Information and Spillovers from Targeting Policy in Peru's Anchoveta Fishery

Gabriel Englander*

This version: November 27, 2020; Newest version: Link

Abstract

This paper establishes that regulations aimed at mitigating common-pool extraction externalities in the world's largest fishery backfire substantially and exacerbate inefficiencies. The most important biological externality in Peru's anchoveta fishery is the harvesting of juvenile anchoveta. To reduce juvenile catch, the regulator temporarily closes areas where the share of juvenile catch is high. Using administrative microdata from hundreds of thousands of vessel-level fishing operations, I estimate substantial temporal and spatial spillovers from closures that undermine the policy's objective and lead to a net *increase* in juvenile catch of 50%. I explain this result using both theory and data as being due to the fact that closure announcements implicitly provide valuable information regarding the locations of schools of anchoveta. Fishermen exploit this information, catching more juveniles before closures begin, just outside closures during closure periods, and after closures end.

*University of California, Berkeley; englander@berkeley.edu. I am indebted to Maximilian Auffhammer, Solomon Hsiang, James Sallee, Dale Squires, and Reed Walker for invaluable guidance. I am deeply grateful to Peru's Ministry of Production, Sociedad Nacional de Pesquería, Mariano Gutiérrez and Jorge Risi for providing data. I thank the many stakeholders in Peru who have taken the time to teach me about the anchoveta industry, especially Eloy Aroni, Juan Carlos Suiero, Mariano Gutiérrez, Leonor Lamas, Rosa Vinatea, and José Luis Rojas Toledo. I thank Michael Anderson, Manuel Barron, Bjorn Bergman, Abdoulaye Cisse, Eyal Frank, Meredith Fowlie, Nick Hagerty, Alain de Janvry, Larry Karp, David Kroodsma, Megan Lang, Peiley Lau, Anouch Missirian, Frederik Noack, Paula Ochiel, Kate Pennington, Wenfeng Qiu, Elisabeth Sadoulet, Jeffrey Shrader, and seminar audiences at Columbia University, Universidad del Pacífico, University of British Columbia, NOAA Southwest Fisheries Science Center, and University of California, Berkeley for helpful comments. Javier Duarte, William Fulton, and Gonzalo Moromizato provided excellent research assistance. I acknowledge generous financial support from NOAA grant No. NA18OAR4170326, California Sea Grant College Program Project No. E/MRE-9.

1 Introduction

Managing common-pool resource extraction is complicated because extraction causes multiple externalities (Smith, 1969). The most well-known is the stock externality: extraction by one agent harms other agents by reducing the amount of resource available to them (Gordon, 1954). Some regulators have been able to mitigate this market failure by setting a cap on total extraction and assigning quasi-property rights to the resource by dividing the cap among agents (Costello et al., 2008; Isaksen & Richter, 2019). But there are many other production externalities that property rights-like instruments do not address, including externalities related to the timing of extraction, the location of extraction, and biological and environmental characteristics of the resource (Smith, 2012).

I study the effects of a policy that is targeted to reduce the most important biological externality in the world's largest fishery: the capture of juvenile anchoveta in Peru (Paredes, 2014; Salvatteci & Mendo, 2005). I find that the policy reduces juvenile catch in the areas and time periods to which it applies (direct effect). But the policy also has the unintended consequence of increasing juvenile catch in nearby areas and during time periods in which the policy does not apply. These spatial and temporal spillovers more than offset the direct effect of the policy. The policy backfires, *increasing* juvenile catch by 50% on net, because the policy implicitly reveals that nearby areas and time periods are high-productivity fishing grounds. These information spillovers are valuable because finding quality fishing locations is a key challenge for fishermen (Asriyan et al., 2017; Joo et al., 2015).

Peru's anchoveta fishery is the world's largest, accounting for 8% of global marine fish catch, and it contributes nearly $2 billion dollars in export revenues for Peru each year (FAO, 2018; PRODUCE, 2018a). The regulator restricts fishing to allow the anchoveta stock to grow quickly, enabling large, sustainable harvests (Pikitch et al., 2012). One important variable for the growth of the anchoveta stock is the level of juvenile catch. Catching juvenile anchoveta reduces the future anchoveta stock more than catching adult anchoveta, in part because juveniles have lower reproductive capacity than adults (Salvatteci & Mendo, 2005). Fishermen do not account for this biological externality because they are paid according to the tons they catch and the international price of fishmeal, not the composition of juveniles and adults they catch (Fréon et al., 2014; Hansman et al., 2019; SUPNEP, 2017). Taxing juvenile catch is the first-best solution, but the fishing industry opposes such a policy (Instituto Humboldt et al., 2018). Instead, the regulator attempts to reduce juvenile catch by implementing temporary spatial closures in areas where they believe juveniles are abundant.

I analyze the regulator's temporary spatial closures policy in this paper, accounting for other regulations that also affect fishing behavior. I use administrative microdata and data from fishing firms, which together contain the location, time, and number of juvenile anchoveta each vessel catches each time it sets its net in the water. These data comprise hundreds of thousands of individual fishing operations. Fishermen report the percentage juvenile they catch to the regulator in real-time.[1] When the measured percentage of catch that are juvenile in an area exceeds 10%, the regulator can temporarily ban fishing in that area for three to five days. Fishermen are not allowed to fish inside actively closed areas. But they are allowed to fish inside closed areas between the announcement and the beginning of closure periods, just outside closed areas during closure periods, and inside closed areas after the end of closure periods. Between 2017 and 2019, the regulator implemented 410 temporary spatial closures, each covering a different area of ocean and time period.

Due to other regulations in the fishery, reducing search costs is the primary margin by which fishermen can increase profits within a fishing season. Vessels spend more than 20% of their time on fishing trips searching for anchoveta, and fuel comprises one-third of variable costs (Joo et al., 2015; Kroetz et al., 2016). Closures might help fishermen reduce search costs because closures implicitly signal high-productivity fishing locations: the regulator implements closures in response to real-time anchoveta catch data from all vessels, and there is only anchoveta catch in an area if anchoveta are sufficiently abundant. This information is potentially valuable because there is zero anchoveta catch in most areas, but strong correlations in anchoveta catch over time and space.[2] I develop a simple game theoretic model to show that total juvenile catch can increase as a result of the closures policy given two conditions: (1) closures announcements are a sufficiently large positive signal of fishing productivity near closures (before, just outside, and after closures) and (2) productivity and relative juvenile abundance near closures are sufficiently high.

Estimating the causal effect of the temporary spatial closures policy requires counterfactual areas and times that could have been closed and are comparable to closures declared by the regulator. To address this challenge, I generate "potential closures" by creating an algorithm that mimics the regulator's closure rule. I intersect potential closures with the closures declared by the regulator, yielding treatment units (potential closures that get closed) and control units (potential closures that do not get closed). I estimate whether juvenile catch is different inside treated potential closures compared to control potential closures—the direct

[1]In my regressions, I correct for misreporting to the regulator by matching fishermen-reported data to percentage juvenile measured by third-party inspectors. Third-party inspector data is not used by the regulator to determine closures.

[2]The daily probability any vessel catches anchoveta in a given .1° grid cell (~11 by 11 km) during the fishing season is 0.5%. However, conditional on at least one vessel catching anchoveta in a .1° grid cell yesterday, the probability of positive anchoveta catch today in the same grid cell is 31% (temporal correlation). Conditional on at least one vessel catching anchoveta in a .1° grid cell today, the probability of positive anchoveta catch in at least one adjacent grid cell on the same day is 92% (spatial correlation).

effect of the policy—as well as whether juvenile catch is different before, just outside, and after treated potential closures compared to control potential closures—the temporal and spatial spillover effects of the policy.

Treatment variation occurs because the regulator declares closures based on one sample statistic: the percentage juvenile measured by fishermen. I obtained data from fishing firms which contains the distributions that percentage juvenile values are drawn from.[3] This data is not available to the regulator when it is making closure decisions. By controlling for this distribution (rather than the percentage juvenile values themselves), the identifying variation comes from comparing potential closures that by chance had higher percentage juvenile draws (so were declared actual closures by the regulator) to potential closures that by chance had lower percentage juvenile draws (so were not declared closures by the regulator). I also flexibly control for location, time, and fishing productivity. Identification occurs from comparing potential closures that are equally desirable fishing locations and contain similar concentrations of juveniles, but which the regulator believes are different because the data available to the regulator are lower resolution.

I test three hypotheses. The first concerns measuring the causal effect of the policy and the second and third relate to testing a potential mechanism. First, do temporary spatial closures reduce total juvenile catch when accounting for temporal spillovers, spatial spillovers, and other regulations that affect fishing? Second, does the policy communicate information about the value of fishing before, just outside, and after closures? Third, does this information mechanism increase spillovers?

To test the first hypothesis I empirically estimate direct, temporal spillover, and spatial spillover effects of the policy. I find that the policy reduces juvenile catch inside closed areas during closure periods (direct effect). But the policy also causes large spillovers that more than offset the direct effect of the policy. I estimate that the policy increases juvenile catch inside closed areas between the announcement and the beginning of closure periods (temporal spillover), it increases juvenile catch just outside closed areas during closure periods (spatial spillover), and it increases juvenile catch inside closed areas after the end of closure periods (temporal spillover). These areas and time periods are not targeted by the policy; the

[3]To estimate percentage juvenile, fishermen measure the length of 200 anchoveta out of the several million individual anchoveta caught per set of the fishing net (an individual fishing operation). Fishermen record the length distribution of these 200 anchoveta—the number of measured anchoveta in each half-cm length interval–and report this data to their firm but not to the regulator. My regressions control for the average length distribution of the sets that generate each potential closure. Percentage juvenile from each set is one sample statistic from this distribution: the percentage of measured individuals that are less than 12 cm (juveniles are anchoveta less than 12 cm). Each percentage juvenile value reported to the regulator is a "draw" from this distribution because sets that generate the same potential closure are fishing from the same local anchoveta population. I account for misreporting to the regulator or to fishing firms with an additional dataset from third-party inspectors.

regulator only intends to change juvenile catch inside closed areas during closure periods. Summing the direct, temporal spillover, and spatial spillover effects, I estimate that the policy increases total juvenile catch by 50% despite a second regulation which sets binding limits on total catch (juveniles and adults). The closures policy backfires—worsening its target outcome on net—because closures cause vessels to reallocate fishing to areas where the share of juveniles is higher.

Second, I posit that closures backfire because they implicitly provide information about the value of fishing near closures. If information is the key mechanism, then it must be the case that fishing near closures is more productive than fishing elsewhere, absent vessels' responses to the information. However, this need not be true in equilibrium because of congestion: the more vessels that fish in the same location, the less each vessel catches per unit of fishing effort (Huang & Smith, 2014; Smith, 1969). I support these predictions from my game theoretic model with the following empirical evidence. I estimate that vessels that fish near *potential* closures (before, just outside, or after potential closures) catch 9% more tons of anchoveta per unit of fishing effort than if they fished elsewhere. However, I also estimate that vessels that fish near *actual* closures declared by the regulator do not catch more tons of anchoveta per unit of fishing effort. Indeed, the policy increases total tons caught near closures by 35%, but this increase is shared across a larger number of vessels and a higher degree of fishing effort (congestion). Together, the result using potential closures suggests that closures do provide valuable information, but the result using actual closures suggests that the value of this information is competed away by rational vessels in the equilibrium. These results illustrate one benefit of the identification strategy in this paper. The component of information that is valuable in closures declared by the regulator is also contained in potential closures; they are both correlated with anchoveta presence. But because potential closures are unobservable to fishermen, the value of this information cannot be competed away, making it observable econometrically.

Finally, I estimate whether the information provided by closures increases spillovers. If information is a mechanism underlying the policy's spillover effects, then vessels that receive larger information shocks from closure announcements should have larger treatment effects. I test this model prediction by dividing vessels into those that did and did not fish inside a given potential closure the day before closure announcement would occur (if the potential closure is declared an actual closure by the regulator). Juvenile catch increases by 88% for vessels that did not fish inside a given potential closure the day before closure announcement. For vessels that already had information about the productivity of fishing near a potential closure because they fished there the day before closure announcement, there is no treatment effect. This information mechanism also operates at the firm-level. Among firms that own multiple fishing vessels, the response to closures is driven by vessels in firms with less information about an area before closure announcement. Juvenile catch increases by 78% for vessels that had no other member of their firm fish inside a given potential closure the day before closure announcement would occur. For vessels that already had information about the productivity of fishing near a potential closure because a different vessel in their firm fished there the day before closure announcement, the increase in juvenile catch because of the policy is only 19%.

The primary contributions of this paper are to the literature on targeted policies. Because governments have finite capacity to solve market failures, policymakers often attempt to reduce an externality by targeting only the highest marginal damage places, time periods, or firms (Gray & Shimshack, 2011; Greenstone & Jack, 2015). Whether targeted policies succeed in reducing externalities depends on their direct effects on targeted units and their spillover effects on non-targeted units. Previous papers have estimated spatial or temporal spillovers from a targeted policy, such as spatial spillovers from a hot-spot policing intervention in Colombia (Blattman et al., 2019), temporal spillover from the US Endangered Species Act's critical habitat provision (List et al., 2006), and spatial spillovers from blacklisting high-deforestation municipalities in Brazil (Assunção et al., 2019), but estimating both effects simultaneously is rare.[4] Additionally, while previous papers have estimated spillovers that partially offset or augment the direct effect of a targeted policy, it is uncommon to find spillovers so large they reverse the sign of the policy's effect. Finally, I provide evidence for an information mechanism underlying the large spillovers I estimate. Because the direction and magnitude of spillovers are context-dependent, identifying the mechanisms through which targeting causes spillovers is necessary for yielding generalizable lessons for targeted policy design (Pfaff & Robalino, 2017).

The information mechanism I uncover in this paper is most similar to the concept of "information spillovers" in financial economics. In Asriyan et al. (2017), information spillovers occur because sellers' private asset values are correlated, so a trade by one agent is a signal of the value of other agents' assets. In this paper, information spillovers occur through the policy, which communicates information about non-targeted units (the value of fishing near closures), which in turn changes the outcomes of non-targeted units. Policy-induced information spillovers likely operate in a range of other contexts as well. For example, rationing the consumption of some goods to reduce stockpiling could increase stockpiling of non-rationed goods if the policy causes consumers to believe shortages of non-rationed goods

[4]Estimating direct, temporal spillover, and spatial spillover effects requires substantial treatment variation. Two other papers that estimate these effects are Ladino et al. (2019), which studies Colombia's illegal crop substitution program, and Gibson and Carnovale (2015), which evaluates driver responses to road pricing.

are more likely (Erdem et al., 2003; Keane & Neal, 2020). Alternatively, targeting infectious disease tests to priority groups could increase social activity and disease transmission among non-targeted people if the policy causes them to lower their subjective probability of infection (Acemoglu et al., 2020). Though policy-induced information spillovers are probably common, I know of no prior research that empirically shows that information spillovers can undermine the intended goals of a second-best externality mitigation policy.

Most economics research on common-pool resources since the seminal works of Gordon (1954) and Scott (1955) has focused on alleviating the stock externality by assigning quasi-property rights to the resource. But rights-based instruments defined in terms of tons, as in Peru and in the vast majority of fisheries with rights-based instruments, do not account for age-specific differences in reproduction, growth, and mortality (Quaas et al., 2013; Smith, 2012). I contribute to the literature on common-pool resource extraction by estimating the extent to which a new type of place-based policy, known as "dynamic ocean management", succeeds in reducing the most important biological externality in the world's largest fishery (Dunn et al., 2016; Hazen et al., 2018).

In Section 2 I describe fishermen's economic incentives, the temporary spatial closures policy, and the structure of the anchoveta industry. I present my game theoretic model in Section 3, data in Section 4, and empirical strategy in Section 5. I test my three hypotheses in Sections 6, 7, and 8 and discuss policy alternatives in Section 9.

2 Institutional context

Fishermen's economic incentives (Section 2.1) and the temporary spatial closures policy and broader regulatory environment (Section 2.2) inform my game theoretic model and empirical strategy. This contextual information is also necessary for understanding the data I use and my empirical results.

Globally, capture fisheries generate revenues of $130 billion per year, provide 17% of animal protein directly consumed by humans, and directly employ 40.3 million people (FAO, 2018). Of these, the Peruvian anchoveta fishery is the largest, accounting for 8% of tons caught between 2005 and 2016 (FAO, 2018). Peruvian anchoveta (*Engraulis ringens*) are a species of anchovy. 97% of anchoveta tons are processed into fishmeal and fish oil, which are primarily used for aquaculture and livestock feed (PRODUCE, 2018a). There are two Peruvian anchoveta stocks (populations): the North-Central stock, which occurs entirely within Peruvian jurisdiction, and the Southern stock, which is shared with Chile. I limit my analysis to the North-Central stock, which accounts for 95% of tons landed during my study period, the six fishing seasons of 2017, 2018, and 2019. "Landing" refers to the point of landing, when a vessel transfers its catch to a processing plant.

2.1 Fishermen incentives

In most industries, firms choose output and input quantities to maximize profits. Depending on market structure, firms' choices may also affect output and input prices. In the Peruvian anchoveta fishery, policy and contracts constrain fishermen's ability to adjust most of these variables. First, as described below in Section 2.2, individual vessel quotas limit the tons of anchoveta that vessels can land each season. This output quantity constraint is typically binding. Second, output price is exogenous because fishermen are paid a fixed percentage of the international price of fishmeal for each ton of anchoveta they land.[5] Individual fishermen or fishing vessels cannot affect the international price of fishmeal. Input prices are also not affected by fishermen's decisions. The price of fuel is exogenous and wages equal a fixed percentage of the international price of fishmeal.

Given these constraints on output quantity, output price, and input prices, input quantities are the main margin fishermen can adjust to increase profits within a fishing season. In particular, fishermen can increase profits by reducing the quantities of labor and fuel they spend searching for anchoveta. Examining the potential effects of temporary spatial closures on search costs can therefore guide predictions of how the policy might change fishermen behavior.

Vessels spend more than 20% of their time on fishing trips searching for anchoveta; fuel comprises about one-third of variable costs; and maintenance is about one-fifth of total costs (Joo et al., 2015; Kroetz et al., 2016). Fishermen who do not pay fuel costs or maintenance costs directly (because they work for a large fishing company) still incur an opportunity cost of their time that could be reduced by finding anchoveta more quickly. Closures might help fishermen reduce search costs because closures implicitly signal high-productivity fishing locations: the regulator implements closures in response to real-time anchoveta catch data from all vessels, and there is only anchoveta catch in an area if anchoveta are sufficiently abundant.[6]

[5] Fishermen are paid per ton of anchoveta they land. The price per ton is a fixed percentage of the average monthly free-on-board (FOB) price of fishmeal in Hamburg (Fréon et al., 2014). According to a collective bargaining agreement with companies that account for more than 33% of landings, fishermen that land anchoveta that will be processed into fishmeal and fish oil receive 1.792% of the FOB price per ton of anchoveta (SUPNEP, 2017). Under this agreement, crews divide revenue amongst themselves in fixed proportions: the captain receives "two parts" (twice as much as a regular fisherman), the second-in-command and first engineer receive one and a half parts, and regular fishermen recieve one part (SUPNEP, 2017). Interviews and analysis conducted by Hansman et al. (2019) indicate that fishermen not covered by this agreement are also paid a fixed percentage of the FOB price of fishmeal.

[6] There is also a positive correlation between percentage juvenile reported to the regulator and tons caught: a one percentage point increase in percentage juvenile predicts 1.8% more tons caught. While my game

This information is potentially valuable because there is zero anchoveta catch in most areas; the daily probability any vessel catches anchoveta in a given .1° grid cell (∼11 by 11 km) during the fishing season is 0.5%. However, anchoveta catch is highly correlated over time and space. Conditional on at least one vessel catching anchoveta in a .1° grid cell yesterday, the probability of positive anchoveta catch today in the same grid cell is 31% (temporal correlation). Conditional on at least one vessel catching anchoveta in a .1° grid cell today, the probability of positive anchoveta catch in at least one adjacent grid cell on the same day is 92% (spatial correlation).

Fishing near closures (just before, outside, or after closures) could also reduce costs by increasing average tons caught per set of the fishing net (an individual fishing operation). Tons caught per set is a measure of fishing productivity conditional on finding anchoveta because fishermen only perform a set when they see anchoveta in the water.[7] The average fishing trip lasts 22.3 hours and features 2.3 sets (medians are 18.5 and 2). An increase in tons caught per set would be valuable to fishermen because each set requires about one and a half hours of physically demanding labor and increases the cost of maintaining the net (e.g. by causing wear and tear). Moreover, an increase in tons per set would be indirect evidence that closures reduce search costs. In this case, vessels need fewer sets to reach their quota for the season, which suggests lower time and fuel costs from searching for anchoveta in order to perform sets.

The percentage or number of juveniles that fishermen catch does not affect profits during my study period.[8] Fishermen are paid per ton, not by the composition of juveniles and adults they catch. Moreover, the regulator eliminated penalties for catching juveniles in 2016 (PRODUCE, 2016a).[9] Though juvenile anchoveta are relatively more abundant near closures, as one would expect given that the regulator closes areas where the share of juvenile catch is high, fishermen do not have an economic incentive to avoid catching juvenile

theoretic model and main empirical result can accommodate both explanations of why vessels fish more near closures—because of information on anchoveta presence or because high percentage juvenile areas are especially desirable fishing grounds—the results in Section 7 support the former explanation.

[7]For example, an executive at a large fishing company told me in an interview that tons per set is the primary performance metric his company uses to evaluate the captains of their fishing vessels.

[8]Very small juveniles can get stuck in the holes in the net, increasing the time fishermen need to spend cleaning the net before they can resume fishing. But these events are rare (Instituto Humboldt et al., 2018).

[9]This regulatory change was motivated by concerns that fishermen were discarding catch with a high percentage of juveniles at sea to avoid being penalized, as well as complaints from the fishing industry that fishermen have limited control over juvenile catch because fishermen cannot predict percentage juvenile before performing a set (Instituto Humboldt et al., 2018; Paredes, 2014). Discarding juveniles is wasteful because juveniles can be processed into fishmeal and fish oil. The same regulation also required vessels to begin reporting the percentage of juveniles they catch at sea in real-time via electronic logbooks. Eliminating penalties for juvenile catch reduces the incentive for fishermen to underreport, potentially increasing the accuracy of the data the regulator uses to determine closures.

anchoveta. If fishing near closures reduces search costs or increases tons caught per set, profit-maximizing fishermen will do so.

2.2 Temporary spatial closures and other relevant regulations

The anchoveta fishery is subject to a suite of regulations designed to promote economically profitable and biologically sustainable fishing. For this paper, the three most important regulations are an industry-wide catch limit for each fishing season, individual vessel quotas, and the temporary spatial closures policy.

The regulator (PRODUCE) sets an industry-wide limit on the total tons that can be landed during each fishing season, called the Total Allowable Catch (TAC). Population estimates before the beginning of the fishing season from IMARPE, Peru's marine science agency, guide this decision (IMARPE, 2019). The regulator sets the TAC such that the remaining biomass of adult (sexually mature) anchoveta at the end of the fishing season will exceed 4 to 5 million tons, depending on environmental conditions. The regulator and scientific agency do not want adult biomass to fall below 4 million tons because when this has occurred in the past the stock grew more slowly than usual, reducing the tons of anchoveta that could be caught in the next season and in future seasons (Pikitch et al., 2012). While TAC prevents biological overexploitation, it does not by itself prevent the dissipation of economic rents if new vessels can enter the fishery, existing vessels can upgrade their capacity, or fishermen "race to fish" before the TAC is reached (Homans & Wilen, 1997, 2005; Huang & Smith, 2014; Reimer & Wilen, 2013; Smith, 1969). There are two fishing seasons per year in the North-Central zone. The first season is typically between April and July and the second season is typically between November and January. The season ends at the scheduled end date or when the TAC is reached, whichever comes first. The season can also be cancelled preemptively due to biological or oceanographic conditions. In the second season of 2017 and 2019, the season was cancelled when only 44% and 36% of the TAC had been reached.[10] In the second season of 2018 and the first season of 2017, 2018, and 2019, tons landed were 99%, 85%, 98%, and 96% of the TAC.

The second important regulation is individual vessel quotas (IVQs). The regulator assigned IVQs in 2009 and they are defined as a percentage of the TAC (Kroetz et al., 2019; Tveteras et al., 2011). Vessels have the same IVQ each season. For example, the IVQ for the vessel with the unique identifier CE-4122-PM is 0.22858% of each season's TAC. In the

[10] The second season of 2017 was cancelled because IMARPE detected significant spawning activity (IMARPE, 2018). The second season of 2019 was cancelled because oceanographic conditions led schools of juveniles to inhabit the same areas as schools of adults (PRODUCE, 2020b). The co-occurrence of juveniles and adults led to high rates of juvenile catch because fishermen have limited ability to predict whether anchoveta in the water are juveniles or adults (IMARPE, 2019; Paredes, 2014).

first season of 2017, when the TAC was 2.8 million tons, this vessel was entitled to land ∽6,400 tons. IVQs are only transferrable within-firm. To transfer an IVQ across firms, the vessel itself must be sold (Natividad, 2016). By limiting entry and reducing the race to fish, the implementation of IVQs in the Peruvian anchoveta fishery increased firms' profits (Kroetz et al., 2019; Natividad, 2016; Tveteras et al., 2011). IVQs belong to a larger class of (property) "rights-based instruments".[11] In addition to improving economic outcomes, rights-based instruments increase biomass (size of a stock in tons) and reduce the probability of fisheries "collapse" (Costello et al., 2008; Costello et al., 2016; Isaksen & Richter, 2019). But rights-based instruments defined in terms of tons, as in Peru and in the vast majority of fisheries with rights-based instruments, do not account for age-specific differences in reproduction, growth, and mortality (Quaas et al., 2013; Smith, 2012).

The third important regulation, temporary spatial closures, attempts to address this age externality. The regulator's goal in implementing temporary spatial closures is to reduce the capture of juvenile anchoveta. The purpose of this paper is to analyze the extent to which temporary spatial closures achieve this objective.

The excess capture of juvenile fish was a leading explanation for decreased catch in many European fisheries in the late 1800s. The debate over the effects of juvenile catch was partly responsible for the beginnings of fish biology research (Smith, 1994, p. 70-76). If fish are not allowed to reach maturity and reproduce, the stock will diminish. Larger fish also tend to be more valuable. These two consequences of excessive juvenile catch are known as "recruitment overfishing" and "growth overfishing", respectively (Quaas et al., 2013). It is now a common goal of fisheries management to allow most fish in a stock to spawn at least once in their life (Paredes, 2014; Wallace & Fletcher, 1997).

The regulator began implementing temporary spatial closures, called Suspensiones Preventivas, in the first fishing season of 2014.[12] The regulator can temporarily close an area of ocean when the percentage of individuals caught in that area that are juvenile exceeds 10%. The 10% juvenile threshold is not strictly applied in practice (many instances of percentage juvenile greater than 10% do not result in closures). Juvenile anchoveta are anchoveta smaller than 12 cm. Percentage juvenile therefore refers to the percentage of individual anchoveta that are less than 12 cm.

Initially, the data used to determine closures came from third-party inspectors sampling anchoveta at the point of landing. When a vessel landed anchoveta with a high percentage of

[11] IVQs and other rights-based instruments are not formal property rights because they entitle fishermen to a flow from the resource (e.g. a percentage of the TAC), but do not typically confer ownership of the stock itself (the state retains ownership of the stock) (Reimer & Wilen, 2013)

[12] The regulation allowing the regulator to declare temporary spatial closures was published in 2012 (PRODUCE, 2012).

juveniles, the regulator could determine where the vessel had fished by asking the captain and reviewing the vessel's locations and movement patterns. Each vessel's location, heading, and speed is transmitted live to the regulator through an on-board GPS transponder, referred to as a "vessel monitoring system" (VMS).[13] In the first year of the policy, the regulator was required to announce closures at least 24 hours before the start of closure periods. In August 2015, the announcement period was shortened to its current form (PRODUCE, 2015). Closures that begin at midnight (93% of all closures) must be announced by 3 PM (9 hours in advance). Closures announced between 3 and 6 PM begin at 6 AM the next day (announcement is at least 12 hours in advance).

I analyze the effects of the policy during the six fishing seasons of 2017, 2018, and 2019, when the use of "electronic logbooks" further enhanced the regulator's ability to target high-juvenile areas. Electronic logbooks refer to software fishermen use to record ("log") their catch at sea. Beginning with the first fishing season of 2017, the regulator required vessels to report to the regulator the location, estimated tons caught, and estimated percentage juvenile caught immediately after each set (an individual fishing operation). Since estimating percentage juvenile at point of landing typically measures anchoveta from several sets, hours after they were caught at sea, electronic logbook data are both higher-resolution and timelier. For my empirical analysis, I calculate juvenile catch by matching electronic logbook and landings data, which preserves the resolution of the electronic logbook data while eliminating the bias that would occur if I only used the fishermen-reported electronic logbook data (Section 4).

The regulator determines closures as follows.[14] An official monitors the electronic logbook data in real-time, which appear as points on a digital map. The color of each point depends on its percentage juvenile value and the official can click on a point to view its other statistics, such as estimated tons. The official can select a group of points by drawing a rectangle with their mouse. Based on the points selected by the official, a computer algorithm calculates the centroid, size, and number of days for the resulting closure. The exact algorithm is not publicly available, but government officials have been willing to describe it in workshops and personal conversations. The size of a closure is increasing in the total tons caught by the group of points (Instituto Humboldt & SNP, 2017; Instituto Humboldt et al., 2018). Closures last three, four, or five days.[15] Longer closures are declared in response to higher

[13]The acronym used in Peru is SISESAT.

[14]The government officials tasked with determining closures demonstrated their process to me in a December 2019 interview.

[15]I estimate the effects of closures declared by the regulator, PRODUCE. The scientific agency, IMARPE, can declare closures of up to 10 days. These closures can apply to all fishing grounds (i.e., Peru's entire Exclusive Economic Zone). IMARPE also has the power to end a fishing season before the scheduled end date and before vessels have reached the TAC.

percentage juvenile values. Manual adjustments to the closure generated by the computer algorithm are allowed, for example to ensure the closure covers all the points selected by the official (Instituto Humboldt & SNP, 2017; Instituto Humboldt et al., 2018).

Vessels are not allowed to fish inside active closures (inside closed areas during the three-to-five-day closure periods). However, vessels are allowed to fish inside closed areas after the announcement but before the beginning of closure periods, outside closed areas during closure periods, and inside closed areas after the end of closure periods.

The regulator uses real-time VMS data to monitor fishing inside active closures. The regulator defines fishing as moving slower than two knots at a non-constant heading for more than one hour and penalizes vessels that move in this way inside an active closure (PRODUCE, 2016a).[16] VMS transponders can be physically disabled but not manipulated; the transponder is inside a closed, metal box and it transmits data to the regulator automatically every 10 minutes. The vessel owner is penalized if the vessel's transponder does not transmit data to the regulator for more than two hours for any reason (e.g., PRODUCE, 2017b). To the extent that vessels are able to conceal fishing inside active closures, this detection avoidance would accentuate my main result that the policy increases total juvenile catch.

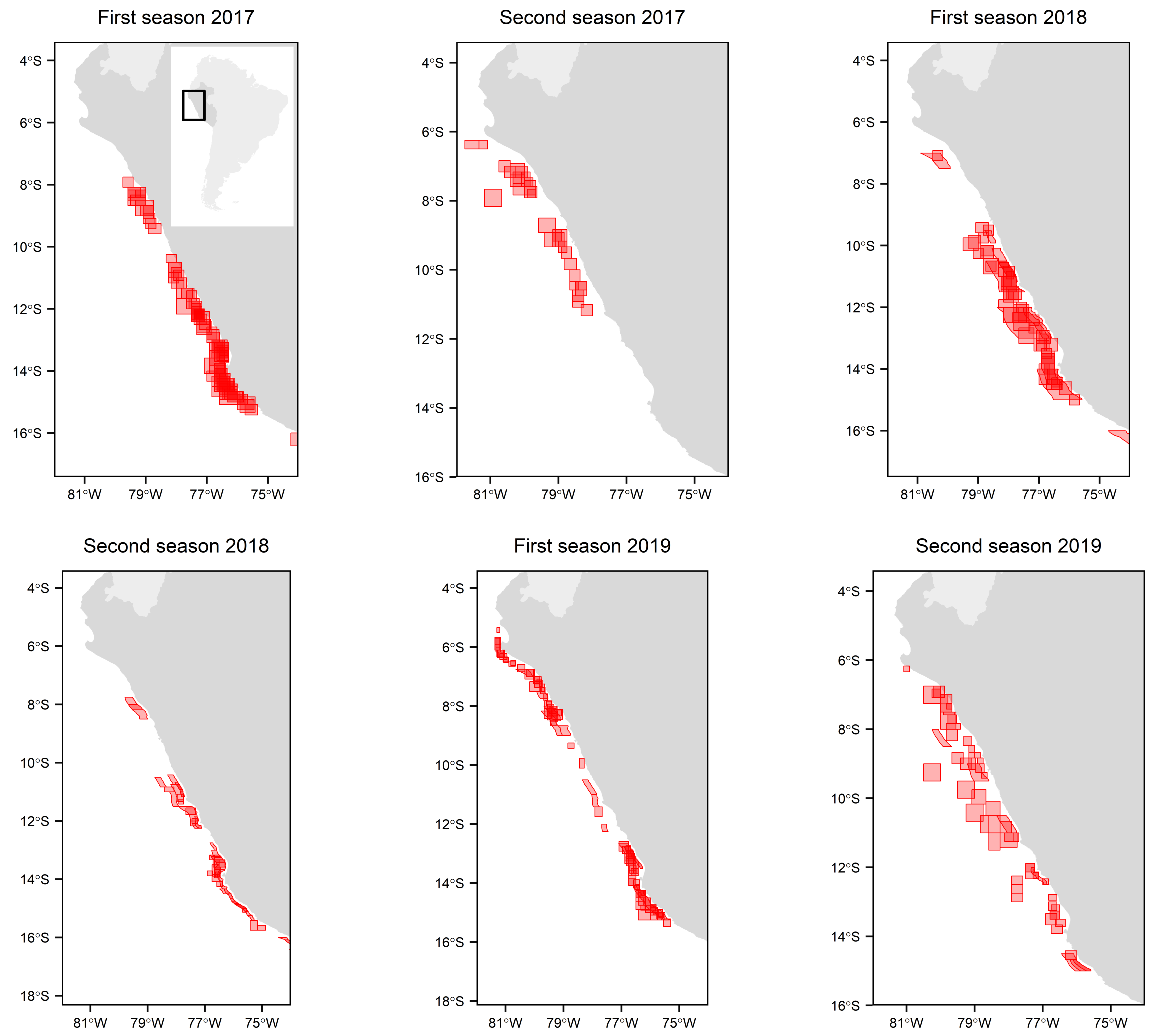
The regulator announces closures on their website and by sending emails and WhatsApp messages to firms (PRODUCE, 2020c). Firms then communicate closures to vessels at sea using radio, and fishermen enter the coordinates of closed areas into their on-board electronic navigation systems. Some fishing companies also monitor the locations of their vessels in relation to active closed areas and call vessels on the radio when they are near an active closure. 43% of closure announcements create multiple closures; the average announcement creates 1.58 closures.

I downloaded all temporary spatial closures announcements from the regulator's website. The regulator declared 410 closures in the North-Central zone during my study period (Figure 1). The smallest and largest closures are 170 and 12,512 km^2. The mean and median closures are 1,328 and 1,211 km^2 (about twice as large as New York City). The share of closures that last three, four, and five days is 48%, 15%, and 36%. There are 0.73 active closures on an average day during the fishing season. Closures are spatially correlated: 26% of closures border or intersect a closure created by the regulator's next closure announcement. On average, the minimum distance between closures created by successive announcements is 178 km.

Temporary spatial closures are a type of "dynamic ocean management", in that they vary

[16] All fishing in the Peruvian anchoveta fishery is "purse seine fishing", which involves dragging a net in a circle around a group of anchoveta.

Figure 1: Temporary spatial closures in the North-Central zone by fishing season



Notes: Each red polygon represents a closure. Closures last three, four, or five days. The average closure is 1,328 km^2. The regulator declared 410 closures in the North-Central zone during the six fishing seasons of 2017, 2018, and 2019. There are 0.73 active closures on an average day during the fishing season. The inset map in the top, left panel shows South America (light grey), Peru (dark grey), and the North-Central zone (black rectangle).

over space and time and are updated by the regulator in response to real-time data (Lewison et al., 2015; Maxwell et al., 2015). Dynamic ocean management is different from more traditional management approaches, like marine protected areas, which are time-invariant, and seasonal closures of an entire fishery, which are space-invariant. Dynamic ocean management is designed to reduce fishing in the areas and times where fishing is likely to cause ecologically undesirable outcomes. But it does not need to cover as much space-time as more traditional management approaches because it only targets relevant areas and times. For these reasons, simulations of dynamic ocean management find it can achieve the same ecological objectives as more traditional management approaches at a much lower economic cost (Dunn et al., 2016; Hazen et al., 2018). The same win-win idea motivates temporary spatial closures. By allowing fishing to continue in most places, temporary spatial closures could reduce total juvenile catch while minimizing the cost of the policy to fishermen.

Temporary spatial closures could reduce total juvenile catch by causing fishermen to search for new fishing grounds. In this case, closures would reduce total juvenile catch if the share of juveniles is lower in these new fishing grounds. There is substantial variation in relative juvenile abundance across space because schools of fish tend to be age-segregated (i.e., each school of anchoveta contains mostly juveniles or mostly adults).

The fishing industry opposes penalties on juvenile catch because of fishermen's limited ability to predict percentage juvenile before performing a set (Instituto Humboldt et al., 2018). In interviews I conducted in Peru in December 2019, fishing industry employees and stakeholders argued that a tax on juvenile catch would be "unfair" for this reason. Temporary spatial closures do not suffer from this political economy constraint because fishermen are able to entirely control their compliance with closures (Paredes, 2014).

The regulator believes the temporary spatial closures policy reduces total juvenile catch. For example, they calculated the 174 closures during the first and second season of 2017 and the first season of 2018 protected 1,049,411 tons of juvenile anchoveta (PRODUCE, 2017a, 2018b, 2018c). The regulator does not describe how they calculate this number, nor do they define the meaning of "protected" in this context. The regulator also implements temporary spatial closures to protect juvenile horse mackerel and may expand the policy to other stocks (PRODUCE, 2020a).

2.3 Industry structure

There is an average of 730 vessels active each fishing season. IVQs preclude entry of new vessels into the fishery. Vessels are made of steel or wood (40% and 60%). On average, steel vessels are longer than wood vessels (37.5 m compared to 17.5 m), have greater storage capacity (354 m^3 compared to 72.5 m^3), and have more powerful engines (797 horsepower compared to 339 horsepower). Steel vessels also have larger crews than wood vessels (about 20 people compared to 12 people on wood vessels) and are more likely to belong to a firm that owns multiple fishing vessels (92% compared to 41% of wood vessels).

All vessels are privately owned. Seven large firms own at least 19 vessels each, which together account for 60.3% of landings (Table A1). All seven large firms are vertically integrated in that they also own fishmeal processing plants (Hansman et al., 2019). 271 vessels are "singletons"; they belong to a firm that owns only one vessel. Singleton vessels account for 12.8% of landings. Finally, there are medium firms that each own 2 to 10 vessels. Vessels that belong to medium firms account for 26.8% of landings. The level of market concentration in Peru's anchoveta fishery is similar to other fisheries with rights-based instruments. For example, the top 10 largest firms in Iceland own quotas equal to 50.5% of annual landings (Agnarsson et al., 2016).

3 Model

I present a simple game theoretic model to interpret my empirical result that the temporary spatial closures policy increases total juvenile catch. The proposed mechanism is that closure announcements are a positive signal of fishing productivity near closures. This mechanism implies an auxiliary prediction regarding treatment effect heterogeneity, which I also test empirically. Namely, vessels that receive a positive information shock from closure announcements will have larger treatment effects than vessels who already had the signal.

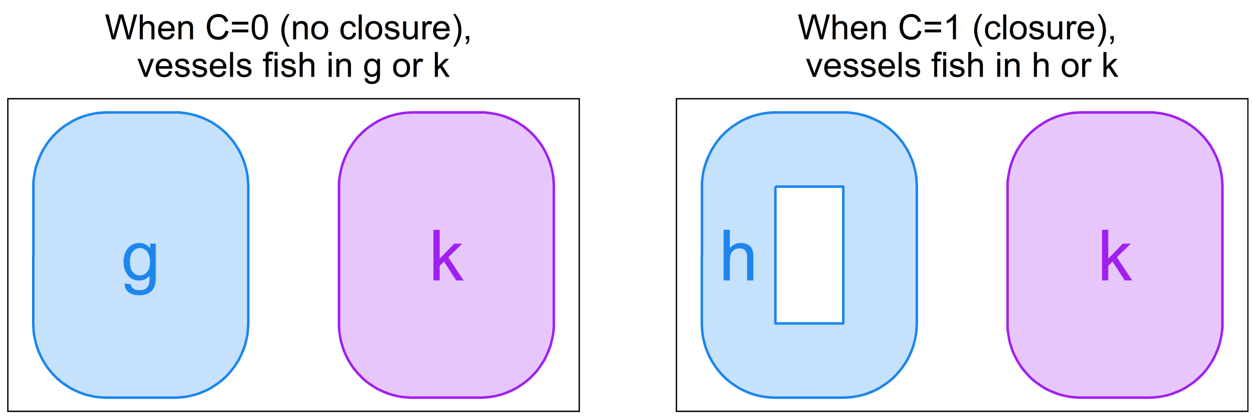
I vessels simultaneously choose where to fish in order to maximize expected profits, which depend on the state variable C.[17] When $C = 0$, there is no closure and vessels choose from two possible fishing locations: g and k. Each vessel i chooses exactly one of the two available fishing locations. When $C = 1$, part of location g is closed to fishing, but $h \subset g$ remains open to fishing (Figure 2). Vessels choose whether to fish in h or k when $C = 1$.[18] Location h represents areas and times that are near closures, such as inside closed areas after closure announcements but before the beginning of closure periods, just outside closed areas during closure periods, and inside closed areas after closure periods end. I derive testable predictions from this model by comparing outcomes across the two values of the state variable C, such as whether the closures policy reduces total juvenile catch (i.e., whether total juvenile catch is lower when $C = 1$ than when $C = 0$).

Let ℓ denote a generic fishing location. Profit π decreases in the number of other vessels who make the same choice, $I_{-i,\ell}$, due to congestion (Huang & Smith, 2014; Smith, 1969). Profit increases in the productivity (e.g. tons per set) of the fishing location, which is summarized by the scalar μ_ℓ. Vessels know that draws of μ_ℓ are independent across locations conditional on C. But vessels do not observe the vector of true productivity $\vec{\mu}$ in the possible fishing locations before making their location choice. For the base case suppose that vessels are identical and that they have the same beliefs regarding the mean productivity of each

[17] This model abstracts away from important institutional details, such as heterogeneity among vessels and dynamic decision-making. Its purpose is to provide a simple, single framework for understanding the three main empirical results of this paper.

[18] Suppose the fine from fishing in the closed part of location g is sufficiently large that expected profit from fishing in h or k is always greater.

Figure 2: Illustration of model



Notes: When $C = 0$ (no closure), vessels choose to fish in location g or in location k. When $C = 1$, part of location g is closed to fishing. Vessels choose to fish in location h (the part of g that remains open to fishing) or in location k.

location (e.g., the value of $\tilde{\mu_k}$ is the same across vessels).

There are two differences when $C = 1$ compared to when $C = 0$. First, the closure announcement is a positive signal to vessels: $\tilde{\mu_h} > \tilde{\mu_g}$. The closure announcement does not change vessels' beliefs regarding mean productivity of location k ($\tilde{\mu}_{k|C=1} = \tilde{\mu}_{k|C=0}$). Second, since location h covers less area than g, marginal congestion costs are higher in h than in g ($|\frac{\partial \pi_{i,h}(\cdot)}{\partial I_{-i,h}}| > |\frac{\partial \pi_{i,g}(\cdot)}{\partial I_{-i,g}}|$).

When there is no closure, vessel i's objective is

$$\max_{\ell \in \{g,k\}} \quad \mathrm{E}[\pi_{i,\ell}(\mu_\ell, I_{-i,\ell})|\tilde{\mu_g}, \tilde{\mu_k}, C = 0].$$

Vessel i chooses to fish in g if the expected profit from doing so exceeds the expected profit from fishing in k; $\mathrm{E}[\pi_{i,g}(\mu_g, I_{-i,g})|\tilde{\mu_g}, \tilde{\mu_k}, C = 0] > \mathrm{E}[\pi_{i,k}(\mu_k, I_{-i,k})|\tilde{\mu_g}, \tilde{\mu_k}, C = 0]$. When part of location g is closed ($C = 1$), vessels choose between h and k to maximize their expected profit, yielding a similar decision rule. Let I_ℓ denote the number of vessels who choose location ℓ and let $TotJuv(C)$ denote total juvenile catch given the value of C. Suppose total juvenile catch is the product of the number of vessels who fish in each location, productivity, and percentage juvenile, summed over locations. Then $TotJuv(C = 0)$ equals $\gamma(I_g\mu_g\rho_g + I_k\mu_k\rho_k)$ and $TotJuv(C = 1)$ equals $\gamma(I_h\mu_h\rho_h + I_k\mu_k\rho_k)$, where γ is a constant and ρ_ℓ is percentage juvenile.[19]

[19] In reality, I calculate the number of juvenile anchoveta caught by each set in Section 4, which forms the main outcome variable of interest in my regressions (Section 5).

There exists unique Bayes-Nash equilibria (I_g^*, I_k^*) and (I_h^*, I_k^*) such that:

Proposition 1. If (1) the closure announcement is a sufficiently large positive signal relative to congestion costs and (2) productivity and percentage juvenile are sufficiently high in location h relative to locations g and k, then the closures policy increases total juvenile catch; $TotJuv(C=1) > TotJuv(C=0)$.

Proposition 2a. When there is no closure, the expected profit from fishing in location g equals the expected profit from fishing in location k; $\mathrm{E}[\pi_{i,g}(\mu_g, I_{-i,g})|\tilde{\mu_g}, \tilde{\mu_k}, C=0] =$
$\mathrm{E}[\pi_{i,k}(\mu_k, I_{-i,k})|\tilde{\mu_g}, \tilde{\mu_k}, C=0]$ $\forall i$. The same is true when $C=1$;
$\mathrm{E}[\pi_{i,h}(\mu_h, I_{-i,h})|\tilde{\mu_h}, \tilde{\mu_k}, C=1] = \mathrm{E}[\pi_{i,k}(\mu_k, I_{-i,k})|\tilde{\mu_h}, \tilde{\mu_k}, C=1]$ $\forall i$.

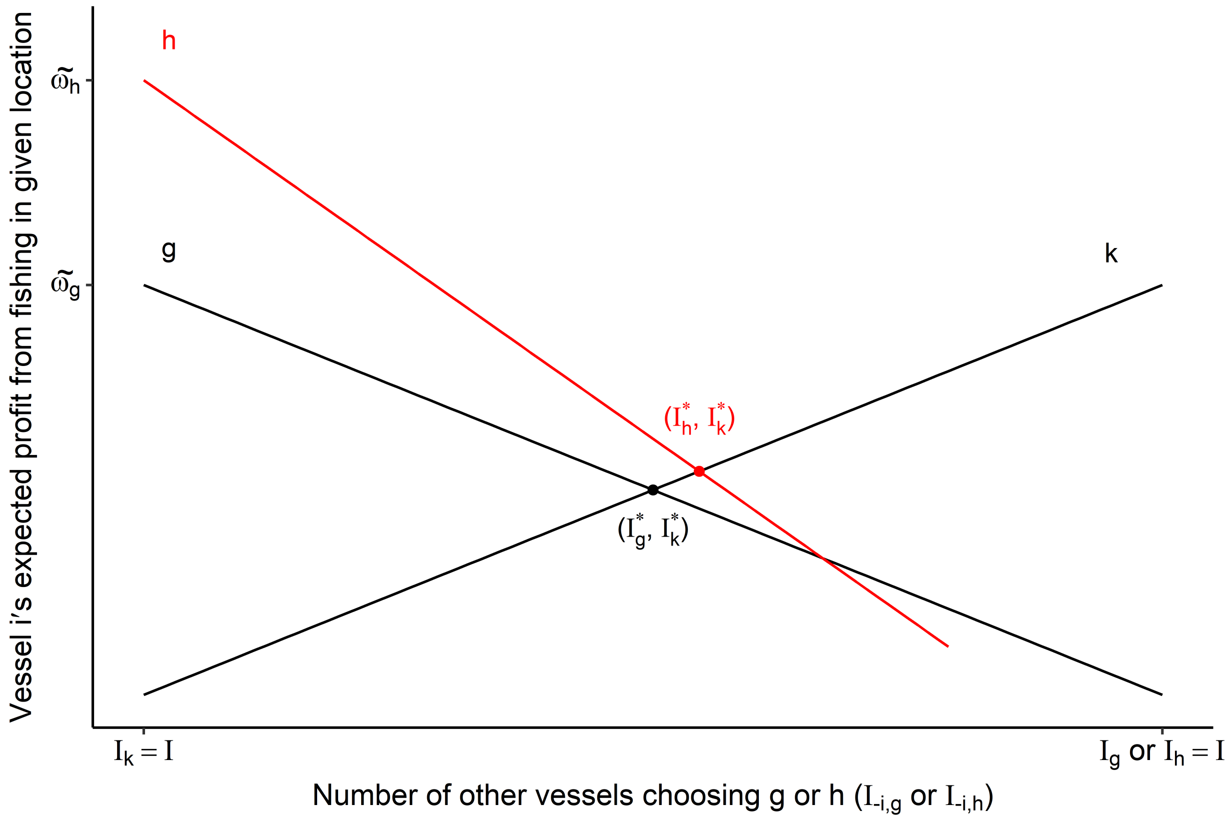
Proposition 2b. However, profit from fishing in g exceeds profit from fishing in k if true, unobservable productivity is higher in g than in k $(\mu_g > \mu_k)$ and vessels believe mean productivity is the same in both locations $(\tilde{\mu_g} = \tilde{\mu_k})$; $\pi_{i,g}(\mu_g, I_{-i,g}) > \pi_{i,k}(\mu_k, I_{-i,k})$ $\forall i$.

The proofs are in Appendix D. Figure 3 displays the equilibria when $\mathrm{E}[\pi_{i,\ell}(\mu_\ell, I_{-i,\ell})|\vec{\tilde{\mu}}, C] = \tilde{\mu_\ell} - \alpha_\ell I_{-i,\ell}$, where α_ℓ is the cost to vessel i from one additional vessel fishing in location ℓ. Vessel i's expected profit (y-axis) depends on its choice (lines) and the choices of the other I_{-i} vessels (x-axis). Consider Proposition 2a first. When $C=0$, the equilibrium (I_g^*, I_k^*) is given by the intersection of the two black lines, which represent vessel i's expected profit from fishing in g and expected profit from fishing in k. At this point, no vessel can increase their expected profit by changing their location choice. Similarly, when $C=1$ the equilibrium (I_h^*, I_k^*) is given by the intersection of the red line (expected profit from fishing in location h) and the upward-sloping black line (expected profit from fishing in location k).

Now consider Proposition 1 as illustrated in Figure 3. Expected profit from fishing in h has a higher intercept than for g, because the closure announcement is a positive signal of productivity, but it also has a steeper slope, because marginal congestion costs are higher $(\alpha_h > \alpha_g)$. Figure 3 displays the case where the positive signal is sufficiently large relative to congestion costs, such that more vessels choose to fish in h in equilibrium than in g $(I_h^* > I_g^*)$, even though h is a subset of g. In order for this increase in fishing near closures to translate into an increase in total juvenile catch, it must also be the case that productivity and percentage juvenile in h are sufficiently high relative to productivity and percentage juvenile in g and k. Because I is fixed, $I_h^* > I_g^* \Rightarrow I_{k|C=1}^* < I_{k|C=0}^*$. If productivity and percentage juvenile are the same across locations, the closures policy will not increase total juvenile catch because the vessels who switch from k to h catch the same quantity of juveniles in both locations.[20] Whether the temporary spatial closures policy increases total juvenile

[20]Fixed I in the model is similar to the role that the total allowable catch limit plays in mediating the effect of the closures policy: if the closures policy increases tons caught near closures, then tons caught elsewhere in the same season must fall by an offsetting amount.

Figure 3: Illustration of Propositions 1 and 2a



Notes: The y-axis is vessel i's expected profit when $\mathrm{E}[\pi_{i,\ell}(\mu_\ell, I_{-i,\ell})|\tilde{\vec{\mu}}, C] = \tilde{\mu_\ell} - \alpha_\ell I_{-i,\ell}$. The x-axis is the number of other vessels who choose g when $C = 0$ and the number of other vessels who choose h when $C = 1$. The black and red lines indicate vessel i's expected profit from fishing in a given location. The black point is the Nash equilibrium when $C = 0$ and the red point is the Nash equilibrium when $C = 1$. In this parametric example, $I^*_h > I^*_g$ because the closure announcement is a sufficiently large positive signal (difference in intercepts) relative to congestion costs (difference in slopes of lines).

catch is therefore an empirical question. This outcome is possible, but only if (1) closure announcements are a sufficiently large positive signal relative to congestion costs and (2) productivity and percentage juvenile near closures are sufficiently high.

For Proposition 2b, note that fishing location decisions depend on vessels' beliefs regarding mean productivity in each location ($\tilde{\vec{\mu}}$), but not true productivity $\vec{\mu}$ because $\vec{\mu}$ is unobserved. Then $\pi_{i,g}(\mu_g, I_{-i,g}) > \ \pi_{i,k}(\mu_k, I_{-i,k}) \ \ \forall i$ because vessels are identical and profit is increasing in true productivity. If the closure announcement contains valuable information in that it informs vessels that the true productivity of g is higher than k, then vessels that happen to fish in g when $C = 0$ have higher profits because there is no closure announcement that vessels can use to change their fishing location decisions.

The information mechanism proposed in this model also implies a prediction regarding treatment effect heterogeneity. Instead of assuming identical vessels, now suppose there are two types of vessels. When $C = 0$, type a vessels already have the signal regarding location g, but type $-a$ vessels do not: $\tilde{\mu}_{g,a} > \tilde{\mu}_{g,-a}$. When $C = 1$, both types receive the positive signal from the closure announcement, so $\tilde{\mu}_{h,a} = \tilde{\mu}_{h,-a}$. The closures policy treatment effect $\tau = TotJuv(C = 1) - TotJuv(C = 0)$ and the treatment effect as a percentage of the number of type a vessels is $\frac{\tau_a}{I_a}$. Because type $-a$ vessels receive a positive information shock from the closure announcement and type a vessels do not, the percentage treatment effect for type $-a$ vessels will be larger than the percentage treatment effect for type a vessels.

Proposition 3. Consider two types of vessels, indicated by the subscript a. Suppose $\tilde{\mu}_{g,a} > \tilde{\mu}_{g,-a}$ when $C = 0$, $\tilde{\mu}_{h,a} = \tilde{\mu}_{h,-a}$ when $C = 1$, juvenile catch per vessel is higher in location g than in location k ($\mu_g \rho_g > \mu_k \rho_k$), and an interior Bayes-Nash equilibrium when $C = 0$ ($I_g^*, I_k^* > 0$) and when $C = 1$ ($I_h^*, I_k^* > 0$). Then type $-a$ vessels have a larger percentage treatment effect than type a vessels; $\frac{\tau_{-a}}{I_{-a}} > \frac{\tau_a}{I_a}$.

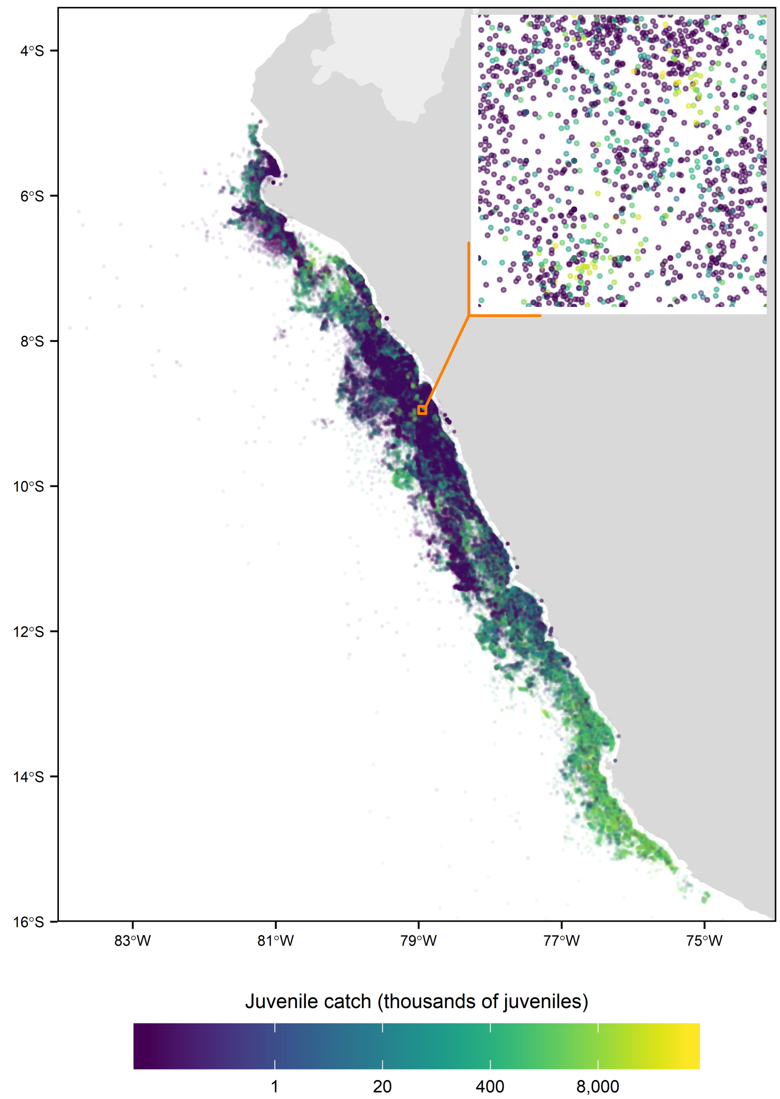
The proof is in Appendix D. If some vessels receive a positive information shock from the closure announcement and others do not, total juvenile catch will increase by a larger percentage among vessels that receive the information shock.

4 Data

The recent emergence of vessel-level GPS data has enabled researchers to predict when and where vessels are fishing at a global scale (Kroodsma et al., 2018). These new data, made publicly available by the organization Global Fishing Watch, have expanded the set of answerable research questions (Englander, 2019; Sala et al., 2018), but they do not measure the most important outcomes caused by fishing: the quantities and types of fish that vessels catch. As a result of fieldwork I conducted in Peru, I obtained two administrative datasets that contain these variables from Peru's Ministry of Production (PRODUCE) in March 2020. Both datasets contain the tons of anchoveta and the percentage juvenile caught by all vessels in the North-Central zone during the six fishing seasons of 2017, 2018, and 2019. However, they differ in important ways which allow me to accurately calculate the number of juvenile anchoveta each vessel catches each time it sets its net (an individual fishing operation).

The first dataset is *electronic logbook data*. Fishermen report to the regulator when a fishing trip begins, when a fishing trip ends, and the location, time, tons caught, and percentage juvenile caught from each set during a fishing trip. Fishermen record this information on a smartphone or tablet application, which transmits data in real-time to the regulator

Figure 4: Electronic logbook data, 2017 to 2019



Notes: Each point is a set (vessel-level fishing operation). The color of each point is the number of juvenile anchoveta caught by that set, which I calculate by matching sets to landing events and using the percentage juvenile measured by third-party inspectors at landing. There are 246,914 sets reported by 806 unique vessels in the electronic logbook data. All vessels are prohibited from fishing within 5 nautical miles (9.3 km) of the coast. There are 572 sets per day on average during a fishing season. The average set catches 570,103 juvenile anchoveta. The inset map in the top, right panel magnifies the sets that occur inside the orange rectangle.

through the vessel's on-board GPS transponder. Fishermen perform sets once they have located anchoveta in the water. They encircle the anchoveta with a large net (a "purse seine"), close the net, and transfer the anchoveta from the net into the vessel's hold. As fishermen transfer anchoveta from the net into the hold, a trained fisherman estimates the percentage of juveniles by taking three samples using a standardized bucket: once during the first 30% of transference and two more times during the remaining 70% (PRODUCE, 2016b). The fisherman measures each fish in the sample in half-cm intervals, producing data on the length distribution of anchoveta caught by that set (e.g., 10 individuals between 11 and 11.5 cm, 17 individuals between 11.5 and 12 cm, etc.). Out of the several million individual anchoveta caught in a typical set, approximately 200 are measured. The percentage juvenile for the set is the percentage of measured individuals that are less than 12 cm. Percentage juvenile is reported to the regulator but the length distribution is not. I obtained a supplementary electronic logbook dataset for a group of vessels that report length distribution data to their owners. These vessels represent 55% of tons landed and I imputed the length distribution for sets by other vessels given sets' location, time, and percentage juvenile (see Appendix C).

The second dataset is *landings data*. When vessels finish fishing, they return to shore and transfer ("land") the anchoveta they caught on their trip to a fishmeal and fish oil processing plant. Each time a vessel lands its catch at a processing plant, a third-party inspector measures percentage juvenile and tons landed and reports this data to the regulator. The third-party inspector follows the same procedure described above, taking three samples and measuring approximately 200 individuals in total. The landings data are lower resolution than the electronic logbook data because third-party inspectors measure percentage juvenile from the sum of anchoveta caught by all sets on a fishing trip (average number of sets per trip is 2.3), whereas fishermen measure percentage juvenile after each set in the electronic logbook data. However, unlike fishermen in the electronic logbook data, the closures policy does not give third-party inspectors an incentive to misreport percentage juvenile, because the regulator does not use landings data to determine closures during my study period. Third-party inspectors are from one of three international firms and tend to have more rigorous technical training in measurement and sampling than fishermen (PRODUCE, 2018d).

I match sets in the electronic logbook data to landings in the landings data at the vessel-trip level and use the percentage juvenile measured by third-party inspectors and length distribution data to calculate the number of juvenile anchoveta caught by each set in the electronic logbook data (Appendix C). I also use the percentage juvenile measured by third-party inspectors to calculate corrected length distribution for each set. If I did not have landings data, I would mismeasure juvenile catch because fishermen seem to underreport percentage juvenile in the electronic logbook data. The weighted average percentage juvenile is 40% lower in the electronic logbook data than in the landings data (11% compared to 18.3%).[21] Fishermen might underreport percentage juvenile to avoid triggering a closure in

[21]Weights are the number of individual anchoveta caught by each set or landed in each landing event.

the area they are fishing.[22] This phenomen also occurs in other settings where agents may be regulated as a consequence of the data they report, such as industrial plants in India and car owners in Mexico (Duflo et al., 2013; Oliva, 2015). Matching the electronic logbook and landings data preserves the resolution of the electronic logbook data while ensuring that the outcome variable in my main regression—juvenile catch at a given location and time—is not systematically manipulated.

There are 246,914 sets reported by 806 unique vessels in the electronic logbook data. 95% of sets occur within 80 km of the coast (Figure 4). During a fishing season there are 572 sets per day on average. The average set catches 570,103 juvenile anchoveta and 2,508,791 adult anchoveta, which together weigh 50.2 tons. Fishermen do not underestimate tons caught in the electronic logbook data, perhaps because this variable has little effect on whether a closure is declared (Section 2.2).[23] The median anchoveta caught is between 13 and 13.5 cm long (Figure C1).

As discussed in Section 2.2, I downloaded all closures announcements from the regulator's website. Closure announcements are pdf documents containing the areas and time periods vessels are not allowed to fish. I geo-coded closure boundaries and recorded the time each closure begins and ends, creating a complete digital record of the 410 temporary spatial closures during my study period. In the next section, I will detail how I use the electronic logbook data to construct "potential closures". I use potential closures to identify the effect of the temporary spatial closures policy on juvenile catch, to test whether the policy implicitly provides valuable information to fishermen, and to estimate whether this information is a mechanism underlying the policy's effects on juvenile catch.

5 Empirical strategy

This paper first seeks to quantity the total effect of the temporary spatial closures policy on juvenile catch, including the policy's direct, temporal spillover, and spatial spillover effects. After doing so in Section 6, I explore a mechanism underlying the policy's effects in Sections 7 and 8.

I observe juvenile catch inside closures during closure periods, as well as juvenile catch

[22] Closures provide valuable information to fishermen regarding the location of anchoveta, but only to fishermen who did not recently fish in that area (Section 8).

[23] 8% more tons are reported in the electronic logbook data than are measured at landing. This difference is within the range at which fish can degrade or be lost between being caught at sea and landed (Getu et al., 2015). Fishermen have little incentive to bias their estimates of tons caught in the electronic logbook data. Tons measured at landing, rather than tons estimated by fishermen in the electronic logbook data, are the data used by processing plants to pay fishermen and by the regulator to determine when a vessel has reached its quota for the season.

before, nearby, and after closures, but I do not observe what juvenile catch would have been in those same places and times in a counterfactual world without the temporary spatial closures policy (Holland, 1986). If the regulator randomly assigned closures, I could estimate the effect of the policy by comparing juvenile catch in treated areas to control areas. In reality, the variables that affect the probability of closure—percentage juvenile caught by vessels before closure announcements—are correlated with the main outcome variable of interest—the number of juvenile individuals caught after closure announcements. My solution to this causal inference challenge is to generate "potential closures" via an algorithm that mimics the regulator's closure rule and takes as its input the same data the regulator uses to determine closures. I intersect potential closures with the closures declared by the regulator, yielding treatment areas (potential closures that get closed) and control areas (potential closures that do not get closed). Potential closures are the unit of observation. I estimate whether juvenile catch is different inside treated potential closures compared to control potential closures—the direct effect of the policy—as well as whether juvenile catch is different before, just outside, and after treated potential closures compared to control potential closures—the temporal and spatial spillover effects of the policy.

The unconditional difference in juvenile catch from comparing treated potential closures to control potential closures is likely biased upward relative to the causal effect of the policy because potential closures that would have had high juvenile catch independent of treatment are more likely to be treated. For example, pre-period juvenile catch is 28% higher inside treated potential closures compared to control potential closures. As discussed below in Section 5.2, I adjust for the systematic differences between treated and control potential closures by including potential closure-level control variables and flexible fixed effects in my regressions, which balances treated and control potential closures on pre-period juvenile catch levels, pre-period juvenile catch trends, and observable measures of fishing productivity. Identification occurs from comparing potential closures that are equally desirable fishing locations and contain similar concentrations of juveniles, but which the regulator believes are different because the data available to the regulator is lower resolution.

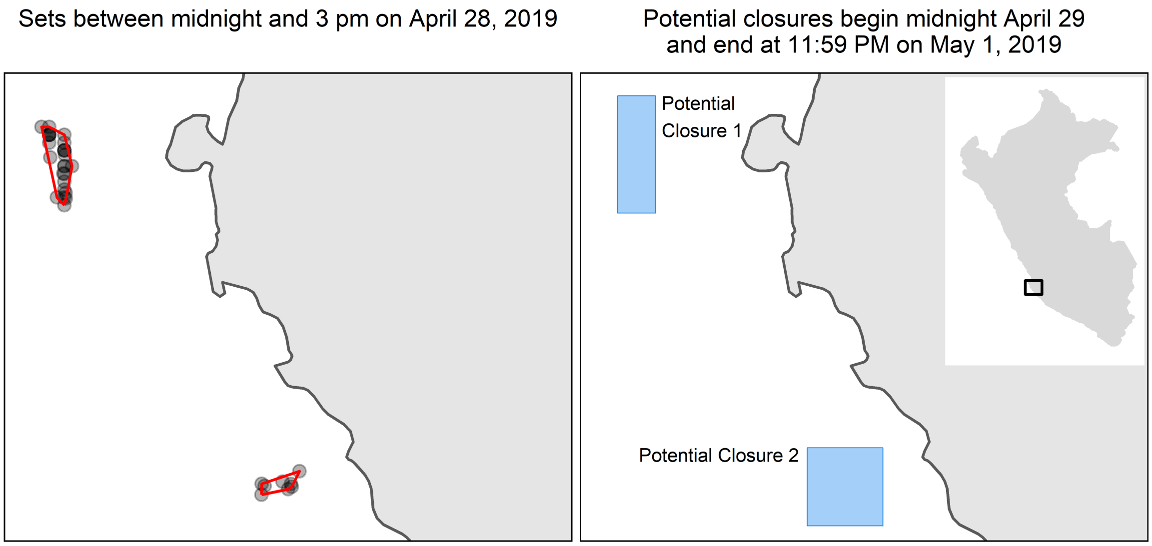
I also estimate the effect of the temporary spatial closures policy with a more standard approach, where the unit of observation is a .05° grid cell by three-hour period of time (Appendix B.3). In this model, any .05° grid cell by three-hour period of time has the potential to be treated. The results from this regression using rasterized data display the same pattern of treatment effects as my estimates using potential closures, but potential closures offer three advantages. First, potential closures make the valuable information provided by closures observable econometrically (Section 7). In the regression using rasterized data, vessels can compete away the value of information provided by closures because closure announcements are public information, making the value of this information unobservable econometrically. This limitation applies to any estimation approach that does not explicitly include counterfactual closures (areas that could have been but were not declared closures by the regulator). Second, potential closures collapse the data to the policy-relevant unit of observation; the regulator is choosing to close an area of ocean for three to five days, not an individual grid cell. Finally, collapsing the data makes the mechanism test (whether the information provided by closures increases spillovers) and the construction of control variables computationally feasible, in part because the rasterized data contains nearly 100 million observations.

5.1 Potential closures

As detailed in Section 2.2, the regulator uses real-time electronic logbook data to determine closures. When a government official wants to create a closure, they select a group of sets that occurred near each other during the same time period. A computer algorithm then calculates the exact boundaries and number of days the resulting closure will last. The first step of my empirical strategy is to develop an algorithm that mimics the first stage of the closure rule, when the official selects a group of sets (vessel-level fishing observations) on their computer. I cluster sets in the electronic logbook data that occur near each other and record the bounding box around each cluster. The resulting rectangles are "potential closures". Unlike the regulator, I create potential closures from *every* cluster of sets. I do not attempt to reproduce the second stage of the closure rule, when the computer algorithm determines the exact boundaries and time length of a closure, because this algorithm depends on variables endogenous to my outcome of interest, such as the percentage juvenile values in the cluster of sets reported to the regulator.

First, I use the single-linkage clustering algorithm to group sets that occur within 5 nautical miles of each other on the same day between midnight and 3 PM (R Core Team, 2019). I choose this time period because closures that begin at midnight (91% of closures during my study period) must be announced by 3 PM (9 hours in advance). The remaining 9% of closures begin at 6 AM and must be announced by 6 PM of the previous day. I use a 5 nautical mile threshold in the single-linkage clustering algorithm because the regulator's algorithm rounds the boundaries of rectangular closures to the nearest 5 nautical mile interval (Instituto Humboldt & SNP, 2017). Then for each cluster containing more than three sets, I draw a rectangle to cover its convex hull (the smallest convex polygon that encloses the cluster), rounded up to the nearest 5 nautical mile interval. I drop all potential closures that are smaller than the smallest closure declared by the regulator that season.

Figure 5: Creation of potential closures example



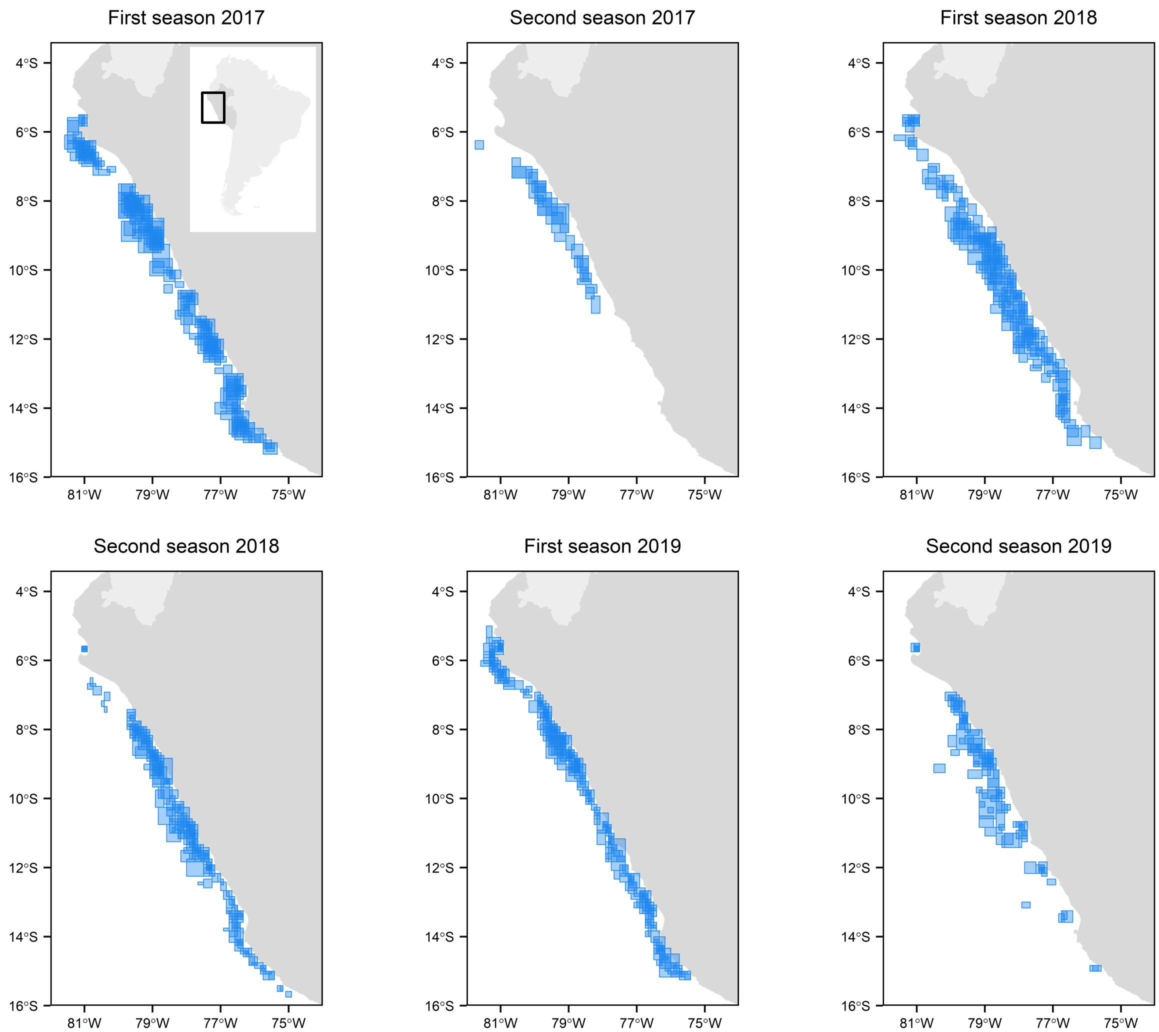
Notes: (a) Sets that occur between midnight and 3 pm on April 28, 2019 in one region of the North-Central zone are displayed as points. The single-linkage clustering algorithm groups these sets into two clusters. The red polygons enclosing each cluster are the clusters' convex hulls. (b) Potential closures are rectangles covering clusters' convex hulls, rounded up to the nearest 5 nautical mile interval. Rectangular closures declared by the regulator are also rounded to the nearest 5 nautical mile interval. These potential closures begin at midnight on April 29, 2019 and last for three days. The inset map in the upper right corner shows Peru (dark grey) and the region these potential closures occur in (black rectangle).

As an illustrative example, Figure 5 displays sets from the electronic logbook data in one region of the fishery between midnight and 3 pm on April 28, 2019. The single-linkage clustering algorithm creates two clusters from these sets (Figure 5a). Figure 5b displays the potential closures that result from these clusters.

I assume all potential closures last for three days, which is the modal length of closures declared by the regulator (the regulator can also declare closures that last four or five days). Since a closure cannot be declared in the same place and time as an already-existing closure, I loop forward in time and subtract areas of potential closures that overlap with already-existing potential closures. I drop potential closures that have become non-convex or smaller than that season's smallest closure after this procedure.

My potential closures algorithm generates 969 potential closures in total, compared to 410 actual closures declared by the regulator during my study period. 89 percent of actual closures have positive overlap with a potential closure (intersect in space at the same time). The average potential closure is smaller than the average actual closure (957 km^2 compared to 1,328 km^2). Figure 6 displays the potential closures in each fishing season. My results

Figure 6: Potential closures in the North-Central zone by fishing season



Notes: Blue polygons are potential closures. Potential closures last three days by assumption. The average potential closure is 957 km^2. My potential closures algorithm generates 969 potential closures in the North-Central zone during the six fishing seasons of 2017, 2018, and 2019, compared with 410 actual closures declared by the regulator (Figure 1). There are 1.72 active potential closures on an average day during the fishing season. The inset map in the top, left panel shows South America (light grey), Peru (dark grey), and the North-Central zone (black rectangle).

are robust to a variety of alternative specifications, such as assuming potential closures last for four days instead of three days, assuming potential closures last for five days, and making potential closures 40% larger so that they are the same average size as actual closures (Appendix B.2). My results are also robust to estimating the effect of the policy via synthetic controls, where actual closures (treatment units) are matched to potential closures (control units) (Appendix B.4).

5.2 Outcome, treatment, control variables, and identifying variation

The main outcome of interest is juvenile catch inside potential closures. In Figure 5, juvenile catch is the number of juvenile anchoveta that are caught inside each blue rectangle from midnight on April 29, 2019 until 11:59 PM on May 1, 2019. I filter sets to those that occur inside a potential closure during these three days. Then I sum juvenile catch over sets that occur inside the same potential closure. For example, suppose there are two sets that each catch 1 million juveniles inside Potential Closure 1 between midnight on April 29, 2019 and 11:59 PM on May 1, 2019. Then juvenile catch for Potential Closure 1 is 2 million juveniles. Note that the three days of Potential Closure 1 occur after the sets that generated Potential Closure 1 (midnight to 3 PM on April 28, 2019). They represent the time period that Potential Closure 1 would be closed if the regulator decided to create an actual closure based on the sets that occurred between midnight and 3 PM on April 28, 2019.

I define treatment by the intersection of potential closures with actual closures declared by the regulator. Specifically, I compute the average spatial and temporal overlap between potential closures and actual closures. For example, a potential closure that shares 60% of its area and is active for two of the three same days as an actual closure would have a treatment fraction of .4 (60% spatial overlap $\times$ two-thirds temporal overlap = .4). If a potential closure intersects multiple actual closures, I compute the treatment fraction with each actual closure and record the sum of treatment fractions. The average treatment fraction for potential closures is .2 (median is 0).

The most important control variables in my regressions are the length distribution of anchoveta caught by the sets that generate potential closures (e.g. 1% of individuals caught were between 10 and 10.5 cm, 1.4% caught were between 10.5 and 11 cm, etc.). Controlling for length distribution is akin to controlling for the probability distribution function from which percentage juvenile values for those sets are drawn. Sets that generate the same potential closure are "drawing" percentage juvenile values from the same length distribution because they occur near each other during the same 15 hour time period; they are fishing from the same local anchoveta population. The percentage juvenile values reported to the regulator affect the probability a potential closure is declared an actual closure; length distribution does not because fishermen do not report length distribution to the regulator (Section 4). I do not control for the percentage juvenile values reported to the regulator in order to preserve treatment variation. By instead controlling for length distribution, the identifying variation comes from comparing potential closures that by chance had higher percentage juvenile draws (so were declared closures by the regulator) to potential closures that by chance had lower percentage juvenile draws (so were not declared closures by the regulator). There is variation in percentage juvenile draws conditional on potential closure-level length distribution because of the sampling procedure discussed in Section 4, wherein fishermen estimate percentage juvenile by measuring 200 anchoveta out of the several million anchoveta caught in a typical set. One way to quantify this variation is by regressing set-level percentage juvenile reported to the regulator on potential closure-level length distribution, for the subset of sets that generate potential closures. The R^2 from this regression is 0.45, indicating ample identifying variation.[24]

I control for length distribution by calculating the weighted-average proportion of anchoveta individuals in each half-cm length interval among the sets that generate potential closures, where the weights are the total number of anchoveta individuals caught by each set. Recall that these sets occur before potential closures begin. For example, the weighted-average proportion of anchoveta in each length interval for potential closures in Figure 5b is constructed from sets that occur between midnight and 3 pm on April 28, 2019 (Figure 5a).

In Appendix C I detail how given sets' location, time, and percentage juvenile, I impute the length distribution for sets from vessels that do not report length distribution to their owners. I use percentage juvenile measured by third-party inspectors to calculate corrected length distributions for all sets. I control for corrected length distributions in my regressions so that potential misreporting to vessel owners does not bias my results.[25] My regressions thus compare potential closures whose true anchoveta populations are similar, but which the regulator believes are different because the regulator does not use length distribution or third-party inspector data when making closure decisions.

Controlling for the length distribution adjusts for differences in the size-structure of anchoveta populations across potential closures, but not for differences in fishing productivity or fishing costs across potential closures. If treated potential closures are more desirable fishing locations, either because anchoveta are more abundant or because fishing costs are lower, juvenile catch would be higher (all else equal) in treated potential closures independent of treatment because total catch would be higher. I avoid bias due to differences in fishing productivity and fishing costs by controlling for the number of sets that generate each potential closure, the total tons caught by the sets that generate each potential closure, the size of each potential closure in km^2, the distance of each potential closure's centroid to Peru's coast in km, tons caught per set among the sets that generate each potential closure, and tons caught per km^2 among the sets that generate each potential closure. The number

[24] Additionally, for the subset of sets that generate potential closures, the standard deviation in percentage juvenile reported to the regulator is 16, compared to within-potential closure standard deviation of 11.2.

[25] I also use the corrected number of individuals caught by each set in calculating the average length distribution for each potential closure.

of sets, tons caught, and potential closure area are proxies for anchoveta abundance, distance to the coast is a proxy for fishing costs, and tons per set and tons per km^2 are proxies for fishing productivity.

Finally, I include in my regressions two-week-of-sample by two-degree grid cell fixed effects and day-of-sample fixed effects (defined by the centroid or date a potential closure begins). The first set of fixed effects ensure identification comes from comparing potential closures that occur near each other during a similar time period. On average, there are 4.4 potential closures per two-week-of-sample by two-degree grid cell. The day-of-sample fixed effects control for aggregate juvenile catch and fishing productivity that day, as well as the number and size of active closures declared by the regulator.

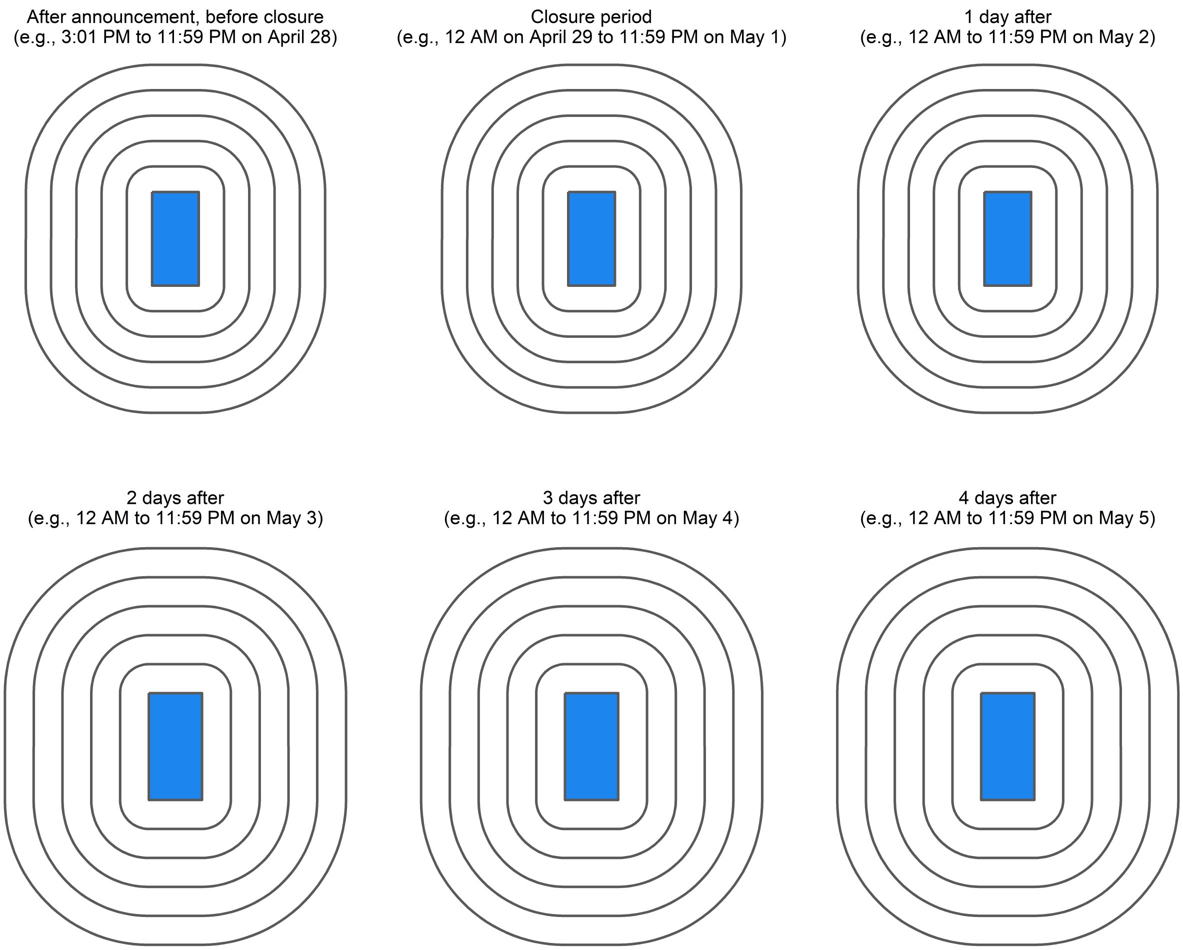
The fixed effects and control variables described in this section balance treated and control potential closures on pre-period juvenile catch levels, pre-period juvenile catch trends, and observable measures of fishing productivity (Appendix B.1).

5.3 Spatial and temporal spillover bins

The temporary spatial closures policy may reduce juvenile catch inside closed areas during the active closure period (the direct effect). But it could also cause spillovers over space or time: changes in juvenile catch because of the policy outside the closed area or outside the closure period. Estimating these spatial and temporal spillovers in addition to the direct effect of the policy is critical because both vessels and anchoveta move. Instead of fishing inside active closures, vessels could fish inside closed areas after closure announcements but before the beginning of closure periods, outside closed areas during closure periods, or inside closed areas after closure periods have ended. All of these types of fishing reallocation do not violate the policy. Moreover, closures need not merely reallocate fishing. If closures are a sufficiently large positive signal of fishing productivity, they could also increase the total quantity of fishing that occurs near closures (Section 3).

The "treatment window" over which I allow the policy to affect juvenile catch is from nine hours before a potential closure begins until four days after a potential closure has ended, within 50 km of the potential closure. I chose this treatment window empirically; it is large enough to observe the effect of the temporary spatial closures policy fully dissipate over both space and time (Section 6). There are 6 time periods of interest for each potential closure: 9 hours before the potential closure begins, the three day period in which the potential closure is active, and 1, 2, 3, and 4 days after the potential closure has ended. For each time period, there are 6 spatial units of interest: inside the potential closure, 0 to 10 km outside the potential closure, 10 to 20 km outside the potential closure, 20 to 30 km outside the

Figure 7: Treatment window over which the temporary spatial closures policy can affect juvenile catch



Notes: There are 36 treatment bins in the treatment window: 6 time periods × 6 spatial units. The inside potential closure spatial unit is blue and the other five spatial units (spatial rings) are white.

potential closure, 30 to 40 km outside the potential closure, and 40 to 50 km outside the potential closure. There are thus 36 "treatment bins" of interest (6 time periods × 6 spatial units). Since there are 969 potential closures, there are 34,884 potential closure-treatment bin observations. Figure 7 visualizes the 36 treatment bins in the treatment window. I refer to the spatial units outside the potential closure as "rings". For example, the 10 km ring is the 0 to 10 km outside the potential closure unit.

The original potential closure is the three day closure period, inside the potential closure treatment bin. I calculate treatment fraction and juvenile catch for the other 35 treatment bins in the same way that I calculate them for this treatment bin (Section 5.2). To calculate treatment fraction, I create the same spatial and temporal leads and lags for each actual closure declared by the regulator. Then I compute the treatment fraction of each potential closure-treatment bin with the same treatment bin of actual closures.[26] I calculate juvenile catch inside each potential closure-treatment bin by summing juvenile catch over sets that occur inside the same potential closure-treatment bin.[27] Finally, the control variables are defined at the level of a potential closure; their values are the same for all treatment bins for a given potential closure.[28]

5.4 Estimating equation

I estimate whether the temporary spatial closures policy affects juvenile catch with the following ordinary least squares regression:

$$\begin{aligned} JuvenileCatch_{ist} =& \alpha_{st} + \beta_{st} TreatFraction_{ist} + \sum_{\ell=[3,3.5)}^{[18.5,19)} \xi_\ell Prop_{i\ell} \\ &+ \gamma_1 Sets_i + \gamma_2 Tons_i + \gamma_3 Area_i + \gamma_4 DistToCoast_i \\ &+ \gamma_5 TonsPerSet_i + \gamma_6 TonsPerArea_i + \sigma_{wg} + \delta_d + \epsilon_{ist} \end{aligned} \tag{1}$$

where i = potential closure, s = spatial unit, t = time period, ℓ = half-cm length interval, w = two-week-of-sample, g = two-degree grid cell, and d = day-of-sample. I defined the construction of the data and all variables in this equation in Sections 5.1 to 5.3.

The outcome variable is the inverse hyperbolic sine of millions of juveniles caught in each potential closure-treatment bin. The inverse hyperbolic sine transformation allows coefficients to be interpreted in elasticity terms, but unlike a logarithmic transformation allows zero values (Bellemare & Wichman, 2020). The $Prop_{i\ell}$ terms are the proportion of anchoveta individuals in each half-cm length interval ℓ that are caught by the sets that generate potential closure i. Recall from Section 5.2 that these sets, from which $Sets_i$, $Tons_i$, $Area_i$, $Dist2Coast_i$, $TonsPerSet_i$, and $TonsPerArea_i$ are also defined, occur before the treatment window for potential closure i begins.

The coefficients of interest are β_{st}, which measure the effects of the temporary spatial closure policy on juvenile catch. The identifying variation is across potential closures, within the same treatment bin and conditional on the fixed effects and potential closure-level con-

[26] Some potential closure-treatment bins partially overlap with each other (cover the same area during the same time period). However, this overlap is uncorrelated with treatment fraction, both unconditionally and conditional on the control variables and fixed effects in Equation 1. This non-correlation indicates that overlap between potential closure-treatment bins does not bias my estimated treatment effects.

[27] Some sets occur inside multiple potential closure-treatment bins. I correct for this "double-counting" when estimating the effect of temporary spatial closures on juvenile catch (detailed in Footnote 29).

[28] However, the fixed effects are specific to each potential closure-treatment bin.

trols. For example, comparing 10 km-wide rings around potential closures that begin on the same day within the same two-week-of-sample by two-degree grid cell and conditional on potential-level controls, $\beta_{s=10,t=closure\ period}$ captures the change in juvenile catch 10 km outside closures during the closure period that is due to treatment. Potential closures are balanced on pre-period juvenile catch (levels and trends) and on observable measures of fishing productivity, conditional on fixed effects and potential closure-level controls (Appendix B.1). Identification occurs from comparing potential closures that are equally desirable fishing locations and contain similar concentrations of juveniles, but which the regulator believes are different because the data available to the regulator is lower resolution (Section 5.2).

I cluster standard errors at the level of two-week-of-sample by two-degree grid cell. I cluster at this level because it is greater than the level at which treatment is assigned: the 50 km ring around the largest closure (potential or actual) is smaller than a two-degree grid cell and the maximum temporal window over which closures affect juvenile catch is less than two weeks (Abadie et al., 2017). I drop 20 potential closures that do not have length distribution data because there were no sets from vessels that report length distribution data to their owner in the same two-week-of-sample by two-degree grid cell (Appendix C). There are 256 clusters and 34,164 observations when I estimate Equation 1 (949 potential closures $\times$ 36 treatment bins).

6 Does the temporary spatial closures policy reduce total juvenile catch?

I estimate the effect of the temporary spatial closures policy on juvenile catch with Equation 1, convert the treatment coefficients into levels, and compute standard errors using the delta method.[29] Figure 8 shows the main result of this paper. The y-axis is the change in the number of juveniles caught because of the policy and the x-axis is the treatment bin. I estimate the effect of the policy with a single regression (Equation 1) but plot the results in

[29] Figure B2 displays the treatment coefficients (the β_{st} terms in Equation 1). I convert the treatment coefficients into levels as follows. First, I convert the treatment coefficients into percent changes with the transformation $exp(\beta_{st}) - 1$. The percentage change in juvenile catch in treatment bin st equals ($ObservedJuvenileCatch_{st}$ − $CounterfactualJuvenileCatch_{st}$) divided by $CounterfactualJuvenileCatch_{st}$. $ObservedJuvenileCatch_{st}$ is the total juvenile catch that occurs in the data in bin st, multiplied by the ratio of total juvenile catch observed anywhere to $ObservedJuvenileCatch_{st}$ summed over all treatment bins. This ratio is .394; many potential closure-bins are overlapping so I rescale $ObservedJuvenileCatch_{st}$ to avoid artificially inflating observed juvenile catch. Then I re-arrange terms and calculate $CounterfactualJuvenileCatch_{st} = \frac{ObservedJuvenileCatch_{st}}{exp(\beta_{st})}$. Then the change in juvenile catch in bin st in levels is $ObservedJuvenileCatch_{st} - CounterfactualJuvenileCatch_{st}$.

six separate subfigures, with one subfigure for each of the six time periods in my treatment window.

After the announcement of closures but before the beginning of closure periods, juvenile catch increases by 1.2 billion inside soon-to-be closed areas (leftmost point in Figure 8a). There is no change in juvenile catch outside closed areas before the beginning of closure periods. Vessels catch more juveniles in the places where fishing will soon be temporarily banned (inside closed areas), but they do not catch more juveniles in the places where fishing will be allowed to continue (outside closed areas).

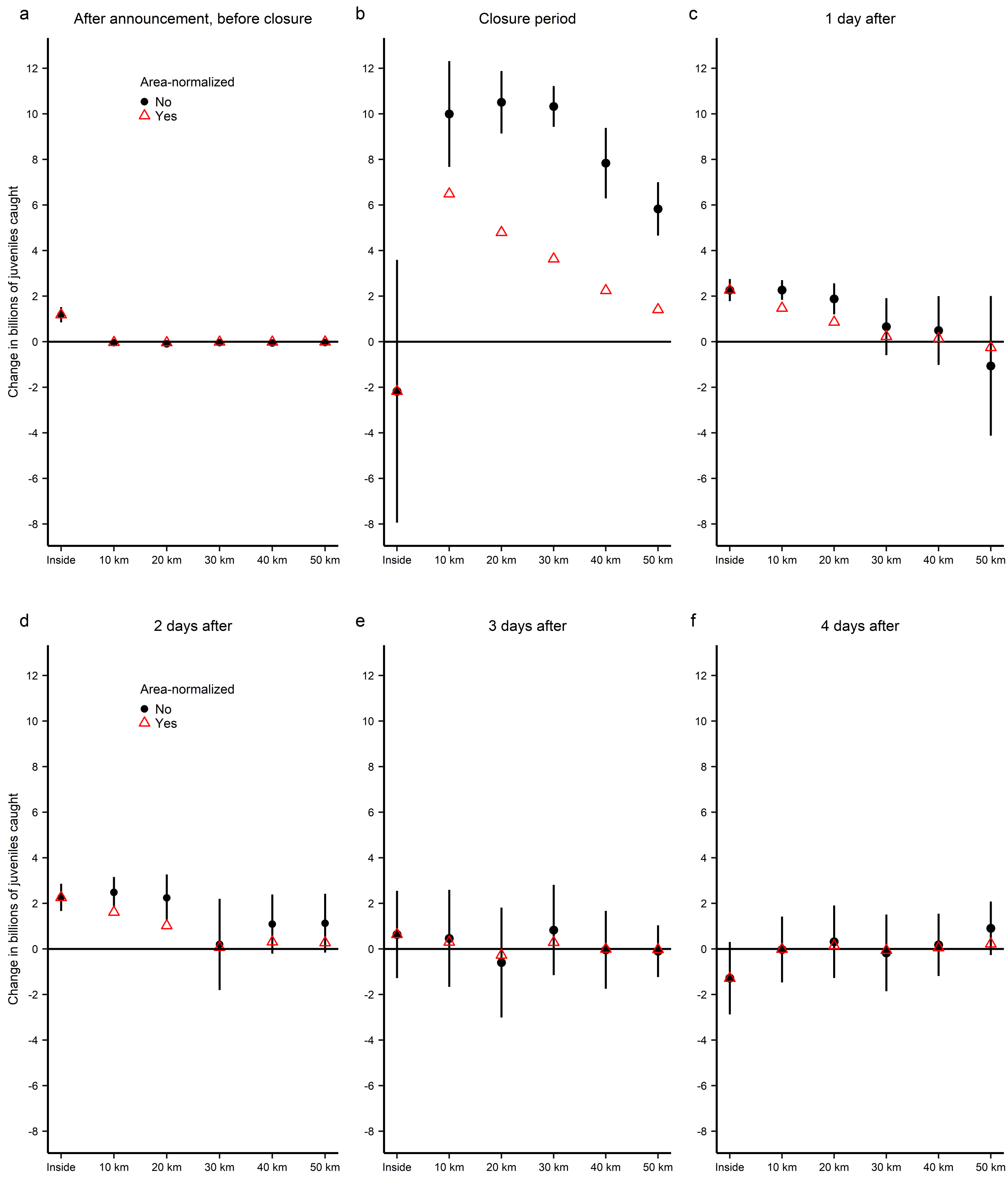
During closure periods, juvenile catch decreases by 2.2 billion inside closed areas. This effect is not statistically significant. Outside closed areas during closure periods, there are large, statistically significant increases in juvenile catch. In total, spatial spillovers during the closure period sum to a 44.5 billion increase in juvenile catch.

The hollow, red triangles in Figure 8 are the level estimates (black points) normalized by the area inside potential closures. This area-normalization accounts for the fact that each subsequent spatial ring covers a larger area (see Figure 7 for a representative illustration). There is a mechanical increase in the level estimates in larger spatial rings for this reason. The area-normalized estimates reveal intuitive spatial decay. The increase in juvenile catch because of the policy during closure periods is largest just outside closed areas, and the effect of the policy diminishes farther from closed areas.

The policy also increases juvenile catch in the first two days after the end of closure periods. Juvenile catch increases by 2.3 billion inside closed areas in the first 24 hours after the end of closure periods (leftmost point in Figure 8c). Juvenile catch is also significantly higher 10 and 20 km outside closed areas, but the effect of the policy is insignificant in the 30, 40, and 50 km rings. This pattern is the same in the second day after the end of closure periods. By the third and fourth day after the end of closure periods, the effect of the policy on juvenile catch attenuates to 0, both inside and outside closed areas.

Summing the level estimates over treatment bins implies that the total effect of the temporary spatial closures policy is to increase the number of juveniles caught by 60 billion, or 75%. However, this approach is naïve because it ignores the total allowable catch limit the regulator sets each season (Section 2.2). When I use tons as the dependent variable in Equation 1, I estimate the temporary spatial closures policy increases tons caught by 35% on average across the 36 treatment bins. But total tons caught cannot increase in the four (of six) fishing seasons during my study period in which the total allowable catch limit was binding. In those seasons, though there was an increase in tons caught within the treatment window, tons caught necessarily decreased by the same amount outside the treatment window. When I account for this mechanical re-allocation, the total effect of the

Figure 8: Change in billions of juveniles caught because of the closures policy



Notes: The six subfigures (a to f) correspond to the six time periods in my treatment window. In each time period, there are six spatial units of interest (x-axis). The black points are the treatment effects in levels and the black whiskers are 95% confidence intervals. The hollow, red triangles are normalized by the area inside potential closures because larger spatial rings cover more area. N = 34,164. Standard errors clustered at level of two-week-of-sample by two-degree grid cell.

temporary spatial closures policy is to increase the number of juveniles caught by 47 billion, or 50% (delta method standard errors are 5.2 billion and 5.5%). This 50% increase in juvenile catch is my preferred estimate of the total effect of the temporary spatial closures policy. This result is robust to alternative specifications and estimation approaches (Appendices B.2, B.3 and B.4).

The regulator's total allowable catch limit, enforced through individual vessel quotas, reduces the damage caused by temporary spatial closures because without it, temporary spatial closures would increase juvenile catch by an even greater amount. The increase in juvenile catch because of temporary spatial closures remains substantial even after accounting for the reallocation in tons caught due to the total allowable catch limit because relative juvenile abundance is much higher near closures declared by the regulator (the second condition in Proposition 1 from Section 3). Average percentage juvenile among sets within the treatment window of actual closures is 25%, compared to 9% outside the treatment window (Figure C2).

7 Do closures provide valuable information?

Why does the temporary spatial closures policy increase total juvenile catch? Closures might provide valuable information regarding the location of anchoveta because the regulator declares closures in response to real-time anchoveta catch data from all vessels, and there is only anchoveta catch in an area if anchoveta are sufficiently abundant in the area. If vessels respond to closures because closures provide valuable information, one might expect vessels who fish near closures to reap the value of that information by catching more tons of anchoveta per set (Section 2.1). On the other hand, since closures are publicly announced, the value of this information might be competed away due to congestion; there are diminishing marginal returns when more vessels fish from the same local, exhaustible anchoveta population (Huang & Smith, 2014; Smith, 1969). It is even possible that vessels could overreact to closures, such that tons per set near closures is lower.

Proposition 2a in Section 3 states that in the Bayes-Nash equilibrium, vessels fish near closures until the profit from doing so equals the profit from fishing elsewhere. Using the electronic logbook data, I test whether observed vessel response to closures is consistent with the Bayes-Nash equilibrium by estimating the difference in tons caught per set near closures compared to tons caught per set elsewhere, conditional on the distance of the set to shore, vessel by season fixed effects, day-of-sample fixed effects, and two-degree grid cell by season fixed effects. I estimate the following ordinary least squares regression in Column 2 of Table

1:

$$Tons_{vjk} = \beta_1 \mathbb{1}\{Near\}_{vjk} + \beta_2 DistToShore_{vjk} + \delta_{vj} + \gamma_d + \alpha_{jg} + \epsilon_{vjk} \tag{2}$$

where v = vessel, j = season, k = set, d = day-of-sample, and g = two-degree grid cell.

The outcome variable is the inverse hyperbolic sine of the tons caught by a given set.[30] The explanatory variable $\mathbb{1}\{Near\}_{vjk}$ equals 1 for sets that occurred inside a treatment bin in which there was a statistically significant change in juvenile catch (see Figure 8) and equals 0 otherwise. I define $\mathbb{1}\{Near\}_{vjk}$ in this way because I am interested in whether the large spillover effects I estimated in Section 6 can be explained by the closures policy communicating information about the value of fishing in those places and times. In this regression, treatment bins are defined relative to closures declared by the regulator. The coefficient of interest is β_1, which measures the difference in tons caught by sets near closures declared by the regulator. I include fixed effects and the distance of each set to shore in order to partially control for differences in the cost of each set (e.g., sets farther from shore require more fuel, all else equal). Day-of-sample fixed effects also control for the international price of fishmeal, which determines the price fishermen receive per ton of anchoveta they land. Therefore, the change in tons per set captured by β_1 represents the change in revenue per set from fishing near closures declared by the regulator. To the extent that the fixed effects and the distance of each set to shore control for cost per set, β_1 can also be interpreted as the change in profit per set from fishing near closures declared by the regulator.

Without fixed effects, sets near closures declared by the regulator catch 36% more tons (Column 1 in Table 1). However, including fixed effects reduces β_1 by an order of magnitude and makes it statistically insignificant: sets near closures declared by the regulator do not catch more tons of anchoveta (Column 2). While this null result suggests that vessels' response to closures is rational and consistent with the Bayes-Nash equilibrium, the same null result would also occur if closures do not provide valuable information.

I use potential closures to test whether actual closures declared by the regulator provide valuable information. In Section 5, I described how I generate potential closures from the same fishing data the regulator uses to determine actual closures. If they were announced, potential closures would communicate similar information to fishermen as actual closures: fishing occurred recently in the area, so anchoveta are likely abundant nearby (see Section 2.1). But because potential closures and the electronic logbook data they are based on are not public, potential closures enable a test of whether actual closures provide valuable information that is unconfounded by vessels' response to this information (Proposition 2b in Section 3).

[30] Fishermen report catching 0 tons for 11% of sets in the electronic logbook data.

Table 1: Closures provide valuable information, but the value of this information is competed away

	Dependent variable: asinh(tons caught)			
	Actual closures		Potential closures	
	(1)	(2)	(3)	(4)
$\mathbb{1}\{\text{Near}\}$	0.310	-0.022	0.177	0.082
	(0.080)	(0.033)	(0.073)	(0.026)
Distance to shore (km)	0.011	0.004	0.011	0.004
	(0.002)	(0.001)	(0.002)	(0.001)
Constant	3.306		3.289	
	(0.082)		(0.101)	
Fixed effects		X		X

All regressions have 246,914 observations. $\mathbb{1}\{\text{Near}\}$ is an indicator for whether the set occurred inside a treatment bin in which there is a significant change in juvenile catch because of the temporary spatial closures policy. In Columns 1 and 2, Near is defined relative to actual closures declared by the regulator (mean of this indicator equals .391). In Columns 3 and 4, Near is defined relative to potential closures (mean of this indicator is .798). Electronic logbook data is for all vessels from April 2017 to January 2020. Regressions in Columns 2 and 4 include vessel by season fixed effects, day-of-sample fixed effects, and two-degree grid cell by season fixed effects. Standard errors clustered at level of two-week-of-sample by two-degree grid cell.

I now estimate the same regression as Equation 2, except the Near indicator is defined relative to potential closures, rather than actual closures. Without fixed effects, sets near potential closures catch 19% more tons (Column 3 in Table 1). Including fixed effects in Column 4 reduces β_1 by half, but the difference in tons caught per set near potential closures remains statistically significant (t-statistic > 3). Sets near potential closures catch 9% more tons on average. Taken together, these results suggest closures do provide valuable information (second testable hypothesis from introduction), but the value of this information is competed away.

8 Does the information provided by closures increase spillovers?

In Section 7, I presented evidence that closures implicitly provide information to fishermen about the value of fishing before, just outside, and after closures. I now test explicitly whether the information provided by closures is a mechanism underlying the policy's large spillover effects.

Proposition 3 in Section 3 states that vessels that receive a larger positive information shock from closure announcements will have larger treatment effects. Vessels experience a larger information shock from closure announcements if they have less information about an area before closure announcement. I test Proposition 3 using the same potential closures I generated to estimate the effect of the policy on juvenile catch, except I now calculate juvenile catch inside a potential closure-treatment bin separately for two types of vessels: vessels with more information about a potential closure and vessels with less information about a potential closure. I consider two ways in which vessels can acquire information about a potential closure before closure announcement would occur (if the potential closure is declared an actual closure by the regulator). First, vessels have more information about a potential closure if they fished inside the potential closure the day before closure announcement would occur. Second, vessels have more information about a potential closure if another vessel in their firm fished inside the potential closure the day before closure announcement would occur. I estimate the following equation by ordinary least squares regression:

$$\begin{aligned} JuvenileCatch_{isth} = &\alpha_{sth} + \beta_{sth} TreatFraction_{ist} + \sum_{\ell=[3,3.5)}^{[18.5,19)} \xi_\ell Prop_{i\ell} + \\ &\gamma_1 Sets_i + \gamma_2 Tons_i + \gamma_3 Area_i + \gamma_4 DistToCoast_i + \\ &\gamma_5 TonsPerSet_i + \gamma_6 TonsPerArea_i + \sigma_{wg} + \delta_d + \epsilon_{isth} \end{aligned} \tag{3}$$

where h indicates heterogeneity (in information) category and all other variables and subscripts are as defined for Equation 1.

Note that Equation 3 is identical to Equation 1 except there are now twice as many treatment coefficients of interest (two heterogeneity categories for each treatment bin). There are also twice as many observations in this regression because I calculate juvenile catch in the potential closure-treatment bin among vessels with less information and among vessels with more information. Figure 9 presents the result when I categorize vessels by whether they personally fished inside the potential closure the day before closure announcement would occur, and Figure 10 displays the result when I categorize vessels by whether a vessel in their firm fished inside the potential closure the day before closure announcement would occur.[31] Unlike in Figure 8, I present the results in percent changes rather than changes in

[31] I leave out own-fishing in the firm-level categorization, so that vessels are coded as having had a member of their firm fish inside a potential closure the day before closure announcement would occur only if a different vessel in their firm did so.

levels because more vessels belong to the lower information group.

Vessels that fished inside a potential closure the day before closure announcement have a much smaller total treatment effect (0.7% increase in total juvenile catch) than vessels that did not (87.5% increase in total juvenile catch).[32] The treatment effect for vessels that fished inside a potential closure the day before closure announcement is not different from zero (delta method standard error is 3.2%), whereas the treatment effect for vessels that did not fish inside a potential closure the day before closure announcement is highly statistically significant (standard error is 5.5%). The difference in treatment effects between the two information groups is also statistically significant (difference is 86.8% with a standard error of 6.4%).[33]

The information mechanism also operates at the firm-level. Vessels who had a different member of their firm fish inside a potential closure the day before closure announcement would occur have a much smaller treatment effect (19.1% increase in juvenile catch) than vessels who did not (77.9% increase in juvenile catch). Both treatment effects are different from zero (standard errors are 3.7% and 5.2%), as is the difference in treatment effects (difference is 58.8% with a standard error of 6.4%).

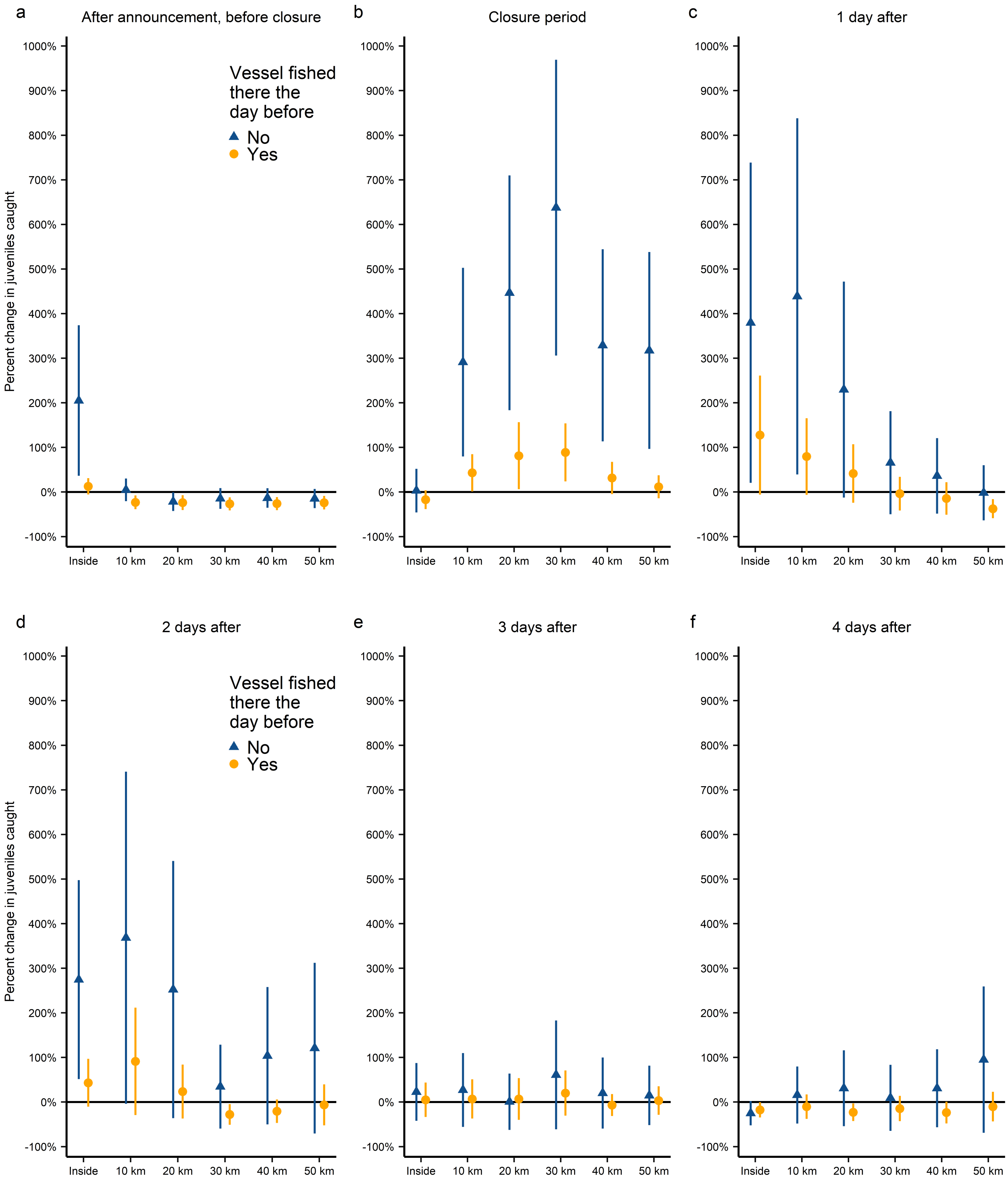
The results in this section support Proposition 3 that vessels that receive a larger information shock from closures have larger treatment effects. The information provided by closures is a mechanism underlying the policy's large spillover effects. The vessel-level result in Figure 9 also explains why fishermen underreport percentage juvenile in the raw electronic logbook data (Section 4) even though closures provide valuable information (Section 7). Closures only provide information to fishermen who were not already fishing in the area, so fishermen might underreport percentage juvenile to avoid triggering a closure in the area they are already fishing.[34]

[32] I calculate the percent change in total juvenile catch for both groups and both categorizations in the same way as in Section 6, converting the treatment coefficients into changes in levels and accounting for the reallocation in tons caught in the four fishing seasons the total allowable catch limit was binding.

[33] Note that this result does not reflect mean reversion because estimation is across potential closures. The treatment effect for vessels that fished inside a potential closure the day before closure announcement would occur is estimated by comparing juvenile catch by these vessels near potential closures that get closed to juvenile catch by this same group of vessels near potential closures that do not get closed.

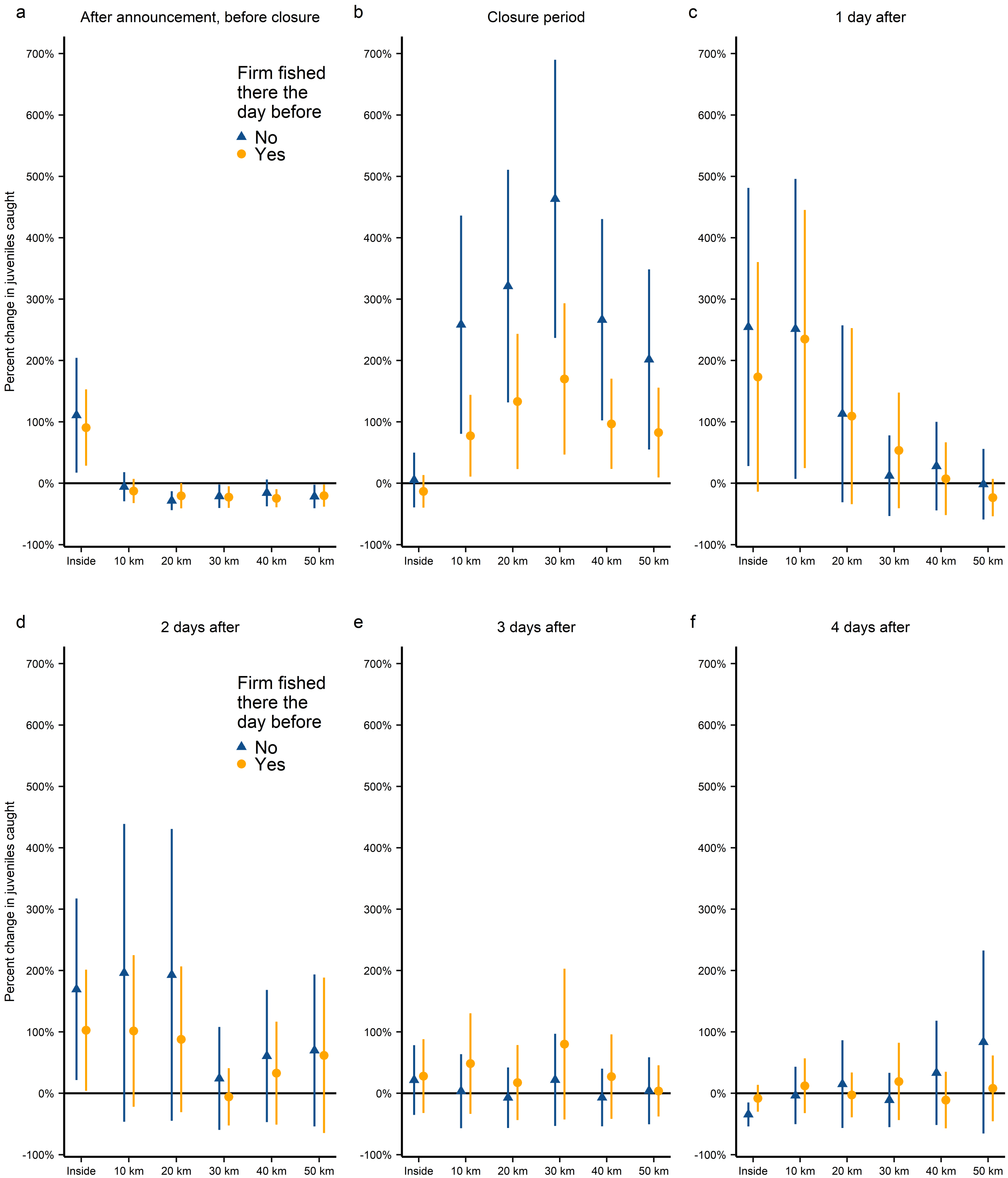
[34] Recall that underreporting by fishermen does not bias my estimates because I calculate my regressions' outcome variable and control variables using third-party inspector data.

Figure 9: Percent change in juvenile catch because of the closures policy by whether vessels fished in the potential closure the day before closure announcement



Notes: The six subfigures (a to f) correspond to the six time periods in my treatment window. In each time period, there are six spatial units of interest (x-axis). The points are the treatment effects in percentages and the whiskers are 95% confidence intervals. N = 68,328. Standard errors clustered at level of two-week-of-sample by two-degree grid cell.

Figure 10: Percent change in juvenile catch because of the closures policy by whether vessels had a different member of their firm fish in the potential closure the day before closure announcement



Notes: The six subfigures (a to f) correspond to the six time periods in my treatment window. In each time period, there are six spatial units of interest (x-axis). The points are the treatment effects in percentages and the whiskers are 95% confidence intervals. N = 68,328. Standard errors clustered at level of two-week-of-sample by two-degree grid cell.

9 Discussion

Peru's temporary spatial closures policy is targeted to reduce juvenile catch by temporarily banning fishing in the places with the highest relative abundance of juvenile anchoveta. While there is a (noisy) decrease in juvenile catch inside closed areas during closure periods, there are large increases in juvenile catch inside closed areas between the announcement and the beginning of closures, just outside closed areas during closures periods, and inside closed areas one and two days after closures end. On net, this policy worsens the target outcome, increasing total juvenile catch by 50%.

The failure of this policy to achieve its objective is not due to a failure of targeting. The regulator is closing the right areas; in the 9 to 12 hours before the beginning of a closure, 47% of individuals caught inside the soon-to-be closed area are juveniles, higher than in any other treatment bin and much higher than the 9% juvenile caught outside the treatment window (Figure C2). Within the support of the data, longer closures offer no improvement and larger closures perform even worse than smaller closures (Appendix A.1). Larger closures could cause larger increases in juvenile catch if larger closures are a larger positive signal of fishing productivity. A conservative estimate implies that the policy reduces exports by \$38 million per year (2017 USD).[35] Despite the sophistication of the temporary spatial closures policy, the empirical results in this paper support a new approach to reduce juvenile catch.

The information design literature studies the relationship between a Sender of information and a Receiver who acts based on this information (Kamenica, 2019; Kamenica & Gentzkow, 2011). The Sender's problem is to choose the decision rule that induces the Receiver to act in a way that maximizes the Sender's utility. Here, the regulator (Sender) wants fishermen (Receivers) to catch fewer juveniles, but the signal conveyed by closures announcements directs fishing to the places with the most juveniles. Instead, the regulator could tell fishermen where high percentages of adults are being caught. Because fishermen are paid by the ton, they are not fishing more near closures because they specifically want to catch juveniles. They fish more near closures because they want to reduce their search costs

[35] I reproduce the method of Salvatteci and Mendo (2005), who estimate the cost of juvenile catch by comparing status quo landings to a counterfactual where juveniles make up a smaller fraction of individuals landed. I similarly project forward the length distribution of individuals caught during my study period until counterfactual juvenile catch is 33% lower than status quo juvenile catch (equivalently, until status quo juvenile catch is 50% higher than counterfactual juvenile catch). Status quo tons landed are 2.1% lower than in my counterfactual projection because anchoveta growth exceeds natural mortality within the support of the data (i.e., the closures policy causes "growth overfishing"). In the most recent year of data (2017), FOB export revenues were \$1.79 billion (PRODUCE, 2018a). Then the closures policy reduces exports by \$38 million (\$1.79 billion $\times$ -2.1%). This projection is conservative because it does not account for the lower reproductive capacity of juveniles, known as "recruitment overfishing". Recruitment overfishing reduces future spawning activity, which likely reduces the size of the future stock (Quaas et al., 2013).

and increase the tons of anchoveta they catch per set. The regulator could use the electronic logbook data to calculate locations with high percentages of adult catch. Fishermen might react to this information in the same way they react to closures, except they would now be reallocating their fishing to places with few juveniles.

I perform a back-of-the-envelope calculation to explore the effect of replacing the current closures policy with an alternative policy that reveals the locations with the highest percentages of adult catch. I identify the 410 potential closures with the highest weighted-average percentage of adult catch.[36] I assume that the 35% increase in tons caught near the 410 closures declared by the regulator instead occur within the treatment window of the 410 potential closures with the highest percentage of adult catch. Juvenile catch changes in three ways in this scenario. First, fishermen catch 47 billion *fewer* juveniles due to the elimination of the closures policy (fewer tons in high percentage juvenile areas). Second, they catch 5 billion *more* juveniles near the 410 high-adult potential closures (more tons in low percentage juvenile areas). Third, they catch 37 billion *fewer* juveniles due to the compensating decrease in tons caught outside the treatment window of the 410 high-adult potential closures in the four of six fishing seasons in which the total allowable catch limit binds (fewer tons in areas with above-average percentage juvenile). On net, juvenile catch is 56% lower in this counterfactual scenario compared to the status quo level of juvenile catch (62 billion juveniles are caught compared to 141 billion). By attracting fishing to the places with the lowest percentage juvenile, the regulator could help fishermen reduce search costs while also reducing the capture of juvenile anchoveta, the most important biological externality in the world's largest fishery.

This calculation illustrates that policy-induced information spillovers need not cause targeted policies to backfire. When carefully designed, targeted policies that convey information about non-targeted units could simultaneously increase economic profits *and* mitigate the externality. Achieving such a win-win outcome requires understanding how the policy's information spillovers relate to agents' economic incentives.

References

Abadie, A., Athey, S., Imbens, G. W., & Wooldridge, J. (2017). *When Should You Adjust Standard Errors for Clustering?* (Working Paper No. 24003). National Bureau of Economic Research.

[36]Calculated from the sets that generate each potential closure and weighted by the number of individuals caught by each set.

Abadie, A., Diamond, A., & Hainmueller, J. (2010). Synthetic Control Methods for Comparative Case Studies: Estimating the Effect of California's Tobacco Control Program. *Journal of the American Statistical Association*, *105*(490), 493–505.

Abadie, A., Diamond, A., & Hainmueller, J. (2011). Synth: An R Package for Synthetic Control Methods in Comparative Case Studies. *Journal of Statistical Software*, *42*(13), 1–17.

Abadie, A., & Gardeazabal, J. (2003). The Economic Costs of Conflict: A Case Study of the Basque Country. *American Economic Review*, *93*(1), 113–132.

Acemoglu, D., Makhdoumi, A., Malekian, A., & Ozdaglar, A. (2020). *Testing, Voluntary Social Distancing and the Spread of an Infection* (Working Paper No. 27483). National Bureau of Economic Research.

Agnarsson, S., Matthiasson, T., & Giry, F. (2016). Consolidation and distribution of quota holdings in the Icelandic fisheries. *Marine Policy*, *72*, 263–270.

Asriyan, V., Fuchs, W., & Green, B. (2017). Information Spillovers in Asset Markets with Correlated Values. *American Economic Review*, *107*(7), 2007–40.

Assunção, J., McMillan, R., Murphy, J., & Souza-Rodrigues, E. (2019). *Optimal Environmental Targeting in the Amazon Rainforest* (Working Paper No. 25636). National Bureau of Economic Research.

Bellemare, M. F., & Wichman, C. J. (2020). Elasticities and the Inverse Hyperbolic Sine Transformation. *Oxford Bulletin of Economics and Statistics*, *82*(1), 50–61.

Blattman, C., Green, D., Ortega, D., & Tobón, S. (2019). *Place-Based Interventions at Scale: The Direct and Spillover Effects of Policing and City Services on Crime* (Working Paper No. 23941). National Bureau of Economic Research.

Castillo, R., Dalla Rosa, L., García Diaz, W., Madureira, L., Gutierrez, M., Vásquez, L., & Koppelmann, R. (2019). Anchovy distribution off Peru in relation to abiotic parameters: A 32-year time series from 1985 to 2017. *Fisheries Oceanography*, *28*(4), 389–401.

Costello, C., Gaines, S. D., & Lynham, J. (2008). Can Catch Shares Prevent Fisheries Collapse? *Science*, *321*(5896), 1678–1681.

Costello, C., Ovando, D., Clavelle, T., Strauss, C. K., Hilborn, R., Melnychuk, M. C., Branch, T. A., Gaines, S. D., Szuwalski, C. S., Cabral, R. B., et al. (2016). Global fishery prospects under contrasting management regimes. *Proceedings of the National Academy of Sciences*, *113*(18), 5125–5129.

Duflo, E., Greenstone, M., Pande, R., & Ryan, N. (2013). Truth-telling by third-party auditors and the response of polluting firms: Experimental evidence from India. *The Quarterly Journal of Economics*, *128*(4), 1499–1545.

Dunn, D. C., Maxwell, S. M., Boustany, A. M., & Halpin, P. N. (2016). Dynamic ocean management increases the efficiency and efficacy of fisheries management. *Proceedings of the National Academy of Sciences*, *113*(3), 668–673.

Englander, G. (2019). Property rights and the protection of global marine resources. *Nature Sustainability*, *2*(10), 981–987.

Erdem, T., Imai, S., & Keane, M. P. (2003). Brand and Quantity Choice Dynamics Under Price Uncertainty. *Quantitative Marketing and Economics*, *1*(1), 5–64.

FAO. (2018). The State of World Fisheries and Aquaculture 2018 - Meeting the sustainable development goals [Food and Agricultural Organization of the United Nations (FAO)].

Fréon, P., Sueiro, J. C., Iriarte, F., Evar, O. F. M., Landa, Y., Mittaine, J.-F., & Bouchon, M. (2014). Harvesting for food versus feed: A review of Peruvian fisheries in a global context. *Reviews in Fish Biology and Fisheries*, *24*(1), 381–398.

Getu, A., Misganaw, K., & Bazezew, M. (2015). Post-harvesting and Major Related Problems of Fish Production. *Fisheries and Aquaculture Journal*, *6*(4).

Gibson, M., & Carnovale, M. (2015). The effects of road pricing on driver behavior and air pollution. *Journal of Urban Economics*, *89*, 62–73.

Gordon, H. S. (1954). The Economic Theory of a Common-Property Resource: The Fishery. *Journal of Political Economy*, *62*(2), 124–142.

Gray, W. B., & Shimshack, J. P. (2011). The Effectiveness of Environmental Monitoring and Enforcement: A Review of the Empirical Evidence. *Review of Environmental Economics and Policy*, *5*(1), 3–24.

Greenstone, M., & Jack, B. K. (2015). Envirodevonomics: A Research Agenda for an Emerging Field. *Journal of Economic Literature*, *53*(1), 5–42.

Hansman, C., Hjort, J., León-Ciliotta, G., & Teachout, M. (2019). Vertical Integration, Supplier Behavior,and Quality Upgrading among Exporters. *Journal of Political Economy*, Forthcoming.

Hazen, E. L., Scales, K. L., Maxwell, S. M., Briscoe, D. K., Welch, H., Bograd, S. J., Bailey, H., Benson, S. R., Eguchi, T., Dewar, H., et al. (2018). A dynamic ocean management tool to reduce bycatch and support sustainable fisheries. *Science Advances*, *4*(5), eaar3001.

Holland, P. W. (1986). Statistics and Causal Inference. *Journal of the American Statistical Association*, *81*(396), 945–960.

Homans, F. R., & Wilen, J. E. (1997). A Model of Regulated Open Access Resource Use. *Journal of Environmental Economics and Management*, *32*(1), 1–21.

Homans, F. R., & Wilen, J. E. (2005). Markets and rent dissipation in regulated open access fisheries. *Journal of Environmental Economics and Management*, *49*(2), 381–404.

Huang, L., & Smith, M. D. (2014). The Dynamic Efficiency Costs of Common-Pool Resource Exploitation. *American Economic Review*, *104*(12), 4071–4103.

IMARPE. (2018). *Situación del Stock Norte-Centro de la anchoveta peruana (Engraulis ringens) al 01 de Abril de 2018 y perspectivas de explotación para la primera temporada de pesca de 2018*. Instituto del Mar del Perú (IMARPE).

IMARPE. (2019). *Elaboración de la Tabla de Decisión para la determinación del Límite Máximo de Captura Total Permisible por temporada de pesca en la pesquería del Stock Norte-Centro de la anchoveta peruana* (IMP-DGIRP/AFDPERP Edición 4 Revisión 1). Instituto del Mar del Perú (IMARPE).

Instituto Humboldt, & SNP. (2017). Segundo conversatorio sobre el diseño de las redes de cerco de anchoveta y el diseño de una red experimental para su uso en Operaciones Eureka [Instituto Humboldt de Investigación Marina y Acuícola (Instituto Humboldt) and Sociedad Nacional de Pesquería (SNP)].

Instituto Humboldt, SNP, & CeDePesca. (2018). Propuesta de modificaciones al régimen sancionador pesquero en relación con las limitaciones de selectividad y el incentivo de prácticas que apoyen la sostenibilidad de la pesquería del recurso anchoveta desde la perspectiva de la pesca industrial [Instituto Humboldt de Investigación Marina y Acuícola (Instituto Humboldt), Sociedad Nacional de Pesquería (SNP), Centro Desarrollo y Pesca Sustentable (CeDePesca)].

Isaksen, E. T., & Richter, A. (2019). Tragedy, Property Rights, and the Commons: Investigating the Causal Relationship from Institutions to Ecosystem Collapse. *Journal of the Association of Environmental and Resource Economists*, *6*(4), 741–781.

Joo, R., Salcedo, O., Gutierrez, M., Fablet, R., & Bertrand, S. (2015). Defining fishing spatial strategies from VMS data: Insights from the world's largest monospecific fishery. *Fisheries Research*, *164*, 223–230.

Kamenica, E. (2019). Bayesian Persuasion and Information Design. *Annual Review of Economics*, *11*, 249–272.

Kamenica, E., & Gentzkow, M. (2011). Bayesian Persuasion. *American Economic Review*, *101*(6), 2590–2615.

Keane, M. P., & Neal, T. (2020). *Consumer Panic in the COVID-19 Pandemic* (No. 2020-06). UNSW Economics Working Paper.

Kroetz, K., Sanchirico, J. N., Contreras, E. G., Novoa, D. C., Collado, N., & Swiedler, E. W. (2016). *Examination of the Peruvian anchovy Individual Vessel Quota (IVQ) System* (Working Paper No. 749). Inter-American Development Bank.

Kroetz, K., Sanchirico, J. N., Contreras, E. G., Novoa, D. C., Collado, N., & Swiedler, E. W. (2019). Examination of the Peruvian Anchovy Individual Vessel Quota (IVQ) System. *Marine Policy*, *101*, 15–24.

Kroodsma, D. A., Mayorga, J., Hochberg, T., Miller, N. A., Boerder, K., Ferretti, F., Wilson, A., Bergman, B., White, T. D., Block, B. A., et al. (2018). Tracking the global footprint of fisheries. *Science*, *359*(6378), 904–908.

Ladino, J. F., Saavedra, S., & Wiesner, D. (2019). *One Step Ahead of the Law: The Net Effect of Anticipation and Implementation of Colombia's Illegal Crops Substitution Program* (Serie Documentos de Trabajo No. 238). Universidad del Rosario.

Lewison, R., Hobday, A. J., Maxwell, S., Hazen, E., Hartog, J. R., Dunn, D. C., Briscoe, D., Fossette, S., O'Keefe, C. E., Barnes, M., et al. (2015). Dynamic Ocean Management: Identifying the Critical Ingredients of Dynamic Approaches to Ocean Resource Management. *BioScience*, *65*(5), 486–498.

List, J. A., Margolis, M., & Osgood, D. E. (2006). *Is the Endangered Species Act Endangering Species?* (Working Paper No. 12777). National Bureau of Economic Research.

Maxwell, S. M., Hazen, E. L., Lewison, R. L., Dunn, D. C., Bailey, H., Bograd, S. J., Briscoe, D. K., Fossette, S., Hobday, A. J., Bennett, M., et al. (2015). Dynamic ocean management: Defining and conceptualizing real-time management of the ocean. *Marine Policy*, *58*, 42–50.

Natividad, G. (2016). Quotas, productivity, and prices: The case of anchovy fishing. *Journal of Economics & Management Strategy*, *25*(1), 220–257.

Oliva, P. (2015). Environmental Regulations and Corruption: Automobile Emissions in Mexico City. *Journal of Political Economy*, *123*(3), 686–724.

Paredes, C. E. (2014). *La anchoveta: Pesca y descarte de juveniles. Análisis de la regulación pesquera y propuestas para su perfeccionamiento* (Cuadernos de Investigación Edición N° 20). Universidad de San Martín de Porres.

Pfaff, A., & Robalino, J. (2017). Spillovers from conservation programs. *Annual Review of Resource Economics*, *9*, 299–315.

Pikitch, E., Boersma, P. D., Boyd, I., Conover, D., Cury, P., Essington, T., Heppell, S., Houde, E., Mangel, M., Pauly, D., et al. (2012). *Little fish, big impact: Managing a crucial link in ocean food webs*. Lenfest Ocean Program.

PRODUCE. (2012). Decreto Supremo N° 008-2012: Decreto Supremo que establece medidas para la conservacion del recurso hidrobiologico [Ministerio de la Producción (PRODUCE)].

PRODUCE. (2015). Resolución Directoral N° 035-2015-PRODUCE/DGSF [Ministerio de la Producción (PRODUCE)].

PRODUCE. (2016a). Decreto Supremo N° 024-2016: Decreto Supremo que establece medidas para fortalecer el control y vigilancia de la actividad extractiva para la conservación y aprovechamiento sostenible del recurso anchoveta [Ministerio de la Producción (PRODUCE)].

PRODUCE. (2016b). Resolución Directoral N° 014-2016-PRODUCE/DGSF [Ministerio de la Producción (PRODUCE)].

PRODUCE. (2017a). I temporada de pesca – 2017 Zona Norte-Centro: Resumen Ejecutivo de Seguimiento y Control [Ministerio de la Producción (PRODUCE)].

PRODUCE. (2017b). Resolución Directoral N° 687-2017-PRODUCE/DS-PA [Ministerio de la Producción (PRODUCE)].

PRODUCE. (2018a). Anuario Estadístico Pesquero y Acuícola 2017 [Ministerio de la Producción (PRODUCE)].

PRODUCE. (2018b). I temporada de pesca – 2018 Zona Norte-Centro: Resumen Ejecutivo de Seguimiento y Control [Ministerio de la Producción (PRODUCE)].

PRODUCE. (2018c). II temporada de pesca – 2017 Zona Norte-Centro: Resumen Ejecutivo de Seguimiento y Control [Ministerio de la Producción (PRODUCE)].

PRODUCE. (2018d). Resolución Directoral N° 127-2018-PRODUCE/DGSFS-PA [Ministerio de la Producción (PRODUCE)].

PRODUCE. (2020a). Comunicado N° 010-2020-PRODUCE/DGSFS-PA-SP Suspensión Preventiva de Actividades de Extractivas del Recurso Caballa [Ministerio de la Producción (PRODUCE)]. https://www.gob.pe/institucion/produce/informes-publicaciones/460558-suspension-preventiva-de-actividades-extractivas-del-recurso-caballa

PRODUCE. (2020b). Resolución Ministerial N° 015-2020-PRODUCE [Ministerio de la Producción (PRODUCE)].

PRODUCE. (2020c). Suspensiones preventivas de zonas DGSFS - PA – SP [Ministerio de la Producción (PRODUCE)]. https://www.produce.gob.pe/index.php/dgsfs-pa/suspensiones-preventivas-de-pesca

Quaas, M. F., Requate, T., Ruckes, K., Skonhoft, A., Vestergaard, N., & Voss, R. (2013). Incentives for optimal management of age-structured fish populations. *Resource and Energy Economics*, *35*(2), 113–134.

R Core Team. (2019). *R: A language and environment for statistical computing*. R Foundation for Statistical Computing. Vienna, Austria. https://www.R-project.org/

Reimer, M., & Wilen, J. (2013). Regulated Open Access and Regulated Restricted Access Fisheries. In *Encyclopedia of Energy, Natural Resource, and Environmental Economics* (pp. 215–223). Elsevier.

Sala, E., Mayorga, J., Costello, C., Kroodsma, D., Palomares, M. L., Pauly, D., Sumaila, U. R., & Zeller, D. (2018). The economics of fishing the high seas. *Science Advances*, *4*(6), eaat2504.

Salvatteci, R., & Mendo, J. (2005). Estimación de las pérdidas bio-económicas causadas por la captura de juveniles de anchoveta (*Engraulis ringens*, j.) en la Costa Peruana (Estimation of bio-economic losses caused by the capture of juvenile anchovy (*Engraulis ringens*, j.) in the Peruvian Coast). *Ecología Aplicada*, *4*(1-2), 113–120.

Scott, A. (1955). The Fishery: The Objectives of Sole Ownership. *Journal of Political Economy*, *63*(2), 116–124.

Smith, M. D. (2012). The New Fisheries Economics: Incentives Across Many Margins. *Annual Review of Resource Economics*, *4*(1), 379–402.

Smith, T. D. (1994). *Scaling Fisheries: The Science of Measuring the Effects of Fishing, 1855-1955*. Cambridge University Press.

Smith, V. L. (1969). On Models of Commercial Fishing. *Journal of Political Economy*, *77*(2), 181–198.

SUPNEP. (2017). Convenio Colectivo por Rama de Actividad, SUPNEP - AANEP, 12 de marzo 2017 al 11 marzo del 2022 [Sindicato Univo de Pescadores de Nuevas Embarcaciones del Peru (SUPNEP)]. http://www.supnep.pe/assets/convenio_colectivo_supnep_aanep_2017_2022.pdf

Tveteras, S., Paredes, C. E., & Peña-Torres, J. (2011). Vessel Quotas in Peru: Stopping the Race for Anchovies. *Marine Resource Economics*, *26*(3), 225–232.

Wallace, R. K., & Fletcher, K. M. (1997). Understanding fisheries management: A Manual for understanding the Federal Fisheries Management Process, Including Analysis of the 1996 Sustainable Fisheries Act.

A Additional results

A.1 Heterogeneity by size of closure and length of closure period

Perhaps the temporary spatial closures policy does not reduce juvenile catch because the closures are not large enough or do not last long enough. The average size of a closure declared by the regulator is 1,328 km^2, or 36 by 36 km for a square closure. A school of anchoveta can swim 20 to 30 km in a day (Peraltilla and Bertrand, 2014). If juvenile anchoveta swim outside the closed area during the closure period, then closures might not be large enough to prevent them from being caught. With respect to the length of the closure period, the closures policy is intended to reduce juvenile catch by encouraging fishermen to find new places to fish (Section 2.2). The regulator can declare closures that last three to five days, which might not be enough time for this process to occur.

I test for treatment effect heterogeneity by size of closure and by the length of the closure period. I estimate the following regression:

$$\begin{aligned} JuvenileCatch_{ist} =& \alpha_{sth} + \beta_{sth} TreatFraction_{isth} + \sum_{\ell=[3,3.5)}^{[18.5,19)} \xi_\ell Prop_{i\ell} + \\ & \gamma_1 Sets_i + \gamma_2 Tons_i + \gamma_3 Area_i + \gamma_4 DistToCoast_i + \\ & \gamma_5 TonsPerSet_i + \gamma_6 TonsPerArea_i + \sigma_{wg} + \delta_d + \epsilon_{isth} \end{aligned} \tag{4}$$

where h indicates heterogeneity category and all other variables and subscripts are as defined for Equation 1.

The outcome variable, control variables, and the number of observations are the same as in Equation 1. The only difference is there are now twice as many treatment coefficients (72, instead of 36). In the test for heterogeneity by size of closure, h denotes treatment fraction overlap with actual closures that are either above-median size or below-median size. For example, to estimate Equation 1 I estimated treatment fraction overlap between potential closure-treatment bin ist and actual closure-treatment bin ist. Now I calculate treatment fraction overlap between potential closure-treatment bin ist and actual closure-treatment bin ist for actual closures that are above-median size, and also calculate treatment fraction overlap between potential closure-treatment bin ist and actual closure-treatment bin ist for actual closures that are below-median size.

In the test for heterogeneity by length of closure, h indicates treatment fraction overlap with actual closures that last either three days or five days. I do not estimate treatment effect heterogeneity for actual closures that last 4 days because only 15% of actual closures are 4 days long. I compute treatment fraction overlap with 3- and 5-day actual closures separately, creating 72 treatment bins of interest. I do not include bins that are four days after the closure period in my regression because the treatment effect estimates for these bins for five-day closures are very large and noisy. As Figure 8 shows, these bins are not important for understanding the effect of the policy, and including them in this test for heterogeneity by length of closure distorts the total percentage change I calculate for five-day closures.

I convert the treatment coefficients from these two regressions into total percentage changes in juvenile catch because of the policy in the same manner as in Section 6. Larger closures seem to perform even worse than smaller closures. Above-median-size closures increase juvenile catch by 66%, while below-median-size closures increase juvenile catch by 31% (p-value on this difference is .002). There does not appear to be treatment effect heterogeneity by length of closure. 3-day closures increase juvenile catch by 49%, while 5-day closures increase juvenile catch by 61% (p-value on this difference is .611). Within the support of the data, it does not seem that making closures larger or longer improves the performance of the policy.

A.2 Heterogeneity by firm size and vessel size

Certain types of vessels may respond to closures more than others. I test for treatment effect heterogeneity along two related dimensions: firm size, measured by the number of vessels a firm owns that are authorized to fish in the North-Central zone, and vessel size, measured by vessel length in meters. These dimensions are related because large firms tend to own large vessels (see Table A1). I test for treatment effect heterogeneity by re-estimating Equation 3 from Section 8, with subscript h now denoting firm size category in the first regression and vessel size category in the second regression. I convert the treatment coefficients from these two regressions into total percentage changes in juvenile catch because of the policy in the same manner as in Section 6.

First, I find that vessels belonging to large firms have larger treatment effects than vessels belonging to smaller firms. The increase in total juvenile catch because of the closures policy is 60% for the vessels that belong to the seven largest firms, which each own at least 19 vessels. The increase in total juvenile catch is 44% for vessels that belong to medium-sized firms, who own between 2 and 10 vessels, and is 11% for vessels that belong to firms that own only one vessel. Large-firm vessels account for 77% of the closures policy treatment effect, which is greater than their share of total juvenile catch in the fishery (70%).

Second, I find that above-median-length vessels have larger treatment effects than below-median-length vessels. The increase in juvenile catch because of the closures policy is 60%

Table A1: Vessel characteristics in the six fishing seasons of 2017, 2018, and 2019

	All vessels (1)	Large-firm vessels (2)	Medium-firm vessels (3)	Singleton vessels (4)
A. Average tons landed per season				
Minimum	3.11	105.45	14.64	3.11
Mean	2607.41	6311.44	1842.81	903.56
Median	1324.49	6004.34	1303.62	722.86
Max	22261.65	22261.65	10852.99	9389.83
B. Average number of active vessels per season				
Minimum	708	178	263	256
Mean	730.17	182	276.67	271.5
Median	731	182	278.5	269
Maximum	750	185	283	288
C. Vessel length (m)				
Minimum	11.23	15.85	11.3	11.23
Mean	26.05	41.64	24.06	17.62
Median	20.9	40.48	21.72	17.05
Max	77	77	53.75	42.57

Large-firm vessels are vessels that belong to one of the seven largest firms, which each own at least 19 vessels. Medium-firm vessels belong to firms that own 2 to 10 vessels. Singleton vessels belong to a firm that owns only one vessel. Data is for the North-Central zone only. Landings data is used to calculate the number of active vessels each season. Landings and vessel length data are from PRODUCE.

for above-median vessels, compared to 24% for below-median vessels. Above-median vessels account for 91% of the closures policy treatment effect, which is greater than their share of total juvenile catch in the fishery (83%).

It is difficult to determine whether above-median-length vessels respond more to closures because they are large, so have more flexibility in the length of their fishing trips, or because they belong to larger firms. 96% of large-firm vessels are above-median length, but among medium firms it is possible to examine heterogeneity by vessel length because 77% are below-median length and 23% are above-median length. I re-estimate equation 3 using only juvenile catch among medium firms to calculate the outcome variable. In contrast to the result using all vessels, I find the that increase in juvenile catch because of the closures policy is smaller for above-median-length vessels (24%) than for below-median-length vessels (45%). This difference is statistically significant and could indicate that firm size is the more relevant dimension of heterogeneity (p-value on the difference is .003). Larger firms may be more able to aggregate information from closures and dispatch vessels accordingly.

A.3 Vessel response to other information

I estimate that vessels change their fishing locations according to information gathered from their own past fishing trips and from past fishing by other vessels in their firm. These results further support the hypothesis that vessels respond to closures because closures provide valuable information.

First, I consider how the success of a previous fishing trip affects a vessel's fishing location choices on its next trip (Table A2). I test whether vessels fish farther from where they fished on their previous trip if the previous trip was bad, and whether vessels fish closer to where they fished on their previous trip if the previous trip was good. In Columns 1 to 3, bad previous trips are those in which tons landed are in the bottom tercile for that vessel that season, and good previous trips are those in which tons landed are in the top tercile for that vessel that season. In Columns 4 to 6, bad and good trips are defined by tons landed per trip hour. The landings data contains tons landed from each trip and I calculate trip hours by matching the the electronic logbook data, which contains the times fishing trips begin and end, to the landings data (Appendix C). Tons landed per trip hour can be thought of as a measure of trip productivity. I drop trips that lasted less than one hour from the regressions (.1% of trips). The omitted category is previous trips that were in the middle tercile.

The outcome variable is the minimum distance between sets on the previous trip and sets on the current trip. I calculate the outcome variable using the electronic logbook data, which contains the locations of sets and the times fishing trips begin and end. For example, suppose there were two sets on the previous trip, called A and B, and two sets on the current trip, called C and D. I calculate the distance between C and A, C and B, D and A, and D and B. The outcome variable is the minimum of these four values. I transform the outcome variable using the inverse hyperbolic sine, rather than a logarithmic transformation, because .17% of trips had a set in the same location as a set on the previous trip (in the electronic logbook data, fishing locations are reported to the nearest second, or $\frac{1}{3600}$ of a decimal degree).

I estimate the following ordinary least squares regression in Columns 1 and 4 of Table A2:

$$mindist_{vjf} = \beta_1 \mathbb{1}\{Bottom\ Tercile\}_{vj,f-1} + \beta_2 \mathbb{1}\{Top\ Tercile\}_{vj,f-1} + \epsilon_{vjf} \qquad (5)$$

where v = vessel, j =season, f =fishing trip, and $f-1$ = previous fishing trip.

In Columns 2 and 5, I add vessel by season fixed effects (α_{vj}) to Equation 5. In Columns 3 and 6, I include day-of-sample fixed effects, defined by the landing date of fishing trip f, in addition to vessel by season fixed effects.

Table A2: Vessels fish farther from previously bad locations and closer to previously good locations.

	Dependent variable: asinh(mindist)					
	(1)	(2)	(3)	(4)	(5)	(6)
Bottom tercile	0.267	0.269	0.271	0.243	0.243	0.254
	(0.028)	(0.024)	(0.023)	(0.025)	(0.022)	(0.019)
Top tercile	-0.110	-0.111	-0.124	-0.165	-0.165	-0.156
	(0.023)	(0.022)	(0.019)	(0.026)	(0.024)	(0.020)
Constant	4.092			4.117		
	(0.037)			(0.038)		
Tons tercile	X	X	X			
Tons/hour tercile				X	X	X
Vessel-season FE		X	X		X	X
Date FE			X			X

All regressions have 88,543 observations. Data is for all vessels from April 2017 to January 2020. Dependent variable is inverse hyperbolic sine minimum distance between sets on current trip and on previous trip. Standard errors clustered at level of two-week-of-sample by two-degree grid cell.

Across these specifications and definitions of previous trip success, I find consistent evidence that vessels' fishing location choices are a function of the information obtained on their previous trip. Vessels fish 28 to 31% farther from where they fished on their previous trip if that previous trip was in the bottom tercile (Columns 1 to 6). They fish 10 to 15% closer to where they fished on their previous trip if that previous trip was in the bottom tercile.

Next, I consider how vessels change their fishing locations in response to the success of past fishing by vessels in their firm. For each firm-day, I identify the locations of the worst and best sets. I standardize tons caught within vessel-season and define the worst (best) set as the set that caught the minimum (maximum) standardized tons. For each fishing trip that began on day d, I calculate the minimum distance between sets on the fishing trip to all sets by vessels in the firm on day $d-1$ (the day before the trip began). I do not include singleton vessels in these regressions (vessels that belong to a firm that only owns one vessel). I include indicators for distance to the worst and best set. The omitted category is distance to sets that are neither the worst nor the best.

I estimate the following ordinary least squares regression in Column 1 of Table A3:

$$mindist_{vjd} = \beta_1 \mathbb{1}\{Worst\}_{vj,d-1} + \beta_2 \mathbb{1}\{Best\}_{vj,d-1} + \epsilon_{vjd} \tag{6}$$

Table A3: Vessels fish farther from their firm's worst fishing location yesterday and closer to firm's best fishing location yesterday

Dependent variable: asinh(mindist)			
	(1)	(2)	(3)
Worst	-0.366	0.061	0.090
	(0.039)	(0.019)	(0.018)
Best	-0.515	-0.088	-0.059
	(0.039)	(0.023)	(0.022)
Constant	5.281		
	(0.056)		
Vessel-season FE		X	X
Date FE			X

All regressions have 1,593,180 observations. Data is from April 2017 to January 2020 for all vessels that belong to a firm that owns more than one vessel. Dependent variable is inverse hyperbolic sine minimum distance between sets on current trip and sets for vessels in the same firm on the day before the beginning of the current trip. Worst (best) is an indicator for distance to the set with the minimum (maximum) standardized tons caught by a vessel in the same firm the day before the beginning of the current trip. Standard errors clustered at level of two-week-of-sample by two-degree grid cell.

where v = vessel, j =season, d =starting date of fishing trip, and $d-1$ = day before start of fishing trip.

In Column 2, I add vessel by season fixed effects (α_{vj}) to Equation 6. In Column 3, I include day-of-sample fixed effects, defined by the starting date of the fishing trip, in addition to vessel by season fixed effects.

I estimate vessels fish 6 to 9% farther from the worst set by a vessel in their firm the previous day (Columns 2 and 3). They fish 6 to 8% closer to the best set by a vessel in their firm the previous day. In addition to responding to information provided by closures (by fishing more and catching more juveniles near closures), vessels also change where they fish in response to information obtained by vessels in their firm and information collected from their own previous fishing trips.

B Robustness checks

B.1 Balance tests: Pre-period juvenile catch levels and trends and observable measures of fishing productivity

In this section, I test whether treated and non-treated potential closures are balanced in their level of pre-period juvenile catch, trend in pre-period juvenile catch, and observable measures of fishing productivity.

First, I calculate juvenile catch and treatment fraction for inside potential closures on the day before sets would generate an actual closure.[37] I calculate treatment fraction for this inside, day-before bin in the same way as for the other bins (overlap with inside, day-before bin of actual closures declared by the regulator). I add these rows to my main dataset and estimate versions of Equation 1 in Table B1. I estimate treatment effects for all treatment bins (now 37 instead of 36), but only report the coefficient on the inside, day-before treatment bin.

Without control variables or fixed effects, there is a marginally significant correlation between treatment and juvenile catch in the inside, day-before treatment bin (Column 1). Potential closures that will eventually be closed (treatment fraction = 1) have 28% higher juvenile catch than potential closures that will not be declared actual closures by the regulator (treatment fraction = 0). In Columns 2 to 4 of Table B1, I test whether the control variables and fixed effects in Equation 1 eliminate this difference in pre-period juvenile catch, which would support the identifying assumption that treated and non-treated potential closures are comparable conditional on controls.

I include day-of-sample and two-week-of-sample by two-degree grid cell fixed effects in Column 2, potential closure-level controls from Equation 1 in Column 3 (excluding fixed effects), and the full set of potential closure-level controls and fixed effects from Equation 1 in Column 4. In all three specifications, these control variables and fixed effects make the correlation between treatment and juvenile catch in the inside, day-before bin at least one order of magnitude smaller and statistically insignificant (the treatment coefficients on the

[37] This time period is 48 to 72 hours before the beginning of the closure period, rather than 24 to 48 hours before the beginning of the closure period, because in some instances sets influence the probability of actual closures up to 48 hours before the beginning of the closure period. Therefore, there might be a mechanical correlation between juvenile catch 24 to 48 hours before the beginning of the closure period due to reverse causality. In the cases when a cluster of sets 24 to 48 hours before the closure period affect the probability of an actual closure, the treatment fraction for the potential closure generated by that cluster of sets could be up to one-third smaller than the true treatment fraction (because the potential closure could end one day before the actual closure, and the closure period for potential closures is three days). This occasional measurement error in treatment fraction does not affect my results, which exhibit the same pattern when I replace treatment fraction in Equation 1 with an indicator that equals 1 if the treatment fraction for a potential closure-bin is greater than 0 (Figure B5).

Table B1: Test for difference in pre-period juvenile catch

Dependent variable: asinh(JuvenilesCaught)				
	(1)	(2)	(3)	(4)
Treatment fraction	0.249	-0.001	-0.058	0.002
	(0.140)	(0.177)	(0.152)	(0.180)
Controls			X	X
Fixed effects		X		X

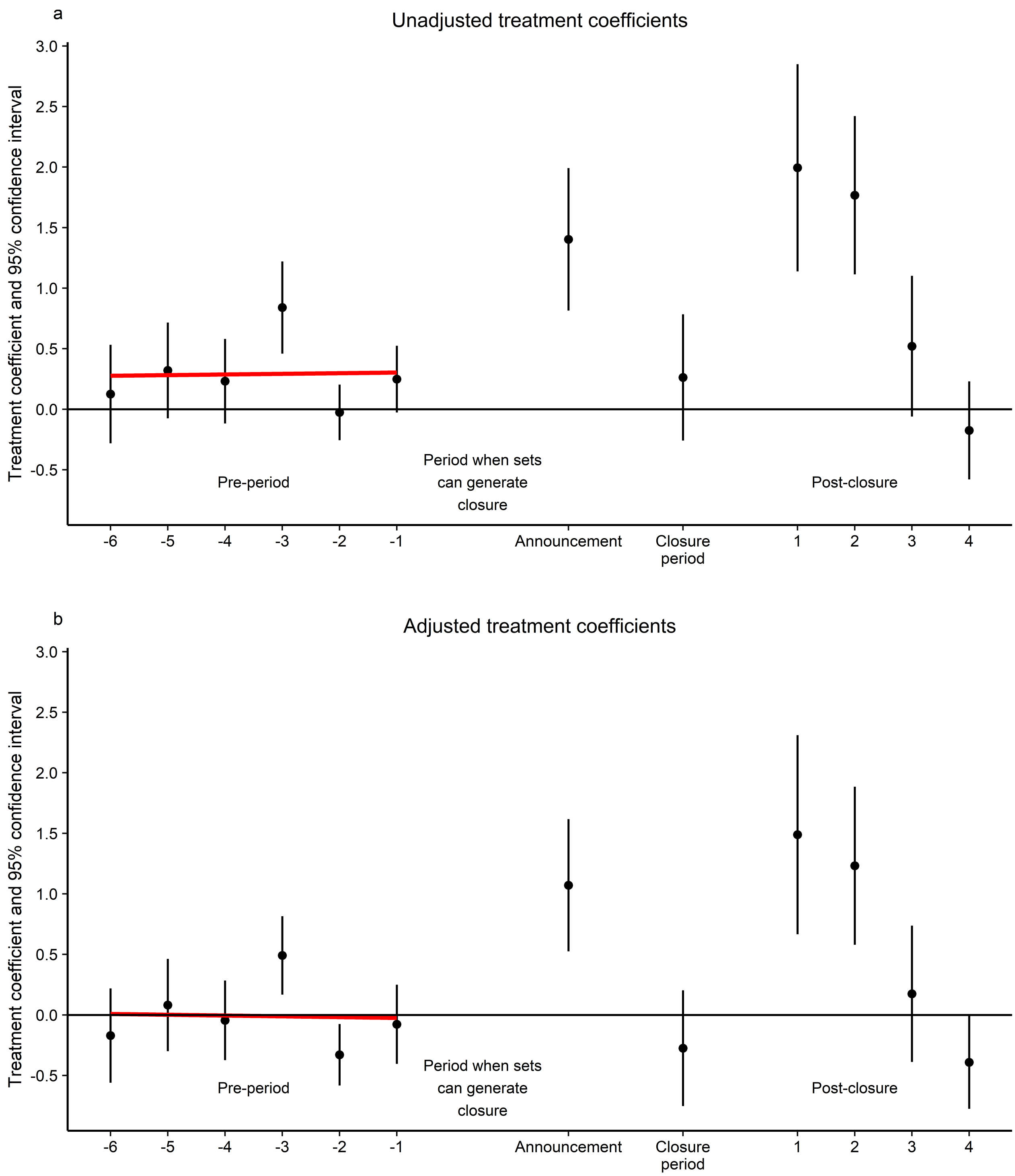
Notes: All regressions have 35,113 observations. Dependent variable is the inverse hyperbolic sine of millions of juveniles caught. All regressions include bin + treatment fraction × bin variables for all bins, but only the coefficent on treatment fraction for the inside, day-before bin is displayed in this table. Standard errors clustered at level of two-week-of-sample by two-degree grid cell.

other 36 treatment bins are very similar to those displayed in Figure B2 and are available upon request). They reduce the treatment coefficient by an order of magnitude without meaningfully increasing the standard error, emphasizing their importance for the validity of the identifying assumption.

To examine trends in pre-period juvenile catch, I also calculate juvenile catch and treatment fraction for inside potential closures up to six days before the period in which sets would generate an actual closure. I add these rows to my main dataset, so that there are now 42 treatment bins of interest (the original 36 plus the six new pre-period bins). I estimate treatment effects for all treatment bins (now 42 instead of 36), but only display the treatment coefficients for the inside potential closure treatment bins in Figure B1.

Without control variables or fixed effects, pre-period juvenile catch is consistently higher in the potential closures that will eventually be closed, though the trend is not different from zero (Figure B1a). But when I include the full set of control variables and fixed effects in Equation 1, the difference in pre-period juvenile catch levels is eliminated and the trend remains indistinguishable from zero (Figure B1b). The treatment coefficients after closures would be announced mirror my main result: an increase in juvenile catch after closure announcements but before the beginning of closure periods, a noisy decrease in juvenile catch during closure periods, increases in juvenile catch one and two days after closures end, and a dissipation of effects three and four days after closures end. The absence of a trend in pre-period juvenile catch lends further credence to the identification strategy used in this paper.

Figure B1: Test for pre-trend in juvenile catch inside potential closures



Notes: Both regressions have 39,858 observations. The dependent variable is the inverse hyperbolic sine of millions of juveniles caught. Both regressions include bin + treatment fraction × bin variables for all bins, but only the treatment fraction coefficients for inside potential closures treatment bins are displayed. In the second regression (b), I include the control variables and fixed effects in Equation 1. The red line is the linear trend in pre-period treatment coefficients. Standard errors clustered at level of two-week-of-sample by two-degree grid cell.

Table B2: Correlation between juvenile catch and measures of fishing productivity

	Dependent variable: asinh(JuvenileCatch)			
	(1)	(2)	(3)	(4)
DistToCoast	0.0051			0.0027
	(0.0017)			(0.0015)
TonsPerSet		0.0073		0.0067
		(0.0007)		(0.0009)
TonsPerArea			0.0140	0.0030
			(0.0037)	(0.0032)
Intercept	0.6408	0.3953	0.7011	0.3255
	(0.0666)	(0.0537)	(0.0518)	(0.0572)

All regressions have 34,164 observations. Dependent variable is inverse hyperbolic sine of millions of juveniles caught in a potential closure-treatment bin. Standard errors clustered at level of two-week-of-sample by two-degree grid cell.

To test whether potential closures are balanced on observables, I focus on three of the control variables in Equation 1 that are likely correlated with fishing productivity: distance to the coast, tons per set, and tons per area (km^2).[38] If treated potential closures are more desirable to fish near, juvenile catch inside the treatment window will be mechanically higher for treated potential closures than for non-treated potential closures, all else equal, because there will be more fishing near treated potential closures. Indeed, in Table B2 I find positive, significant correlations between juvenile catch inside potential closure-treatment bins and each of these three variables (Columns 1 to 3). When I regress juvenile catch on all three variables together in Column 4, all three coefficients remain positive but only tons per set is statistically significant. I record these fitted values and also use them to test for balance in Table B3.

In Table B3, I test whether potential closures are balanced on each variable and on the fitted values, conditional on the length distribution caught by the sets that generate the potential closure and the fixed effects in Equation 1. I find a significant correlation between treatment fraction and tons per area, but not between treatment fraction and distance to the coast, tons per set, or the fitted values. The non-correlation between treatment fraction and tons per set is more relevant than the correlation between treatment fraction and tons per area because tons per set is the only significant predictor of juvenile catch when juvenile catch is regressed on all three variables (Column 4 of Table B2). The lack of a correlation

[38] Anchoveta are more abundant closer to the coast (Castillo et al., 2019).

Table B3: Test for balance on measures of fishing productivity

	DistToCoast (1)	TonsPerSet (2)	TonsPerArea (3)	FittedVals (4)
Treatment fraction	-1.219 (1.850)	3.096 (4.482)	2.972 (1.162)	0.026 (0.034)

All regressions have 34,164 observations and control for two-week-of-sample by two-degree-grid-cell fixed effects, day-of-sample fixed effects, and potential closure length distribution. Standard errors clustered at level of two-week-of-sample by two-degree grid cell.

between treatment fraction and the fitted values is also reassuring because the fitted values are based on all three variables. These results should therefore be interpreted as evidence that treated and non-treated potential closures would offer similar fishing opportunities to vessels if not for treatment.

B.2 Treatment coefficients from Estimating Equation 1 and variants of Equation 1

Figure B2 displays the treatment coefficients from estimating Equation 1. My results exhibit the same pattern if I drop zero values and use a logarithmic transformation on the dependent variable instead of an inverse hyperbolic sine transformation (Figure B3). They also hold if I replace the dependent variable in Equation 1 with a binary indicator for positive juvenile catch (Figure B4) or if I replace treatment fraction with a binary indicator for positive treatment fraction (Figure B5).

My results are also robust to replacing the outcome variable with the inverse hyperbolic sine of tons of juveniles caught (Figure B6). Juvenile catch in tons is the number of individuals in each length times the weight of an individual in each length interval, summed over length intervals less than 12 cm.[39] When I convert these treatment coefficients into changes in levels and account for the reallocation in tons (of juveniles and adults) caught due to the total allowable catch limit, I estimate that the temporary spatial closures policy increases juvenile catch by 332 thousand tons of juveniles, or 44% (delta method standard errors are 71 thousand tons and 9.4%). For comparison, the regulator calculates that the temporary

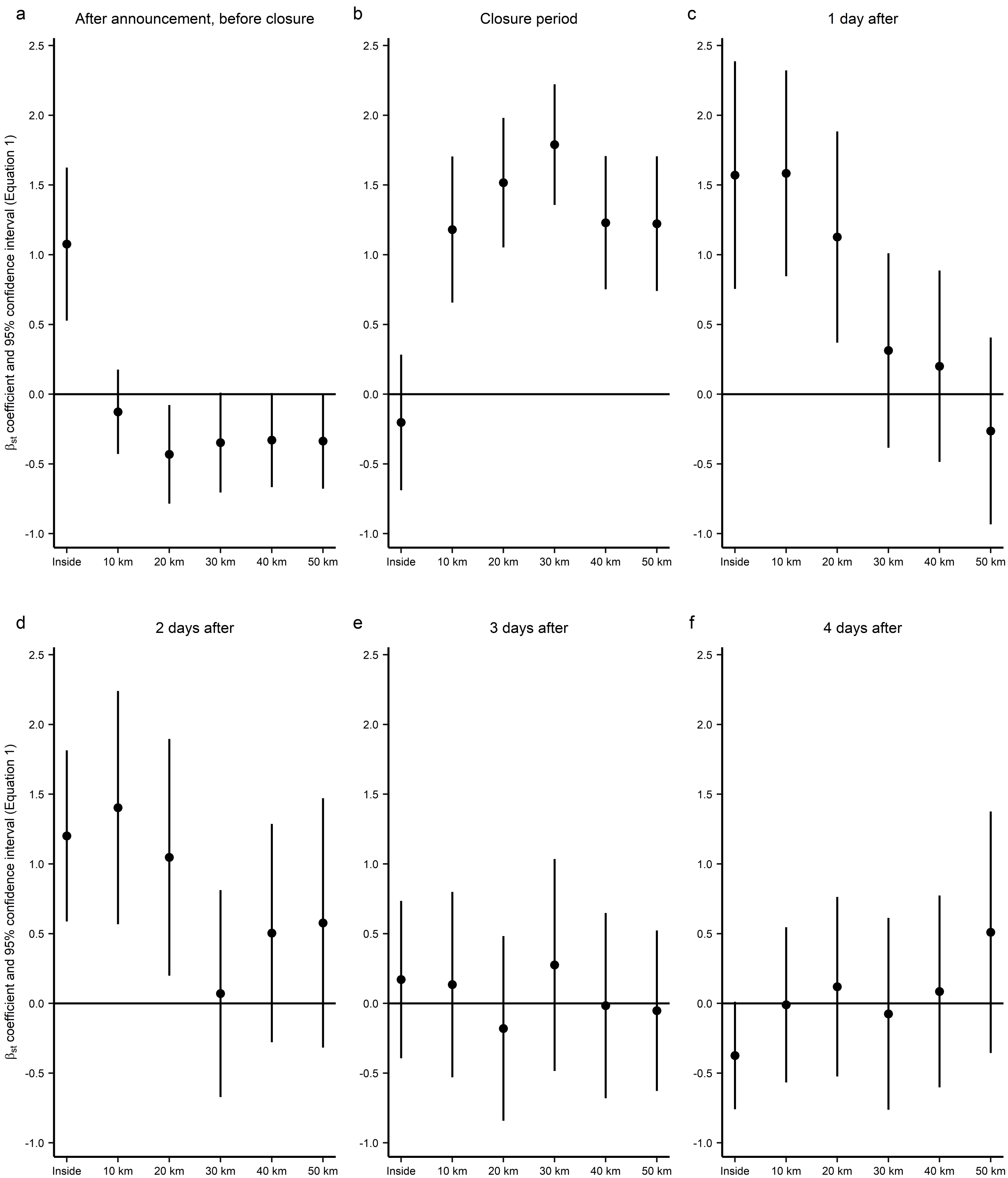
[39]For a given set, tons of juveniles caught equals $\sum_{\ell=[3,3.5)}^{[11.5,12)} w_\ell N_\ell$, where w_ℓ is the weight of an individual in length interval ℓ (e.g. 6 grams) and N_ℓ is the number of individuals in length interval ℓ that the set caught.

spatial closures policy "protected" 1,049,411 tons of juvenile anchoveta in the first and second season of 2017 and the first season of 2018 (PRODUCE, 2017a, 2018b, 2018c). The regulator does not describe how they calculate this number, nor do they define the meaning of "protected" in this context.

Finally, my results are robust to assuming potential closures last for four or five days (instead of three days) and to making my potential closures 40% larger (so that they are the same average size as actual closures). I display the treatment coefficients for these three alternative specifications in Figures B7 to B9. When I convert the treatment coefficients from each of the three specifications into changes in levels and account for the reallocation in tons caught due to the total allowable catch limit, I find that the closures policy increases total juvenile catch by 49%, 66% and 52%, respectively (delta method standard errors are 4.7%, 4.5%, and 5.9%, respectively).

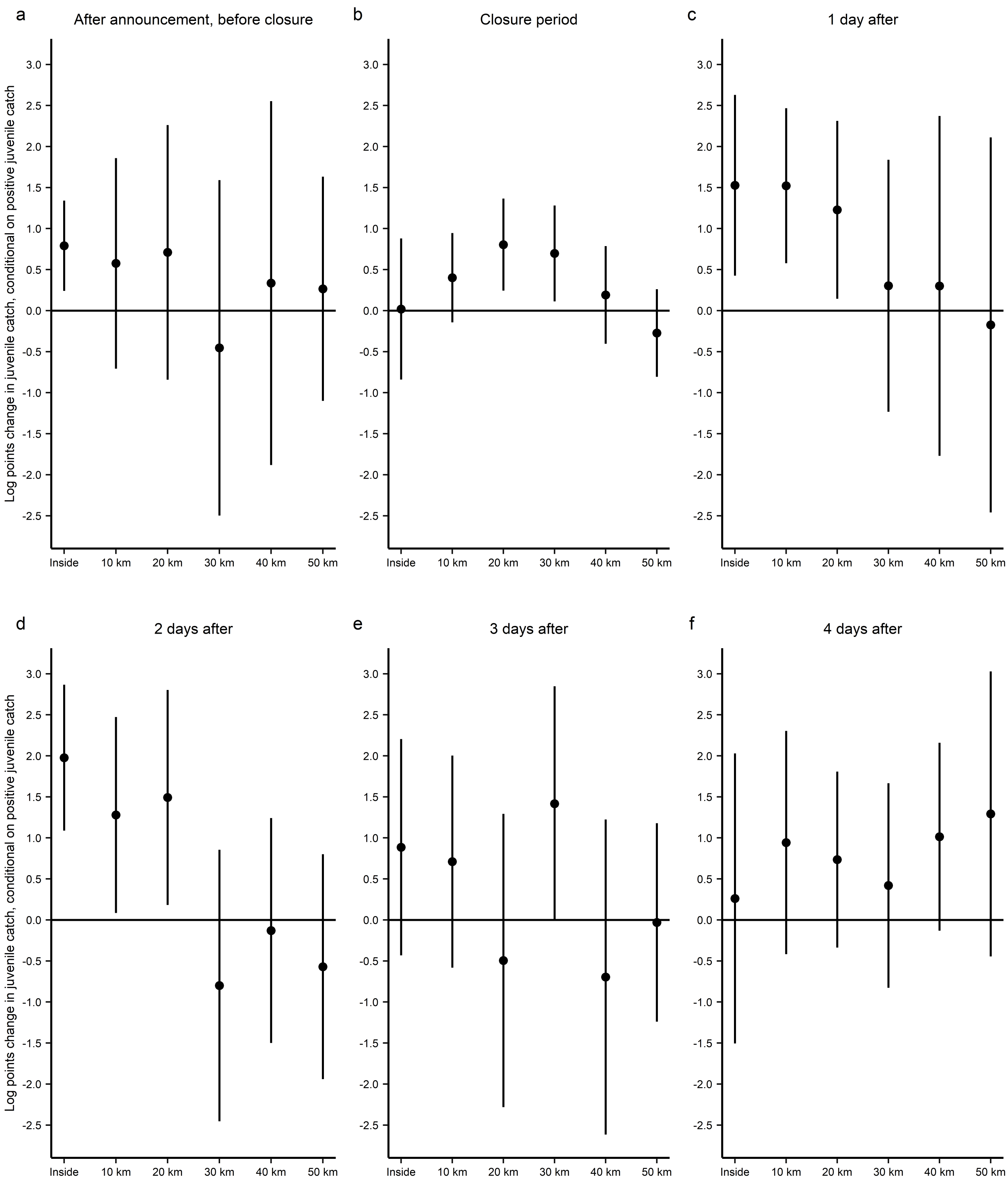
If closures shift juvenile catch forward in time during a fishing season, then my treatment effects would be upward biased because some of the increase in juvenile catch due to closures would have occurred later in the season, even if the closures policy did not exist. This "harvesting" concern also occurs in studies on human mortality (e.g., some of the people killed by heat waves would have died soon anyway). I re-estimate Equation 1 with one change: I interact treatment fraction with an indicator for whether potential closure i occurs in the first- or second-half of a fishing season (defined relative to the start of potential closure i's closure period). I find no evidence of heterogeneity along this dimension and display the treatment coefficients from this regression in Figure B10. When I convert the treatment coefficients into changes in levels and account for the reallocation in tons caught due to the total allowable catch limit, I find that closures increase total juvenile catch by 54% in the first-half of fishing seasons and they also increase total juvenile catch by 54% in the second-half of fishing seasons. This result indicates that closures do not cause significant "harvesting" of juveniles that would have been caught even in the absence of closures (i.e., in the second-half of fishing seasons).

Figure B2: Treatment coefficients from estimating Equation 1



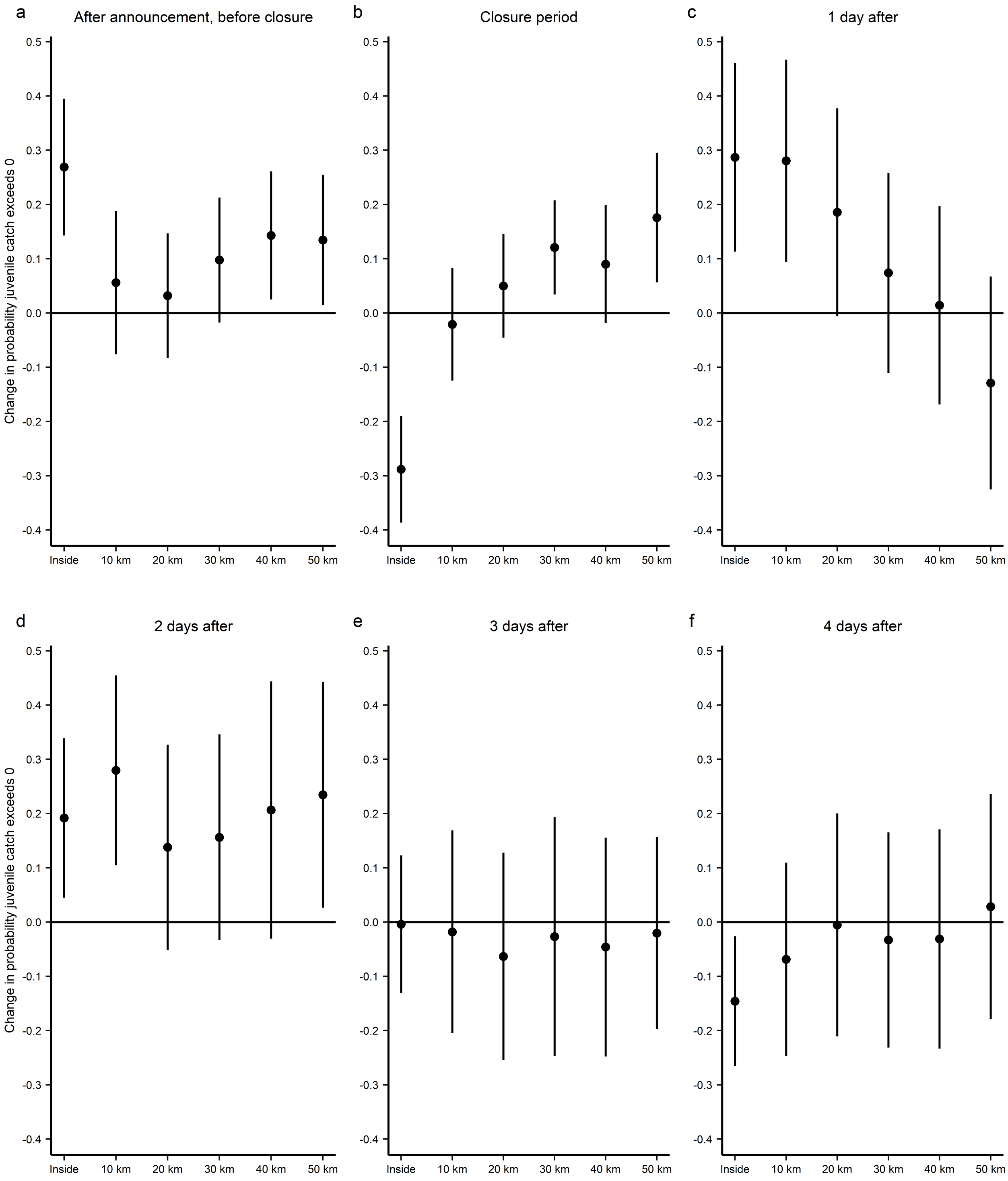
Notes: N = 34,164. Standard errors clustered at level of two-week-of-sample by two-degree grid cell.

Figure B3: Treatment coefficients from dropping observations with zero juvenile catch and re-estimating Equation 1 with a logarithmic transformation instead of an inverse hyperbolic sine transformation



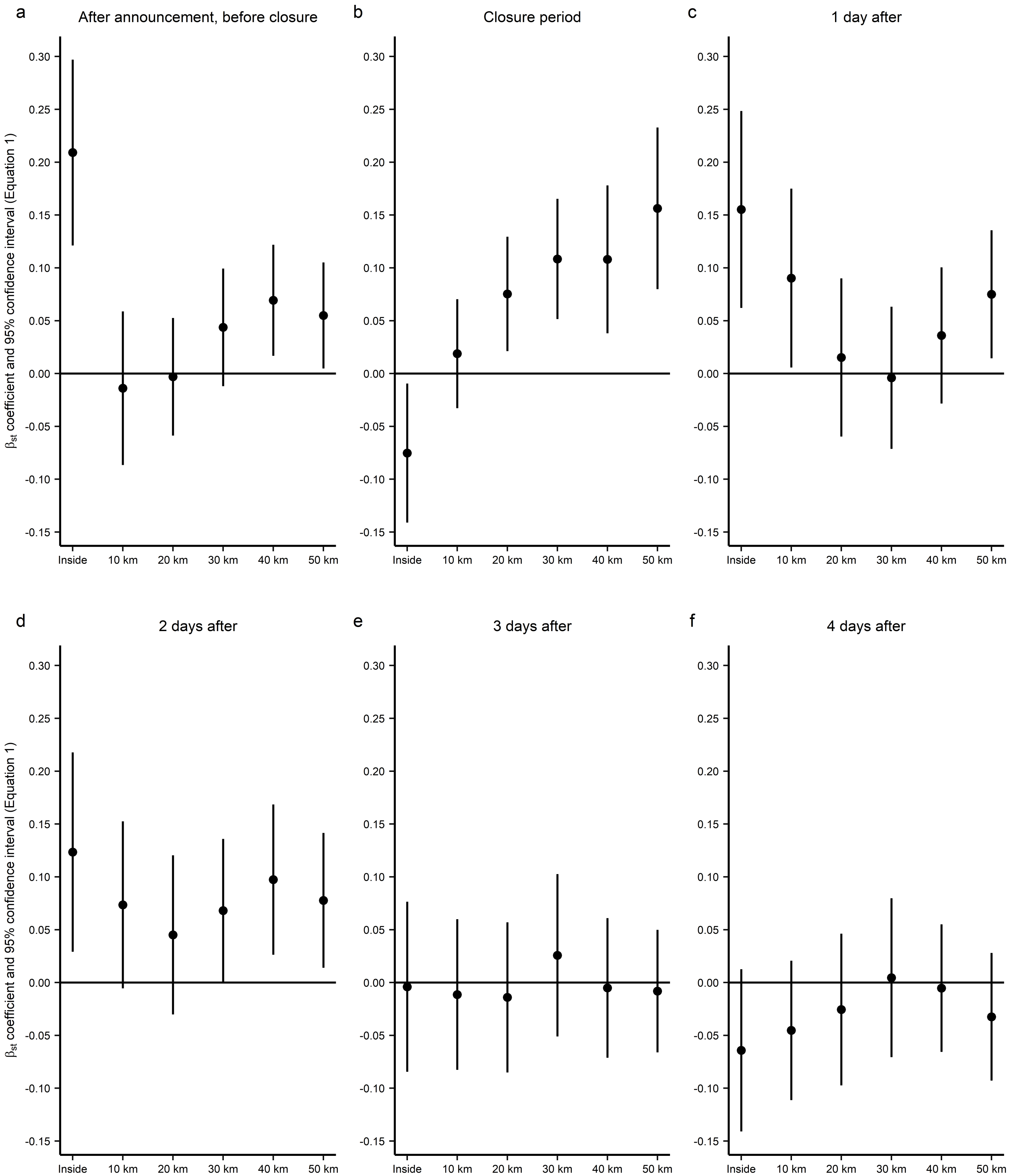
Notes: N = 12,143. Standard errors clustered at level of two-week-of-sample by two-degree grid cell.

Figure B4: Treatment coefficients from re-estimating Equation 1 with a binary indicator for positive juvenile catch as the dependent variable



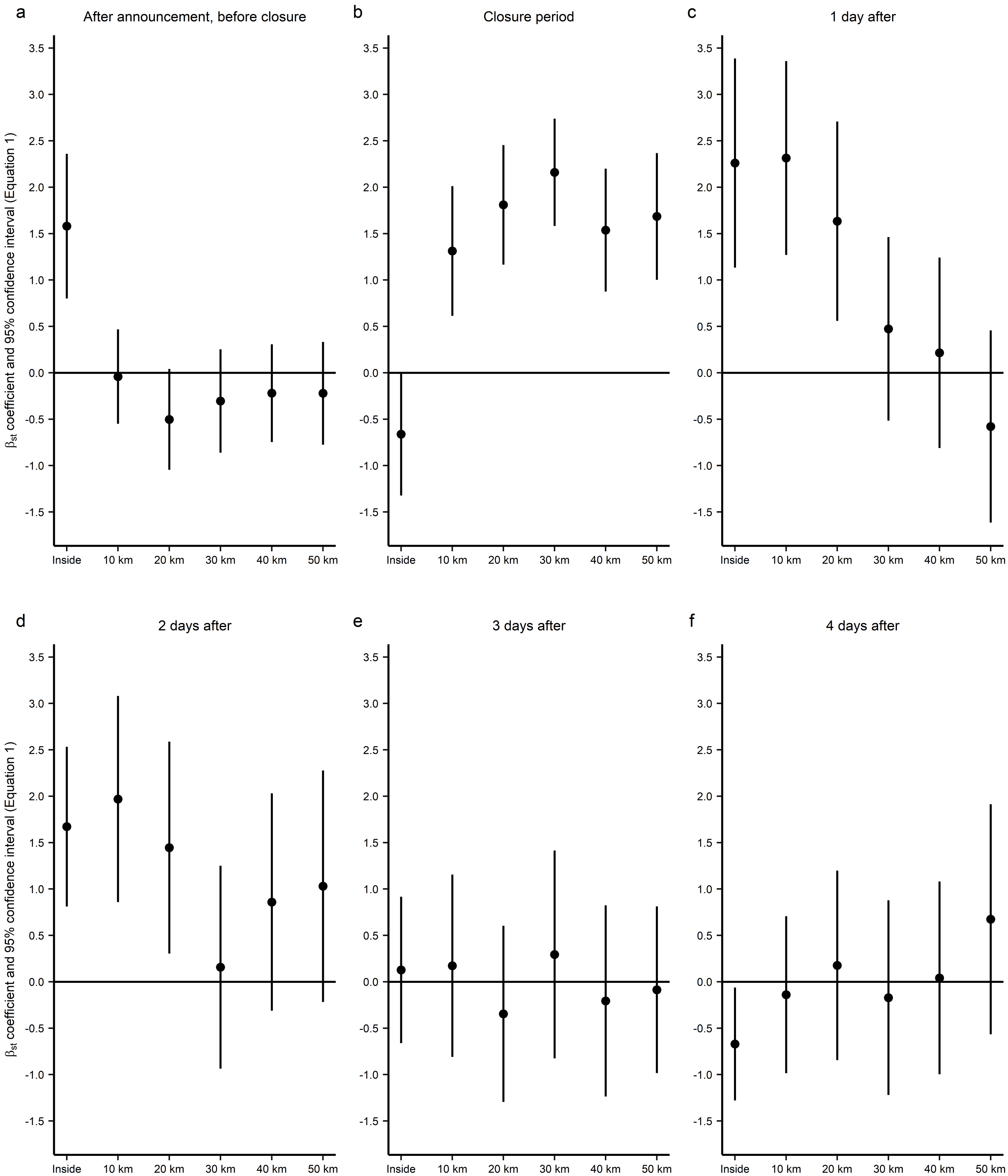
Notes: N = 34,164. Standard errors clustered at level of two-week-of-sample by two-degree grid cell.

Figure B5: Treatment coefficients from re-estimating Equation 1 with a binary indicator for positive treatment fraction, rather than defining treatment fraction as a continuous variable



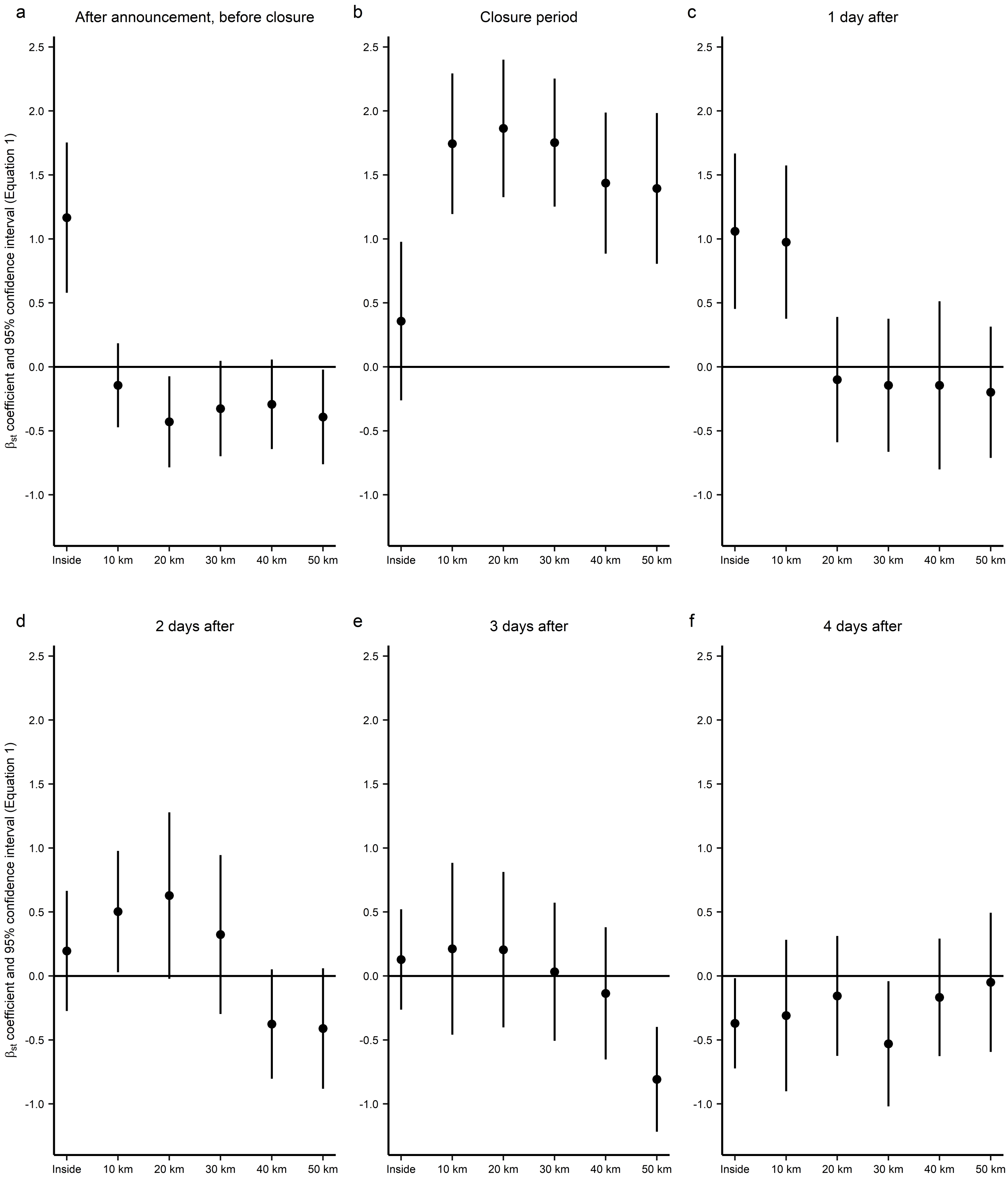
Notes: N = 34,164. Standard errors clustered at level of two-week-of-sample by two-degree grid cell.

Figure B6: Treatment coefficients from re-estimating Equation 1 with tons of juveniles caught as the dependent variable



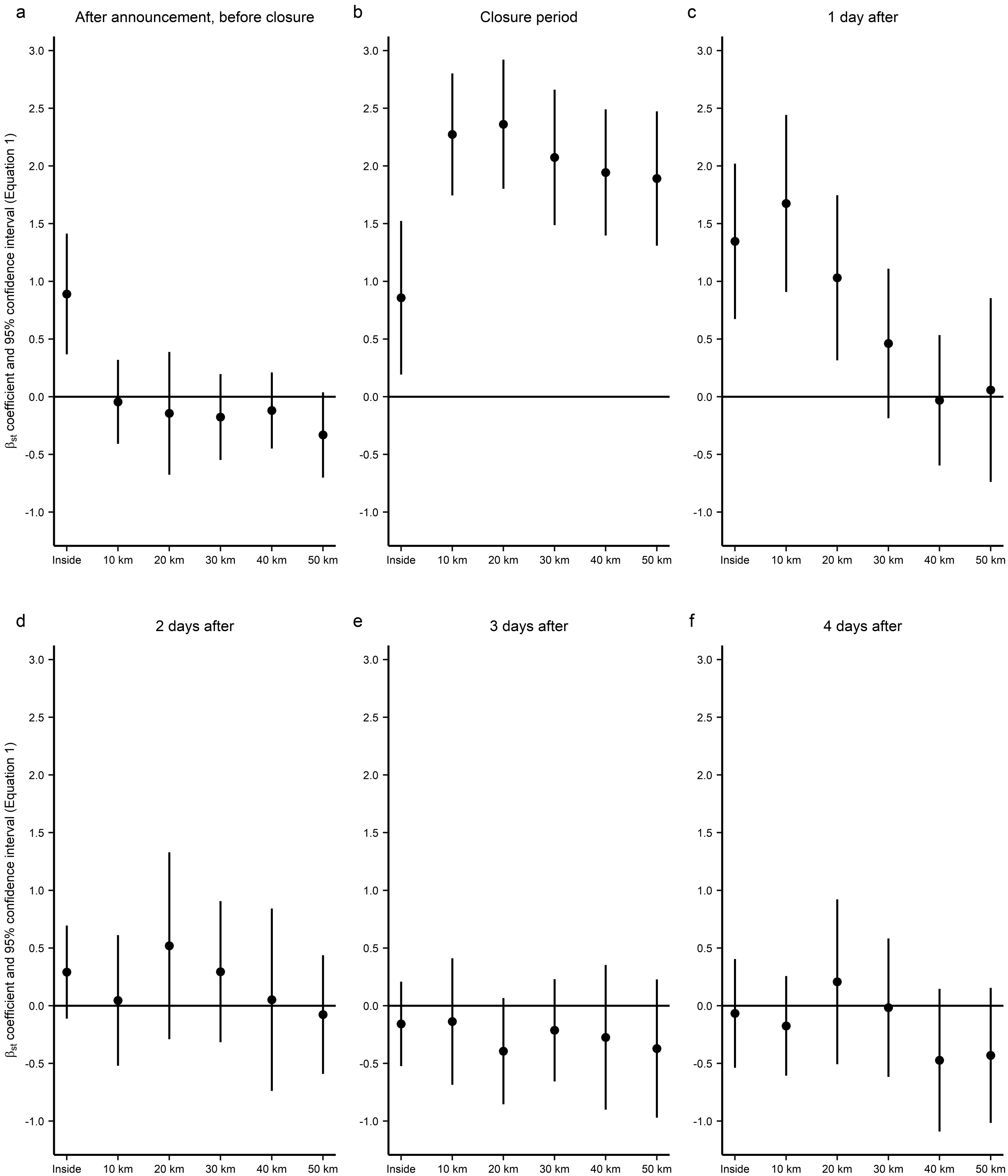
Notes: The dependent variable is the inverse hyperbolic sine of tons of juveniles caught in each potential closure-treatment bin. N = 34,164. Standard errors clustered at level of two-week-of-sample by two-degree grid cell.

Figure B7: Treatment coefficients from re-estimating Equation 1 with potential closures that last four days



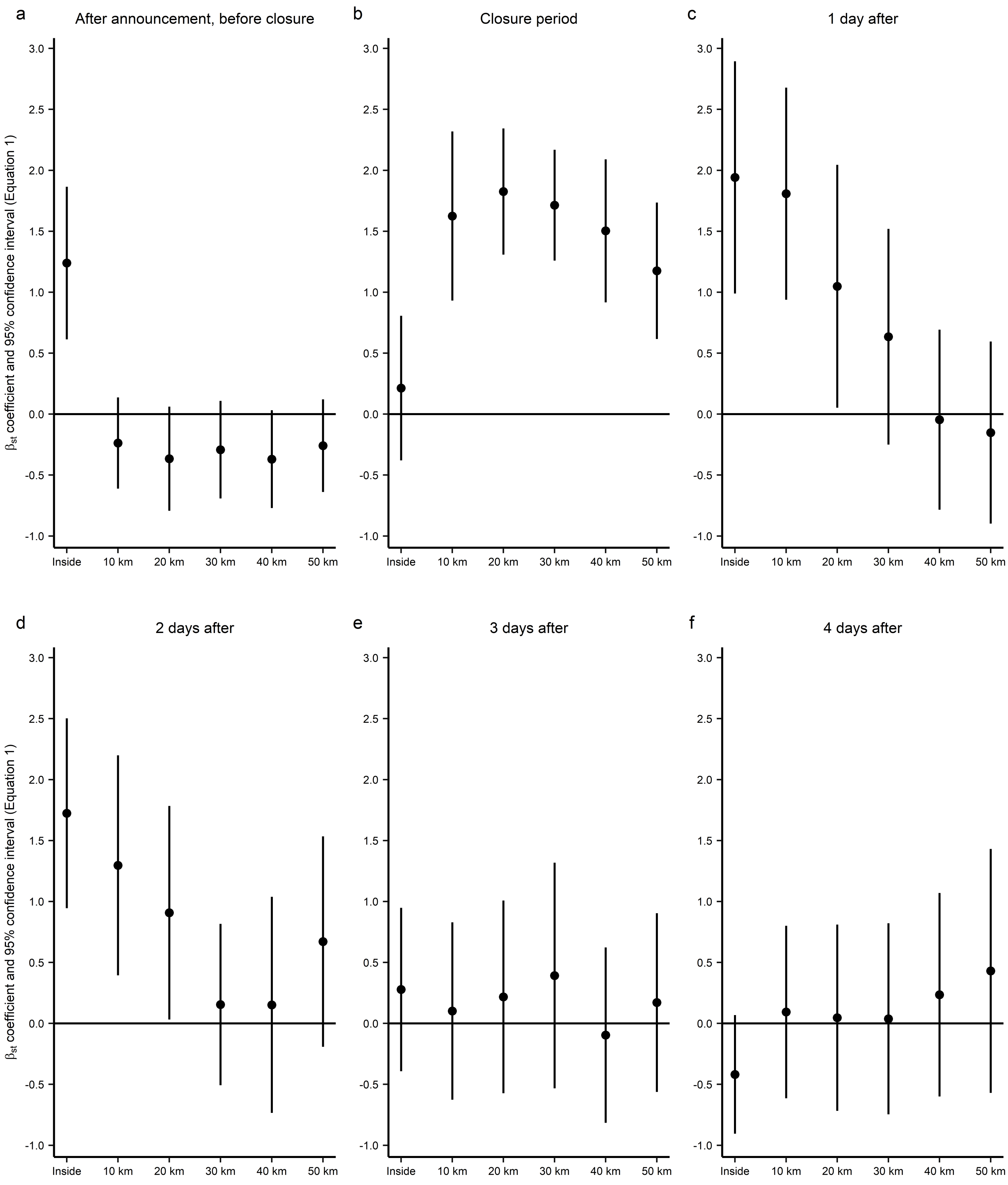
Notes: N = 31,608. Standard errors clustered at level of two-week-of-sample by two-degree grid cell.

Figure B8: Treatment coefficients from re-estimating Equation 1 with potential closures that last five days



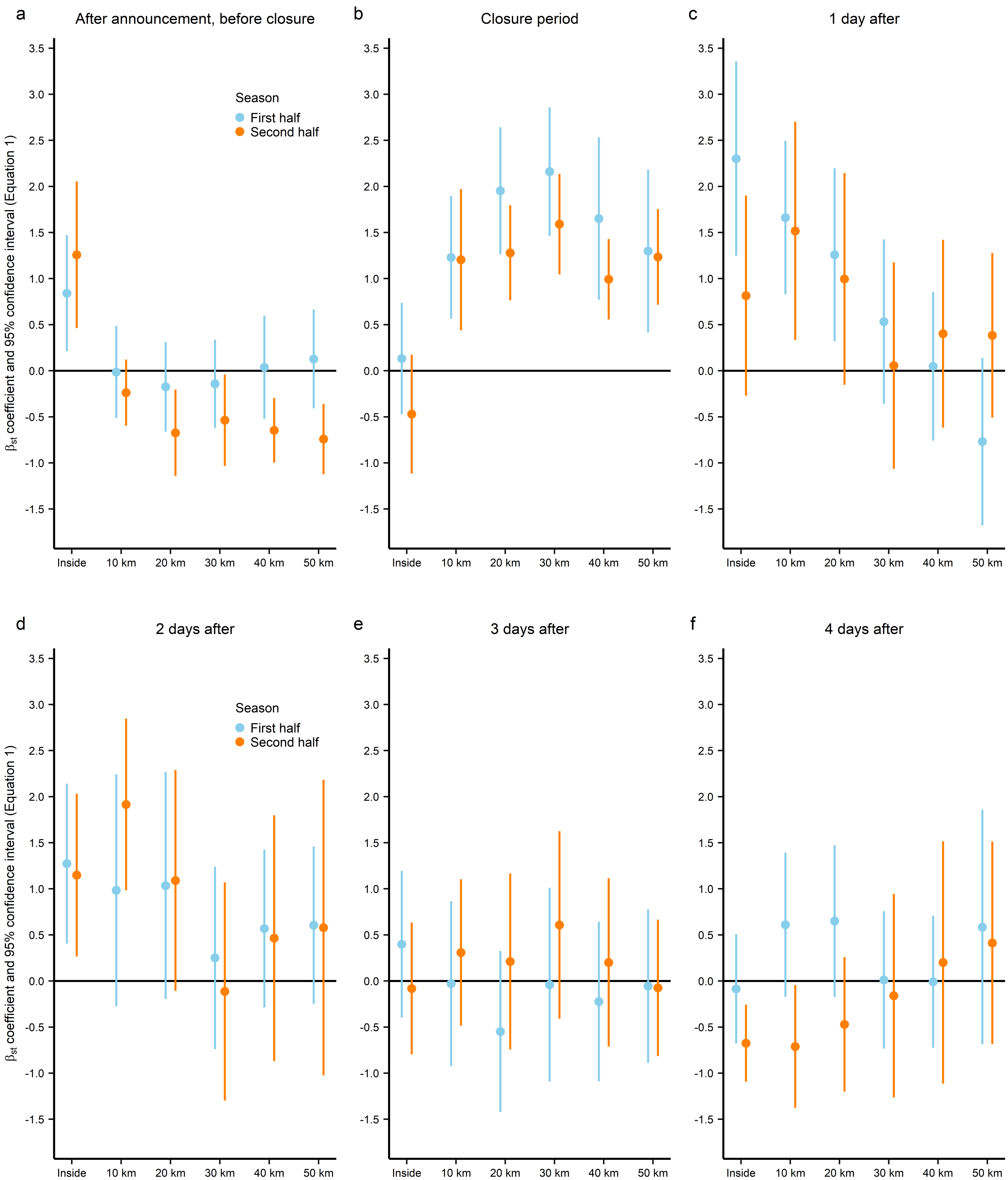
Notes: N = 29,664. Standard errors clustered at level of two-week-of-sample by two-degree grid cell.

Figure B9: Treatment coefficients from re-estimating Equation 1 with potential closures 40% larger



Notes: N = 34,164. Standard errors clustered at level of two-week-of-sample by two-degree grid cell.

Figure B10: Treatment coefficients from re-estimating Equation 1 with time-of-season interactions



Notes: N = 34,164. I re-estimate Equation 1 with one change: I interact treatment fraction with an indicator for whether potential closure i occurs in the first- or second-half of a fishing season (defined relative to the start of potential closure i's closure period). Standard errors clustered at level of two-week-of-sample by two-degree grid cell.

B.3 Re-estimating the effect of the policy on juvenile catch with gridded, balanced panel data

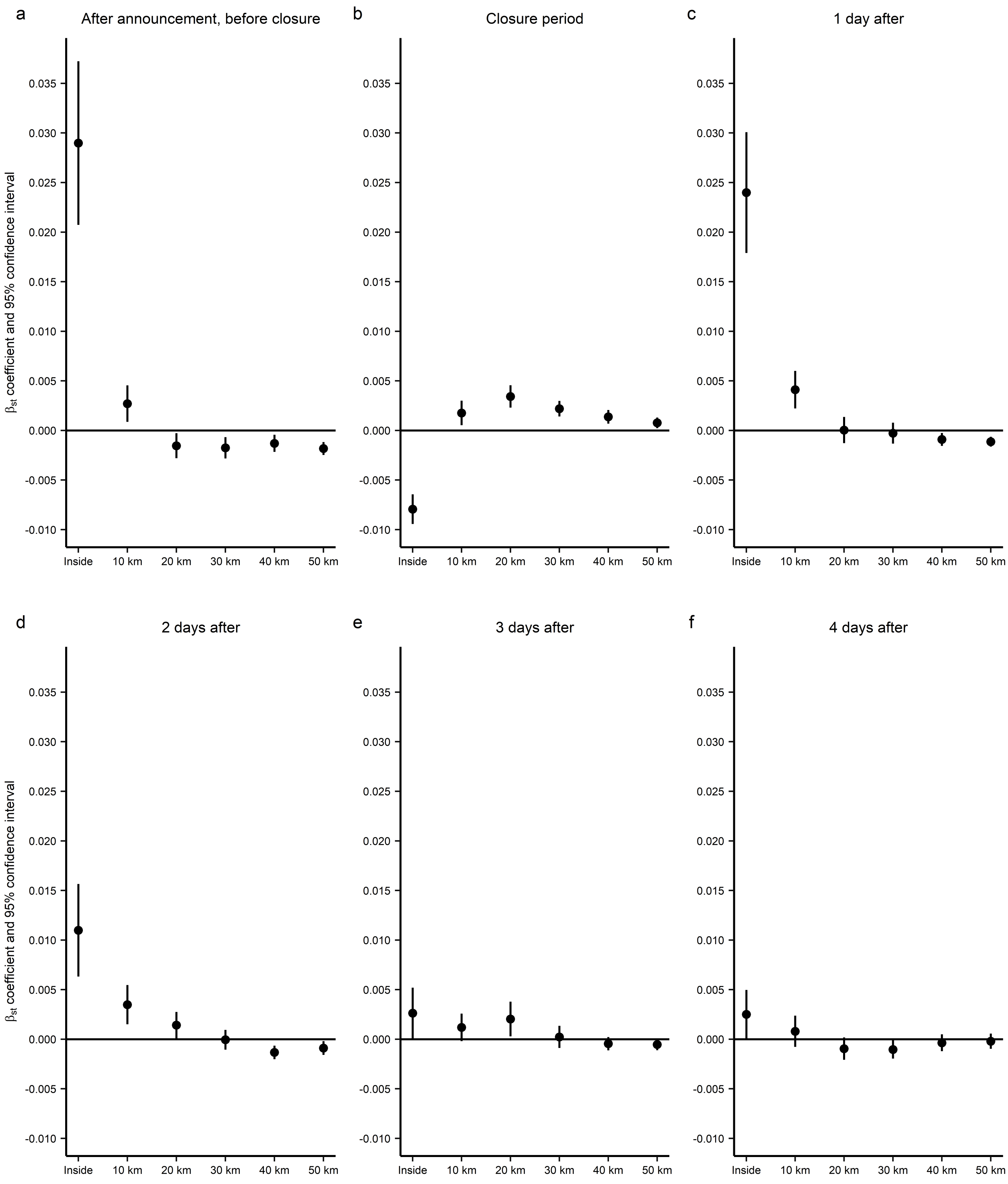
As an alternative estimation approach, I create a regular grid of .05° cells covering the North-Central zone and calculate the millions of juveniles caught in each cell each three-hour period during a fishing season. I rasterize the data at this resolution to match the resolution of treatment assignment as closely as possible without exceeding my server's memory capacity. This procedure yields 95,416,620 observations, or 21,346 grid cells × 4,470 three-hour time periods. I regress juvenile catch in a grid cell-time period on indicators for whether the centroid of the grid cell-time period is inside each of the 36 treatment bins in the treatment window of actual closures, .05° grid cell fixed effects, three-hour time period fixed effects, and two-week-of-sample by two-degree grid cell fixed effects:

$$JuvenileCatch_{jk} = \beta_{st}\mathbb{1}\{jk \in st\} + \alpha_j + \delta_k + \sigma_{wg} + \epsilon_{jk} \tag{7}$$

where j = .05° cell, k = three-hour time period, s = spatial unit, t = (treatment bin) time period, w = two-week-of-sample, and g = two-degree grid cell. For a given cell-period jk and treatment bin st, $\mathbb{1}\{jk \in st\}$ equals 1 if the centroid of jk is inside treatment bin st of an actual closure and equals 0 otherwise. I cluster standard errors at the level of two-week-of-sample by two-degree grid cell. The dependent variable is the inverse hyperbolic sine of millions of juveniles caught, as in Equation 1.

I plot the coefficients of interest, β_{st}, in Figure B11. The coefficient magnitudes are smaller than in Figure B2, possibly due to the large number of zeros in the rasterized data (99.96% of observations have 0 juvenile catch). However, the treatment effects are precisely estimated and the pattern of treatment effects is the same as in my preferred specification (Figure B2). My finding that closures cause temporal and spatial spillovers and increase total juvenile catch is robust to this alternative estimation strategy.

Figure B11: Treatment coefficients from estimating Equation 7



Notes: N = 95,416,620. Standard errors clustered at level of two-week-of-sample by two-degree grid cell.

B.4 Re-estimating the effect of the policy on juvenile catch with actual closures as treated units and potential closures as control units

In my preferred specification, I estimate the effect of the temporary spatial closures policy across potential closures, where actual closures declared by the regulator are only used to calculate the treatment fraction for each potential closure-treatment bin. An alternative estimation approach is to use actual closures declared by the regulator as the treated units and potential closures whose treatment fraction equals 0 as the control units. I estimate the effect of the closures policy with this alternative approach here.

In my preferred specification in Equation 1, I control for characteristics of the sets that generate potential closures, such as the length distribution of anchoveta caught by the sets that generate potential closures. For each actual closure declared by the regulator, I now also construct these same control variables from the sets that occur inside the closure in the 9 to 24 hours before the closure begins.[40] 8 of the 410 actual closures declared by the regulator do not have sets inside them with non-missing length distribution values (see Footnote 41). I drop these 8 actual closures from this analysis because I cannot construct closure-level length distribution control variables for them. I create the same spatial and temporal leads and lags as for potential closures, yielding 14,472 observations (36 treatment bins $\times$ 402 = 14,472). I construct the same fixed effects as in Equation 1 and calculate juvenile catch inside each actual closure-treatment bin by summing juvenile catch over sets that occur inside the same actual closure-treatment bin.

The control units are potential closure-treatment bin observations whose treatment fraction equals 0. I re-estimate Equation 1 with 39,334 observations: 14,472 treated observations and 24,862 control observations. Figure B12 displays the treatment coefficients. The treatment coefficients display the same pattern as in my preferred specification except that there is a small decrease in juvenile catch four days after closures end. When I convert the treatment coefficients into changes in the number of juveniles caught because of the policy, accounting for the reallocation in tons caught due to the total allowable catch limit, I estimate that the policy increases total juvenile catch by 34 billion juveniles, or 32% (delta method standard errors are 3.0 billion and 2.8%, respectively). My finding that closures cause temporal and spatial spillovers and increase total juvenile catch is robust to this alternative estimation strategy.

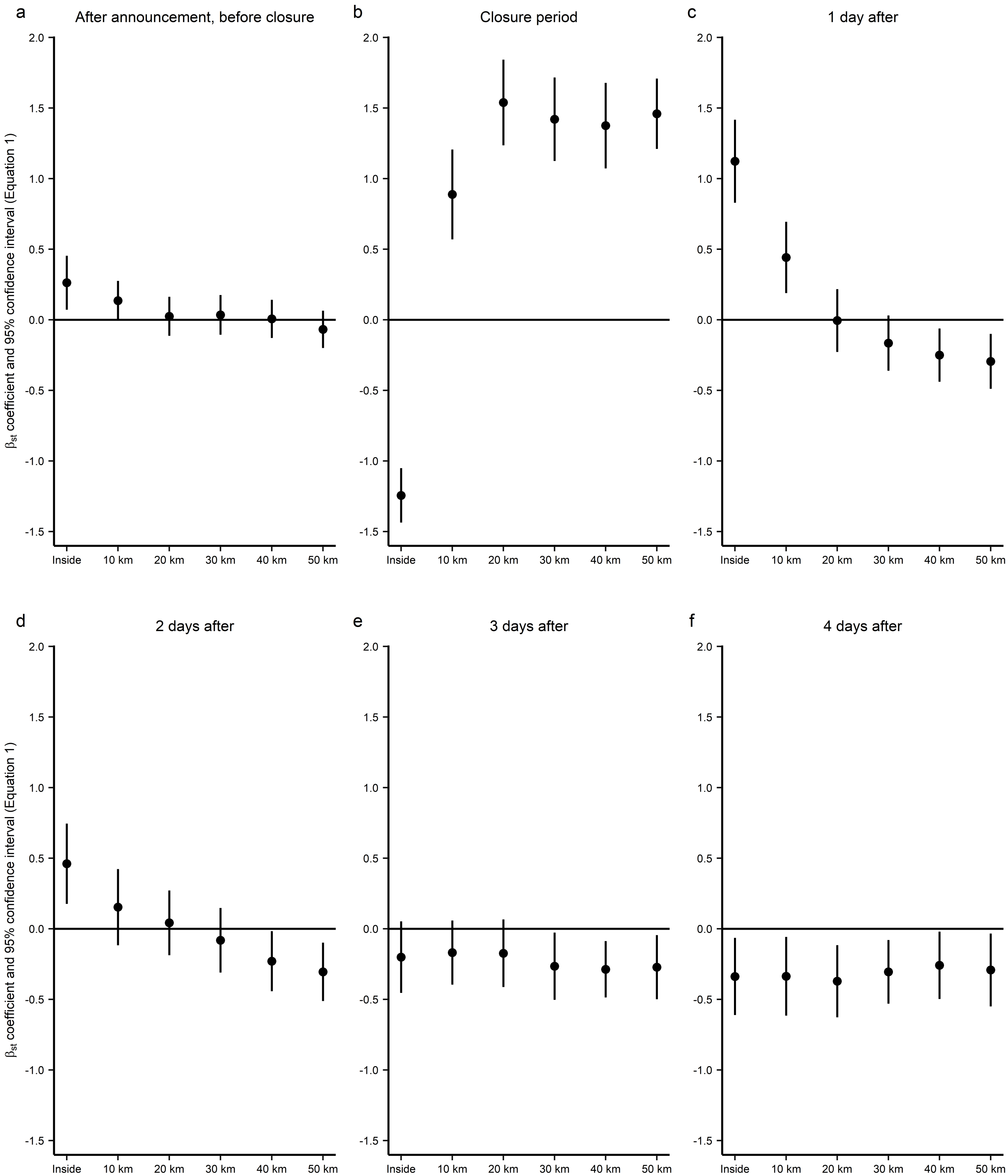
I also re-estimate the effect of the policy using synthetic controls (Abadie et al., 2010;

[40] For the 9% of closures during my study period that begin at 6 AM instead of midnight, I construct control variables from the sets that occur inside the closure in the 12 to 27 hours before the closure begins, because closures that begin at 6 AM must be announced by 6 PM the previous day.

Abadie & Gardeazabal, 2003). For each actual closure declared by the regulator, I construct a synthetic control group from the potential closures whose treatment fraction equals 0. I include as predictors all of the control variables in Equation 1 (excluding fixed effects) as well as pre-period juvenile catch up to 8 days before the beginning of closure periods. I use the Synth package in R, which returns an error for 97 out of 410 actual closures (Abadie et al., 2011). However, I am able to obtain a synthetic control group for each of the remaining 313 actual closures.

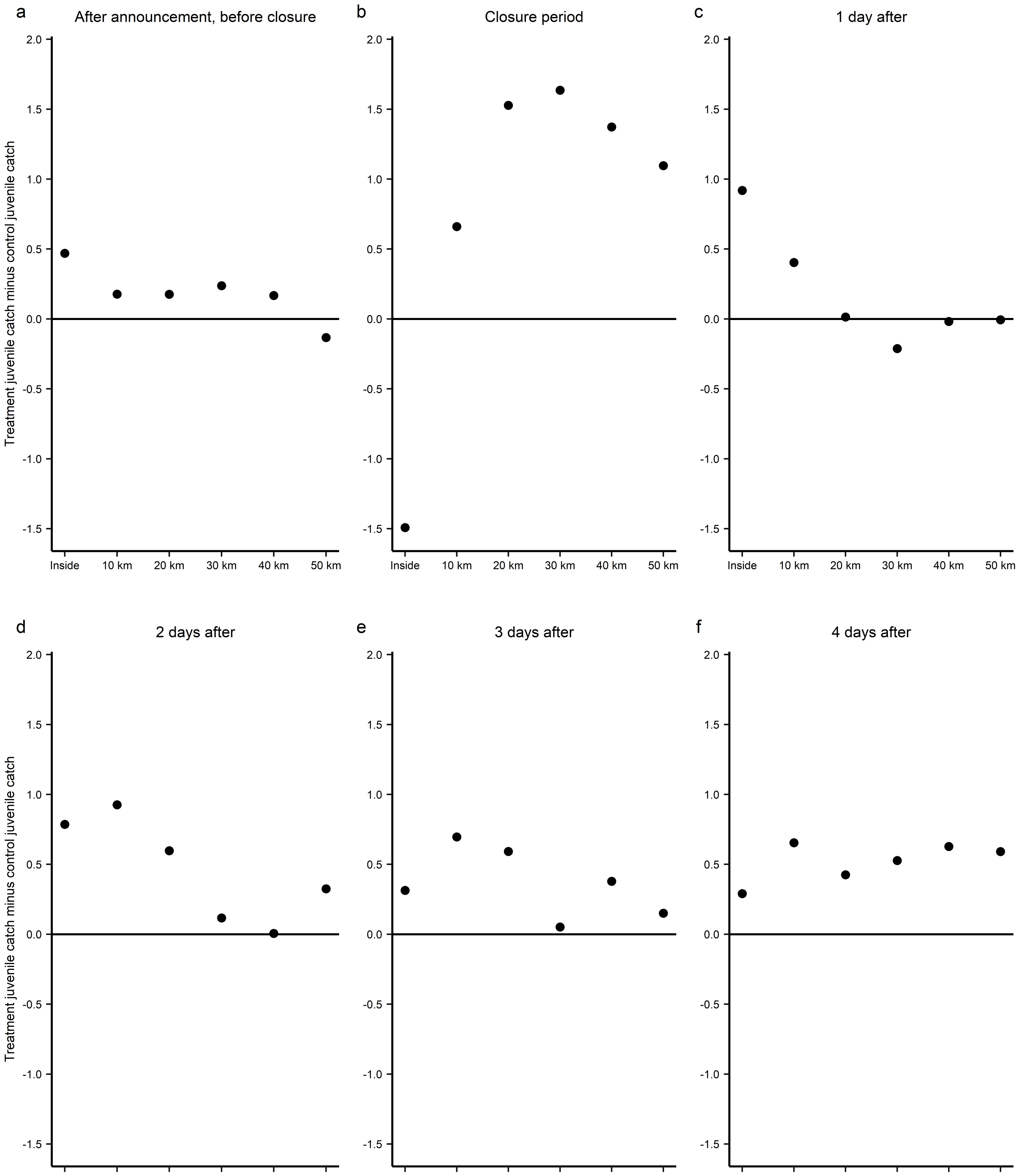
Figure B13 displays the synthetic control results. The y-axis is the average juvenile catch for treated observations (actual closures) minus the average juvenile catch for control observations (weighted average of potential closures). As in my preferred specification, juvenile catch is the inverse hyperbolic sine of millions of juveniles caught. I do not provide p-values in Figure B13 because the synthetic control procedure of computing weights on potential closures for each actual closure is computationally intensive (the single run I performed took several hundred CPU hours). When I convert the difference in average juvenile catch in each treatment bin into changes in the number of juveniles caught because of the policy, accounting for the reallocation in tons caught due to the total allowable catch limit, I estimate that the policy increases total juvenile catch by 40 billion juveniles, or 39%. This result is similar to my preferred estimate of the effect of the temporary spatial closures policy—an increase in juvenile catch of 47 billion juveniles, or 50%—even though it was obtained with a different identification strategy.

Figure B12: Treatment coefficients from re-estimating Equation 1 with actual closures as treated units and potential closures as control units



Notes: Actual closure-treatment bins are the treated units. Potential closure-treatment bins whose treatment fraction equals 0 are the control units. N = 39,422. Standard errors clustered at level of two-week-of-sample by two-degree grid cell.

Figure B13: Synthetic control estimates of the effect of closures



Notes: The y-axis is the average juvenile catch for treated observations (actual closures) minus the average juvenile catch for control observations (weighted average of potential closures), where juvenile catch is the inverse hyperbolic sine of millions of juveniles caught.

C Data Appendix

The main outcome variable of interest in this paper is juvenile catch: the number of individual anchoveta that are caught and are less than 12 cm. There are two challenges in calculating juvenile catch in an unbiased and accurate manner.

First, fishermen may underreport percentage juvenile in the electronic logbook data in order to avoid triggering a closure in the area they are fishing. If I only used raw electronic logbook data to calculate juvenile catch and underreporting is correlated with closures declared by the regulator, my treatment effect estimates would be biased.

Second, even if percentage juvenile reported by fishermen in the electronic logbook data is unbiased, percentage juvenile and tons caught are not sufficient measures of juvenile catch because the number of individuals caught depends on the length distribution of those individuals. For example, consider two sets that both catch 40 tons of anchoveta that are 20% juvenile. In the first set, 20% of individuals are between 11.5 and 12 cm and 80% of individuals are between 12 and 12.5 cm (actual length distributions are much more diffuse; see Figure C1). In the second set, 20% of individuals are between 10 and 10.5 cm and 80% of individuals are between 14 and 14.5 cm. The weight of an anchoveta in grams equals $.0036length^{3.238}$ (IMARPE, 2019). Therefore, 683,137 juvenile anchoveta are caught by the first set and 469,685 are caught by the second set, even though both sets caught the same tons and percentage juvenile. My results are robust to measuring juvenile catch in terms of tons of juveniles caught (Figure B6).

Recall from Section 4 that fishermen report percentage juvenile to the regulator in the electronic logbook data, but not the length distribution from which percentage juvenile is calculated (percentage juvenile is the percentage of measured individuals that are less than 12 cm). I obtained a supplementary electronic logbook dataset for a group of vessels that report length distribution data to their owners. These vessels represent 55% of landings and their data was provided by Sociedad Nacional de Pesquería (SNP), a consortium of fishing companies, in January 2020.

To calculate juvenile catch for each set, I first use the length distribution values from sets in the SNP electronic logbook data to impute length distributions for non-SNP sets, based on the location, time, and percentage juvenile caught by non-SNP sets. After obtaining length distributions for all sets in the electronic logbook data, I match sets to landings events. I then use the percentage juvenile measured by third-party inspectors at landing to update length distributions in the electronic logbook data and calculate juvenile catch for each set.

Specifically, first I identify sets in the full electronic logbook data (reported to the regulator) that are also in the SNP data based on unique vessel identifiers and the time each set occurred. I calculate the number of individual anchoveta (both juveniles and adults) caught by these sets based on their length distribution and tons caught. When percentage juvenile for a set in the SNP data does not match its counterpart in the full electronic logbook data (i.e., the vessel reported a different percentage juvenile to its owner than to the regulator), I shift the length distribution up or down in half-cm increments until the absolute difference between the implied percentage juvenile (updated percentage of individuals that are less than 12 cm) and the percentage juvenile reported to the regulator is minimized (i.e., a one unit shift in either direction would result in a larger absolute difference between implied and reported percentage juvenile).

I then impute length distributions for non-SNP sets as follows. For each two-week-of-sample by two-degree grid cell, I calculate the individuals-weighted average proportion of individuals in each half-cm length interval. Given the percentage juvenile value for each non-SNP set, I adjust the length distribution for that set's two-week-of-sample by two-degree grid cell to match the set's percentage juvenile value. For sets with percentage juvenile above (below) the individuals-weighted average percentage juvenile for their two-week-of-sample by two-degree grid cell, I inflate (deflate) the proportion of individuals below 12 cm and deflate (inflate) the proportion of individuals above 12 cm so that the imputed length distribution for each set implies a percentage juvenile equal to the percentage juvenile reported for that set.[41] I use the resulting length distribution and tons caught to calculate the number of individuals caught by non-SNP sets.

For example, suppose the reported percentage juvenile for a non-SNP set is 20%, the individuals-weighted average percentage juvenile among SNP sets in the two-week-of-sample by two-degree grid cell is 10%, and the average length distribution for the two-week-of-sample by two-degree grid cell is as follows: 2% of individuals are between 11 and 11.5 cm, 8% of individuals are between 11.5 and 12 cm, 60% of individuals are between 12 and 12.5 cm, and 30% of individuals are between 12.5 and 13 cm. Then the imputed length distribution for the non-SNP set would be: 4% of individuals are between 11 and 11.5 cm, 16% of individuals are between 11.5 and 12 cm, 53.33% of individuals are between 12 and 12.5 cm, and 26.67% of individuals are between 12.5 and 13 cm. This length distribution implies that the average weight of individuals caught by this set is 12.1 grams. If the set caught 50 tons of anchoveta, then it caught 4,132,685 individual anchoveta, of which 826,537 are juvenile.

Next, I match sets to landing events in order to correct the length distribution, percentage

[41] 96.5% of non-SNP sets occur in a two-week-of-sample by two-degree grid cell that also contains SNP sets. For the remaining 3.5% of non-SNP sets, I use the average length distribution at the two-week-of-sample level in the above procedure. 58 non-SNP sets (.04%) occur during a two week period without any SNP sets. I record the length distribution values, number of individuals caught, and number of juveniles caught as missing for these 58 sets.

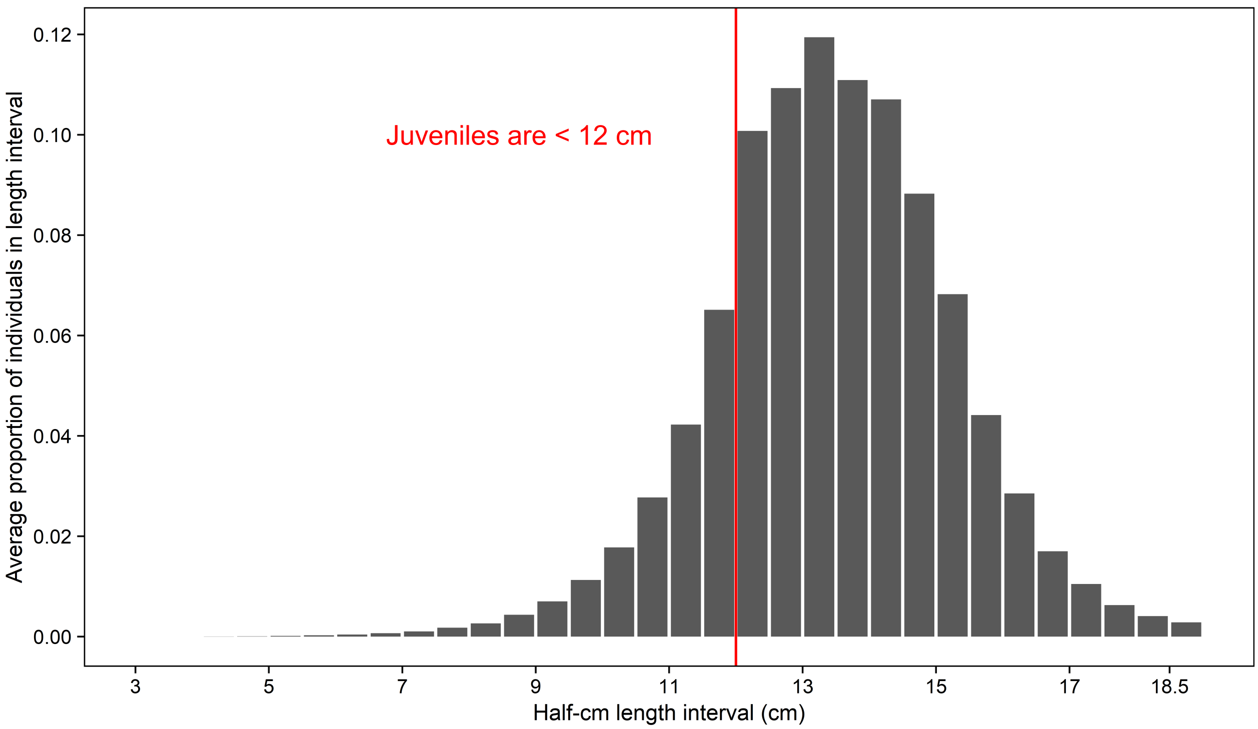
juvenile, and number of individuals caught by each set. Percentage juvenile in the landings data is less prone to manipulation than percentage juvenile in the electronic logbook data because third-party inspectors are from one of three international firms and are paid by the government, not by fishing companies (PRODUCE, 2018d).

Fishermen report when each fishing trip begins and ends in the electronic logbook data. For each landing event by a vessel, I record the vessel's most recent preceding electronic logbook fishing trip and the sets that occurred on the trip. I matched 93.1% of sets to landing events. When the individuals-weighted average percentage juvenile across sets on a trip does not equal the percentage juvenile measured by third-party inspectors at landing, I multiply each set-level percentage juvenile value by the ratio of landing-level percentage juvenile to average set-level percentage juvenile. For the 6.9% of sets I was unable to match to landing events, I multiply percentage juvenile by the ratio of average landing-level percentage juvenile to average set-level percentage juvenile, where averages are calculated among matched sets in the same two-week-of-sample by two-degree grid cell and weighted by number of individuals caught.

For example, suppose there are two sets on a trip, the first set caught 1 million individuals of which 10% are reported juvenile, the second set caught 4 million individuals of which 5% are reported juvenile, and 12% juvenile is measured at landing, when the fishing trip ends. The "corrected" percentage juvenile values are 20% and 10% for the first and second set and the weighted average percentage juvenile across sets is now 12%. I make additional adjustments when this procedure results in set-level percentage juvenile values that are undefined or greater than 100%. After this procedure, percentage juvenile averaged across sets in a trip equals landing-level percentage juvenile.

Finally, I shift the length distribution of each set up or down in half-cm increments until the absolute difference between the implied percentage juvenile (updated percentage of individuals that are less than 12 cm) and the corrected percentage juvenile is minimized. I use the resulting length distribution to calculate the corrected number of individuals caught by each set. The number of juveniles caught by each set is the corrected number of individuals times the corrected percentage juvenile. The procedure described in this section preserves the resolution of the electronic logbook data while ensuring that my main outcome of interest—juvenile catch at a given location and time—is not systematically manipulated.

Figure C1: Corrected length distribution of anchoveta caught in the North-Central zone, 2017 to 2019 fishing seasons



Notes: The y-axis indicates the average proportion of anchoveta caught in each half-cm length interval, weighted by the number of individuals caught by each set. I calculated these values from the corrected electronic logbook data. There are 246,914 sets (observations) in the corrected electronic logbook data. 18.3% of individuals caught during my study period are juvenile.

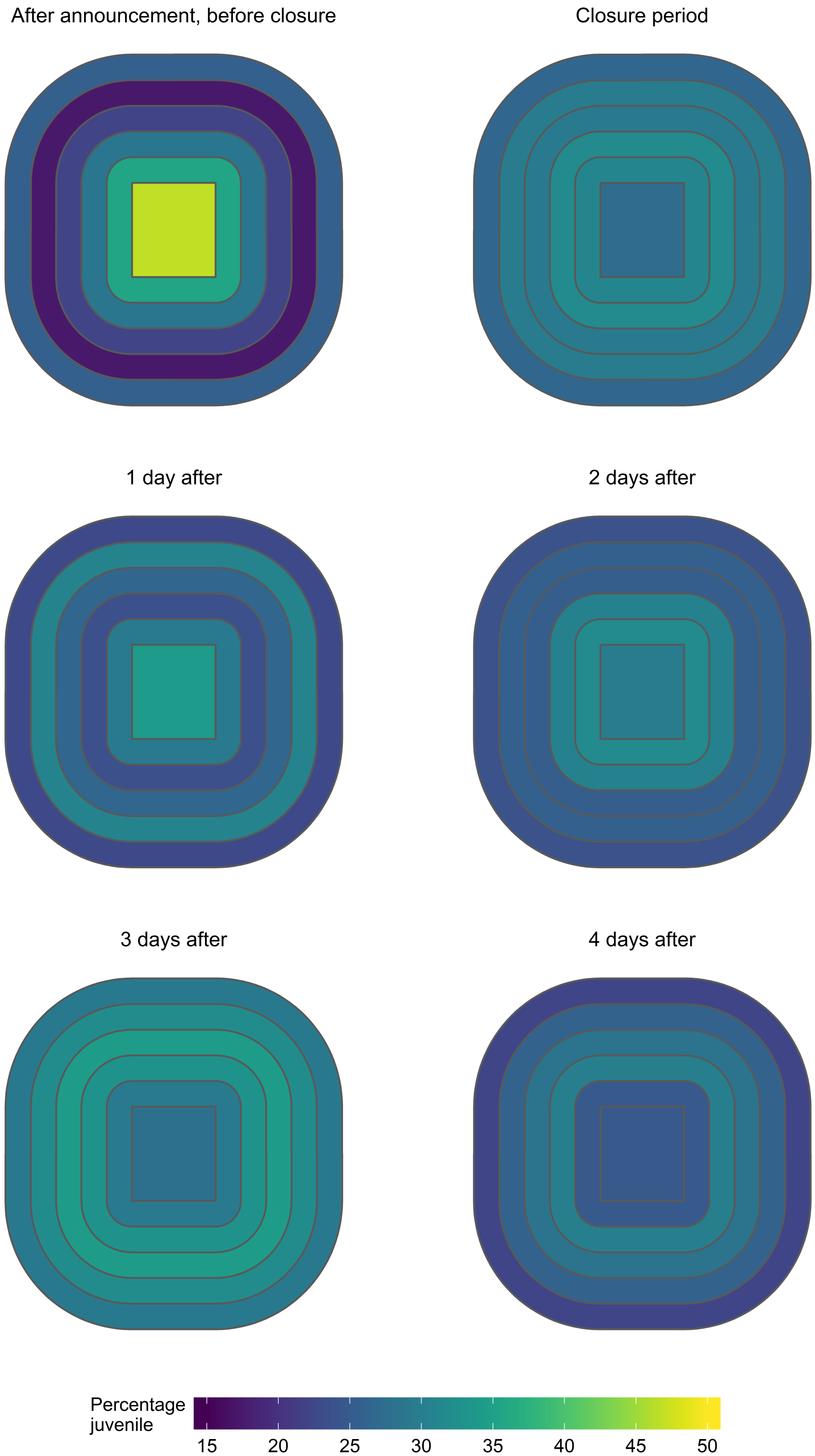


Figure C2: Individuals-weighted average percentage juvenile in each treatment bin, for actual closures declared by the regulator. Percentage juvenile values are from the corrected electronic logbook data. Average percentage juvenile outside the treatment window is 9%.

D Proofs of Propositions 1 to 3

I prove Proposition 2 first because the proof of Proposition 1 relies on the proof of Proposition 2a.

Proof of Proposition 2a. To prove the equality of expected profit in locations g and k, suppose the contradiction that $\exists j$ s.t. $\mathrm{E}[\pi_{j,g}(\mu_g, I_{-j,g})|\tilde{\mu_g}, \tilde{\mu_k}, C=0] \neq \mathrm{E}[\pi_{j,k}(\mu_k, I_{-j,k})|\tilde{\mu_g}, \tilde{\mu_k}, C=0]$. Suppose without loss of generality that $\mathrm{E}[\pi_{j,g}(\mu_g, I_{-j,g})|\tilde{\mu_g}, \tilde{\mu_k}, C=0] > \mathrm{E}[\pi_{j,k}(\mu_k, I_{-j,k})|\tilde{\mu_g}, \tilde{\mu_k}, C=0]$. If $j \in I_k$, then vessel j could increase expected profit by choosing g instead. If $j \in I_g$, $\exists r \in I_k$ s.t. vessel r could increase their expected profit by choosing g instead (because vessels are identical). To satisfy the definition of a Bayes-Nash equilibrium, expected profit from fishing in location g must equal expected profit from fishing in location k. The same argument proves the claim for when $C=1$ as well.

Uniqueness. Suppose there exists Bayes-Nash equilibria $(\hat{I}^*_g, \hat{I}^*_k)$ and $(\hat{I}^*_h, \hat{I}^*_k)$ such that $(\hat{I}^*_g, \hat{I}^*_k) \neq (I^*_g, I^*_k)$ and $(\hat{I}^*_h, \hat{I}^*_k) \neq (I^*_h, I^*_k)$. Without loss of generality, suppose $\hat{I}^*_g > I^*_g$. Then $\hat{I}^*_k < I^*_k$ since I is fixed. Since profit is decreasing in the number of vessels who fish in the same location ($\frac{\partial \pi_{i,\ell}(\mu_\ell, I_{-i,\ell})}{\partial I_{-i,\ell}} < 0$), expected profit from fishing in location g is lower in the $(\hat{I}^*_g, \hat{I}^*_k)$ equilibrium than in the (I^*_g, I^*_k) equilibrium ($\mathrm{E}[\pi_{i,g}(\mu_g, \hat{I}^*{}_{-i,g})|\tilde{\mu_g}, \tilde{\mu_k}, C=0] < \mathrm{E}[\pi_{i,g}(\mu_g, I^*_{-i,g})|\tilde{\mu_g}, \tilde{\mu_k}, C=0] \quad \forall i$). Similarly, expected profit from fishing in location k is higher in the $(\hat{I}^*_g, \hat{I}^*_k)$ equilibrium than in the (I^*_g, I^*_k) equilibrium ($\mathrm{E}[\pi_{i,k}(\mu_k, \hat{I}^*{}_{-i,k})|\tilde{\mu_g}, \tilde{\mu_k}, C=0] > \mathrm{E}[\pi_{i,k}(\mu_k, I^*_{-i,k})|\tilde{\mu_g}, \tilde{\mu_k}, C=0] \ \forall i$). By the proof of Proposition 2a, $\mathrm{E}[\pi_{i,g}(\mu_g, I^*_{-i,g})|\tilde{\mu_g}, \tilde{\mu_k}, C=0] = \mathrm{E}[\pi_{i,k}(\mu_k, I^*_{-i,k})|\tilde{\mu_g}, \tilde{\mu_k}, C=0] \quad \forall i$. Then $(\hat{I}^*_g, \hat{I}^*_k)$ cannot be a Bayes-Nash equilibrium because $\mathrm{E}[\pi_{i,g}(\mu_g, \hat{I}^*{}_{-i,g})|\tilde{\mu_g}, \tilde{\mu_k}, C=0] < \mathrm{E}[\pi_{i,k}(\mu_k, \hat{I}^*{}_{-i,k})|\tilde{\mu_g}, \tilde{\mu_k}, C=0] \quad \forall i$. $(\hat{I}^*_h, \hat{I}^*_k)$ cannot be a Bayes-Nash equilibrium by the same argument. Therefore the Bayes-Nash equilibria (I^*_g, I^*_k) and (I^*_h, I^*_k) are unique.

Proof of Proposition 2b. Note that $\frac{\partial I_\ell}{\partial \mu_\ell} = 0$; fishing location decisions I_ℓ depend on $\tilde{\mu_\ell}$, but not true productivity μ_ℓ because μ_ℓ is unobserved. Then $\pi_{i,g}(\mu_g, I_{-i,g}) > \pi_{i,k}(\mu_k, I_{-i,k})$ $\forall i$ because vessels are identical and $\frac{\partial \pi_{i,\ell}(\mu_\ell, I_{-i,\ell})}{\partial \mu_\ell} > 0$. If the closure announcement contains valuable information in that it informs vessels that the true productivity of location g is higher than location k, then vessels that happen to fish in location g when $C=0$ have higher profits because there is no closure announcement that vessels can use to change their fishing location decisions.

Proof of Proposition 1. I will first prove $\frac{\partial I_\ell}{\partial \tilde{\mu_\ell}} > 0$ in the case where congestion costs are the same across locations, then use this fact to complete the proof. Because marginal congestion costs are in fact higher in location h than in locations g and k, the positive signal from the closure announcement must be sufficiently large in order for there to be an increase in the number of vessels fishing in h when $C = 1$ relative to the number of vessels fishing in g when $C = 0$ ($\tilde{\mu_h} >> \tilde{\mu_k}$ in order for $I_h^* > I_g^*$). If this condition is met, total juvenile catch will be higher with the closure than without it as long as percentage juvenile is sufficiently high in location h relative to locations g and k.

To prove $\frac{\partial I_\ell}{\partial \tilde{\mu_\ell}} > 0$ when congestion costs are the same across locations, I suppress some of the arguments of expected profit for notational compactness. For example, let $\mathrm{E}[i, g]$ denote $\mathrm{E}[\pi_{i,g}(\mu_g, I_{-i,g})|\tilde{\mu_g}, \tilde{\mu_k}, C = 0]$. Suppose the contradiction, that the number of vessels who choose location ℓ is not increasing in $\tilde{\mu_\ell}$ ($\frac{\partial I_\ell}{\partial \tilde{\mu_\ell}} \leq 0$). Since profit is decreasing in the number of vessels who fish in the same location ($\frac{\partial \pi_{i,\ell}(\mu_\ell, I_{-i,\ell})}{\partial I_{-i,\ell}} < 0$), expected profit from fishing in location h is higher than in g ($\mathrm{E}[i, h^*] > \mathrm{E}[i, g^*]\ \ \forall i$), because marginal congestion costs are the same in h and g and the number of vessels who choose h is not higher by assumption ($I_h^* \leq I_g^*$). Because the total number of vessels is fixed, the number of vessels who choose location k when $C = 1$ is greater than or equal to the number of vessels who choose location k when $C = 0$ ($I^*_{k|C=1} \geq I^*_{k|C=0}$). Since vessels have the same beliefs about k's productivity in both states of the world, expected profit from fishing in location k when $C = 0$ is at least as great as expected profit from fishing in k when $C = 1$ ($\mathrm{E}[i, k^*|C = 0] \geq \mathrm{E}[i, k^*|C = 1]\ \ \forall i$). Since (I_g^*, I_k^*) is a Bayes-Nash equilibrium, $\mathrm{E}[i, g^*] = \mathrm{E}[i, k^*|C = 0]$ by the proof of Proposition 2a. Then $\mathrm{E}[i, h^*] > \mathrm{E}[i, k^*|C = 1]\ \ \forall i$ because $\mathrm{E}[i, h^*] > \mathrm{E}[i, g^*] = \mathrm{E}[i, k^*|C = 0] \geq \mathrm{E}[i, k^*|C = 1]$. Then (I_h^*, I_k^*) cannot be a Bayes-Nash equilibrium by the proof of Proposition 2a. Contradiction. Therefore, $\frac{\partial I_\ell}{\partial \tilde{\mu_\ell}} > 0$ when congestion costs are the same across locations.

However, marginal congestion costs are in fact higher in location h (because h covers less area than g and k). Though vessels believe mean productivity is higher in h than in g and k, the higher marginal congestion cost in h counteracts the effect of higher mean productivity on the number of vessels who choose h. For this reason, it is not necessarily the case that the closures policy increases fishing near closures ($I_h^* > I_g^*$).

To see how higher marginal congestion costs in h reduce the number of vessels who choose h, consider the case when $\tilde{\mu_h} = \tilde{\mu_g}$ and suppose the contradiction that $I_h^* \geq I_g^*$. Expected profits are lower in h than in g because marginal congestion costs are higher in h ($\mathrm{E}[i, h^*] < \mathrm{E}[i, g^*]\ \ \forall i$). Since $\tilde{\mu_k}$ is the same in both states of the world and $I^*_{k|C=0} \geq I^*_{k|C=1}$ (because $I_g^* \leq I_h^*$), expected profit in k when $C = 0$ is less than or equal to expected profit in k when $C = 1$ ($\mathrm{E}[i, k^*|C = 0] \leq \mathrm{E}\ [i, k^*|C = 1]\ \ \forall i$). Since (I_g^*, I_k^*) is a Bayes-Nash equilibrium, $\mathrm{E}[i, g^*] = \mathrm{E}[i, k^*|C = 0]$ by the proof of Proposition 2a. Then $\mathrm{E}[i, h^*] < \mathrm{E}[i, k|C = 1]\ \ \forall i$ because $\mathrm{E}[i, h^*] < \mathrm{E}[i, g^*] = \mathrm{E}[i, k|C = 0] \leq \mathrm{E}[i, k|C = 1]$. Then (I_h^*, I_k^*) cannot be a Bayes-Nash equilibrium by the proof of Proposition 2a. Contradiction. Therefore, the higher marginal congestion costs in h reduce the number of vessels who fish in location h when

$\tilde{\mu_h} = \tilde{\mu_g}$ ($I_h^* < I_g^*$).

Together, the fact that vessels believe mean productivity in h is higher but know that marginal congestion costs are also higher in h means that the effect of the closures policy on fishing location choice is ambiguous. The closure announcement must be a sufficiently large positive signal relative to congestion costs in order to increase the number of vessels who choose to fish near closures (location h). In this case, there is a second condition necessary for the closures policy to increase total juvenile catch: productivity and percentage juvenile must be sufficiently high in location h relative to locations g and k.

The treatment effect τ of the closures policy on total juvenile catch is

$$\begin{aligned}\tau &= TotJuv^*(C=1) - TotJuv^*(C=0)\\ &= \gamma(I_h^*\mu_h\rho_h + I_{k|C=1}^*\mu_k\rho_k - (I_g^*\mu_g\rho_g + I_{k|C=0}^*\mu_k\rho_k))\\ &= \gamma(I_h^*\mu_h\rho_h - I_g^*\mu_g\rho_g + \mu_k\rho_k(I_{k|C=1}^* - I_{k|C=0}^*))\end{aligned} \tag{8}$$

If $I_h^* > I_g^*$, the third term in the expression, $\mu_k\rho_k(I_{k|C=1}^* - I_{k|C=0}^*)$, is negative because $I_{k|C=1}^* < I_{k|C=0}^*$. In order for the closures policy to increase total juvenile catch ($\tau > 0$), the number of vessels fishing, productivity, and percentage juvenile in location h must be sufficiently high relative to location g when there is no closure ($I_h^*\mu_h\rho_h >> I_g^*\mu_g\rho_g$), as well as relative to productivity and percentage juvenile in location k.

Parametric example of Propositions 1 and 2a. Figure 3 displays the Bayes-Nash equilibria when $\mathrm{E}[\pi_{i,\ell}(\mu_\ell, I_{-i,\ell})|\vec{\tilde{\mu}}, C] = \tilde{\mu_\ell} - \alpha_\ell I_{-i,\ell}$, where α_ℓ is the cost to vessel i from one additional vessel fishing in location ℓ. The equilibrium when $C = 0$ results from setting $\tilde{\mu_g} - \alpha_g I_{-i,g} = \tilde{\mu_k} - \alpha_k I_{-i,k}$ and the equilibrium when $C = 1$ is similarly defined. Recall that marginal congestion costs are only different for h; $\alpha_g = \alpha_k$ and let α represent this value. The equilibrium when $C = 0$, (I_g^*, I_k^*), is $(\frac{\tilde{\mu_g}-\tilde{\mu_k}}{2\alpha} + \frac{1}{2}I,\ \frac{\tilde{\mu_k}-\tilde{\mu_g}}{2\alpha} + \frac{1}{2}I)$. The equilibrium when $C = 1$, (I_h^*, I_k^*), is $(\frac{\tilde{\mu_h}-\tilde{\mu_k}}{\alpha_h+\alpha} + \frac{\alpha}{\alpha_h+\alpha}I,\ \frac{\tilde{\mu_k}-\tilde{\mu_h}}{\alpha_h+\alpha} + \frac{\alpha_h}{\alpha_h+\alpha}I)$. Substituting these values into Equation 8 gives the change in total juvenile catch due to the policy.

Proof of Proposition 3. Since $\tilde{\mu_{g,a}} > \tilde{\mu_{g,-a}}$ and congestion costs are the same in locations g and k, the proof of Proposition 1 implies that type $-a$ vessels will only choose g after all type a vessels have chosen g ($\frac{I_{g,-a}^*}{I_{-a}} > 0$ only when $\frac{I_{g,a}^*}{I_a} = 1$). Since the Bayes-Nash equilibria are interior by assumption, $\frac{I_{g,-a}^*}{I_{-a}} < 1$ (if $\frac{I_{g,-a}^*}{I_{-a}} = 1$, then $I_k^* = 0$). Then a greater percentage of type a vessels choose g than type $-a$ vessels: $\frac{I_{g,a}^*}{I_a} > \frac{I_{g,-a}^*}{I_{-a}}$. Conversely, a lower percentage of type a vessels choose k than type $-a$ vessels: $\frac{I_{k,a}^*}{I_a} < \frac{I_{k,-a}^*}{I_{-a}}$.

Since type a and type $-a$ vessels are identical when $C = 1$ ($\tilde{\mu_{h,a}} = \tilde{\mu_{h,-a}}$), the same percentage of each type choose locations h and k ($\frac{I_{h,a}^*}{I_a} = \frac{I_{h,-a}^*}{I_{-a}}$ and $\frac{I_{k,a}^*}{I_a} = \frac{I_{k,-a}^*}{I_{-a}}$). Since both types of vessels catch the same number of juveniles when they fish in the same location,

$\frac{TotJuv(C=1)^*_{-a}}{I_{-a}} = \frac{TotJuv(C=1)^*_{a}}{I_{a}}$. Then the percentage difference in treatment effects between the two types of vessels can be written as

$$
\begin{aligned}
\frac{\tau_{-a}}{I_{-a}} - \frac{\tau_a}{I_a} &= \frac{TotJuv(C=1)^*_{-a} - TotJuv(C=0)^*_{-a}}{I_{-a}} - \frac{TotJuv(C=1)^*_{a} - TotJuv(C=0)^*_{a}}{I_a} \\
&= \frac{TotJuv(C=0)^*_{a}}{I_a} - \frac{TotJuv(C=0)^*_{-a}}{I_{-a}} \\
&= \gamma(\frac{I^*_{g,a}\mu_g\rho_g + I^*_{k,a}\mu_k\rho_k}{I_a} - \frac{I^*_{g,-a}\mu_g\rho_g + I^*_{k,-a}\mu_k\rho_k}{I_{-a}}) \\
&= \gamma(\mu_g\rho_g(\frac{I^*_{g,a}}{I_a} - \frac{I^*_{g,-a}}{I_{-a}}) + \mu_k\rho_k(\frac{I^*_{k,a}}{I_a} - \frac{I^*_{k,-a}}{I_{-a}})) \\
&= \gamma(\mu_g\rho_g(\frac{I^*_{g,a}}{I_a} - \frac{I^*_{g,-a}}{I_{-a}}) + \mu_k\rho_k(\frac{I_a - I^*_{g,a}}{I_a} - \frac{I_{-a} - I^*_{g,-a}}{I_{-a}})) \\
&= \gamma((\mu_g\rho_g - \mu_k\rho_k)(\frac{I^*_{g,a}}{I_a} - \frac{I^*_{g,-a}}{I_{-a}})) \\
&> 0
\end{aligned}
$$

because $\mu_g\rho_g > \mu_k\rho_k$ and $\frac{I^*_{g,a}}{I_a} > \frac{I^*_{g,-a}}{I_{-a}}$.