Participation, Government Legitimacy, and Regulatory Compliance in Emerging Economies: A Firm-Level Field Experiment in Vietnam

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This paper employs a field experiment in single-party–ruled Vietnam to test whether providing a broad-based representative sample of firms the opportunity to comment on draft regulations increases their subsequent compliance. We find three main outcomes of this treatment. First, treated firms exhibited greater improvement in their views of government's regulatory authority. Second, these firms were more likely to allow government-affiliated auditors to examine their factories. Third, treated firms demonstrated greater compliance on the factory floor. Access and compliance were not explained by the receipt of advance information about the regulation's requirements, and none of the three outcomes required that firms offer substantive comments.

"The question should not be why compliance by firms is low. The question we need to be asking is what government can do to increase the degree to which firms believe *the government is a legitimate regulator* and that it is producing laws that *should* be followed."

–Nguyen Dinh Cung, Director, Central Institute of Economic Management (CIEM) during "Regulatory Participation and Compliance" workshop at CIEM in Hanoi, Vietnam on November 1, 2016.

An explosion in a Sinochem subsidiary's warehouse in Tianjin, China, on August 12, 2015, killed 173 people and injured 795 (Merchant 2017). Subsequent investigations revealed that the state-owned firm's storage procedures were illegal. Two nearby Sinochem warehouses were found to be guilty of similar violations, including close proximity to nursery and primary schools (Phillips 2015). The regulatory state failed in even more extreme and deadly fashion with the April 24, 2013, collapse of Rana Plaza in Dhaka, Bangladesh. A day earlier, after meeting with the building's owner about clear and dangerous violations to structural safety standards exposed during onsite inspections, government officials had chosen to allow business as usual (BBC 2013). Tragic industrial accidents such as these, involving self-interested firms and poorly equipped or even unethical regulators, are more likely when state institutions are of low quality (Takala et al. 2014).

Under such conditions of weak states, what can realistically be done to increase incentives for firms to act in the public interest and abide by government regulations. Greater focus on punishment of violations is one answer (Andreoni, Harbaugh, and Vesterlund 2003; Becker and Stigler 1974; Fehr, Fischbacher, and Gächter 2002). However, empirical evidence on the effectiveness of punishment is mixed (Braithwaite and Makkai 1991), and the anecdotes above show how resource constraints and bureaucratic corruption are obstacles to effective enforcement. The same government weakness and malpractice also increase the odds that firms will question government's regulatory legitimacy and defy its laws (Webb et al. 2009) and also hide their transgressions from authorities (Glaeser and Shleifer 2003). Under these conditions, major international organizations, such as the World Bank, have begun to promote political participation as an alternative to punishment to change beliefs and induce voluntary compliance in emerging economies (World Bank 2017b).

This alternative is inspired by extensive theoretical work on the behavior of citizens in political science's deliberative democracy tradition (Fishkin 1991; Fung and Wright 2001) and the behavior of employees and other group and community members in psychology's procedural justice literature (Tyler 2006, 1990). Both streams argue that personal involvement in the rule-making process makes people more likely to view rulemaking bodies, enforcement authorities, and the rules themselves as legitimate. This greater legitimacy should, in turn, lead individuals to be more likely to accept the constraints and costs of the resulting rules. Recent work has extended this logic to show that

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consultative institutions contribute to the stability and longevity of authoritarian regimes (Balla and Liao 2013; He and Warren 2011; Truex 2017). We build on this work by arguing that this "legitimacy mechanism" can be extended from the individual level to that of firms.

This paper describes a test of the legitimacy mechanism within the context of an initiative to collect feedback from a broad and representative set of local firms on a new draft regulation in authoritarian and nominally communist Vietnam. Specifically, we embedded a two-year randomized controlled trial within a pilot consultation program implemented by the reforming, but still government-affiliated, Vietnam Chamber of Commerce and Industry (VCCI).[1] This design helped us to overcome the broad range of empirical challenges that have plagued past efforts to evaluate policies aimed at improving regulatory compliance (Carrigan and Coglianese 2011). The initiative involved two stages. First, VCCI solicited comments from affected firms on a draft labor regulation meant to protect workers dealing with hazardous chemicals in the workplace. Second, it fed this input to the government committee designing the regulation.

We differentiate the legitimacy mechanism from two key alternatives. According to the first, which we label the "information mechanism," participation in the regulatory design process increases the odds of compliance by exposing firms to more information about the regulation. According to the second, which we label the "substantive change mechanism," participation facilitates compliance by altering the actual constraints imposed by the regulation.

It is critically important to distinguish the legitimacy, information, and substantive change mechanisms. Each offers a logical alternative path by which participation can potentially increase the likelihood of a firm's regulatory compliance. Each also has vastly different implications for how we understand firm behavior and for the design of potential solutions to regulatory noncompliance. Although it is possible for all three to operate simultaneously, within our experimental design, we must guard against the possibility that these alternatives are associated with both our participation treatment and the outcomes that we hope to explain, leading to bias in our average treatment effects. Testing the substantive change mechanism, in particular, is also complicated by the fact that firms in the control and treatment groups are likely to have very similar preferences over the regulation. Consequently, if the regulation is changed in a way that engenders compliance, we are likely to only see null effects, as both groups similarly alter their behavior.

To help identify the legitimacy and information mechanisms, our study design involved random assignment of sampled firms to one of three distinct interactions with VCCI representatives in the baseline round. Firms assigned to the main participation treatment group were informed about the operational requirements of a newly drafted labor regulation and asked for feedback on how it could be improved. Firms in a second treatment group received the same information, but were not asked to provide comments. This distinction between treatments allows us to cleanly separate the effects of the legitimacy and information mechanisms. Firms in a placebo treatment were informed about broader VCCI efforts to facilitate communication between government and the business community during the regulatory design process, but not given special notice about the targeted draft labor regulation. All sample firms were visited again, approximately a year later. This time, VCCI sent experienced chemical safety auditors to provide advice on how to most cost effectively adhere to the revised regulation, whereas at the same time judging the degree to which firms were already in compliance.

Although we are unable to directly test the substantive change mechanism within our experiment, we do carefully consider the potential threat of confounding it poses with sensitivity tests of our core findings. In particular, 72% of the sample firms randomly assigned to our participation treatment group did not provide feedback of sufficient clarity and substance to even have the potential to contribute to a change in the regulation (comments that did provide such clarity and substance are henceforth referred to as "substantive comments"). We focus an additional set of tests on this majority subsample of participating firms in the second half of the "Additional Sensitivities Tests" section after description of our main results. These analyses serve to clarify the critical point that our study's theoretical focus is the compliance effects of providing firms with the *opportunity* to participate, not the effects of whether firms take up this opportunity in an active and engaged manner.

Our experiment delivers three key results. First, we find direct evidence that the opportunity to participate in the regulatory design process leads firms to hold more positive views about government's regulatory authority. We asked firms during both rounds whether government regulators had sufficient industry knowledge to competently perform their regulatory duties. The likelihood of positive responses on this measure of process legitimacy increased by 17-percentage points, even among firms outside the participation treatment, potentially reflecting an overall positive influence of their shared interactions with VCCI. Firms in the participation treatment, however, exhibited a much more striking increase of 24-percentage points (representing 40% higher growth than the other two groups).

Second, a firm given the chance to comment was 8–10% more likely than firms in other groups to allow a chemical safety expert sent by VCCI to audit its factory operations and suggest how to most efficiently achieve compliance. This decision to let these expert auditors onto the factory floor was particularly meaningful because of the combination of VCCI's close association with the government and the lack of clarity in the target regulation regarding the delineation of firms' obligations. We interpret this finding as evidence that participation in the regulatory design phase makes a firm more open to cooperating with government regulators to sort through the messy and subjective compliance process that necessarily follows introduction of a flawed new regulation.

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[1] VCCI receives a portion of its annual budget from the central government and its top leadership is comprised of Vietnamese Communist Party (VCP) members.

Our third finding is that the randomly assigned participation opportunity was associated with a higher likelihood of auditors judging firms to be in compliance with the target regulation. This is true as long as firms that refused auditors access to their factory floors were no more than half as likely to be compliant as firms that did allow access. Under the stronger assumption that no auditor access equals noncompliance, firms receiving the participation treatment demonstrated average compliance of 42% on relevant clauses, compared with 36% in the control group. This constitutes a nearly 15% improvement. Importantly, this result is not driven by the 28% of firms in the participation treatment that provided substantive comments. In fact, the average participation treatment effect actually increases when we eliminate commenters from our analyses. As a result, contrary to the substantive change mechanism, it is highly unlikely that the greater compliance witnessed among firms in the participation treatment was motivated by idiosyncratic benefits achieved through their own comments.

THEORIES CONNECTING PARTICIPATION TO COMPLIANCE

It is increasingly well established that there are important and far-reaching societal benefits to more democratic institutions. At the individual level, the deliberative democracy literature has argued that giving citizens greater voice in the shaping of rules that regulate their behavior leads to greater consensus around the decisions of authorities (Dryzek 2000; Elster 1998; Fishkin 1991; Fung and Wright 2001).

The procedural justice stream, which is prominent in both the fields of psychology and organizational behavior, has similarly found evidence that people are more likely to follow laws and employees are more likely to follow organizational rules when they are consulted by leaders (Folger and Konovsky 1989; Thibaut and Walker 1975; Tyler 1990). In the field of economics, there is also recognition of the reciprocal obligations created when design of the rules is opened up to input by those who are to be constrained by them. Of particular relevance is Dal Bó, Foster, and Putterman (2010), who use a laboratory experiment to show that players of a prisoner's dilemma game are more likely to comply with rules incentivizing socially beneficial behavior after learning that these constraints came about through consultation with fellow players. Researchers in management have begun to build on the above individual-level work to theorize similar behavior at the firm level (Bosse and Phillips 2016; Kreps 1996).

Taken together, this previous work generates our first hypothesis:

H1: A firm is more likely to comply with regulatory requirements that introduce new costs and/or constrain its operations if government provides it with the opportunity to comment on *a draft version of the underlying regulation.*

The most well-developed theoretical mechanism linking participation to greater compliance is what we label the legitimacy mechanism. Scholars in the deliberative democracy tradition argue that, after participating, citizens come to see the legislative process and governing institutions as more legitimate (Dryzek 2000; Fishkin 1991; Parkinson 2003; Weatherford 1992). In fact, simply making citizens aware of participatory processes can increase perceptions of regime legitimacy, even in authoritarian contexts such as China (Fishkin et al. 2010; Truex 2017). The procedural justice literature has similarly shown that the primary mechanism underlying participation's effect on individual-level compliance operates through the effect that the opportunity to participate has on people's perceptions of the rulemaking authority's legitimacy (Leventhal 1980; Tyler 1990).

It is important to distinguish the concept of legitimacy from its close cousins: regime support, loyalty, and trust (Gerschewski 2018; Lipset 1960; Weatherford 1992). Legitimacy differs in its specific focus on the acceptance of the ruler's authority to govern and the processes by which that authority is exercised (Dickson, Gordon, and Huber 2015; Gerschewski 2018; Weber [1922] 1978). Furthermore, some scholars argue that citizens believe authority is legitimate when they view the state as competent and fair in the exercise of its authority (Dickson, Gordon, and Huber 2017; Murphy 2005; Tyler 1990). This refinement is sometimes referred to as "process legitimacy" (Meunier 2003; Scharpf 1999).[2] Relatedly, Tyler (2006) defines legitimacy as "a psychological property of an authority… that leads those connected to it to believe that it is appropriate, proper, and just." He further explains that "because of legitimacy, people feel that they ought to defer to decisions and rules, following them voluntarily out of obligation rather than out of fear of punishment or anticipation of reward." With this definition in mind, we hypothesize the legitimacy mechanism as follows:

M1: A firm is likely to hold a higher opinion of government's legitimacy as a regulatory authority if government provides it with the opportunity to comment on *a draft version of a regulation.*

In emerging economies, firms are often completely unaware of the content of new business regulations or their specific obligations until either formal implementation or, worse still, regulatory inspectors arrive at their factory gates. Related to this context, the information mechanism proposes that a positive relationship between participation and regulatory compliance is simply a matter of participation increasing firms' understanding of their regulatory obligations. The reasoning for this mechanism builds on previous work showing that participation in rulemaking processes can have the very straightforward benefit of teaching citizens about the law (Pateman 1970; Sabatier and Jenkins-Smith 1993).

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[2] For shorthand throughout this paper, we use the term "legitimacy mechanism," but our theory and experimental tests are more precisely aimed at the concept of "process legitimacy."

Learned information can be of two main types: (a) the substance of new regulatory requirements or (b) signaling government's commitment to enforce these new requirements. The role of both types of information is particularly relevant in emerging economies where business regulation is commonly disparaged as a confusing, confused, and costly mass of overlapping "red tape" (Djankov et al. 2002). The effect of these poorly designed systems is that business managers, especially those in charge of resource-constrained small- and medium-sized enterprises (SMEs), are less able to stay on top of and to fully understand all the regulations to which they are required to adhere.

Consistent with the information mechanism, Olson (1999) finds that regulatory compliance increases when requirements are clearer and less complex. Awareness of the rules has been shown to play a role in compliance behavior in the procedural justice literature (Winter and May 2001). These more informed stakeholders are less likely to make mistakes, which lead to accidental violations (Fearon 1998; Mackie 2006). As a result, paralleling our hypothesis examining the legitimacy mechanism, we also examine the information mechanism:

M2: A firm is more likely to comply with regulatory requirements that introduce new costs and/or constrain its operations if government provides it with early access to information about a draft version of the underlying regulation.

It is common to conceive of business participation in regulatory design in emerging economies with underdeveloped democratic systems as primarily a process of informal "back room" connections through which large, politically connected firms capture policy making (Hellman and Kaufmann 2001; Hellman and Schankerman 2000). Indeed, this is a fair characterization of the Vietnamese status quo (Pincus 2015; Pincus, Anh, and Le Thuy 2008). By sharp contrast, our focus in this paper is on the introduction of formal and broad-based systems that mobilize ideas from across a representative spectrum of firms and inserts those insights into the government's regulatory design process. Most importantly, this involves expanding policy input beyond only the political connected elite to include previously disenfranchised firms. Although business associations can serve as effective representatives of non-elite firms in developed democracies (Brammer, Jackson, and Matten 2012; Crouch and Streeck 2006; Marques and Utting 2010), they appear to struggle to play this role when democratic institutions are less developed (Doner and Schneider 2000; Moore and Hamalai 1993).

Previous work has shown that the existence of competing interest groups can increase government's power to arbitrate in ways that benefit broader society (Laffont and Tirole 1991; Peltzman 1976). This work has tended to frame business as a homogenous group of elite economic interests competing with other more socially oriented interest groups (e.g., Gilens and Page 2014), but we suggest that there may be benefits to seeing the business community as fragmented and home to significant internal competition of ideas and perspectives. For example,

because of their limited resources, larger numbers in the economy, and presence in more competitive industries, some have argued that SMEs have less structured relationships with government than do large elite firms in more concentrated industries (Baron 2000; Bertrand and Kramarz 2002).

Recognizing the differences between SMEs and large, politically connected firms is critical for proper consideration of the threat of the substantive change mechanism as an alternative to the legitimacy mechanism in our research setting. This alternative holds that participation in the regulatory design process could result in substantive change to the regulation that influences the participating firm's costs of compliance and thereby its incentives to comply. There are two distinct versions of the substantive change mechanism: one positive for the public interest and the other negative, but both potentially increasing compliance.

The "better law" version builds on work indicating that participation alters legislative quality by identifying problems and tailoring policy to citizens' preferences (Coglianese 2006; Horsley 2009; Stern, Powell, and Ardoin 2008). Going back to Stigler (1971) and corroborated by more recently by Yackee and Yackee (2006), scholars have recognized that regulators lack sufficient information on cost, demand, quality, and other dimensions of firm behavior. It follows that officials lack information needed to optimally promote the public interest when regulating firms. Within this context of capacity-constrained government, consultation with business owners and managers can leverage their expertise and experiences to identify problems with the logic and implementation of regulation and thereby better tailor policy to the spectrum of real world, factory floor conditions (Ayres and Braithwaite 1992; Sappington and Stiglitz 1987).

The negative "weaker law" variant of the substantive change mechanism, in contrast, clearly undermines the public interest case for participation programs. In this version, providing profit-maximizing firms access to the rulemaking process improves compliance by weakening the degree to which regulations limit firm operations and their negative externalities. This view relates to the theory of regulatory capture, which characterizes firm influence in the policy process as collusion between the private interests of regulators (Posner 1974; Stigler 1971) and rent-seeking firms (Buchanan and Tullock 1975; Krueger 1974). Recent work has challenged the prevalence of capture, but focuses exclusively on Western democracies (Bardhan and Mookherjee 2000; Carpenter and Moss 2013; Posner 2013). Importantly, work on the "Notice and Comment" form of participation we focus on in this paper has found evidence that comments can lead to self-interest–driven substantive change (Yackee 2005) and that such change is more likely to be driven by input from business interests than other types of commenters (Yackee and Yackee 2006).

Combining the two versions into a general prediction, the substantive change mechanism holds that:

M3: A firm is more likely to comply with regulatory requirements that introduce new costs and/or constrain its operations if the impact of those costs and/or constraints is

lessened by changes based on comments by firms on an earlier draft version of the regulation.

CONTEXT AND EXPERIMENTAL DESIGN

Study Context

Our study covers 11 provinces in the densely populated Red River Delta region, with Vietnam's capital, Hanoi, at their center.[3] From 1990, when market reforms shifting the country away from central planning began in earnest, to 2013, Vietnam was one of the fastest growing economies in the world (World Bank 2017a). Following the 1999 passage of a new company law that led to rapid growth in the number of domestic private firms, through 2013, no region grew faster than the Red River Delta (Vietnamese General Statistics Office Multiple Years). Development of government institutions, however, significantly lagged economic growth. Vietnam's regulatory system remains among the world's most cumbersome, corrupt, and opaque (Transparency International 2017; World Economic Forum 2017). Worsening overall transparency in the drafting of new regulations was even highlighted in VCCI's annual report on ministerial efficiency (VCCI 2014).

Vietnam's domestic private SMEs are spread throughout the country, hard to reach, and have limited technological capacity. This makes them exactly the type of firms for which regulatory compliance is hardest to achieve (MOLISA 2016) and contributes to the prevalence of industrial accidents. They also see the policy environment as unfair: in a 2016 survey of a representative sample of domestic private firms, 61% said the state is biased in favor of large, elite, private firms, particularly in regard to firm entry, land access, and procurement (Malesky 2016). This underlines how the chief beneficiaries of any initiative to expand access to the policy-making process would be politically unconnected SMEs.

Vietnam's Law on the Promulgation of Legal Normative Documents, beginning in 2008, formally mandated that all ministries publicly post all draft regulations for a public comment period of at least 60 days.[4] Even with this requirement placed by the government on itself, however, adherence by ministries has been poor and inconsistent (Online Appendix A shows variation across ministries in formal rules on the posting of draft documents, the frequency of posting, and the length of delays along the way). Consequently, our study essentially involves experimenting with implementation of an insufficiently utilized government policy.

Within this context, the first task in our research design was to identify an appropriate not-yet-completed draft regulation on which to conduct our experiment.[5] We arranged a national workshop to explain our needs to key officials responsible for designing business regulations and learn about ministerial plans for regulations to be drafted in the coming months. We settled on a planned regulation by the Ministry of Labor, Invalids and Social Affairs (MOLISA)'s Worker Safety Department, which aimed to introduce protections for workers dealing with hazardous chemicals.

Experimental Sample and Design

We created an initial sampling frame of 18,701 firms[6] from a national firm list, which VCCI accessed from Vietnam's General Department of Taxation (GTD). However, consultation with a professional survey firm led us to conclude that it was necessary to first screen the list to ensure firms were legitimate, active, and operating in sectors that used dangerous chemicals. In Vietnam's highly dynamic market, firms frequently go out of business, change operations, or simply disappear without notifying the GTD. There are also "ghost firms" with tax codes and contact information, but no actual operations, which may be fronts for illicit activities, such as money laundering. Such problems with sampling frames are, in fact, relatively common in emerging economies. As a result, sending interviewers to firms from the GTD list without screening would have been extremely inefficient, wasting valuable time and financial resources.

Our screening, performed primarily by phone, bore out the above concerns. More than 11,000 firms were eliminated because they were no longer active. A further 3,550 firms had to be dropped because they were incorrectly listed as operating in sectors that used chemicals or refused to answer questions about the sector in which they operated. We were left with 2,635 firms verifiably operating in chemical sectors in our target provinces. Of these, 1,200 agreed to participate (an acceptance rate of 46%).[7] This satisfied our target of 300 firms per treatment group.[8]

For the baseline round, our research teams visited sample firms over a three-month treatment period (October 2014 to January 2015). Visits closely followed our receipt of a draft version of the hazardous chemical regulation from MOLISA, dated September 12, 2014. All visits involved a tablet-based survey with 37 questions about the CEO, firm size and performance, and feelings about government's regulatory legitimacy. Blocking on available data regarding firm size, two-digit industry codes, and the CEO's gender, we assigned the 1,200 firms across our three treatment groups. For all

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[3] The other provinces are Bac Ninh, Hai Duong, Hung Yen, Vinh Phuc, Phu Tho, Thai Nguyen, Ninh Binh, Hai Phong, Nam Dinh, and Ha Nam.
[4] This document can be thought of as the Vietnamese equivalent of the United States' Administrative Procedure Act, passed in 1946.
[5] Our criteria are detailed in Online Appendix H.

[6] These firms were registered with a four-digit industry code that our chemical safety experts linked with the use of hazardous chemicals. Online Appendices B1 and B2 provide further details on the evolution of our sampling frame.
[7] Our original target sample size was 1,800 firms, but we revised this downward because of implementation costs. Appendix B3 illustrates the differences between our 1,200 respondents and the 1,435 that chose not to participate in the experiment. The two groups are very similar but small differences are apparent in firm location (non-response was higher in Hanoi) and capital size.
[8] Based on power calculations that used a MOLISA estimate to put status quo compliance at 8%.

FIGURE 1. Experimental Treatment Conditions
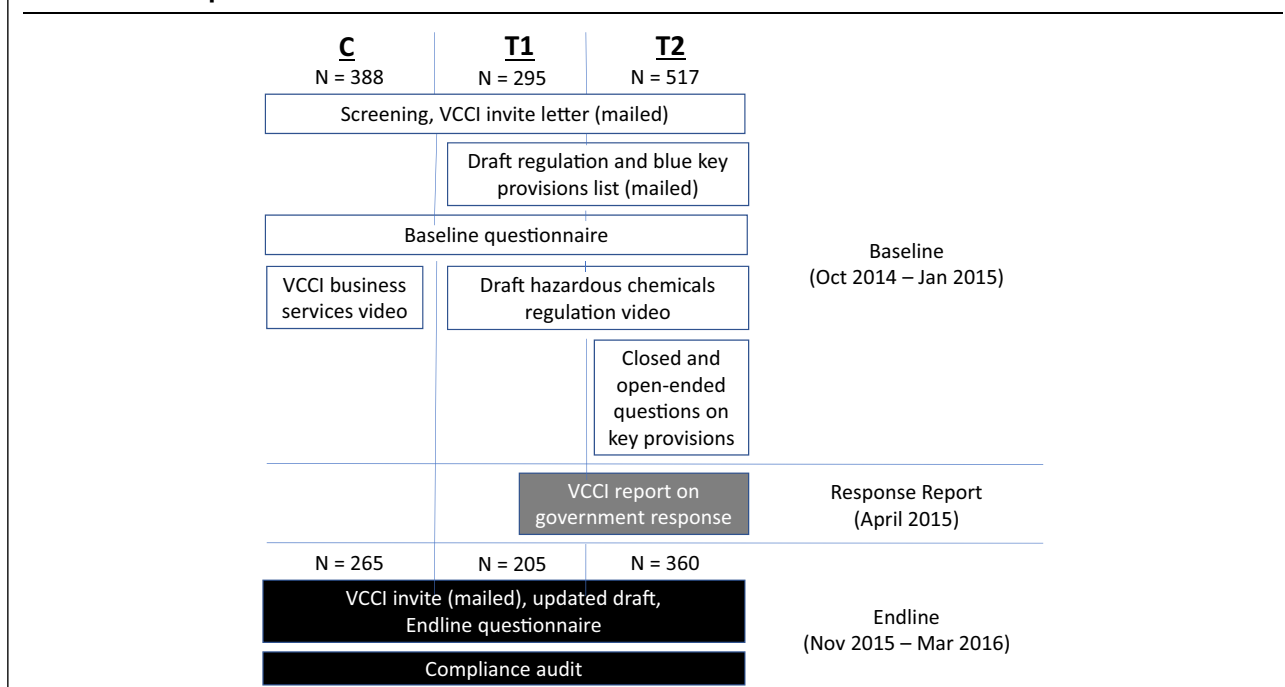
firms, we insisted on meeting the CEO. We were successful 64% of the time. In cases where we could not meet the CEO, which generally occurred because they were located in a different province or country, we met with the highest ranking onsite manager.

We illustrate the key differences across treatment groups in Figure 1. The first group (henceforth, the Control) received our placebo treatment and consisted of 388 firms at baseline.[9] Control firms were shown a placebo video presentation about VCCI efforts to mobilize input from firms on draft regulations. The video was shown on the tablet and lasted six minutes and 48 seconds.

The second group (T1) consisted of 295 firms at baseline and was designed to test the information mechanism (M2 above). To this end, the invitation letter mailed to T1 CEOs ahead of baseline round interviews included a copy of the draft regulation and a distinctively blue-colored form that summarized 11 key clauses identified by chemical safety experts, hired by our project to provide advice and firm auditing, as particularly likely to require firm-level investments of time, effort, and money. Further, after completing the baseline survey, T1 firm representatives were shown a video on the labor protection aims of the target draft

regulation and the operational effects of the 11 clauses. As with the placebo video, this video was tablet-based and lasted about six and a half minutes (6:24).

The third and final group (T2) included 517 firms at baseline and tested the legitimacy mechanism.[10] After receiving the entire T1 treatment experience, these firms were asked to respond to a tablet-based series of open- and closed-ended questions on the costs, quality, and need for improvement for each clause. According to our chemical safety experts, 28% of these firms offered comments that were of sufficient substance to have potential use for altering the regulation. All others only answered the close-ended questions or offered feedback on the regulation that lacked clear enough policy implications for regulators to respond.

All T2 firms subsequently received a report that described results of the participation exercise. This was sent to firms through the mail in late April 2015, more than three months after our final baseline visit.[11] The report included information on all changes made by the government's drafting committee, as of its April 13 revision, and responses to a subset of comments that our chemical safety experts identified as particularly salient. To test whether this additional round of contact with government had an influence on compliance, we randomly assigned 97 T1 firms to also receive the report. We analyze the effect of the response report in "Sensitivity Tests" below.

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[9] The Control is larger than T1 because we originally hoped to include an additional test of what we conceived as "indirect democracy," whereby firms learned about the participation of similar firms in the regulatory design process. We ultimately decided against this treatment because of reduced total sample size and resulting concern of insufficient power.

[10] T2 is the largest treatment group, because we originally hoped to include a treatment of government responsiveness to firm comments. Unfortunately, too few firms provided substantive comments to adequately randomize this treatment, so it had to be dropped.

[11] Firms that made truly substantive comments received reports with tailored responses.

TABLE 1. Clauses in Original Draft and Final Draft of Hazardous Chemical Regulation

Original draft regulation (September 12, 2014)	Revised draft regulation (April 13, 2015)	Original draft regulation (September 12, 2014)	Revised draft regulation (April 13, 2015)
1. Hazardous chemicals must be placed inside a storage place. Hazardous chemicals must be arranged according to their chemical properties. Do not put chemicals that can form chemical reactions or have different extinguishing methods in the same storehouse	Hazardous chemicals must be arranged according to their chemical properties. Do not put chemicals that can form chemical reactions or have different extinguishing methods in the same storehouse	7. Breaker, fuse, socket outlet shall be installed outside areas containing flammable chemicals, explosives. Any branch wires must always have a fuse or equivalent protective equipment	*(No changes)*
2. For **merchandise** that is packaged in woven plastic bags, such items must be placed on brackets, shelves at least 0.5 meter away from the wall; hydrophobic chemicals must be placed in a platform that is at least 0.3 meters high	For **chemicals** that are packaged in woven plastic bag, such items must be placed on brackets, shelves at least 0.5 meter away from the wall; **for** hydrophobic chemicals, they must be placed in a platform that is at least 0.3 meters high (REVISED BUT DROPPED in FINAL REGULATION AUGUST 2015)	8. Lighting system must be the type made for explosion prevention; it is necessary to prevent the penetration of flammable, combustible gas, dust into lighting equipment	*(No changes)*
3. Outside factories, storehouses, there must be sign with "No Fire," "No Smoking" in large and red font and notation of extinguishing equipment. All of the signs must be placed in a clear and easy-to-read area	*(No changes)*	9. The process of mixing solvents into chemicals in an exposed device must be done at least 10 meters away from the areas which produce flames	The process of mixing solvents into chemicals in an exposed device must be **shielded from the areas that produce flames or** done at least 10 meters away from the areas that produce flames
4. Storehouse of hazardous chemicals must be dry, leak-proof, have lightning protection system, and must be inspected according to the existing regulations	Storehouse of hazardous chemicals must be dry, leak-proof, have lightning protection system **or situated inside the area that is lightning-proof**, and must be inspected according to the existing regulations	10. Prohibit welding or other activities that can spark fire within **20 meters** from the storehouse	Prohibit welding or other activities that can spark fire within **15 meters** from the storehouse
5. Cleaning basin must be equipped right outside the areas of hazardous chemicals to make sure that the workers can wash out chemicals that splash in the body or wash their hands and bodies at the end of their shifts	Cleaning basin or **water tap** must be equipped **inside** or right outside the areas of hazardous chemicals to make sure that the workers **can wash out chemicals that splash on the body in a timely manner** or wash their hands and bodies at the end of their shifts	11. The path above devices containing corrosive chemicals must have sturdy barriers and handrails. The equipment and storage tanks must be higher than **the operator position** at least **0.9 meters**. It is prohibited to build the platform in any way or to stack anything that reduces the above height	The path above devices containing corrosive chemicals must have sturdy barriers and handrails. The equipment and storage tanks must be higher than **the position of the platform where the operator stands** at least **1 meter**. It is prohibited to build the platform in any way or to stack anything that reduces the above height

(continued on following page)

536

TABLE 1. Clauses in Original Draft and Final Draft of Hazardous Chemical Regulation (continued)			
Original draft regulation (September 12, 2014)	Revised draft regulation (April 13, 2015)	Original draft regulation (September 12, 2014)	Revised draft regulation (April 13, 2015)
6. Specialized trucks for transporting flammable liquid chemicals **must have grounding wire and the sign "No Fire."** The trucks must be equipped with suitable fire extinguishing equipment	Specialized trucks for transporting flammable liquid chemicals must be equipped with suitable fire extinguishing equipment		
Bolded words depict changed language.			

Table 1 lists the key 11 clauses highlighted in the T1 video and how these clauses had been revised as of the mailing of the report to firms. Overall, few changes could be connected to firm comments.[12]

An area of potential concern relating to our treatments is the extent to which they were absorbed by sample firms. In the case of T1, it is important to determine whether firms were actually better informed about the hazardous chemicals regulation. In the case of T2, absorption implies that firms were convinced that their participation was meaningful.

Figure 2 presents responses to three questions on awareness, understanding, and perceptions of quality in the endline round survey that speak to treatment absorption. Assessments of each were extremely low in the Control (20%, 1.2, and 1.21, respectively). These numbers were all significantly higher in T1 (45%, 1.54, and 1.69, respectively) and higher still in T2 (58%, 1.8, and 2.0, respectively).[13] The high share of firms that did not remember hearing of the regulation may reflect the regulatory environment's lack of transparency and the preponderance and constantly shifting nature of regulatory red tape in Vietnam's transition economy. Furthermore, the absorptive capacity of SMEs may be particularly limited.[14] The large difference in quality assessments points toward our legitimacy mechanism. Participating firms overwhelmingly believe the regulation benefitted from their input. In a separate question in the endline survey, 91% of firms in T2 agreed that "providing comments improved their opinion of the regulation's quality." Notably, T1 firms were also significantly more likely than the placebo to acknowledge the regulation's quality.

Compliance Monitoring

The endline round began in November 2015 and finished in March 2016. This meant an average of roughly 13 months between treatment and the endline. Requests for return visits to perform compliance audits were framed as a free business support service by VCCI, including technical advice on how to most effectively invest into complying with the target regulation.
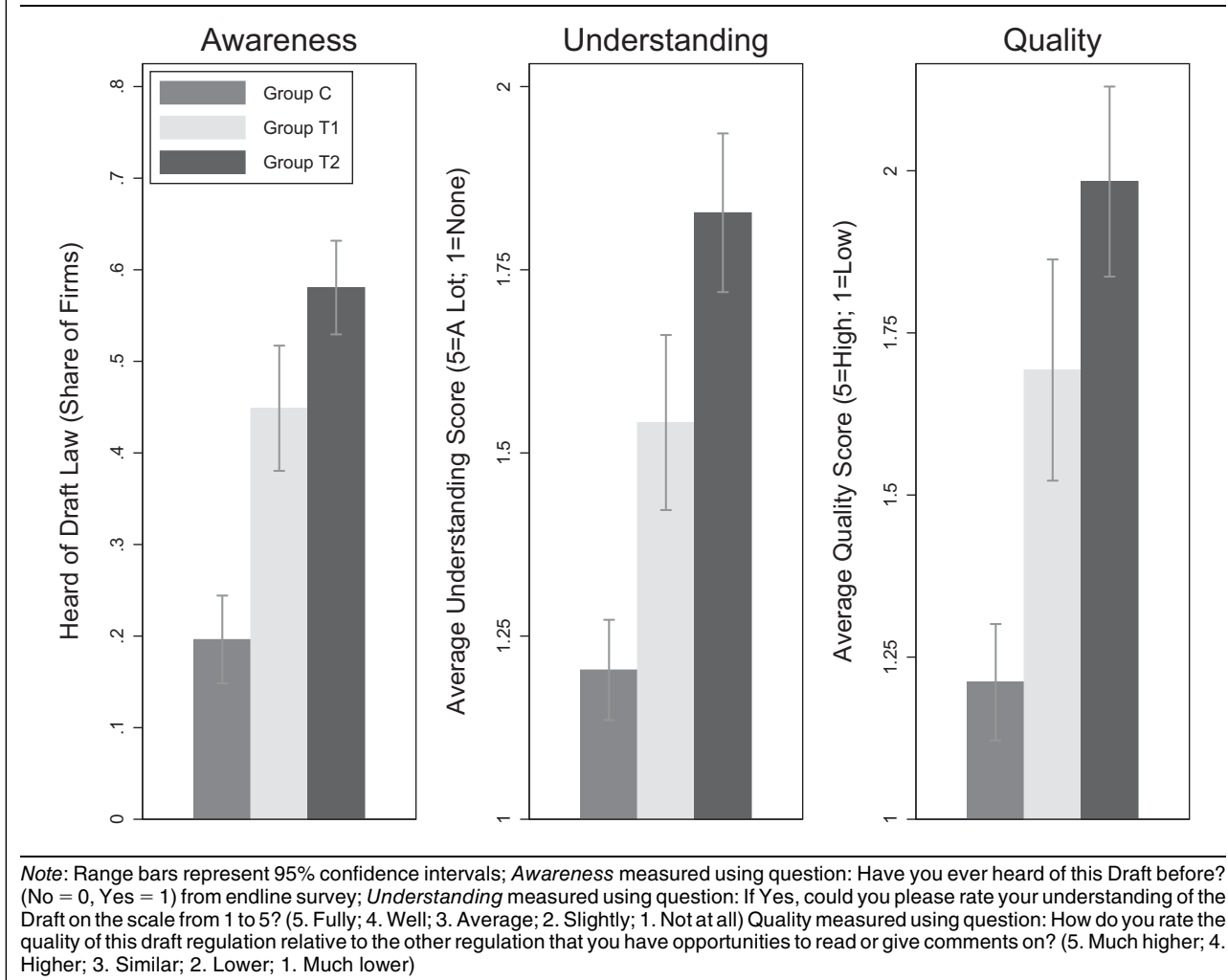
To provide this service, we hired a set of auditors with substantial professional experience judging the chemical safety conditions of factories in Vietnam. This experience equipped them to engage firm managers in serious discussions about what constructive and cost-efficient steps could be taken to maximize the odds of being judged to be in compliance by government regulators. Importantly, auditors were not informed of the study's hypotheses or experimental design. They were simply asked to provide a standard inspection for all firms.

The advanced technical expertise of our auditors was of heightened importance because of the fundamentally low quality of both the original and final draft versions of the target regulation. First, the low quality increased the degree to which well-intentioned firms really did need expert advice in interpreting what the government wanted them to do in regard to chemical safety. Second, it meant that we ultimately had to rely on auditors' subjective judgments of safety conditions across the key provisions.[15]

Figure 1 notes the sample sizes for each treatment group at baseline and endline. Despite our efforts to frame our return visit as a free business service from VCCI, we experienced significant attrition between rounds. For each of the three treatment groups, the decline was about 30%. Some of it was due to normal churn, with firms going out of business (4% of baseline sample), moving to an unknown location (3%), or changing into a business line that no longer related to hazardous chemicals (2%). Another 249 firms (21%) refused to participate despite still operating in the same line of business.

Importantly, this attrition between rounds was not systematically correlated with features of the treatment

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[12] Firm comments contributed directly to change in three cases. See Online Appendix C for details.

[13] In a few cases, the firm's representative was different for the baseline and endline surveys. But, even if we limit the analysis to only firms for which the same manager answered in both rounds, awareness still only increases to 51%.

[14] Across all three treatment groups, larger firms demonstrated higher levels of recall.

[15] Online Appendix C illustrates the evolution of the regulation's key clauses over the study period.

FIGURE 2. Manipulation Checks

Note: Range bars represent 95% confidence intervals; *Awareness* measured using question: Have you ever heard of this Draft before? (No = 0, Yes = 1) from endline survey; *Understanding* measured using question: If Yes, could you please rate your understanding of the Draft on the scale from 1 to 5? (5. Fully; 4. Well; 3. Average; 2. Slightly; 1. Not at all) Quality measured using question: How do you rate the quality of this draft regulation relative to the other regulation that you have opportunities to read or give comments on? (5. Much higher; 4. Higher; 3. Similar; 2. Lower; 1. Much lower)

groups and thereby is not a threat to our random assignment. Online Appendix D shows that refusal rates were identical and a variety of reasonable and observable covariates were balanced across the three groups.[16]

EXPERIMENTAL ANALYSIS RESULTS

Outcome 1: Firm Perceptions of Government Legitimacy

Our first analysis examines the legitimacy mechanism (M1) by studying changes in firm perceptions of

government regulatory competence. We begin here, rather than with our general hypothesis, for both theoretical and empirical reasons. Theoretically, the legitimacy mechanism is a critical pathway between participation and compliance for both the deliberative democracy and procedural justice literatures. Empirically, we apply a rigorous difference-in-difference analysis using a dependent variable based on measures of legitimacy that were collected in both the baseline and endline surveys. Such an evaluation of change was not possible in the tests of compliance that follow, because we were only able to audit the factories at endline.

Asking about legitimacy in Vietnam required additional caution, because the question needed to be worded in a subtle enough way that respondents did not think we were asking them to question the authority of Vietnam's Communist Party rule, which would have generated preference falsification and bias in favor of high legitimacy responses (Kuran 1997). For this reason, we chose to restrict our measure of competence and fairness to the specific application of regulations, by

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[16] Two exceptions of post-treatment imbalance are observable. First, firms in the Control reported significantly worse post-treatment business performance than firms in T1 and T2 (see row 13). Second, our auditors were more likely to receive permission to view the factory floor in T2 than T1 (see row 2). As we noted in the introduction, we perceive this as a meaningful experimental outcome and focus on it in detail as "Outcome 2" in Section III.

TABLE 2. Difference-in-Difference Analysis of Legitimacy Growth Between Rounds

Dependent variable: "Government officials have sufficient understanding of business like this one to effectively carry out their regulatory duties." (1 Strongly Disagree to 4 Strongly Agree)	All firms from round 1 and round 3					
	No controls (1)	Blocking variables (2)	Sector Fixed Effects (FE) (3)	Audited firms (4)	Treatment 1 (5)	Audited firms (6)
Endline = 1	0.413***	0.448***	0.445***	0.438***	0.443***	0.447***
	(0.081)	(0.079)	(0.076)	(0.102)	(0.086)	(0.107)
Participation treatment = 1	−0.069	−0.070	−0.066	−0.029	−0.145	−0.082
	(0.062)	(0.062)	(0.064)	(0.077)	(0.095)	(0.115)
Endline × participation	0.190**	0.191**	0.192**	0.199**	0.189**	0.212**
	(0.092)	(0.093)	(0.091)	(0.088)	(0.096)	(0.107)
Female CEO = 1		−0.027	−0.039	0.199**	−0.044	0.196**
		(0.095)	(0.086)	(0.081)	(0.087)	(0.083)
Hanoi = 1		−0.233***	−0.222***	−0.308***	−0.220***	−0.306***
		(0.054)	(0.039)	(0.057)	(0.040)	(0.055)
Information treatment = 1					0.138	0.090
					(0.092)	(0.111)
Endline × information					0.005	−0.022
					(0.076)	(0.110)
Cut point 1	−1.688***	−1.777***	−1.765***	−1.795***	−1.694***	−1.753***
	(0.045)	(0.058)	(0.078)	(0.148)	(0.078)	(0.138)
Cut point 2	−0.073	−0.154***	−0.136*	−0.184	−0.063	−0.141
	(0.047)	(0.039)	(0.075)	(0.151)	(0.086)	(0.146)
Cut point 3	1.825***	1.757***	1.781***	1.805***	1.856***	1.849***
	(0.063)	(0.074)	(0.100)	(0.197)	(0.096)	(0.187)
Size FE	No	Yes	Yes	Yes	Yes	Yes
Sector FE	No	No	Yes	Yes	Yes	Yes
Observations	1,888	1,888	1,888	950	1,888	950
Clusters	53	53	53	44	53	44
Pseudo R-squared	0.0240	0.0291	0.0320	0.0467	0.0331	0.0471
Log likelihood	−1,802	−1,792	−1,787	−877.6	−1,785	−877.3

Ordered probit with standard errors, clustered by province-sector, in parentheses (*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$). Equation (1) is unadjusted, equation (2) controls only for blocking variables, equation (3) introduces ISIC two-digit sector fixed effects, and equation (4) removes all firms that did not grant access to factory floor. Equations (5) and (6) control for firms receiving Treatment 1. Models 4 and 6 have smaller sample sizes, because we are restricting analyses to firms that permitted endline auditing of compliance.

asking firms for their level of agreement with the following statement that adheres to Tyler's (2006, 357) characterization of process legitimacy as, "*appropriate, proper, and just.*" Specifically, we asked whether firms agreed with the statement: "Government officials have sufficient understanding of business like this one to effectively carry out their regulatory duties."[17] At baseline, only 48% of firms in the Control agreed. By the endline survey, views of government had improved significantly, with 64% of Control firms agreeing with the statement.

Table 2 shows the results of a difference-in-difference analysis testing the effect of our randomized participation

intervention on firm perceptions about government across the two rounds. Using an ordered probit specification,[18] we regress *Legitimacy* on our *Participation* treatment variable. *Participation* is coded as 1 if the firm had an opportunity to provide comments on the draft regulation (see formulas (1) and (2) below). This applies only to firms in T2.

$$\text{Participation Treatment} = 1 \text{ if } T2 = 1$$
$$\text{Information Treatment} = 1 \text{ if } T1 = 1 \text{ or } T2 = 1$$
$$\text{Reference Category} = \text{Control Group} = 1$$
$$\text{if } T1 = 0 \text{ \& } T2 = 0, \quad \textbf{(1)}$$

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[17] Original response values ranged along a scale of 1 ("Strongly Agree") to 4 ("Strongly Disagree"). When we refer to agreement, we mean a response of either 1 or 2. We then reversed the scale by subtracting the original values from 5, such that an increase means greater legitimacy.

[18] Online Appendix I1 replicates the analysis using Ordinary Least Squares (OLS) regression with similar results.

TABLE 3. Predicted Probabilities From Legitimacy Analysis

			Probability of agree (3)				
Round	Treatment 2	Prob.	SE	z	P > |z|	95% CI	
Baseline	No	49.1%	1.2%	40.16	0	46.7%	51.5%
Baseline	Yes	47.0%	2.7%	17.33	0	41.7%	52.3%
Endline	No	61.1%	2.7%	22.35	0	55.7%	66.4%
Endline	Yes	63.3%	2.2%	28.49	0	58.9%	67.6%
			Probability of strongly agree (4)				
Round	Treatment 2	Prob.	SE	z	P > |z|	95% CI	
Baseline	No	3.3%	0.6%	5.88	0	2.2%	4.4%
Baseline	Yes	2.9%	0.3%	8.34	0	2.2%	3.5%
Endline	No	8.1%	0.7%	12.21	0	6.8%	9.4%
Endline	Yes	10.2%	1.2%	8.46	0	7.8%	12.5%

Results calculated from Column 3 of Table 2 using Stata's margins command.

$$Legitimacy_{it} = \beta_0 + \beta_1 Endline_t + \beta_2 Participation_i$$
$$+ \beta_3 Endline_t \times Participation_i + \beta_5 Hanoi + \beta_6 Female_i$$
$$+ \lambda + \alpha + u_{it}.$$

$$(2)$$

As formula (2) shows, *Participation* was interacted with a dummy variable, *Endline*. *Endline* was coded 0 for *Legitimacy* scores recorded in the baseline survey and 1 for those that came from the endline survey. The interaction is displayed in Column 1 of Table 2. Following standard experimental methodology, we included fixed effects for blocking variables used in the randomization process in Columns 2 and 3.[19] Column 4 restricts analysis to firms that allowed auditors to assess compliance (See the discussion of Outcome 2 below).

Although our theory views information as a separate mechanism and does not offer a prediction between information and increased legitimacy, our experimental design allowed for the possibility that regulatory information may influence post-treatment survey responses. We therefore add this treatment and its interaction as controls in Columns 5 and 6. As noted in formula (1), *Information* is coded as 1 if the firm received the presentation on the forthcoming hazardous chemical law and 0 otherwise. As shown in Figure 1, this applies to firms in both the T1 and T2 groups.[20]

Results are robust across specifications. Importantly, the component term for *Participation* (β_2) is not statistically significant throughout Table 2, indicating that the treatment groups were statistically balanced in their views of government legitimacy at baseline.[21] The coefficient β_3, the change in *Legitimacy* over time within the *Participation* group, is statistically significant at the 0.05 level and robust across specifications. Ordered probit coefficients can be difficult to interpret, so we calculate the predicted probabilities from Table 2's fully specified Column 3 in Table 3 below.

In sum, Tables 2 and 3 provide strong support for the legitimacy mechanism. The predicted probability of agreement with the *Legitimacy* statement among firms assigned the opportunity to comment was 49.8%, growing to 73.4% in the endline survey, an increase of 23.6 percentage points. For nonparticipants (*Control* and *T1* groups), *Legitimacy* was 52.4% (49.1% Agree and 3.3% Strongly Agree) at baseline and 69.2% (61.1% + 8.1%) at *Endline*. Thus, growth in *Legitimacy* within the *Participation* group was about 6.8 percentage points greater than nonparticipants (a 40.5% change).

Note that the information mechanism does not entail any change to firms' views of government legitimacy. As a result, it is not surprising that neither of the coefficients on *Information* or its interaction is significant in Column 5 or 6. Much more interesting, however, is that the predicted probability on *Endline* (β_1) in the fully specified Column 3 indicates that *Legitimacy* in the Control significantly increased between rounds by 16.8 percentage points. This notable increase in *Legitimacy* likely results from that fact that all sample firms were exposed to an unusually positive picture of the government's regulatory design process. For firms in the Control, this included viewing a video on VCCI's efforts

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[19] These included dummy variables for whether the firm was located in Hanoi (=1) and whether the CEO was female (=1), plus fixed effects for our four-point employment size measure (λ) and the two-digit sector in which the firm operated. Sector fixed effects primarily discern between firms that produced chemicals, transported chemicals, or used chemicals in their production (α).

[20] An alternative specification is to use dummies for the original treatment conditions (T1 and T2) and not recode to isolate the effect of participation. We also run these specifications as robustness tests in Online Appendix I2. We find substantively similar results for the effect of participation.

[21] The coefficient on *Information* in Models 5 and 6 is also insignificant, further attesting to experimental balance.

FIGURE 3.   Justification of Assumption that Access Proxies Compliance
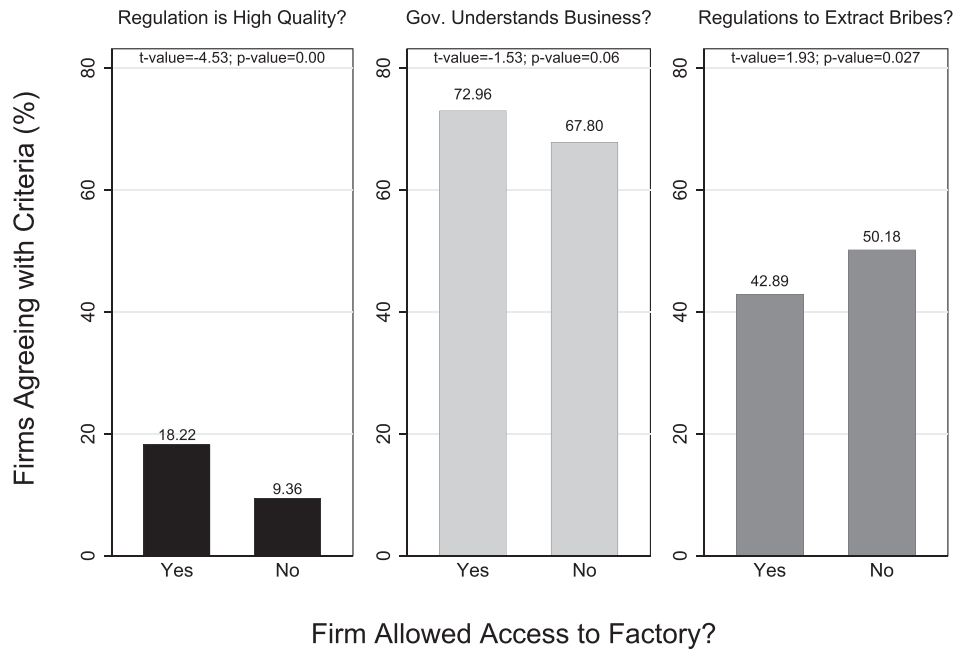
Note: "Regulation is High Quality" measured using question: How do you rate the quality of this draft regulation relative to the other regulations that you have opportunities to read or give comments on? (5. Much higher; 4. Higher; 3. Similar; 2. Lower; 1. Much lower) We recoded, so that Agree = Much higher and Higher and Disagree = Similar, Lower, or Much lower. Regulators Understand Business measured using "Government officials have sufficient understanding of business like this one to effectively carry out their regulatory duties." We recoded, so that Agree = Strongly Agree and Agree and Disagree = Strongly Disagree and Disagree. Regulators to Extract Bribes measured using "The government officials may take advantage of the regulation to extract bribes" We recoded, so that Agree = Strongly Agree and Agree and Disagree = Strongly Disagree and Disagree.

to help government better understand firms' perspectives on draft regulations, receiving mailed copies of the final regulation, and being offered a voluntary audit and advice on compliance from VCCI.

We caution that our Outcome 1 results are based on a single, self-reported measurement that is subject to perception bias and alternative interpretation. Although we did replicate our tests with an alternative measure of legitimacy, the assessment of regulatory quality in Figure 2 above (See Online Appendix I3), this measure does not have the same benefit of over-time measurement. As a result, the next analyses highlight changes in behavioral outcomes that we deem more reliable.

Outcome 2: Access to the Factory Floor

Having introduced supporting evidence for the legitimacy mechanism, we now move to a series of tests of H1, the general relationship between participation and compliance. For our first direct test of H1, we treat the ability of auditors to enter the factory as a measure of compliance. Because this measure represents a direct behavioral measure that is not subject to post-treatment selection bias or social desirability bias, we consider this analysis to offer the most reliable test of our general theory.

Of the 830 firms that participated in the endline round, 38% did not allow access to their warehouses or

factories. Importantly, in each case, representatives of sample firms first met face-to-face with the auditor and answered the endline survey. Only after clearly understanding the auditor's technical expertise, and ability to recognize regulatory noncompliance, did they then choose to refuse access.[22] This apparent fear about giving access to a true expert appears to indicate that many firms were concerned about VCCI's ties to the state and skeptical about the claim that no information from the factory floor visit would be disclosed to government regulators. Although providing access to the factory is certainly costly and time consuming, use of random assignment should mean that these costs did not vary across our treatment groups, which we are able to show do, on average, constitute firms of similar sizes, sectors, and business performance.

Thus, there are two interrelated interpretations of the outcome that firms given an opportunity to participate in government's regulatory design process were more likely to grant government-affiliated auditors access to their factory floors. First, one can view provision of factory access as an indicator of a firm's opinion of government legitimacy, capturing a firm's general interest in better understanding the regulation and what tangibly

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[22] We did not include access to the factory floor as an outcome variable for regulatory compliance in our pre-registered analysis plan.

comprises regulatory compliance. When doors are kept shut, the firm chooses not to engage in this constructive back and forth. Second, access to the factory floor can be viewed as a direct measure of compliance with the regulation. A firm that blocks access for a business-friendly audit is more likely to have something to hide and less likely to be compliant with the underlying regulation than a firm that does provide access.

Although we cannot observe compliance for firms that did not provide access, Figure 3 provides some justification for these interpretations. Although firms were randomly assigned to the different treatment groups and therefore are similar on average in terms of both observable and unobservable characteristics, those who allowed access answered survey questions very differently than those who did not. Firms that allowed access were more likely to say the regulation was of higher quality (18.2% vs. 9.36%), more likely to agree with our *Legitimacy* question in Table 2 (73.0% to 68.0%), and less likely to believe that officials use regulations to extract bribes (42.9% vs. 50.2%). These results indicate that firms providing access were more accepting of regulation than their peers.

Table 4 presents the results of tests of the relationship between access and our experimental treatments. We use a probit specification with standard errors clustered at the firm-industry level in every specification to address the fact that clusters of industries in provinces may share certain features that affect the ability to treat them as independent draws.[23] We regress *Access*, defined dichotomously (Access = 1, No Access = 0), on our two treatment variables, *Information* and *Participation*, which are coded the same as in the previous legitimacy analysis. As before, we begin with an unadjusted model in Column 1, then add design-based controls for blocking variables (λ) in Column 2 and sector (α) fixed effects in Column 3. Column 4 further adds dummies for individual auditors in the endline round, to account for variation in levels of experience and personalities that may have affected their ability to convince firms to permit inspection and their subjective evaluations of compliance.

$$\Pr(Access_i = 1) = \beta_0 + \beta_1 Information_i$$
$$+ \beta_2 Participation_i + \beta_3 Hanoi + \beta_4 Female_i$$
$$+ \lambda + \alpha + u_i. \quad (3)$$

The results of the fully specified estimating equation, displayed in Column 3, are striking. First, despite the earlier finding that *Information* was associated with firms being over twice as likely to be aware of the regulation, increased knowledge did not make firms more likely to allow access to their factories. In fact, firms receiving the information treatment actually provided marginally lower access than firms from the Control (although the coefficient is insignificant). By

contrast, auditors visiting firms that received the participation treatment were 9.3% more likely to gain factory access than when visiting Control firms, and nearly 11% more likely than with firms receiving the information treatment. These results are robust to specification, including linear probability models, and strongly significant ($p < 0.01$).[24]

Refusal to allow access to the factory is clearly different than normal attrition. Columns 5 through 7 replace factory access with agreement to participate in the endline round at all and show no differences across treatment groups in whether or not firms agreed to the return visit.

We see these results as strong evidence for our theory. Participating groups were far more likely to allow factory audits, which we view as a critical first step toward acceptance of the government's regulatory regime and compliance with the outcomes that regime produces.

Outcome 3: Factory Compliance with the Hazardous Chemical Regulation

Our second analysis of H1 relates to auditor judgments of actual compliance with worker protections on the factory floor, another measure of actual firm behavior. Auditors who gained access to sample firms' factory floors created scores of overall compliance for each of the core 10 clauses.[25] Based on these subjective scores, we created dichotomous measures for each clause, scoring a firm as compliant if it received a score of three or above (Compliance = 0 if Assessment <3; Compliance = 1 if Assessment >3).[26] Importantly, when we deemed a particular clause to *not* be relevant for a firm, based on its industry classification, the measure for that clause received a null score. If, for example, a firm operated in fabricated metal manufacturing (ISIC C25), the clause relating to welding equipment was clearly relevant. By contrast, this clause was clearly not relevant for firms operating in food processing (ISIC C10); therefore, these firms did not receive a score for this clause.[27]

We used these firm- and clause-specific measures to construct our primary dependent variable for factory compliance by calculating the share of relevant clauses with which each firm was judged to be compliant.

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[23] Online Appendix J1 replicates our analysis with a linear probability model. Online Appendix J2 controls for baseline legitimacy. Online Appendix J3 estimates using the original treatment conditions. In all cases, we recover extremely similar average treatment effects.

[24] The size of the coefficient on the Hanoi dummy variable drops sharply when auditor fixed effects are added. This strange behavior results from the fact that two auditors operating only in Hanoi had particular difficulty accessing factories relative to their peers operating across provinces. But Online Appendix L shows that dropping these problematic auditors has little influence on the results.

[25] "Rate the level of compliance with this clause," (1) Very Low, (2) Low, (3) Compliant, (4) High, (5) Very High.

[26] This is in line with the primary outcome described in our pre-analysis plan and also reduces interview treatment effects caused by inconsistent application of the Likert scale.

[27] We determined eligibility by studying the firms that allowed auditors to access the factory floor. From this group, we calculated the share of firms for which each clause was deemed non-applicable (NA) by the professional auditor. If more than 80% of firms in a particular industrial sector received an NA mark from the auditor, we coded the clauses as not applying to that industry.

TABLE 4. Effects of Experiment on Access of Auditors to Factory Floor

Dependent variable (DV)	DV: Allowed audit of factory = 1				DV: Agreed to interview = 1		
	No controls (1)	Blocking variables (2)	Sector FE (3)	Auditor FE (4)	No controls (5)	Blocking variables (6)	Sector FE (7)
Information treatment = 1	−0.019 (0.035)	−0.022 (0.039)	−0.017 (0.037)	−0.023 (0.034)	0.012 (0.030)	0.034 (0.058)	0.032 (0.054)
Participation treatment = 1	0.080*** (0.030)	0.086*** (0.031)	0.093*** (0.031)	0.100*** (0.035)	0.001 (0.030)	0.006 (0.043)	0.015 (0.043)
Hanoi = 1		−0.255*** (0.038)	−0.218*** (0.041)	−0.050* (0.027)			
Female CEO = 1		−0.148*** (0.056)	−0.155*** (0.055)	−0.151** (0.065)		−0.041 (0.048)	−0.027 (0.052)
Baseline probability	0.622	0.622	0.622	0.736	0.692	0.548	0.548
Size FE	No	Yes	Yes	Yes	No	Yes	Yes
Sector FE	No	No	Yes	Yes	No	No	Yes
Auditor FE	No	No	No	Yes	No	No	No
Observations	830	830	830	700	1,200	819	819
Clusters	48	48	48	48	53	52	52
Pseudo R-squared	0.00400	0.0684	0.0944	0.0970	0.000137	0.00455	0.0516
Log likelihood	−548.3	−512.8	−498.5	−365.0	−741.2	−561.3	−534.8

Probit model with standard errors, clustered by province-sector, in parentheses (*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$). Marginal probabilities instead of coefficients presented. Panel 1 studies whether auditors were able to visit the factory after conducting the endline interview. Panel 2 studies normal attrition in the panel. Equations (1) and (5) are unadjusted, equations (2) and (6) control only for blocking variables, equations (3) and (7) introduce ISIC two-digit sector fixed effects, and equation (4) introduces auditor fixed effects. Adding auditor fixed effects leads to a reduction in sample size to 700, because of difficulties two auditors had in accessing firms in Hanoi. In panel 2, adding blocking variables reduces sample size because some information is missing from firms that declined interviews.

Formula (4) shows the simple index of average compliance across the audited clauses we created to explore these patterns more systematically. For each firm (i), each relevant clause (k) is coded as 1 if the firm was compliant and 0 if it was not. We sum up the number of instances of compliance and divide by the number of relevant clauses (t) for each firm.[28]

$$Compliance_i = \frac{\sum_1^t Clause_{k,i}}{t_i}. \quad (4)$$

We regress *Compliance* on our treatment variables following the same specification as the *Access* regressions above, controlling for blocking variables, and clustering standard errors at the province-sector level.

$$Compliance_i = \beta_0 + \beta_1 Information_i + \beta_2 Participation_i$$
$$+ \beta_3 Hanoi + \beta_4 Female_i + \lambda + \alpha + u_i. \quad (5)$$

A tricky feature of this analysis is how to address the selection bias problems posed by the refusal of some firms to allow access to their factories. Our auditors were, of course, unable to construct measures of

compliance with the target regulation for these firms. As a result of these missing data, there is reason to believe that any compliance variables based solely on factory floor compliance audits suffer from a selection bias that makes a positive relationship between participation and compliance more difficult to identify. We base this assertion on the combination of the evidence of a positive relationship between participation and access presented in the previous section and our exploration of the assumption that a firm that grants factory access is also more likely to be in compliance in Figure 3. In other words, firms that did not allow access probably had something to hide.[29] Based on this reasoning, we simply coded nonaccess as full noncompliance (0%) in our main models (Columns 1 through 4 in Table 5).

Some will understandably view equating nonaccess with full noncompliance as too strong an assumption. We take three approaches to this address challenge. First, we perform a bounds analysis, following the approach described in Angrist, Bettinger, and Kremer (2006). Second, we limit our analysis to administrative districts where auditors received nearly perfect access by firms (see Columns 5 and 6). This limits our statistical power by

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[28] Clause 2, which related to aquaphobic chemicals, was completely dropped from the final version of the draft regulation because of the complexity of monitoring. As a result, 10 clauses were present in both the baseline and endline rounds.

[29] This is similar to the selection problem encountered by Angrist, Bettinger, and Kremer (2006), when they found that lottery winners receiving educational vouchers were more likely to take college admissions tests needed to measure their ultimate outcome variable of test performance.

TABLE 5. Effects of Experiment on Aggregate Score of Regulatory Compliance Judgments by Auditors

Dependent variable: Clauses with which firm is judged to be in compliance (% of total)	All firms				High access districts		Coarsened exact matching	
	No controls	Blocking variables	Sector FE	Auditor FE	Sector FE	Auditor FE	Sector FE	Auditor FE
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Information treatment = 1	−0.017 (0.024)	−0.016 (0.024)	−0.017 (0.022)	−0.020 (0.024)	−0.013 (0.055)	−0.021 (0.057)	0.027 (0.075)	0.003 (0.073)
Participation treatment = 1	0.052** (0.023)	0.049** (0.022)	0.055** (0.022)	0.047* (0.025)	0.064 (0.045)	0.057 (0.054)	0.046 (0.043)	0.053 (0.048)
Hanoi = 1		−0.137*** (0.024)	−0.113*** (0.031)	−0.025 (0.023)	0.086 (0.060)	0.100 (0.086)	0.014 (0.039)	0.159** (0.060)
Female CEO = 1		−0.085* (0.044)	−0.084* (0.044)	−0.059 (0.046)	−0.123** (0.051)	−0.105* (0.054)	0.107* (0.063)	0.199** (0.078)
Constant	0.364*** (0.035)	0.398*** (0.050)	0.354*** (0.046)	0.554*** (0.044)	0.701*** (0.118)	0.714*** (0.141)	0.718*** (0.100)	0.585*** (0.125)
Size FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sector FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Auditor FE	No	Yes	No	Yes	No	Yes	No	Yes
Observations	813	813	813	813	207	207	162	162
Clusters	48	48	48	48	38	38	28	28
Mean in control group	0.36	0.36	0.36	0.36	0.526	0.526	0.56	0.56
R-squared	0.003	0.062	0.086	0.255	0.134	0.198	0.112	0.203
Root Mean Squared Error (RMSE)	0.376	0.367	0.363	0.330	0.321	0.316	0.297	0.290

Ordinary Least Squares (OLS) models with standard errors, clustered by province-sector, in parentheses (*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$). The first panel analyzes all firms where auditors were given access. The second panel drops firms in the participation treatment that provided comments. Equation (1) is unadjusted, equation (2) controls only for blocking variables, equation (3) introduces ISIC two-digit sector fixed effects, and equation (4) introduces auditor fixed effects. Estimating equations (5) and (6) restrict the analysis to districts where auditors were able to access more than 80% of factories in the jurisdiction, leading to a smaller sample size of 207. Because these models use selection strategy at the district level, standard errors are now clustered at district level. Equations (7) and (8) restrict the analysis to a matched sample of firms that allowed access across treatment groups. The matches are created using coarsened exact matching (CEM) based on observable characteristics of the firms. Again, this procedure is restrictive and limits the sample size to 162 firms.

cutting our sample size by 75%, but it also ensures that access to the factory floor is not associated with our experimental treatments. Third, we use coarsened exact matching (CEM) to identify audited firms from the *Participation* group that were similar to audited firms from the *Information* and *Control* groups based on observable characteristics (Iacus, King, and Porro 2012).[30] We then drop all unaudited firms and perform the analysis on the matched set of 162 operations (Columns 7 and 8).

A first review of our factory floor dependent variable *Compliance* reveals an average of just 36% across all clauses and groups.[31] We do, however, also find initial evidence that this variable was influenced by the chance to participate. Firms in T2 had average overall compliance scores of 40%, compared with 35% in T1 and 36% in the Control. Individual clauses, such as "washing facilities"

and "lighting systems," also exhibited meaningful differences across groups.[32]

Moving to our regression analyses, Table 5 follows the same progression as Table 4. The fully specified Column 3 shows that participation treatment firms demonstrated 5.5 percentage points greater compliance than the 35.4% compliance rate recorded in the Control—a 15.1% improvement. The results are robust across specifications, including the addition of auditor fixed effects, and statistically significant at the ($p < 0.05$) level. Again, firms in the information treatment demonstrated marginally worse compliance, although the effects are not statistically significant.[33]

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[30] Implemented using Stata's CEM command based on firm age, whether respondent was CEO, capital size, labor size, gender of CEO, location, sector, and interviewer.

[31] The average is 61% among firms that permitted auditing.

[32] Online Appendix E displays the average compliance on each clause by our three treatment groups, after coding nonaccess as noncompliant.

[33] The alternative specification using dummies for the original treatment conditions (T1 and T2) is presented in Online Appendix K1. In addition, we provide robustness tests controlling for baseline legitimacy (Online Appendix K2). Results for participation's effect are substantively similar in all cases.

FIGURE 4. Bounds Analysis of Average Treatment Effect
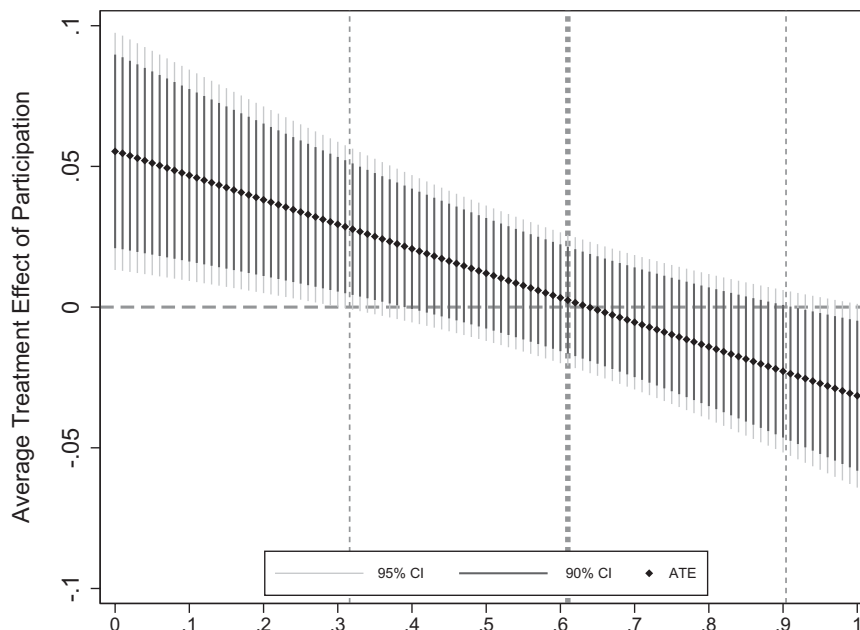
Note: Range bars depict upper and lower bounds at different levels of simulated compliance of the unaudited group. Dashed horizontal lines represents an average treatment effect (ATE) of zero. The thick dashed vertical line represents average compliance among firms that allowed audits (61%). The thin dashed lines represent one standard deviation shifts from mean compliance. Estimated derived from Table 5 (Column 3).

Thus far, our analyses of compliance have assumed that not permitting access to the factory floor is equivalent to noncompliance. Figure 4 relaxes this assumption with a bounds analysis, where we randomly assigned a compliance score to firms that did not provide access to auditors. The goal is to test how much compliance would be required from unaudited firms to diminish the positive relationship between participation and regulatory compliance.

We begin by assigning each unaudited firm a compliance score of zero. We then regress compliance on the treatment conditions, replicating Column 3 (Table 5) and plot the resulting ATE (diamond) of participation and confidence intervals (range bars). Next, we randomly assign a compliance score, but restrict the mean, so that the average unaudited firm received a compliance score of 1%. We repeated this exercise 1,000 times, incrementally increasing the average score until we reached average compliance in the unaudited group of 1 (or 100%), where every firm receives perfect compliance. Figure 4 reports the sensitivity of our analysis to assumptions about the unaudited groups. The dashed vertical lines depict average compliance (61%) and standard deviations observed in the set of firms that allowed auditing. Using this approach, we can observe the sensitivity of our estimates to changes in the assumed level of compliance in the unaudited group. We find that the participation treatment would generate a statistically significant ATE up until 32% average compliance in the unaudited group (40% if we accept a 90% confidence interval). These scores are within one standard deviation of average compliance in the audited group. Although

not statistically significant, the ATE remains positive until it reaches 60% average compliance in the unaudited group. We never observe statistically significant negative effects (at the 95% level) for the participation treatment, regardless of our assumptions about the unaudited group. As a result, the bounds analyses give us confidence that our regulatory compliance results are not the result of selection bias in our ability to audit.[34] The bottom line is under the weaker assumption that compliance in the unaudited firms was less than half the compliance in the audited firms; we would observe results significant at the 0.05 level. If we were a bit more generous and assumed compliance in the unaudited group was 60% of that in an audited firm, we would observe a significant treatment effect at the 0.10 level.

The rest of Table 5 presents robustness tests with less technical solutions to the problems raised by nonaccess. Columns 5 and 6 present results from limiting the analysis to subprovincial jurisdictions, called districts, where auditors received near perfect factory access (>80%). Our assumption is that, for political or sociocultural reasons, firms in these areas felt greater trust that the audit would not lead to negative consequences. As a result, there was less likely to be selection bias associated with our experimental conditions in these groups, allowing us to more accurately measure the effects of participation on

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[34] Calculation of Lee bounds, accounting for biased access, delivers similar results. The upper bound, assuming low compliance in non-audited firms, is 0.078 and statistically significant at the 0.05 level. The lower bound, assuming high compliance among nonaudited firms, however, is −0.059 and not statistically significant.

TABLE 6.   Differentiating Legitimacy From Learning (Limited to Firms Assigned to Receive Government Response Report)

| | Access to factory floor = 1 | | | | Compliance with regulation (%) | | | |
| | | | | | No access = 0 | | High access districts | |
Dependent variable: *Specifications*	No controls (1)	Blocking variables (2)	Sector FE (3)	Auditor FE (4)	Sector FE (5)	Auditor FE (6)	Sector FE (7)	Auditor FE (8)
Participation treatment = 1	0.104** (0.044)	0.132*** (0.050)	0.145*** (0.047)	0.147*** (0.054)	0.068** (0.029)	0.054* (0.029)	0.068 (0.064)	0.037 (0.082)
Hanoi = 1		−0.293*** (0.043)	−0.246*** (0.053)	−0.063 (0.048)	−0.106*** (0.038)	−0.055 (0.037)	0.241*** (0.078)	0.180 (0.118)
Female CEO = 1		−0.199** (0.083)	−0.195** (0.082)	−0.187** (0.077)	−0.116** (0.056)	−0.081 (0.058)	−0.135 (0.093)	−0.100 (0.101)
Baseline probability/constant	0.639	0.639	0.639	0.741	0.339*** (0.042)	0.536*** (0.055)	0.685*** (0.113)	0.683*** (0.152)
Size FE	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sector FE	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Auditor FE	No	No	No	Yes	No	Yes	No	No
Observations	457	457	457	379	450	450	110	110
Clusters	43	43	43	43	43	43	32	32
Pseudo R^2/R2	0.00592	0.103	0.136	0.132	0.104	0.283	0.217	0.306
Log likelihood	−297.1	−268.1	−258.2	−188.0	−173.7	−123.3	−25.76	−19.15
Root Mean Squared Error (RMSE)					0.362	0.328	0.327	0.322

Standard errors, clustered by province-sector, in parentheses (*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$). Analysis restricted to firms that received the government response report. The first panel analyzes whether firms were given access using a probit specification. The second panel studies compliance with the regulation. Equation (1) is unadjusted, equation (2) controls only for blocking variables, equation (3) introduces ISIC two-digit sector fixed effects, and equation (4) introduces auditor fixed effects. Estimating Equations (5) and (6) test compliance with the regulation, treating non-access as zero compliance. Estimating equations (7) and (8) restrict the analysis to districts where auditors were able to access more than 80% of factories in the jurisdiction.

TABLE 7. Differentiating Legitimacy From Substantive Change (by Dropping Commenting Firms)

					Compliance with regulation (%)	
	Access to factory floor = 1				No access = 0	
Dependent variable:	No controls	Blocking variables	Sector FE	Auditor FE	Sector FE	Auditor FE
Specifications	(1)	(2)	(3)	(4)	(5)	(6)
Information treatment = 1	−0.050 (0.078)	−0.038 (0.091)	−0.032 (0.093)	0.029 (0.069)	−0.052 (0.055)	−0.004 (0.043)
Participation treatment = 1	0.173*** (0.050)	0.186*** (0.053)	0.189*** (0.054)	0.099* (0.055)	0.131** (0.051)	0.064 (0.038)
Hanoi = 1		−0.347*** (0.055)	−0.314*** (0.047)	−0.212*** (0.071)	−0.161*** (0.042)	−0.051 (0.056)
Female CEO = 1		−0.100 (0.086)	−0.111 (0.076)	0.002 (0.132)	−0.097 (0.063)	−0.022 (0.071)
Constant	0.0166	0.127	0.127	0.101	0.297*** (0.062)	0.443*** (0.063)
Size FE	No	Yes	Yes	Yes	Yes	Yes
Sector FE	No	No	Yes	Yes	Yes	Yes
Auditor FE	No	No	No	Yes	No	Yes
Observations	337	336	329	256	330	330
Clusters	23	23	20	20	23	23
Pseudo R^2/R2	0.0166	0.127	0.127	0.101	0.123	0.310
Log likelihood	−224.9	−199.0	−194.4	−128.4	−112.5	−72.89
Root Mean Squared Error (RMSE)					0.348	0.314

Standard errors, clustered by province-sector, in parentheses (*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$). Analysis uses coarsened exact matching (CEM) to identify noncommenters in Control and T1 groups. All commenters and potential noncommenters are dropped from this analysis. The first panel analyzes all interviewed firms to see whether or not they were given access using a probit specification. The second panel studies compliance using an OLS specification. Equation (1) is unadjusted, equation (2) controls only for blocking variables, equations (3) and (5) introduce ISIC two-digit sector fixed effects, and equations (4) and (6) introduce auditor fixed effects.

compliance. The coefficients on *Participation Treatment* in Column 5 and 6 are remarkably similar to the previous estimates in Table 4, corresponding to a 5.7 percentage point increase in average compliance in Column 6. Because of the dramatic reduction in sample size, however, the standard errors are larger and the results are not statistically significant. Results in Columns 7 and 8 using CEM also have similarly sized coefficients but are underpowered because of the data trimming necessary for matching. Online Appendix N shows the experiment was most effective among SMEs, which tend to be excluded from Vietnam's highly captured policy-making process.

Our identification of a significant relationship between the opportunity to comment on a draft of the target regulation and subsequent factory floor compliance is particularly impressive in light of the array and degree of real-world challenges that threatened to obscure it.[35] In particular, noise was generated by the opacity of the draft document, which obscured firms' understanding of their obligations and our efforts to measure compliance.

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[35] In Online Appendices F and G, we study how our experimental treatments relate to compliance with each clause in the target regulation, including tests for multiple comparisons.

ADDITIONAL SENSITIVITY TESTS

Was the Participation Effect Still Driven More by Information than Legitimacy?

One potential threat to our findings above is that participation may have generated a higher dosage of information, because the participation group simply had more opportunities to learn about the hazardous chemical regulation than the other groups. More opportunities could generate either more regulatory knowledge or greater worries about potential government enforcement. If this was correct, participation could simply be seen as a stronger treatment for the M2 mechanism. Our information treatment was designed to address these concerns by providing firms with the text of the clauses and the video outlining responsibilities. Nevertheless, all firms in the participation group also received a response report from the Labor Safety Bureau in April 2015.

To test whether this additional round of contact with government influenced compliance, we randomly assigned 97 T1 firms to receive the response report. This allows us to test the effect of *Participation* among firms with extremely high levels of knowledge about the regulation. All of these firms saw the 11 clauses, watched

the video, and received a response report. In Table 6, we calculate the ATE of participation only among highly informed respondents. Focusing on the fully specified Columns 3 and 5, firms in the participation group were about 11.8% more likely to provide access and had average compliance scores that were 6.8 percentage points higher than firms in the information group. These sizable substantive effects give us further confidence that legitimacy is more important than repeated learning in generating regulatory compliance. Furthermore, the randomly assigned response report has no discernable effect on factory access or compliance when analysis is restricted to only the T1 information group (See Online Appendix M).

Was the Participation Effect Driven by Substantive Change?

In Table 7, we examine whether our findings are an artifact of the substantive change mechanism (M3 above), which holds that participation may have altered the regulation, making it easier to comply. Perniciously for our findings, these changes might be idiosyncratic to particular firms and hard to detect, implying that participation may have generated regulatory changes that increased compliance for that specific group of commenters. To address this potential threat, we drop the 28% of firms in the participation treatment that provided comments seen as truly substantive. This left only firms that had the opportunity to comment, but did not exercise it fully enough to affect the regulation. It is therefore impossible for the remaining firms to have contributed to changes in the regulation. Because providing substantive comments is not randomly assigned, we again use CEM to identify firms from the *Control* and *Information* conditions that were similar to responders in observable characteristics and dropped them as well (Iacus, King, and Porro 2012). This allows as unbiased a comparison as possible between noncommenters and those likely to be noncommenters in the control conditions.[36]

Table 7 shows that dropping commenters (and potential commenters in C and T1) actually increases the ATE of participation. The participation treatment is now associated with between 17.3% higher access to the factory in the pre-registered model and 9.9% when auditor fixed effects are included, and between 6.4 and 13.1 percentage points greater overall compliance, although the result with auditor fixed effects is only significant at the 0.1 level. The fact that the coefficient increases substantively when commenting firms are dropped is consistent with the legitimacy mechanism and not the positive or the negative form of the

substantive change mechanism. The significance of results weakens somewhat when we use auditor fixed effects.

CONCLUSIONS

Governments play an important role in using regulatory authority to protect society by limiting the negative externalities of business operations. But governments in many countries have done a poor job in designing and enforcing regulations and—in large part due to corruption—are not seen as governing based on the true interests of their citizens. Based on these unfortunate realities, we hypothesized that firms are more likely to see their governments as legitimate arbiters of right and wrong, and more willing to comply with the constraints and costs of government regulations if they are consulted during the regulatory design process. This view was informed by theoretical work in political science's deliberative democracy and organizational behavior's procedural justice literatures, as well as work across disciplines on the role of reciprocity.

This paper reports on our test of this theory using a field an experiment embedded within an effort by a government-affiliated business association to consult firms in the design of a new labor regulation in authoritarian Vietnam. Our study of this initiative focused on distinguishing between three key mechanisms through which the opportunity to comment on the draft regulation could increase a firm's likelihood of compliance: (1) greater perceptions of legitimacy of government's regulatory authority and of the individual regulation; (2) greater learning about regulatory responsibilities; (3) and change to the regulation itself, making compliance easier for participants. Firms were randomly assigned to treatments representing the legitimacy and information mechanisms, but experimentation was not possible on the substantive change mechanism.

Our study produces encouraging results on the potential benefits to government of making firms feel they have a voice in the regulatory design process. We find that firms asked for comments held state regulators in higher esteem were more likely to provide access to their factories, to engage with government-affiliated auditors offering to help them better understand how to comply, and to exhibit greater actual compliance on the factory floor. These outcomes were not positively influenced by early transmission of information about the regulation during the participation period.

Our results have important implications for four broad literatures in political economy. First, our study overcomes questions of biased selection into participation that have been raised about previous empirical work testing the benefits of consultation (Isham, Narayan, and Pritchett 1995; Mansuri and Rao 2012). Second, we extend the logic of the deliberative democracy and procedural justice literatures to the relationship between firm managers and regulators. These findings contribute to growing literatures questioning and offering solutions to the threat of regulatory capture (Carpenter and Moss 2013; Carrigan and

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[36] Online Appendix O provides a more conventional test by simply controlling for those who responded. This procedure, however, is not recommended because of post-treatment bias. The recommended IV-2SLS approach for identifying the treatment effect on the treated (TET) is also not appropriate in our setting, because theoretically both the opportunity to participate and actual participation are associated with greater compliance. As a result, the participation treatment does not satisfy the exclusion criterion for instrumental analysis.

Coglianese 2011; Lall 2015; Posner 2013; Wilson 1980). Third, we provide a direct test of the consultative authoritarian literature (He and Warren 2011), showing the legitimizing benefits of participatory process in a single-party regime. These findings inform an emergent literature on the enhanced longevity of smarter authoritarians, which has argued that consultation has strengthened regimes' information gathering capabilities and responsiveness (Guriev and Treisman 2015; Morgenbesser 2016; Shambaugh and Brinley 2008). Finally, our findings also speak to an ongoing debate over how greater formalization of interest group representation in the policy process affects the public interest, based on differences over whether interest group involvement improves or exacerbates systematic inequalities (Bartels 2016; Gilens and Page 2014; Greenwood 2017; Hacker and Pierson 2014; Walker and Rea 2014).

Because of our study's broad theoretical reach, it is critical to be clear and reflective about its limitations. First, we could not directly test the substantive change mechanism because of the fact that firms randomly assigned to the treatment and control groups hold identical preferences over the law by design. A variant of the substantive change mechanism is that *perceived change* is driving the results, as previously disenfranchised individuals in the participation group are buoyed by the optimism of seeing themselves as influential. More fine-grained testing of these alternatives is an important space for future research. That said, our evidence shows that firms responded to the opportunity to participate, not to any actual changes to the substance of the regulation.[37] This is a critical nuance in interpreting our findings. Further testing should aim to distinguish whether participation may also affect compliance through a process of active learning about regulations or signaling a threat of greater enforcement (e.g., Kolb and Kolb 2005).

There is also the question of the generalizability of our results beyond our empirical context of northern Vietnam. On the one hand, one could argue that this setting was particularly unlikely for identifying a positive effect of business participation, given the government's continued struggles with corruption and titular communist ideology. As such, the fact that we do find positive effects could be quite broadly applicable to governments around the world that have historically been plagued by poor governance, but have enough capacity to implement policy reform. On the other hand, a less generous interpretation might be that our study benefitted from Vietnam's location in a particular window of opportunity. That is, the legitimizing benefits of recent economic growth and collective memory of the significant advances since central planning have left the country's population unusually optimistic about the intentions of regulatory authorities. These unique circumstances may not be applicable to other states characterized by weak capacity and corruption.

A related limitation concerns the sustainability of the relationships that we identify in this study, and how it may be shaped by the behavior of government. Simply put, if government sees business participation in the regulatory design process as only window dressing, how long will firms believe they have a voice? Although this was not a primary focus within this study, we observed only a few instances where a firm's comments influenced the regulation. This relative lack of influence could eventually undermine firms' belief in government's sincerity about their consultative role. Early efforts to explore the effects of limited government responsiveness over time have produced mixed results (Balla 2017; Malesky and Taussig 2017; Stromseth, Malesky, and Gueorguiev 2017).

For those seeking to extend the lessons from our study into policy recommendations for other poorly governed countries, it is important to understand two sets of scope conditions. First, the costs of noncompliance in our experimental context were, to a large degree, felt directly by firms. Damage because of fire or explosions and production delays caused by dissatisfied or even injured workers all directly affect their bottom line. Moreover, SME owners tend to work on-site and would therefore be personally endangered by poor chemical safety practices. Further research is needed to test whether participation's benefits extend to environmental or food safety regulations, for example, which relate to issues that are more fully external to the firm and its self-interest.

Second, visits to every firm affected by every new regulation are unrealistically expensive and time consuming. Future research should explore lower-cost means by which the state can meaningfully consult a broad-based set of firms. One possibility could involve aiming for spillover benefits from consultation efforts by broadly disseminating information about those efforts to firms not directly involved. Within this study, we did send an additional report about the results of the participation exercise to a subset of firms within the Information group and found that these firms were not any more likely to comply with the regulation. However, it is possible that this treatment was too weak. Another possibility involving online participation is the norm in some countries, including the United States, and spreading around the world, including to middle income countries like Malaysia. The big question is whether such reliance on technology can be designed in ways whereby firms still feel heard and the legitimacy mechanism described in this paper can still function.

SUPPLEMENTARY MATERIAL

To view supplementary material for this article, please visit https://doi.org/10.1017/S0003055418000849.

Replication materials can be found on Dataverse at: https://doi.org/10.7910/DVN/IANHOG.

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