago, there is a strong theoretical basis for the argument that electoral democracy can prove an insufficient foundation for holding politicians accountable in the presence of an uninformed electorate. Buchanan (1989) demonstrated that when voters are uninformed, they are unable to effectively sanction the behavior of politicians, who can use the opportunity to engage in corruption, self-dealing, or to cater to more informed constituents (Besley and Burgess 2002).

Among the most important links between transparency and improvements in public sector performance, particularly in developing democracies, is the theoretical role that transparency can play in improving electoral democracy. Because politicians value holding office, they have an incentive to best serve their constituency's interests and behave honestly if they fear voters may turn against them for not doing so (Barro 1973, Ferejohn 1986). Uninformed voters, however, are handicapped in their ability to police and sanction the actions of their agents in public office (Buchanan 1989). As a result, in non-transparent environments, the accountability between the principals (voters) and their agents (politicians) is much weaker (Alt et al. 2004, Besley and Burgess 2002, Lassen 2005).

Humphreys and Weinstein (2009) delineate two separate hypotheses linking transparency to legislator performance. They first refer to the *incentive hypothesis*, the notion that increased openness forces delegates to perform better in order to win over voters in an electoral democracy. This is distinguished from the *selection hypothesis* that takes place a bit further downstream, which is that increased transparency enables voters to choose better candidates for office – tossing out the laggards and selecting delegates who are more likely to have the constituency's interest in mind (Besley 2005, Besley and Prat 2005). [2]

---

[2] While we are interested in the selection hypothesis as well, Vietnam does not make the best test case for it. First, there is reason to be suspicious about the power of voters in authoritarian elections to throw out incumbents, even

A number of studies have tested and found empirical support for most of the nodes in the causal chain linking transparency to improved delegate behavior. First, there is observational evidence that more well-informed voters act to hold their agents accountable. More informed voters are more likely to turn out for elections (Lassen 2005) and ensure that the fruits of the political process are brought home to their localities (Stromberg 2004). There is also evidence that politicians respond to greater transparency with better performance. In the U.S., fiscal transparency is associated with higher levels of observed effort on the part of politicians (Alt et al. 2002). In India, disaster relief efforts are better where media penetration is higher (Besley and Burgess 2002) and public goods delivery is enhanced in localities with higher citizen literacy and participation in decision making (Besley, Pande, and Rao 2006). Analyses of press freedom and governance have drawn similar conclusions (Brunetti and Weder 2003, Djankov et al. 2003, Besley and Pratt 2006). Better access to information seems to force politicians to respond more appropriately to citizen demands.

Adverse Consequences

There is by no means a consensus when it comes to the positive benefits of transparency. An alternative literature has struggled to identify the micro-logic between increased openness, the actions of citizens, and public sector performance. Some scholars, particularly in Italy, question the selection hypothesis, having found little evidence that voters actually use the ballot box to punish corrupt politicians (Chang, Golden & Hill 2010; Golden & Picci 2008).

Other scholars question whether the incentive mechanism can be effective, when citizens do not make use of the new information and politicians are not affected by revelations. Bauhr and Grimes (2011)

---

under the best conditions. More immediately, however, we were not able to test it in our original experiment, as Vietnamese elections will not be held until May 2011. In this paper, we concentrate specifically on the impact of transparency on altering the incentives of existing politicians, but a new experiment is now online in anticipation of the May 2011 elections.

show that the ability of citizens to sanction corrupt politicians through the actions of civil society is highly constrained. In doing so, they cite Jonathan Fox to explain why transparency may fail to enhance rule of law: "If the power of transparency is based on the 'power of shame', then its influence over the really shameless could be quite limited" (Fox 2007, 665). In a related piece, Datta (2008) fins that rather than improving performance, televising debate sessions in the Indian parliament only increased the activity of upwardly mobile delegates, who were preening for the cameras rather than debating substantive issues (Datta 2008).

A third group of scholars argue that transparency can be effective, but its impact is dependent on societal conditions. Linstadt and Naurin (2010), for instance, demonstrate that the positive impact of transparency in reducing corruption is conditional on the underlying press freedom, levels of education, and quality of elections in the country initiating the policy. Moreover, they find that transparency initiatives implemented by a particular agent are not as effective as transparency provided by external agents, such as a free press.

Most importantly, for our project, Andrea Prat (2005) has demonstrated formally that when the outcomes of an agent's acts are observable (i.e. televised news reports of activities in a national parliaments), but the relationship between actions and outcomes is better understood by the agent than the principal (i.e. understanding of how policy pronouncements made in parliaments are shaped into legislation), more transparency could actually lead the agent to curtail non-conformist behavior and refrain from actions that could yield better outcomes. Prat (2005) argues that this is the motivation behind executive privilege in the United States and other countries, where it is feared that open access discussions of those counseling key decision makers may damage their ability to be frank and candid.

As evidence of this effect, Naurin (2008) shows that transparency reforms had such a conforming effect on representatives at the European Council, who feared that the negotiations between lobbyists and politicians would become public. Naurin argues that publicity led to less efficient negotiations and fruitful

side-deals, leading to limited improvements in policies. In fact, policy-makers are keenly aware of this problem. Countries that have adopted open information codes specifically separate information about the decision-making process and the final decision. In most countries, citizens cannot request the working papers and recommendations underlying a public decision until after the decision has been implemented, so that the leaders can receive honest counsel during the decision-making process (Frankel 2001).

Prat's *adverse consequences hypotheses* is a helpful guide to considering when and how transparency will be effective at improving public sector performance. In settings where agents understand the dark art of policy-making better than the principal and feel the need to engage in types of negotiations that would be publically embarrassing, transparency may lead to less effective activity. For instance, one concern about the Wikileaks release of classified cables from U.S. Foreign Service Officers (FSOs) was that it may reduce their effectiveness in representing U.S. interests abroad. Indeed, the cables show that FSOs engaged in a host of embarrassing side-deals with foreign leaders and informants in order to accomplish their objectives. Prat counsels against openness under such conditions.

Experimental Evidence

While well-executed and informative, the observational studies cited both in favor of transparency and against it have limitations that inhibit causal inference and the ability to draw out actionable policy lessons. Simultaneity bias haunts direct causal identification. For instance, do voters who care deeply about better public goods delivery make more efforts to inform themselves about politician behavior? Unobserved heterogeneity is also a problem. Do the same features that make a state more open to diverse media outlets also lead to better selection of politicians? Of course, these problems are inherent in most work and many of these articles are quite careful about causal identification. More important from a policy stand-point is that the studies offer little insight into what can be done to improve political transparency going forward. Both media penetration and informed voters are the result of long-

term multi-faceted process, but what opportunities are available to countries and localities not blessed with such endowments?

A new literature has begun to use experimental approaches to better sort out the relationship between transparency and political performance. Carey (2010) shows that students placed under laboratory conditions responded to openness with more generous offers to their co-players. In India, an experiment provided randomly selected voters with details about political candidates' criminal histories, finding that informed voters were significantly less likely to vote for co-ethnics when that politician had a criminal background (Banarjee et al. 2010). Also in India, Peisakhin and Pinto (2010) exploited a new Freedom of Information Act in a randomized design evaluating access to ration cards, finding that use of the act out-performed bribery in securing access to public services. Humphreys and Weinstein (2009) take a slightly different tack by randomizing which Ugandan parliamentarians were treated with a scorecard of information recording their attendance, participation, legislative initiatives, and contact with their constituencies. In a pilot analysis, the authors found that treated delegates were 50% more likely to participate in subsequent parliamentary sessions. Most recently, Simon Hix and Sara Hageman started a website (www.votewatch.eu), which makes the voting records of European Parliamentarians available online (Barber 2009). Hix and Hageman informed a randomly selected group of parliamentarians about the digital record, but have yet to publish results of their experiment.[3]

---

[3] One experimental analysis from a Democratic setting offers sobering evidence of public openness, but the subjects were individual voters rather than politicians. Nevertheless, the finding does offer some insights into how individuals may curtail behavior in the presence of transparency in line with the adverse consequences theory. In a field experimented conducted before the 2008 Iowa Democratic Caucus, Grose and Russell (2010) randomly notified some voters that the caucus did not have a secret ballot and that their friends and neighbors would be attending. The authors find that turnout at the caucus was significantly reduced in the treatment group, attributing this effect to the higher social costs of voting.

Authoritarian Regimes

The large number of experimental studies on transparency is encouraging for the incentive hypothesis, but all take place in the setting of an electoral democracy. Even Uganda, which is coded as only partially free on the most recent Freedom House Index, has exhibited multiple, competitive elections. How might such experiments fare in non-democracies or in competitive authoritarian regimes, where the relationship between citizens and voters is less well-understood?

This is becoming an increasingly relevant question, as more and more authoritarian regimes adopt transparency initiatives at the behest of western donors. The strength of the theoretical arguments for increased transparency has persuaded a number a number of international organizations and non-government organization (NGOs), which seek to improve public service delivery in developing country settings by funding interventions that enhance the transparency of policy-making, particularly national legislatures. In authoritarian settings, practitioners have speculated that transparency may achieve these positive benefits even in the absence of a functioning electoral democracy.

Better information about political behavior, may allow media and civil society to report on political abuses and embarrass political leaders, creating "millions of auditors" (Kaufman 2004, 21) as a substitute for direct accountability through elections to voters (Smulovitz and Perruzzotti 2000; Peruzzotti and Smulovitz 2006) This logic builds off the fire alarm style of public monitoring first emphasized by McCubbins and Schwartz (1984). In essence, the advocates of transparency argue that the incentive mechanism can still apply even in polities that have non-functioning or highly-flawed electoral systems (Bauhr and Grimes 2011,1).

There is reason to believe, however, that the logic linking transparency to parliamentary performance does not travel well to authoritarian settings, even in quasi-democratic institutions, such as hegemonic regimes with universally-elected national legislatures (Lindsted and Naurin 2010). The critical difference is that nominally democratic institutions may serve a starkly different role in authoritarian countries (Gandhi and Lust-Okar 2009), even though they carry the same names.

The vast majority of authoritarian regimes have legislatures and elections. Hegemonic party and single-party regimes accounted for more than 60% of the non-democracies in 2006 (Magaloni and Kricheli 2010), and virtually all those countries had national legislatures during the postCold War period. Monarchies and personalist regimes are also extremely likely to have parliaments (Wright 2008). Elections are similarly widespread. Between 2000 and 2008, of the 172 existing independent states with a population greater than 500,000, only six countries failed to hold some form of direct national election (Hyde and Marinov 2009).

Not only is the proliferation of elections and parliaments widespread, but empirical analyses have consistently shown that authoritarian regimes with elections perform better on a range of critical outcome variables than those authoritarian states that have yet to adopt them. Contributors to this literature have demonstrated a strong association between having a national legislature and regime longevity, reduced violence, and economic growth (Gandhi 2009, Gandhi and Przeworski 2007, Gandhi and Vreeland 2004, Geddes 2006, Wright 2008).

The benefits of nominally democratic institutions in authoritarian regimes have rarely been attributed to increased citizen-voters linkage. Authors in the authoritarian institutions argue that the use of elections and assemblies have a separate set of causal logics (Gandhi and Lust-Okar 2009) that either involve signaling or cooptation of a potential opposition. Signaling theories suggest that elections can deliver supermajorities for regime-backed candidates, thus bolstering their own legitimacy and preventing the opposition from mounting a challenge (Geddes 2006, Magaloni 2007, Simpser 2005).

Cooptation theorists, by contrast, argue that authoritarians relying too heavily on repression for survival become dependent on their security apparatus (e.g., police force or military), which carries out the state's heavy-handed enforcement. Maintaining authority in this manner places a large share of resources in the hands of security elites, who could turn against the regime leaders down the road (Gandhi 2009; Haber 2006). Cooptation theorists therefore argue that a less dangerous, long-term strategy is to allow groups from outside the inner circle to have a formal say in the policy-making process through the use of quasi-

democratic institutions. Elections allow leaders to identify the most popular local notables or potential opposition forces (Boix and Svolik 2007). Once identified, the dictator can placate these elites by giving them some say over policy making (Gandhi 2009; Gandhi and Przeworski 2006, 2007) and/or access to rents (Lust-Okar 2006) through membership in a national parliament.

For cooptation theorists, formal legislatures are more effective than simply negotiating directly with outside groups. While individual legislators may be highly active, the primary role of a national parliament is to serve as forums where "demands can be revealed without appearing as acts of resistance... and where the resulting agreements can be dressed in a legalistic form and publicized as such" (Gandhi and Przeworski 2007, 1282). Critical to this argument is that disagreements are unveiled, but in a controlled and unthreatening manner that will not generate larger protests.

If the positive benefits associated with National Assemblies are due to this co-optive exchange, transparency could potentially undermine the delicate balance worked out between leaders and potential opposition. More transparency could allow intense and volatile debates to spill outside of the contained forum and into environments where it is more difficult to identify solutions through side-deals and particularistic payments. Delegates may find it more difficult to criticize regime leaders and provide their honest opinions on state initiatives when they fear their arguments could have adverse consequences. For instance, regime leaders may be open to direct criticism in a closed forum, but may fear the damage such criticism could do to their legitimacy if heard by a larger audience.

In short, authoritarian legislatures provide exactly the setting that Prat (2005) warned about when discussing the negative effects of transparency when agents understand the relationship between actions and outcome better than their principals. In the messy and secretive world of authoritarian bargaining, concessions to potential opposition requires a delicate dance whereby authoritarian leaders must be made aware of the demands of the group, and benefit from the alternative sources of information, but fear that agreeing openly to such demands may inspire other groups or may reveal regime weakness to the population

or potential threats.   Shining a light on this activity may actually force potential opposition to curtail their demands and leaders to remain more recalcitrant in their concessions.

For qualitative evidence of the dangers of transparency for authoritarian stability, one must look no further than the recent revelations about the government permission for a Chinese bauxite mine in the Central Highlands of Vietnam. Well-known leaders challenged the legality of the policy both within the VNA query sessions and in other forums. In the June 2009 query session, the deputy prime minister was forced to fend off a series of difficult questions about why the highly controversial decision to allow Chinese investment in the bauxite mine was not discussed openly. After the session, the VCP committed to delivering periodic written reports to the VNA on the status of the bauxite project (Anh 2009; Vietnam News Service 2009). When the elite debate entered the public consciousness through a speech by Vietnam war hero, General Vo Nguyen Giap, however, it created a public display of intra-elite dissent that challenged the party's "performance legitimacy" and ignited the activity of street demonstrations, lawsuits against the Prime Minister, and dissident bloggers, who argued that the VCP had not gone far enough to rectify the situation.   Afraid that the nationalist protests would damage relations with China, the regime eventually cracked down hard on dissenters, particularly bloggers and journalists (Thayer 2009).

We take no normative stance on the legality of the Chinese mine, or the right of Vietnamese citizens to protest government actions. Our only objective is to point out that there is qualitative evidence that contained debates within the VNA can create firestorms of protest and political activity when revealed to the public.   Vietnamese delegates were aware of these dangers to the co-optive exchange at the time of our treatment

2. Query Sessions in the Vietnamese National Assembly

The 2007-2012 Vietnamese National Assembly (VNA) consists of 493 delegates, who represent 182 electoral districts based in Vietnam's 64 (now 63 provinces). These delegates ran for election in districts against competitors that were organized by the provincial election board (controlled by the provincial party committee) and elected through universal elections (voted on by provincial voters). It is difficult to know how much the delegates act in the interest of either of these two groups, who influence their election prospects. As party officials certainly have information on what their delegates are doing, we believe the primary effect of our experiment is to increase information on delegate responsiveness for the latter group: the voters.

Delegates can be distinguished based on their nomination status. About 153 are central nominees, meaning they were nominated by government, party, and military institutions in Hanoi, but are sent to provincial electoral districts to run for election. Therefore, they nominally represent the interests of the province where they were sent even though they may have never visited that area. Central nominees are often designated for leadership positions in the VNA and thus, are placed in easier-to-win districts with lower candidate-to-seat ratios and low competition (Malesky and Schuler, Forthcoming). 340 delegates are local nominees, meaning they are either local officials or other notables (researchers, educators, doctors, businessmen), who were nominated by provincial election boards and are expected to represent local interests. One delegate in Nghe An province is a self-nominee, the only survivor of the 30 self-nominees who made it to the final ballot.

Delegates can also be distinguished by whether they are fulltime or part-time members. Fulltime members work in the VNA year-round as opposed to the part-time delegates, who take part only in the biannual legislative and query sessions. Centrally-nominated fulltime delegates staff the National Assembly Standing Committee (NASC) or serve as chairs and deputy chairs of the ten legislative committees in the

VNA. Locally-nominated fulltime delegates are designated to work in the office of the provincial delegation, where they interact regularly with officials and citizens of the province.

The VNA query sessions have been conducted twice a year since the ratification of the 1992 Constitution, which sought to give the VNA a greater role in policymaking. Although policy is not made during these sessions, delegates present grievances from different regions and sectors of society directly to the ministers. Sometimes the question can become quite pointed, and in extreme cases force a minister to resign, such as when Minister of Transportation Dao Dinh Binh was forced to step down after failing to defend his role in a major corruption scandal. In 2003 a delegate requested no confidence after a particularly poor performance by the Minister of Education (Salomon 2007), while in 2010 another delegate called for a no-confidence vote on the sitting Prime Minister Nguyen Tan Dung.

Although most sessions are not quite so dramatic, they are still often uncomfortable for the ministers. In a more representative example, delegate Danh Ut pressed the Minister of Natural Resources and Environment Pham Khoi Nguyen in November 2008 on his handling of a scandal involving Taiwanese MSG producer Vedan's illegal dumping millions of gallons of untreated waste directly into a local river. After failing to provide a satisfactory answer to his first question, Danh Ut repeated his query:

> "Why hasn't Vedan been ordered to shut down? You said in Lao Dong [Labor] Newspaper on October 23, 2008 that you were going to close them down. However, you have yet to do so and have not yet answered why. Has anyone in the government been reprimanded yet? You also have failed to answer this question."

Many delegates, particularly from the Mekong Delta region, which is Vietnam's "rice bowl", will also press ministers on agricultural policy. For example, in November 2009 several delegates criticized the Minister of Industry and Commerce on a decision to halt rice exports during a global spike in rice prices – a decision which they claimed prevented rice farmers from being able to take advantage of the situation.

These week-long sessions have attained greater prominence in the broader public ever since the Party began televising them in the mid-1990s. Although most Vietnamese do not watch the full sessions,

many pay attention to the highlights that are shown on evening news programs and discussed in Vietnam's numerous newspapers. As a consequence, almost 84% of Vietnamese claim to be familiar with the query sessions in a recent survey. However, the highlights do not necessarily give voters a clear idea of what their delegates are doing. News programs tend to highlight the most controversial speeches and debates, while leaving a large portion of the interactions uncovered. Transcripts of queries and responses are posted on the National Assembly website after each session, but these are not presented in an easy to find location and have wildly differing titles. Moreover, the transcripts are posted in long *Word* files that make it difficult for a citizen to quickly identify what her delegate said. Given that delegate votes on individual bills are not made public, the lack of a systematic account of delegate performances during the query sessions means that voters have almost no way of knowing what delegates are up to in Hanoi.

Because of this general lack of transparency and the weakness of the electoral sanctioning mechanism, previous work has raised the question of what type of delegates are responsive at all. By combining query data with a dataset of delegate biographies, which includes information on career, party status, geographic representation education, demographics, previous electoral performance, and status in the VNA, Malesky and Schuler (2010) were are able to test whether delegates were responsive to their constituents by criticizing ministers and raising issues relevant to their localities. They used the data to examine whether institutional roles and electoral backgrounds have a measurable impact on the level and type of delegate activity in order to see how a regime might manage conflict within a parliament. This combined dataset allowed them to conduct the first comprehensive analysis of delegate behavior in an authoritarian system.

The authors drew three conclusions from this exercise. First, nomination procedures greatly impact whether delegates are responsive upward toward national leaders or downward to their underlying provincial constituency. Delegates who are more dependent on the central party and state for nomination and future promotion appear less willing to participate, challenge the government and represent the interests of their voters. Surprisingly for the setting, electoral competitiveness also matters. Delegates emerging from

closely contested elections raise more critical queries of national authorities than those with safe seats. This result provided some inspiration for the idea that transparency might have a positive effect in Vietnam. If close elections were a result of actual contestation, the positive effect on delegate activity might signal responsiveness to voters. Transparency could be expected to augment this effect.

Finally, the professionalism of delegates dictates how well they respond to constituency interests. Fulltime members, who have more information and a greater stake in their roles as representatives, tend to perform their representative functions more seriously. Specifically, fulltime local delegates are three times more likely to ask questions, criticize ministers, and twice as likely to reference local issues in their speeches as their peers.

Malesky and Schuler (2010) conclude that a non-random distribution of delegates were willing challenge regime leaders and respond to constituencies, even in a low transparency environment. In this paper, we press the issue further, asking whether transparency will inspire greater or lesser responsiveness.

3. Experimental Design

Our experiment was launched in January 2010, which was four months before the 6th session of the VNA. It was carried out by *VietnamNet*, a major online newspaper in the country. Vietnam's internet penetration is increasingly rapidly and currently reaches more than 24 million people, or 27.1 percent of the population, a rate which is higher than the world's average. Online newspapers have surpassed printed newspapers in terms of readership advertising revenue in the country. *VietnamNet* is already the leading online newspaper and by far the most-read outlet for political news. The French Ambassador Hervé Bolot commented in early 2010 that "Online newspapers like *VietnamNet* have really changed the media" of the country.

In collaboration with the newspaper, our main intervention launched individual websites on *VietnamNet* for randomly selected delegates in the National Assembly to inform citizens daily about these

delegates' activities in the National Assembly. This was motivated by an observation that constituents had very limited ability to monitor their delegates and therefore these delegates did not work actively to represent the interest of their constituents.[4] For this experiment, the newspaper launched a major political column called Đại Biểu Quốc Hội (or, Delegates of the National Assembly),[5] which introduced the selected delegates.

The selection of delegates into the program was randomized. Politburo and cabinet members, who were not expected to respond to the intervention because of their position, were removed from the initial list of 493 delegates. Among the remaining delegates, 144 were randomly selected for the experiment to have their individual websites posted. Besides these randomly selected candidates, the newspapers added five well-known delegates to boost the reputation of the website. Our analysis addresses this addition explicitly.

The experiment was implemented in two phases: the Introduction Phase and the Reporting Phase. The Introduction Phase started in mid-January 2010 and completed in mid-May 2010, just before the beginning of the National Assembly's session. The Reporting Phase was conducted in parallel with the National Assembly's session and completed when the session ended in mid-June 2010.

During the four months of the Introduction Phase, each day, the newspaper introduced one or two delegates on their political homepage and launched their individual websites. These websites presented photos, news articles, and CVs of the delegates (See sample in Appendix 1). All selected delegates were presented with the opportunity to have an interview with a *VietnamNet* reporter posted online, but only 59 delegates allowed themselves to be interviewed. The interviews were posted on their websites. Being posted prominently in the main political homepage of *VietnamNet*, the column gained popularity quickly. During the introduction phase, there were 1.3 million page views of the column, originating from a range

---

[4] In fact, only about 30% of delegates had ever spoken in the Assembly Hall. Most others follow the of the Vietnamese dictum "Silence is golden"

[5] On the English-language version of the site, this was translated as "Know Your Delegate."

of provinces throughout the country. Over 800,000 visitors clicked through to look at individual delegate pages.

The experiment moved to the Reporting Phase when the National Assembly started its session. To commence this phase, the newspaper hand-delivered a letter to each of the 144 treated delegates with the following wording:

> "VietnamNet Online Newspaper is pleased to inform you that your personal webpage on our Column 'Your Delegates' is followed by a large audience. You can access your website here: http://daibieuquochoi.vietnamnet.vn/XXXX. We will continue to update all your speeches and queries to your website daily. This website will provide information about the quantity and content of your speeches and queries in this session. The purpose of this website is for constituents to know the importance of the query session and understand your efforts in promoting the interests of your constituents."

At the same time, the newspaper put a scorecard on each delegate's website that shows performance in terms of: (i) The total number of the speeches and queries that the delegate made; (ii) The number of speeches and queries by the delegate that were critical of the government policies; (iii) The number of the speeches and queries by the delegate that were relevant to the interests of delegate's constituents, province, and profession; and (iv) Comparison of the delegate' performance in the above indicators with the best, average and worse delegates.

The delegates' performance scorecards were updated daily (see Appendix 3). The newspaper also published an overall chart on its political homepage that showed participation of the treated delegates, updated daily (see Appendix 2).

Balance between Treatment and Control Groups

In conducting our randomization, to ensure balance we stratified the sample frame of 462 delegates on the four variables that have been previously shown to determine who speaks in the query sessions. These variables included 1) whether or not the delegate was centrally nominated; 2) a fulltime delegate; 3) over the age of 65 (past retirement age), and 4) historical activity measured by how many questions they asked in previous sessions (Malesky and Schuler 2010).

<Insert Table 1 About Here>

Table 1 shows the result of our randomization with p-values and t-scores, based on t-tests comparing means of unequal variance between the treatment and control group for each variable. Positive t-scores indicate a higher mean score for the untreated group while negative t-scores indicate a lower t-score for the untreated group. The table indicates that balance was achieved on all of the stratification variables, which was expected.

Our experiment was conducted between the November 2009 and June 2010 assembly sessions. Prior to November 2009, there were four query sessions for the 12th VNA conducted in November 2007, May 2008, November 2008, and June 2009. Table 1 also shows that were able to achieve balance in terms of previous activity in the first five pre-treatment query sessions, with the untreated delegates somewhat less likely to speak than treated delegates, but not at levels of commonly accepted significance. However, of slightly greater concern is that members of our untreated group were significantly more likely to be in a province with greater central transfers and were more likely to have won with a larger percentage of the vote share in the 2007 election. These small deviations were expected, however, given the small sample size for the treatment.

Possible Uniqueness of the Sixth Session

An additional empirical concern with the experiment is that the most recent query session may be fundamentally different from previous sessions. Most strikingly, the 11th Party Congress took place in 2011. It is thought to be commonplace that political activity is restrained in the year before the congress, as politicians and bureaucrats jockey for high-level party appointments and shy away from activities that might bring negative attention to them before the congress. If this is true, we would still be able to evaluate the difference between treated and control delegates across levels of activity within the sixth session, but a diff-in-diff analysis, where we assesses comparative changes in delegate behavior, would be invalid.

Qualitatively, the 6th session of the VNA appeared no less active than previous session. In fact, it appears to be quite the opposite. In the sixth session, the VNA voted to reject a resolution (20% abstain; 38% in favor; 42% against), sponsored by the Prime Minister, to build a $56 billion high speed railway down the coast of the country. Putting aside the merits of the proposal, this was the first time that the VNA has ever rejected a piece of legislation sponsored by either the Prime Minister or President, and was immediately dubbed historic by the participants in the debate (Ninh 2010).

Quantitatively, the session does not appear to be very different from previous sessions either. Table 2 demonstrates that this session was well within the range of activity shown in previous sessions. In terms of quantity of activity, the 6th session ranked in the lower half; each delegate asked about 0.37 questions, but those who spoke asked over three questions per delegate. In terms of the quality of activity, the session was similar to previous sessions. The percentage of critical questions asked (31.46%), those using the name of their province (14.83%), or employing the word voter or constituency in a query (16.11%) were not out of the ordinary relative to other sessions.[6]

(Table 2 About Here)

---

[6] Detailed coding rules for critical and local questions can be found at (http://dvn.iq.harvard.edu/dvn/dv/emalesky) in the *Nodding or Needling* Archive.

Regression analysis also demonstrates that the types of delegates asking questions matches previous sessions. In an analysis of the first five query sessions of the 12th VNA, Malesky and Schuler (2010) found that the most active, critical, and constituency-oriented delegates tended to be locally-nominated full-time delegates, delegates who had survived close elections, representatives of southern provinces, and representatives of provinces that are net providers to the national budget. The 6th session mirrors these exact patterns, as seen in Table 3. Fulltime delegates spoke more often and more critically than other delegates, with locally-nominated, fulltime delegates speaking most of all. As in previous sessions, higher central transfers also appeared to depress participation.

While some of the coefficients narrowly miss traditional metrics of statistical significance, the sign of the effect is in the observed direction. As this is only one session (as opposed to five), it is reasonable to conclude that the size of the standard error is due to the lower number of observations. In sum, the June 2010 session does not appear vastly different from earlier sessions.

(Table 3 About Here)

The vote share a delegate received in the 2007 election is also significantly and negatively correlated with delegate participation, criticism, and representation of constituency in the query sessions. Each 10% increase in a delegate's vote share is associated with a 0.10 decrease in questions asked, a 6% decrease in the share of critical question, and a 3% decrease in the share of locally-oriented questions. As Figure 1, demonstrates, in every session after the 1st, this relationship is replicated -- there is a statistically significant and substantively large relationship between participation and vote share.

<Insert Figure 1 About Here>

The strong relationship between vote share and activity is important because it offers up the intriguing possibility of responsiveness to voters in an authoritarian regime, and more immediately, the incentive hypothesis might be upheld in Vietnam. In a democratic setting, we could imagine that the relationship implies that poor-performing delegates are working harder to be noticed by voters, to improve

their chances for re-election.   But as Malesky and Schuler (2010,Forthcoming) acknowledge, there are

reasons to be cautious that an electoral democratic interpretation can so easily be imposed.   While there is

universal voting, the election of candidates is a highly politicized process with candidates favored by the

regime receiving special accommodations that make their pathway to election easier. Provincial election

boards, made up of local leaders and members of the Vietnamese Fatherland Front, are charged with

selecting local candidates and organizing the electoral districts. As such, they have an important influence

on who is elected by what margin. Specially, centrally nominated candidates, those nominated from central

government and party branches in Hanoi, who are sent to compete in provinces, are placed in districts with

lower candidate-to-seat ratios are weaker competition (Malesky and Schuler 2009).   Well-known local

nominees also receive such benefits in certain provinces.

The enormous power of provincial election boards in determining the level of opportunity available

for a candidate calls into question the level of responsiveness of a particular delegate to underlying voters.

What we may be seeing in the relationship between vote share and level of participation is the

responsiveness to provincial leaders rather than voters. Because provincial leaders would have better

information already on the behavior of their delegates in the VNA, they would not benefit from the

increased availability of information on query sessions. If this is the case, our experimental treatment

should be ineffective at stimulating increased activity of delegates.

4. Direct Effects of Transparency Treatment

Dependent Variables and Empirical Design:

Due to space constraints, we focus our attention in the experimental analysis on two dependent

variables from Table 3: 1) how many questions delegates asked, and 2) how many questions were critical of

the government, a ministry, or a particular minister. We leave out speeches, because of its high correlation

with questions asked. Local and constituency based questions were analyzed, but do not offer substantively distinct results.

We employ a difference-in-differences framework in order to observe how the treatment may have altered performance, given delegate's behavior in previous sessions. This approach offers the most rigorous test, by ruling out the possibility that the average treatment effect may be driven by a few very active delegates, who were randomly assigned to the treatment group, as could happen with a single-shot difference-in-level analysis. Differences are calculated in three ways. In the first set of results, we examine the difference between delegate activity in the June 2010 session and the immediately previous November 2009 session (see Table 4: Panel A). While these results are telling, there is a possibility that activity in Session 5 may have been deviant.   Therefore, in the second set of results, we examined the difference between delegate participation in the June 2010 session versus an average score on each dependent variable from the previous five query sessions (Table 4: Panel B). Finally, there is a possibility that delegates' expertise drives their decisions to query ministers. For instance, scientists may feel more comfortable addressing science and technology issues than economic debates. To address this possibility, in the third set of results, we measured the difference between the delegates' speeches in the June 2010 session and an average of their level of activity with the same ministers in the previous sessions (Table 4, Panel C). Five ministers were questioned in the 6th session: The ministers of Rural Development; Finance; Transportation; and Culture, Sports and Tourism, and the Deputy Prime Minister. Totals from the Minister of Culture, Sports, and Tourism were not used, because he was not questioned in previous sessions. Caution should be taken on the similar minister analysis, however, as the number of questions asked is very small, so small movements can have quite large effects.[7]

[Table 4 About Here]

---

[7] In all three analyses we employ ordinary least squares (OLS) with robust standard errors, clustered at the provincial level.

Each panel contains six models. Models 1 and 2 calculate the direct effects of the experiment without control variables. Noticeably, the treatment effect is not significantly different from zero in any of the specifications.

These results are interesting, but slightly incorrect, as they do not include the stratification variables from the randomization exercise. To ensure balanced samples of different types of delegates, we stratified based on the nomination status of the delegate, whether they were fulltime, whether they would retire after the sixth session (age >65), and previous level of participation in the query session. In Model 3 & 4, we add control variables for the first three stratifications to ensure that our results are robust within delegate type. The fourth stratification variable is addressed automatically by the difference-in-differences estimator. Again, the treatment effect is not significantly different from zero in any specification.

Another concern is that the treatment may actually be conditional on delegate type. Consequently, we re-ran all three diff-in-diff analyses, allowing for interactions between the treatment and stratification variables. In addition, we interacted the treatment with vote share in the 2007 election. While vote share was not a stratification variable, its robust effect on delegate participation led us to wonder whether delegates may respond differently to the treatment if they must compete for votes, than if they are guaranteed a safe seat. The results of these models are displayed in Appendices 4-6. With one minor exception, the conditional effects were all insignificant and substantively small. The exception occurred in the questions of similar ministers (Appendix 6, Models 2 & 6), where we found that that treated local nominees asked significantly less questions and critical queries, while treated central nominees asked about 1/3 more questions, but were not significantly more critical than the control group. These results offer very tentative evidence that the adverse effects hypothesis operates on local nominees, while central nominees are more likely to face positive incentives for speaking. We should be cautious about interpreting too much from these results, however, as the effects are substantively small, not robust, and occur in the most fragile specification.

Models 5 & 6 in Table 4 explore the intensity of our experimental treatment. As we noted above, only 59 of the 143 treated delegates allowed their interviews to be posted on the *VietnamNet* website as part of the experiment's first stage, while all delegates were subjected to the scorecard posting. The interview stage was a necessary part of the experiment in order to build the website's credibility and receive permission from national authorities. Nevertheless, because the interviews were available for several months before the onset of the query session, it is possible that these delegates received an exceptionally strong treatment, as more viewers were familiar with them. Moreover, the interviews provided greater context for the scorecards and may have attracted more viewers while the query session was taking place. If this was the case, we should expect the interviewed delegates to receive higher treatment dosage and therefore to have more pronounced effects.

Although, there is not a significant treatment effect for the number of questions asked, the effect of the interviews on the percentage of critical questions is striking. Interviewed delegates asked 4.3% less critical questions between Session 5 & 6 and 5.4% less critical questions than their historical average, compared to the control group. In the similar minister analysis in Panel C, we find that when controlling for interviewed delegates, treated delegates ask about one less question than previously, compared to the control group.

These findings appear supportive of the adverse consequences hypothesis, as greater exposure to the treatment may lead to less critical activity in parliament. Unfortunately, the fact that delegates had the option to decline the interview stage creates a problematic selection effect. Delegates may have only agreed to the interview with the knowledge that they were unlikely to take part actively in the 6th query session. Delegates planning to be active may have shunned the higher profile. This selection process would be unobservable *ex ante* and would generate the same type of results as increased treatment dosage. To correctly analyze intensity of treatment, we need a measure that is exogenous to delegate participation.

5. Intensity of Treatment Effect

Internet Penetration as an Exogenous Measure of Intensity

The nature of our experiment allowed for the possibility that some delegates may have received a stronger treatment than others. This would occur as more visitors were drawn to a particular delegate's web page, thereby exposing him/her to a higher-level of scrutiny than peers in the treatment group. Under the incentive hypothesis, we would expect that increased exposure to the treatment would lead to more participation in query sessions. If as Prat (2005) argued, transparency during the policy-making process may curtail participation, we should find the reduction to be highest on more heavily exposed delegates.

Although it is the most direct measure, judging the level of exposure to the treatment by clicks on a delegate's page, is unfortunately endogenous to the quality and quantity of a delegate's query session performance. For instance, Le Minh Thuyet drew thousands of visitors to his query session page when he criticized the high speed rail by saying, "Some people say that all countries with high IQs have high speed railways. Well, I must not have a high IQ, because I think this is a very bad idea." The quote was humorous and pointed, as "some people" clearly referred to a specific high-level individual. Nevertheless, his hit count spiked after the statement, as readers rushed to read the statement in its entirety.

To avoid confusing dosage with popularity, we instead interact the treatment with the level of internet penetration in each province in 2009. This measure is certainly exogenous as the infrastructure and number of internet subscriptions, pre-date the 2010 experiment. Although official internet penetration in Vietnam is listed at 30% nationally, this includes assumptions made about the number of users at community access location, such as internet cafes, libraries and post-offices. Because these figures are inexact and subject to measurement error that may bias our results, we rely on the number of internet subscriptions per 100 residents in each province. The variable ranges from a penetration of 0.22 in the rural agricultural province of Thai Binh to a high of 8.63 in the capital city of Hanoi.

In Table 5, we provide the results of a regression of the number of total page views for a particular delegate on our measure of internet penetration. Total page views includes hits on the delegate's introductory page, interview page, and scorecard. Among treated delegates, the average page views was 6,300 with the most popular delegate receiving 60,000 hits and a few delegates receiving no hits at all. Although we display the aggregate amount, our findings are robust to regressing hits on each page separately. Model 1 provides the bivariate analysis. Model 2 controls for whether the delegate agreed to be interviewed. Model 3 controls for the number of questions asked by a delegate in Session 6 to address the endogeneity of page views. Model 4 adds individual-level control variables, Model 5 adds provincial structural controls that are plausibly correlated with both internet penetration and page views (GDP per capita, population, and whether the province is a national-level city), and Model 6 drops two outliers that received exceedingly low page views and therefore have a great deal of influence on the regression line (as measured by their Cook's D statistics).

[Table 5 About Here]

There a few things to note in Table 5. First, in the fully-specified model, the relationship between the exogenous internet penetration and page views is very strong. Each 1% increase in internet penetration increases page views by about 1,900 hits. Thus, we can conclude that delegates in areas with higher internet accessibility, more likely faced greater scrutiny of their participation from friends, co-workers, and local-leaders. The conditional effect of internet penetration can be observed directly in Figure 2, where we show the relationship in the full sample after outliers are dropped. Secondly, agreeing to be interviewed increased scrutiny on individual delegates. Interviewed delegates received 14,900 more hits than other treated delegates. Finally, page views alone cannot be used alone in a regression model, as it is clearly endogenous to the activity of delegates in the 6th Session. Each question a delegate asked drew an additional 1,300 viewers to their page.

[Figure 2 About Here]

Conditional Effect of Treatment and Internet Penetration

Confident that internet penetration provides a strong and exogenous measure of treatment intensity, we interact it with the treatment in Table 6. We re-run two diff-in-diff analyses from above: 1) the change between the 5th and the 6th sessions and 2) the change between the delegate average on previous sessions and the 6th session.[8] We also use the same dependent variables of questions asked and share of critical queries. Three models are displayed for each dependent analysis. The first model uses only the component variables and the multiplicative interaction, the second model controls for stratification variables, and the third model allows for provincial covariates.

Control variables

Although our treatment is randomly assigned, internet penetration is not, leading to the possibility that are results could be an artifact of omitted variable bias. Consequently, it is important to control for variables that may be associated with both internet penetration and increased (or decreased) participation in the VNA. These include: 1) a dummy variable for whether the locality is designated as one of Vietnam's five national-level cities, and therefore a populous, dense, and technologically advanced urban center; 2) GDP per capita to capture provincial wealth; 3) Population, measured in millions of citizens; 4) Transfers as a percentage of locally-produced revenue, which gauges the dependence of a province on central beneficence and has been shown to affect delegate participation (Malesky and Schuler 2010) and local-level market-orientation (Malesky and Taussig 2009); and 5) The quality of provincial governance as measured by the Vietnamese Provincial Competitiveness Index, an annual ranking of economic governance for private sector development in the country, measuring such issues as corruption, property rights protection, and local transparency (Malesky 2009).

---

[8]Due to space constraints, the diff-in-diff for similar ministers is presented in Appendix 7.

Intensity of Treatment Results

In Table 6, we illustrate that in the query sessions, internet penetration significantly magnifies the impact of the treatment in a negative direction for the number of questions asked and the percentage of critical queries. These results appear to be robust across specifications.

<Insert Table 6 About Here>

Substantively, each additional internet subscription per 100 citizens is associated with a 0.18 reduction in the number of questions asked in the treated group and 1.9% decrease in the percentage of critical queries between the 5th and 6th sessions. Thus, when internet penetration is about 8 (the level observed in Hanoi and Ho Chi Minh City) , we find that treated delegates ask a full question less and reduce their criticism more than 12% below the delegates in the control group. A highly significant difference, as measured by the t-value over 6. When we compare the treatment-intensity between the 6th session and average participation, we see similar though slightly less pronounced results. Here, the effect is a reduction of about 0.6 questions and 0.7% less criticism in the fully-specified model.

The component terms in the interaction are not robustly significant, but are uniformly positively signed. Nevertheless, we cannot say definitively that the transparency or internet penetration have independent effects on delegate participation. Figure 3 provides a graphic illustration of the predicted effects from the four, fully-specified models. Blue diamonds depict treated delegates, red triangles depict control delegates, and range bars show 90% confidence intervals around the predicted effects. The graphs show clearly that when number of questions asked is the dependent variable, internet penetration has a negative effect on treated delegates, while it has a positive effect on control delegates. The confidence intervals around the predicted values separate after about 4% penetration. For critical questions, the intensity of treatment is less pronounced, reducing the slope of internet penetration, although the effect remains positive for both treated and control delegates.

[Figure 3 About Here]

The results of the treatment-intensity analysis could lead to several interpretations. Based solely on the performance in the query sessions, delegates appear to behave according to the adverse consequences hypothesis. Delegates, receiving the strongest exposure to the "Know Your Delegate" web page were the most likely to curtail their activities and criticism of national policy and top leaders. As Prat (2005) hypothesized, transparency forced them to behave in a conforming manner, as they feared their comments may be revealed to the public. Such revelations may have led to public unrest and damaged the co-optive exchange worked out between delegates and the leadership.

Robustness Tests for Treatment Intensity

Before concluding that the adverse consequences hypothesis better explains behavior in authoritarian setting, additional robustness tests are required. Because internet penetration is a provincial-level variable that was not considered in the randomization exercise, its usage raises concerns about spurious correlation. If the Prat logic is correct that delegates who faced greater public scrutiny as a result of the treatment would be most likely to act in a conformist manner, we should observe the same behavior with alternative measures of public scrutiny. In Table 7, we re-run the four full specifications from Table 6, but employ three alternative measures of intensity. First, we use the share of the population that lives in urban areas, as urban citizens have greater access to print media, which found and began to discuss the "Know Your Delegate" page. In addition, higher population density means that words travels faster about a delegate's questions, and delegates are more likely to interact directly with constituents, co-workers, and local leaders, who may be aware of their participation. Secondly, we use the share of the population employed in government offices or state-owned-enterprises (SOEs) to capture the level of political awareness of the population. Third, we take the percentage of college students in the population, as the younger generation is more politically attuned and technologically savvy enough to access *VietnamNet*.

Figure 4 shows that these alternative measures of intensity are positively correlated with internet penetration, but are far from perfect substitutes.

[Figure 4 About Here]

Table 7 reveals that the alternative measures of treatment intensity demonstrate a similar pattern in all specifications. Although the findings are only statistically significant when it comes to the number of questions asked for urbanization and college share, the robustness of the pattern is telling. The interaction between the treatment and intensity measure is always negative and sizable, indicating that increased exposure reduces the willingness of delegates to participate and criticize. This is strong evidence for adverse consequences

[Table 7 About Here]

6. Conclusion

In this paper, we designed a randomized experiment to test whether the incentive or adverse consequences hypothesis best applied to transparency in authoritarian regimes. A randomly selected treatment group of 144 delegates from the Vietnamese National Assembly had transcripts and scorecards from the sixth legislative query session posted on the website of the country's most popular online newspaper, *VietnamNet*. The treatment was extremely strong, drawing over 1.3 million page views and over 800,000 hits to specific delegate pages.

With this online intervention, we used the internet penetration rate of a province as an exogenous measure of intensity of treatment. Surprisingly, but in line with the *adverse consequences hypothesis*, we learned that treated delegates in provinces with high internet penetration were significantly *more* likely to curtail their participation in the VNA query sessions. That is, the higher the exposure to transparency, the more likely a delegate was to behave in a conformist manner.

These findings should make us cautious about the export of transparency to authoritarian systems, where similarly-named institutions play wildly different roles in the polity. Moreover, it underscores the general lessons of the New School of Development Economics, that cookie-cutter approaches to economic development and democratic transition are not available, and that interventions need to subject to the strict scrutiny of randomized-evaluations. While we cannot say that increased transparency definitively decreases performance, we can say with some confidence that it does not bring the same benefits consistently found in democratic systems.

There may still be hope, however, for increased transparency in authoritarian systems. Humphreys and Weinstein (2009) and Besley (2005) also describe the *selection hypothesis*, whereby better informed voters toss-out under-performing politicians and replace them with voters who they suspect will do a better job in office. In the next stage of our experiment, we repeat our experiment with a second group of 140 delegates prior to the 2012 national election. This will allow us to differentiate the incentive and selection hypotheses in an authoritarian setting, and see if the selection hypothesis operates where the incentive hypothesis failed. At the same time, it will allow us to continue observing whether the adverse consequences hypothesis continues to dominate in the presence of possible electoral sanctioning.

Works Cited

Alt James, David Lassan, and David Skilling. 2001. Fiscal Transparency, Gubernatorial Popularity and the Scale of Government: Evidence from the States. EPRU Working Paper. University of Copenhagen.

Anh, Ngan. 2009. "Lawmaker Grills Deputy PM on Bauxite Project." *Thanh Nien Newspaper* June 14. www.lexisnexis.com/us/lnacademic/results/docview/docview.do?docLinkInd=true&risb=21_T85704 44167&format=GNBFI&sort=BOOLEAN&startDocNo=26&resultsUrlKey=29_T8570444170&cisb =22_T8570444169&treeMax=true&treeWidth=0&csi=10962&docNo=35 (July 15, 2010).

Bentham, Jeremy. 1816 [1999]. *Political Tactics* Ed Michael James, Cyprian Blamires, and Catherine Pease-Watkin. Oxford: Clarendon Press.

Banarjee, Abhijit, Donald Green, Jennifer Green and Rohini Pande. 2009. "Can Voters be Primed to Choose Better Legislators? Experimental Evidence from Rural India." Unpublished Manuscript, Massachusetts Institute of Technology.

Barber, Tony. 2009. "New Website Puts European Parliament Under Scrutiny." *Financial Times.* May 12. http://blogs.ft.com/brusselsblog/2009/05/new-website-puts-european-parliament-under-scrutiny/

Barro, Robert. 1973. "The Control of Politicians: An Economic Model." *Public Choice.* 14:1. 1942.

Bauhr, Monika and Marcia Grimes 2011. "Seeing the State: The Implications of Transparency for Societal Accountability." Presented at the Annual Meeting of the Midwest Political Science Conference, Chicago Il.

Besley, Timothy and Robin Burgess. 2002. "The Political Economy of Government Responsiveness: Theory and Evidence from India." *Quarterly Journal of Economics* 117 (November): 1415-1452.

Besley, Timothy, Rohini Pande, and Vijayendra Rao. 2006. "Political Selection and the Quality of Government: Evidence from South India." Unpublished Manuscript, London School of Economics.

Besley, Timothy and Andrea Prat. 2006. "Handcuffs for the Grabbing Hand? Media Capture and Government Accountability." *The American Economic Review.* 96:3. 720—736.

Besley, Timothy. 2005. "Political Selection." *Journal of Economic Perspectives* 19 (Summer): 43-60.

Blaydes, Lisa. 2006. "Who Votes in Authoritarian Elections and Why? Determinants of Voter Turnout in Contemporary Egypt." Presented at the Annual Meeting of the American Political Science Association. Philadelphia, PA, August 31- September 3.

Boix, Carles and Milan Svolik. 2007. "Non-tyrannical Autocracies." Unpublished Manuscript.

Brunetti, Aymo, and Beatrice Weder. 2003. "A free press is bad news for corruption," *Journal of Public Economics* 87, 1801-1824.

Buchanan, James M. 1989. "The Public- Choice Perspective," in *Essays on the Political Economy*. Honolulu: University of Hawaii Press, pp. 13–24.

Carey, John. 2010. "Transparency and Legislative Behavior." *Dartmouth Working Paper.*

Datta, Saugato. 2008. "Television Coverage and Political Voice: Evidence from Parliamentary Question Hour in India." *World Bank Group.*

Djankov, Simeon, Caralee McLiesh, Tatiana Nenova, and Andrei Shleifer. 2003. "Who Owns the Media?" *Journal of Law and Economics*, 46(2): 341–81.

Djankov, Simeon, Rafael La Porta, Florencio Lopez-de-Silanes, and Andrei Shleifer. 2008. "The Law and Economics of Self-Dealing." *Journal of Financial Economics*.

Ferejohn, John. 1986. "Incumbent Performance and Electoral Control." *Public Choice.* 50:1-3. 5—25.

Fox, Johnathan. .2007. "The uncertain relationship between transparency and accountability."*Development in Practice* .4/5: 663-771.

Frankel, Maurice. 2001. "Freedom of Information: Some International Characteristics." *Campaign for Freedom of Information*. (cf01.org.uk/pdf/amsterdam.pdf).

Gandhi, Jennifer, and Adam Przeworski. 2006. "Cooperation, Cooptation, and Rebellion under Dictatorships." *Economics and Politics* 18 (1): 1–26.

Gandhi, Jennifer, and Adam Przeworski. 2007. "Authoritarian Institutions and the Survival of Autocrats." *Comparative Political Studies* 40 (1): 1279–1301.

Gandhi, Jennifer, and Ellen Lust-Okar. 2009. "Elections under Authoritarianism." *American Review of Political Science* 12: 403–22.

Gandhi, Jennifer and James Vreeland. 2004. "Political Institutions and Civil War: Unpacking Anocracy." Unpublished Manuscript.

Gandhi, Jennifer. 2008. "Dictatorial Institutions and Their Impact on Economic Growth." *European Journal of Sociology* 49: 3–30.

Gandhi, Jennifer. 2009. *Political Institutions under Dictatorship.* New York: Cambridge University Press.

Geddes, Barbara. 2006. "Why Parties and Elections in Authoritarian Regimes." Revised version of paper presented at the 2005 Annual Meeting of the American Political Science Association, Washington, DC.

Grose, Christian and Carrie A. Russell. 2010. "Avoiding the Vote: A Theory of the Social Costs of Public Political Participation." Working Paper, Vanderbilt University.

Haber, Stephen. 2006. "Authoriarianism." Chapter 38 In *The Oxford Handbook of Political Economy.* Eds. Barry Weingast and Donald Wittman. 693—707.

Holmström, Bengt. 1999. "Managerial Incentive Problems: A Dynamic Perspective." *Review of Economic Studies*. January 66(1): 169-182.

Humphreys, Macartan and Jeremy Weinstein. 2007. "Policing Politicians: Citizen Empowerment and Political Accountability in Africa." Presented at the Annual Meeting of the American Political Science Association. Chicago 2007.

Hyde, Susan and Nikolay Marinov. 2009. "National Elections Across Democracy and Autocracy: Putting the 'Competitive' into Competitive Authoritarianism." Unpublished Manuscript.

Kaufmann, Daniel. & Bellvar, Ana. 2005. "Transplanting Transparency: Initial Empirics and Policy Applications." *MRPA Paper* No. 8188.

Lassen, David. 2005. "The Effect of Information on Voter Turnout: Evidence from a Natural Experiment." *American Journal of Political Science* 49(1): 103-118.

Lindstedt, Catherina and Daniel Naurin. 2010. "Transparency is Not Enough: Making Transparency Effective in Reducing Corruption*," International Political Science Review* 31.3: 301–322Lust-Okar, Ellen. 2006. "Elections under Authoritarianism: Preliminary Lessons from Jordan." *Democratization* 13 (3): 456–71.

Magaloni, Beatriz, and Ruth Kricheli. 2010. "Political Order and One Party Rule." *Annual Review of Political Science* 13: 123–43.

Magaloni, Beatriz. 2006. *Voting for Autocracy*. New York: Cambridge University Press.

Magaloni, Beatriz. 2007. "Elections under Autocracy and the Strategic Game of Fraud." Revised version of a paper presented at the 2005 Annual Meeting of the American Political Science Association, Washington, DC.

Malesky, Edmund, and Paul Schuler. Forthcoming . "The Single-Party Dictator's Dilemma Information in Elections without Opposition." *Legislative Studies Quarterly*. .

Malesky, Edmund, and Paul Schuler. 2009. "Paint-by-numbers Democracy: The Stakes, Structure, and Results of the 2007 Vietnamese National Assembly Election." *Journal of Vietnamese Studies* 4 (1): 1–48.

Malesky, Edmund, and Paul Schuler. 2010. "Nodding or Needling: Analyzing Delegate Responsiveness in an Authoritarian Parliament." *American Political Science Review* (August, Forthcoming).

Malesky, Edmund and Markus Taussig. 2009. "Where is Credit Due? Legal Institutions, Connections, and the Efficiency of Bank Lending in Vietnam." *The Journal of Law, Economics, & Organization.* 25:2. 535—578.

Malesky, Edmund. 2009. "The Vietnam Provincial Competitiveness Index: Measuring Economic Governance for Private Sector Development." *2009 Final Report, Vietnam Competitiveness Initiative Policy Paper #14.* US AID's Vietnam Competitiveness Initiative and Vietnam Chamber of Commerce and Industry: Hanoi.

McCubbins, Mathew D & Thomas Schwartz. 1984. "Congressional oversight overlooked: policy patrols versus fire alarms." *American Journal of Political Science*. 281: 165-79.

Naurin, Daniel. 2007. *Deliberation Behind Closed doors. Transparency and Lobbying in the European Union.* Colchester: ECPR Press.

Ninh, Kim. 2010. "Votes National Assembly Votes on Contested High-Speed Rail Project." *In Asia*. July 5th. < http://asiafoundation.org/in-asia/2010/07/07/vietnams-national-assembly-votes-on-contested-high-speed-rail-project/>

Peruzzotti, Enrique, & Smulovitz, Cataline (Eds.). 2006. *Enforcing the Rule of Law: Social Accountability in the New Latin American Democracies.* Pittsburgh, PA: University of Pittsburgh Press, xiv, 362 pp.

Peisakhin, Leonid and Paul Pinto. 2010. "Is transparency an effective anti-corruption strategy? Evidence from a field experiment in India," *Regulation & Governance* 4.3: 261–280.Prat, Andrea. 2005. "The Wrong Kind of Transparency." *American Economic Review* 80: 21-36.

Quality of Government: Evidence from South India." Unpublished Manuscript, London

Reinikka, Ritva and Jakob Svensson. 2006. "The Power of Information: Evidence from a Newspaper Campaign to Reduce Capture of Public Funds." Unpublished Manuscript, Institute for International Economic Studies, Stockholm School of Economics.

Salomon, Mathieu. 2007. "Power and Representation at the Vietnamese National Assembly: The Scope and Limits of Political Doi Moi." in *Vietnam's New Order*, eds. Stephanie Balme and Mark Sidel New York: Palgrave MacMillan. 198–216.

Simpser, Alberto. 2005. "Making Votes Not Count: Strategic Incentives for Electoral Corruption." Ph.D diss. Stanford University.

Smulovitz, Catalina and Peruzzotti, Enrique. 2000. "Societal Accountability in Latin America." *Journal of Democracy* 1.4: 147-158.

Stasavage, David. 2004. Open-Door or Closed-Door? Transparency in Domestic and International Bargaining. *International Organization* (2004), 58: 667-703

Strömberg, David. 2004. "Radio's Impact on Public Spending." *Quarterly Journal of Economics* 119(1): 189-212.

Thayer, Carlyle. 2009. "Political Legitimacy of Vietnam's One Party-state: Challenges and Responses." *Journal of Current Southeast Asian Affairs* 4: 47–70.

Vietnam Development Report. 2010. *Modern Institutions.* Joint Donor Report to the Vietnam Consultative Group Meeting. Hanoi, December 3-4, 2009.

Vietnam News Service (VNS). 2009. "Bauxite Mining Needs Careful Plan: Politburo." July 8. http://www.lexisnexis.com/us/lnacademic/results/docview/docview.do?docLinkInd=true&risb=21_T9745916755&format=GNBFI&sort=BOOLEAN&startDocNo=26&resultsUrlKey=29_T9745916758&cisb=22_T9745916757&treeMax=true&treeWidth=0&csi=227171&docNo=28 (July 15, 2010)

Wright, Joseph. 2008. "Do Authoritarian Institutions Constrain? How Legislatures Affect Economic Growth and Investment." *American Journal of Political Science*. 52:2. 322-343.
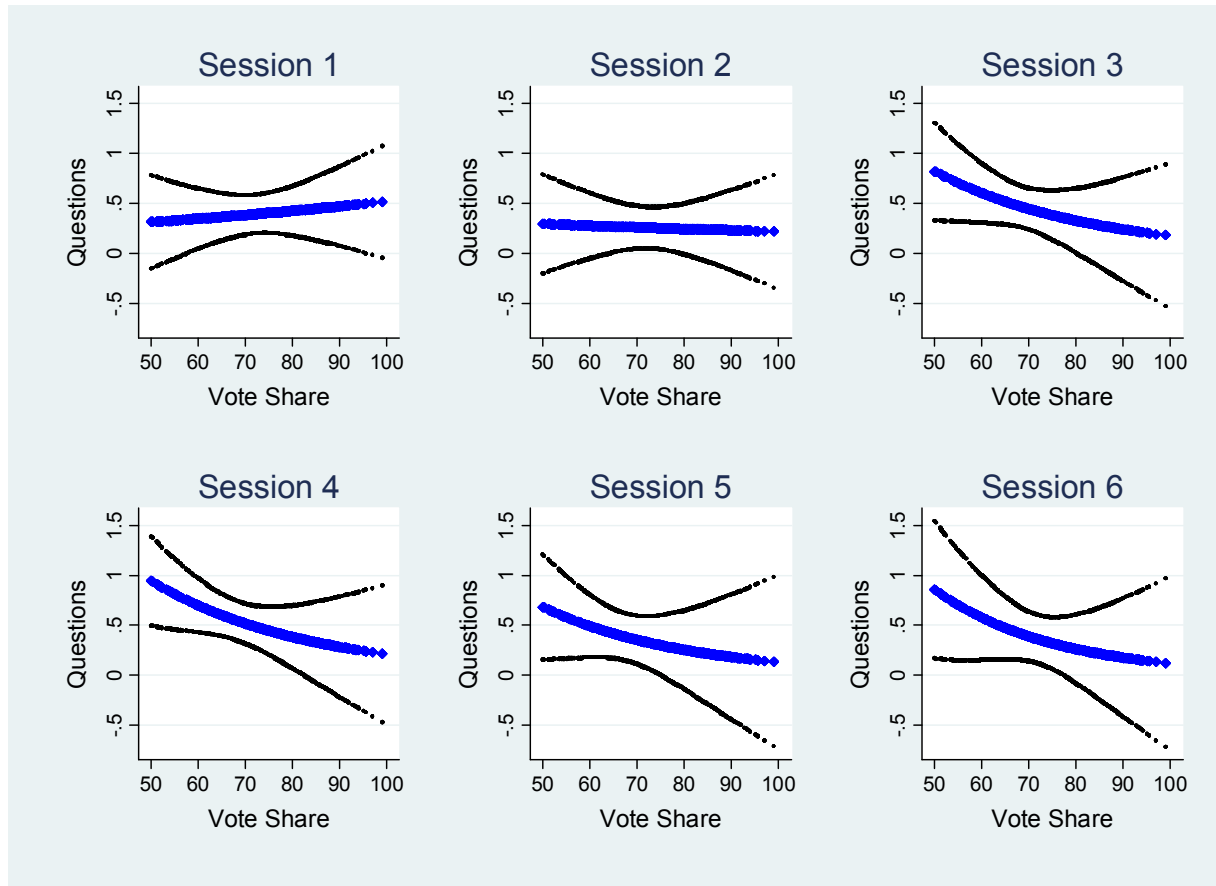
Figure 1: Relationship between Vote Share and Participation. This figure displays the bivariate correlation between vote share and participation in query sessions in all six sessions. The statistically significant negative relationship in the final four sessions indicates that delegates are responding either to voters or local leaders who arrange provincial electoral districts. Note that Session 6 demonstrates the same pattern as previous sessions and the relationship is not altered by proximity to the 11th Party Congress.
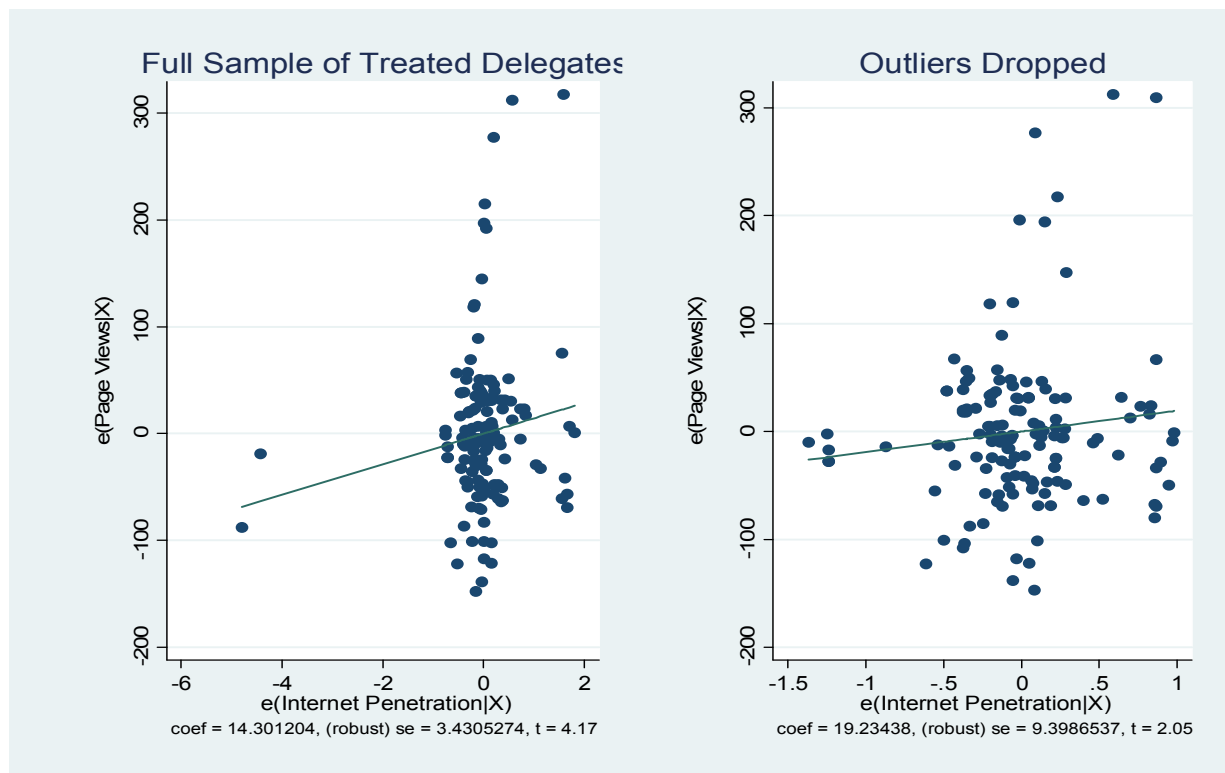
Figure 2: Conditional Effect of Internet Penetration on Page Views. This figure displays the conditional correlation between internet penetration and total page views (measured in 100s) after controlling for stratification variables and provincial covariates (Models 4 & 5 of Table 5). The first panel uses the full sample of delegates, while the second panel drops the two outliers which appear to be driving the result. In the fully-specified model with dropped outliers, a 1% increase in internet penetration accounts for 1,900 additional page views on a delegate's main page, scorecard, or interview page.
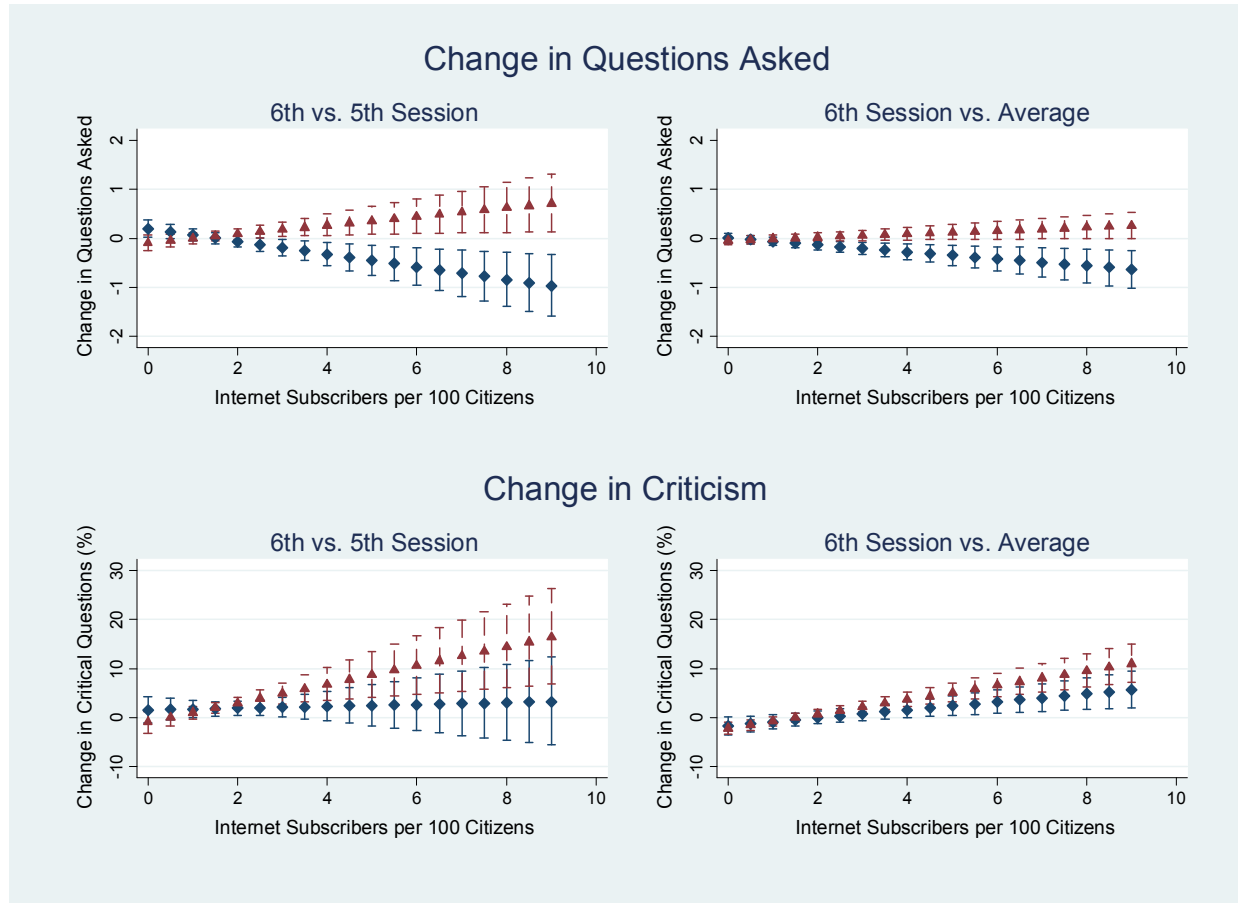
Figure 3: Intensity of Treatment Effects. This figure displays the predicted change in number of critical questions asked and percentage of critical questions, based on treatment and internet penetration, which impacts the intensity experienced by delegates. The panels are derived from the fully-specified models (3, 6, 9, and 12) in Table 6. **Blue diamonds** depict treated delegates, **red triangles** depict control delegates, and range bars show 90% confidence intervals around the predicted effects.
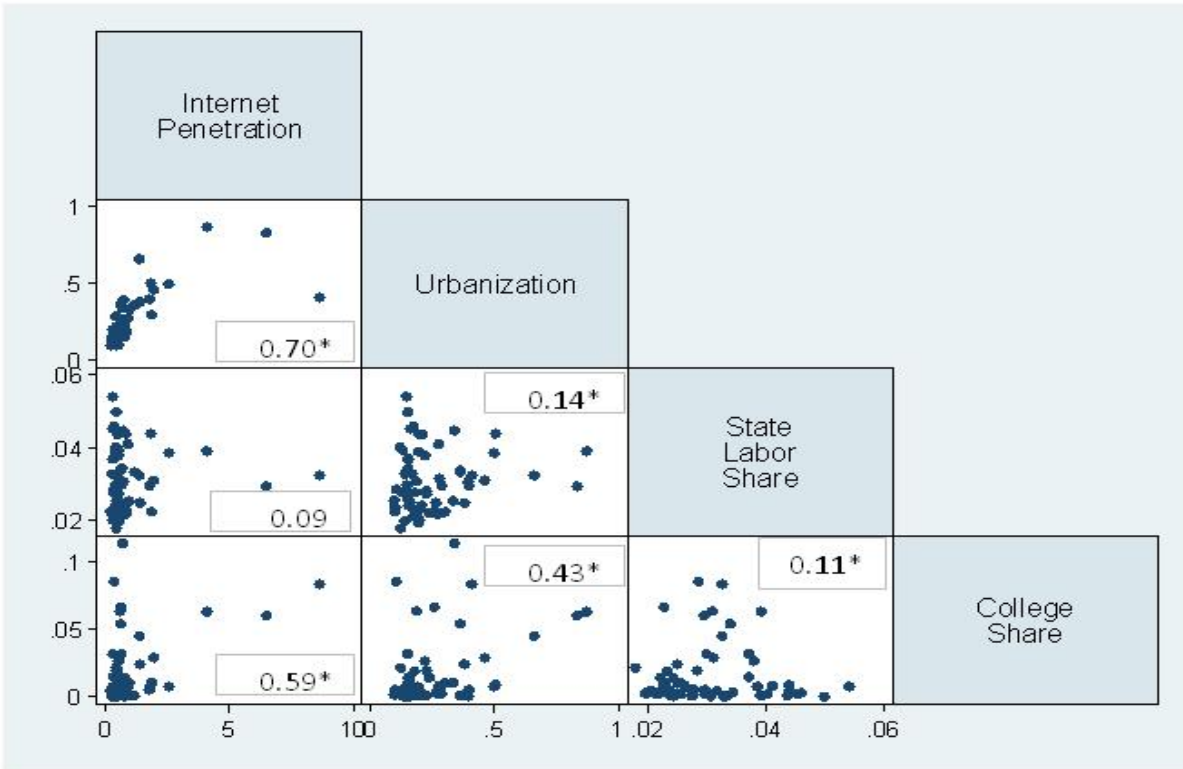
Figure 4: Scatter Plot Matrix of Alternative Measures of Treatment Intensity. Bivariate correlations displayed in boxes. *p<.05

Table 1: Balance and Descriptive Statistics										
Other Variables	**Treatment Group (N=144)**				**Control Group (N=318)**				**Difference in Means**	
	Mean	**Std. Dev.**	**Min**	**Max**	**Mean**	**Std. Dev.**	**Min**	**Max**	**P-Value**	**T-Score**
Age	48.24	8.73	25.00	71.00	49.63	8.35	24.00	80.00	0.11	1.62
Male	0.64	0.48	0.00	1.00	0.74	0.44	0.00	1.00	0.03	2.24
Minority	0.18	0.38	0.00	1.00	0.19	0.39	0.00	1.00	0.66	0.44
Religion	0.01	0.12	0.00	1.00	0.04	0.19	0.00	1.00	0.10	1.63
Party Member	0.88	0.33	0.00	1.00	0.93	0.26	0.00	1.00	0.12	1.51
Vote Share (%)	70.98	10.47	51.46	95.61	73.54	10.86	50.03	93.85	0.02	2.32
Speeches (Sessions 1-5)	1.17	2.19	0.00	13.00	0.91	1.83	0.00	13.00	0.20	-1.29
Criticisms (Sessions 1-5)	0.65	1.46	0.00	9.00	0.52	1.45	0.00	15.00	0.36	-0.92
Provincial Variables	**Mean**	**Std. Dev.**	**Min**	**Max**	**Mean**	**Std. Dev.**	**Min**	**Max**	**P-Value**	**T-Score**
GDP (Billion VND)	14.23	0.55	0.79	111.82	15.15	0.78	0.79	111.82	0.34	-0.95
Population (Million)	1.69	0.04	0.31	6.35	1.70	0.03	0.31	6.35	0.89	0.14
National-Level City	0.11	0.01	0.00	1.00	0.14	0.01	0.00	1.00	0.05	1.98
Governance	58.41	0.18	46.74	72.87	57.97	0.14	45.29	72.87	0.06	-1.89
Internet Penetration	1.39	0.07	0.22	8.63	1.28	0.05	0.22	8.63	0.18	-1.33
Urbanization	0.26	0.01	0.10	0.83	0.27	0.00	0.10	0.87	0.45	0.76
State Labor Share	0.03	0.00	0.02	0.05	0.03	0.00	0.02	0.05	0.00	3.70
Southern Province	0.57	0.50	0.00	1.00	0.51	0.50	0.00	1.00	0.26	-1.14
Transfers/GDP	79.13	86.42	0.32	552.90	97.10	102.50	0.32	552.90	0.06	1.90
College Share	0.02	0.00	0.00	0.11	0.02	0.00	0.00	0.11	1.70	0.09
Stratification Variables	**Mean**	**Std. Dev.**	**Min**	**Max**	**Mean**	**Std. Dev.**	**Min**	**Max**	**P-Value**	**T-Score**
Centrally Nominated	0.26	0.44	0.00	1.00	0.29	0.46	0.00	1.00	0.45	0.75
Fulltime	0.30	0.46	0.00	1.00	0.29	0.45	0.00	1.00	0.87	-0.16
Questions (Sessions 1-5)	2.31	4.67	0.00	32.00	1.74	3.56	0.00	24.00	0.18	-1.33
Retirement	0.03	0.16	0.00	1.00	0.03	0.18	0.00	1.00	0.70	0.38

Table 2: Summary Statistics by Session					
For all delegates					
Session	**Speeches**	**Questions**	**Critical (%)**	**Local (%)**	**Constituency (%)**
1 Nov. 2007	0.21	0.40	4.15%	2.76%	3.29%
2 May 2008	0.18	0.26	5.16%	1.68%	2.87%
3 Nov. 2008	0.24	0.42	6.94%	2.90%	3.42%
4 Jun. 2009	0.21	0.49	3.68%	3.11%	3.52%
5 Nov. 2009	0.16	0.33	2.42%	0.60%	2.00%
6 Jun. 2010	0.17	0.37	3.01%	1.44%	1.57%
For delegates asking at least one question*					
Session	**Speeches**	**Questions**	**Critical (%)**	**Local (%)**	**Constituency (%)**
1 Nov. 2007	1.33	2.51	25.93%	17.25%	20.51%
2 May 2008	1.36	1.98	39.74%	12.94%	22.08%
3 Nov. 2008	1.56	2.79	45.60%	19.04%	22.51%
4 Jun. 2009	1.37	3.18	23.90%	20.15%	22.86%
5 Nov. 2009	1.68	3.51	25.40%	6.31%	21.03%
6 Jun. 2010	1.71	3.77	31.46%	14.83%	16.11%

* Or making at least one speech during the debates. Detailed coding rules for critical and local questions can be found at (http://dvn.iq.harvard.edu/dvn/dv/emalesky) in the *Nodding or Needling* Archive.

Table 3: Determinants of Participation in June 2010 Session					
Dependent Variable	Speeches (NBREG)	Questions (NBREG)	LN Critical % (OLS)	LN Local % (OLS)	LN Constituency % (OLS)
Model	(1)	(2)	(4)	(5)	(6)
Fulltime	1.516***	1.765***	0.0565**	0.0290	0.0393*
	(0.419)	(0.462)	(0.0252)	(0.0175)	(0.0198)
Centrally Nominated	0.402	0.685	-0.00673	-0.00394	-0.00450
	(0.610)	(0.713)	(0.00860)	(0.00648)	(0.00564)
Fulltime*Centrally Nominated	-0.953	-1.291	-0.0320	-0.0274	-0.0305
	(0.818)	(0.950)	(0.0284)	(0.0196)	(0.0194)
Vote Share (%)	-0.0464**	-0.0505***	-0.00149**	-0.000256	-0.000541*
	(0.0184)	(0.0186)	(0.000654)	(0.000273)	(0.000302)
South of 17th Parallel	-0.183	-0.0953	-0.0245	0.00405	-0.0113
	(0.306)	(0.298)	(0.0150)	(0.00674)	(0.00784)
Transfers	-0.00124	-0.00251	-9.85e-05**	-5.10e-05**	-4.79e-05*
	(0.00162)	(0.00179)	(4.39e-05)	(2.36e-05)	(2.59e-05)
Constant	1.145	2.139*	0.152***	0.0329	0.0603**
	(1.304)	(1.249)	(0.0554)	(0.0204)	(0.0249)
N	493	493	493	493	493
Clusters	64	64	64	64	64
R-squared			0.052	0.026	0.037
Chi-Squared	23.61	24.95			
Log Likelihood	-204.6	-263.0			

This table depicts the results of the controlled field-experiment across five different dependent variables and four different types of analyses. The dependent variable are: 1) Number of speeches made by a delegate; 2) Number of questions asked by a delegate; 3) Percentage of queries which were critical of ministers, ministries, or the national government; 4) Percentage of questions which used the name of the province the delegate represents; 5) Percentage of questions which used the words "constituency" or "voter." Analysis was divided into four separate tests: A) Difference in Levels within Session 6. For this analysis negative binomial regression was employed for the first two variables, as they are count variables demonstrating evidence of over-dispersion. Log-linear regression was employed on the second three variables, as these were percentages censored at 0 on the left-side and 100 on the right-side. Robust standard errors, clustered at provincial-level are shown in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 4: Direct Effects of Controlled-Experiment on Query Session Participation

Panel A: Difference in Differences between Session 5 (November 2009) and Session 6 (June 2010)

Dependent Variable	Questions (#)	Critical (%)	Questions	Critical (%)	Questions	Critical (%)
Model	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	0.016	0.942	0.016	0.016	-0.025	2.452
	(0.146)	(1.802)	(0.146)	(0.146)	(0.168)	(2.459)
Agreed to Interview					0.110	-4.318*
					(0.219)	(2.500)
Constant	0.019	1.389	0.019	0.019	-0.054	1.012
	(0.078)	(0.948)	(0.078)	(0.078)	(0.087)	(0.991)
Block Fixed Effects	No	No	Yes	Yes	Yes	Yes
Observations	461	461	461	461	461	461
R-Squared	3.07e-05	0.000758	3.07e-05	3.07e-05	0.00626	0.0233
RMSE	1.347	15.85	1.347	1.347	1.349	15.74

Panel B: Difference in Differences between Average Delegate Performance Sessions (1-5) and Session 6 (June 2010)

Dependent Variable	Questions (#)	Critical (%)	Questions	Critical (%)	Questions	Critical (%)
Model	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	-0.055	0.454	-0.055	0.454	-0.085	2.450
	(0.106)	(1.436)	(0.106)	(1.436)	(0.122)	(1.893)
Agreed to Interview					0.085	-5.420***
					(0.197)	(1.927)
Constant	-0.011	-0.501	-0.011	-0.501	-0.059	-0.422
	(0.062)	(0.812)	(0.062)	(0.812)	(0.062)	(0.880)
Block Fixed Effects	No	No	Yes	Yes	Yes	Yes
Observations	461	461	461	461	461	461
R-Squared	0.000546	0.000241	0.000546	0.000241	0.00557	0.0176
RMSE	1.091	13.54	1.091	13.54	1.093	13.48

Panel C: Difference in Differences between similar ministers in previous sessions and Session 6 (June 2010)

Dependent Variable	Questions (#)	Critical (%)	Questions	Critical (%)	Questions	Critical (%)
Model	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	-0.048	0.003	-0.048	0.003	-0.096**	-0.027
	(0.052)	(0.031)	(0.054)	(0.032)	(0.044)	(0.017)
Agreed to Interview					0.125	0.078
					(0.096)	(0.064)
Constant	0.062**	0.029***	0.013	0.001	0.018	0.004
	(0.031)	(0.011)	(0.032)	(0.015)	(0.030)	(0.013)
Block Fixed Effects	No	No	Yes	Yes	Yes	Yes
Observations	461	461	461	461	461	461
R-Squared	0.00162	2.65e-05	0.0175	0.0216	0.0211	0.0282
RMSE	0.553	0.258	0.551	0.256	0.550	0.255

This table depicts the results of the controlled field-experiment across five different dependent variables and four different types of analyses. The dependent variables in Panels B-D are: 1) Number of questions asked by a delegate; 2) Percentage of queries which were critical of ministers, ministries, or the national government. Analysis was divided into three separate tests: A) Difference-in-Difference between Session 6 and Session 5. ; B) Difference-in-Difference between Session 6 and average questions asked in Sessions 1-5=; C) Difference-in Difference between queried ministers in Session 6 and similar ministers in Sessions 1-5. OLS Coefficients are displayed with robust standard errors, clustered at provincial-level are shown in parentheses. Models 1 and 2 of each panel display the unadjusted average treatment effect. Model 3 & 4 add fixed effects for the four stratification variables used in the randomization process (1) Nomination Status; 2) Fulltime v. Part-time; 3) Retirement Age; 4) Previous Activity). Models 5&6 add an intensity of treatment control, based on whether the subject agreed to have his/her interview posted on *Vietnamnet* before the query session. *** p<0.01, ** p<0.05, * p<0.1

Table 5: Determinants of Page Views for Treated Delegates

Dependent Variable= 100s of Page Views	Baseline (1)	Interview (2)	Participation (3)	Individual (4)	Provincial (5)	Outliers Dropped (6)
Internet Penetration Rate	7.587***	3.623***	2.734*	4.014***	14.301***	19.234**
	(2.340)	(1.347)	(1.428)	(1.391)	(3.431)	(9.399)
Agreed to Interview		165.331***	161.656***	147.072***	147.697***	148.789***
		(15.225)	(15.422)	(13.725)	(13.973)	(14.083)
Questions in Sixth Session			15.276*	12.408*	13.245*	13.395*
			(7.967)	(6.862)	(6.750)	(6.732)
Centrally Nominated				-2.798	1.022	-0.695
				(20.832)	(18.920)	(19.577)
Fulltime				51.506**	55.434***	54.500**
				(20.186)	(20.696)	(20.955)
Retirement				15.631	-8.003	-5.744
				(55.464)	(52.834)	(53.670)
Female				14.766	7.498	7.795
				(11.501)	(11.795)	(12.022)
GDP per Capita (ln)					-20.617	-24.194*
					(13.043)	(14.317)
Population (ln)					48.034***	49.804***
					(10.782)	(11.579)
National-Level City					-109.416***	-145.623**
					(24.493)	(67.436)
Constant	53.808***	-4.252*	-7.688**	-22.931**	0.809	3.930
	(9.832)	(2.259)	(3.751)	(9.134)	(24.712)	(25.644)
Observations	143	143	143	143	143	141
R-squared	0.020	0.503	0.534	0.566	0.609	0.610
RMSE	114.8	82.06	79.71	78.06	74.92	75.33

Dependent variable is the total page views on delegates main page, scorecard, and interview page. OLS coefficients displayed with robust standard errors, clustered at province level, in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 6: Intensity of Treatment Effect (Interaction between Treatment and Internet Penetration)

Specification	A. Difference in Differences between Session 5 (November 2009) and Session 6 (June 2010)						B. Difference in Differences between Average Delegate Performance Sessions (1-5) and Session 6 (June 2010)					
Dependent Variable	**Question Count (#)**			**Critical Questions (%)**			**Question Count (#)**			**Critical Questions (%)**		
Model	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Treated	0.247*	0.249	0.271*	3.188	3.013	2.799	0.007	0.011	0.021	1.067	0.965	0.937
	(0.147)	(0.151)	(0.154)	(2.008)	(1.956)	(1.907)	(0.124)	(0.128)	(0.129)	(1.756)	(1.728)	(1.762)
Internet Penetration	0.058	0.062*	0.105	0.744	0.771	2.850***	0.028	0.033	0.048	0.724	0.731	2.247***
	(0.035)	(0.033)	(0.070)	(0.578)	(0.593)	(0.843)	(0.035)	(0.034)	(0.033)	(0.522)	(0.544)	(0.319)
Treated*Internet	-0.170***	-0.171***	-0.179***	-1.673***	-1.658***	-1.865***	-0.047	-0.048	-0.058**	-0.501*	-0.493	-0.738***
	(0.030)	(0.027)	(0.027)	(0.383)	(0.381)	(0.330)	(0.030)	(0.029)	(0.027)	(0.294)	(0.304)	(0.276)
Centrally Nominated		0.057	0.046		-3.411**	-3.803**		0.124	0.120		-1.871	-2.200*
		(0.149)	(0.149)		(1.462)	(1.449)		(0.147)	(0.147)		(1.267)	(1.266)
Fulltime		0.192	0.209		4.392*	4.522*		0.079	0.082		1.453	1.579
		(0.205)	(0.213)		(2.330)	(2.383)		(0.158)	(0.164)		(2.066)	(2.136)
Retirement		0.208	0.289		-0.383	-0.150		-0.136	-0.069		-2.387	-1.932
		(0.217)	(0.239)		(1.374)	(1.377)		(0.182)	(0.205)		(2.518)	(2.592)
National-level city			-0.066			-12.064**			-0.114			-9.919***
			(0.429)			(5.321)			(0.173)			(1.703)
GDP per Capita (ln)			-0.038			-2.488			0.174			0.037
			(0.205)			(1.746)			(0.150)			(1.412)
Population (ln)			-0.150			-1.239			-0.163*			-1.283*
			(0.101)			(0.975)			(0.083)			(0.735)
Transfer/Revenue			0.000			-0.016**			-0.000			-0.011*
			(0.001)			(0.007)			(0.001)			(0.006)
South			-0.251*			-3.041*			-0.287**			-4.215***
			(0.150)			(1.548)			(0.112)			(1.299)
Governance			0.015			0.098			0.008			0.017
			(0.012)			(0.146)			(0.009)			(0.118)
Constant	-0.055	-0.139	-0.825	0.439	0.159	1.739	-0.047	-0.107	-0.687	-1.425	-1.221	0.734
	(0.088)	(0.099)	(0.748)	(1.042)	(1.248)	(8.613)	(0.070)	(0.083)	(0.533)	(0.872)	(1.075)	(7.071)
Observations	461	461	461	461	461	461	461	461	461	461	461	461
R-squared	0.015	0.021	0.033	0.012	0.029	0.056	0.003	0.008	0.024	0.009	0.015	0.048
RMSE	1.339	1.340	1.341	15.80	15.71	15.59	1.092	1.093	1.091	13.51	13.52	13.38

This table depicts the results of the controlled field-experiment across four different dependent variables. In this analysis, the treatment is interacted with internet penetration to derive an exogenous intensity of treatment effect. The dependent variables are: 1) Number of questions asked by a delegate; 2) Percentage of queries which were critical of ministers, ministries, or the national government. Panel A uses Difference-in-Difference between Session 6 and Session 5. Panel B analyzes Difference-in-Difference between Average Questions and Session 6. All analyses used OLS on the first differences between sessions. Robust standard errors, clustered at provincial-level are shown in parentheses. The first model in each set is unadjusted, the second model controls for individual-level stratification variables, and the third model controls for provincial co-variates that are potentially associated with internet penetration. *** p<0.01, ** p<0.05, * p<0.1

Table 7: Robustness of Intensity of Treatment Effects

Specification	A. Difference in Differences between Session 5 (November 2009) and Session 6 (June 2010)						B. Difference in Differences between Average Delegate Performance Sessions (1-5) and Session 6 (June 2010)					
Dependent Variable	**Question Count (#)**			**Critical Questions (%)**			**Question Count (#)**			**Critical Questions (%)**		
Model	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Treatment	0.421*	0.547	0.276	3.769	12.291	2.354	0.098	0.398	0.097	2.947	5.235	1.346
	(0.222)	(0.612)	(0.174)	(2.807)	(7.391)	(2.191)	(0.193)	(0.489)	(0.134)	(2.457)	(6.348)	(1.995)
Urban (%)	0.511			-6.917			-0.272			-3.256		
	(0.672)			(8.485)			(0.562)			(9.196)		
Treatment*Urban	-1.503***			-11.196			-0.590			-8.987		
	(0.549)			(7.200)			(0.533)			(5.640)		
State Labor Share		24.352*			273.883**			10.781			233.067*	
		(12.236)			(131.406)			(9.941)			(127.277)	
Treatment*State		-17.396			-381.642			-15.142			-152.929	
		(20.695)			(228.591)			(17.318)			(194.279)	
College Students (%)			4.650			10.613			3.257			-4.385
			(3.958)			(36.206)			(3.266)			(29.624)
Treatment*Student			-12.085**			-68.845			-7.218**			-33.885
			(4.611)			(54.808)			(2.774)			(31.508)
Constant	-0.881	-1.505*	-0.945	-4.822	-11.338	-2.758	-0.887	-1.125	-0.963	-5.050	-11.105	-2.775
	(0.794)	(0.891)	(0.930)	(9.038)	(10.940)	(9.823)	(0.549)	(0.688)	(0.687)	(8.052)	(10.498)	(8.554)
Stratification Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Provincial Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	451	451	451	451	451	451	451	451	451	451	451	451
R-squared	0.023	0.023	0.029	0.030	0.034	0.027	0.024	0.024	0.029	0.030	0.033	0.027
RMSE	1.360	1.359	1.355	15.69	15.66	15.71	1.103	1.103	1.100	13.33	13.30	13.34

This table replicates models 3,6, 9, and 12 of Table 6. Instead of internet penetration, however, three other measures of treatment intensity are employed to test for robustness (urban population share, state labor share, and college student share of population). The dependent variables are: 1) Number of questions asked by a delegate; 2) Percentage of queries which were critical of ministers, ministries, or the national government. Panel A uses Difference-in-Difference between Session 6 and Session 5. Panel B analyzes Difference-in-Difference between Average Questions and Session 6. All analyses used OLS on the first differences between sessions. Robust standard errors, clustered at provincial-level are shown in parentheses. All model include controls for stratification variables and provincial co-variates.. *** p<0.01, ** p<0.05, * p<0.1

The Adverse Effects of Sunshine

Evidence from a Field Experiment on Legislative Transparency in an Authoritarian Assembly

Web Appendix

Appendix 1. Sample Website for an Individual Delegate

Appendix 2. Portion Chart Comparing Participations of Delegates

(The full chart showed all 154 treated delegates and was posted in the prime location on the political homepage of the newspaper throughout the two months of the National Assembly's session)

Chart URL: http://daibieuquochoi.vietnamnet.vn/content.aspx?id=888
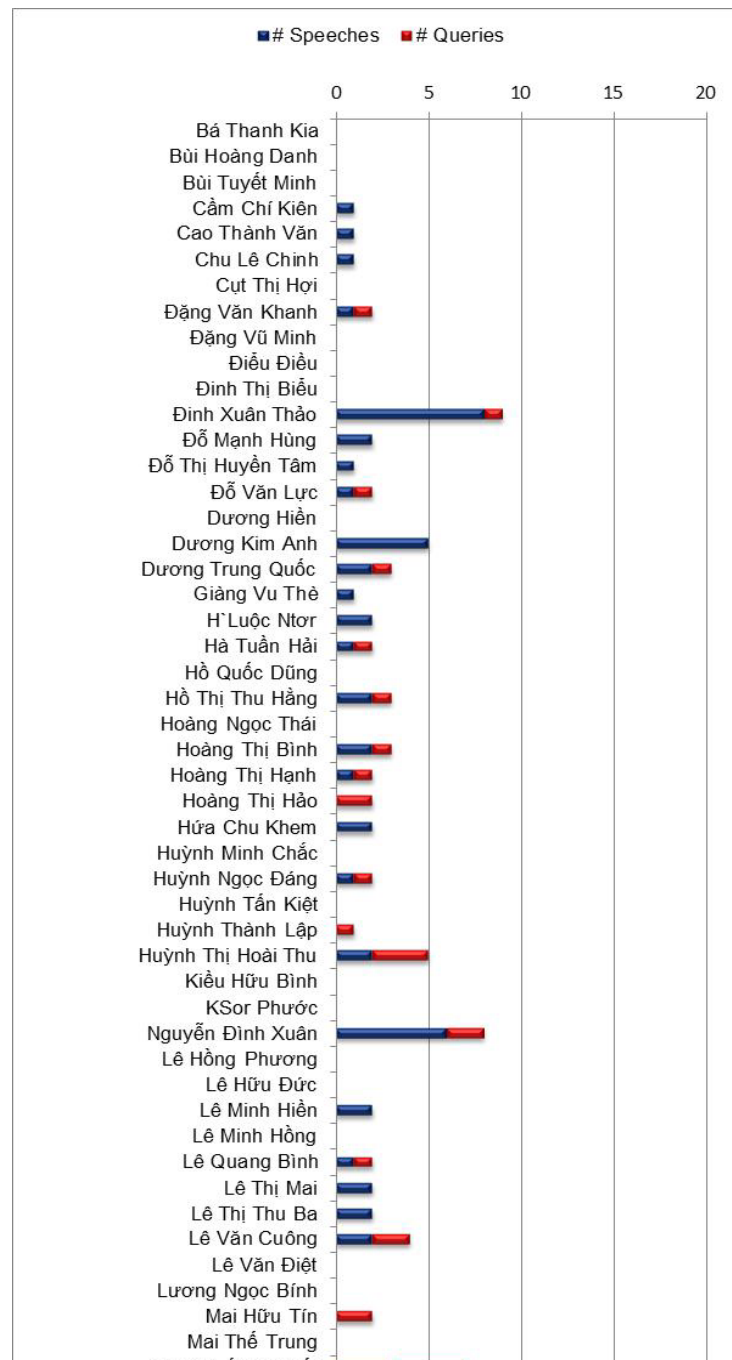
Appendix 3. Sample Score Card of an Individual Delegate
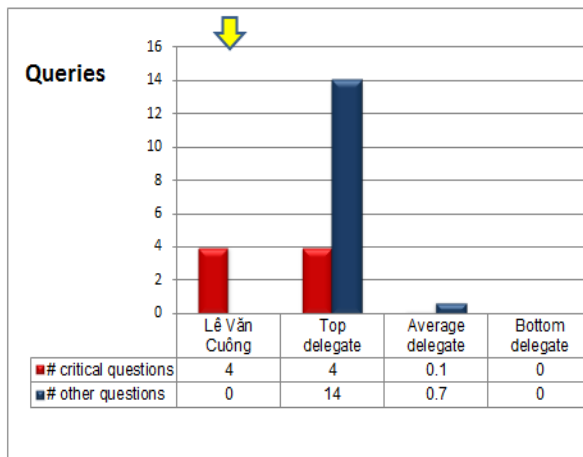
Queries	Lê Văn Cuông	Top delegate	Average delegate	Bottom delegate
# speeches	2	8	0.5	0
# questions	4	16	0.8	0
# critical questions	4	4	0.1	0
# other questions	0	14	0.7	0
# question representing his province	0	2	0.0	0
# questions representing his constituents	1	2	0.0	0
# questions representing his sector	0	2	0.0	0
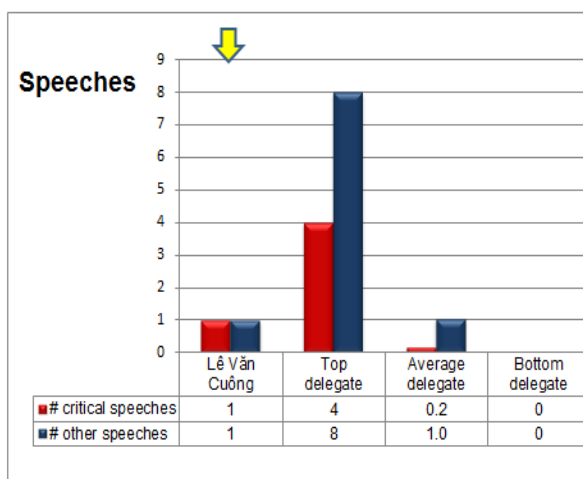
Speeches	Lê Văn Cuông	Top delegate	Average delegate	Bottom delegate
# speeches	2	11	1.2	0
# critical speeches	1	4	0.2	0
# other speeches	1	8	1.0	0
# question representing his province	0	2	0.1	0
# speeches representing his constituents	0	1	0.1	0
# speeches representing his sector	0	3	0.1	0

URL of the actual scorecard of this delegate: http://daibieuquochoi.vietnamnet.vn/content.aspx?id=1017

Appendix 4: Interaction between Transparency Treatment and Stratification Variables
(Difference in Differences between Session 5 (November 2009) and Session 6 (June 2010))

	Question Count (#)				Critical Questions (%)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment	0.053	-0.076	0.027	-0.596	1.905	1.039	0.754	5.784
	(0.143)	(0.164)	(0.153)	(0.951)	(1.964)	(2.425)	(1.814)	(11.662)
Fulltime	0.230	0.184	0.192	0.178	5.531**	4.372*	4.352*	4.406*
	(0.247)	(0.205)	(0.205)	(0.203)	(2.423)	(2.303)	(2.300)	(2.231)
Centrally Nominated	0.051	-0.050	0.051	0.079	-3.477**	-3.268*	-3.539**	-3.216**
	(0.145)	(0.159)	(0.147)	(0.141)	(1.480)	(1.828)	(1.460)	(1.457)
Retirement Age	0.221	0.210	0.302		-0.128	-0.120	-0.457	
	(0.206)	(0.210)	(0.265)		(1.329)	(1.358)	(1.682)	
Vote Share				-0.005				-0.064
				(0.007)				(0.087)
Treat*Fulltime	-0.121				-3.825			
	(0.347)				(3.701)			
Treat*Centrally Nominated		0.351				-0.933		
		(0.285)				(3.152)		
Treat*Retirement			-0.305				1.163	
			(0.303)				(2.288)	
Treat*Vote Share				0.008				-0.072
				(0.012)				(0.145)
Constant	-0.070	-0.026	-0.061	0.301	0.827	1.097	1.194	5.770
	(0.083)	(0.091)	(0.089)	(0.593)	(0.983)	(1.104)	(0.977)	(6.792)
Observations	461	461	461	461	461	461	461	461
R-Squared	0.00615	0.00864	0.00611	0.00628	0.0206	0.0182	0.0181	0.0218
RMSE	1.349	1.347	1.349	1.349	15.76	15.78	15.78	15.75

This table depicts the results of the controlled field-experiment across two different dependent variables. In this analysis, the transparency treatment is interacted with core determinants of query session participation that were used as stratification variables in the randomization process. Vote share is not a stratification variable but proved significant in previous work. The dependent variables are: 1) Number of questions asked by a delegate; 2) Percentage of queries which were critical of ministers, ministries, or the national government. Ordinary Least Squares coefficients displayed. Robust standard errors, clustered at provincial-level are shown in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Appendix 5: Interaction between Transparency Treatment and Stratification Variables
(Difference in Differences between Average Delegate Performance Sessions (1-5) and Session 6 (June 2010))

	Question Count (#)				Critical Questions (%)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment	-0.042	-0.150	-0.033	-0.167	0.381	-0.126	0.589	7.302
	(0.111)	(0.116)	(0.108)	(0.532)	(1.619)	(1.828)	(1.457)	(10.574)
Fulltime	0.086	0.066	0.074	0.079	1.327	1.262	1.290	1.310
	(0.198)	(0.156)	(0.156)	(0.157)	(2.161)	(2.003)	(2.009)	(1.982)
Centrally Nominated	0.118	0.014	0.121	0.152	-2.048	-2.564*	-2.007	-1.951
	(0.144)	(0.140)	(0.145)	(0.147)	(1.279)	(1.500)	(1.306)	(1.251)
Retirement Age	-0.122	-0.133	0.046	-0.140	-1.967	-2.022	0.047	-2.005
	(0.180)	(0.193)	(0.196)	(0.192)	(2.441)	(2.504)	(2.624)	(2.443)
Vote Share				-0.009				0.002
				(0.006)				(0.085)
Treat*Fulltime	-0.035				-0.068			
	(0.336)				(3.238)			
Treat*Centrally Nominated		0.363				1.816		
		(0.279)				(2.338)		
Treat*Retirement			-0.635				-7.596	
			(0.430)				(5.714)	
Treat*Vote Share				0.001				-0.098
				(0.007)				(0.134)
Constant	-0.066	-0.029	-0.069	0.553	-0.214	-0.042	-0.285	-0.363
	(0.062)	(0.064)	(0.063)	(0.486)	(0.821)	(0.970)	(0.856)	(6.748)
Observations	461	461	461	461	461	461	461	461
R-Squared	0.00518	0.00979	0.00723	0.0113	0.00629	0.00704	0.00823	0.00797
RMSE	1.094	1.091	1.092	1.091	13.56	13.55	13.55	13.56

This table depicts the results of the controlled field-experiment across two different dependent variables. In this analysis, the transparency treatment is interacted with core determinants of query session participation that were used as stratification variables in the randomization process. Vote share is not a stratification variable but proved significant in previous work. The dependent variables are: 1) Number of questions asked by a delegate; 2) Percentage of queries which were critical of ministers, ministries, or the national government. Ordinary Least Squares coefficients displayed. Robust standard errors, clustered at provincial-level are shown in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Appendix 6: Interaction between Transparency Treatment and Stratification Variables

(Difference in Differences between similar ministers in previous sessions and Session 6 (June 2010))

	Question Count (#)				Critical Questions (%)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment	-0.047	-0.123**	-0.038	-0.064	-0.014	-0.044***	0.008	0.085
	(0.033)	(0.051)	(0.053)	(0.224)	(0.012)	(0.016)	(0.032)	(0.126)
Fulltime	0.150	0.142	0.148	0.150	0.057	0.071**	0.075**	0.075**
	(0.115)	(0.089)	(0.090)	(0.091)	(0.041)	(0.034)	(0.035)	(0.036)
Centrally Nominated	0.010	-0.069	0.011	0.021	0.015	-0.035	0.016	0.018
	(0.073)	(0.075)	(0.074)	(0.075)	(0.034)	(0.030)	(0.035)	(0.036)
Retirement Age	0.110	0.102	0.199	0.104	0.062	0.057	0.103	0.061
	(0.160)	(0.165)	(0.208)	(0.166)	(0.083)	(0.087)	(0.108)	(0.085)
Vote Share				-0.003				-0.000
				(0.002)				(0.001)
Treat*Fulltime	-0.005				0.058			
	(0.164)				(0.100)			
Treat*Centrally Nominated		0.277*				0.177		
		(0.161)				(0.108)		
Treat*Retirement			-0.335				-0.155	
			(0.235)				(0.120)	
Treat*Vote Share				0.000				-0.001
				(0.003)				(0.002)
Constant	0.013	0.038	0.010	0.208	0.007	0.017*	-0.000	0.024
	(0.025)	(0.030)	(0.032)	(0.149)	(0.009)	(0.010)	(0.015)	(0.077)
Observations	461	461	461	461	461	461	461	461
R-Squared	0.0175	0.0280	0.0198	0.0201	0.0239	0.0414	0.0238	0.0228
RMSE	0.551	0.548	0.551	0.551	0.256	0.254	0.256	0.256

This table depicts the results of the controlled field-experiment across two different dependent variables. In this analysis, the transparency treatment is interacted with core determinants of query session participation that were used as stratification variables in the randomization process. Vote share is not a stratification variable but proved significant in previous work. The dependent variables are: 1) Number of questions asked by a delegate; 2) Percentage of queries which were critical of ministers, ministries, or the national government. Ordinary Least Squares coefficients displayed. Robust standard errors, clustered at provincial-level are shown in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Appendix 7: Intensity of Treatment Effect
(Interaction between Treatment and Internet Penetration)

Specification	Difference in Differences between similar ministers in previous sessions and Session 6 (June 2010)					
Dependent Variable	**Question Count (#)**			**Critical Questions (%)**		
Model	(1)	(2)	(3)	(4)	(5)	(6)
Treated	-0.044	-0.044	-0.046	-0.010	-0.010	-0.012
	(0.063)	(0.066)	(0.068)	(0.037)	(0.038)	(0.039)
Internet Penetration	-0.006	-0.003	-0.009	-0.002	-0.000	0.008
	(0.007)	(0.006)	(0.013)	(0.003)	(0.003)	(0.005)
Treated*Internet	-0.002	-0.002	-0.002	0.010	0.009	0.009
	(0.011)	(0.010)	(0.011)	(0.007)	(0.008)	(0.009)
Centrally Nominated		0.009	0.010		0.016	0.015
		(0.075)	(0.077)		(0.035)	(0.035)
Fulltime		0.148	0.149		0.076**	0.076**
		(0.090)	(0.094)		(0.036)	(0.036)
Retirement		0.113	0.114		0.061	0.062
		(0.160)	(0.163)		(0.085)	(0.086)
National-level city			0.035			-0.037
			(0.069)			(0.025)
GDP per Capita (ln)			0.002			-0.026
			(0.069)			(0.026)
Population (ln)			-0.010			-0.014
			(0.038)			(0.018)
Transfer/Revenue			-0.000			-0.000
			(0.000)			(0.000)
South			-0.014			-0.013
			(0.059)			(0.025)
Governance			0.003			-0.000
			(0.005)			(0.002)
Constant	0.069*	0.018	-0.111	0.032**	0.001	0.092
	(0.037)	(0.038)	(0.222)	(0.013)	(0.017)	(0.139)
Observations	461	461	461	461	461	461
R-squared	0.002	0.018	0.019	0.001	0.023	0.027
RMSE	0.554	0.552	0.555	0.258	0.256	0.257

This table depicts the results of the controlled field-experiment across four different dependent variables. In this analysis, the treatment is interacted with internet penetration to derive an exogenous intensity of treatment effect. The dependent variables are: 1) Number of questions asked by a delegate; 2) Percentage of queries which were critical of ministers, ministries, or the national government. All analyses used OLS on the first differences between sessions.   Robust standard errors, clustered at provincial-level are shown in parentheses. The first model in each set is unadjusted, the second model controls for individual-level stratification variables, and the third model control for provincial co-variates that might be associated with internet penetration. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Appendix 8: Robustness of Intensity of Treatment Effects

Specification	Difference in Differences between similar ministers in previous sessions and Session 6 (June 2010)					
Dependent Variable	**Question Count (#)**			**Critical Questions (%)**		
Model	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	-0.054	-0.203	-0.003	0.017	-0.176	0.004
	(0.110)	(0.226)	(0.065)	(0.060)	(0.163)	(0.046)
Urban (%)	-0.267			-0.171		
	(0.266)			(0.133)		
Treatment*Urban	0.011			-0.056		
	(0.230)			(0.124)		
State Labor Share		0.634			0.414	
		(5.184)			(1.615)	
Treatment*State		5.245			6.096	
		(7.925)			(6.056)	
College Students (%)			1.477			-0.048
			(2.106)			(0.554)
Treatment*Student			-2.212			-0.032
			(1.728)			(1.085)
Constant	-0.127	-0.137	-0.203	0.048	0.045	0.072
	(0.237)	(0.322)	(0.364)	(0.148)	(0.159)	(0.172)
Stratification Controls	Yes	Yes	Yes	Yes	Yes	Yes
Provincial Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	451	451	451	451	451	451
R-squared	0.020	0.019	0.022	0.028	0.032	0.024
RMSE	0.560	0.560	0.560	0.259	0.259	0.260

This table replicates models 3,6, 9, and 12 of Table 6. Instead of internet penetration, however, three other measures of treatment intensity are employed to test for robustness (urban population share, state labor share, and college student share of population). The dependent variables are: 1) Number of questions asked by a delegate; 2) Percentage of queries which were critical of ministers, ministries, or the national government. Panel A uses Difference-in-Difference between Session 6 and Session 5. Panel B analyzes Difference-in-Difference between Average Questions and Session 6. All analyses used OLS on the first differences between sessions. Robust standard errors, clustered at provincial-level are shown in parentheses. All model include controls for stratification variables and provincial co-variates.. *** p<0.01, ** p<0.05, * p<0.1