

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

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**WORLD BANK GROUP**

Development Economics

Development Research Group

December 2022

## Abstract

This paper establishes that a targeted policy backfires because it reveals information about non-targeted units. In the world's largest fishery, the regulator attempts to reduce the harvesting of juvenile fish by temporarily closing areas where the share of juvenile catch is high. By combining administrative microdata with biologically richer data from

fishing firms, the analysis isolates variation in closures that is due to the regulator's lower resolution data. Closures cause substantial temporal and spatial spillovers. Closures increase total juvenile catch by 48 percent because closure announcements implicitly signal that there is high productivity fishing before, just outside, and after closures.

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

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**JEL codes:** Q28, O13, Q56, D83, Q22

**Keywords:** information spillovers, targeted policies, place-based policies, fisheries, Peru

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\*Development Research Group, The World Bank; [aenglander@worldbank.org](mailto:aenglander@worldbank.org). 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 coeditor Matthew Notowidigdo, three anonymous referees, 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, Gianmarco León-Ciliotta, 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 UC 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.

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, Gaines and Lynham, 2008; Isaksen and 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 (Costello and Deacon, 2007; Smith, 2012).

I study the effects of a policy designed to reduce the most important biological externality in the world's largest fishery: the capture of juvenile anchoveta in Peru (Salvatteci and Mendo, 2005; Paredes, 2014). 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 48% on net, because the policy implicitly reveals that nearby areas and time periods are high-productivity fishing grounds.

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 (Food and Agriculture Organization (FAO), 2018; Ministerio de la Producción (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 higher remaining reproductive capacity over their lifetimes (Salvatteci and Mendo, 2005). Fishers



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; Sindicato Univo de Pescadores de Nuevas Embarcaciones del Peru (SUPNEP), 2017; Hansman et al., 2020). A tax could achieve the optimal level of juvenile catch, but the fishing industry opposes such a policy (Grainger and Parker, 2013; Instituto Humboldt de Investigación Marina y Acuícola (Instituto Humboldt), Sociedad Nacional de Pesquería (SNP), Centro Desarrollo y Pesca Sustentable (CeDePesca), 2018). Instead, the regulator attempts to reduce juvenile catch by implementing temporary spatial closures in areas where it believes juveniles are abundant.

I analyze the regulator’s temporary spatial closures policy in this paper, accounting for other regulations that also affect fishing. 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 vessel-level fishing operations. Vessels report the percentage juvenile they catch to the regulator in real-time.<sup>1</sup> When percentage juvenile values in an area are high, the regulator temporarily bans fishing in that area for three to five days. Vessels 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.

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 and takes as its input the same data the

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<sup>1</sup>In my regressions, I correct for misreporting to the regulator by matching vessel-reported data to percentage juvenile measured by third-party inspectors. Third-party inspector data is not used by the regulator to determine closures.

regulator uses to determine closures. 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 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 vessels. I obtained biologically richer data from fishing firms which contain the distributions that percentage juvenile values are drawn from. These data are 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 have sampling error. This identification strategy can be applied in other settings where the regulator targets policy based on noisier data than are available to the econometrician (Chodorow-Reich, Coglianese and Karabarbounis, 2019).

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 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 48% 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.

Why do closures change where vessels fish? Due to contracts and other regulations in the fishery, reducing search costs is the primary margin by which fishers can increase profits. Closures might help fishers 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,<sup>2</sup> but strong correlations in anchoveta catch over time and space.<sup>3</sup>

For these reasons I posit that closures implicitly provide information about the value of fishing near closures (before, just outside, and after closures). I confirm that one way in which vessels respond to closures is by fishing closer to them. Then I provide evidence that absent vessels' responses to the information, fishing near closures is more productive than fishing elsewhere.

If information is a mechanism underlying the policy's spillover effects, then vessels that receive stronger information shocks from closure announcements should have larger treat-

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<sup>2</sup>I observe the universe of fishing events. Zero anchoveta catch includes instances in which there is no fishing in an area as well as instances in which vessels set their nets but catch zero tons.

<sup>3</sup>For example, the daily probability any vessel catches anchoveta in a given  $.1^\circ$  grid cell ( $\sim 11$  by  $11$  km) during the fishing season is 0.5%. But conditional on at least one vessel catching anchoveta in a  $.1^\circ$  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  $.1^\circ$  grid cell today, the probability of positive anchoveta catch in at least one adjacent grid cell on the same day is 92% (spatial correlation).

ment effects. I test this prediction by dividing vessels into those that did or 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 87% for vessels that did not fish inside a given potential closure the day before closure announcement. But 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. But 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 and Shimshack, 2011; Greenstone and 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 (in addition to the direct effect), such as spatial spillovers from a hot-spot policing intervention in Colombia (Blattman et al., 2021), temporal spillover from the announcement of a new marine protected area (McDermott et al., 2019), and spatial spillovers from blacklisting high-deforestation municipalities in Brazil (Assunção et al., 2019). However, estimating the total effect of a targeted policy (direct, temporal spillover, and spatial spillover effects) is rare.<sup>4</sup> Additionally, while previous papers have estimated spillovers

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<sup>4</sup>Estimating direct, temporal spillover, and spatial spillover effects requires substantial treatment variation. Two other papers that estimate these effects are Ladino, Saavedra and Wiesner (2019), which studies

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 (Davis, 2008).<sup>5</sup> 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 and Robalino, 2017).

The information mechanism I uncover in this paper is most similar to the concept of “information spillovers” in financial economics. In Asriyan, Fuchs and Green (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.<sup>6</sup> 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 can occur whenever a targeted policy conveys information about non-targeted units. For example, if hot-spot policing causes potential criminals to believe enforcement is lower in non-targeted areas, then hot-spot policing could increase crime in non-targeted areas (Banerjee et al., 2019; Blattman et al., 2021); if rationing the consumption of some goods causes consumers to believe shortages of non-rationed goods are more likely, then the policy could increase stockpiling (Erdem, Imai and Keane, 2003; Keane and Neal, 2021); and if targeting infectious disease tests or vaccines to priority groups causes non-targeted people to lower their subjective probability of infection, then the policy could increase social activity and disease transmission among non-targeted people (Acemoglu et al., 2020). This paper provides the first empirical evidence that information spillovers can cause a targeted policy to backfire.

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Colombia’s illegal crop substitution program, and Gibson and Carnovale (2015), which evaluates driver responses to road pricing.

<sup>5</sup>For example, in the energy efficiency literature, there is no empirical evidence for the Jevons Paradox hypothesis that increases in energy efficiency increase total energy use (Gillingham, Rapson and Wagner, 2016).

<sup>6</sup>Information spillovers also occur in natural resource settings, such as the effect of oil discovery by one firm on the incentives for oil exploration by other firms (Hodgson, 2021).

# I Institutional context

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. 97% of anchoveta tons are processed into fishmeal and fish oil, which are primarily used for aquaculture and livestock feed (PRODUCE, 2018*a*).

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). There are two fishing seasons per year, each of which lasts about three months. Individual vessel quotas (IVQs) entitle each vessel to land a fixed share of the TAC.<sup>7</sup> IVQs preclude entry of new vessels into the fishery.<sup>8</sup> Property rights-based instruments like IVQs increase fisher profits, increase biomass (size of stock in tons), and reduce the probability of fisheries “collapse” (Costello, Gaines and Lynham, 2008; Tveteras, Paredes and Peña-Torres, 2011; Costello et al., 2016; Natividad, 2016; Isaksen and Richter, 2019; Kroetz et al., 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 address the differential externality from catching juvenile fish (Smith, 2012; Quaas et al., 2013).

The regulator’s goal in implementing temporary spatial closures is to reduce the capture of juvenile anchoveta. 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

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<sup>7</sup>For example, the IVQ for the vessel with the unique identifier CE-4122-PM is 0.22858% of each season’s TAC. In the first season of 2017, when the TAC was 2.8 million tons, this vessel was entitled to land ~6,400 tons.

<sup>8</sup>IVQs are only transferrable within-firm. To transfer an IVQ across firms, the vessel itself must be sold (Natividad, 2016).

a stock to spawn at least once in their life (Wallace and Fletcher, 1997; Paredes, 2014).

The regulator began implementing temporary spatial closures, called *Suspensiones Preventivas*, in 2014. The regulator can temporarily close an area of ocean when the percentage of individuals caught in that area that are juvenile exceeds 10% (PRODUCE, 2012). The regulator exercises discretion; many instances of percentage juvenile greater than 10% do not lead to closures.<sup>9</sup> The regulator defines juvenile anchoveta as anchoveta smaller than 12 cm. Percentage juvenile therefore refers to the percentage of individual anchoveta that are less than 12 cm long.

Between 2014 and 2016, the regulator determined closures with data measured at the point of landing. I analyze the effects of the policy between 2017 and 2019, when the use of “electronic logbooks” enhanced the regulator’s ability to target high-juvenile areas. Electronic logbooks refer to software vessels use to record (“log”) their catch at sea. Beginning in 2017, the regulator required vessels to report to the regulator the location, estimated tons caught, and estimated percentage juvenile caught immediately after each fishing operation, called a “set”. 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 than landings data.

During my study period there are no penalties for catching juveniles or for reporting inaccurate percentage juvenile values (PRODUCE, 2016*a*). The price fishers receive for their catch is exogenous and does not depend on percentage juvenile (Section V).

The regulator determines closures as follows.<sup>10</sup> An official monitors the electronic logbook data in real-time, which appear as points on a digital map. Each point is a set and the color of each point is the set’s percentage juvenile. When the official decides that the percentage juvenile values for a group of sets are too high, he selects that group of sets by drawing

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<sup>9</sup>If the regulator applied the 10% threshold strictly, 69% of potential closures would have been closed, compared to the 19% that were in fact closed. Since potential closures represent fishing grounds, the regulator may use discretion to reduce the cost of the policy to the fishing industry.

<sup>10</sup>The government officials tasked with determining closures demonstrated their process to me in a December 2019 interview in their Lima office.

a rectangle around them with his mouse. Based on the points selected by the official, a computer algorithm calculates the boundary and number of days for the resulting closure. The computer algorithm is not publicly available, but government officials have described it in workshops and personal conversations. Closure area increases in the total tons caught by the group of sets (Instituto Humboldt and SNP, 2017; Instituto Humboldt, SNP and CeDePesca, 2018). Closures last three, four, or five days.<sup>11</sup> Higher percentage juvenile values result in longer closures. The official can make manual adjustments to the closure generated by the computer algorithm, for example to ensure the closure covers all the sets selected by the official (Instituto Humboldt and SNP, 2017; Instituto Humboldt, SNP and CeDePesca, 2018).

Vessels are not allowed to fish inside active closures (inside closed areas during the three-to-five-day closure periods). To the extent that vessels conceal fishing inside active closures, this detection avoidance would accentuate my main result that the policy backfires.<sup>12</sup>

The regulator announces closures on their website and sends emails and WhatsApp messages to firms (PRODUCE, 2020*b*). Firms communicate closures to vessels at sea using radio, and vessels enter the coordinates of closed areas into their onboard 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

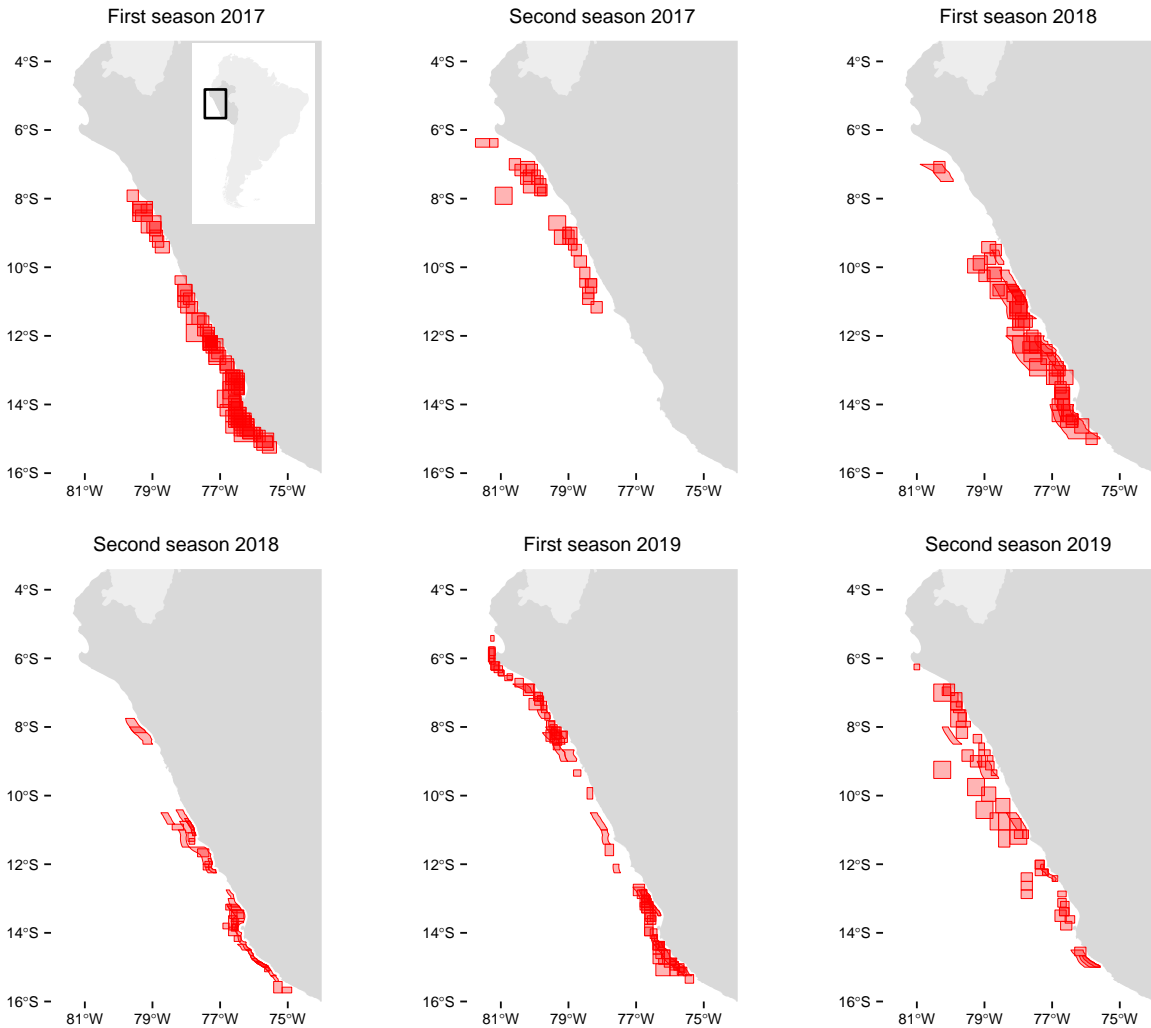
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<sup>11</sup>I estimate the effects of closures declared by the regulator, PRODUCE. The scientific agency, Instituto del Mar del Perú (IMARPE), can declare closures of up to 10 days. These closures can apply to all fishing grounds (i.e., Peru’s entire Exclusive Economic Zone).

<sup>12</sup>The regulator uses real-time vessel movement data to monitor fishing. 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, 2016*a*). This movement indicates fishing because all fishing in the Peruvian anchoveta fishery is “purse seine fishing”, which involves dragging a net in a circle around a group of anchoveta. Vessels’ GPS 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. If the vessel’s transponder does not transmit data to the regulator for more than two hours, for any reason, the regulator penalizes the vessel owner (e.g., PRODUCE, 2017*b*).



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<sup>2</sup>. 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).

1). The smallest and largest closures are 170 and 12,512 km<sup>2</sup>. The mean and median closures are 1,328 and 1,211 km<sup>2</sup>. 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 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 lower economic cost (Dunn et al., 2016; Hazen et al., 2018; Pons et al., 2022).

The same win-win idea motivates temporary spatial closures. By allowing fishing to continue in most places, temporary spatial closures could reduce juvenile catch while minimizing the cost of the policy to fishers. If temporary spatial closures cause fishers to search for new fishing grounds, they could reduce juvenile catch because there is substantial variation in relative juvenile abundance across space. This variation occurs because schools of fish tend to be age-segregated (i.e., each school of anchoveta contains mostly juveniles or mostly adults). The regulator believes the closures policy reduces total juvenile catch. They calculated that 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, 2017*a*, 2018*c,b*).<sup>13</sup>

## II 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

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<sup>13</sup>The regulator does not describe how they calculate this number, nor do they define the meaning of “protected” in this context.

answerable research questions (Sala et al., 2018; Englander, 2019), but they do not measure the most important outcomes caused by fishing: the quantities and types of fish that vessels catch. I obtained from the regulator two administrative datasets that contain these variables. 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* (Englander, 2022). Vessels 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. Vessels record this information on a smartphone or tablet application, which transmits data in real-time to the regulator through the vessel’s onboard GPS transponder. Vessels 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. While transferring anchoveta from the net into the hold, a trained fisher 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, 2016*b*). The fisher 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, fishers measure approximately 200. The percentage juvenile for the set is the percentage of measured individuals that are shorter than 12 cm. Vessels report percentage juvenile to the regulator, but they do not report the length distribution.

The second dataset is *landings data* (Englander, 2022). 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 these 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 2.2), whereas fishers measure percentage juvenile after each set in the electronic logbook data. However, unlike vessels 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 fishers (PRODUCE, 2018*d*).

Even if percentage juvenile reported by vessels in the electronic logbook data is unbiased, I cannot calculate juvenile catch as percentage juvenile times tons caught because the weight of an anchoveta is approximately cubic in its length (IMARPE, 2019). I therefore obtained a supplementary electronic logbook dataset for a group of vessels that report length distribution data to their owners (but not to the regulator) (Englander, 2022). These vessels represent 56% of tons landed. For each two-week-of-sample by two-degree grid cell, I calculate the individuals-weighted average proportion of individuals in each half-cm interval caught by these vessels. Then I impute length distributions for sets by vessels that do not report these data to their owners. 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.

Next, I match all sets to landing events in order to correct the percentage juvenile, length distribution, and number of individuals caught by all sets. 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.

Finally, I shift the length distribution of each set up or down in half-cm increments until I minimize the absolute difference between the implied percentage juvenile (updated percentage of individuals that are less than 12 cm) and the corrected percentage juvenile. 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. In Appendix C I describe the construction of these data in detail and demonstrate robustness to the assumptions I make in doing so.

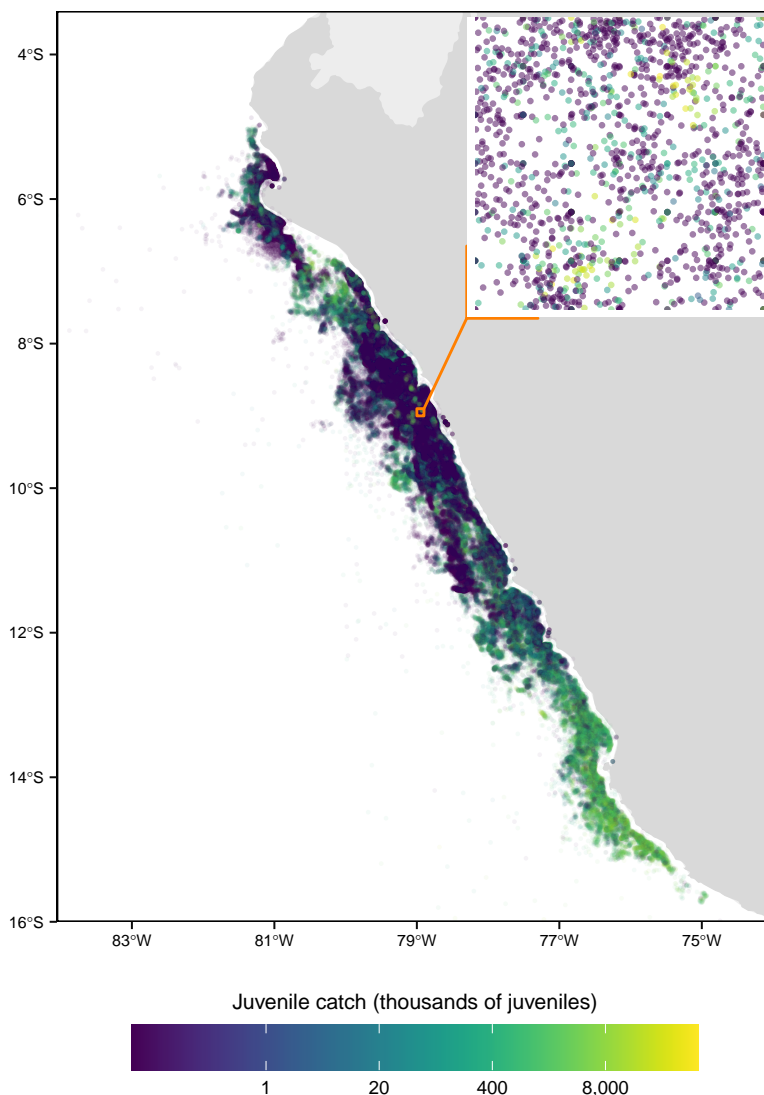
Figure C1 displays the average uncorrected length distribution, the average corrected length distribution, the reported percentage juvenile values, and the corrected percentage juvenile values. If I did not have landings data, I would mismeasure juvenile catch because vessels seem to underreport percentage juvenile in the electronic logbook data. The individuals-weighted average percentage juvenile is 40% lower in the electronic logbook data than in the landings data (11% compared to 18.3%). Vessels might underreport percentage juvenile to avoid triggering a closure in the area they are fishing.<sup>14</sup> This phenomenon 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 2). During a fishing season there are 572 sets per day on average. The average set catches 570,103 juvenile anchoveta and 2,508,788 adult anchoveta, which together weigh 50.2 tons. Vessels do not underestimate tons caught in the electronic logbook data, perhaps because this variable has little effect on whether the

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<sup>14</sup>Closures provide valuable information to vessels regarding the location of anchoveta, but only to vessels who did not recently fish in that area (Section VI).

Figure 2: 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. The regulator prohibits 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.

regulator declares a closure (Section I).<sup>15</sup> The median anchoveta caught is between 13 and

<sup>15</sup>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, Misganaw and Bazezew, 2015). Vessels have little incentive to bias their estimates of tons caught in the electronic logbook data. Tons measured at landing, rather than tons estimated by vessels in the electronic logbook data, are the data that processing plants use to pay fishers and that the regulator uses to determine

13.5 cm long (Figure C1b).

### III Empirical strategy

In this section, I describe how I estimate the total causal effect of the temporary spatial closures policy on juvenile catch. Potential closures are the unit of observation. I estimate the effect of the policy across potential closures, comparing juvenile catch inside and near treated potential closures—those that the regulator declare as actual closures—to juvenile catch inside and near control potential closures—those that the regulator does not close.

I detail the construction of potential closures in Section III.A. In Section III.B I discuss identifying variation and define the variables in my estimating equation. I describe how I estimate spillover effects in Section III.C. I present my estimating equation in Section III.D. In Section III.E I demonstrate balance of treated and control potential closures on pre-period juvenile catch and observable measures of fishing productivity.

#### A Potential closures

As detailed in Section I, the regulator uses real-time electronic logbook data to determine closures. When a government official wants to create a closure, they draw a rectangle around 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. I develop an algorithm that mimics the first stage of the closure rule, when the official selects a group of sets 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,

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when a vessel has reached its quota for the season.

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.

Specifically, 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 second-stage algorithm rounds the boundaries of rectangular closures to the nearest 5 nautical mile interval (Instituto Humboldt and 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.

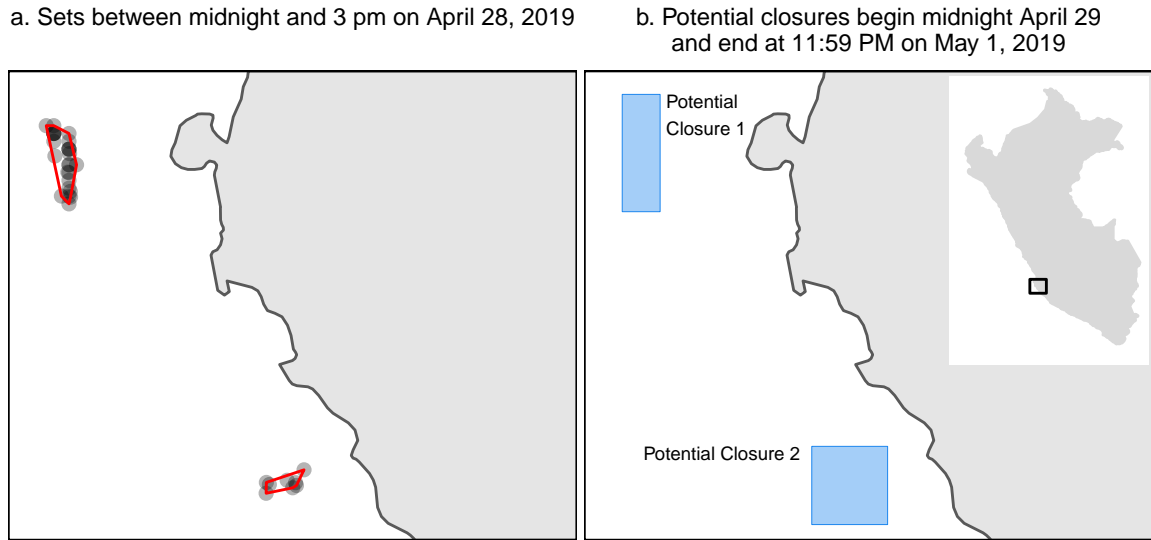
As an illustrative example, Figure 3 displays sets from the electronic logbook data in one region of the fishery between midnight and 3 pm on April 28, 2019. The single-linkage clustering algorithm creates two clusters from these sets (Figure 3a). Figure 3b displays the potential closures that result 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 973 potential closures in total, compared to 410 actual closures declared by the regulator during my study period. 88% 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 (950 km<sup>2</sup> compared to 1,328 km<sup>2</sup>).



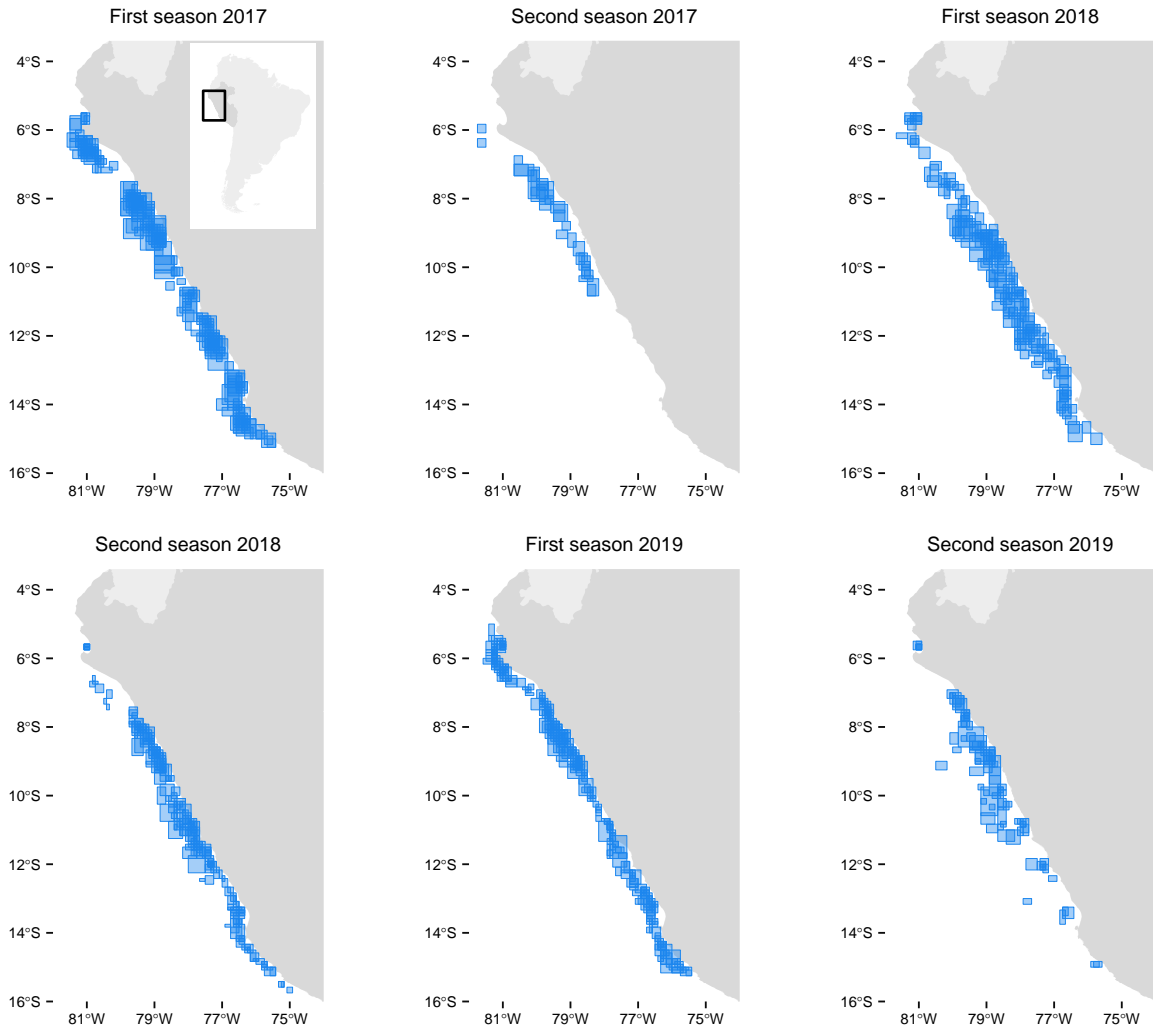
Figure 3: Creation of potential closures example



Notes: (a) Points are sets that occur between midnight and 3 pm on April 28, 2019 in one region of the North-Central zone. 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. The regulator also rounds rectangular closures 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 (grey) and the region these potential closures occur in (black rectangle).

Figure 4 displays the potential closures in each fishing season. My results 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 A.2). My results are also robust to estimating the effect of the policy via synthetic controls, which matches actual closures (treatment units) to control potential closures (those that the regulator did not close) (Appendix A.4).

Figure 4: 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 950 km<sup>2</sup>. My potential closures algorithm generates 973 potential closures in the North-Central zone during the six fishing seasons of 2017, 2018, and 2019, compared to 410 actual closures declared by the regulator (Figure 1). There are 1.73 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).

## B Outcome, treatment, control variables, and identifying variation

The main outcome of interest is juvenile catch inside potential closures. In Figure 3, juvenile catch is the number of juvenile anchoveta 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 decides 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 0.4 (60% spatial overlap  $\cdot$  two-thirds temporal overlap = 0.4). 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 0.19.

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 the length distribution is akin to controlling for the probability distribution function from which percentage juvenile values for those sets are drawn because percentage juvenile is a statistic of the length distribution (fraction of individuals smaller than 12 cm). 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 vessels do not report length distribution

to the regulator. 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 II, wherein fishers 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 values 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.

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 individuals caught by each set. Recall that these sets occur before potential closures begin. For example, I calculate the weighted-average proportion of anchoveta in each length interval for potential closures in Figure 3b from sets that occur between midnight and 3 pm on April 28, 2019. 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.<sup>16</sup> 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.

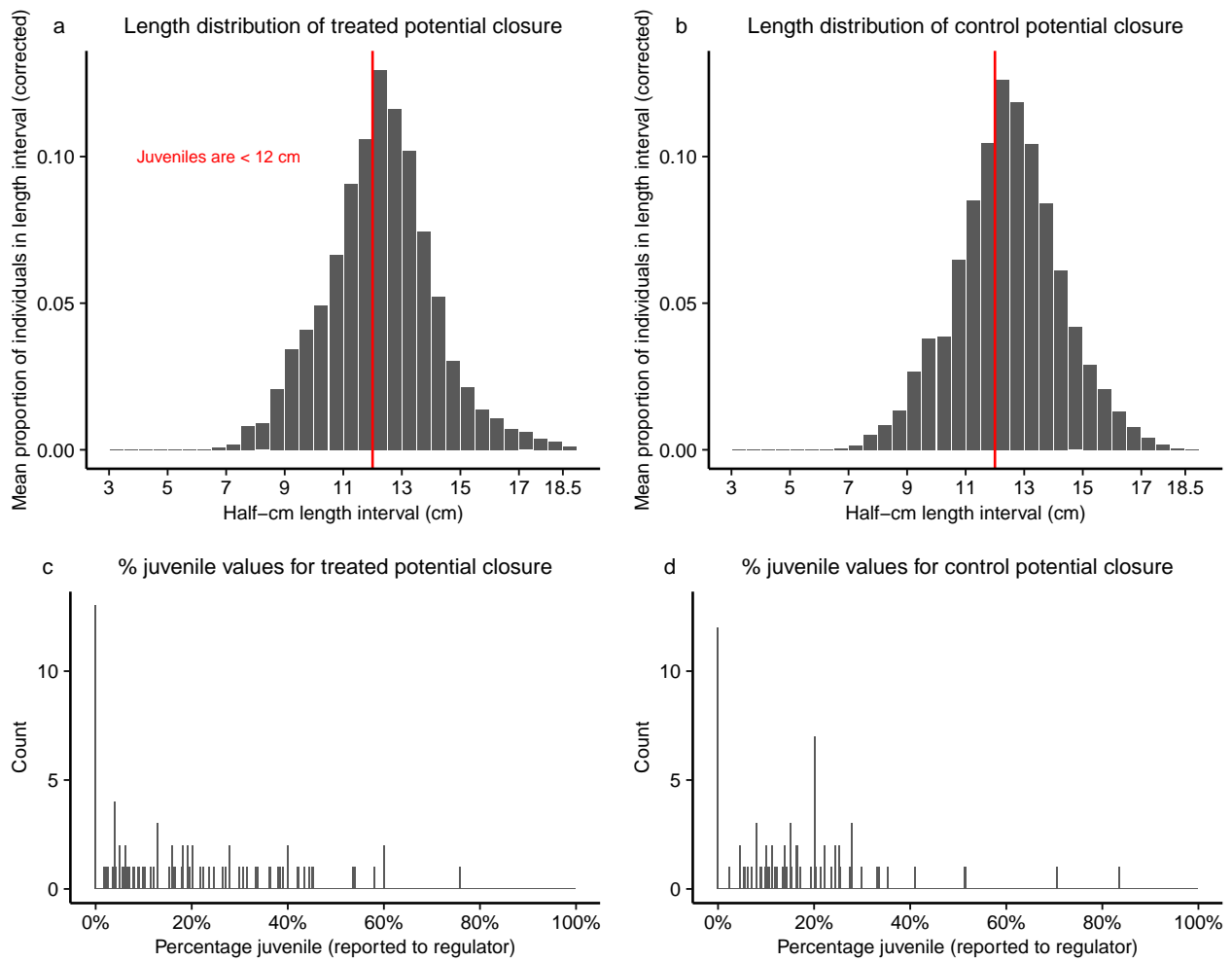
Figure 5 displays the length distributions and the reported percentage juvenile values from these distributions for two potential closures: a “treated” potential closure that was

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<sup>16</sup>I also use the corrected number of individuals caught by each set in calculating the average length distribution for each potential closure.

declared an actual closure by the regulator (treatment fraction = 1), and a “control” potential closure that was not (treatment fraction = 0). Both potential closures have similar length distributions, but there are more percentage juvenile values above 25% reported to the regulator for the sets that generate the treated potential closure compared to the sets that generate the control potential closure. This difference in the number of very high reported percentage values is likely why the treated potential closure gets closed and the control potential closure does not.

Figure 5: Length distribution and percentage juvenile values example



Notes: The regulator closes the treated potential closure (left), but does not close the control potential closure (right). The regulator observes the reported percentage juvenile values (c and d), but does not observe the length distributions (a and b).

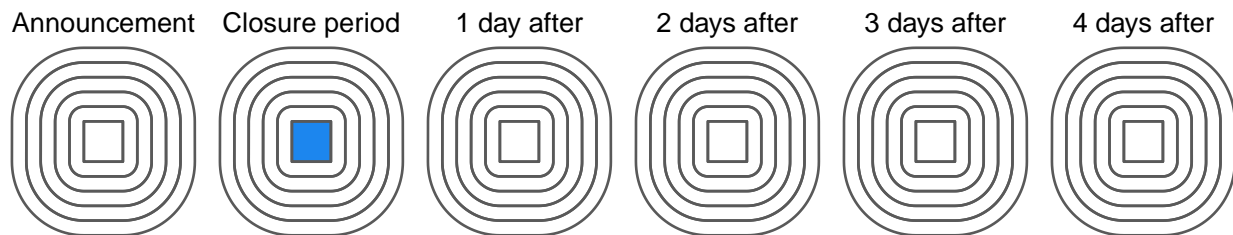
Controlling for the length distribution adjusts for differences in the size-structure of anchoveta populations across potential closures, but not for differences in fishing productivity or fishing costs across potential closures. If treated potential closures are more desirable fishing locations, either 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  $\text{km}^2$ , 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  $\text{km}^2$  among the sets that generate each potential closure. The number of sets, tons caught, and potential closure area are measures of anchoveta abundance, distance to the coast is a proxy for fishing costs, and tons per set and tons per  $\text{km}^2$  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 3.8 potential closures per two-week-of-sample by two-degree grid cell. The day-of-sample fixed effects control for the following possible confounders: the number of actual closures that are active that day and the area they cover; aggregate juvenile catch and fishing productivity that day; and the international price of fishmeal, which determines the price fishers receive for their catch (Section V).

## **C Spatial and temporal spillover bins**

The temporary spatial closures policy may reduce juvenile catch inside closed areas during the active closure period (direct effect). But it could also cause spillovers over space or time:

Figure 6: Treatment bins and treatment window over which the closures policy can affect juvenile catch



Notes: The original potential closure is the closure period, inside the potential closure treatment bin (blue). The other 35 treatment bins in the treatment window are white.

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, just 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 strong positive signal of fishing productivity, they could also increase the total quantity of fishing that occurs near closures (Appendix B).

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 closures policy dissipate over both space and time (Section IV). There are six time periods of interest for each potential closure: nine hours before the potential closure begins (“Announcement” in Figure 6), the three-day period in which the potential closure is active (“Closure period”), and one, two, three, and four 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 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” (6 time periods · 6 spatial units). A treatment bin is a time period-spatial unit pair. A potential closure-treatment bin is a potential closure-time period-spatial unit triple. Since there are 973 potential closures, there are 35,028 potential closure-treatment bin observations. Figure 6 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 spatial 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 III.B). 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. 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.<sup>17</sup> 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.<sup>18</sup>

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.

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<sup>17</sup>Some sets occur inside multiple potential closure-treatment bins. I correct for this “double-counting” when estimating the effect of the closures policy on juvenile catch (detailed in Footnote 22).

<sup>18</sup>However, the fixed effects are specific to each potential closure-treatment bin.



## D Estimating equation

I estimate the effect of the temporary spatial closures policy on juvenile catch with the following ordinary least squares regression:

$$\begin{aligned}
 (1) \quad \text{JuvenileCatch}_{ist} &= \alpha_{st} + \beta_{st} \text{TreatFraction}_{ist} + \sum_{\ell=[3,3.5]}^{[18.5,19]} \xi_{\ell} \text{Prop}_{i\ell} \\
 &+ \gamma_1 \text{Sets}_i + \gamma_2 \text{Tons}_i + \gamma_3 \text{Area}_i + \gamma_4 \text{DistToCoast}_i \\
 &+ \gamma_5 \text{TonsPerSet}_i + \gamma_6 \text{TonsPerArea}_i + \sigma_{wg} + \delta_d + \epsilon_{ist}
 \end{aligned}$$

where  $i$  = potential closure,  $s$  = spatial unit,  $t$  = time period,  $\ell$  = half-cm length interval,  $w$  = two-week-of-sample,  $g$  = two-degree grid cell, and  $d$  = day-of-sample. I defined potential closures in Section III.A, all variables in Section III.B, and treatment bins (spatial unit-time period pairs) in Section III.C.

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 and Wichman, 2020).<sup>19</sup> The  $\text{Prop}_{i\ell}$  terms are the proportion of anchoveta individuals in each half-cm length interval  $\ell$  that are caught by the sets that generate potential closure  $i$ . Recall from Section III.B that these sets, from which  $\text{Sets}_i$ ,  $\text{Tons}_i$ ,  $\text{Area}_i$ ,  $\text{DistToCoast}_i$ ,  $\text{TonsPerSet}_i$ , and  $\text{TonsPerArea}_i$  are also defined, occur before the treatment window for potential closure  $i$  begins.

The coefficients of interest are  $\beta_{st}$ , which measure the effects of the closures 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 controls. For example, comparing 10 km-wide rings around potential closures that begin on the same day within the

<sup>19</sup>65% of potential closure-treatment bins have zero juvenile catch inside them. 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 A3). They also hold if I replace the dependent variable in Equation 1 with a binary indicator for positive juvenile catch (Figure A4).

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.

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 21 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 259 clusters and 34,272 observations when I estimate Equation 1 (952 potential closures  $\cdot$  36 treatment bins).

## E Balance tests

I assess the identifying assumption that treatment and control potential closures are comparable conditional on control variables and fixed effects. I test for balance on juvenile catch levels in Table 1 and on observable measures of fishing productivity in Table 2.

First, I calculate juvenile catch and treatment fraction for inside potential closures on the day before sets would generate an actual closure.<sup>20</sup> 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

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<sup>20</sup>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 affects 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 A5).

Table 1: Test for difference in pre-period juvenile catch

| Dependent variable: asinh(juvenile catch) |                  |                  |                   |                   |                   |
|---|------------------|------------------|-------------------|-------------------|-------------------|
|   | (1)              | (2)              | (3)               | (4)               | (5)               |
| Treatment fraction                        | 0.271<br>(0.143) | 0.002<br>(0.167) | -0.055<br>(0.147) | -0.099<br>(0.145) | -0.029<br>(0.170) |
| Fixed effects                             |                  | X                |                   |                   | X                 |
| Length distribution                       |                  |                  | X                 | X                 | X                 |
| Other controls                            |                  |                  |                   | X                 | X                 |

Notes: All regressions have 35,224 observations. Dependent variable is the inverse hyperbolic sine of millions of juveniles caught. All regressions estimate treatment effects for all 37 treatment bins, but this table only displays the coefficient on treatment fraction for the inside, day-before bin. Standard errors clustered at level of two-week-of-sample by two-degree grid cell.

estimate versions of Equation 1 in Table 1. I estimate treatment effects for all treatment bins (now 37 instead of 36), but only report the coefficient on the inside, day-before 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 26% higher juvenile catch than potential closures that will not be declared actual closures by the regulator (treatment fraction = 0). In Columns 2 to 5 of Table 1, I test whether the control variables and fixed effects in Equation 1 eliminate this difference in pre-period juvenile catch.

I include day-of-sample and two-week-of-sample by two-degree grid cell fixed effects in Column 2, length distribution controls in Column 3 (excluding fixed effects and the six other control variables in Equation 1), all potential closure-level controls from Equation 1 in Column 4 (excluding fixed effects), and the full set of potential closure-level controls and fixed effects from Equation 1 in Column 5. In all four specifications, these control variables and fixed effects balance treatment and control potential closures on pre-period juvenile catch. 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. I find that treatment and control potential closures are also

Table 2: Test for balance on measures of fishing productivity

|                    | DistToCoast<br>(1) | TonsPerSet<br>(2) | TonsPerArea<br>(3) | FittedVals<br>(4) |
|--------------------|--------------------|-------------------|--------------------|-------------------|
| Treatment fraction | -0.362<br>(1.797)  | 5.559<br>(4.319)  | 3.238<br>(1.203)   | 0.046<br>(0.033)  |
| Mean dep. var.     | 30.748             | 54.506            | 6.804              | 0.791             |

All regressions have 34,272 observations and control for two-week-of-sample by two-degree-grid-cell fixed effects, day-of-sample fixed effects, and potential closure-level length distribution. Standard errors clustered at level of two-week-of-sample by two-degree grid cell.

balanced on pre-period juvenile catch trends in Figure A1.

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 ( $\text{km}^2$ ).<sup>21</sup> 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 control potential closures, all else equal, because there will be more fishing near treated potential closures. Indeed, in Table A1 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, though tons per area is no longer significantly correlated with juvenile catch. I record these fitted values and also use them to test for balance in Table 2.

In Table 2, 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 statistically 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 insignificant correlations between treatment fraction and tons per set and between treatment fraction and distance to the coast are more

<sup>21</sup>In addition to being a proxy for fishing costs, distance to the coast is a proxy for fishing productivity because anchoveta are more abundant closer to the coast (Castillo et al., 2019).

relevant than the correlation between treatment fraction and tons per area because tons per area is not a significant predictor of juvenile catch when I regress juvenile catch on all three variables (Column 4 of Table A1). The insignificant correlation 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 treatment and control potential closures would offer similar fishing opportunities to vessels if not for treatment.

## IV Do closures reduce total juvenile catch?

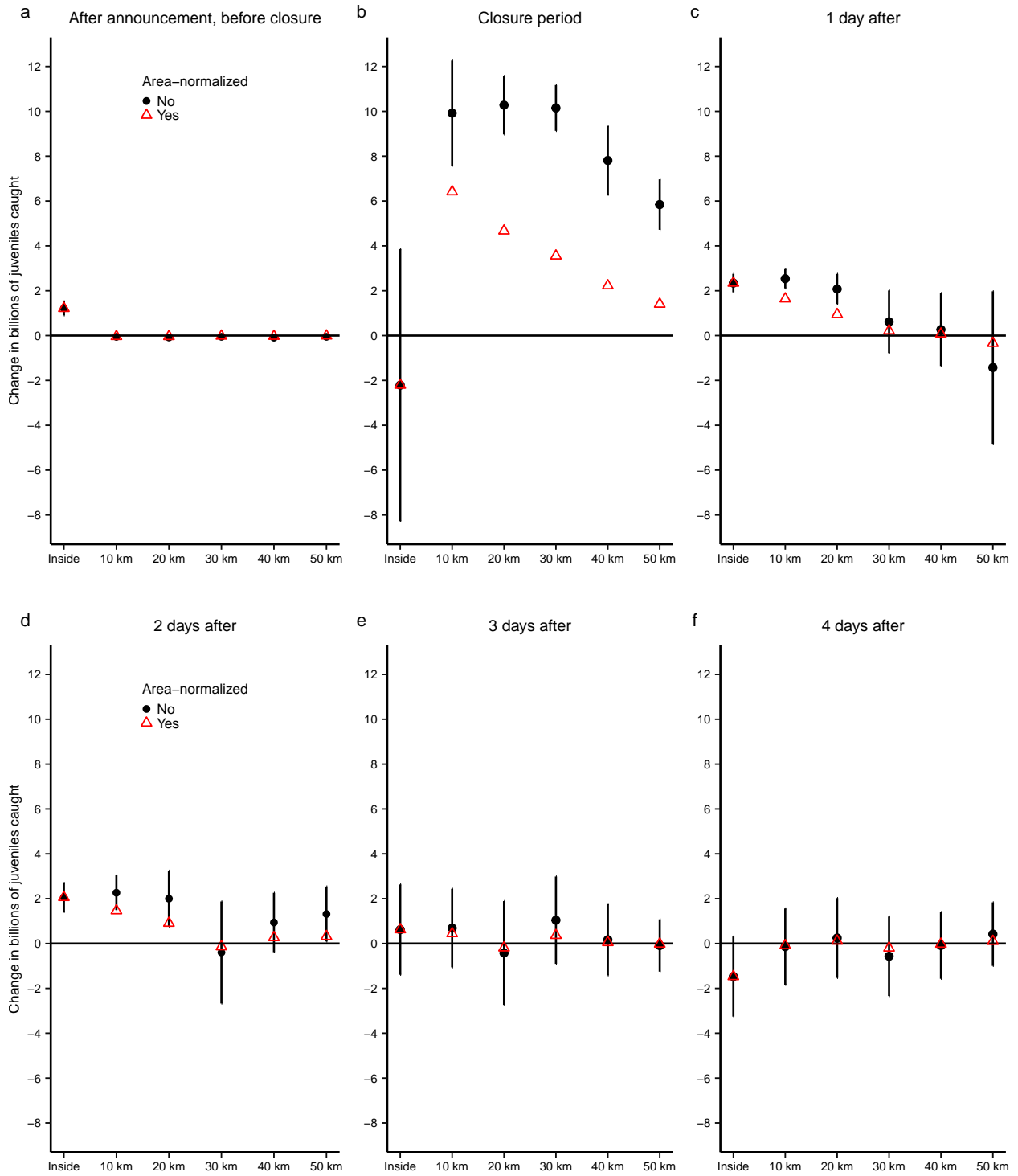
I estimate the effect of the temporary spatial closures policy on juvenile catch with Equation 1, convert the treatment coefficients into levels, and compute standard errors using the delta method.<sup>22</sup> Figure 7 shows the main result of this paper. The y-axis is the change in the number of juveniles 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 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 (Figure 7a), juvenile catch increases by 1.2 billion inside soon-to-be closed areas (temporal spillover). 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

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<sup>22</sup>Figure A2 displays the treatment coefficients (the  $\beta_{st}$  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 rearrange terms and calculate  $CounterfactualJuvenileCatch_{st} = (ObservedJuvenileCatch_{st}) / (\exp(\beta_{st}))$ . Then the change in juvenile catch in bin  $st$  in levels is  $ObservedJuvenileCatch_{st} - CounterfactualJuvenileCatch_{st}$ .

Figure 7: 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.  $N = 34,272$ . Standard errors clustered at level of two-week-of-sample by two-degree grid cell.

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.<sup>23</sup> 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 billion increase in juvenile catch.

The hollow, red triangles in Figure 7 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 6 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 (Figures 7c and 7d). Juvenile catch increases by 2.3 billion inside closed areas in the first 24 hours after the end of closure periods (temporal spillover). 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 also occurs in the second day after the end of closure periods. However, by the third and fourth day after closures, 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 an increase in the number of juveniles caught by 58 billion, or 69%. However, this approach is naïve because it ignores the total allowable catch limit the regulator sets each season (Section I). When I use tons as the dependent variable in Equation 1, I estimate that the closures policy increases tons caught by 31% on average across the 36 treatment bins. But total tons caught cannot increase in the four (of six) fishing

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<sup>23</sup>The direct effect's large confidence interval is a consequence of the conversion of the treatment coefficients into levels. The confidence interval of the direct effect's treatment coefficient is similar in magnitude to the confidence interval of other treatment bins (Figure A2). Like the direct effect in levels, the direct effect's treatment coefficient is not statistically significant.

seasons during my study period in which the total allowable catch limit was binding.<sup>24</sup> 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 closures policy is an increase in the number of juveniles caught by 46 billion, or 48% (delta method standard errors are 5.4 billion and 5.7%). This 48% increase in juvenile catch is my preferred estimate of the total effect of the closures policy.

The regulator’s total allowable catch limit, enforced through individual vessel quotas, reduces the backfire caused by the closures policy because without it, closures would increase juvenile catch by an even greater amount. The increase in juvenile catch because of 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. Average percentage juvenile among sets within the treatment window of actual closures is 25%, compared to 9% outside the treatment window (Figure C2).

My results are robust to alternative specifications and estimation approaches (Appendices A.2, A.3 and A.4). I explore treatment effect heterogeneity by size of closure and length of closure period in Appendix D.1, and by firm size and vessel size in Appendix D.2.

## V Do closures provide valuable information?

Why does the closures policy increase total juvenile catch? Closures might provide information regarding the location of anchoveta because the regulator declares closures in response

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<sup>24</sup>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. In the second seasons of 2017 and 2019, the regulator cancelled the seasons, before their scheduled end dates, when only 44% and 36% of the TAC had been reached. The regulator cancelled the second season of 2017 because the scientific agency detected significant spawning activity (IMARPE, 2018). The regulator cancelled the second season of 2019 because oceanographic conditions led schools of juveniles to inhabit the same areas as schools of adults (PRODUCE, 2020*a*). The co-occurrence of juveniles and adults led to high rates of juvenile catch because fishers have limited ability to predict whether anchoveta in the water are juveniles or adults (Paredes, 2014; IMARPE, 2019).



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. This information may be particularly valuable because reducing search costs is the main margin by which fishers can increase profits within a fishing season.

Fishers are largely unable to adjust output quantity, output prices, or input prices. First, individual vessel quotas limit the tons of anchoveta that vessels can land each season. This constraint on output quantity is typically binding. Second, output price is exogenous because firms pay fishers a fixed percentage of the international price of fishmeal for each ton of anchoveta they land.<sup>25</sup> Individual fishers or fishing vessels cannot affect the international price of fishmeal. Input prices are also not affected by fishers' decisions. The price of fuel is exogenous and wages equal a fixed percentage of the international price of fishmeal.

Fishers can adjust input quantities, namely the time and fuel they spend searching for anchoveta. Fishers 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 (Kroetz et al., 2016; Joo et al., 2015).

Fishing near closures could also reduce costs by increasing tons caught per set of the fishing net. Tons caught per set is a measure of fishing productivity conditional on finding anchoveta because fishers only perform a set when they see anchoveta in the water.<sup>26</sup> An increase in tons caught per set would be valuable to fishers 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

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<sup>25</sup>Firms pay fishers 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, fishers 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 among themselves in fixed proportions: the captain receives “two parts” (twice as much as a regular fisher), the second-in-command and first engineer receive one and a half parts, and regular fishers receive one part (SUPNEP, 2017). Interviews and analysis conducted by Hansman et al. (2020) indicate that fishers not covered by this agreement also receive a fixed percentage of the FOB price of fishmeal.

<sup>26</sup>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.

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.

If closures provide information regarding the location of anchoveta, then vessels should change where they fish in response to closures. I test this possibility by estimating the effect of closures on fishing locations. For each potential closure-treatment bin, I filter sets to those that occur during the same time period (between the beginning and the end of the potential closure-treatment bin). Then I calculate the distance of these sets to the potential closure-treatment bin. I record the minimum, 25th percentile, median, mean, 75th percentile, and maximum distance. I drop 1,134 potential closure-treatment bins with no sets between their beginning and end because I cannot compute distance quantiles for these sets. The remaining 33,138 potential closure-treatment bins have an average of 825 sets that occur during the same time period.

I re-estimate Equation 1 with these distance quantiles as the dependent variable. The only other difference between Equation 1 and the results in Table 3 is I estimate a single treatment fraction coefficient, rather than separate coefficients for each treatment bin. The resulting coefficient represents the change in distance to potential closures, averaged over treatment bins, when a potential closure is declared an actual closure compared to when it is not declared a closure by the regulator. The mostly negative and statistically significant coefficients in Table 3 indicate that one way in which vessels respond to closures is by fishing closer to them.

Table 4 offers a second piece of evidence in favor of the hypothesis that closures provide information. If they do, then vessels that fish near them might reap the value of that information by catching more tons of anchoveta per set. I test this hypothesis with the following ordinary least squares regression:

$$(2) \quad Tons_{vjk} = \beta_1 \mathbb{1}\{Near\}_{vjk} + \beta_2 DistToCoast_{vjk} + \delta_{vj} + \gamma_d + \alpha_{jg} + \epsilon_{vjk}$$

Table 3: Sets move toward closures

|                    | Dependent variable: Distance quantile (km) |                 |                 |                 |               |                 |
|--------------------|--|-----------------|-----------------|-----------------|---------------|-----------------|
|                    | 0%   | 25%             | 50%             | Mean            | 75%           | 100%            |
|                    | (1)  | (2)             | (3)             | (4)             | (5)           | (6)             |
| Treatment fraction | -22.5<br>(8.4)                             | -19.5<br>(11.1) | -18.3<br>(11.8) | -16.6<br>(11.7) | 1.0<br>(13.1) | -25.2<br>(11.4) |
| Mean dep. var.     | 33.1                                       | 173.4           | 286.2           | 307.1           | 431.8         | 692.0           |

Notes: All regressions have 33,138 observations. The unit of observation is a potential closure-treatment bin. For each potential closure-treatment bin, I calculate the distance to sets that occur during the period of the potential closure-treatment bin. The dependent variable is a quantile of these distances (in Column 4 it is the mean). All regressions control for the same variables as Equation 1. Standard errors clustered at level of two-week-of-sample by two-degree grid cell.

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.<sup>27</sup> 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 7) 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 IV can be explained by the closures policy communicating information about the value of fishing in those places and times. I include the distance of each set to the coast, vessel by season fixed effects, day-of-sample fixed effects, and two-degree grid cell by season fixed effects 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 fishers receive per ton of anchoveta they land. Therefore, the change in tons per set captured by  $\beta_1$  represents the change in revenue per set from fishing near closures. To the extent that the fixed effects and the distance of each set to the coast control for cost per set,  $\beta_1$  can also be interpreted as the change in profit per set from fishing near closures.

When I define the near indicator relative to potential closures, I find that sets that happen

<sup>27</sup>Vessels report catching 0 tons for 11% of sets in the electronic logbook data.

Table 4: Closures provide valuable information, but the value of this information is competed away

| Dependent variable: asinh(tons) |                    |                  |                  |                   |
|---------------------------------|--------------------|------------------|------------------|-------------------|
|                                 | Potential closures |                  | Actual closures  |                   |
|                                 | (1)                | (2)              | (3)              | (4)               |
| $\mathbb{1}\{\text{Near}\}$     | 0.171<br>(0.073)   | 0.104<br>(0.028) | 0.310<br>(0.080) | -0.022<br>(0.033) |
| Distance to shore (km)          | 0.011<br>(0.002)   | 0.004<br>(0.001) | 0.011<br>(0.002) | 0.004<br>(0.001)  |
| Constant                        | 3.293<br>(0.102)   |                  | 3.306<br>(0.082) |                   |
| Fixed effects                   |                    | X                |                  | X                 |

Notes: 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 potential closures (mean of this indicator is .798). In Columns 3 and 4, Near is defined relative to actual closures declared by the regulator (mean of this indicator equals .391). Electronic logbook data are 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.

to occur near potential closures catch 11% more tons on average (Column 2). But when I instead define the near indicator relative to actual closures declared by the regulator, I find that sets near actual closures do not catch more tons of anchoveta (Column 4). This null result is consistent with Proposition 2a in Appendix B: in the Bayes-Nash equilibrium, vessels fish near closures until the profit from doing so equals the profit from fishing elsewhere.

These results illustrate one benefit of my potential closures identification strategy. Since closures are publicly announced, the *ex post* value of this information can be competed away due to congestion; there are diminishing marginal returns when more vessels fish from the same local, exhaustible anchoveta population (Smith, 1969; Huang and Smith, 2014). I create potential closures from the same electronic logbook data the regulator uses to determine actual closures. If they were announced, potential closures would communicate similar information to vessels as actual closures: fishing occurred recently in the area, so anchoveta are likely abundant nearby. 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 Appendix B). By contrast, estimation strategies that do not create counterfactual closures, such as estimating the policy's effect with rasterized data in Appendix A.3, hinder observation of the information provided by closures (despite revealing the same pattern of treatment effects as Figure 7). Taken together, the results in Table 4 suggest closures do provide valuable information, but vessels compete away the *ex post* value of this information.<sup>28</sup>

## VI Does the information provided by closures increase spillovers?

In Section V, I presented evidence that closures implicitly provide information to vessels about the value of fishing near closures. I now test explicitly whether the information provided by closures is a mechanism underlying the policy's large spillover effects.

Proposition 3 in Appendix B states that vessels that receive a stronger positive information shock from closure announcements will have larger treatment effects. Vessels experience a stronger 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 po-

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<sup>28</sup>These results also suggest that vessels fish more near closures because of information regarding anchoveta presence, as opposed to high percentage juvenile areas being especially desirable fishing grounds. The reason is potential closures are areas where any fishing occurred, as opposed to being areas with high percentages of juveniles, and I find that fishing near potential closures is more productive than fishing elsewhere. Actual closures are areas with high percentages of juveniles, but I estimate that fishing near actual closures is not more productive than fishing elsewhere.

tential 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:

$$\begin{aligned}
 \text{(3)} \quad \text{JuvenileCatch}_{isth} = & \alpha_{sth} + \beta_{sth} \text{TreatFraction}_{ist} + \sum_{\ell=\{3,3.5\}}^{[18.5,19]} \xi_{\ell} \text{Prop}_{i\ell} + \\
 & \gamma_1 \text{Sets}_i + \gamma_2 \text{Tons}_i + \gamma_3 \text{Area}_i + \gamma_4 \text{DistToCoast}_i + \\
 & \gamma_5 \text{TonsPerSet}_i + \gamma_6 \text{TonsPerArea}_i + \sigma_{wg} + \delta_d + \epsilon_{isth}
 \end{aligned}$$

where  $h$  indicates heterogeneity (in information) category and all other variables and subscripts have the same definitions as in 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 8 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 9 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.<sup>29,30</sup> Unlike in Figure 7, I present the results in percent changes rather than changes in levels because more vessels belong to the lower information group.

<sup>29</sup>I leave out own-fishing in the firm-level categorization. I code vessels 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.

<sup>30</sup>The data underpinning Figure 9 necessarily exclude the 271 vessels that belong to a firm that only owns one vessel. However, I include these vessels in the data used to estimate Figure 8. I provide more information on firm heterogeneity and industry structure in Appendix D.2.

Vessels that fished inside a potential closure the day before closure announcement have a much smaller total treatment effect (0.5% increase in total juvenile catch) than vessels that did not (87.2% increase in total juvenile catch).<sup>31</sup> 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 3.3%), 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 5.8%). The difference in treatment effects between the two information groups is also statistically significant (difference is 86.7% with a standard error of 6.7%).<sup>32</sup>

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 (19.4% increase in juvenile catch) than vessels who did not (78.4% increase in juvenile catch). Both treatment effects are different from zero (standard errors are 3.8% and 5.4%), as is the difference in treatment effects (difference is 59% with a standard error of 6.6%).

The results in this section support Proposition 3 that vessels that receive a stronger 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 8 also explains why vessels underreport percentage juvenile in the raw electronic logbook data (Section II) even though closures provide valuable information (Section V). Closures only provide information to vessels who were not already fishing in the area, so vessels might underreport percentage juvenile to avoid triggering a closure in the area they are already fishing.<sup>33</sup>

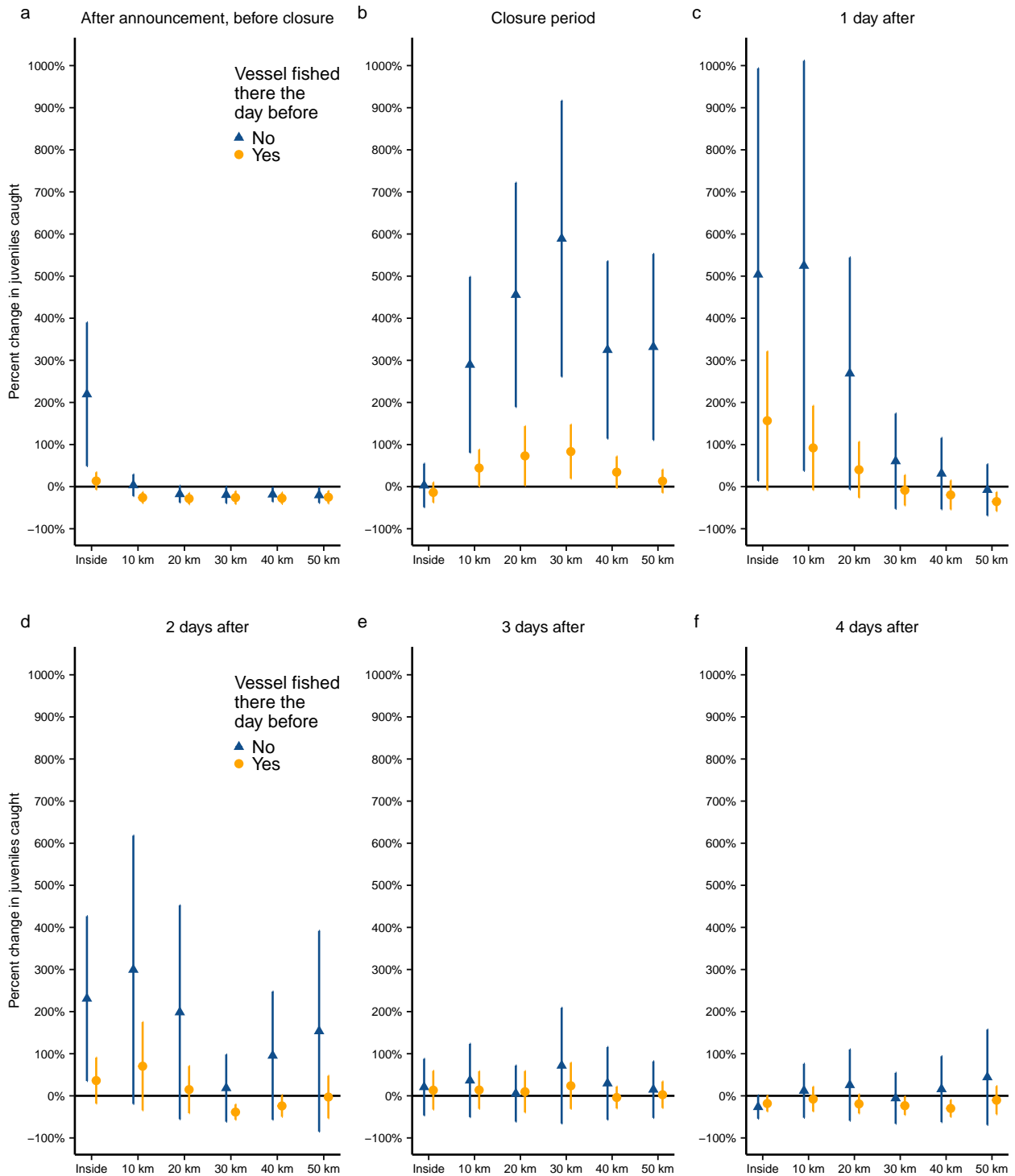
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<sup>31</sup>I calculate the percentage change in total juvenile catch for both groups and both categorizations in the same way as in Section IV, converting the treatment coefficients into changes in levels and accounting for the reallocation in tons caught in the four fishing seasons the total allowable catch limit was binding.

<sup>32</sup>Note that this result does not reflect mean reversion because I estimate treatment effects across potential closures. I estimate the treatment effect for vessels that fished inside a potential closure the day before closure announcement would occur 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.

<sup>33</sup>Recall that underreporting by vessels does not bias my estimates because I calculate my regressions’

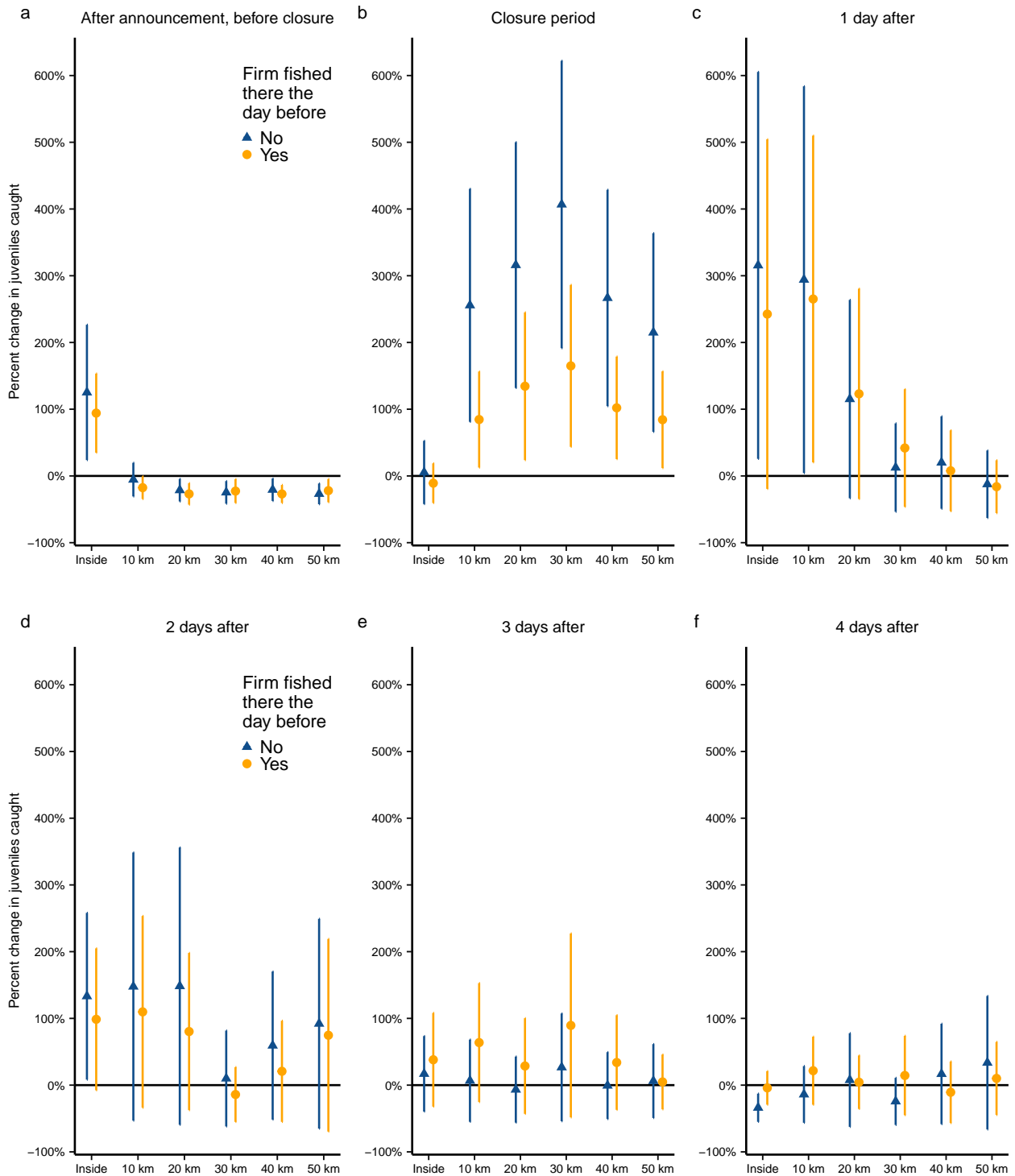
Figure 8: Percent change in juvenile catch because of the closures policy by whether vessels fished in the potential closure the day before closure announcement



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.  $N = 68,544$ . Standard errors clustered at level of two-week-of-sample by two-degree grid cell.



Figure 9: 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.  $N = 68,544$ . Standard errors clustered at level of two-week-of-sample by two-degree grid cell.

## VII Discussion

Peru's temporary spatial closures policy attempts 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 48%.

The failure of this policy to achieve its objective is not due to a failure of targeting. Even though the regulator does not use length distribution data to determine closures, they still succeed in closing areas where juveniles are abundant. 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). Furthermore, within the support of the data, larger and longer closures offer no improvement (Appendix D.1).<sup>34</sup>

I illustrate the short- and long-run costs of the temporary spatial closures policy with two simulations. First, I reproduce the method of Salvattecchi 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 32% lower than status quo juvenile catch (equivalently, until status quo juvenile catch is 48% 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. FOB export revenues were \$1.79 billion in 2017 (PRODUCE, 2018a). I therefore calculate that the closures policy reduces exports by \$75 million per year

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outcome variable and control variables using third-party inspector data.

<sup>34</sup>Larger closures could cause larger increases in juvenile catch because congestion costs are lower or if larger closures are a stronger positive signal of fishing productivity.

(\$1.79 billion  $\cdot$  -2.1%  $\cdot$  two fishing seasons). This projection does not account for the long-run cost of foregoing juveniles' higher remaining reproductive capacity over their lifetimes.

To approximate the long-run cost of the closures policy, I construct a second simulation in which anchoveta reproduce as well as grow, die from natural causes, and die due to fishing at rates that depend on their age (Appendix E). I simulate the population forward under two scenarios: status quo and counterfactual. At the same biomass (population size in tons), tons caught in both scenarios is the same. The only difference between the two scenarios is that the proportion of juveniles caught in the status quo scenario matches what I observe in the data, while the proportion of juveniles caught in the counterfactual scenario equals the proportion that would have been caught in the absence of closures. Fewer juveniles reach adulthood in the status quo scenario, which reduces reproduction, which in turn reduces biomass over time. As the population shrinks, so does catch. With a 5% discount rate and a 20-year time horizon, I calculate that the long-run cost of the closures policy in terms of foregone export revenue is \$5.2 billion (2017 USD), or \$258 million per year.

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 and Gentzkow, 2011; Kamenica, 2019). The Sender chooses the decision rule that induces the Receiver to act in a way that maximizes the Sender's utility. Here, the regulator (Sender) wants vessels (Receivers) to catch fewer juveniles, but the signal conveyed by closure announcements directs fishing to the places with the most juveniles. Instead, the regulator could announce where vessels catch high percentages of adults. Because fishers 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 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. Vessels 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.<sup>35</sup> I assume that the 31% 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, vessels catch 46 billion *fewer* juveniles due to the elimination of the closures policy (fewer tons in high percentage juvenile areas). Second, they catch 4 billion *more* juveniles near the 410 high-adult potential closures (more tons in low percentage juvenile areas). Third, they catch 32 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 52% lower compared to the status quo level of juvenile catch (67 billion juveniles are caught compared to 141 billion). By attracting fishing to the places with the fewest juveniles, the regulator could help fishers 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. Understanding this relationship, and designing targeted policies accordingly, will become even more relevant in the future as increasing data availability enables governments to target a larger share of policies.

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<sup>35</sup>I use the sets that generate each potential closure and I weight them by the number of individuals caught by each set.

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# Online Appendix

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

Gabriel Englander

November 29, 2022

# A Robustness checks

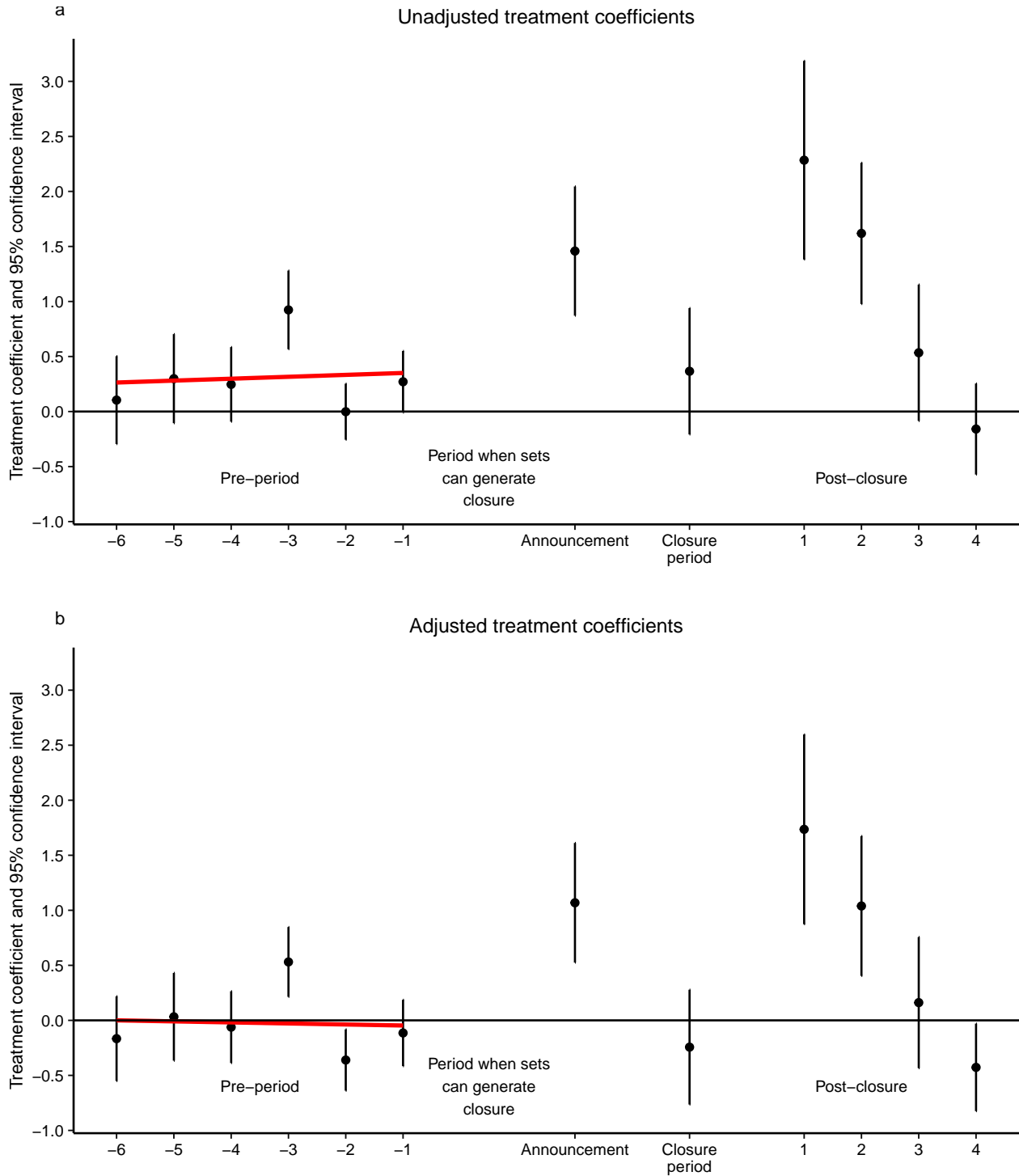
## 1 Additional balance tests

To examine trends in pre-period juvenile catch, I 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 A1.

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 A1a). 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 A1b). 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 primary identification strategy I use in this paper.

Table A1 establishes the correlations between juvenile catch and 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 ( $\text{km}^2$ ). I confirm that treatment and control potential closures are balanced on these variables in Table 2.

Figure A1: Test for pre-trend in juvenile catch inside potential closures



Notes:  $N=39,984$  for both regressions. The dependent variable is the inverse hyperbolic sine of millions of juveniles caught. Both regressions estimate treatment effects for all 42 treatment bins, but this figure only displays the treatment fraction coefficients for inside potential closures treatment bins. 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. Points are coefficients and whiskers are 95% confidence intervals. Standard errors clustered at level of two-week-of-sample by two-degree grid cell.

Table A1: Correlation between juvenile catch and measures of fishing productivity

|             | Dependent variable: asinh(juvenile catch) |                    |                    |                    |
|-------------|---|--------------------|--------------------|--------------------|
|             | (1)                                       | (2)                | (3)                | (4)                |
| DistToCoast | 0.0050<br>(0.0017)                        |                    |                    | 0.0026<br>(0.0015) |
| TonsPerSet  |   | 0.0075<br>(0.0007) |                    | 0.0069<br>(0.0009) |
| TonsPerArea |   |                    | 0.0138<br>(0.0036) | 0.0027<br>(0.0032) |
| Intercept   | 0.6375<br>(0.0660)                        | 0.3826<br>(0.0524) | 0.6975<br>(0.0515) | 0.3169<br>(0.0556) |

All regressions have 34,272 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.

## 2 Treatment coefficients from estimating Equation 1 and variants of Equation 1

Figure A2 displays the treatment coefficients from estimating Equation 1. The reader may interpret this figure in percentage terms by applying the transformation  $\exp(\beta_{st}) - 1$ . 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 A3). They also hold if I replace the dependent variable in Equation 1 with a binary indicator for positive juvenile catch (Figure A4) or if I replace treatment fraction with a binary indicator for positive treatment fraction (Figure A5).

My results are also robust to replacing the outcome variable with the inverse hyperbolic sine of tons of juveniles caught (Figure A6). Juvenile catch in tons is the number of individuals in each length interval times the weight of an individual in each length interval, summed over length intervals less than 12 cm.<sup>36</sup> When I convert these treatment coefficients into changes in levels and account for the reallocation in tons (of juveniles and adults) caught

<sup>36</sup>For a given set, tons of juveniles caught equals  $\sum_{\ell=[3,3.5]}^{[11.5,12]} w_{\ell} N_{\ell}$ , where  $w_{\ell}$  is the weight of an individual in length interval  $\ell$  and  $N_{\ell}$  is the number of individuals in length interval  $\ell$  that the set caught.

due to the total allowable catch limit, I estimate that the temporary spatial closures policy increases juvenile catch by 315 thousand tons of juveniles, or 41% (delta method standard errors are 75 thousand tons and 9.7%). For comparison, the regulator calculates that the temporary 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, 2017, 2018*c,b*). 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 A7 to A9. 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 50%, 65% and 51%, respectively (delta method standard errors are 4.6%, 4.3%, and 6.0%, respectively).

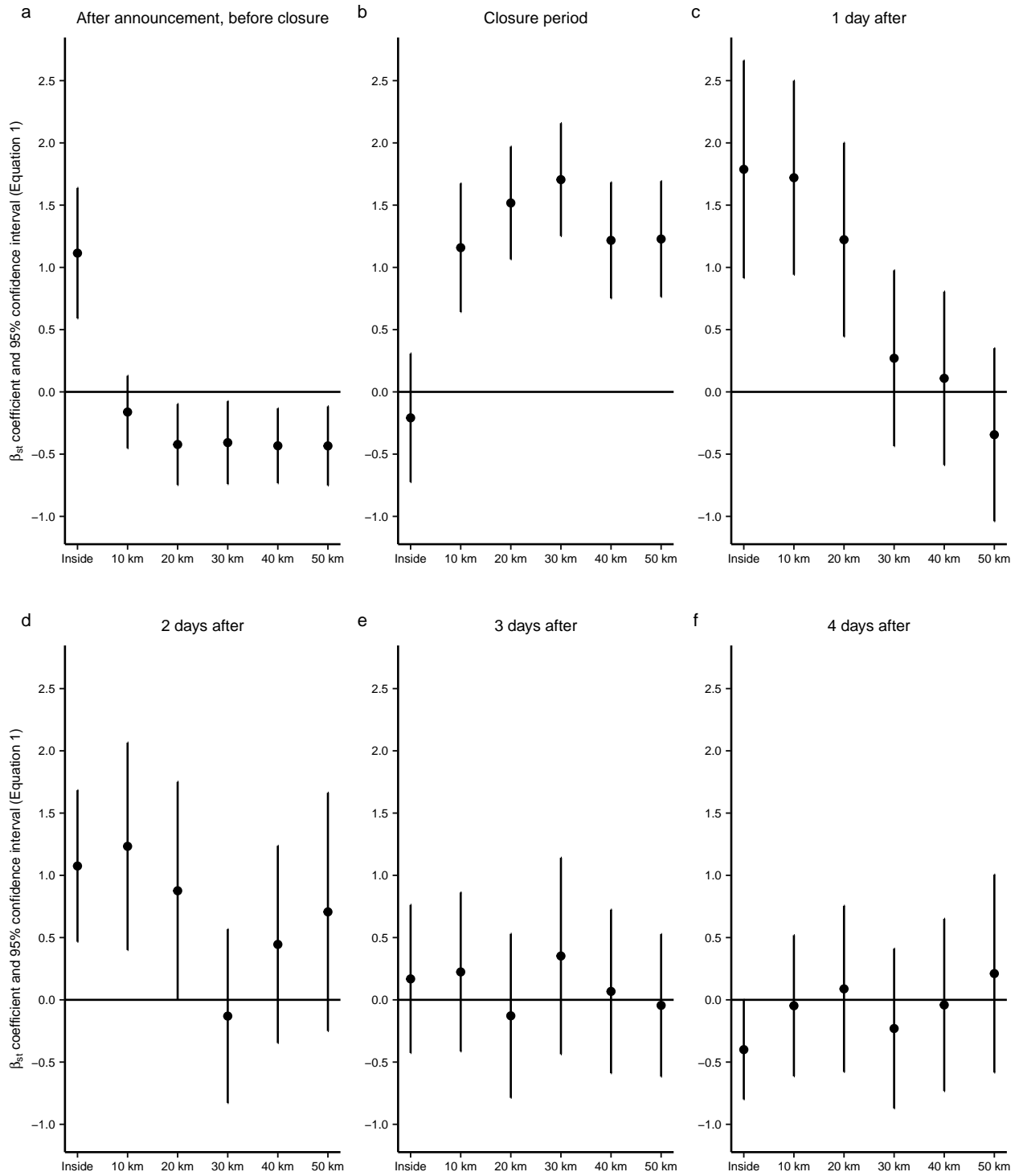
In my main specification, the median fraction of area of actual closures covered by potential closures is 0.5. When I make potential closures 40% larger, the median fraction of area of actual closures covered by potential closures increases to 0.67. Even though the coverage fraction is 34% higher in this case, I estimate a total treatment effect that is similar to the estimate I obtain with my main specification (51% compared to 48%). The similarity of these two estimates indicate that my results are robust to modifying my potential closures algorithm to more closely match the regulator’s closure rule.

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 I display the treatment coefficients from this regression in Figure A10. 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 63% of the increase in juvenile catch due to the closures policy occurs in the first half of fishing seasons. For reference, vessels land 58% of tons during the first 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).

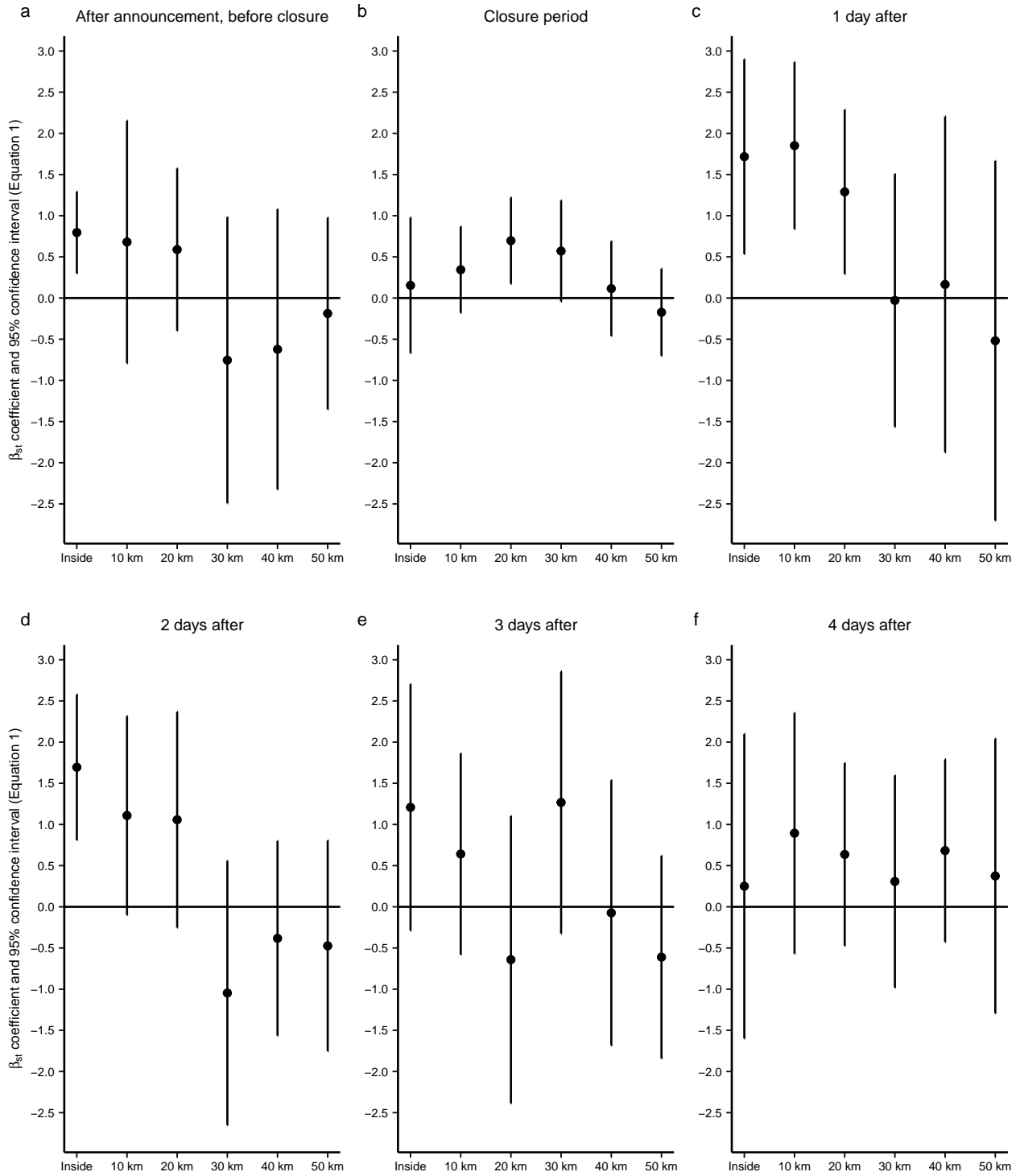


Figure A2: Treatment coefficients from estimating Equation 1



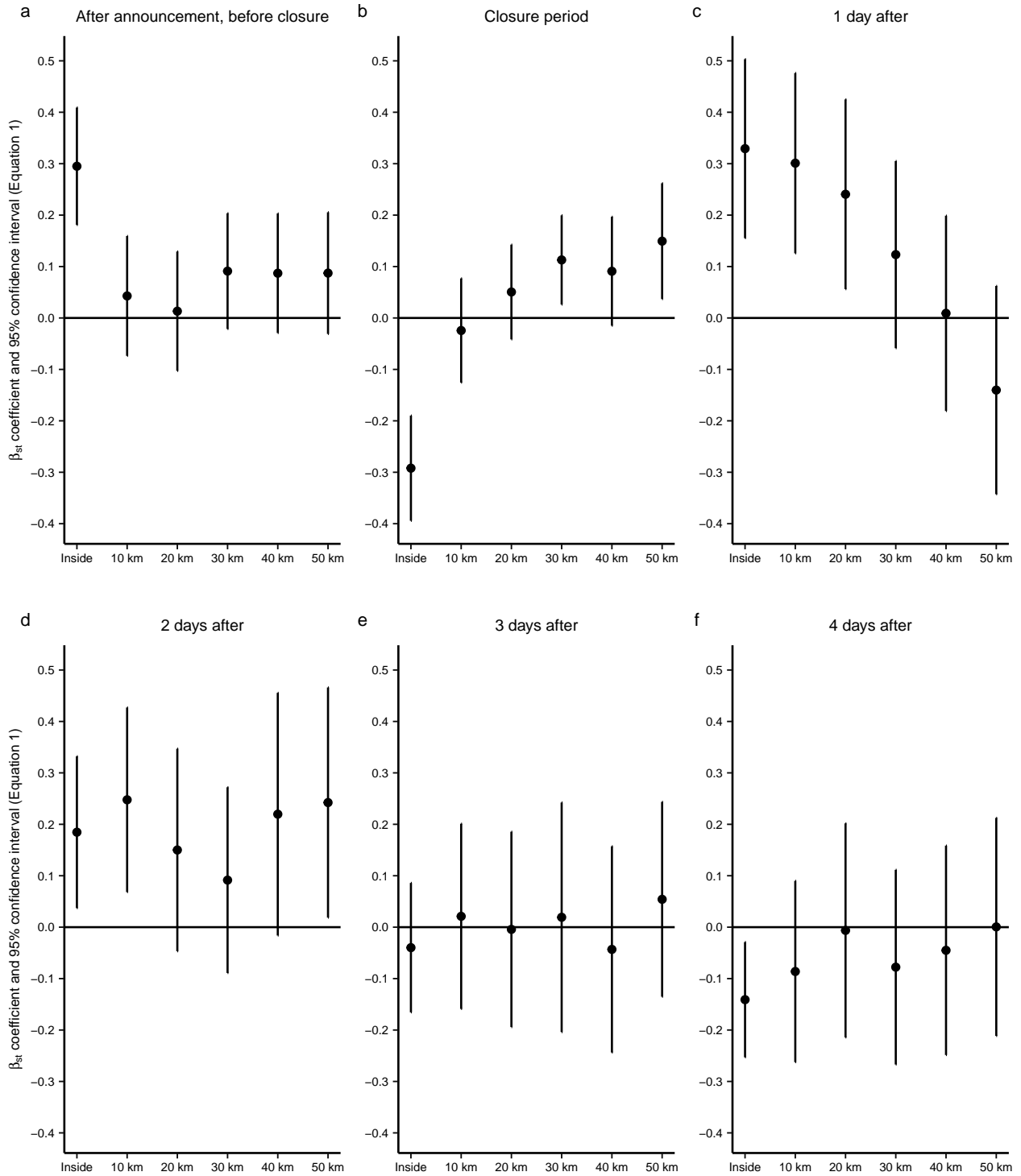
Notes:  $N = 34,272$ . Points are coefficients and whiskers are 95% confidence intervals. Standard errors clustered at level of two-week-of-sample by two-degree grid cell.

Figure A3: 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



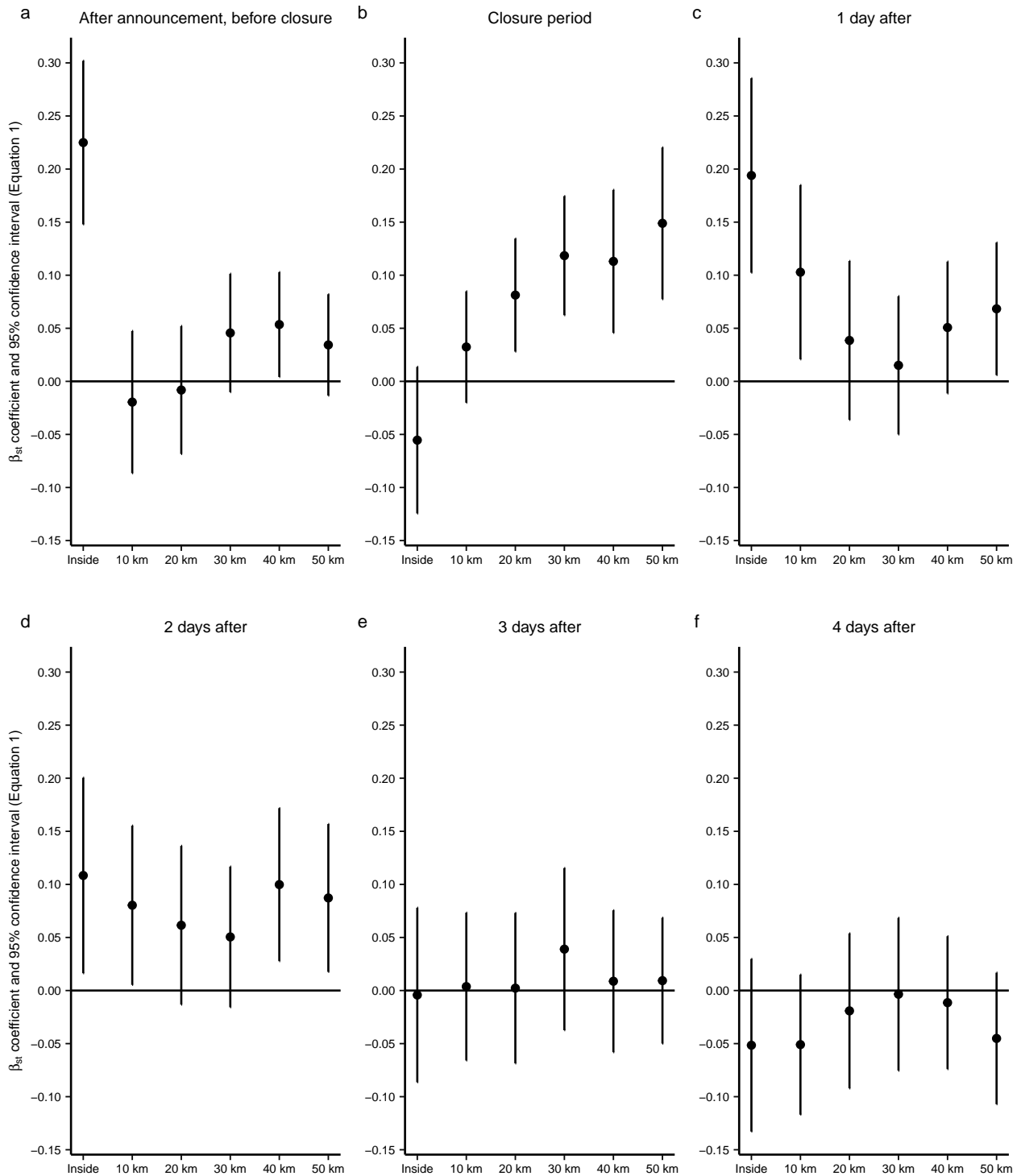
Notes:  $N = 12,181$ . Points are coefficients and whiskers are 95% confidence intervals. Standard errors clustered at level of two-week-of-sample by two-degree grid cell.

Figure A4: Treatment coefficients from re-estimating Equation 1 with a binary indicator for positive juvenile catch as the dependent variable



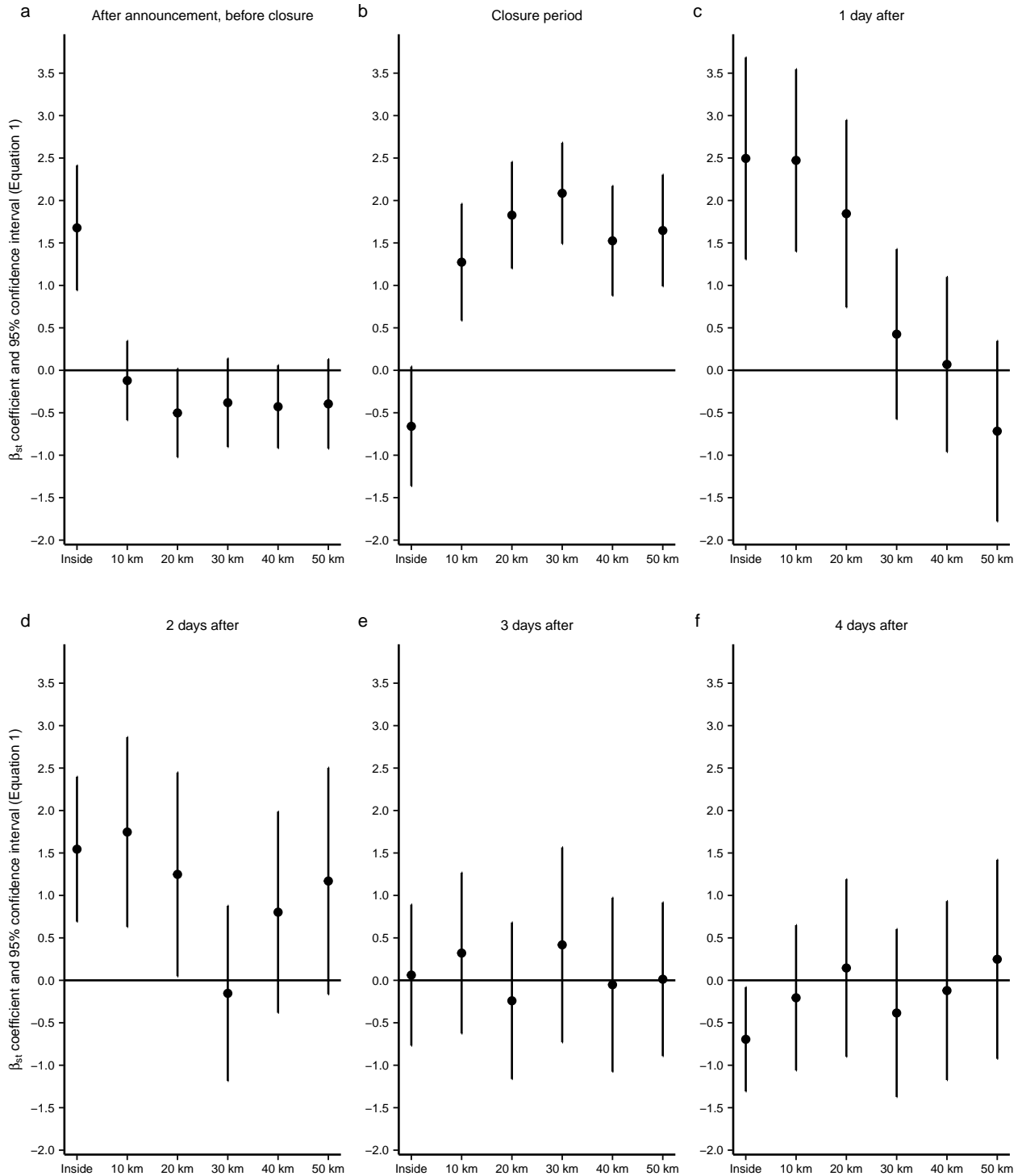
Notes:  $N = 34,272$ . Points are coefficients and whiskers are 95% confidence intervals. Standard errors clustered at level of two-week-of-sample by two-degree grid cell.

Figure A5: Treatment coefficients from re-estimating Equation 1 with a binary indicator for positive treatment fraction, rather than defining treatment fraction as a continuous variable



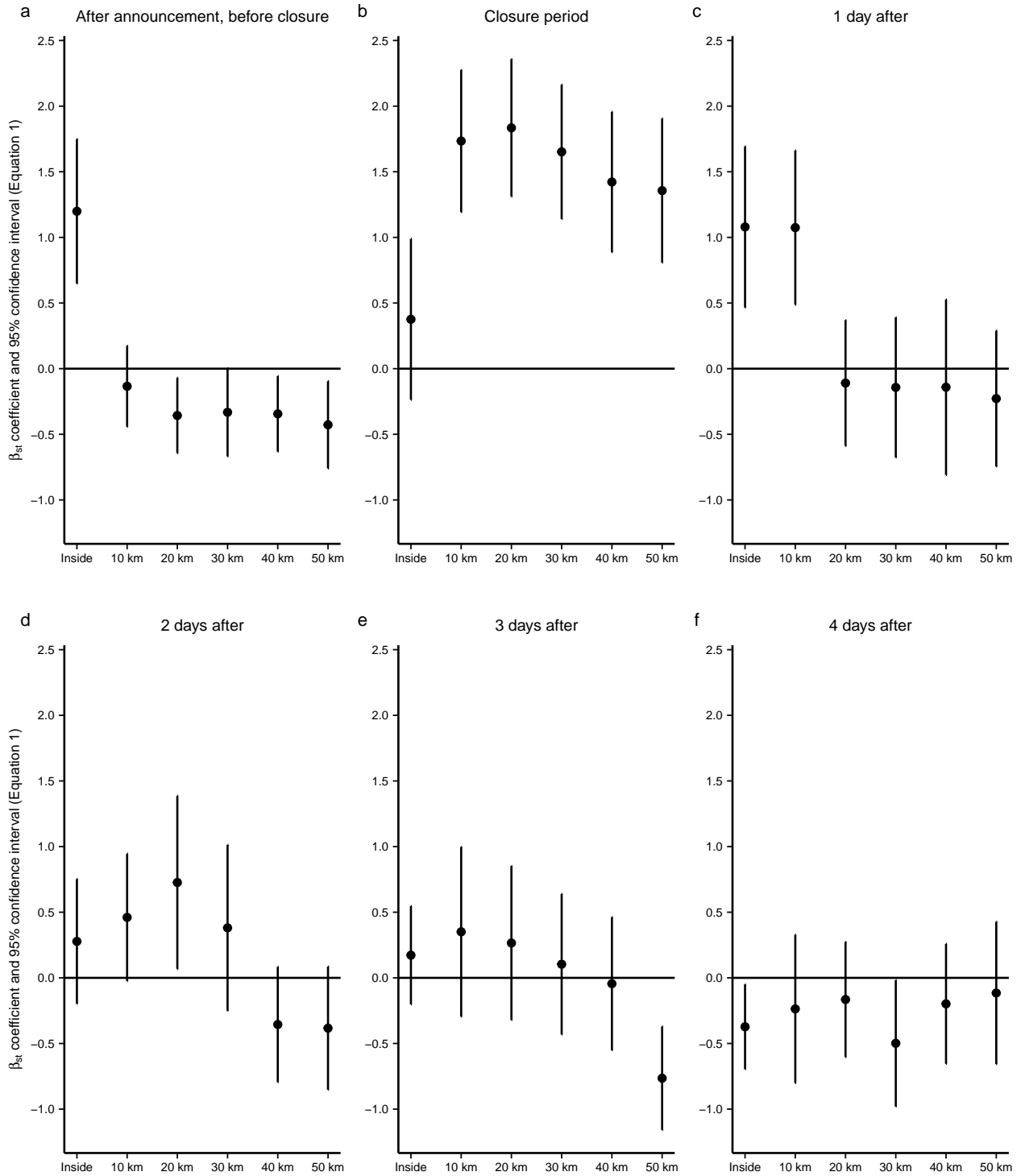
Notes:  $N = 34,272$ . Points are coefficients and whiskers are 95% confidence intervals. Standard errors clustered at level of two-week-of-sample by two-degree grid cell.

Figure A6: Treatment coefficients from re-estimating Equation 1 with tons of juveniles caught as the dependent variable



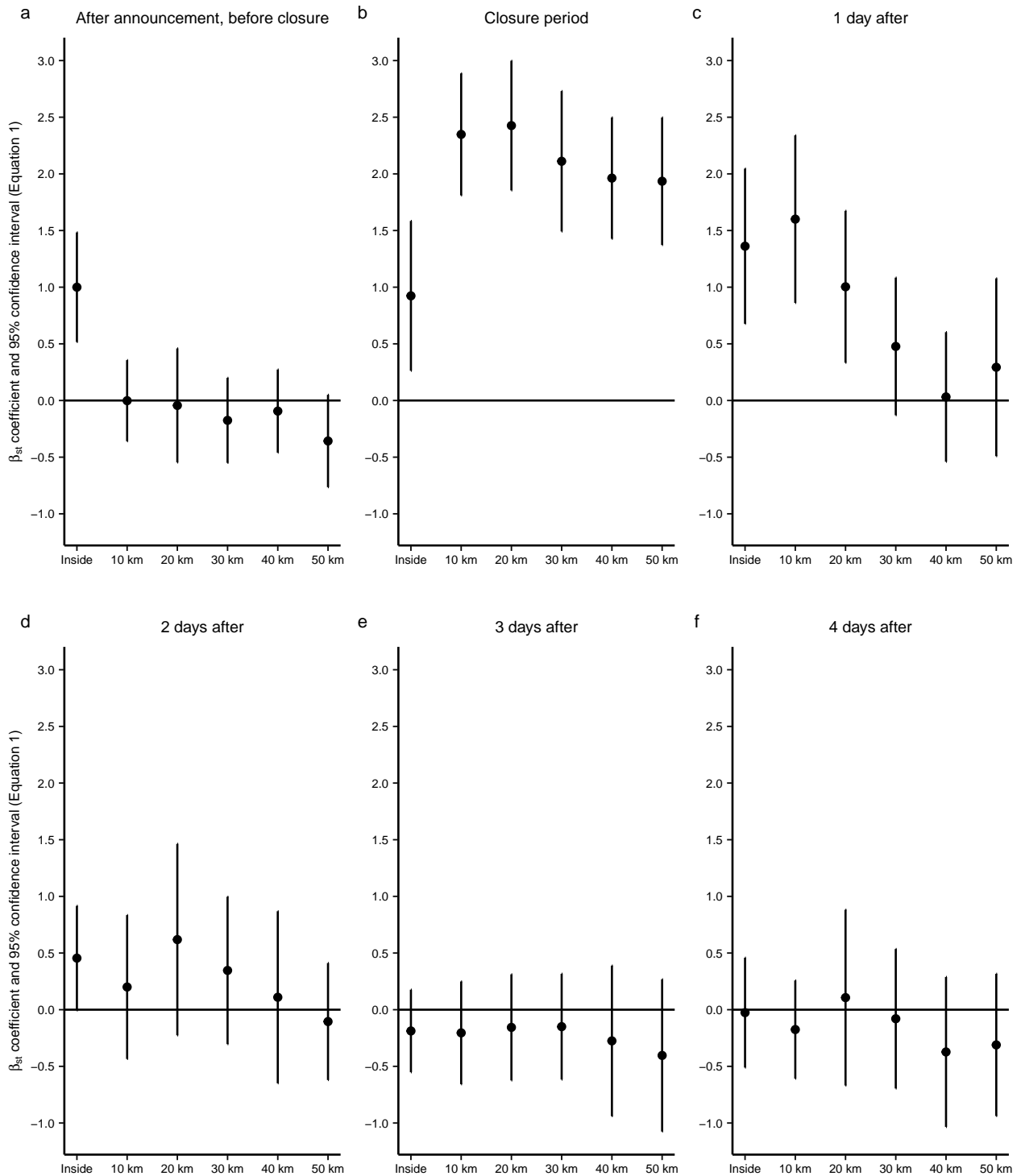
Notes: The dependent variable is the inverse hyperbolic sine of tons of juveniles caught in each potential closure-treatment bin.  $N = 34,272$ . Points are coefficients and whiskers are 95% confidence intervals. Standard errors clustered at level of two-week-of-sample by two-degree grid cell.

Figure A7: Treatment coefficients from re-estimating Equation 1 with potential closures that last four days



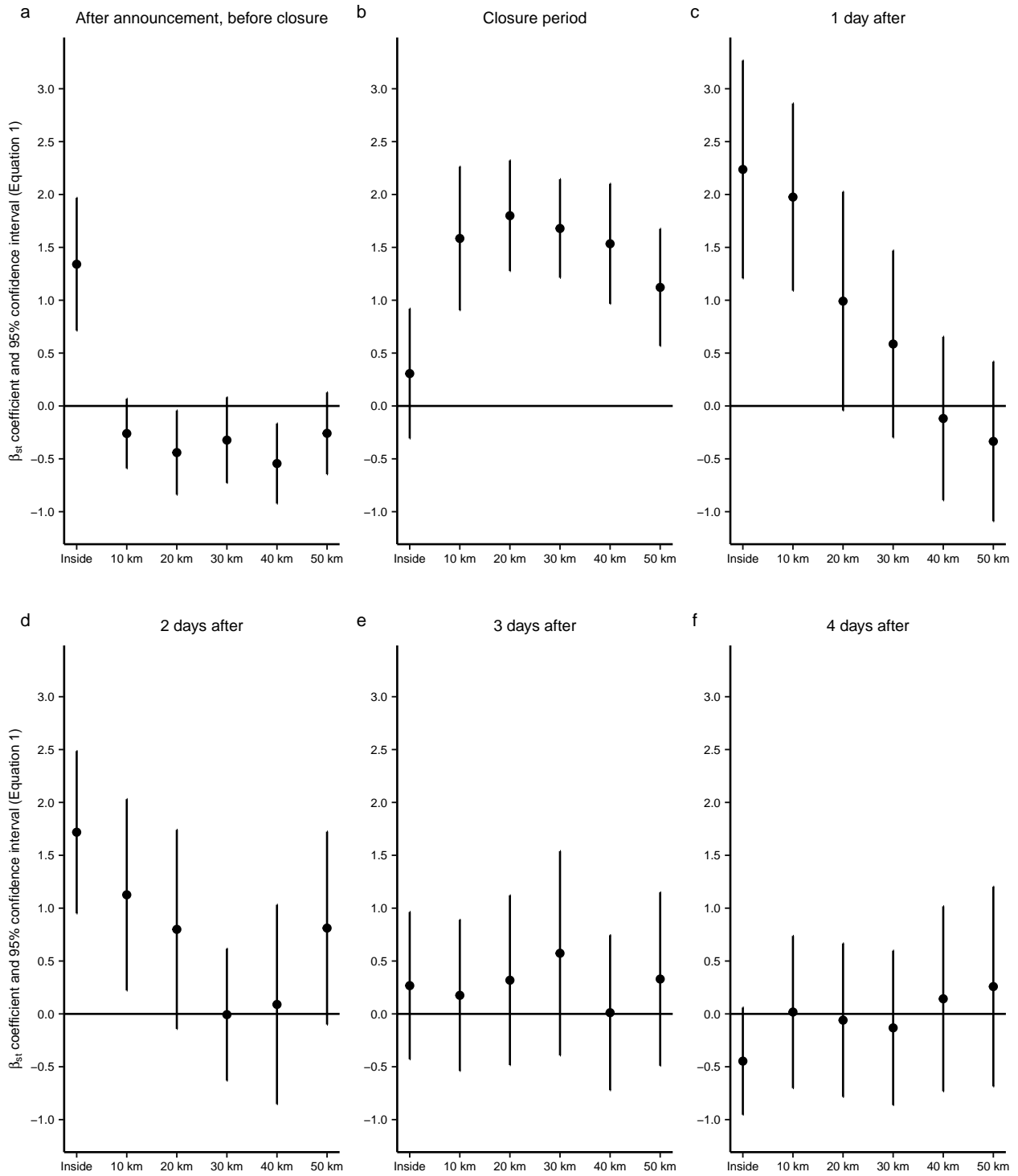
Notes:  $N = 31,608$ . Points are coefficients and whiskers are 95% confidence intervals. Standard errors clustered at level of two-week-of-sample by two-degree grid cell.

Figure A8: Treatment coefficients from re-estimating Equation 1 with potential closures that last five days



Notes:  $N = 29,628$ . Points are coefficients and whiskers are 95% confidence intervals. Standard errors clustered at level of two-week-of-sample by two-degree grid cell.

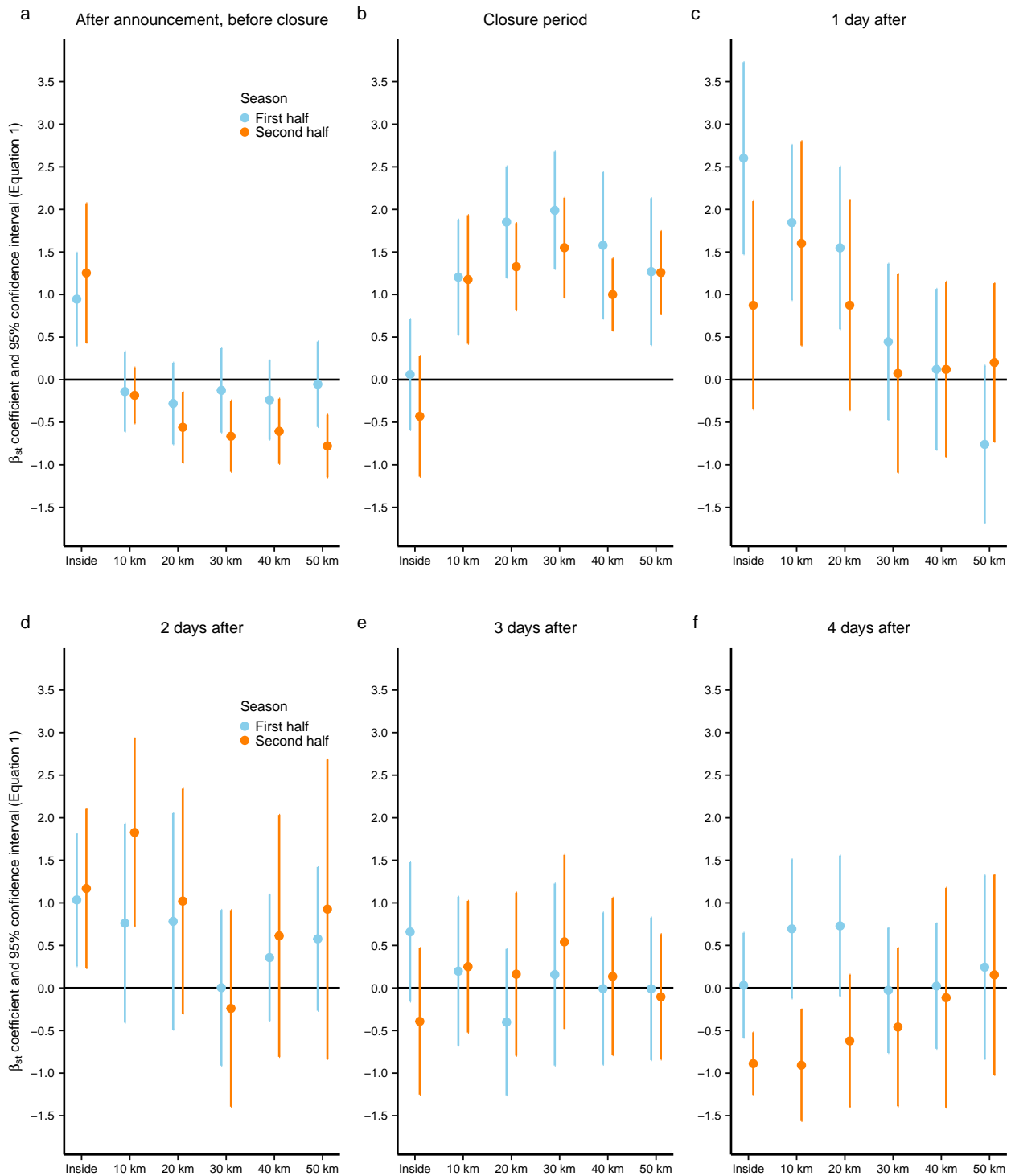
Figure A9: Treatment coefficients from re-estimating Equation 1 with potential closures 40% larger



Notes:  $N = 34,272$ . Points are coefficients and whiskers are 95% confidence intervals. Standard errors clustered at level of two-week-of-sample by two-degree grid cell.



Figure A10: Treatment coefficients from re-estimating Equation 1 with time-of-season interactions



Notes:  $N = 34,272$ . 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). Points are coefficients and whiskers are 95% confidence intervals. Standard errors clustered at level of two-week-of-sample by two-degree grid cell.

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

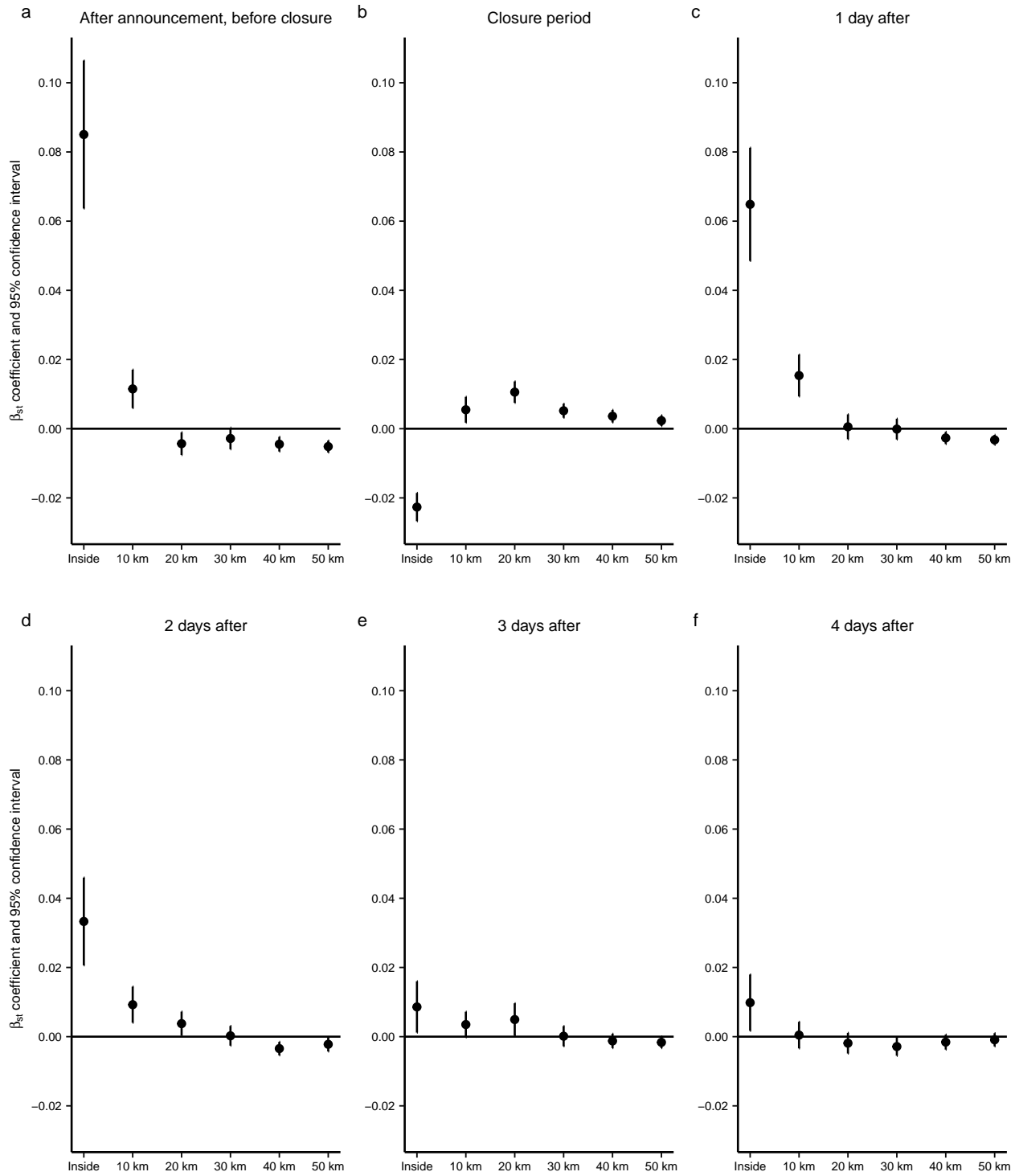
As an alternative estimation approach, I create a regular grid of  $.1^\circ$  cells covering the North-Central zone and calculate the millions of juveniles caught in each cell each three-hour period during a fishing season. I rasterize the data at this resolution to match the resolution of treatment assignment as closely as possible without exceeding my server’s memory capacity. This procedure yields 24,392,790 observations, or 5,457 grid cells  $\cdot$  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,  $.1^\circ$  grid cell fixed effects, three-hour time period fixed effects, and two-week-of-sample by two-degree grid cell fixed effects:

$$(4) \quad \text{JuvenileCatch}_{jk} = \beta_{st} \mathbb{1}\{jk \in st\} + \alpha_j + \delta_k + \sigma_{wg} + \epsilon_{jk}$$

where  $j = .1^\circ$  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,  $\beta_{st}$ , in Figure A11. The coefficient magnitudes are smaller than in Figure A2, possibly due to the large number of zeros in the rasterized data (99.9% 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 A2). My finding that closures cause temporal and spatial spillovers and increase total juvenile catch is robust to this alternative estimation strategy.

Figure A11: Treatment coefficients from estimating Equation 4



Notes:  $N = 24,392,790$ . Points are coefficients and whiskers are 95% confidence intervals. Standard errors clustered at level of two-week-of-sample by two-degree grid cell.

## 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. I only use actual closures declared by the regulator to calculate the treatment fraction for each potential closure-treatment bin. In this section I implement an alternative estimation approach, in which I use actual closures declared by the regulator as the treated units and potential closures whose treatment fraction equals 0 as the control units.

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.<sup>37</sup> 8 of the 410 actual closures declared by the regulator do not have sets inside them with non-missing length distribution values (see Footnote 44). I drop these 8 actual closures from this analysis because I cannot construct length distribution control variables for them. I create the same spatial and temporal leads and lags as for potential closures, yielding 14,472 observations ( $36 \text{ treatment bins} \cdot 402 = 14,472$ ). 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 39,599 observations: 14,472 treated observations and 25,127 control observations. Figure A12 displays the treatment coefficients. The treat-

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<sup>37</sup>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.

ment 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 36 billion juveniles, or 34% (delta method standard errors are 2.9 billion and 2.7%, 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 and Gardeazabal, 2003; Abadie, Diamond and Hainmueller, 2010). 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 116 out of 410 actual closures (Abadie, Diamond and Hainmueller, 2011).<sup>38</sup> However, I am able to obtain a synthetic control group for each of the remaining 294 actual closures.

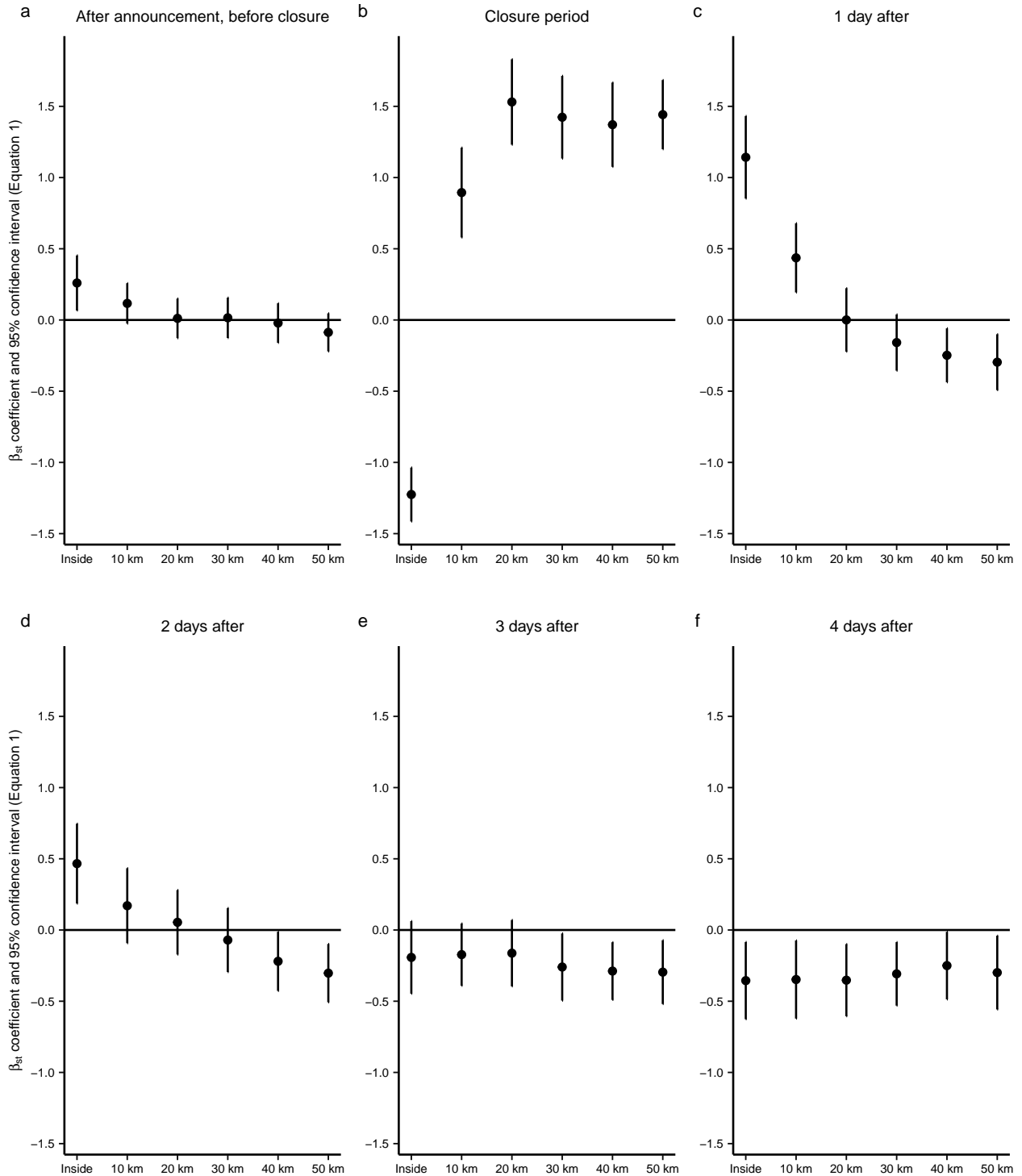
Figure A13 displays the synthetic control results. The y-axis is the average juvenile catch for treated observations (actual closures) minus the average juvenile catch for control observations (weighted average of potential closures). As in my preferred specification, juvenile catch is the inverse hyperbolic sine of millions of juveniles caught. I do not provide confidence intervals in Figure A13 because the synthetic control procedure of computing weights on potential closures for each actual closure is computationally intensive (the single run I performed took approximately 900 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 35 billion juveniles, or 34%.

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<sup>38</sup>8 of these error instances are due to the absence of length distribution control variables described above.

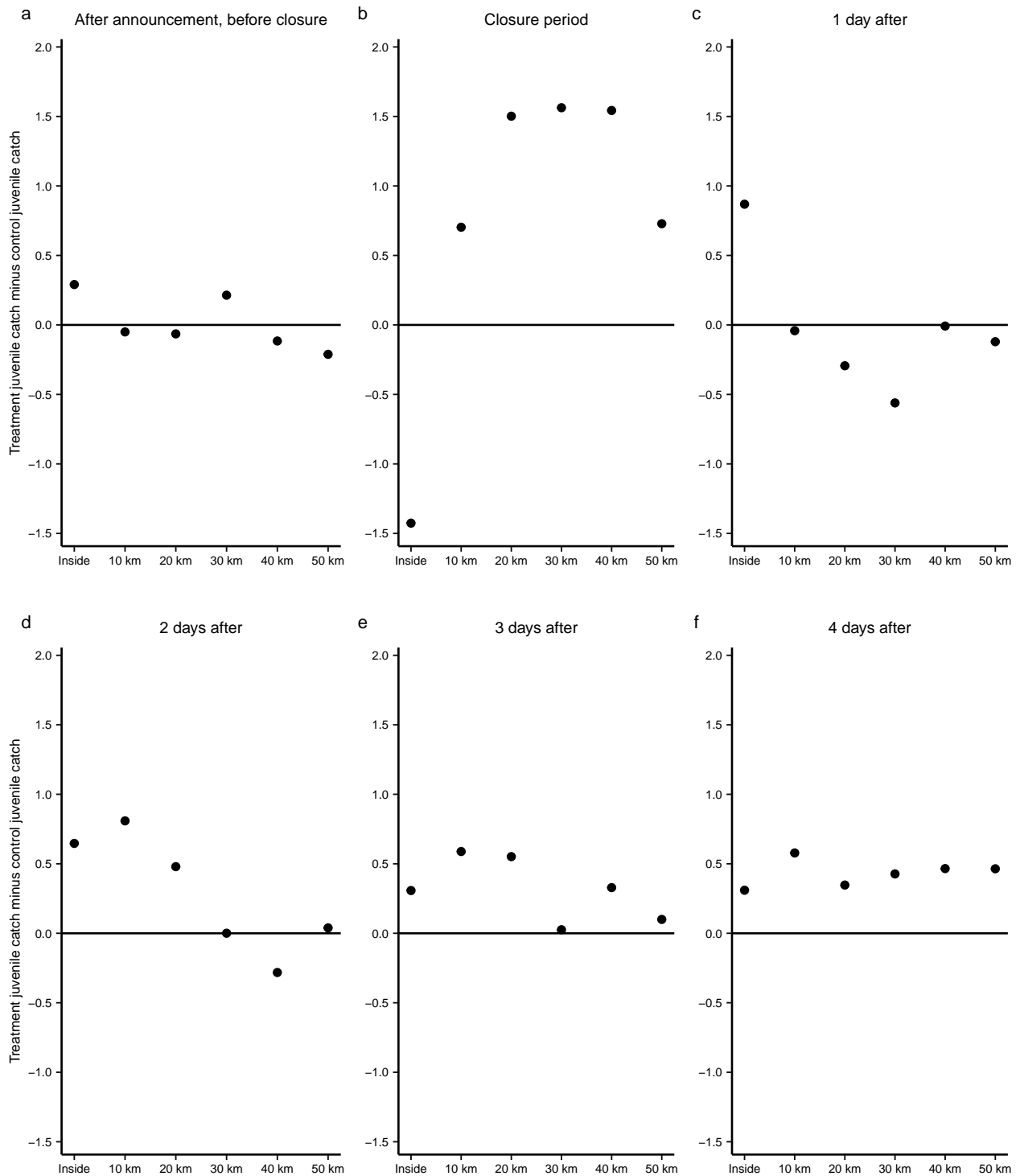
This result is similar to my preferred estimate of the effect of the temporary spatial closures policy—an increase in juvenile catch of 46 billion juveniles, or 48%—even though I obtained it with a different identification strategy.

Figure A12: 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.  $N = 39,599$ . Points are coefficients and whiskers are 95% confidence intervals. Standard errors clustered at level of two-week-of-sample by two-degree grid cell.

Figure A13: Synthetic control estimates of the effect of closures



Notes: Points are average juvenile catch for treated observations (actual closures) minus the average juvenile catch for control observations (weighted average of potential closures) in a given treatment bin, where juvenile catch is the inverse hyperbolic sine of millions of juveniles caught.



## B 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  $C$ .<sup>39</sup> 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 B1). Vessels choose whether to fish in  $h$  or  $k$  when  $C = 1$ .<sup>40</sup> Location  $h$  represents areas and times that are near closures, such as before, just outside, and after closures. 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  $C = 0$ ).

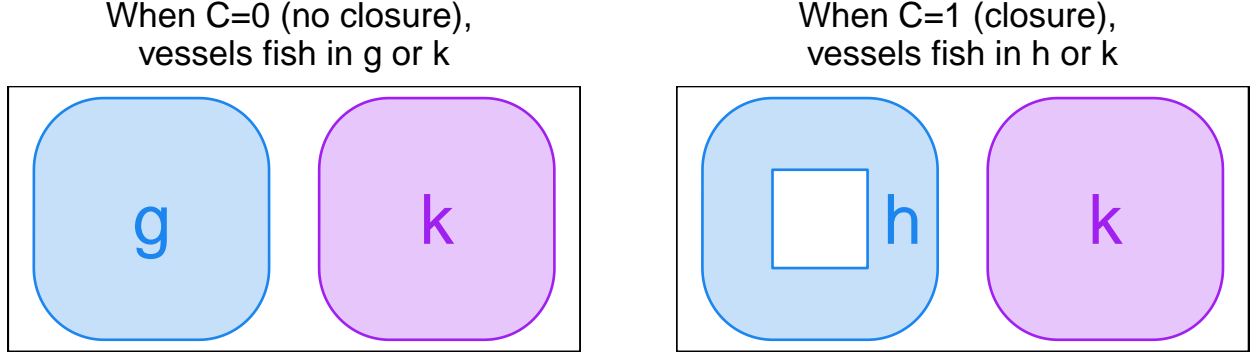
Let  $\ell$  denote a generic fishing location. Profit  $\pi$  decreases in the number of other vessels who make the same location choice,  $I_{-i,\ell}$ , due to congestion (Smith, 1969; Huang and Smith, 2014). Profit increases in the productivity (e.g., tons per set) of the fishing location, which I summarize with the scalar  $\mu_\ell$ . Vessels know that draws of  $\mu_\ell$  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  $\tilde{\mu}$  regarding mean productivity of each location (e.g., the value of  $\tilde{\mu}_k$  is the same across vessels).

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<sup>39</sup>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.

<sup>40</sup>Suppose the expected fine from fishing in the closed part of location  $g$  is sufficiently large such that expected profit from fishing in  $h$  or  $k$  is always greater.

Figure B1: 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  $k$ .

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

$$\max_{\ell \in \{g, k\}} E[\pi_{i,\ell}(\mu_\ell, I_{-i,\ell}) | \tilde{\mu}_g, \tilde{\mu}_k, C = 0].$$

Vessel  $i$  chooses to fish in  $g$  if the expected profit from doing so exceeds the expected profit from fishing in  $k$ ;  $E[\pi_{i,g}(\mu_g, I_{-i,g}) | \tilde{\mu}_g, \tilde{\mu}_k, C = 0] > E[\pi_{i,k}(\mu_k, I_{-i,k}) | \tilde{\mu}_g, \tilde{\mu}_k, C = 0]$ . 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  $I_\ell$  denote the number of vessels who choose location  $\ell$  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.<sup>41</sup> Then  $TotJuv(C = 0)$  equals

<sup>41</sup>In reality, I calculate the number of juvenile anchoveta caught by each set in Section II, which forms the main outcome variable of interest in my regressions (Section III).

$\gamma(I_g\mu_g\rho_g + I_k\mu_k\rho_k)$  and  $TotJuv(C = 1)$  equals  $\gamma(I_h\mu_h\rho_h + I_k\mu_k\rho_k)$ , where  $\gamma$  is a constant and  $\rho_\ell$  is percentage juvenile.

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 strong 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;  $TotJuv(C = 1) > TotJuv(C = 0)$ .

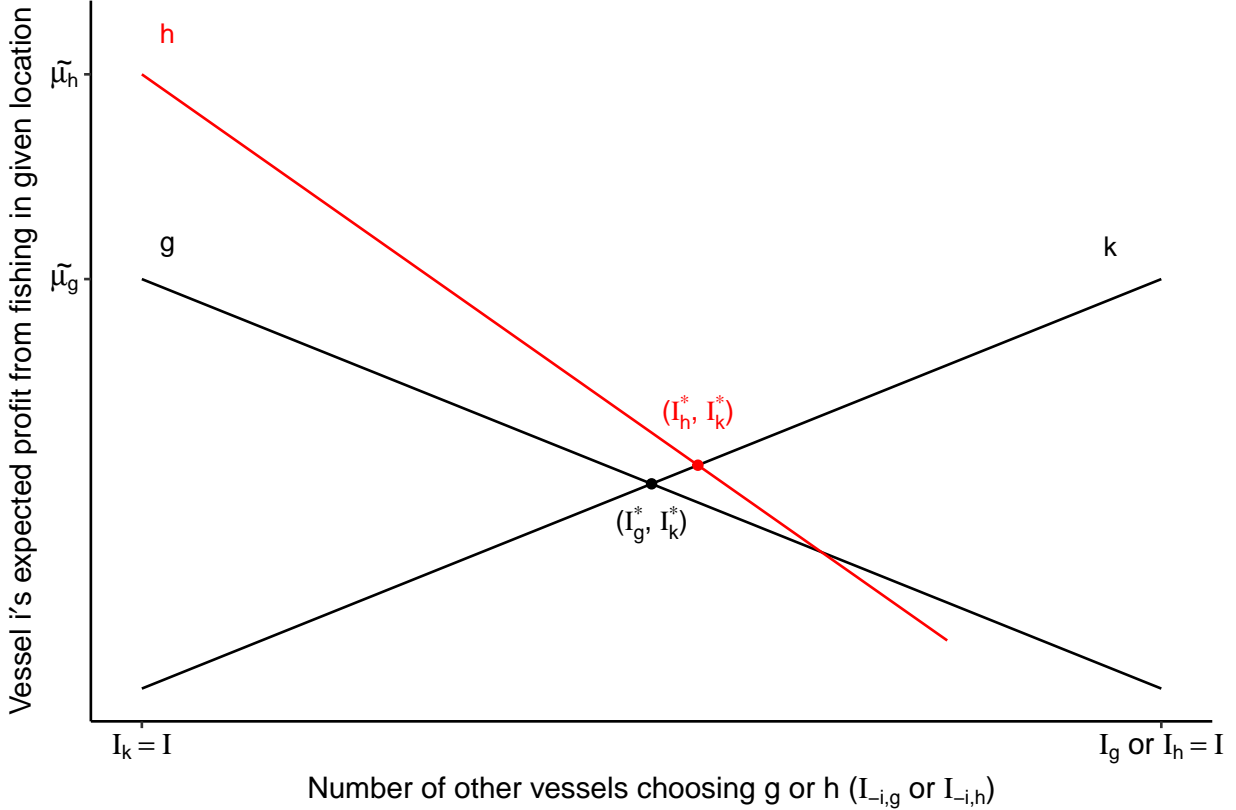
**Proposition 2a.** When there is no closure, the expected profit from fishing in location  $g$  equals the expected profit from fishing in location  $k$ ;  $E[\pi_{i,g}(\mu_g, I_{-i,g})|\tilde{\mu}_g, \tilde{\mu}_k, C = 0] = E[\pi_{i,k}(\mu_k, I_{-i,k})|\tilde{\mu}_g, \tilde{\mu}_k, C = 0] \forall i$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

**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 Section B.1. Figure B2 displays the equilibria when  $E[\pi_{i,\ell}(\mu_\ell, I_{-i,\ell})|\tilde{\mu}, C] = \tilde{\mu}_\ell - \alpha_\ell I_{-i,\ell}$ , where  $\alpha_\ell$  is the cost to vessel  $i$  from one additional vessel fishing in location  $\ell$ . 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 B2. Expected profit from fishing in  $h$  has a higher intercept than for  $g$ , because the closure announcement is a positive signal

Figure B2: Illustration of Propositions 1 and 2a



Notes: The y-axis is vessel  $i$ 's expected profit when  $E[\pi_{i,\ell}(\mu_\ell, I_{-i,\ell})|\tilde{\mu}, C] = \tilde{\mu}_\ell - \alpha_\ell I_{-i,\ell}$ . 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 strong positive signal (difference in intercepts) relative to congestion costs (difference in slopes of lines).

of productivity, but it also has a steeper slope, because marginal congestion costs are higher ( $\alpha_h > \alpha_g$ ). Figure B2 displays the case where the positive signal is sufficiently strong 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.<sup>42</sup> Whether the temporary spatial closures policy increases total juvenile catch is therefore an empirical question. This outcome is possible, but only if (1) closure announcements are a sufficiently strong 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 ( $\tilde{\mu}$ ), but not true productivity  $\mu$  because  $\mu$  is unobserved. Then  $\pi_{i,g}(\mu_g, I_{-i,g}) > \pi_{i,k}(\mu_k, I_{-i,k}) \quad \forall i$  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:  $\mu_{g,a} > \mu_{g,-a}$ . When  $C = 1$ , both types receive the positive signal from the closure announcement, so  $\mu_{h,a} = \mu_{h,-a}$ . The closures policy treatment effect  $\tau$  equals  $TotJuv(C = 1) - TotJuv(C = 0)$  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.

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<sup>42</sup>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 (Section IV).

**Proposition 3.** Consider two types of vessels, indicated by the subscript  $a$ . Suppose  $\mu_{g,a} > \mu_{g,-a}$  when  $C = 0$ ,  $\mu_{h,a} = \mu_{h,-a}$  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;  $\frac{\tau_{-a}}{I_{-a}} > \frac{\tau_a}{I_a}$ .

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.

I test Proposition 1 in Section IV, Proposition 2 in Table 4 (Section V), and Proposition 3 in Section VI.

## 1 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.  $E[\pi_{j,g}(\mu_g, I_{-j,g}) | \tilde{\mu}_g, \tilde{\mu}_k, C = 0] \neq E[\pi_{j,k}(\mu_k, I_{-j,k}) | \tilde{\mu}_g, \tilde{\mu}_k, C = 0]$ . Suppose without loss of generality that  $E[\pi_{j,g}(\mu_g, I_{-j,g}) | \tilde{\mu}_g, \tilde{\mu}_k, C = 0] > E[\pi_{j,k}(\mu_k, I_{-j,k}) | \tilde{\mu}_g, \tilde{\mu}_k, C = 0]$ . 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 ( $E[\pi_{i,g}(\mu_g, \hat{I}_{-i,g}^*) | \tilde{\mu}_g, \tilde{\mu}_k, C = 0] < E[\pi_{i,g}(\mu_g, I_{-i,g}^*) | \tilde{\mu}_g, \tilde{\mu}_k, C = 0] \forall i$ ). 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 ( $E[\pi_{i,k}(\mu_k, \hat{I}_{-i,k}^*) | \tilde{\mu}_g, \tilde{\mu}_k, C = 0] > E[\pi_{i,k}(\mu_k, I_{-i,k}^*) | \tilde{\mu}_g, \tilde{\mu}_k, C = 0] \forall i$ ). By the proof of Proposition 2a,  $E[\pi_{i,g}(\mu_g, I_{-i,g}^*) | \tilde{\mu}_g, \tilde{\mu}_k, C = 0] = E[\pi_{i,k}(\mu_k, I_{-i,k}^*) | \tilde{\mu}_g, \tilde{\mu}_k, C = 0] \forall i$ . Then  $(\hat{I}_g^*, \hat{I}_k^*)$  cannot be a Bayes-Nash equilibrium because  $E[\pi_{i,g}(\mu_g, \hat{I}_{-i,g}^*) | \tilde{\mu}_g, \tilde{\mu}_k, C = 0] < E[\pi_{i,k}(\mu_k, \hat{I}_{-i,k}^*) | \tilde{\mu}_g, \tilde{\mu}_k, C = 0] \forall i$ .  $(\hat{I}_h^*, \hat{I}_k^*)$  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  $I_\ell$  depend on  $\tilde{\mu}_\ell$ , but not true productivity  $\mu_\ell$  because  $\mu_\ell$  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 strong 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  $C = 0$  ( $\tilde{\mu}_h \gg \tilde{\mu}_k$  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  $E[i, g]$  denote  $E[\pi_{i,g}(\mu_g, I_{-i,g}) | \tilde{\mu}_g, \tilde{\mu}_k, C = 0]$ . Suppose the contradiction, that the number of vessels who choose location  $\ell$  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  $g$  ( $E[i, h^*] > E[i, g^*] \quad \forall i$ ), 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  $C = 1$  ( $E[i, k^*|C = 0] \geq E[i, k^*|C = 1] \quad \forall i$ ). Since  $(I_g^*, I_k^*)$  is a Bayes-Nash equilibrium,  $E[i, g^*] = E[i, k^*|C = 0]$  by the proof of Proposition 2a. Then  $E[i, h^*] > E[i, k^*|C = 1] \quad \forall i$  because  $E[i, h^*] > E[i, g^*] = E[i, k^*|C = 0] \geq E[i, k^*|C = 1]$ . Then  $(I_h^*, I_k^*)$  cannot be a Bayes-Nash equilibrium by the proof of Proposition 2a. Contradiction. Therefore,  $\frac{\partial I_\ell}{\partial \mu_\ell} > 0$  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  $h$  ( $E[i, h^*] < E[i, g^*] \quad \forall i$ ). 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  $C = 1$  ( $E[i, k^*|C = 0] \leq E[i, k^*|C = 1] \quad \forall i$ ). Since  $(I_g^*, I_k^*)$  is a Bayes-Nash equilibrium,  $E[i, g^*] = E[i, k^*|C = 0]$  by the proof of Proposition 2a. Then  $E[i, h^*] < E[i, k^*|C = 1] \quad \forall i$  because  $E[i, h^*] < E[i, g^*] = E[i, k^*|C = 0] \leq E[i, k^*|C = 1]$ . 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 strong 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  $\tau$  of the closures policy on total juvenile catch is

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

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 ( $I_h^* \mu_h \rho_h \gg I_g^* \mu_g \rho_g$ ), as well as sufficiently high relative to productivity and percentage juvenile in location  $k$ .

**Parametric example of Propositions 1 and 2a.** Figure B2 displays the Bayes-Nash equilibria when  $E[\pi_{i,\ell}(\mu_\ell, I_{-i,\ell}) | \tilde{\mu}, C] = \tilde{\mu}_\ell - \alpha_\ell I_{-i,\ell}$ , where  $\alpha_\ell$  is the cost to vessel  $i$  from one additional vessel fishing in location  $\ell$ . The equilibrium when  $C = 0$  results from setting  $\tilde{\mu}_g - \alpha_g I_{-i,g} = \tilde{\mu}_k - \alpha_k I_{-i,k}$ . The definition of the equilibrium when  $C = 1$  is similar. Recall that marginal congestion costs are only different for  $h$ ;  $\alpha_g = \alpha_k$  and let  $\alpha$  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 5 gives the change in total juvenile catch due to the policy.

**Proof of Proposition 3.** Since  $\mu_{g,a} > \mu_{g,-a}$  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  $C = 1$  ( $\mu_{h,a} = \mu_{h,-a}$ ), 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

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

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

## C Data Appendix

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

First, vessels 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 vessels in the electronic logbook data is unbiased, percentage juvenile and tons caught are not sufficient for calculating 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 C1b). 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.<sup>43</sup>

Recall from Section II that vessels report percentage juvenile to the regulator in the electronic logbook data, but not the length distribution. I obtained a supplementary electronic logbook dataset for a group of vessels that report length distribution data to their owners. These vessels represent 56% of landings and I received their data from Sociedad Nacional de Pesquería (SNP), a consortium of fishing companies, in January 2020 (Englander, 2022).

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

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<sup>43</sup>My results are robust to measuring juvenile catch in terms of tons of juveniles caught (Figure A6).

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, I first 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 (e.g., 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 I minimize the absolute difference between the implied percentage juvenile (percentage of individuals that are less than 12 cm, as implied by the updated length distribution) and the percentage juvenile reported to the regulator (i.e., a one unit shift of the length distribution 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 caught by SNP sets. 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.<sup>44</sup> My results are robust to imputing length distributions at the two-

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<sup>44</sup>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

week-of-sample by two-degree grid cell level; when I instead impute length distributions at the one-week-of-sample by one-degree grid cell level, I find that the closures policy increases juvenile catch by 49% (delta method standard error is 5.7%).<sup>45</sup> Figure C1a displays the resulting length distribution data for all sets. I calculate the number of individuals caught by non-SNP sets with each set's length distribution and tons caught.

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 set is: 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.<sup>46</sup>

Next, I match all sets to landing events in order to correct the length distribution, percentage juvenile, and number of individuals caught by all sets. Unlike vessels 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.

Vessels 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

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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.

<sup>45</sup>Whenever I create grid cells (of any resolution), I create a grid over Peru's Exclusive Economic Zone. Peru's Exclusive Economic Zone boundaries are from Flanders Marine Institute (2012).

<sup>46</sup>Recall from Section II that vessels do not underreport tons caught.

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 sets I was unable to match to landing events (6.9%), or for which the percentage juvenile is missing for the landing event (6.1%), I multiply percentage juvenile by the ratio of average landing-level percentage juvenile to average set-level percentage juvenile, where I calculate averages among matched sets in the same two-week-of-sample by two-degree grid cell and weight them by the number of individuals caught. Missingness of percentage juvenile measured at landing, and the procedure by which I adjust percentage juvenile for these sets, is unlikely to bias my results because this missingness is uncorrelated with treatment.<sup>47</sup>

For example, suppose there are two sets on a trip, the first set caught 1 million individuals of which the vessel reports 10% are juvenile, the second set caught 4 million individuals of which the vessel reports 5% are juvenile, and the third-party inspector measures 12% juvenile 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.

Figure C1c displays the percentage juvenile values reported to the regulator and Figure C1d displays the corrected percentage juvenile values. Since vessels tend to underreport percentage juvenile to the regulator relative to percentage juvenile measured at landing, as well as report round numbers, the distribution of corrected percentage juvenile values

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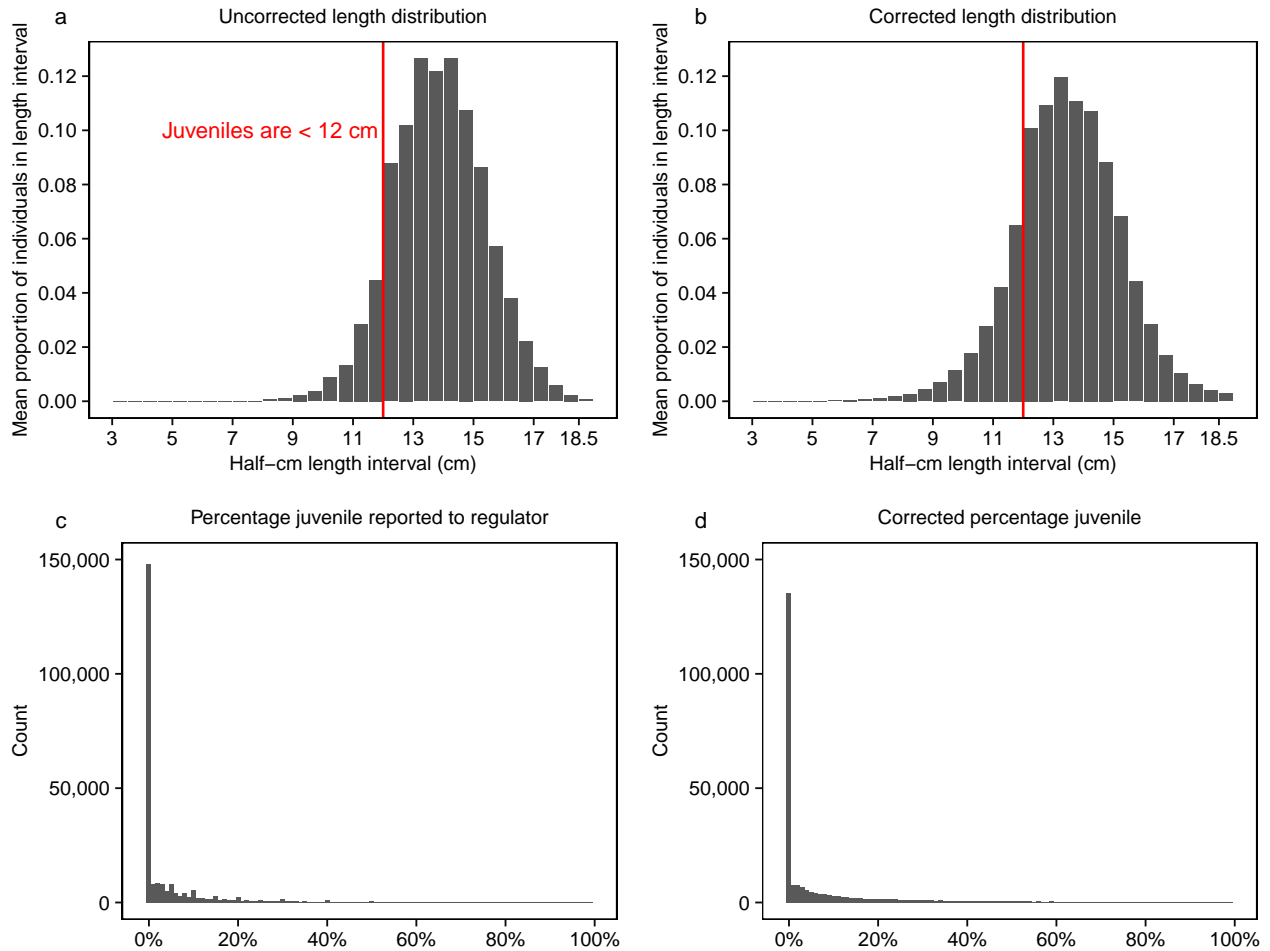
<sup>47</sup>I re-estimate Equation 1 with a single treatment fraction variable (rather than one for each treatment bin) and a new outcome variable: an indicator that equals 1 if any sets inside the potential-closure treatment bin were not matched to a landing event. I choose an indicator variable rather than the share of sets that meet this condition because the share is undefined when there are no sets inside the potential closure-treatment bin. The mean of the dependent variable in this regression is 0.308. The “effect” of closures is near-zero and is statistically insignificant; the treatment fraction coefficient is -0.009 and the standard error is 0.023.

is smoother and shifted to the right compared to the distribution of reported percentage juvenile values.

This procedure assumes that misreporting of percentage juvenile is constant within trip. This assumption would be violated if vessels differentially underreport percentage juvenile in the most desirable fishing grounds, which could occur if vessels want to prevent the closure of productive fishing grounds they have discovered. I support this assumption by matching at the set-level the percentage juvenile reported to vessel owners with the percentage juvenile reported to the regulator. I perform this matching for SNP sets only since there is no percentage juvenile reported to vessel owners for non-SNP sets. I calculate the percentage juvenile reported to the vessel's owner minus the percentage juvenile reported to the regulator, and regress that difference on tons reported to the vessel owner and trip fixed effects. A positive coefficient on tons implies that vessels underreport percentage juvenile by a greater amount to the regulator when they have a more productive set, relative to other sets on the same trip. The coefficient on tons is in fact slightly negative (-0.0002) and not different from zero, supporting my assumption of constant within-trip misreporting.

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. Figure C1b displays the corrected length distribution data for all sets. 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.

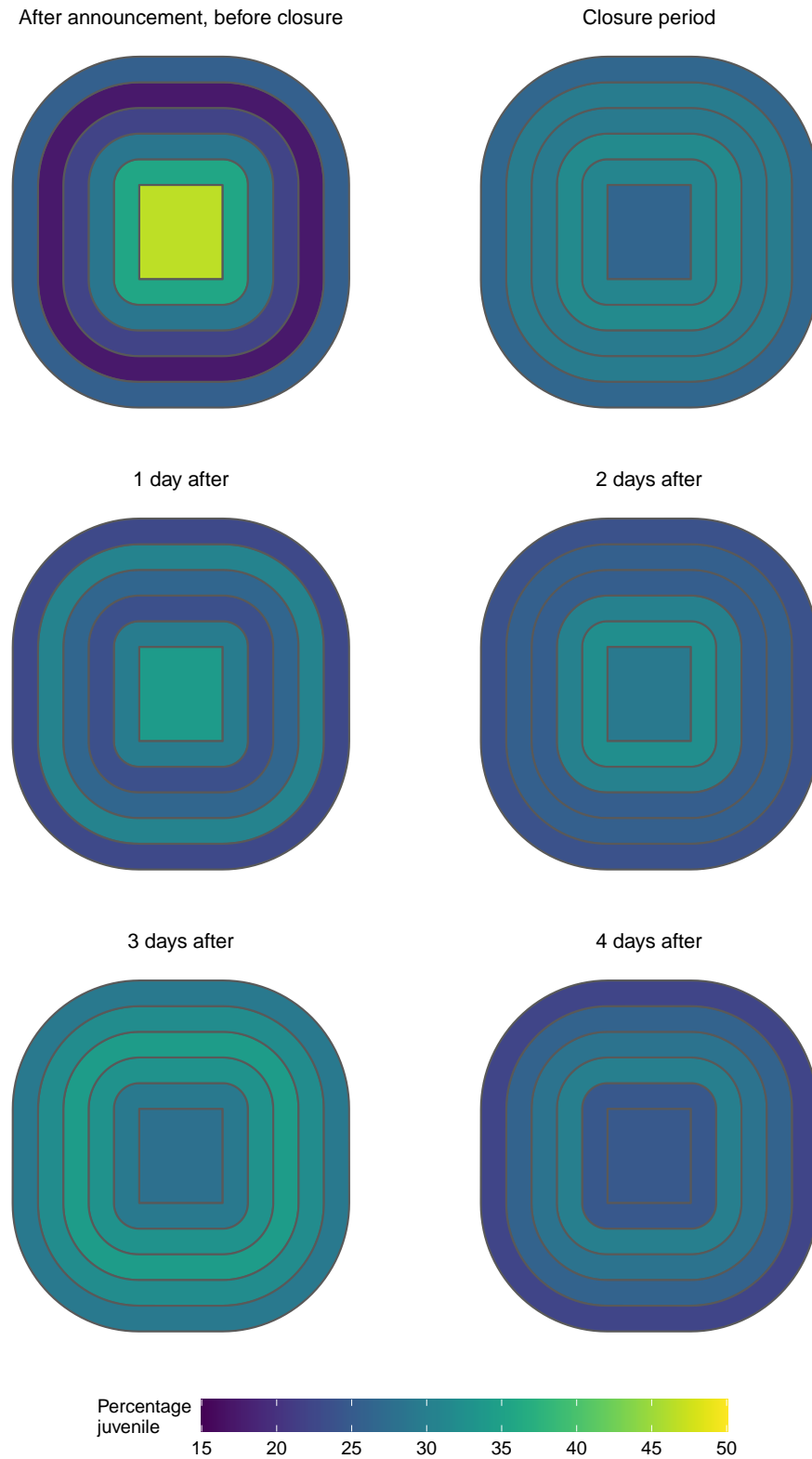
Figure C1: Length distributions and percentage juveniles values of all anchoveta caught in the North-Central zone, 2017 to 2019 fishing seasons



Notes: The y-axes in (a) and (b) indicate the average proportion of anchoveta caught in each half-cm length interval, weighted by the number of individuals caught by each set.



Figure C2: Individuals-weighted average percentage juvenile in each treatment bin, for actual closures declared by the regulator



Notes: Percentage juvenile values are from the corrected electronic logbook data. Average percentage juvenile outside the treatment window is 9%.

## D Additional treatment effect heterogeneity results

### 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<sup>2</sup>, 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 vessels from catching them. With respect to the length of the closure period, the regulator intends that closures reduce juvenile catch by encouraging vessels to find new places to fish (Section I). 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:

$$(6) \quad \begin{aligned} JuvenileCatch_{ist} = & \alpha_{sth} + \beta_{sth}TreatFraction_{isth} + \sum_{\ell=[3,3.5]}^{[18.5,19]} \xi_{\ell}Prop_{i\ell} + \\ & \gamma_1Sets_i + \gamma_2Tons_i + \gamma_3Area_i + \gamma_4DistToCoast_i + \\ & \gamma_5TonsPerSet_i + \gamma_6TonsPerArea_i + \sigma_{wg} + \delta_d + \epsilon_{isth} \end{aligned}$$

where  $h$  indicates heterogeneity category and all other variables and subscripts have the same definitions as in 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. 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 7 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 5-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 IV.

Larger closures do not perform better than smaller closures. Above-median-size closures increase juvenile catch by 49%, while below-median-size closures increase juvenile catch by 32% (p-value on this difference is 0.18). Even though the difference is not statistically significant, some readers may be surprised that larger closures have a larger negative point estimate than smaller closures. Two reasons larger closures could cause larger increases in juvenile catch are if congestion costs are lower or if larger closures are a stronger positive signal of fishing productivity. While I do not observe vessels' productivity beliefs, I can test whether the sets that generate larger actual closures are more productive than the sets that generate smaller actual closures. Tons per set is 16% higher for sets that generate above-median size closures, which suggests that fishing near larger closures is more productive.

There does not appear to be treatment effect heterogeneity by length of closure. 3-day closures increase juvenile catch by 45%, while 5-day closures increase juvenile catch by 60%

(p-value on this difference is .55). Within the support of the data, it does not seem that making closures larger or longer improves the performance of the policy.

## 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 with authorization 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 D1).

Seven large firms own at least 19 vessels each, which together account for 60.3% of landings. All seven large firms are vertically integrated in that they also own fishmeal processing plants (Hansman et al., 2020). 271 vessels are “singletons”: they belong to a firm that owns only one vessel. Singleton vessels account for 12.9% 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, Matthiasson and Giry, 2016).

I test for treatment effect heterogeneity by re-estimating Equation 3 from Section VI, 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 IV.

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 61% for the vessels that belong to the seven largest firms. The increase in total juvenile catch is 43% for vessels that belong to medium-sized firms, and it is 9% for singleton vessels.

Table D1: Vessel characteristics in the six fishing seasons of 2017, 2018, and 2019

|  | All vessels<br>(1) | Large-firm vessels<br>(2) | Medium-firm vessels<br>(3) | Singleton vessels<br>(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  | 22261.64           | 22261.64                  | 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                    |

Notes: 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 are for the North-Central zone only. Landings data are used to calculate the number of active vessels each season. Landings and vessel length data are from PRODUCE (Englander, 2022).

Large-firm vessels account for 78% of the closures policy treatment effect, which is greater than their share of total juvenile catch in the fishery (70%). Large-firm vessels also catch a higher fraction of juveniles and underreport percentage juvenile by a greater amount than medium-firm or singleton vessels (Table D2).

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 59% for above-median vessels, compared to 23% 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

Table D2: Large-firm vessels underreport percentage juvenile more

| Vessel type         | Reported % juvenile | Corrected % juvenile | $\Delta$ % juvenile |
|---------------------|---------------------|----------------------|---------------------|
| Large-firm vessels  | 11.6                | 20.4                 | -8.8                |
| Medium-firm vessels | 11.1                | 16.6                 | -5.5                |
| Singleton vessels   | 7.6                 | 12.4                 | -4.8                |

Notes: All values are weighted averages of North-Central zone data for all vessels between 2017 and 2019, where the weights are the individuals caught by each set. Reported % juvenile is the percentage juvenile that vessels report to the regulator after each set. I describe how I calculate corrected percentage juvenile values in Section 3 and Appendix C.  $\Delta$  % juvenile is the reported % juvenile minus the corrected % juvenile. 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.

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 75% are below-median length and 25% 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 that the 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. Larger firms may be more able to aggregate information from closures and dispatch vessels accordingly.

## E Age-structured fishery simulation

I construct an age-structured model of the Peruvian anchoveta fishery in order to illustrate the long-term effects of the closures policy on catch and biomass. In the simulation, I allow anchoveta to grow, reproduce, die from natural causes, and die due to fishing at rates that depend on length (or equivalently, age). The time step is one day. I simulate the population forward 20 years under two scenarios: status quo harvest and counterfactual harvest. Both scenarios have the same harvest rule in tons: at the same level of biomass, tons caught in both scenarios is the same. The only difference between the two scenarios is the proportion of juveniles caught in the status quo scenario matches what I observe in the corrected electronic logbook data, while the proportion of juveniles caught in the counterfactual scenario equals the proportion that would have been caught in the absence of the closures policy.

Each day  $t$ , population cohort  $c$  changes according to the following four equations. Cohorts contain individuals of the same age, length, and weight.  $\ell$  denotes length in cm of an individual in cohort  $c$ ,  $w$  denotes weight in grams of an individual in cohort  $c$ ,  $a$  denotes age in days,  $N$  denotes number of individuals, *Recruitment* denotes the number of individuals entering the fishery at the smallest length class,  $\alpha$  is a constant,  $B$  denotes biomass (the anchoveta population measured in tons),  $H$  denotes harvest (catch),  $\bar{t}$  denotes the day the fishing season starts, and  $S$  denotes the scenario (status quo or counterfactual).

$$(7) \quad \begin{aligned} \ell_{c,t+1} &= 19.35(1 - e^{-.96(a_{c,t} + \frac{1}{365} + .0193)}) \\ w_{c,t+1} &= .0036(\ell_{c,t+1})^{3.238} \end{aligned}$$

$$(8) \quad \text{Recruitment}_{t+1} = \alpha \cdot (.4 \sum_{\ell \geq 12}^{\ell < 14} N_{\ell,t} + .6 \sum_{\ell \geq 14} N_{\ell,t})$$

$$(9) \quad N_{c,t+1} = N_{c,t} e^{\frac{-8}{365}}$$

$$(10) \quad B_{c,t+1} = w_{c,t+1} N_{c,t+1} - H_{c,t+1} \left( \sum_{\ell \geq 12} B_{c,\bar{t}}, S, \ell_{c,t+1}, B_{c,t} \right)$$

First, individuals grow in length and weight according to growth equations from IMARPE (2019) and Salvattecchi and Mendo (2005). Second, “recruitment occurs”: a new cohort of individuals enter the fishery. Recruitment refers to the size at which fish are large enough to be caught. Perea et al. (2011) estimate that anchoveta between 12 and 14 cm produce 40% of eggs and anchoveta above 14 cm produce 60% of eggs. I choose the constant parameter  $\alpha$  such that biomass after 20 years in the status quo scenario equals biomass at the start of the simulation. Figure 2d of Szuwalski et al. (2015) displays an approximately linear relationship between adult biomass and recruitment. Third, individuals experience natural mortality according to the survival equation from Salvattecchi and Mendo (2005). Finally, cohorts die from fishing according to the harvest rule  $H(\cdot)$ .

There are two fishing seasons per year. Each fishing season lasts for 91 days. Harvest is 0 on days outside the fishing season. Harvest depends on  $\sum_{\ell \geq 12} B_{c,\bar{t}}$ , adult biomass ( $\ell \geq 12$ ) at the start of each fishing season ( $\bar{t}$ ). If adult biomass is below 4 million tons, no harvest occurs that season in order to allow the population to recover. If adult biomass exceeds 4 million tons at the start of the fishing season, then total harvest over the course of the season equals 25% of total biomass (juvenile and adult) at the start of the season.

This harvest rule approximates the relationship between the regulator’s choice of total allowable catch and biomass. In reality, 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 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). On average the TAC



is about 25% of biomass at the start of the fishing season (Kroetz et al., 2016).

During fishing seasons in which adult biomass exceeds 4 million tons at the start of the fishing season, harvest of cohort  $c$  is

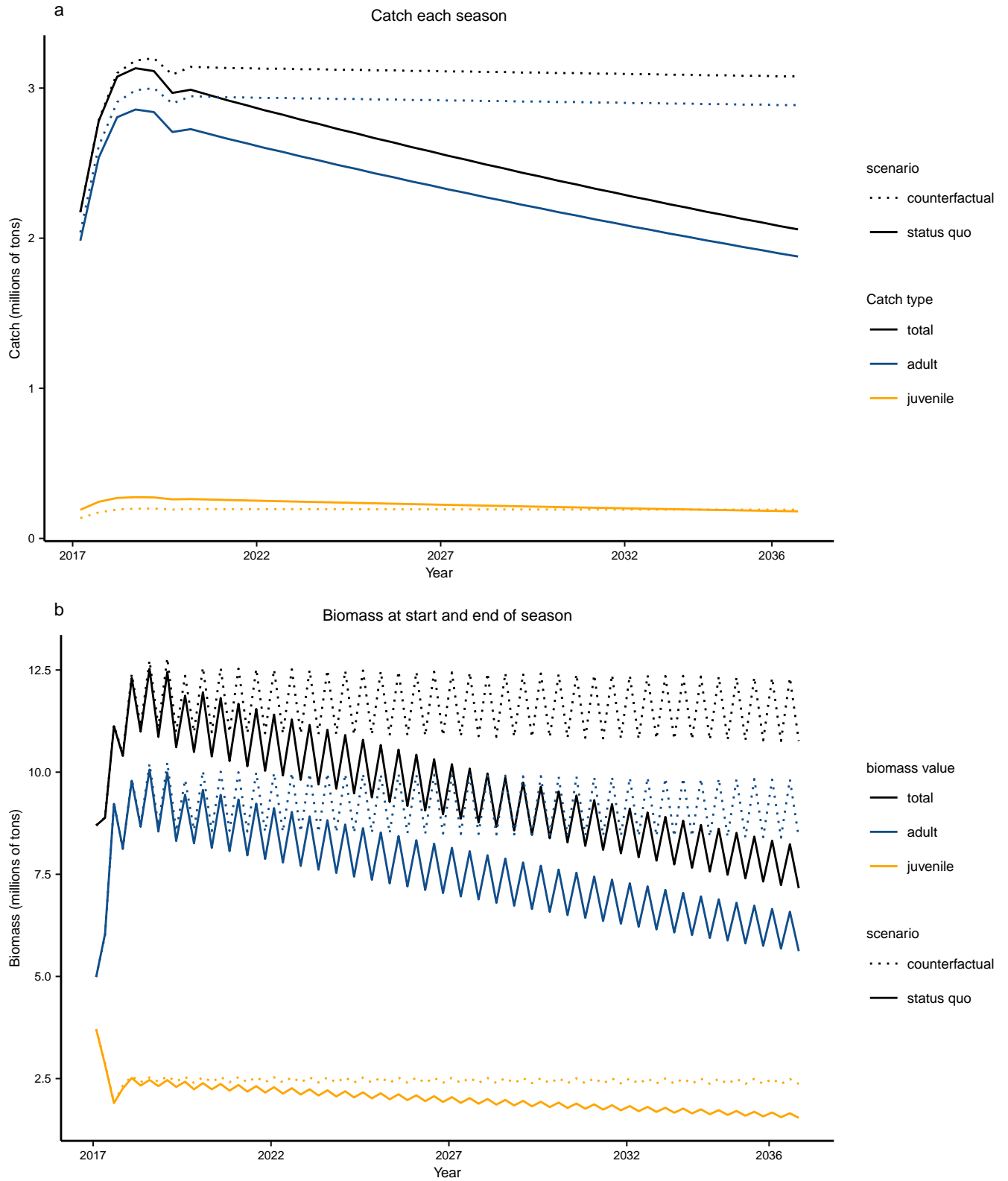
$$(11) \quad H_{c,t+1} = \begin{cases} \frac{\sum B_{c,\bar{\ell}}}{4 \cdot 91} \cdot 0.0878 \cdot \frac{B_{c,t}}{\sum_{\ell < 12} B_{c,t}}, & \text{if } S = \text{status quo and } \ell_{c,t+1} < 12 \\ \frac{\sum B_{c,\bar{\ell}}}{4 \cdot 91} \cdot (1 - 0.0878) \cdot \frac{B_{c,t}}{\sum_{\ell \geq 12} B_{c,t}}, & \text{if } S = \text{status quo and } \ell_{c,t+1} \geq 12 \\ \frac{\sum B_{c,\bar{\ell}}}{4 \cdot 91} \cdot 0.0623 \cdot \frac{B_{c,t}}{\sum_{\ell < 12} B_{c,t}}, & \text{if } S = \text{counterfactual and } \ell_{c,t+1} < 12 \\ \frac{\sum B_{c,\bar{\ell}}}{4 \cdot 91} \cdot (1 - 0.0623) \cdot \frac{B_{c,t}}{\sum_{\ell \geq 12} B_{c,t}}, & \text{if } S = \text{counterfactual and } \ell_{c,t+1} \geq 12 \end{cases}$$

Harvest of cohort  $c$  on day  $t + 1$  depends on three terms, which may vary by scenario  $S$  and whether individuals in cohort  $c$  are juveniles ( $\ell_{c,t+1} < 12$ ) or adults ( $\ell_{c,t+1} \geq 12$ ). The first term is total biomass at the start of the season divided by 4 times 91 (25% of initial biomass divided by the number of days of the fishing season). The second term is the fraction of harvest by weight that juveniles or adults comprise. Between 2017 and 2019, juveniles represent 8.78% of tons caught. Therefore, in the status quo scenario juveniles comprise 8.78% of tons caught and adults comprise 91.22% of tons caught. I estimate that the closures policy increases tons of juveniles caught by 41% (Figure A6). In the counterfactual scenario without the closures policy, juveniles therefore comprise 6.23% of tons caught and adults comprise 93.77% of tons caught ( $6.23\% = 8.78\% / 1.41$ ).

I obtain initial values for the population at each length interval by digitizing IMARPE's estimate of the population size distribution in March 2017, one month before the beginning of my study period (IMARPE, 2017). I divide the population into cohorts of .01 cm length intervals. I then simulate the population forward 20 years under the two scenarios. A new cohort of 7 cm-long individuals enters the fishery each day according to Equation 8. I record harvest each season and biomass at the start and end of each season.

Figure E1 displays the simulation results. Though juveniles comprise a greater fraction of harvest in the status quo scenario, total harvest and biomass are equivalent through the

Figure E1: Catch and biomass with and without closures policy



Notes: There are two fishing seasons per year. (a) displays total catch each season and (b) displays biomass at the start and end of each season.

first year of the simulation. After one year, the effects of greater juvenile harvest begin to emerge. Fewer juveniles become adults, and fewer adults means lower reproduction and recruitment. As fewer individuals enter the population, biomass shrinks until it returns to its initial level after 20 years. If I ran the scenario for additional years, status quo biomass would continue to decline.

By contrast, lower harvest of juveniles in the counterfactual scenario allows more juveniles to grow into adults who can reproduce. The counterfactual biomass is consequently stable rather than declining over time. The greater counterfactual biomass allows greater harvest as well. In contrast, status quo harvest declines with biomass.

Total harvest over the 20 year simulation is 17% lower in the status quo than in the counterfactual. With a 5% discount rate, total harvest is 14.4% lower. FOB export revenues were \$1.79 billion in 2017 (PRODUCE, 2018*a*). This simulation therefore implies that the cost of the closures policy in terms of foregone export revenue over a 20-year time horizon is \$5.2 billion (2017 USD), or \$258 million per year.

This simulation only approximates the Peruvian anchoveta fishery. For example, it is deterministic, when in fact there is a high degree of stochasticity in the fishery, due in part to the effects of the El Niño-Southern Oscillation (Castillo et al., 2019). It does not model other species that interact with anchoveta, such as the plankton that anchoveta eat or the non-human animals that eat anchoveta (Bertrand et al., 2012). It assumes that growth, recruitment, natural mortality, and the harvest rule, which includes the closures policy, are correctly specified. Nonetheless, it is sufficient to illustrate biologists' claim that excessive juvenile catch damages the population (Smith, 1994), and that the long-term costs of the closures policy are large.

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