Slippery Fish:

Enforcing Regulation under Subversive Adaptation

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Abstract

Attempts to curb illegal activity through regulation gets complicated when agents can adapt to circumvent enforcement. Economic theory suggests that conducting audits on a predictable schedule, and (counter-intuitively) at high frequency, can undermine the effectiveness of audits. We conduct a large-scale randomized controlled trial to test these ideas by auditing Chilean vendors selling illegal fish. Vendors circumvent penalties through hidden sales and other means, which we track using mystery shoppers. Instituting monitoring visits on an unpredictable schedule is more effective at reducing illegal sales. High frequency monitoring to prevent displacement across weekdays to other markets backfires, because targeted agents learn faster and cheat more effectively. Sophisticated policy design is therefore crucial for determining the sustained, longer-term effects of enforcement. A simpler demand-side information campaign generates two-thirds of the gains compared to the most effective monitoring scheme, it is easier for the government to implement, and is almost as cost-effective. The government subsequently chose to scale up that simpler strategy.

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1 Introduction

Correcting market failures and improving economic efficiency often requires curbing undesirable behaviors of market agents who act to maximize their private benefits. Examples span actions that affect the natural environment, such as deforestation (Jayachandran et al., 2017), pollution (Duflo et al., 2013, 2018), or over-exploitation of natural resources (Stavins, 2011); Actions that affect community health such as drunk driving (Banerjee et al., 2017) and open defecation (Guiteras et al., 2015); Or actions that undermine government performance such as corruption (Fisman and Wei, 2004; Olken, 2007) and tax evasion (Carrillo et al., 2017; Fan et al., 2018). Enacting and enforcing regulations is the most direct strategy to deter such behaviors. Implementing this strategy requires strong institutions to enforce laws, plus sophisticated policing to track agents' reactions to enforcement, so that rules are robust enough to curb the undesirable behavior even when regulated agents try to 'game' the new system.

Effective enforcement is challenging precisely because the targeted agents will react to the new set of rules, finding loopholes that allow them to continue maximizing private benefits at the expense of others.[1] In many instances, it is therefore insufficient to evaluate the effectiveness of enforcement activities based on their immediate, short-run effects, before regulated agents have had a chance to adjust to the new regime. A more sophisticated evaluation will need to track the (sometimes unanticipated) strategies that targeted agents may deploy to circumvent the regulation.

Economic theory provides guidance on the design of optimal audit strategies when monitored agents can adapt. A large literature on audits in static principal-agent frameworks largely restricted attention to deterministic audit strategies [e.g. Townsend (1979)]. Mookherjee and Png (1989) show that when monitoring is costly and agents can hide their undesirable activity, a policy of random audits dominates deterministic audits. Okat (2016) goes further to show that even when monitoring is costless, stochastic audits are optimal because it dissuades cheating by the agent. His theory also presents an important counter-intuitive insight: The monitor could better deter fraud by *reducing* the frequency of audits. This occurs because in a dynamic model, each audit creates a learning

[1] For example, Carrillo, Pomeranz, and Singhal (2017) show that when the Ecuadorian tax authority improves the quality of their information on firm revenues, the firms react by raising their estimates of costs in line with the revised revenue estimates, to keep total tax payments unchanged. Blattman et al. (2017) shows that intensive policing pushes crime around the corner, with null impacts on overall violent crimes. Health officials adapt to undermine a monitoring scheme to punish delinquent nurses in Banerjee et al. (2008), making an initially-effective program completely ineffective in 18 months.

opportunity for the agent about the nature of the audit technology. By lowering the frequency of audits, the monitor frustrates the auditor's ability to learn.

We test the empirical relevance of these insights by investigating the effects of – and the limits of – enforcement in the context of illegal sales of the critically endangered Pacific hake fish (*merluza*) in Chilean markets. The government of Chile has instituted a ban on fishing and sales of hake during September each year, when the fish reproduces. Catching hake during that period is especially ecologically destructive. We implement a randomized controlled trial (RCT) in which government agents monitor and penalize vendors that sell illegal fish, while we surreptitiously monitor vendors' reactions to that enforcement by deploying "mystery shoppers" to search for illegal hake in markets. Following the theoretical literature on optimal audit strategies, we also randomly varied the predictability and frequency of enforcement activities to study whether those variations are more effective in achieving the social goal in the presence of subversive adaptation by regulated agents. We find evidence consistent with Mookherjee and Png (1989)'s recommendation of random audits, and Okat (2016)'s counter-intuitive insights about low-frequency audits.

To guide policy, we also implement a consumer information campaign which allows us to benchmark the effects of enforcement against this demand-side strategy that is easier for policymakers to implement. When it is difficult or expensive to enforce rules, less direct strategies such as information campaigns designed to change social norms around the undesirable behavior, or marketing that appeals to people's sense of fairness, or encouraging third-party reporting (Naritomi, 2018), may be more reliable or more cost-effective.[2] Our information campaign was designed to educate consumers about the environmental risk associated with over-fishing of hake and to discourage the consumption of illegal hake during the September ban period. This campaign could even complement the audit strategy: If vendors react to the enforcement by hiding their illegal hake sales, then informed consumers may be an important second line of defense. Our 2x2 experimental design with enforcement, information, both, and neither can test for such complementarities.

Since we are tracking an illegal activity, our main outcome variables for the evaluation are collected through a "mystery shopper" methodology. We sent trained surveyors who look like typical shoppers to each market to pose as buyers and (try to) purchase fish during the ban. We collected data on whether it was possible to buy hake and its substitutes, fish prices, and recorded strategies vendors

[2] For example, Chetty et al. (2014) partners with the Bangladesh tax authorities in an attempt to change social norms to encourage firms to pay taxes (as opposed to enforcing tax laws directly), and Guiteras et al. (2015) attempt to change social norms around toilet use (as opposed to directly banning the dangerous practice of open defecation).

were employing to circumvent the ban. Vendors would have an incentive to hide illegal hake sales from enumerators, which is why this approach improves the credibility of our evaluation data.

We also conducted consumer surveys before and after the interventions, to gather data on changes in demand for hake and other substitutes, and consumer knowledge about the hake ban. We mapped all spatial and market relationships between vendors and fishermen to study spill-overs across markets.[3] Finally, we surveyed the fishermen who supply to these markets to explore whether interventions implemented "downstream" (at the point of sale from vendors to consumers) traveled "upstream" the supply chain of fish. It is ultimately the fishermen who make the ecologically sensitive decisions in the seas. Our sample covers all major markets where the majority of hake is caught, which allows us to report on equilibrium outcomes, such as changes in fishermen activities or the price of hake substitutes. This produces a more comprehensive evaluation of the full range of effects up and down the supply chain.

We find that many vendors continue selling illegal hake during the ban, but both the enforcement treatment and the information campaign reduced their propensity to do so. Declines in hake sales in treatment areas during the ban period are twice as large as the decline in the control markets. Enforcement generates slightly larger reductions in hake sales compared to the information campaign. Our mystery shopper data provides clear evidence that vendors react to enforcement activity by engaging in new practices designed to circumvent our attempts to levy penalties. Many vendors do not display the hake openly during the ban, but are willing to sell our mystery shoppers hake fish that is hidden from plain view. They also start keeping the hake on ice and claiming that the fish on display was caught in August when it was still legal to do so. These reactions attenuate the effects of enforcement on the *true* availability of illegal hake in markets.

Our experiments varying the specific attributes of the enforcement policy yields new insights about how to design and implement more effective audit strategies when regulated agents adapt to circumvent monitoring. First, monitoring vendors on a predictable schedule is not effective. Vendors find it easier to cheat (through hiding and freezing) when the enforcement visits become predictable. We also tried increasing monitoring frequency to better contain temporal and spatial spillovers to other days of the week or other nearby markets, but this strategy backfires. Increased frequency evidently allowed fish vendors to learn the monitoring routines more quickly and react with greater hiding and freezing of illegal fish. Okat (2016)'s dynamic model predicts that random and less frequent enforcement hinders

[3] We use the gendered term "fishermen" because every single fisher we interviewed in Chile was a man.

or delays agents' learning about the weaknesses of the auditing process, because they *"learn how to exploit the weaknesses inherent in any audit methodology if they face the same method many times"*. In the subset of locations where we send monitors on an unpredictable and less frequent schedules, vendors were not able to learn and adjust as quickly, and this resulted in large reductions in hake sales, even accounting for the hiding and freezing response. These findings shed light on a larger theoretical literature on subversive reactions to regulations (Glaeser and Shleifer, 2003; Becker, 1968; Eeckhout et al., 2010).

We generate evidence on the real world challenges to implementing an auditing scheme in one specific sector, but the sector and policy we study are globally relevant and important. FAO (2014) estimates that 31.4% of the world's fish stocks were over-exploited to biologically unsustainable levels in 2013, up from 10% in 1974. Illegal fishing accounts for US$10-23 billion worth of fish each year. Fishing bans of the type we study in Chile are in effect in many countries around the world, including China, Fiji, India, Ghana, Bangladesh, Peru and Myanmar. Some of these other policies are extremely similar in structure to the Chile hake ban, such as a 22-day ban on selling Hilsa fish in Bangladesh during the fish's reproduction period, and a 60-day ban on silverfish in Peru.[4]

In summary, we first develop a data collection and evaluation strategy that allows us to clearly document agents' 'hidden' illicit adaptation to enforcement. We then test predictions from the theory of optimal audits and show that random audits conducted at low-frequency are more effective in achieving the principal's goals. Finally, we show that an easier-to-implement consumer information campaign is almost as effective in curbing the illegal activity as direct monitoring.[5]

The paper is organized as follows. We describe the context and experimental design in section 2. Section 3 presents predictions based on an informal framework. Section 4 describes the different data sources and present summary statistics. Section 5 presents the empirical strategy and results, section documents spillovers and market equilibrium effects, and section 8 concludes the paper.

[4]See http://www.newagebd.net/article/52220/22-day-ban-on-hilsa-fishing-from-oct-7 and https://elcomercio.pe/economia/peru/produce-establece-veda-nacional-pejerrey-60-dias-noticia-543012

[5]Our paper is related to the empirical literature on the effects of monitoring and penalties (Boning et al., 2018; Shimshack and Ward, 2005; Gray and Shimshack, 2011; Hansen, 2015; Pomeranz, 2015). Like our consumer information campaign, many other papers have evaluated indirect strategies in pursuit of social goals, in environments where enforcement is expensive or difficult (Jin and Leslie, 2003; Reinikka and Svensson, 2005; Alm et al., 2009; Shimeles et al., 2017; Kollmuss and Agyeman, 2002).

2 Background and Experimental Design

2.1 Context

With around 4,000 miles of coastline, Chile is one of the top ten fish producers in the world (FAO, 2014). However, as in many other low and middle-income countries, the marine ecosystems have been threatened by over-fishing. The Chilean government has passed various regulations to protect threatened species over the last 20 years, including restrictive fishing quotas and fishing ban periods. However, the fish population has continued to decrease, with 72% of species rated as overexploited or *collapsed* by 2015 (Subpesca, 2015). Regulations have been difficult to implement and enforce.

The majority of people carrying out fishing activities are small-scale and artisanal fishermen, which makes the sector very difficult to regulate. Small-scale fishermen contribute almost 40% of the national fishing volume, and up to 75% of the hake fish market. Artisanal fishermen are organized in fishing villages called *Caletas*. Around 76% of the caletas are located in rural areas along the extended Pacific coast, and they are highly spatially dispersed (Subpesca, 2013). Their geographic dispersion, informality, and the small-scale of operations of each individual fisherman make it difficult for the government to monitor their activities. The absence of alternative income-generating activities for these fishermen has also make it difficult to change the norms regarding "appropriate behavior" in this industry. Furthermore, poor small-scale fishermen do not readily accept government-imposed restrictions, and they have organized and unionized to create political opposition to government policies that restrict fishing.

The Pacific Hake is the fish low and middle-income Chileans consume most, and also one of the most important sources of protein for this population. The domestic hake market is served entirely by the domestic supply. Imports and exports of hake are quite uncommon. In an effort to protect the hake population, the Chilean National Marine Authority (*Sernapesca*) and the central government have enacted various policies including restrictive fishing quotas and a one-month ban on fishing and selling hake during the fish's September reproduction cycle. Due to difficulties in enforcing the ban, the hake population is now critically threatened, and has shrunk to 18% of its long-term sustainable level (Subpesca, 2015).

2.2 Supply Chain of Illegal Fish

2.2.1 *Caletas:* Coastal Villages where Artisanal Fishermen Bring in their Catch

Most of the illegal hake fish is captured by small-scale rural fishermen operating out of hundreds of *caletas* dotting the coastline. Each *caleta* contains between 10 and 100 fishing boats. Boats are about 20 and 30 feet in length, and operated by two to three fishermen (see Figure A.1). The fishermen operating out of each *caleta* are organized as a union to internally distribute the fishing rights allocated to that *caleta*. In practice, each fisherman captures illegal, undeclared fish beyond the allocated quota. WWF (2017) estimates that the amount of hake fished by small-scale artisanal fishermen are between 3.8 and 4.5 times the legal quota. As a result, the artisanal sector is responsible for 75% of the hake fish supplied in the market, even though they hold only 40% of the "official" hake quotas.

Fishermen go fishing using artisanal boats and nets at night and sell fish after sunrise. They are able to target specific fish types by varying the location and depth at which the nets are dropped. The fish is sold directly at the docks to three types of buyers: (1) fish vendors who buy the fish to sell them in local markets, (2) intermediaries who supply fish to vendors located in places further from the coast, and (3) households who live close to the *caleta* and buy the fish for their own consumption. There is very little use of ice and refrigeration at this point in the supply chain. The fish that vendors sell in local markets is typically fresh, and captured the night before. Table C.3 in the Appendix describes *caleta* characteristics.

2.2.2 *Ferias:* Outdoor Markets where Hake is Sold

The majority of hake-fish sales to final consumers occur in *ferias*, which are outdoor urban markets organized by municipalities. Each vendor pays a fee every six months to rent a selling spot in the market. In addition to fish, *ferias* sometimes contain stalls offering fruit and vegetables, clothes and other products.

Ferias are typically navigable only by foot, and each feria serves a limited geographic area of surrounding neighborhoods. To cover more neighborhoods, the vendors rotate between different *ferias* in a pre-set pattern - typically setting up in the same location twice a week. For example, they may sell at a first feria every Sunday and Wednesday, at a second feria every Tuesday and Friday, and at a third feria every Thursday and Saturday. The group of vendors who move together across neighborhoods is called a *circuit*. A semi-annual fee paid by the vendor to the municipality covers her inclusion in the entire circuit, so the same group of vendors typically rotate across neighborhoods all together. Vendors are not allowed to sell in public places other than *ferias*.

Each municipality typically organizes one circuit of vendors. Large municipalities may have more than one circuit. In such cases, the municipality area is divided in such a way that there is no geographic overlap between circuits. Figures A.2 and A.3 in the Appendix provide visual examples of ferias and circuits. Table C.1 describes observable characteristics of fish stalls in ferias.

2.3 Experimental Design

This study was implemented in close collaboration with the Chilean National Fish Service (Sernapesca), who has the ultimate regulatory authority over fishing activities in the country. Our implementing partner's goal from this project was to limit hake fishing, sales and consumption during the September ban. It is practically and politically very difficult for them to directly regulate fishermen, because their activities occur out in the water at night, and because the fishermen operating out of the geographically dispersed *caletas* are politically organized. *Sernapesca* therefore expressed an interest in exploring options to better regulate the fish sales at *ferias* where hake is most commonly sold.

2.3.1 Sample

We conduct our experiment in the five central regions of Chile, which is home to 74% of the Chilean population. The caletas located along the coastal villages and cities scattered across these five regions account for 98% of all hake fish harvested in Chile. We conduct our experiment in all ferias in these regions except for the city of Santiago.[6]

An important benefit of conducting the experiments at such a large and comprehensive scale is that it allows us to track any displacement of illegal hake sales towards control markets, because all potential markets (including ones where the interventions were not applied) are in our database. This allows us to trace the market-level equilibrium effects of our interventions. We collected data on the universe of circuits in our sample area, and from every fish vendor operating in those circuits. We

[6]Santiago is unique in that there is one big centralized fish market called *Terminal Pesquero Metropolitano* (TPM) where vendors buy from intermediaries to re-sell at neighborhood ferias. TPM is already well-monitored by *Sernapesca*, and our interventions therefore did not need to be implemented there.

mapped all ferias served by each caleta where the fish are caught. The unique long and thin geographic shape of Chile means that ferias are generally located very close to the caletas from where they source fish (22 miles away on average). This made it relatively easy to connect vendors to the fishermen they source from, and trace how the effects of our interventions are transmitted along the supply chain for hake fish.

There are 280 ferias (fish markets) operating in the 70 municipalities in our sample, and these ferias are organized into 106 separate *circuits*. In order to identify and map all existing ferias and circuits, we combined administrative data from multiple sources (Ministry of Economics and Sernapesca) along with information gathered from phone conversations with staff in every municipality. We then used Google Maps to define the consumer "catchment area" for each feria. We identify the neighborhoods which are likely served by each feria, considering the walking distance and road accessibility from the neighborhood to the feria, as well as the residential versus commercial/industrial characteristics of the neighborhoods. The location of the ferias and their organization as circuits were important for the design of our enforcement intervention. The definitions of the residential neighborhoods and their connections to each feria were important for the design of our consumer information campaign.

2.3.2 Interventions

This study experimentally evaluates the effects of two complementary interventions that aimed to reduce illegal sales of hake during the September ban period. These interventions were designed to affect:

1. The **supply** of hake by monitoring vendors and enforcing penalties on those found to be selling illegal hake.
2. The **demand** for hake through an information campaign designed to sensitize consumers about this environmental problem, and discourage hake consumption during the ban.

2.3.3 Design of Enforcement Intervention

The supply-side enforcement intervention deployed government officials from *Sernapesca* to periodically visit ferias where fresh hake is usually sold, and levy fines if vendors are caught illegally selling hake during the September 2015 ban period. A enforcement visit consisted of two Sernapesca officials visiting all fish stalls in a market. The officials were instructed to follow the usual Sernapesca protocols to search for illegal fish at each stall.[7] Our conversations with vendors prior to September 2015 suggested that they were already well aware of the hake ban. The most important change in 2015 compared to earlier years was that the enforcement activities were applied more consistently and regularly. As a part of this randomized controlled trial, Sernapesca agreed to conduct this monitoring at specific locations and according to schedules defined by the research team. Sernapesca shared the details about their monitoring activities with the research team. The punishment for illegal sales is a US $200 fine plus confiscation of the illegal fish.$200 is equivalent to two weeks of earnings for the average feria vendor, so this represents a significant threat.

We anticipated that fish vendors would react to the enforcement activity by devising new defensive strategies that would help them avoid paying fines while continuing to sell hake in September. We introduced random variations in the enforcement policy design to investigate whether specific design variations make enforcement more or less effective in the presence of agents' efforts to circumvent the policy:

1. *Predictability:* We randomly varied the ease of predictability of the enforcement. In some areas, Sernapesca monitors followed a consistent schedule (e.g. M,W at 9am) while in other areas, they were asked to follow a less predictable schedule defined by the research team. For the less predictable treatment, the research team randomly varied the day in which the visit is deployed, keeping constant the total number visits week by week. The latter is a more expensive enforcement strategy because it requires having monitors on-call for longer windows. This strategy was practically more difficult for Sernapesca to implement.

2. *Frequency:* We randomly varied audit frequency at the circuit level, so that some groups of vendors only received one visit per week, while others were visited multiple times at the various locations in the city where they set up on different days of the week. Increasing the frequency of monitoring visits is more expensive, but our thinking was that it may limit vendors' ability to relocate illegal hake sales spatially and inter-temporally during the week. On the other hand, it may also accelerate vendors' "learning curve" about the nature of the 2015 enforcement, and

[7] The enforcement protocol used in September 2015 was 'business as usual', with no additional instructions to the inspectors. The study design was negotiated at a higher level, and most of the inspectors did not know about the existence (or aim) of the study. They merely followed instructions on where and when to visit markets.

devise effective defensive strategies more quickly.

Enforcement activities were randomized at the circuit-level, covering all 106 market-circuits. This randomization was stratified to ensure balance with respect to a few important spatial and market characteristics: (a) Whether the circuit was located in a coastal municipality, (b) Whether the circuit was the only one operating in its municipality, and (c) whether the circuit served geographically isolated communities.

2.3.4 Design of Information Campaign

The demand-side intervention was a marketing campaign designed to inform consumers about the September ban on hake sales. Sernapesca distributed letters, flyers and hanging posters in the residential neighborhoods randomly assigned to this intervention. The message contained in the flyers and posters was simple: "In September Respect the Hake Ban." The letter, signed by the Director of Sernapesca, included three paragraphs. The first paragraph informed readers about the hake ban every September. The second noted the decline in the hake population to a critical level as a result of over-exploitation, and the third encouraged consumers to not consume hake this month. Appendix B shows samples of flyers and the letter. In previous years the budget allocated to informing consumers about the hake ban was considerably lower and was mostly invested in newspaper articles and highway billboards. September 2015 was the first year that the information was distributed directly to consumers at a household level. For most consumers, the information campaign should be interpreted as new information. We will show that in areas where the information campaign was implemented, consumers become more likely to mention the ban (unprompted) to our enumerators relative to control areas.

Using our mapping exercise described in section 2.3.1, and combining it with the location of major roads and crossings, we define boundaries of neighborhoods and attempted to divide the municipality up such that the population-size of neighborhoods would be roughly equal. We conducted this intervention in the 48 most populated municipalities and identified 270 distinct neighborhoods in those municipalities. Figure B.3 provides example maps. The randomization procedure was as follows:

1. First, 18 of the 48 most populated municipalities were assigned to a high saturation information treatment, 17 to a low saturation information treatment, and the remaining 13 municipalities

did not receive the letters, flyers or posters. “High saturation” was defined to be a case where two-thirds of the neighborhood in the feria’s catchment area would receive the letters, flyers, and posters. In the low saturation treatment area, only one-third of the neighborhoods received those mailings. We randomly varied the proportion of neighborhoods receiving the treatment to examine whether there are larger changes in norms regarding the acceptability of inappropriate or socially harmful behavior when households observe that many of their neighbors simultaneously receive the same information about the illegality of hake consumption.

2. Second, specific neighborhoods within each high or low saturation information treatment area were randomly chosen to receive the treatment.

3. Third, we randomly selected around 200 addresses in each of 102 neighborhoods, and mailed out letters to each of those 20,400 addresses. 200 letters cover roughly 15% of all potential addresses in a representative neighborhood. Based on information from the postal service, we subsequently learnt that at least 13,000 letters were correctly delivered.[8] 80,000 flyers were distributed by trained field personnel to people walking in the streets, and directly to households within the 102 treated neighborhoods. 3,000 posters were placed around treated neighborhoods where they would be publicly visible, such as at bus stations, community centers, and street intersections.

2.3.5 Cross-Randomized Experimental Design

The enforcement treatment and the information campaign were cross randomized in a 2x2 experimental design so that we could study potential complementarities between the two approaches. Table 1 lists the number of circuits assigned to each of the four treatment cells.

[8] Although 13,000 were explicitly tracked, it is likely that around 16,500 were actually delivered, because the postal service did not receive any delivery failure notice in those cases. We inferred and constructed addresses using Google maps, and many of those addresses did not actually exist. That was a leading cause of delivery failure.

Table 1: Treatment Assignment

	No Enforcement	Enforcement	Total
	N	N	N
No Information Campaign	9	41	50
Information Campaign	14	42	56
Total	23	83	106

This table lists the number of circuits assigned to each experimental cell jointly defined by the Information Campaign (row) and the Enforcement treatment (column)

The majority of markets were assigned to Enforcement because that column contains additional sub-treatments in which we conduct experiments on variation in enforcement policy design. Those variations in predictability and frequency of enforcement visits were cross-randomized so that we have sufficient statistical power to study the effect of each variation, one at a time. Table 2 shows the number of circuits assigned to each sub-treatment cell. To study the effects of predictability of enforcement, we will compare the 39 circuits where Sernapesca monitored on a predictable schedule against the 44 circuits where they monitored on an unpredictable schedule. Similarly, to study the effects of audit frequency, we will compare the 34 circuits assigned to high-frequency against the 49 circuits assigned to low-frequency.[9]

Table 2: Enforcement Sub-treatments

	High Intensity Enforcement	Low Intensity Enforcement	Total
	N	N	N
Predictable Enforcement Schedule	19	20	39
Unpredictable Enforcement Schedule	15	29	44
Total	34	49	83

This table lists the number of circuits assigned to each experimental cell jointly defined by the row and column headers

Tests of the information campaign saturation effect (i.e. proportion of neighborhoods around markets that are simultaneously sent letters and flyers), will compare the 30 circuits randomly assigned to a low-saturation campaign (where a third of neighborhoods received letters and flyers), against the

[9]The probability of assignment to low-frequency enforcement and to un-predictable schedules was a little higher compared to other cells. In our analysis, we will control for these differences.

other 26 to a high-saturation campaign. We are able to control for other dimensions of random assignment whenever we focus on the effects of one particular dimension. Each of our treatments could have spillover effects on control markets, and we discuss those issues in section 6.

3 Theoretical Predictions

In theory, the enforcement treatment increases fish vendors' (perceived) probability of being detected and fined when selling illegal hake. The supply curve for hake in September should shift to the left, which will reduce the quantity of hake available in the market, and increase the market price. The subset of vendors who continue selling illegally will need to be compensated with higher prices for the increased risk they take.

The information campaign was designed to deter consumers from buying illegal hake. If successful, the demand curve should shift to the left, with fewer consumers purchasing at any given price. This should also decrease the quantity of hake sold in treated markets. Equilibrium prices should fall with this treatment. In summary, both the enforcement and information treatments should decrease the quantity of hake sold, but the net effect on price when both treatments are administered is ambiguous.

Penalties are levied if inspectors find *any* hake in the market, and most of our empirical analysis will therefore focus on vendors' propensity to sell hake at all. This is also more reliably measured than total quantities of hake sold. It would be unnatural for mystery shoppers to ask vendors how much hake they sell in total when they pose as regular buyers.

3.1 Predictions on Fish Vendors' Reactions to our Interventions

We anticipated that vendors would react to the enhanced monitoring, or to more informed consumers, and would try to continue selling hake illegally by circumventing our enforcement and informational strategies. We designed our data collection strategies to shed light on those reactions as well:

1. Vendors may hide the illegal fish they sell in containers in the back, rather than display the fish openly. Another reaction we had not anticipated, but learnt about quickly from our mystery shoppers, is that vendors try to pass off the hake as legal by keeping it on ice and claiming that it is frozen fish caught legally in August.

What determines vendors' choices to sell visible or frozen or hidden hake? Icing hake to give it a frozen look reduces the quality/taste and appearance of freshness of the fish, which could lead to lower demand or lower prices. Hiding fish may depress demand because vendors have to rely on customers to explicitly ask for the fish. Displaying fish at their stall is the main advertising strategy that vendors use, so hiding is costly. On the other hand, selling visible fish puts the vendor at risk for a fine levied by *Sernapesca*, or complaints from environmentally-conscious consumers. Vendors' decisions on whether and how to sell hake will depend on the tradeoff between reduced prices and demand from freezing or hiding, versus the expected cost of fines.

2. Vendors may also displace hake sales from monitored *ferias* to un-monitored *ferias* on a different week-day.

3. Vendors will need to learn about Sernapesca monitoring, before deciding whether to engage in defensive strategies like hiding and freezing. The speed and ease of learning about the nature of the enforcement will vary depending on how frequently Sernapesca officials visit, and whether those visits occur on a predictable schedule (Okat, 2016). It may also depend on whether consumers are informed, because hiding illegal activity from both clients and regulators may be much more complicated than hiding from regulators alone.

While monitoring on unpredictable schedules is more complicated and costly for the regulator,[10] it may prevent vendors from learning quickly about how to circumvent the enforcement efforts. Economic theory predicts that random monitoring visits will be more successful at reducing illegal hake sales than monitoring on a predictable schedule when vendors have the option to hide or freeze the hake, or displace sales to other market days (Mookherjee and Png, 1989).

Increasing the frequency of monitoring visits in our context implies that we visit the same vendors in multiple locations in their *circuit*, where they set up shop on different weekdays. We anticipated that this would make it more difficult for vendors to displace illegal sales from one week-day to another, from one of their market locations to another. However, high frequency visits can also allow vendors to learn about the regulators' schedule faster, and undertake effective defensive investments (Okat, 2016). They also create more vendor-regulator interactions, and allow vendors to learn quickly about the loopholes and weaknesses in the enforcement strategy, such as Sernapesca's unwillingness

[10] Administrative data from Sernapesca suggests that maintaining unpredictable schedules increase their program costs by 10% over and above a fixed schedule.

to penalize sales of hake that are on ice and plausibly caught in August. Increasing the frequency of enforcement therefore has an ambiguous effect on the sale of illegal hake if it is difficult for the regulator to detect and penalize behaviors like hidden or frozen hake sales.

Finally, while the demand-side information campaign and the supply side enforcement are normally thought of as substitute strategies to deter undesirable behaviors, the two approaches can become complementary in the presence of hidden and frozen sales. Hiding illegal hake only works well if consumers are willing to purchase hake in September. Putting fresh hake on ice can create plausible deniability when facing regulator fines, but may not be sufficient to fool consumers. The presence of an information campaign can therefore enhance the effectiveness of enforcement in this context.

3.2 Predictions on the Nature of Spillover Effects

Enforcing the hake ban can change the demand for other fish species. Vendor stalls often offer more than one type of fish, and hake is the only fish that is banned in September. Consumers may choose to substitute to other fish as a result of our intervention, and fishermen may substitute to catching those other fish. We might expect to see an increase in sales of substitute fish in treatment areas. Prices of substitutes may also change if treatment and control markets are somewhat segmented. The direction of the price change will depend on the strength of the consumer response relative to the fishermen response.

Changes in consumer and vendor behavior may have an upstream effect on fishermen in *caletas* via the fish supply chain. If the quantity of hake fish sold in *ferias* decreases in September, that should affect the behavior of fishermen who serve those vendors. We will map the fish supply chains to empirically examine effects further up the supply chain.

Finally, control areas may experience some spillover effects. There are four possible channels of spillovers in our context:

1. Treated consumers who learn about the ban may inform other untreated consumers.
2. Treated vendors may share information with other vendors operating in control areas.
3. Some *caletas* supply fish to both treatment and control *ferias*. If fishermen switch their fishing strategy in response to treatment, supply of fish in control markets may also be affected.

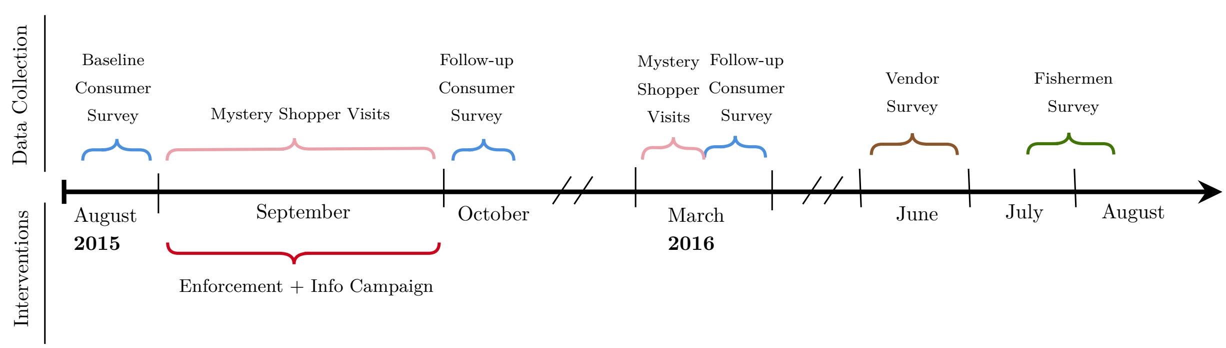
4. If the information campaign is potent enough to decrease aggregate demand and the price of hake, there may be a general equilibrium spillover effect in control markets that enhances the direct effect of the treatment. On the other hand, if enhanced enforcement increases the price of fish, then the general equilibrium price spillover will undermine the direct effect of treatment.

It is important to understand the nature and direction of these spillovers, because the program effects might be under-estimated under certain types of spillovers. Or worse, if fishermen simply sell more illegal hake to control areas in response to treatment, then we will observe a treatment-control difference even if the true effect of the enforcement and information campaigns on aggregate hake fishing is nil. We will collect data on spatial, supply chain, and personal relationships between treatment and control markets in order to analyze (and control for) these potential market spillovers.

4 Data

We conducted several different surveys to evaluate the effects of these interventions. "Mystery shoppers" visited fish markets to surreptitiously gather information about hake availability, once during the ban (September 2015), and again six months later in March 2016. We conducted two rounds of surveys of consumers during those same two periods. We also surveyed fishermen at *caletas* and vendors at *ferias* to map the fish supply chain and investigate spillovers. Figure 1 describes the timing of the interventions and data collection activities. In total, seven different data sources are used in the analysis.

Figure 1: Timeline of Interventions and Data Collection



4.1 Mystery Shopper Surveys

We are interested in studying whether fish vendors engage in an illegal activity. To reliably measure this, the vendors cannot know that they are monitored. This poses an interesting data collection challenge. To develop a strategy to address this challenge, the research team visited dozens of ferias before the ban to understand the market structure and relationships between vendors and consumers. We learnt that vendors do not know most shoppers, so an unfamiliar face will not necessarily raise any suspicion. This made it a good environment to deploy *mystery shoppers* and collect data surreptitiously. 29 enumerators was trained to work as mystery shoppers. They were mostly women between the ages of 40 and 50, because this demographic group represents the typical feria customer profile. The mystery shoppers were trained to look and act like ordinary shoppers, to pose as buyers and (try to) purchase hake fish from the vendors. Mystery shoppers were not told the treatment status of any market, to guard against the possibility that they inadvertently behaved differently in treatment and control markets.

These mystery shoppers gathered information on whether it was possible to buy hake, and on the market price of the fish. They were also instructed to collect information on what else was available for sale at the fish stalls and their prices, and to note down what was being purchased by other shoppers in their presence. The visit protocol was piloted and refined through multiple visits to ferias and informal trial-and-error conversations with vendors before the start of the ban period, to make sure we elicited the required information without raising suspicion. Given this methodology, we could not collect information about the total quantity of hake being sold, because that would be unnatural for a typical shopper to ask about, and it would have made the vendors suspicious. The main outcome variable that this survey therefore produces is an indicator for whether it was possible to buy hake at any particular stall. The mystery shoppers also noted down general characteristics of the stall and vendor. They also wrote down notes on the behavior of fish vendors, including conversations occurring in their presence. This is how we learnt about the practice of selling “frozen hake”, where the vendor kept the fish on ice and claimed that it was caught legally in August. Many of those same vendors admitted to our mystery shoppers that the “frozen” fish was in fact, fresh.

Mystery shoppers visited each circuit three times on average during the September ban. We conducted an additional round of mystery shopper visits in March 2016, to better understand how the market operates outside the September ban period.

4.2 Consumer Surveys at Fish Markets

We also surveyed consumers before and after the ban period. A separate team of enumerators (distinct from our mystery shoppers) stopped consumers close the points of entry and exit for the fish market, and asked questions with a survey instrument in hand. To encourage unbiased responses, enumerators informed consumers that the survey was conducted by university-based researchers, and that it aimed to gather information about food consumption in ferias. They were not asked to provide any personal identifiable information, and we only inquired about the list of food purchased in the feria in the past month - avoiding asking direct questions about the consumption of hake. We also asked consumers to provide a sense of their home location on a physical map we carried, so that we could match their residence to the neighborhoods assigned to the information treatment.

In total, 3,300 consumers were surveyed in October 2015 through 54 enumerator visits, and 3600 in March 2016 through 95 enumerator visits. This produces two rounds of a repeated cross-section; the same consumers were not followed over time.

4.3 Survey of Vendors at Fish Markets

We surveyed fish vendors in every market in our sample in June 2016 (outside the ban period). We asked vendors about the suppliers and intermediaries they source their fish from, so that we could map out the supply chain. We also asked vendors about their contacts with fish vendors who operate in other circuits, in order to study spillover and network effects.

4.4 Survey of Fishermen at Coastal Fishing Villages

To understand whether the effects of our interventions were transmitted upstream via the supply chain, we conducted a survey of fishermen in every coastal village in the region where hake fish is caught and distributed. This survey was carried out during July-August 2016, outside of the ban period. We surveyed 231 fishermen from 74 fishing villages (caletas). Figure A.4 in Appendix A.2 contains a map of all caletas and fish markets.

Surveying fishermen was valuable for two reasons. First, the interventions were designed to ultimately reduce illegal fishing, so understanding the activities of the fishermen is essential for public policy. Second, the treatment effects may have spilled over to control areas if treatment and control markets are served by the same fishing village. Understanding these supply-chain connections are important for analyzing spillovers. These connections are described in Figure 2, which organizes our interventions and data collection activities along the supply chain for fish.

Figure 2: Interventions and Data Collection at different Points along the Fish Supply Chain

5 Results

We start by reporting treatment effects on availability of illegal hake in the market. We then analyze how vendors react in their attempt to circumvent the treatments, and whether our sub-treatment variations limit their ability to do so. We then track market-level spillovers using information from the fishermen and vendor surveys, and re-analyze the main treatment effects accounting for spillovers. Finally, we show effects on prices and quantities of substitutes.

5.1 Relationship of Results to Pre-Analysis Plan

We registered this trial on September 15, 2015 (before the data collection was completed) in the AEA registry. Our approach to analysis and the outcome variables we focus on in this paper closely mirror the project narrative we uploaded before we had access to any data. We highlight the most notable departures from the pre-analysis plan (PAP) here:

1. The experimental design section of the PAP mentions that the enforcement group would be

divided into two sub-groups: One in which vendors would receive only a warning letter about illegal behavior, and one in which we would follow that up with inspections and fines. In practice, *Sernapesca* officials did not implement the treatments any differently across these two sub-groups. So we do not report this sub-sample analysis. Our data show that vendor behavior was not statistically distinguishable across these sub-groups.

2. The PAP mentions our sample size as 153 circuits, based on information we had collected on the existence of fish markets by calling municipalities before launching the project. During data collection we learnt that 40 of those circuits did not have any fish-stalls. Mystery shoppers could not visit another 7 circuits for logistical reasons. Our final analysis sample therefore contains only 106 circuits. These two sources of attrition are not correlated with any observable characteristics, nor with the treatment assignment.

3. We had not anticipated that vendors would try to cheat by claiming that the fish was caught in August. This is something we learnt from our mystery shoppers soon after we started data collection. In the PAP, we mention only that we will track vendor reactions to enforcement activity, but do not mention 'freezing' specifically.

4. The PAP does not delve into the level of detail that this paper does. For example, we did not know exactly which fish were close substitutes for hake. We learnt from our data that pomfret was the other fish most commonly sold by hake vendors, and we therefore analyze effects on the price of pomfret. This price analysis could therefore be viewed as "exploratory" even though we had pre-specified our interest in studying price effects.

5.2 Balance

We did not conduct a full baseline survey, but had access to municipality administrative data and weather data with which we could check balance across treatment arms. The table C.4 in appendix C shows balance tests across the main treatment arms. Tables C.5 and C.6 also show balance tests with respect to the enforcement predictability and intensity (frequency) sub-treatments.

Overall, the various treatment arms appear well balanced in terms of important socio-economic and weather characteristics (e.g. poverty rate, rainfall). The joint test F-statistics of all variables are insignificant for different treatment arms. The delinquency rate (i.e., per-capita police cases for major offenses) is lower in municipalities assigned to receive the information campaign relative to the control group. The regressions reported below control for this variable, but we have verified that the reported treatment effects are not sensitive to adding this control.

5.3 Empirical Strategy

Mystery shoppers visited several stalls in each market multiple times during September 2015. These visits created a stall-day level panel dataset of 906 visits. The first visit to various markets by Sernapesca enforcement officers occurred between Sept 4 and 10, so our panel data consists of 242 visits during the pre-enforcement period that we define as the first week of September, plus 664 visits during the post-enforcement period (the rest of September). We use the following regression specification to evaluate the interventions, where each observation refers to a mystery shopper visit at fish stall s, in feria f, from circuit c visited on day t:

$$y_{sfct} = \beta_0 Post_t + \beta_1 T_c + \beta_2 T_c \times Post_t + \beta_3 y_{sfc0} + {X_{ct}}' \beta_4 + \varepsilon_{sfct} \tag{1}$$

y_{sfct} is the outcome variable, such as an indicator for whether illegal hake fish was available at that stall on that day. The treatment assignment (T_c) varies at the circuit level . The variable $Post_t$ indicates the post-intervention period, September 8-30.[11] We control for weather on each day, whether the inspector visited the market that day, a few socioeconomic covariates (e.g. municipality crime rate), randomization strata fixed effects, and the baseline (pre-intervention) value of the dependent variable. The error term, ε_{sfct}, is clustered at the circuit level, which was the unit of randomization. The coefficient of interest for the evaluation is the parameter β_2, which captures the difference between treatment and control groups during the post-intervention period. In most of our tables, we will only report these coefficients, and suppress all others.

To study consumer fish purchase behavior, we use surveys of consumers conducted at ferias. We use the following regression specification to evaluate the effect of interventions, where each observation refers to a to a single consumer i, surveyed in feria f, from circuit c:

[11] Many of the information campaign letters arrived at households even after September 8. There are other reasonable ways to define the post-intervention period, and we make a conservative choice. We have verified that the exact definition of the post intervention period does not affect our main results.

$$y_{ifc} = \gamma_1 T_c + X_{ic}{}'\Gamma + \epsilon_{ifc} \quad (2)$$

Where y_{ifc} is the outcome variable, e.g., number of times the consumer purchased hake fish in the past month. T_c is the treatment status at the circuit level, and X_{ic} represents a set of covariates, including socioeconomic variables at municipality level, individual information (usual fish consumption, age, gender, and household income) and strata fixed effects. Consumers are assigned treatment status based on the feria where they were interviewed.[12] Standard errors are clustered at the circuit level.

5.4 Hake Sales Observed by Secret Shoppers

Column 1 of Table 3 shows the effect of the interventions on whether fresh, visible hake was available for sale in that stall, as detected by mystery shoppers. Column 2 shows effects on whether hake in any form (fresh and visible, hidden in the back, or "frozen" hake that is kept on ice) was available for sale. Each dependent variable is binary, and we report marginal effects from a Probit regression. The three coefficients of interest are on the variables with a $\times Post_t$ interaction, which track the effects of the demand-side information campaign, the supply-side Enforcement treatment, or the interaction between the two (ferias where both supply and demand interventions were simultaneously administered), during the post-intervention period. These three coefficients summarize treatment effects relative to the control group, which is our omitted category.[13]

[12] While that is the only sensible choice for the enforcement treatment, we could have also used the person's address to link them to the information treatment. Results look very similar either way, and we have imperfect information on individual addresses, so we use the feria location.

[13] We randomized the Information Campaign over the subset of the 48 most populous municipalities in our sample (out of 70 total). We control for an indicator for these 48 municipalities in all our regressions. We have also run regressions restricting the analysis sample to these 48 municipalities, and the results look very similar.

Table 3: Treatment Effects on Hake Sales

VARIABLES	(1) Fresh, Visible Hake	(2) Any Hake Available (Hidden, Frozen, Visible)
Information Campaign Only	0.080	0.029
	(0.056)	(0.058)
Enforcement Only	0.114	0.092
	(0.070)	(0.060)
Information Campaign and Enforcement	0.078	0.100
	(0.070)	(0.065)
Information Campaign Only × Post	-0.133**	-0.131*
	(0.066)	(0.074)
Enforcement Only × Post	-0.178**	-0.130
	(0.082)	(0.089)
Info Campaign and Enforcement × Post	-0.179**	-0.139
	(0.074)	(0.094)
Change in Dep. Var. in Control Group		
During Intervention Period	-0.21	-0.36
N	901	901

This table reports the effect of each treatment arm on the availability of illegal hake fish. The variable Fresh Hake indicates when the hake was available fresh. Hake available indicates when was possible to buy fish in any form. The table reports marginal effects from a Probit regression. Other controls are included: municipality characteristics, strata fixed effects and the average level of the outcome variable in pre-intervention period. We control for pre-treatment values for the outcome variables in addition to the treatment indicator, because not all markets were visited in pre-intervention period. Robust standard errors clustered by circuit (the unit of randomization) in parentheses. *** p<0.01, ** p<0.05, * p<0.1

The first three rows indicate that there were no statistically significant differences between treatment and control groups during the pre-intervention period. As expected, significant differences between markets appear after the interventions are launched (after the first week of September). In column 1, vendors in markets exposed to the information campaign are 13.3 percentage points less likely to be selling fresh, visible hake relative to control group vendors.[14] This is quite a large effect,

[14]The "Information Campaign" group is a marker for circuits located in municipalities assigned to receive the High-Saturation Information Campaign, where the majority of neighborhoods were treated with the campaign. Appendix Table C.8 explains why we made this modeling choice. Our consumer survey data indicates that the majority (69%) of shoppers we found shopping at *ferias* located in "control" neighborhoods in high-saturation treatment municipalities resided in neighborhoods that were treated. It therefore makes more sense to code such *ferias* as 'treated' with the information campaign. Appendix C.5 shows the results of re-estimating the results in Tables 3, but reverting to coding ferias in control neighborhoods as not treated with information. The results are qualitatively similar. The high-saturation information treatment has significantly larger effects on hake sales than the low-saturation treatment.

considering that about 43% of vendors in control markets were selling hake before the interventions were launched. Vendors operating in markets where Sernapesca monitors visit to levy penalties become 17.8 percentage points less likely to sell fresh visible hake. The combination of the two treatments also produces a 17.9 percentage point decrease in hake availability, so there is no evidence that the information campaign complements the enforcement strategy to make it more effective.

When we add "hidden" and "frozen" hake to fresh/visible hake sales in column 2, to create a broader dependent variable that captures any type of hake sales, the treatment effects become smaller and lose statistical significance. Taken together, the two columns suggest that while the interventions reduced vendors' propensity to engage in illegal activity that could be easily monitored by regulators (visible sales in column 1), it is not so clear whether it actually reduced the underlying environmental harm that we care about (column 2). The magnitude of the *reduction* in the treatment effect moving from column 1 to 2 is larger for the enforcement treatment arms. The difference possibly stems from the defensive strategies that vendors adopt in response to the audits. We will explore those defensive strategies in greater detail in section 5.6.[15]

5.5 Consumer Behavior

We consider the mystery shopper data to provide the most reliable measure of the illegal behavior we track. Nevertheless, we also directly surveyed consumers at markets about their purchase behavior. This allows us to report results on the (self-declared) consumption of fish using consumer surveys conducted before and after the ban. The first column of Table 4 shows treatment effects on the number of times that consumers interviewed at the market report buying hake fish during the previous month. The reported coefficients are marginal effects from a Poisson regression, evaluated at the mean of all covariates. We see significant decreases in (self-reported) hake purchase across all treatment arms, and so results are generally consistent with the mystery shopper survey. However, in these consumer reports, the treatment effects appear larger in information campaign areas (relative to enforcement areas), and it decreases consumer hake purchases by 50% compared to the control group. This may be because the consumers received direct communication in the information areas, which may create some self-reporting bias.

[15] It is curious that the control group experienced larger reductions in "any hake" (column 2) than in "fresh, visible hake" (column 1). This is because a few control group vendors practiced freezing during the pre-intervention period (first week of September), but they stopped doing so after the interventions started. Apparently vendors in the control group learnt that there would *not* be much enforcement in their *ferias*, and reacted accordingly.

Table 4: Treatment Effects on Fish Consumption

	(1)	(2)
	Num. Times Hake	Mention Ban
VARIABLES	Purchased	(unprompted)
Information Campaign Only	-0.275***	0.146***
	(0.071)	(0.045)
Enforcement Only	-0.111**	0.082*
	(0.049)	(0.047)
Info Campaign and Enforcement	-0.098**	0.107**
	(0.046)	(0.051)
Mean Dep Var Control Group	0.49	0.07
N	3218	3319

This table presents the effect of different treatments on the reported consumption of hake fish during September 2015. The column 1 shows the marginal effects from a Poisson regression because the dependent variable is count data, the column 2 shows marginal effects from a Probit regression. Consumers were not asked about the ban, but surveyors registered if the ban was mentioned spontaneously. These regressions include socioeconomic characteristics and strata fixed effects. The numbers of observations in columns 1 and 2 differ because some consumers could not recall the number of times they purchased hake in the past month. Both Poisson and Probit are nonlinear models, and the average marginal effects of each treatment depend not only on the coefficients reported in this table, but also on the values of the covariates. Robust standard errors clustered by circuit in parentheses.*** p<0.01, ** p<0.05, * p<0.1

Consumer behavior was also indirectly influenced by the enforcement activity. Not only did self-reported hake purchases decrease there relative to control markets, the third column also shows that consumers were about twice as likely (or 8-11 percentage points more likely) to mention to our enumerators, totally unprompted, that they did not buy hake fish because there was a September ban in place. Our enumerators did not specifically ask consumers any questions that mentioned the ban, but were instructed to note down whenever a consumer spontaneously mentioned the ban. Consumers treated with the information campaign were 15 percentage points more likely to mention the September ban unprompted, so evidently the treatments were at least successful in spreading more information and awareness relative to control areas.

5.6 Vendor Reactions

We quickly learnt from our mystery shoppers that vendors undertake two major defensive strategies in an effort to continue selling illegal hake in September: they hide the hake, or they put the fish over ice and claim that it was caught legally in August, and frozen since then.

Hiding: Mystery shoppers are in principle able to observe a lot more than government inspectors. For example, they could ask vendors for hake even if it was not visibly on sale in the stall. The mystery shoppers noted down each occurrence of "hidden hake", but we never shared the specific vendor or feria identity with our government partners, so as to protect vendor privacy and abide by our research ethics (IRB) protocol. These data were very useful for the evaluation, but were never used to target enforcement.

Our mystery shoppers observed the practice of hiding in 3-4% of stalls in treatment areas.The hidden hake fish was often stored in a cooler behind the board that displayed the stall's fish prices. This was clearly used as a strategy to circumvent the September ban: We conducted another mystery shopper survey six months after the ban, and we did not observe even a single stall selling fish that was not publicly visible at that time. Hiding fish is costly for vendors, because displaying the fish available for sale on any given day and shouting out to potential customers are the main marketing tools at the vendors' disposal. Many of our mystery shopper noted down in survey instruments that they observed regular consumers asking vendors for hake when it was not visible. The hiding strategy evidently works because many consumers are willing to partake.

Freezing: On paper, vendors are not allowed to sell hake fish in any form in September. In practice, Sernapesca inspectors were more lenient with vendors who were detected selling "frozen" hake. This is the practice of putting the fish on ice and claiming that it was harvested in August, before the ban. We had not anticipated this reaction, but a couple of our mystery shoppers noted the practice for us early enough such that we were able to collect systematic data on it. Matching our mystery shopper data at the daily level to the administrative data on fines levied (from Sernapesca's registry of inspector visits) suggests that inspectors were much less likely to levy penalties when the vendor was claiming to sell "frozen" hake.

Selling frozen hake is quite costly for vendors because consumers prefer the taste of fresh fish, and because freezing requires freezers and access to electricity. Using our other rounds of data, we see that freezing is virtually non-existent during the rest of the year. So this does appear to be a strategy that vendors use to circumvent the September ban.

There are several pieces of circumstantial evidence in our data that this is all pretense; that fishermen and vendors are not actually protecting the environment by catching fish in August and freezing it until September. First, we document more freezing in the second half of September 2015 than during the first half (especially in markets inspectors visited on a predictable schedule), after vendors have had a chance to learn about the enhanced regulatory activities. Real freezing would have been much less costly to engage in during the first half of the month. Second, we collected data on stall characteristics, and availability of a freezer in a stall is not at all predictive of freezing. If anything, our mystery shoppers find that stalls without freezers are more likely to be selling frozen fish post-intervention. Third, many secret shoppers noted down that in their conversations with vendors, many vendors admitted (and even insisted) that the fish was fresh even though it was labeled as frozen.

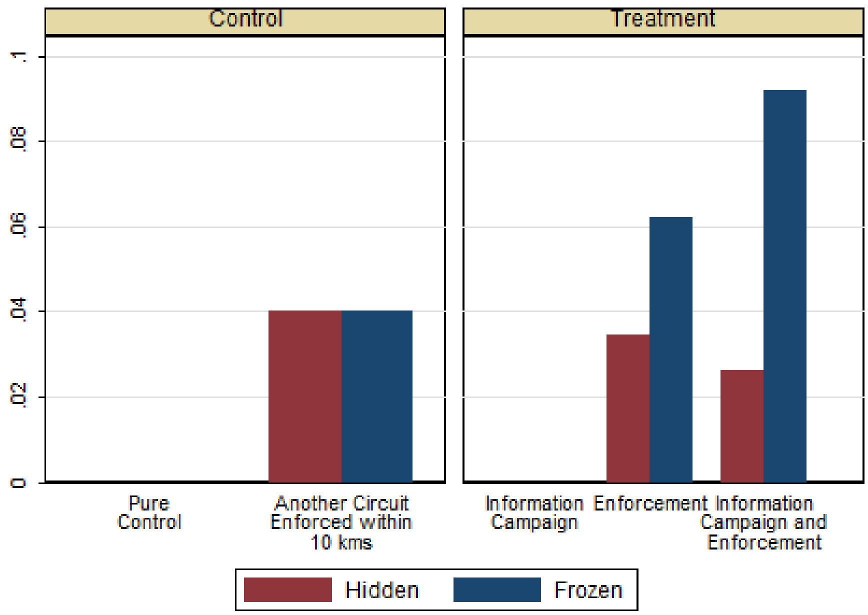
Figure 3 shows the prevalence of freezing and hiding across treatment groups. We divide up the control group into markets that have another circuit that is randomly assigned to enforcement within 10 kilometers (to capture any information spillovers), and *pure control* markets that are more than 10km away from any treated area. Several notable patterns emerge:

1. We do not observe any hiding or freezing at all in pure control markets. In contrast, 7.2% vendors operating in circuits that received Sernapesca inspector visits sell frozen fish (p-value <0.01), and 3.2% of those vendors engage in hidden hake sales (p-value 0.01).

2. Vendors operating in circuits exposed only to the information campaign did not engage in any hiding or freezing at all. It appears that vendors employ these defensive strategies only against Sernapesca inspectors, not informed consumers. Evidently there is something fundamentally different about targeting the demand side: The information campaign did not simply increase the vendors' perceived audit risk, or simply signal enhanced government attention to the problem. The consumers are an important independent actor whose knowledge affects vendor behavior.

3. 4% of vendors who operate in control markets - but located close to treated areas - engage in hiding and freezing, in contrast to 0% in *pure control* markets (p-value 0.02). There appear to be some spatial spillovers in information about Sernapesca visits, and in vendor behavior. We will explore these spillovers at greater depth in Section 6.

Figure 3: Hidden and Frozen Hake Fish



This figure shows the unconditional mean of hidden hake for different treatment status. The level of frozen hake is statistically different from zero for markets assigned to Enforcement and Enforcement and Info Campaign. The level of hidden is statistically different from zero for markets with Enforcement and spill-overs. Standard errors are not shown in the figure, but the accompanying text describes p-values of relevant comparisons.

The overall trend in our sample is that hake vendors who sell the "frozen" variety increases week-to-week all through September. Only 1.5% of those who sell hake choose to sell it in frozen form at the beginning of September. This proportion increases to 16% during Sept 3-9, 53% during the following week, continually increasing to 71% during the last week of September. Evidently vendors learn how to employ this strategy over the course of the month.

These hiding and freezing reactions suggest that vendors learn about Sernapesca inspector visits and penalties, and adapt to circumvent the enforcement. It is important to take such reactions into account when we are interested in comprehensively evaluating the longer-term consequences of policy. In our specific case, the enforcement strategy produced larger decreases in fresh hake sales compared to the information campaign (comparing point estimates in column 1 of table 3), but not once you take vendor adaptation into account (column 2). We next explore whether the specific design of the enforcement strategy can be altered to make it more robust to (anticipated and unanticipated) defensive strategies that vendors may employ. That analysis will also allow us to explore how agents learn about the regulator's activities.

5.7 Variations in the Design of the Enforcement Strategy

Varying the enforcement strategy involves experimentally manipulating the schedule of visits in two dimensions: Predictability and Frequency. Table 5 uses the mystery shopper data, and repeats the regression setup of Table 3, except that the enforcement treatment is now sub-divided into areas where the monitoring schedule was either predictable or unpredictable (panel A), or sub-divided into areas where monitoring was conducted at high versus low frequency (panel B).

Table 5: Treatment Effect on Hake Sales by Enforcement Strategy

	(1)	(2)
	Fresh,	Any Hake Available
VARIABLES	Visible Hake	(Fresh-Visible, Hidden or Frozen)
Panel A: Variation in Predictability of Enforcement Schedule		
Information Campaign only	-0.122*	-0.134*
	(0.068)	(0.073)
Enforcement on Predictable Schedule	-0.092	-0.060
	(0.069)	(0.083)
Enforcement on Unpredictable Schedule	-0.243***	-0.192**
	(0.086)	(0.094)
p-value of Predictable = Unpredictable Sch.	0.011	0.036
Panel B: Variation in Frequency of Enforcement Visits		
Information Campaign only	-0.123*	-0.135*
	(0.067)	(0.072)
High Intensity Enforcement	-0.063	-0.070
	(0.086)	(0.095)
Low Intensity Enforcement	-0.198**	-0.162*
	(0.083)	(0.090)
p-value of Low = High Int. Enf.	0.059	0.280
Change in Dep Var in Control		
During Intervention	-0.21	-0.36
N	901	901

This table presents the coefficient corresponding to the interaction term $T_c \times Post_t$ for each treatment. To retain statistical power, the cells "Enforcement only" and "Enforcement + Info Campaign" from Table 3 are combined under "Enforcement" and then sub-divided by schedule predictability (panel A), or intensity (panel B). So these coefficients should be interpreted as the average effects of enforcement when half the sample is also exposed to the information campaign. Panel A includes a dummy for the intensity sub-treatment, and Panel B includes a dummy for the predictability sub-treatment, but those coefficients are not shown. Each regression controls for the dependent variable in pre-intervention period, strata fixed effects and municipality characteristics. Probit regression marginal effects are reported. Robust standard errors clustered by circuit in parentheses.*** p<0.01, ** p<0.05, * p<0.1

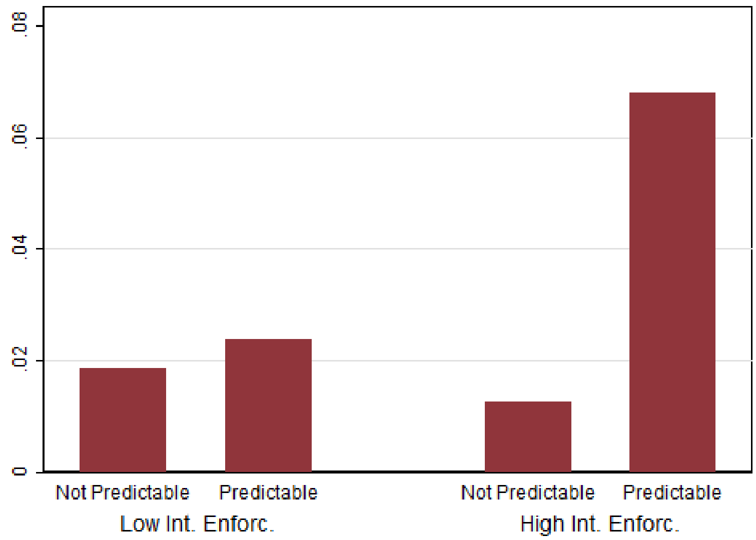
Panel A shows that the enforcement strategy was more effective when it was unpredictable. When enforcement follows a predictable schedule (e.g. every Tuesday at 10am), its effect is not statistically different than zero. However, when we make the monitoring visits difficult for vendors to predict, we see two important changes: (a) There is a much larger and statistically significant decrease of 24% in vendors' propensity to sell fresh, visible hake, and (b) There remains a statistically significant decrease of 19% in the availability of any type of hake even after we account for vendor defensive reactions like hiding and freezing. The effect of the unpredictable schedule is statistically significantly larger than predictable enforcement. The lack of predictability apparently makes it difficult for vendors to effectively engage in strategies like hiding and freezing. We will examine this directly below using data on the prevalence of hiding and freezing across these sub-treatment arms.

Panel B shows results separately for the subgroup of vendors who received monitoring visits once a week (low intensity), and other vendors who were visited twice a week, which means that monitors followed a circuit around in the different market locations where those vendors set up stalls on different days of the week (high intensity). The high frequency visits in principle limit opportunities for spatial and temporal displacement of illegal hake sales. The strategy of devoting additional resources to enforce at high frequency backfired. Enforcement is more effective at reducing hake availability in markets that were visited less frequently. The 14 percentage point gap between low and high intensity in column 1 is statistically significant, but the 9 percentage point larger effect in column 2 is not.

5.8 Learning

One possible explanation for this pattern of results is that more frequent interactions with the inspectors allow vendors to learn about the loopholes of the enforcement strategy more quickly, and how to subvert the audits. We explore this in figure 4 using data on the prevalence of "hidden" hake sales. Hiding is about 3.5 times as common under the high-frequency/predictable enforcement regime than in every other cell ($p-value = 0.01$). Making many monitoring visits on a predictable schedule allows vendors to learn most about the monitoring schedule, and evidently makes it much easier for them to evade enforcement.

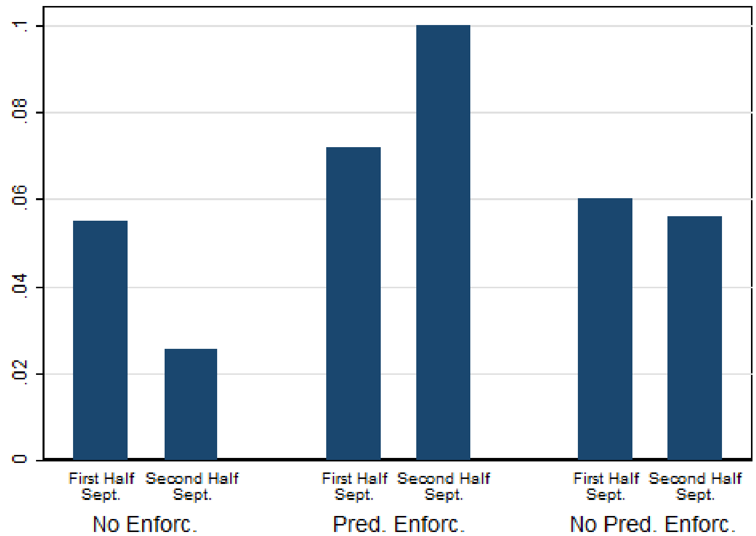
Figure 4: Hidden Hake by Enforcement Schedule



This figure shows the unconditional mean of hidden hake for different sub-groups of enforcement for post-intervention period. The difference between the fourth column and the rest is statistically significant at 1%.

Both hiding *and* freezing are more common when we follow a predictable monitoring schedule. To investigate whether vendors "learn" to cheat after they experience monitoring visits, Figure 5 plots the incidence of freezing separately for the first half of September and the second half. In markets with no enforcement activity, the incidence of frozen fish drops from the first to the second half of the month. This would be consistent with the biology and economics of actual freezing - it's easier and cheaper to freeze August-caught fish for 7 days rather than 20 days. However, in markets inspectors monitored on a predictable schedule, not only is the incidence of freezing higher than under no-monitoring or under unpredictable monitoring, freezing becomes more common in the second half of the month (10%) than in the first half (7.2%).

Figure 5: Frozen Hake by Schedule of Enforcement



This figure shows the unconditional mean of frozen hake for different sub-groups of enforcement for the first and the second half of the month. Markets with predictable enforcement increased the proportion of frozen hake over time.

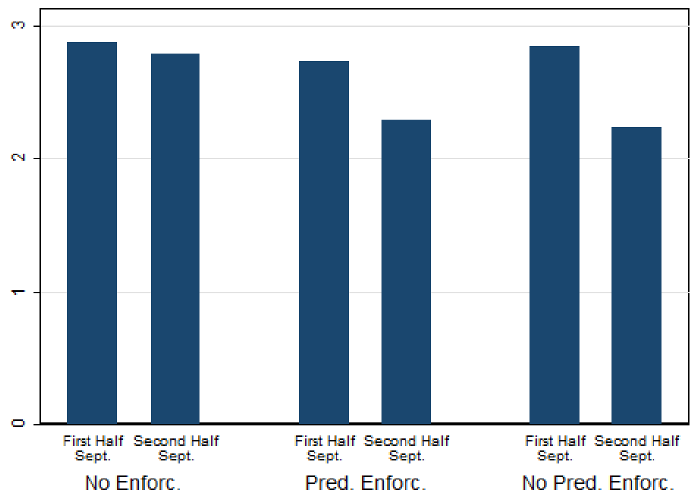
In summary, we see evidence vendors more easily cheat the inspectors under predictable monitoring, and there is some suggestive evidence that learning how to cheat over time is easier when the monitoring schedule is predictable. The learning occurs quite quickly, within the course of September. All of this results in enforcement being much more effective at reducing illegal hake sales - even after accounting for the cheating - when the inspectors visit to levy fines using a random, unpredictable schedule.

5.9 Change in Number of Stalls Selling Fish

Even though most fish vendors sell multiple types of fish, one possible effect of our intervention is that some fish vendors exit the market altogether during September, when they find it difficult to sell hake. This would lead to selection in whom we observe in our follow-up data. Figure 6 shows that the average number of fish stalls does decrease in the markets randomly assigned to the enforcement treatment, especially during the second half of September. This itself is an important effect of the treatment, but it also changes the interpretation of the treatment effect on the propensity to sell hake reported elsewhere in the paper. This finding indicates that some vendors facing enforcement drop to zero sales, but this is not captured in Table 3. That makes the coefficients reported in that table possibly under-estimates of the true effects.

How large an under-estimate it is depends on how likely it is that those "missing" stalls would otherwise be selling hake. If the stalls that exit are in markets where hake is not usually sold anyway, then correcting our estimates for these "missing" stalls would not change our results substantially. Table C.7 in Appendix C.3 describes how we correct our estimates for stalls exiting. The correction makes the effect of Enforcement larger than that reported in Table 3, but it does not affect the coefficients for other treatments very much.

Figure 6: Number of stalls in Feria by Treatment Assignment



This figure shows the average number of stalls in each feria, separately for the first and the second half of the month. Markets assigned to receive enforcement showed a decrease in the number of stalls between the first and the second half of the month.

6 Spillovers and Market Level Effects

While our experiment was targeted to reduce hake sales in treated *ferias*, it may have had spillover effects on control markets through information transmission, or by changing equilibrium prices (Blattman et al., 2017). It may also have affected the behaviors of other market actors, such as the fishermen who supply to vendors. It could have also changed the prices and quantities of other fish that can act as substitutes for hake. We collected additional data to study these spillovers and equilibrium effects, including a survey of fishermen, a survey of vendors to understand their social and supply-chain connections to vendors operating in other markets, GIS data on the location of all markets, and data on the prices of hake and other substitute fish. The vendor and fishermen surveys allow us to map the supply chain for each of the ferias in our sample. The geography of Chile (with a very long coast) creates large spatial variation in the locations of *ferias* where vendors sell and *caletas* where the fishermen bring in their catch, which in turn produces variation in geographic and social connections between different market actors.

6.1 Spillovers on Control Markets

We identified three primary channels through which treatment may affect behavior of control markets, and collected data on each channel:

1. *Spatial spillover:* Control markets located geographically close to a treated market may feel the effects of treatment because they share consumers with the treated area.
2. *Social spillover:* If control market vendors are socially connected to vendors operating in treatment areas, they may be more likely to learn about *Sernapesca*'s enforcement activities.
3. *Supply chain spillover:* Treatment and control vendors may source from the same fishermen. If a supplier changes fishing behavior due to treatment, that could indirectly affect fish sales in control markets.

Of these different channels, an increase in fish sales in control markets due to indirect effects is of greatest econometric concern. If fishermen dump all excess hake in control markets when vendors in treated markets are unwilling to buy hake, then the treatment-control difference will appear to show that the treatment was effective, when in fact hake sales were simply spatially displaced towards the control group. Our regressions would over-estimate the effects of treatment in that scenario. This is why it's important for us to re-investigate these effects controlling for these sources of spillovers.

In Table 6, we re-estimate our main results on the effects of predictable and un-predictable enforcement originally reported in Table 5, but now controlling for potential channels of spillover effects.[16]

[16] We follow a procedure similar to Miguel and Kremer (2004) in estimating treatment effects in the presence of spillovers. We divide the control markets into subgroups; (a) Control areas that are more likely to have been affected by treatment due to geographic or social or supply chain connections, which we call "Spillover Group", and (b) Control areas un-connected to treatment markets, which we call "Pure Control". Note that sub-dividing the control group this way reduces the number of markets allocated to the omitted category. To retain sufficient statistical power, we therefore focus on re-estimating the effects of enforcement treatment variations only, because spillovers cause the greatest econometric concern (of over-estimating treatment effects) for this particular result. In this setup, some of the markets in the omitted category received the information treatment, so the regression coefficients will look a little smaller in this table compared to Table 5. For the same statistical power reasons, we only study an overall spillover effect of enforcement, and do not try to estimate separate sub-treatment spillovers.

We do not have a lot of statistical power to detect spillovers, so we will focus on establishing that our main treatment effects are retained (in fact, gets a little stronger) after controlling for spillovers. The spillovers themselves are only suggestive and statistically imprecise.

The first column presents results without spillover variables, and shows that relative to all other markets, unpredictable enforcement reduces hake availability by 15.7 percentage points, and is marginally significant ($p < 0.10$). The second column controls for spatial spillovers, with the indicator "within 10km of Treated Market" turning on for untreated markets that have at least one treated *feria* within a distance of 10 kilometers.[17] [18] The coefficient of this variable is negative but small and statistically indistinguishable from zero, suggesting very limited spillovers based on shared consumers due to geographic proximity. The third column includes an indicator for control markets where at least one vendor reported that they knew a vendor in a different market that was randomly assigned to the enforcement treatment. The coefficient on this variable suggests that there was a 7 percentage reduction in hake availability in markets experiencing this "social spillover", but this effect cannot be statistically distinguished from a zero effect with any confidence. Controlling for this form of spillover increases the effect of unpredictable enforcement to a 19.9 percentage point reduction in hake availability ($p < 0.05$). Finally, column 4 includes an indicator for control markets who source from fishermen operating out of *caletas* that primarily supply to other markets that were assigned to the enforcement treatment. We again see a 7.7 percentage point reduction in vendors' propensity to sell hake in control markets that are connected to treated markets through shared suppliers, but the effect is not statistically precise.

Importantly, accounting for these spillover effects make the main treatment effects of unpredictable enforcement on enforced areas a little larger and more statistically precise. This is because controlling for spillovers allow us to compare treated areas to the subset of "pure" control areas unaffected by the treatment. Relative to such pure control markets, the unpredictable enforcement reduces hake sales by 18-20 percentage points (as opposed to 16 pp estimated in col. 1).

[17]Using the 10 km radius evenly divides the control group into "pure control" and "spill-over market", and therefore maximizes statistical power. Alternative definitions produce similar results.

[18]Vendors connected to a larger number of other circuits are more prone to being exposed to the treatment, and that variation is not random. To control for this, we include a full set of dummy variables for the number of other circuits that each reference circuit is connected to, separately for spatial, social and supply-chain connections. Thus, the variation of exposure to spillovers stems only from the treatment status of other markets, which is exogenous because it was randomly assigned. (Miguel and Kremer, 2004).

Table 6: Treatment Effects on Hake Sales Controlling for Spillovers to Control Markets

VARIABLES	(1) Any Hake (Fresh/Hidden/Frozen)	(2) Any Hake (Fresh/Hidden/Frozen)	(3) Any Hake (Fresh/Hidden/Frozen)	(4) Any Hake (Fresh/Hidden/Frozen)
Enforcement on Predictable Schedule	-0.023	-0.030	-0.076	-0.058
	(0.083)	(0.069)	(0.080)	(0.060)
Enforcement on Unpredictable Schedule	-0.157*	-0.167*	-0.199**	-0.177**
	(0.091)	(0.075)	(0.084)	(0.084)
Spatial Spillover		-0.017		
(within 10 km of Treated market)		(0.082)		
Social Connection Spill-over			-0.071	
(Vendor knows a Treated Vendor)			(0.076)	
Supply-Chain Spill-over				-0.077
(Sources from same *Caleta* as Treated Vendor)				(0.081)
Change in Dep Var in Control During Intervention	-0.36	-0.36	-0.36	-0.36
N	901	901	901	901

This table re-estimates treatment effects controlling for possible spillover effects from treatment to control markets. We focus on enforcement treatments to ensure that the control cell size is large enough to be divided by exposure to spill-overs. We only present the coefficient corresponding to the interaction term $T_c \times Post_t$ for each treatment. Controls for $T_c, Post_t$, covariates, and baseline value of the dependent variable are included, but those coefficients are not shown. The table reports marginal effects from a Probit regression. The dependent variable is an indicator for any type of hake (fresh-visible, hidden or frozen) for sale in the stall. Robust standard errors are clustered by circuit, which was the unit of randomization. *** p<0.01, ** p<0.05, * p<0.1

6.2 Treatment Effect Transmission along the Supply Chain

For the supply chain spillover channel to be relevant, the fishermen supplying hake to these vendors must have altered their behavior in some way. To understand those changes, we directly survey fishermen operating out of every *caleta* (fishing village) that serves the markets in our sample.[19] The reactions of fishermen are particularly important to track because our interventions conducted at the final point-of-sale has to somehow get transmitted up the supply chain to fishermen, for these interventions to ultimately protect the hake population. Only if fishermen start perceiving the effects of these interventions on demand conditions will they change fishing behavior in ways that improve environmental outcomes that the policymaker cares most about.

Since we did not have baseline data from fishermen for years preceding the September 2015 ban, we ask them retrospective questions in 2016, in which the fishermen are asked to compare demand and profits during September 2015 (when our interventions were launched) relative to September 2014. To minimize possible response bias given the government fishing ban, we were careful to phrase our questions generically, to cover revenues earned from all types of fish, and not just hake specifically.

[19] A few caletas in the regions covered by our sampling frame are only used by divers who harvest seafood, not fish -and we therefore exclude those *caletas*.

Retrospective answers may be subject to recall bias, but since these fishermen were not directly treated, it is less likely that the recall bias is correlated with treatment assignment. To report treatment effects on fishermen, we have to connect each *caleta* to treatment and control markets. We use the vendor survey on the structure of the supply chain -i.e. which caletas each vendor buys from - to link fishermen to the randomized treatments.

Table 7 reports results. Column 1 shows that fishermen operating out of *caletas* that sell to at least one circuit which had been randomly assigned to enforcement, are 24 percentage points more likely to report that they earned less in September 2015 compared to September 2014, relative to fishermen in caletas that supply to control group *ferias*.[20] Fishermen operating out of caletas that supply to both enforced markets and to markets that experienced the information campaign were 36 percentage points more likely to report lower revenues during the month of the interventions, compared to the same month in the previous year. So our interventions not only affected the behavior of vendors, as observed by our mystery shoppers, those changes in behavior were also perceived by fishermen further upstream in the fish supply chain. Column 2 shows that these fishermen are more likely to report that vendors were less willing to buy hake in September 2015 compared to the previous year, but this result is marginally significant with ($p < 0.10$). Column 3 shows suggestive evidence ($p < 0.10$) that fishermen linked to the information campaign areas are more likely to report that final consumers are aware of the hake ban.

[20] We could instead define exposure based on the proportion of circuits enforced, and results look similar. The "at least one" formulation is attractive because this indicator evenly divides the sample into equal halves.

Table 7: Treatment Effect Transmission to Fishermen in Caletas

VARIABLES	(1) Earned Less in Sept 15 than Sept 14	(2) Feria Vendors buy less Hake in Sep15 compared to Sept 14	(3) Consumers are informed of Hake Ban
At least one circuit Enforced	0.238**	0.169	-0.033
	(0.105)	(0.293)	(0.147)
Info Campaign	0.043	-0.101	0.343*
	(0.158)	(0.322)	(0.186)
At least one circuit Enforced and Info Campaign	0.358***	0.553*	0.173
	(0.128)	(0.315)	(0.195)
Mean Dep Var Control Group	0.31	0.40	0.77
N	202	179	217

This table reports OLS coefficients based on fishermen responses. The variable Information campaign correspond to caletas located in municipalities assigned to receive any level of information campaign. The variable "At least one circuit enforced" considers all circuits located in the same municipality of the caleta. Socioeconomic variables of the caletas are included as covariates. In average, three fishermen were surveyed in each caleta. The numbers of observations in columns 1, 2 and 3 differ because some fishermen could not recall the earnings and vendor behavior in specific months. Robust standard errors clustered at caleta level in parentheses. *** p<0.01, ** p<0.05, * p<0.1

6.3 Market Level Effects

In this section we attempt to describe how the market for fish in the aggregate may have been affected by our treatments. We collected data on prices and availability of other fish species in the same markets where hake is sold. The September ban is only specific to hake fish, so we might expect consumers to substitute to other fish varieties. This may be because informed consumers choose to avoid hake fish during the ban, or because the enforcement treatment reduces hake availability or increases its price.

The universe of data from all markets suggests that there are seven possible fish substitutes for hake,[21] but a typical stall only offers two or three varieties of fish. Table C.2 in the Appendix describes the availability and price of different fish species observed by mystery shoppers in ferias during September 2015. The most common fish substitute is pomfret, which can be found in two-thirds of all markets. Pomfret is larger and (arguably) more tasty than hake fish and is not over-exploited. In Table 8, we study the availability of pompfret (column 1), or any other non-hake fish including pomfret (column 2), as a function of the treatment status of the market where the fish stall is located.

[21] They are pomfret, mackerel, silverside, salmon, sawfish, albacore and southern hake. Of these substitutes, the southern hake is the only one with a similar ban, but in August. The southern hake is considerably larger than the common hake and is harvested in the southern regions of the country, without any geographical overlap with the common hake. More details are available in Subpesca (2015).

Table 8: Do Vendors Substitute to Selling Other Fish in Response to Treatment?

	(1)	(2)
VARIABLES	Pomfret Available	Any Other Fish Available
Information Campaign Only	0.146	0.004
	(0.098)	(0.035)
Enforcement on Predictable Schedule	0.133*	0.027
	(0.079)	(0.031)
Enforcement on Unpredictable Schedule	0.115	0.065*
	(0.078)	(0.033)
Change in Dep Var in Control Markets		
During Intervention	0.29	0.09
N	901	6328

The table reports marginal effects from a Probit regression. The unit of observation in the first column is stall x secret shopper visit, and in the second column is stall × secret shopper visit × possible substitute fish variety. We only present the coefficient corresponding to the interaction term $T_c \times Post_t$ for each treatment. Controls for $T_c, Post_t$, covariates, and baseline value of the dependent variable are included, but those coefficients are not shown. Robust standard errors are clustered by circuit in parentheses. *** p<0.01, ** p<0.05, * p<0.1

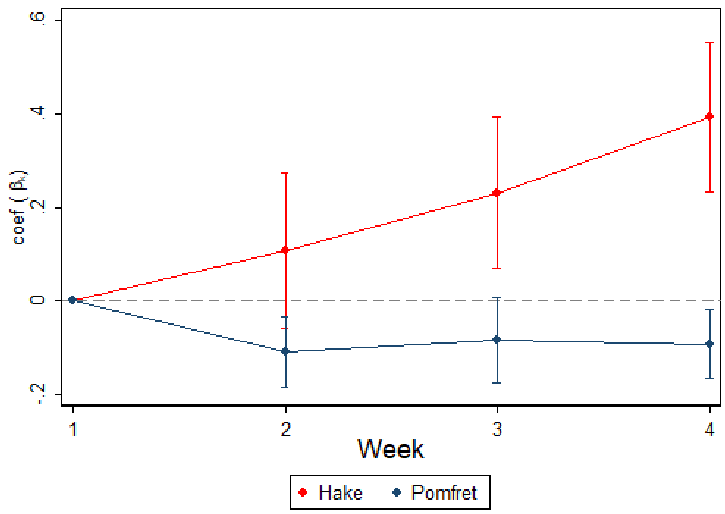
The penultimate row of the table indicates that stalls in control markets are 29 percentage points more likely to start selling pomfret during the September hake ban, so it appears that vendors in general move towards substitutes during the ban. The increase in pomfret sales during September is larger in treated areas (by a further 12-15 percentage points, which results in a 41-44 percentage point increase during the hake ban), but the treatment-control differences are barely statistically significant. The p-value for only one of the three coefficients (associated with Predictable Enforcement) is below 0.10. Column 2 investigates treatment effects on the vendor's decision to offer each of seven different fish substitutes for hake. The sample size is larger in this regression because selling each fish variety is treated as a separate decision, but our standard errors are still clustered by the unit of randomization of the treatment (the circuit). The coefficients indicate that vendors who faced unpredictable enforcement become 15.5 percentage more likely to switch to selling other fish during the hake ban, compared to the 9 percentage point increase in control markets. This 6.5 percentage point treatment-control difference is statistically significant (p=0.051).

6.4 Effects on Prices

We collected data on fish prices during all our mystery shopper visits. However, prices are observed only when the fish is available for sale. Indeed, during September, hake is only available in 26% of markets, which implies that the price of fish can be analyzed using a relatively small sample of observations. Further, our earlier analysis indicates that treatment changes the propensity to sell illegal hake fish. In other words, treatment affects the selection of which prices are observed. There are therefore large sample-selection issues that complicates any analysis of treatment effects on prices, and we refrain from running regressions on the price of hake. The most consumed fish during September (and second most consumed fish during the rest of the year) is Pomfret, which is available in 68% of the stalls (see Appendix Table C.2). Since pomfret is more often available (and not banned), we instead run regressions to study treatment effects on the price of pomfret.

As a descriptive exercise, Figure 7 shows that the price of hake increased week-to-week in September, over the course of the ban period. Pomfret prices fell by 10% in the second week and that lower price remained stable thereafter. This time-series pattern in prices is consistent with fishermen upstream in the supply chain shifting away from hake and towards catching pomfret during our interventions in September 2015. Through conversations with fishermen during our survey, we learnt that they are able to adjust their fishing strategy to target different species if there are market signals that hake demand is low. To do so, they change the location and depth at which their nets are dropped.

Figure 7: Log Prices of Fish During the Ban



This figure shows the evolution of log prices for hake and pomfret, using the first week as a reference. The price of hake continuously increased over the course of September 2015. Hake was 40% more expensive by the fourth week relative to the first week. The price of pomfret decreased around 10% after the first week.

Table 9 shows pomfret prices observed in a stall as a function of our randomized treatments assigned to the market where that stall is (column 1), as well as the price of any other substitute fish including pomfret (column 2). We find that the price of substitutes weakly increase ($p - value < 0.1$) as a result of the information campaign discouraging hake consumption, suggesting that part of the demand for hake shifted towards substitutes. Relative to the control group, markets that received enforcement show small and insignificant price decrease. The fact that we observe such price effects suggests that fish markets are at least somewhat segmented.

Table 9: Treatment Effect on Fish Prices

	(1)	(2)
VARIABLES	Log Price Pomfret	Log Price Substitute
Information Campaign Only	0.210*	0.140
	(0.109)	(0.096)
Enforcement Only	-0.017	-0.021
	(0.066)	(0.055)
Info Campaign and Enforcement	0.081	0.047
	(0.065)	(0.059)
Change in Dep Var in Control		
During Intervention	-0.20	-0.27
N	614	939

The table reports treatment effects on hake substitutes' price from OLS regressions. The outcome variable is the log of price per kilo. The unit of observation in the first column is *stall with pomfret available* × *secret shopper visit*, and in the second column is *stall with any substitute available* × *secret shopper visit* × *substitute available fish variety.* We only present the coefficient corresponding to the interaction term $T_c \times Post_t$ for each treatment. Controls for T_c, $Post_t$, covariates, and baseline value of the dependent variable are included, but those coefficients are not shown. Robust standard errors are clustered by circuit in parentheses. *** p<0.01, ** p<0.05, * p<0.1

7 Relative Cost-Effectiveness of Enforcement vs. Information

We conducted an information campaign in addition to the enforcement activities because our regulatory agency partner, *Sernapesca*, believed that demand-side strategies, if they work, would be more cost-effective and easier for them to implement in the future. Given the complications associated with enforcing regulations documented in this paper, and the complexity of designing regulations that are robust to unanticipated defensive reactions from enforced agents, it is useful to determine how cost-effective the enforcement strategies were relative to an information campaign. We collected data from *Sernapesca* on the full administrative costs of implementing each treatment, so that we can report on the relative cost-effectiveness of enforcement and information strategies. This allows us to report on the best use of (limited) public resources to protect hake populations.

We define effectiveness of our interventions on the basis of our data on the reduced probability of observing hake sales in treated ferias, accounting for hidden and frozen fish. Since the fish sold in ferias comes directly from fishermen villages and was harvested the same day or the day before, we assume that reduced hake sales directly correlates with the decrease in hake fishing in caletas. This assumption is supported by the fact that our interventions were conducted at scale covering all major markets where hake is sold in the sampling regions. This means that our data are net of "leakages" of hake from our sampling areas. The fishermen survey results we report in Section 6.2 also suggests that fishermen did feel the effects of the interventions.

Table 10: Cost-Effectiveness Analysis

	(1) Reduction of Hake Sale	(2) Units of Hake Saved	(3) Implementation Costs (USD)	(4) Cost of Saving One Hake (USD)
Enforcement (*Overall*)	0.13	10,399	$ 62,900.25	$ 6.05
Unpredictable	0.192	15,358	$ 69,190.27	$ 4.51
Predictable	0.06	4,799	$ 62,900.25	$ 13.11
Low Intensity	0.162	12,959	$ 53,475.84	$ 4.13
High Intensity	0.07	5,599	$ 99,613.61	$ 17.79
Info Campaign	0.13	3,257	$ 16,213.53	$ 4.98

This table shows the benefits and costs of implementing each intervention. Column (1) reports the estimated effects (in percentage points) of treatments in the sale of any type of hake. Column (2) is computed based on the numbers of stall per feria, number of days a week the feria operate and number of fish available in a normal stall. Column (3) is reported by Sernapesca and represents a combination of fixed and variable costs. Finally, column (4) correspond to the ration of (3) over (2).

In Table 10, we conduct the relative cost-effectiveness analysis by taking our best estimates of the effects of treatments on reduction in hake sales and combining it with an estimate of the number of fish available in the market that we compute using the data we collected from vendors. This allows us to create an estimate of the extra hake fish that are "saved" due to these treatments. Methodological details underlying these calculations are in Appendix Section D.

We compare this number with the cost of implementing each intervention to compute how much it cost to save each fish, under the different assignments. Sernapesca's prior was correct that overall, the information campaign appears more cost effective than the enforcement strategy. This is partly because enforcement becomes less effective as vendors learn to hide and freeze fish and circumvent regulation. Enforcement costs US$6.05 per saved fish, compared to $4.98 under the information campaign.

However, once we examine specific versions of the enforcement strategy that were more successful at curbing hake sales, we see that sending monitors on an unpredictable schedule is an even more cost effective way to protect hake, even after accounting for the fact that unpredictable monitoring schedules were more costly for *Sernapesca* to maintain because it required slack personnel capacity. The cost of "saving" a hake via unpredictable enforcement drops to $4.51. Not surprisingly, low-intensity enforcement (i.e. a less frequent monitoring schedule) is most cost-effective (only $4.13 per saved hake) because it was both more effective at reducing hake sale than high-intensity enforcement, and it was obviously also cheaper to implement. Predictable and high-frequency audits were total policy failures in that they were 250-400% too expensive per hake saved, given the subversive adaptation by hake vendors.

These calculations are useful to gauge the *relative* cost-effectiveness of alternative strategies to protect hake, but it does not tell us whether any of these strategies would pass a cost-benefit test. Sophisticated benefit calculation would require us to take a stance on the biology of hake fish (how saving a hake in September 2015 translates into a dynamic effect on the hake population via reproduction), and the ecological value of protecting hake. These considerations are outside the scope of our analysis, but our results can be easily combined with benefit numbers from ecology studies. The analysis in this paper takes the government's regulatory goal ("Protect hake fish") as given, and studies the consequences of enforcing that regulation, and analyzes the best ways to achieve that goal.

8 Conclusion

Research in many fields of applied microeconomics evaluate the effects of new regulations, such as anti-corruption campaigns, fines for non-compliance with health, hygiene or environmental standards, or penalties for tax evaders. The effectiveness of such policies depend on the (sometimes unanticipated) reactions of the regulated agents to the new enforcement regime, which is in essence a micro version of the "Lucas critique" (Lucas, 1976). Agents adapt once they have had a chance to learn about the new rules, and may discover new methods to circumvent the rules. This paper presents a research strategy - composed of an experimental design and creative data collection - that permits an investigation of the effects of regulation net of agent adaptive behaviors.[22] This research approach should be broadly

[22] An alternative evaluation strategy would be to collect data in the short run before agents have an opportunity to react to the new regime, and in the long-run after they have reacted. This is more expensive, requires more time, and fundamentally more difficult, because researchers do not always know when and how agents would learn and adapt.

useful for policy evaluation whenever agents can adapt to circumvent enforcement. As one important example, such concerns were first-order in the design of the Dodd-Frank Wall Street Reform and Consumer Protection Act in 2010 following the global financial crisis. Smith and Muñiz-Fraticelli (2013) write about this regulatory effort:

> "[A] major problem with the new financial legislation is that it is responsive to past market innovations without being sensitive to future innovations...The problem is that these actors will not always behave in a predictable way. That is the genius of financial innovation; the market always looks for new opportunities for profit, and, as the dawn follows the dark, mischief may arise."

Our experimental variations that change the specific attributes of enforcement policy yield novel empirical insights about the behavior of regulated agents, and how to better design policy accounting for their adaptation. Data collected via mystery shoppers help us identify the ways in which agents exploit loopholes to continue selling fish illegally. As a result, the standard measure of illegal activity (sales of fresh, visible hake) overstates the true effect of enforcement. Monitoring on an unpredictable schedule makes it more difficult for agents to circumvent enforcement. This, in turn, makes that strategy the most cost-effective way to reduce hake sales even though it is more expensive to implement. In contrast, a high frequency monitoring schedule produces a counter-intuitive result – but one predicted by economic theory incorporating dynamic responses (Okat, 2016) – it allows vendors to learn the regulators' strategies faster, and more effectively cheat, thereby undermining enforcement efforts.

We use multiple surveys of different market actors to document that these interventions travel downstream to affect consumer behavior and travel upstream to affect the behavior of fishermen who supply to vendors. Our investigation of vendor reactions through mystery shoppers, spillover effects on other market actors, and benchmarking these results against the effects of an information campaign, all combine to produce a comprehensive evaluation of an important environmental program.

Ultimately we learn that without sophisticated design-thinking, attempts at enforcement can backfire. Designing and implementing a consumer information campaign is a much less complex task, it leverages consumer ethics (Hainmueller et al., 2015), and many regulators may rationally choose to proceed with such simpler approaches. After observing the results of this evaluation, the Chilean government decided to scale-up the information campaign during the 2016 ban on hake fish sales, and conduct similar information campaigns for fishing bans for three other species.[23] While the unpredictable, low-frequency monitoring proved to be the single-most cost-effective strategy in our evaluation, the government correctly surmised that vendors may have other second and third order subversive adaptations to audits in the long run. In contrast to an enforcement strategy which may need to be constantly revised in response to regulated agents' adaptation, the information campaign is easier to replicate and scale, especially once the government has already incurred the fixed costs of developing campaign materials.

[23] See https://www.povertyactionlab.org/sites/default/files/documents/creating-a-culture-of-evidence-use-lessons-from-jpal-govt-partnerships-in-latin-america_english.pdf

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A Appendix Figures on the Research Context

A.1 Fishermen Villages

Figure A.1: Fishermen Village (Caleta)

A.2 Outdoor Markets

Figure A.2: Examples of Ferias

Figure A.3: Example of a Circuit

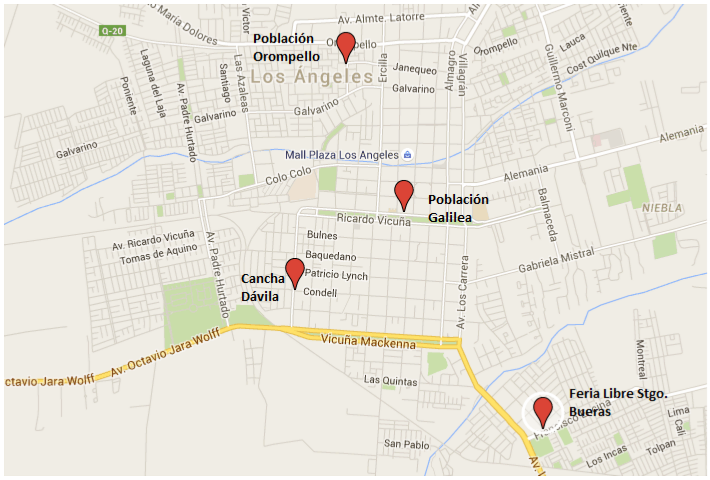
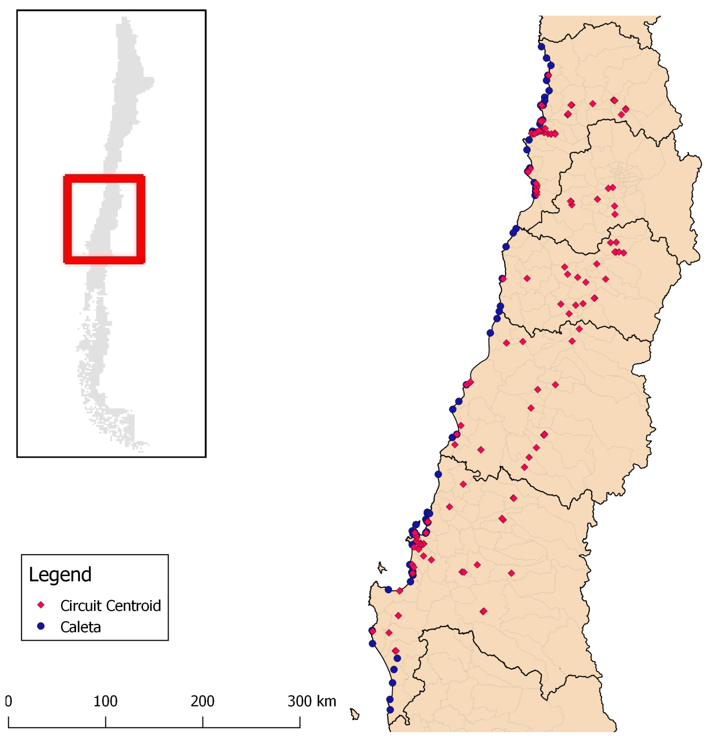


Figure A.3 maps the four ferias that compound one circuit of the city of Los Angeles, VII region.

Figure A.4: Map of Circuits and Caletas

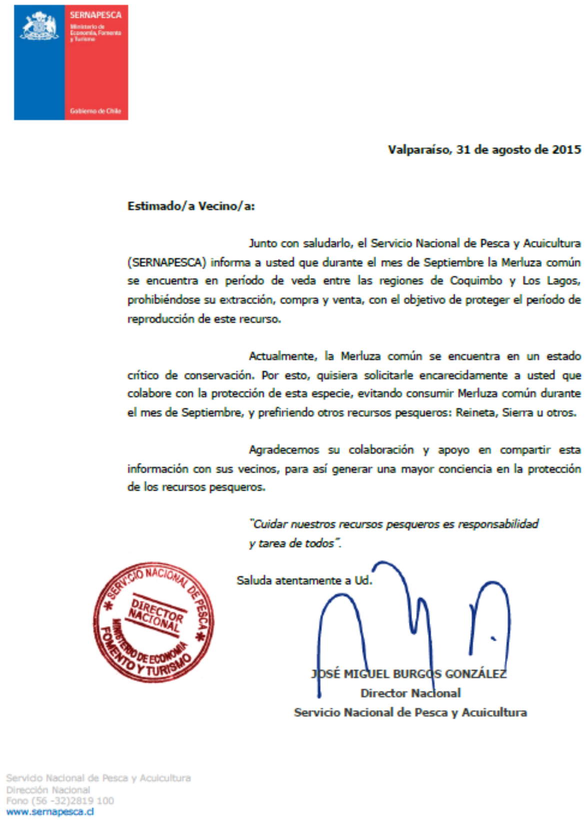


B Interventions

Figure B.1: Flyers

The figure B.1 shows the two types of flyers distributed during the ban period. The message of the one in the right says, “In September respect the Ban”, the one in the right says “This month respect the Ban”.

Figure B.2: Letter to Consumers



SERNAPESCA
Ministerio de Economía, Fomento y Turismo
Gobierno de Chile

Valparaíso, 31 de agosto de 2015

Estimado/a Vecino/a:

Junto con saludarlo, el Servicio Nacional de Pesca y Acuicultura (SERNAPESCA) informa a usted que durante el mes de Septiembre la Merluza común se encuentra en período de veda entre las regiones de Coquimbo y Los Lagos, prohibiéndose su extracción, compra y venta, con el objetivo de proteger el período de reproducción de este recurso.

Actualmente, la Merluza común se encuentra en un estado crítico de conservación. Por esto, quisiera solicitarle encarecidamente a usted que colabore con la protección de esta especie, evitando consumir Merluza común durante el mes de Septiembre, y prefiriendo otros recursos pesqueros: Reineta, Sierra u otros.

Agradecemos su colaboración y apoyo en compartir esta información con sus vecinos, para así generar una mayor conciencia en la protección de los recursos pesqueros.

"Cuidar nuestros recursos pesqueros es responsabilidad y tarea de todos".

Saluda atentamente a Ud.

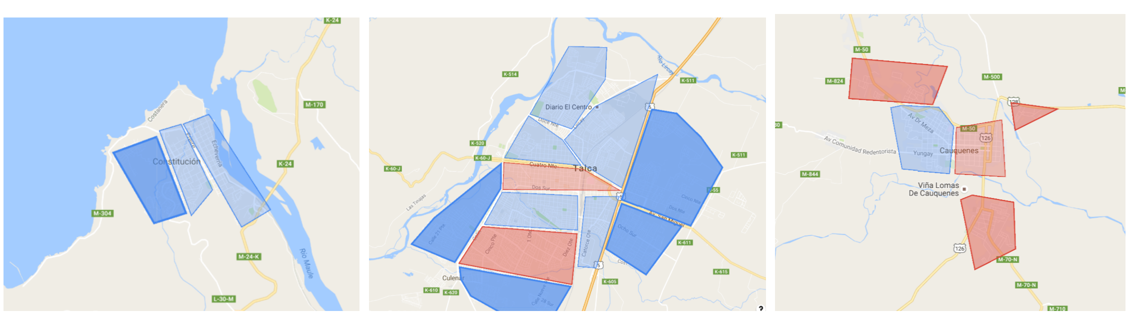
SERVICIO NACIONAL DE PESCA
DIRECTOR NACIONAL
MINISTERIO DE ECONOMÍA FOMENTO Y TURISMO

JOSÉ MIGUEL BURGOS GONZÁLEZ
Director Nacional
Servicio Nacional de Pesca y Acuicultura

Servicio Nacional de Pesca y Acuicultura
Dirección Nacional
Fono (56 -32)2819 100
www.sernapesca.cl

The figure B.2 shows the letter distributed to households during September 2015. The letter, signed by Sernapesca's director, informs about the September ban and the fact that hake's conservation is threatened because of overfishing.

Figure B.3: Examples of Neighborhood Treatment Assignment



The 48 most populated comunas were divided randomly into three levels of saturation: high, low and zero. Based on the level of saturation, the information campaign was assigned at the neighborhood level. The figure B.3 shows the map of three different comunas: The comuna in the left didn't receive information campaign, the one in the center received low level of saturation, the one in the right received high level of saturation. In red, those neighborhoods assigned to receive the information campaign.

C Appendix Tables

C.1 Descriptive Statistics

C.1.1 Data Collected by Mystery Shoppers

During September 2015, the mystery shoppers interacted with fish-vendors 908 times. The table C.1 describes observable characteristics of the stalls visited and the vendors. In general, each stall was operated by one person. Mostly man, and based on mystery shoppers' guess, 47-year-old. The type of weight used informs about the formality of the stall; digital weights are more precise and expensive.

Table C.1: Fish Vendors in Ferias

Variable	Mean	SD	Min	Max	N
Number of Vendors per Stall	1.089	0.326	1	4	883
Proportion Female Fish Vendor	0.425	0.479	0	1	882
Age Vendor	47.438	10.126	19	75	876
Prices Visibly Listed	0.242	0.429	0	1	908
Type of Weight					
No Weight	0.262	0.440	0	1	848
Mechanical Weight	0.410	0.492	0	1	848
Digital Weight	0.308	0.462	0	1	848

This table presents observable characteristics of fish stalls visited by mystery shoppers during September 2015. The variable "Age Vendor" was not directly asked but guessed by mystery shoppers. The type of weight used to weight fish is a proxy of the level of formality of the fish stall.

The table C.2 describes the availability of different types of fish in feria stalls during the ban period. During a typical month, hake would be available in roughly 90% of stalls, however, due to the ban period (and our interventions), only 26% of stalls had hake for sale. The fish species offered in markets depend largely on the latitude of the market, i.e., markets located in the southern regions offer slightly different fish species than the stalls located in northern regions.

Table C.2: Fish Availability in Feria Stalls

Fish	Availability	Price/unit (USD)	Price/kg (USD)	Unitary Weight (kg)	N
Hake	0.263	1.08	3.81	0.284	239
Pomfret	0.684	5.27	4.99	1.057	621
Mackerel	0.124	2.05	4.16	0.492	113
Silverside	0.096	0.17	2.92	0.059	87
Salmon	0.139	7.53	9.22	0.816	126
Sawfish	0.057	6.27	6.25	1.003	52
Albacore	0.051	.	9.21	.	46
Southern Hake	0.042	7.30	5.59	1.306	38

This table presents the availability and average prices of different fish types in feria stalls during September 2015. The mystery shoppers recorded the price for each fish offered for sale in each fish-stall visited. The sale price in each stall was based on units, kilos or both. The unitary weight is estimated using the ratio of these two prices. The albacore is a considerably larger fish type (over 20 kgs) and is only sold in pieces (by kg).

C.1.2 Data Collected in the Fisherman Survey

A round of surveys to Fishermen was collected in August 2016. In total, 231 fishermen were surveyed and asked about their work, typical buyers and fishing behavior. The table C.3 describes the main variables collected in the survey.

Table C.3: Fishermen Characteristics

Variable	Mean	SD	Min	Max	N
Fisherman Boat characteristics:					
Boat Length (mts)	8.52	3.11	6	24	227
Boat Powered by a Motor	0.88	0.33	0	1	231
Fiberglass Boat	0.57	0.50	0	1	228
Wooden Boat	0.39	0.49	0	1	228
Union Participation:					
Number of Unions in the Caleta	1.67	0.90	0	3	230
Fisherman Member of a Union	0.82	0.38	0	1	230
Number of Days that Goes Fishing Every Week:					
Summer	5.01	1.47	1	7	226
Winter	2.25	1.14	0	7	227
Number of Boats in the Caleta:					
Less than 10	0.24	0.43	0	1	189
Between 10 and 30	0.22	0.41	0	1	189
Between 31 and 60	0.24	0.43	0	1	189
Between 61 and 100	0.12	0.32	0	1	189
More than 100	0.19	0.39	0	1	189
Top 3 Most Captured Fishes in the Caleta:					
Hake	0.56	0.50	0	1	230
Sawfish	0.24	0.43	0	1	230
Cuttlefish	0.24	0.43	0	1	230
Pomfret	0.13	0.34	0	1	230
Bass	0.10	0.30	0	1	230
Usual Buyer of the Fish at the Dock:					
Final Consumer	0.58	0.49	0	1	230
Feria Vendor	0.27	0.45	0	1	228
Intermediary	0.60	0.49	0	1	227

This table describes the responses to the Fishermen Survey carried out in August 2016 to 231 fishermen. On average, three fishermen were surveyed in each of the 74 Caletas that operate in the four coastal regions included in our sample. The last section of the table represents the proportion that responded that *Always* or *Most of the Time* the fish was sold to these type of buyers.

C.2 Balance Tables

The table C.4 presents the balance of relevant characteristics across different treatment arms. These variables include market's observable characteristics, socioeconomic characteristics of the municipality and weather information of the day of the visit by a mystery shopper. The columns 1, 2, 4 and 6 present the mean and SD of these variables for different treatment arms. The columns 3, 5 and 7 compare the difference relative to the control group as well as its p-value. Finally, joint significance tests are also reported in the last two columns. The tables C.5 and C.6 present the same estimates but decomposing by the enforcement variations: predictability and intensity/frequency.

Table C.4: Randomization Balance on Observables

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Variable	Control	Info Campaign		Enforcement		Enforc. and Info Camp.	
	Mean	Mean	Diff	Mean	Diff	Mean	Diff
Indicator Fixed Stalls	0.573	0.644	0.083	0.489	-0.069	0.509	0.013
	(0.497)	(0.484)	[0.740]	(0.501)	[0.574]	(0.501)	[0.920]
Distance to Closest Caleta (kms)	16.507	11.572	3.465	14.863	-3.606	26.425	2.245
	(25.082)	(9.503)	[0.516]	(22.626)	[0.386]	(29.037)	[0.682]
Poverty Rate Municipality	19.006	17.567	-2.148	18.026	-1.079	16.734	-0.316
	(4.780)	(3.313)	[0.244]	(5.483)	[0.412]	(7.549)	[0.851]
Av. Monthly Income Municipality (USD)	791.767	875.475	18.446	790.514	-2.506	830.683	20.464
	(149.808)	(172.858)	[0.846]	(140.334)	[0.953]	(139.251)	[0.673]
Deliquency Rate Municipality	0.038	0.029	-0.013**	0.036	-0.001	0.034	-0.004
	(0.015)	(0.002)	[0.016]	(0.015)	[0.835]	(0.009)	[0.480]
Rain Indicator	0.290	0.133	-0.124	0.178	-0.114	0.142	-0.122
	(0.456)	(0.344)	[0.455]	(0.383)	[0.318]	(0.349)	[0.301]
Average Temperature (Celsius)	12.200	12.126	0.081	11.993	-0.192	11.936	-0.688
	(2.281)	(2.087)	[0.942]	(2.021)	[0.797]	(2.196)	[0.346]
Joint Significance							
F statistic			0.609		1.094		0.816
p-value			0.747		0.371		0.575

This table reports characteristics of circuits included in our sample across treatment arms. The columns (1), (2), (4) and (6) show the mean and the standard deviation for the control and treatment groups. The columns (3), (5) and (7) show the coefficient on treatments from regressions of each characteristic on treatments and strata fixed effects, clustreing standard errors at the circuit level. The p-values are reported in brackets. The socio-economic characteristics are aggregated at Municipality level. These variables should be interpreted as the characteristics of the Municipality where the circuit is located. Also, this table reports weather information of the day that different circuits were visited by mystery shoppers. Finally, joint significance test statistics: F statistic and p-values, for all variables on each treatment arm are reported in the last two rows of the table. *** p<0.01, ** p<0.05, * p<0.1

Table C.5: Randomization Balance: Enforcement Predictability

Variable	(1) Control	(2) Info Campaign	(3)	(4) Enforc: Predictable Schedule	(5)	(6) Enforc: Unpredictable Schedule	(7)
	Mean	Mean	Diff	Mean	Diff	Mean	Diff
Indicator Fixed Stalls	0.573	0.644	0.080	0.396	-0.148	0.575	0.045
	(0.497)	(0.484)	[0.749]	(0.490)	[0.270]	(0.495)	[0.710]
Distance to Closest Caleta (kms)	16.507	11.572	3.822	17.286	1.704	20.487	-4.926
	(25.082)	(9.503)	[0.479]	(23.417)	[0.678]	(27.384)	[0.333]
Poverty Rate Municipality	19.006	17.567	-2.173	17.130	-1.739	17.890	-0.075
	(4.780)	(3.313)	[0.240]	(6.185)	[0.212]	(6.450)	[0.960]
Av. Monthly Income Municipality (USD)	791.767	875.475	18.828	809.457	-1.068	801.805	9.235
	(149.808)	(172.858)	[0.843]	(153.747)	[0.981]	(130.506)	[0.834]
Deliquency Rate Municipality	0.038	0.029	-0.013**	0.036	-0.002	0.035	-0.001
	(0.015)	(0.002)	[0.015]	(0.015)	[0.648]	(0.012)	[0.782]
Rain Indicator	0.290	0.133	-0.126	0.117	-0.158	0.202	-0.080
	(0.456)	(0.344)	[0.446]	(0.322)	[0.162]	(0.402)	[0.488]
Average Temperature (Celsius)	12.200	12.126	0.076	12.058	-0.170	11.904	-0.491
	(2.281)	(2.087)	[0.946]	(2.057)	[0.824]	(2.107)	[0.498]
Joint Significance							
F statistic			0.609		1.954		1.717
p-value			0.747		0.067		0.111

This table reports characteristics of circuits included in our sample across treatment arms. The columns (1), (2), (4) and (6) show the mean and the standard deviation for the control and treatment groups. The columns (3), (5) and (7) show the coefficient on treatments from regressions of each characteristic on treatments and strata fixed effects, clustering standard errors at the circuit level. The p-values are reported in brackets. The socio-economic characteristics are aggregated at Municipality level. These variables should be interpreted as the characteristics of the Municipality where the circuit is located. Also, this table reports weather information of the day that different circuits were visited by mystery shoppers. Finally, joint significance test statistics: F statistic and p-values, for all variables on each treatment arm are reported in the last two rows of the table. *** p<0.01, ** p<0.05, * p<0.1

Table C.6: Randomization Balance: Enforcement Intensity

Variable	(1) Control	(2) Info Campaign	(3)	(4) Enforc: High Intensity	(5)	(6) Enforc: Low Intensity	(7)
	Mean	Mean	Diff	Mean	Diff	Mean	Diff
Indicator Fixed Stalls	0.573	0.644	0.098	0.372	-0.164	0.591	0.039
	(0.497)	(0.484)	[0.696]	(0.484)	[0.211]	(0.492)	[0.753]
Distance to Closest Caleta (kms)	16.507	11.572	4.009	16.224	-5.302	21.269	0.600
	(25.082)	(9.503)	[0.451]	(23.309)	[0.330]	(27.292)	[0.885]
Poverty Rate Municipality	19.006	17.567	-1.975	16.242	-2.256	18.564	0.140
	(4.780)	(3.313)	[0.279]	(7.011)	[0.101]	(5.576)	[0.922]
Av. Monthly Income Municipality (USD)	791.767	875.475	17.128	821.315	23.237	792.779	-8.734
	(149.808)	(172.858)	[0.857]	(142.660)	[0.587]	(138.930)	[0.840]
Deliquency Rate Municipality	0.038	0.029	-0.013**	0.036	-0.000	0.035	-0.003
	(0.015)	(0.002)	[0.014]	(0.013)	[0.973]	(0.014)	[0.547]
Rain Indicator	0.290	0.133	-0.128	0.193	-0.077	0.143	-0.143
	(0.456)	(0.344)	[0.439]	(0.396)	[0.491]	(0.350)	[0.215]
Average Temperature (Celsius)	12.200	12.126	0.050	12.127	-0.192	11.853	-0.447
	(2.281)	(2.087)	[0.964]	(2.204)	[0.799]	(1.984)	[0.545]
Joint Significance							
F statistic			0.609		2.016		2.090
p-value			0.747		0.058		0.049

This table reports characteristics of circuits included in our sample across treatment arms. The columns (1), (2), (4) and (6) show the mean and the standard deviation for the control and treatment groups. The columns (3), (5) and (7) show the coefficient on treatments from regressions of each characteristic on treatments and strata fixed effects, clustering standard errors at the circuit level. The p-values are reported in brackets. The socio-economic characteristics are aggregated at Municipality level. These variables should be interpreted as the characteristics of the Municipality where the circuit is located. Also, this table reports weather information of the day that different circuits were visited by mystery shoppers. Finally, joint significance test statistics: F statistic and p-values, for all variables on each treatment arm are reported in the last two rows of the table. *** p<0.01, ** p<0.05, * p<0.1

C.3 Exit of Stalls Correction

Our main results are based on the information gathered by mystery shoppers from the operative stalls at the moment of the visit, which does not capture the fact that the "missing" stalls are not selling hake. To correct for this issue we identify the average number of stalls per circuit/visit before and after the interventions. The comparison between these two averages informs about the number of "missing" stalls per circuit.[24] The number of stalls observed by mystery shoppers in every visit in the post treatment period is increased by computed number of missing stalls. The added observations

[24] We allow the number of missing stalls to be non-integer, and negative if the number of stalls increased.

have zero fish available.[25] [26]

Table C.7: Treatment Effect on Hake Availability Correcting for the Exit of Stalls

VARIABLES	(1) Fresh, Visible Hake	(2) Any Hake Available (Fresh-Visible, Hidden or Frozen)
Panel A: Main Specification		
Info Campaign Only	-0.118**	-0.115*
	(0.060)	(0.065)
Enforcement Only	-0.190**	-0.141
	(0.082)	(0.091)
Info Campaign and Enforcement	-0.156*	-0.130
	(0.084)	(0.104)
Panel B: Variation in Predictability of Enforcement		
Info Campaign Only	-0.111*	-0.121*
	(0.062)	(0.064)
Enforcement on Predictable Schedule	-0.091	-0.061
	(0.073)	(0.087)
Enforcement on Unpredictable Schedule	-0.246***	-0.197**
	(0.089)	(0.100)
Panel C: Variation in Frequency of Enforcement		
Info Campaign Only	-0.113*	-0.121*
	(0.062)	(0.064)
High Intensity Enforcement	-0.086	-0.092
	(0.092)	(0.101)
Low Intensity Enforcement	-0.184**	-0.148
	(0.086)	(0.095)
Change in Dep Var in Control		
During Intervention	-0.17	-0.28
Covariates	Yes	Yes
Baseline Control	Yes	Yes
N	1014	1014

This table presents the coefficient corresponding to the interaction term $T_c \times Post_t$ for each treatment correcting for the exit of stalls. The increase in the number of observations relative to results presented earlier is due to the fact that the correction is done by adding the "missing" stalls (calculated compraing the number of stalls per circuit before and after the interventions). The panel A describes the same specification presented in Table 3. Panels B and C follow the same specification than Table 5. Probit regression marginal effects are reported. Robust standard errors clustered by circuit in parentheses.*** p<0.01, ** p<0.05, * p<0.1

[25]If the number "missing" stalls is negative: the number of stalls observed by mystery shoppers in every visit in the pre-treatment period is increased by that number.

[26]Since we allow the number "missing" stalls to be non-integer, we add a random noise that distributes uniform between -0.5 and 0.5, and then, the sum of the "missing" number and the noise is rounded to the closest integer. This correction makes the expansion more representative of the right (possibly non-integer) number.

C.4 Consumer Mobility Between Neighborhoods

The table C.8 shows the proportion of consumers treated by the information campaign depending on the location of the feria where they are surveyed. The striking fact in this table is that in high-saturation municipalities, the proportion of consumers treated with the information campaign is high, regardless of whether we found that person shopping in a feria located in a treatment neighborhood (78%) or in a control neighborhood (69%). High Information campaign saturation is therefore the effective treatment variable, and conditional on that, the specific location of the feria does not matter too much.

Table C.8: Proportion of Consumers located in Treated Neighborhoods

	Survey in Feria located in Treated Neigh		Survey in Feria located in Control Neigh	
	Prop	N	Prop	N
High Saturation Municipality	0.78	1114	0.69	389
Low Saturation Municipality	0.57	559	0.17	825
Zero Saturation Municipality	0	0	0.00	1014
Overall	0.71	1673	0.18	2228

This table shows the proportion of consumers whose households are located in neighborhoods assigned to receive the information campaign. These statistics are based on households' location reported by surveyed consumers. 71% of consumers surveyed in a feria located in a treated neighborhood live in a household located in a treated neighborhood; the remaining 29% are consumers who live in a household located in a control neighborhood. This table informs about the high consumers' mobility between neighborhoods. In fact, in high-saturation municipalities, the proportion of treated consumers is higher in both, ferias located in treatment and control neighborhoods.

C.5 Alternative Definition Information Campaign Treatment

Tables C.9 and C.10 present the main results using a different definition of the Information Campaign treatment: The variable "Information campaign" indicates whether the observations were collected by mystery shoppers in ferias located in treated neighborhoods - regardless of the level of saturation of the municipality. This definition does not include possible information spill-overs between neighborhoods within municipalities assigned to receive information.

Table C.9: Treatment Effect on Hake Availability

VARIABLES	(1) Fresh, Visible Hake	(2) Any Hake Available (Fresh-Visible, Hidden or Frozen)
Information Campaign Only	-0.082	-0.070
	(0.064)	(0.071)
Enforcement Only	-0.157**	-0.101
	(0.079)	(0.094)
Info Campaign and Enforcement	-0.169**	-0.121
	(0.079)	(0.094)
Change in Dep Var in Control Markets During Intervention	-0.21	-0.36
N	901	901

This table reports the effect of each treatment arm on the availability of illegal hake fish. The variable "Info Campaign" indicates if the feria where the data was collected is located in a neighborhood assigned to receive the information campaign. Probit Marginal effects of he interactions $T_c \times Post_t$ are reported. Robust standard errors are clustered by circuit and presented in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table C.10: Treatment Effect on Hake Sales by Enforcement Strategy

	(1)	(2)
VARIABLES	Fresh Hake	Any Hake
Panel A: Variation in Predictability of Enforcement Schedule		
Info Campaign Only	-0.082	-0.073
	(0.063)	(0.071)
Enforcement on Predictable Schedule	-0.075	-0.036
	(0.071)	(0.089)
Enforcement on Unpredictable Schedule	-0.223**	-0.169*
	(0.088)	(0.099)
Panel B: Variation in Frequency of Enforcement Visits		
Info Campaign Only	-0.082	-0.073
	(0.063)	(0.071)
High Intensity Enforcement	-0.045	-0.049
	(0.087)	(0.101)
Low Intensity Enforcement	-0.177**	-0.140
	(0.086)	(0.095)
Change in Dep Var in control Markets		
During Intervention	-0.21	-0.36
N	901	901

This table reports the effect of each treatment arm on the availability of illegal hake fish. Panel A includes a dummy for the intensity sub-treatment, and Panel B includes a dummy for the predictability sub-treatment, but those coefficients are not shown. Each regression controls for the dependent variable in pre-intervention period, strata fixed effects and municipality characteristics. The variable "Info Campaign" indicates if the feria where the data was collected is located in a neighborhood assigned to receive the information campaign. Probit Marginal effects of he interactions $T_c \times Post_t$ are reported. Robust standard errors are clustered by circuit and presented in parentheses. *** p<0.01, ** p<0.05, * p<0.1

D Further Details on the Cost-Effectiveness Analysis

The section 7 describes the cost-effectiveness analysis of each treatment. These calculations are based on the following parameters:

- Costs: The total cost of implementing enforcement was \$ 62,900.25, which is divided into fixed costs \$ 7,338.06, and variable costs: \$ 55,562.19. The fixed costs include administrative staff salaries, central office coordination, etc. The variable costs include the specific costs incurred to implement the enforcement (i.e., financial compensation of inspectors, gasoline, etc.). Based on Sernapesca information, deploying enforcement in an unpredictable way is 10% more costly regarding staff availability. The cost of implementing enforcement at low frequency is obtained by calculating the (variable) cost of each visit and multiplying by the number of visits under this new regime, adding the fixed costs.

The total cost of implementing the information campaign was $ 16,213.53, which includes the printing and distribution of flyers, posters, and letters in treated neighborhoods.

- Reduction of fish sales: The estimated effects of selling hake during the ban presented in section 5 are translated into numbers of fishes "saved." This exercise takes into account that every stall has 25 hake fishes available, there are 2.57 fish stalls in each feria. Each circuit operates 5 days a week, and the effects consider the three last weeks of September. The enforcement treatment contemplated 83 circuits, whereas 26 circuits are located in municipalities assigned to receive information campaign with a high level of saturation. The information gathered from the vendors surveyed provided useful information to define the right parameters regarding the likely reduction on fish sales as a result of our interventions.