Online Appendix

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

Gabriel Englander

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A Robustness checks

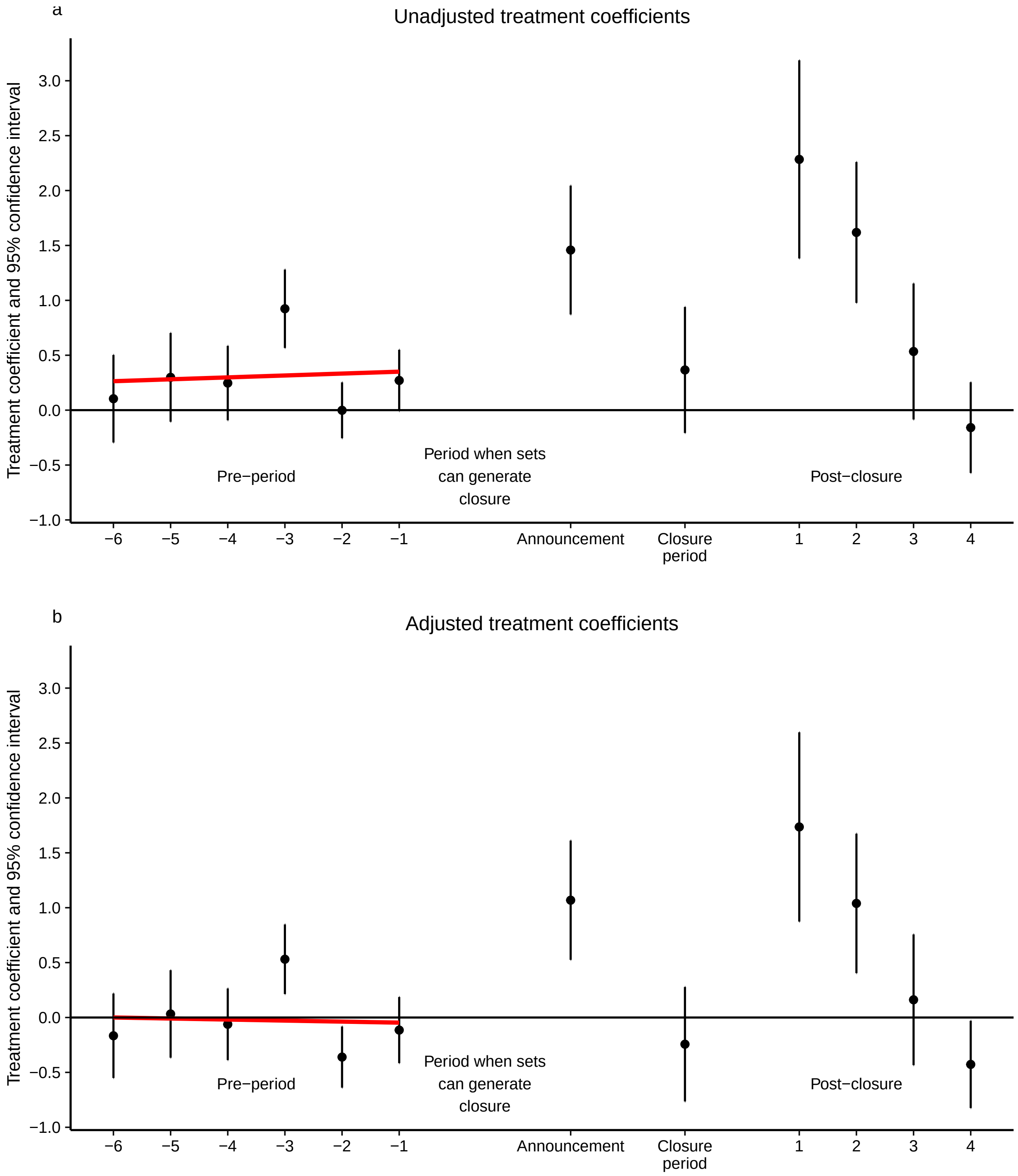
1 Additional balance tests

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 (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			0.0026
	(0.0017)			(0.0015)
TonsPerSet		0.0075		0.0069
		(0.0007)		(0.0009)
TonsPerArea			0.0138	0.0027
			(0.0036)	(0.0032)
Intercept	0.6375	0.3826	0.6975	0.3169
	(0.0660)	(0.0524)	(0.0515)	(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.[36] When I convert these treatment coefficients into changes in levels and account for the reallocation in tons (of juveniles and adults) caught

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

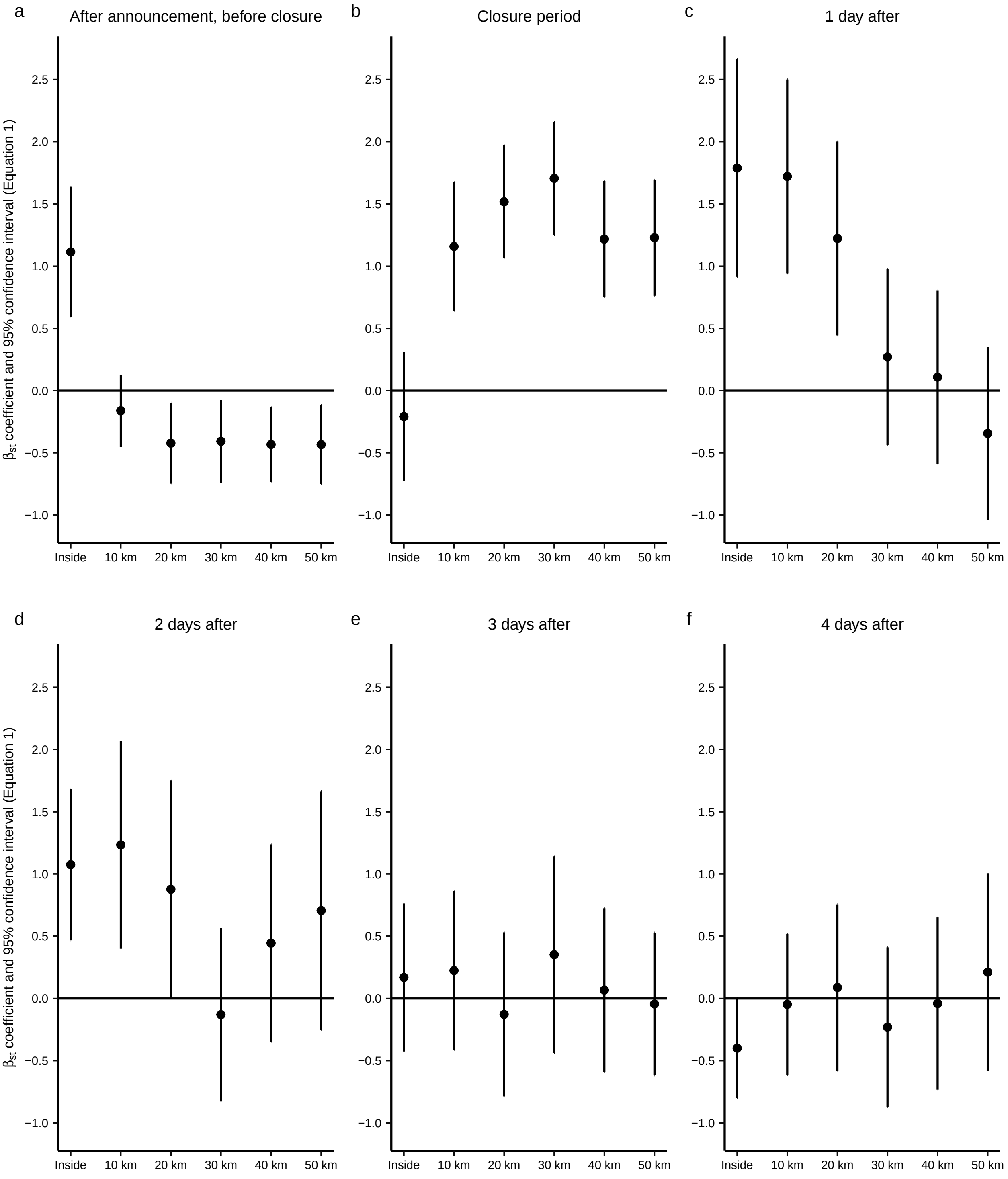
due to the total allowable catch limit, I estimate that the temporary spatial closures policy increases juvenile catch by 315 thousand tons of juveniles, or 41% (delta method standard errors are 75 thousand tons and 9.7%). For comparison, the regulator calculates that the temporary spatial closures policy "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.

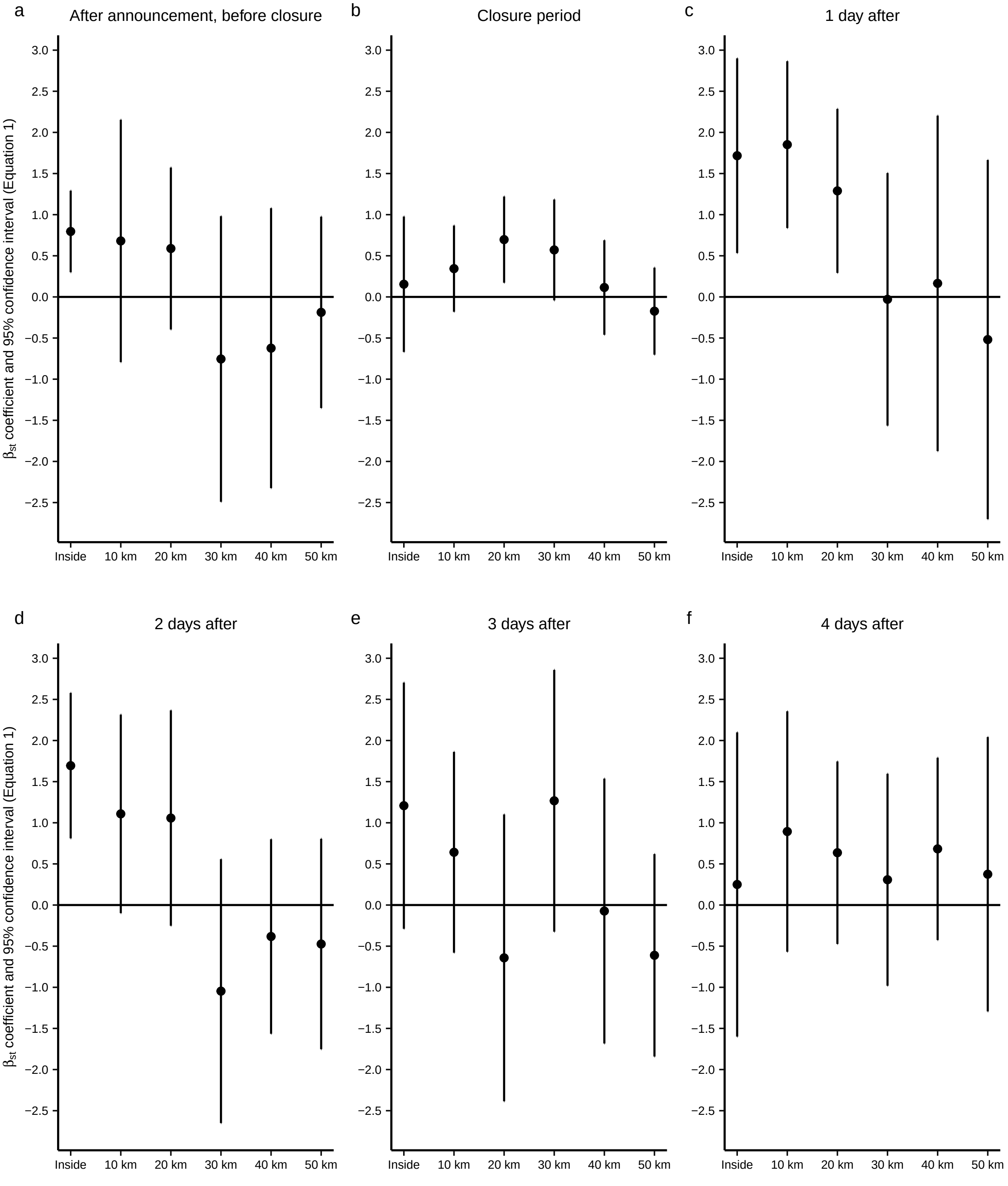
If closures shift juvenile catch forward in time during a fishing season, then my treatment effects would be upward biased because some of the increase in juvenile catch due to closures would have occurred later in the season, even if the closures policy did not exist. This "harvesting" concern also 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



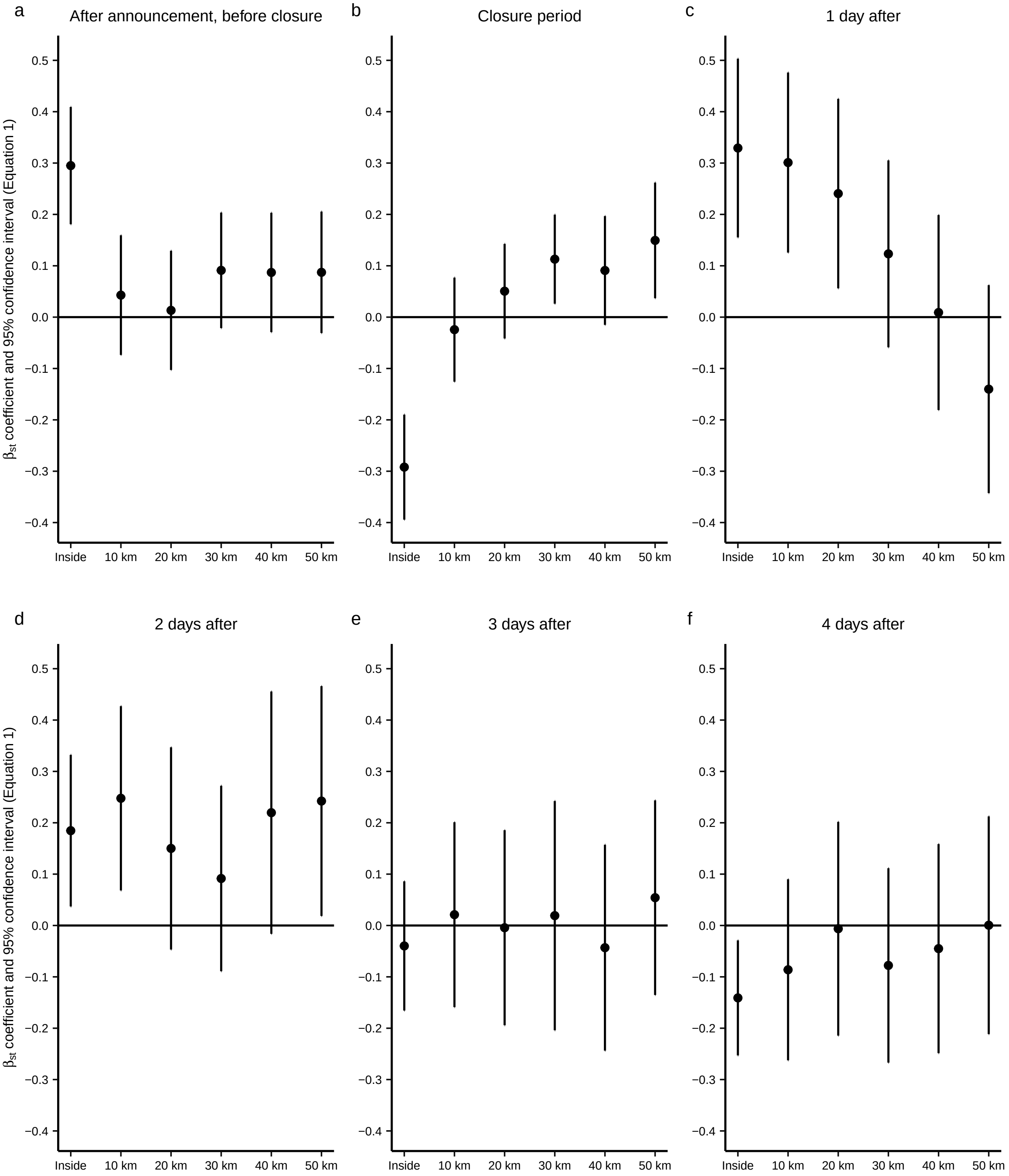
Notes: N = 34,272. Points are coefficients and whiskers are 95% confidence intervals. Standard errors clustered at level of two-week-of-sample by two-degree grid cell.

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



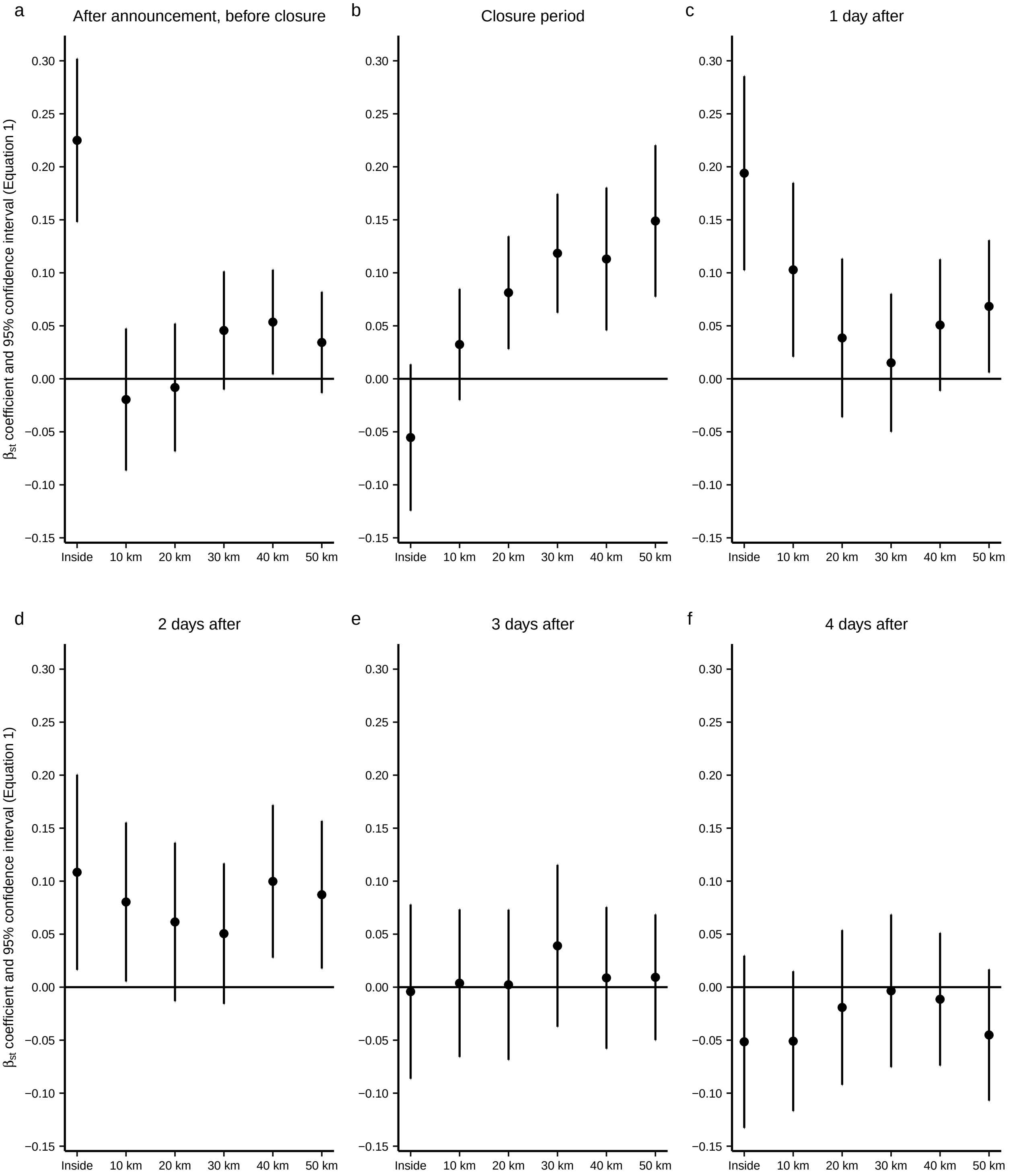
Notes: N = 12,181. Points are coefficients and whiskers are 95% confidence intervals. Standard errors clustered at level of two-week-of-sample by two-degree grid cell.

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



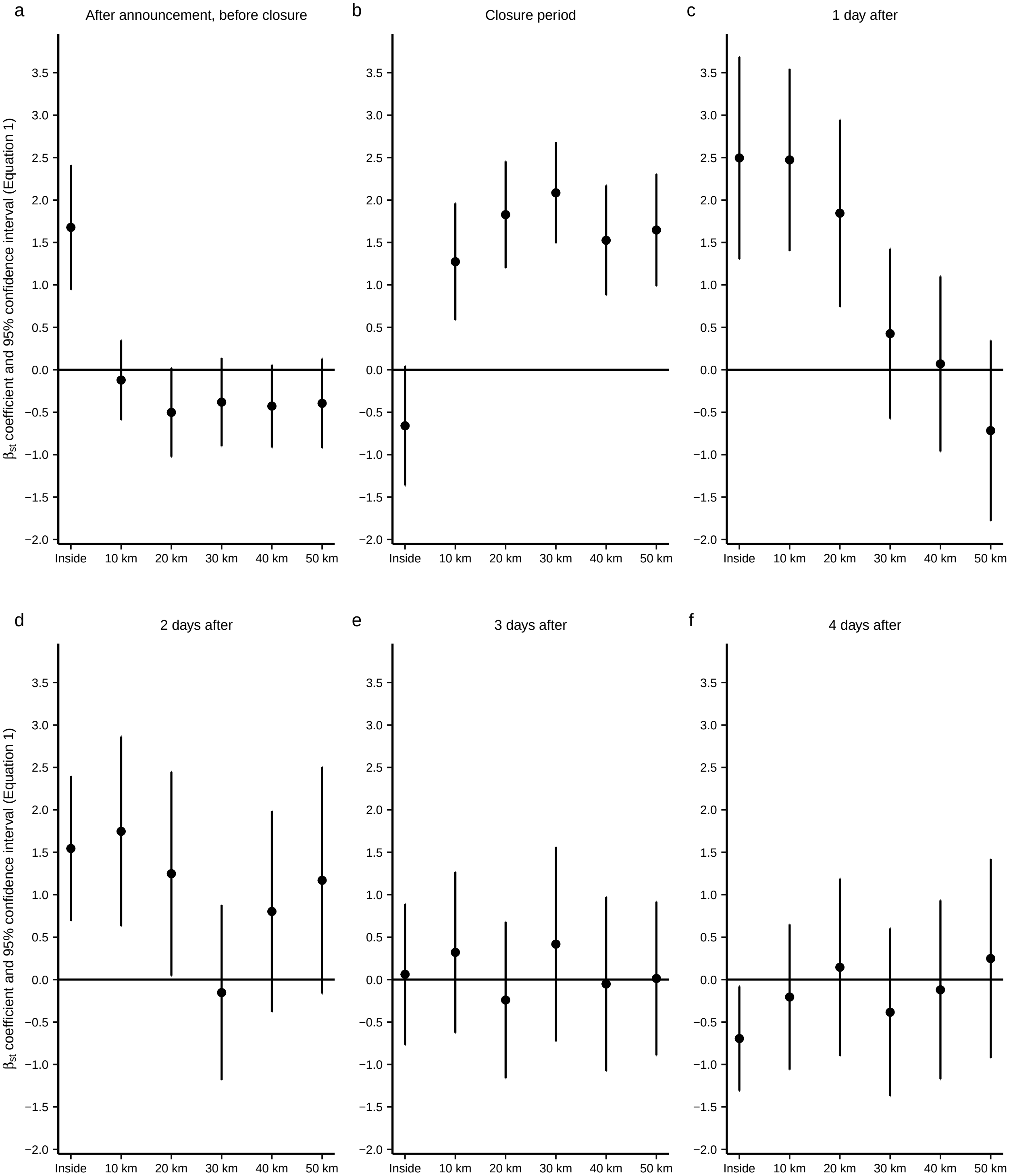
Notes: N = 34,272. Points are coefficients and whiskers are 95% confidence intervals. Standard errors clustered at level of two-week-of-sample by two-degree grid cell.

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



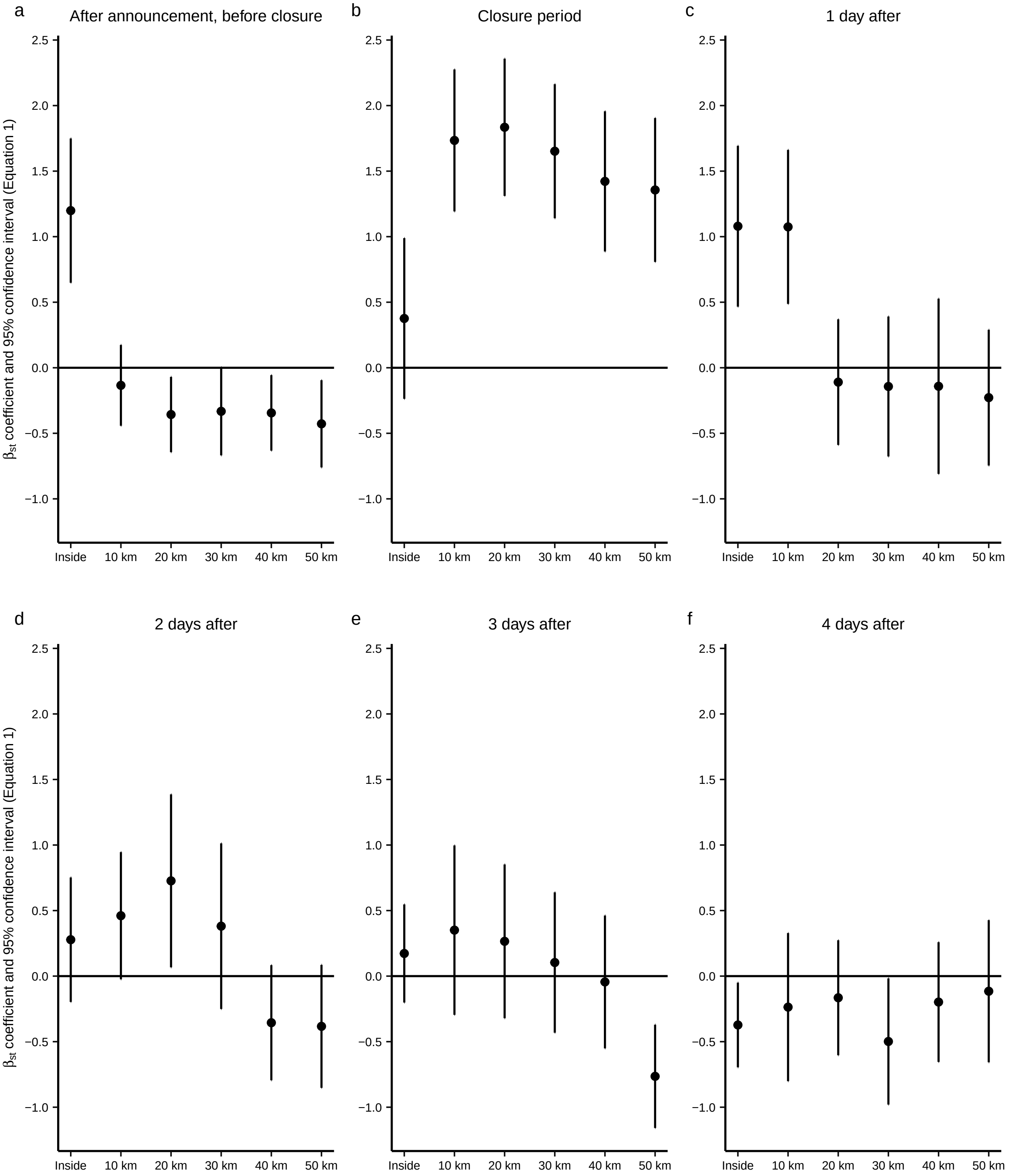
Notes: N = 34,272. Points are coefficients and whiskers are 95% confidence intervals. Standard errors clustered at level of two-week-of-sample by two-degree grid cell.

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



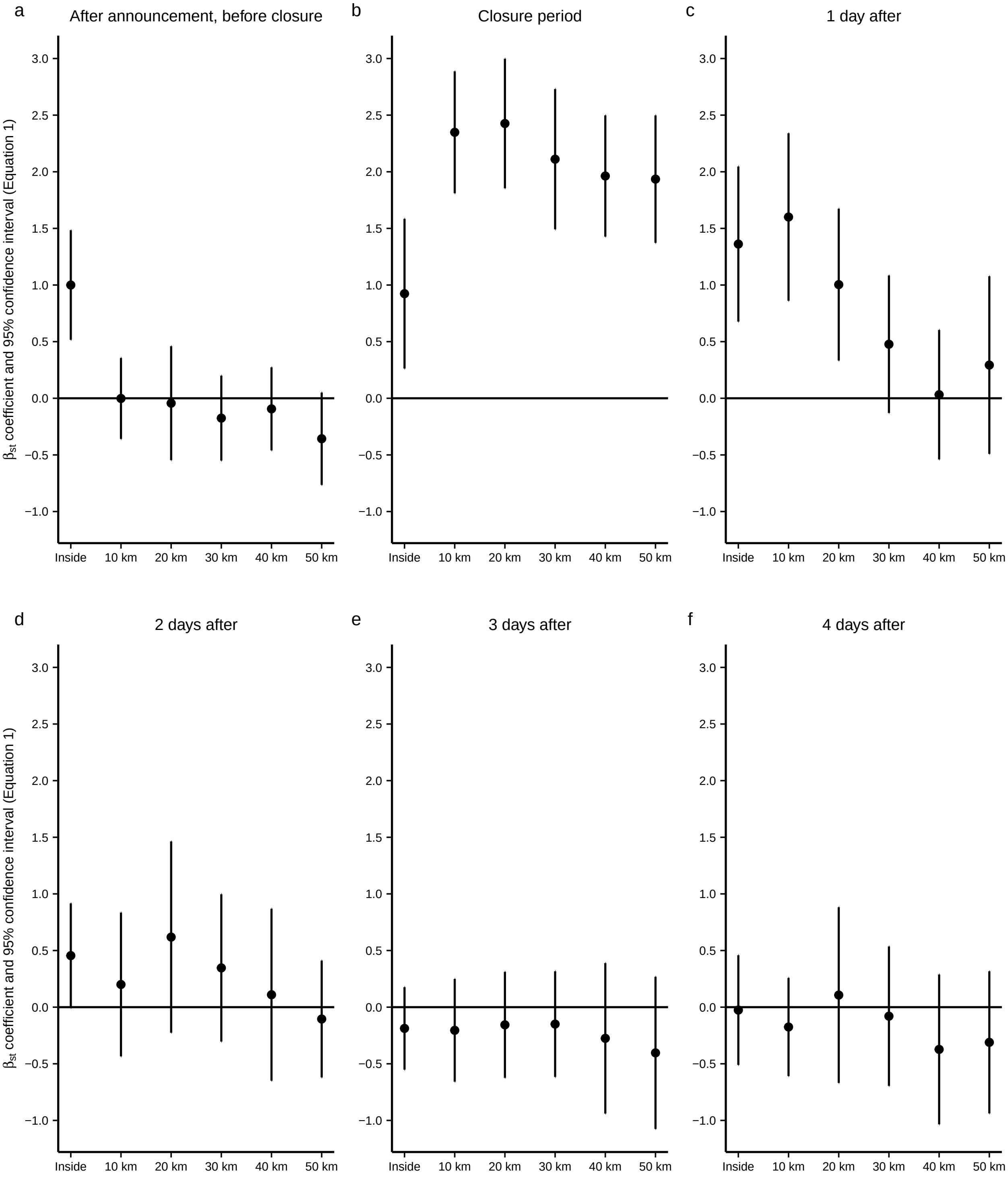
Notes: The dependent variable is the inverse hyperbolic sine of tons of juveniles caught in each potential closure-treatment bin. N = 34,272. Points are coefficients and whiskers are 95% confidence intervals. Standard errors clustered at level of two-week-of-sample by two-degree grid cell.

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



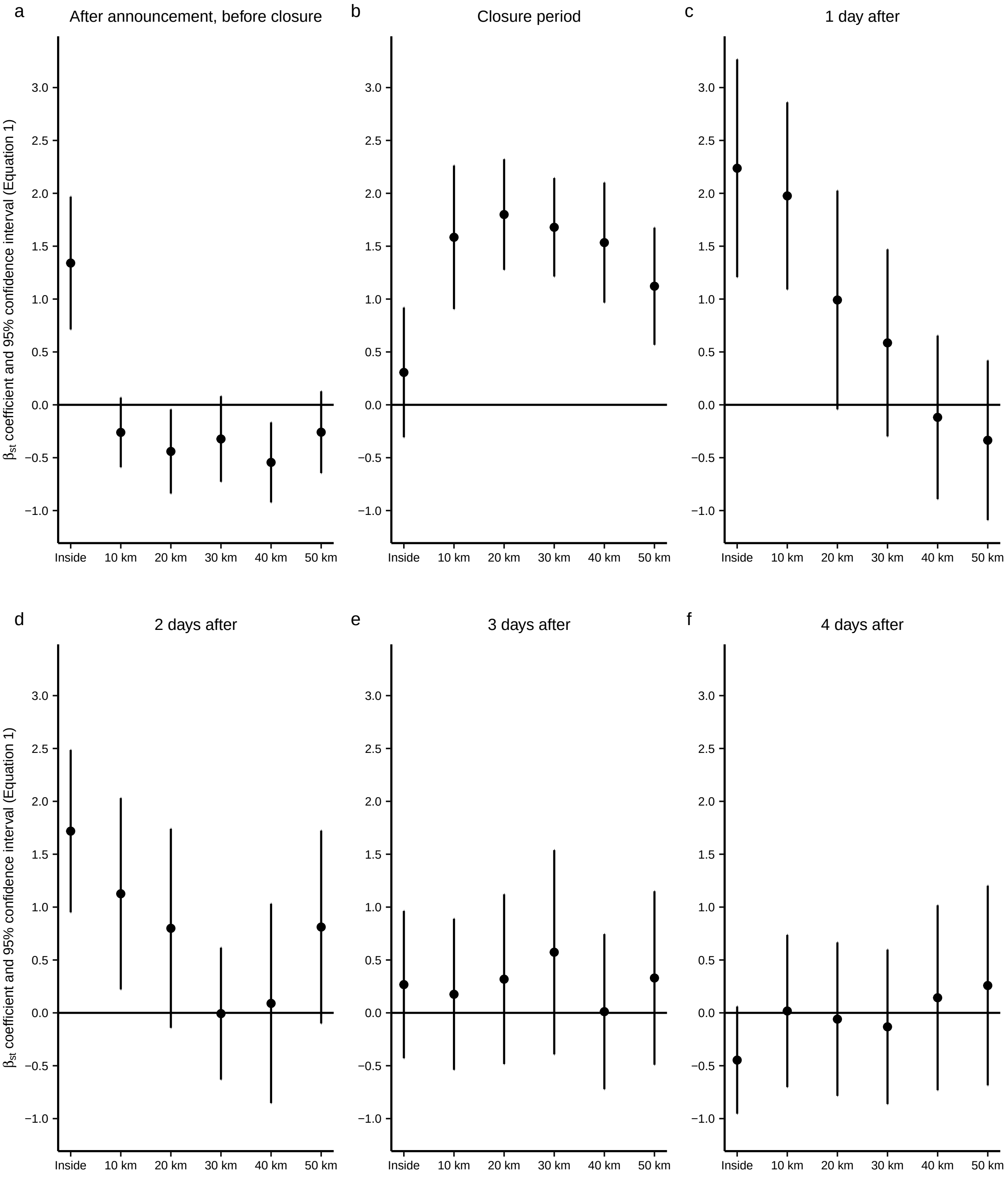
Notes: N = 31,608. Points are coefficients and whiskers are 95% confidence intervals. Standard errors clustered at level of two-week-of-sample by two-degree grid cell.

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



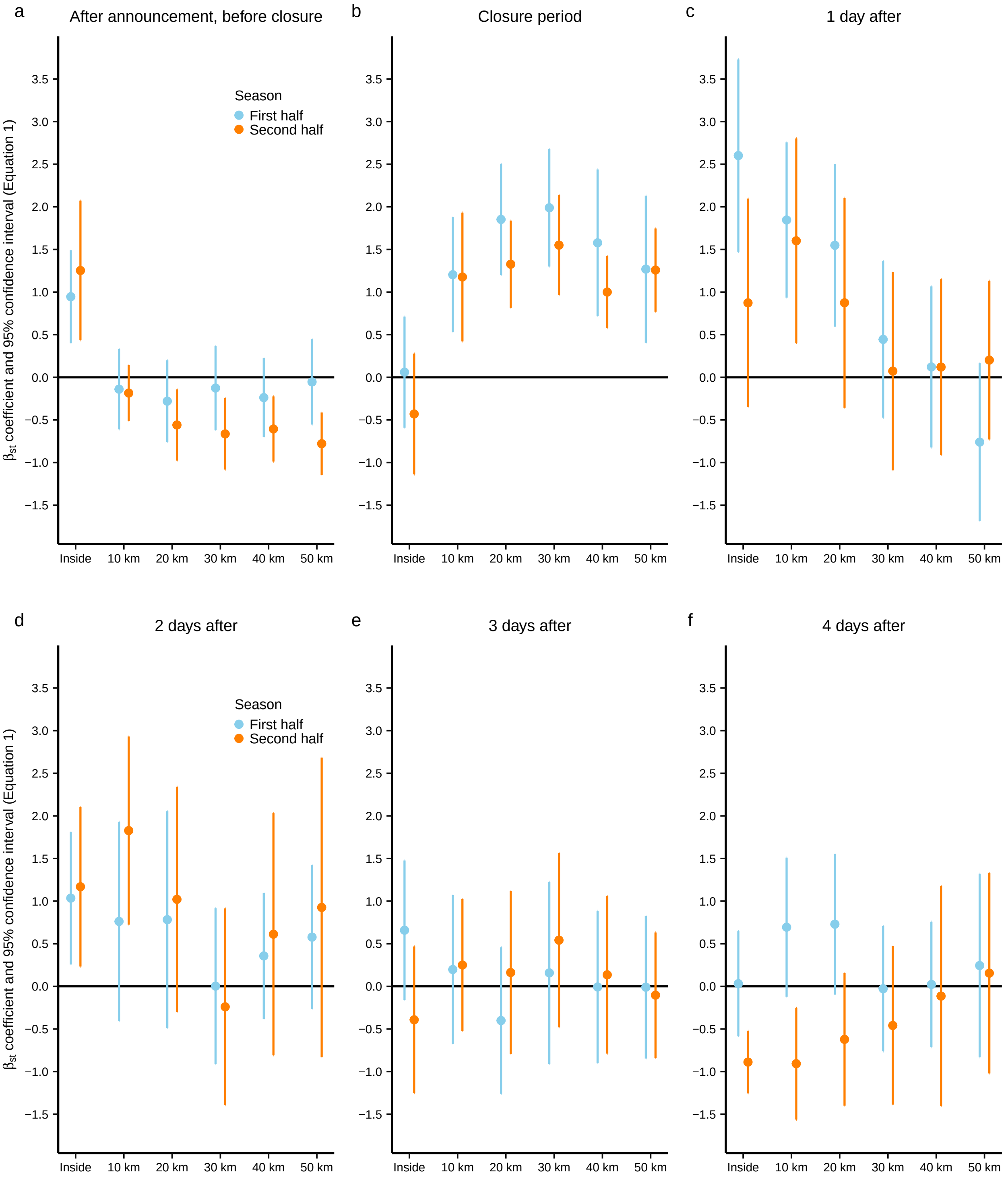
Notes: N = 29,628. Points are coefficients and whiskers are 95% confidence intervals. Standard errors clustered at level of two-week-of-sample by two-degree grid cell.

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

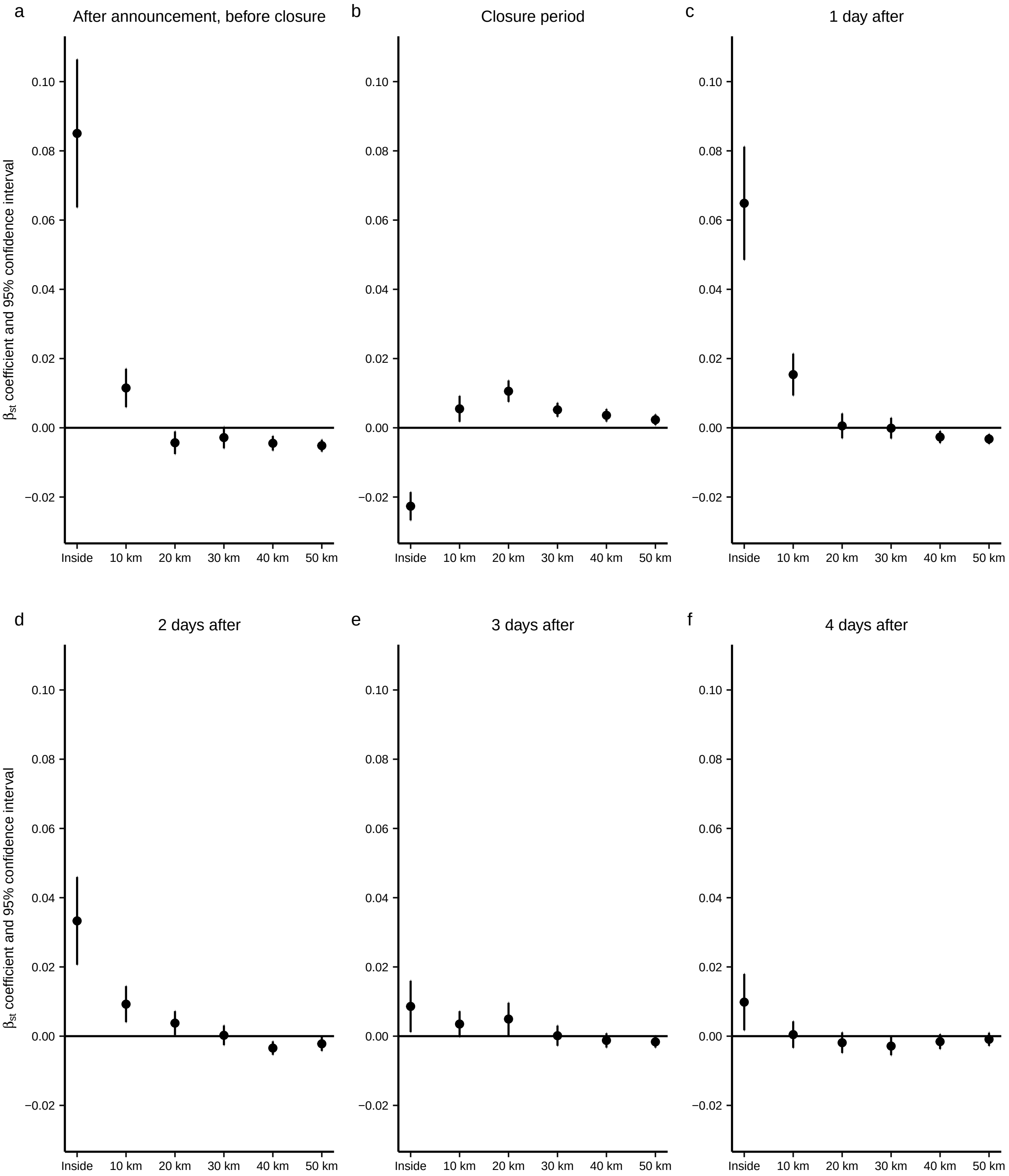
As an alternative estimation approach, I create a regular grid of .1° cells covering the North-Central zone and calculate the millions of juveniles caught in each cell each three-hour period during a fishing season. I rasterize the data at this resolution to match the resolution of treatment assignment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

$$JuvenileCatch_{jk} = \beta_{st}\mathbb{1}\{jk \in st\} + \alpha_j + \delta_k + \sigma_{wg} + \epsilon_{jk} \tag{4}$$

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

I plot the coefficients of interest, β_{st}, in Figure 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.[37] 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 treatment bins · 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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[37] 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).[38] 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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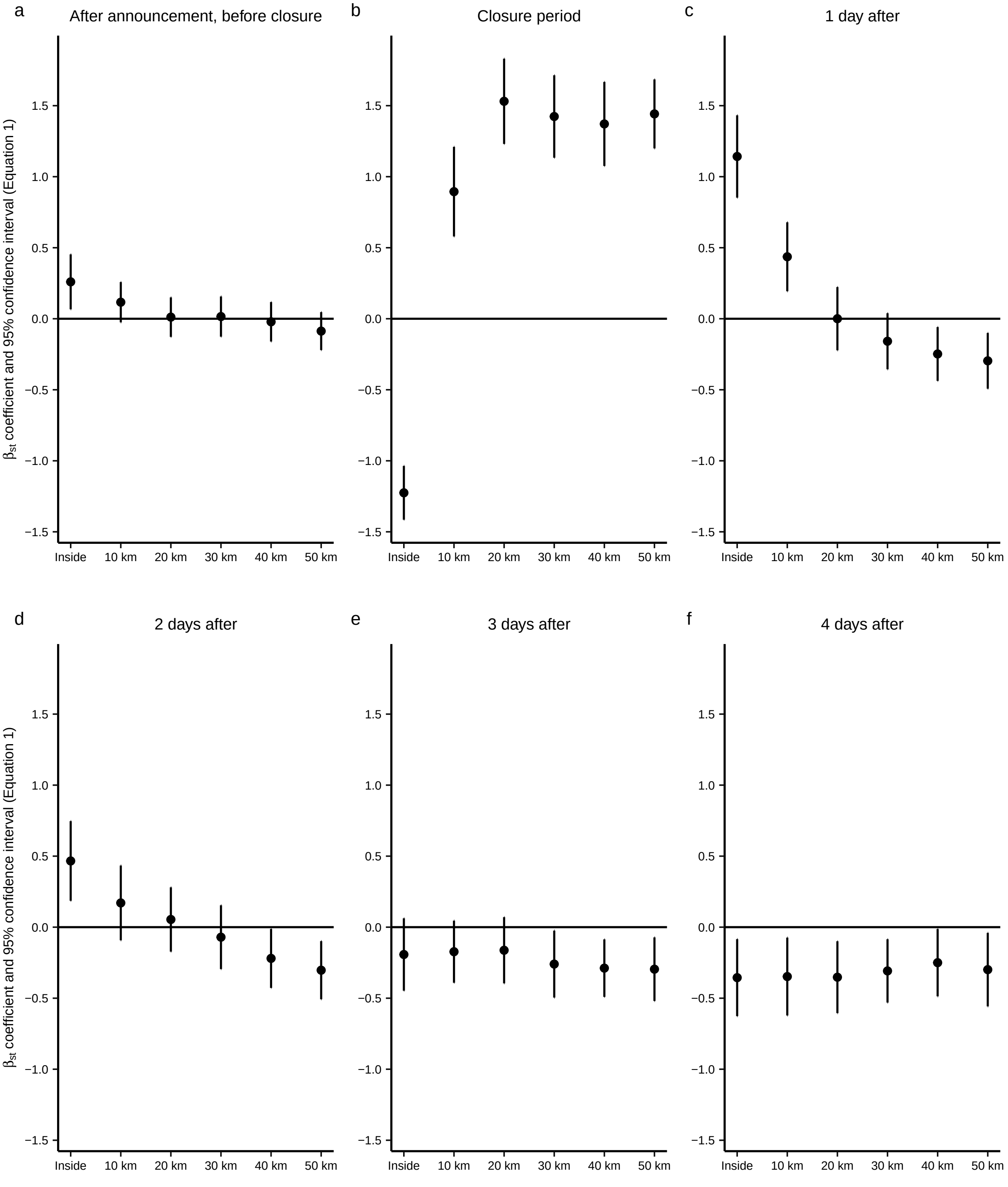
[38] 8 of these error instances are due to the absence of length distribution control variables described above.

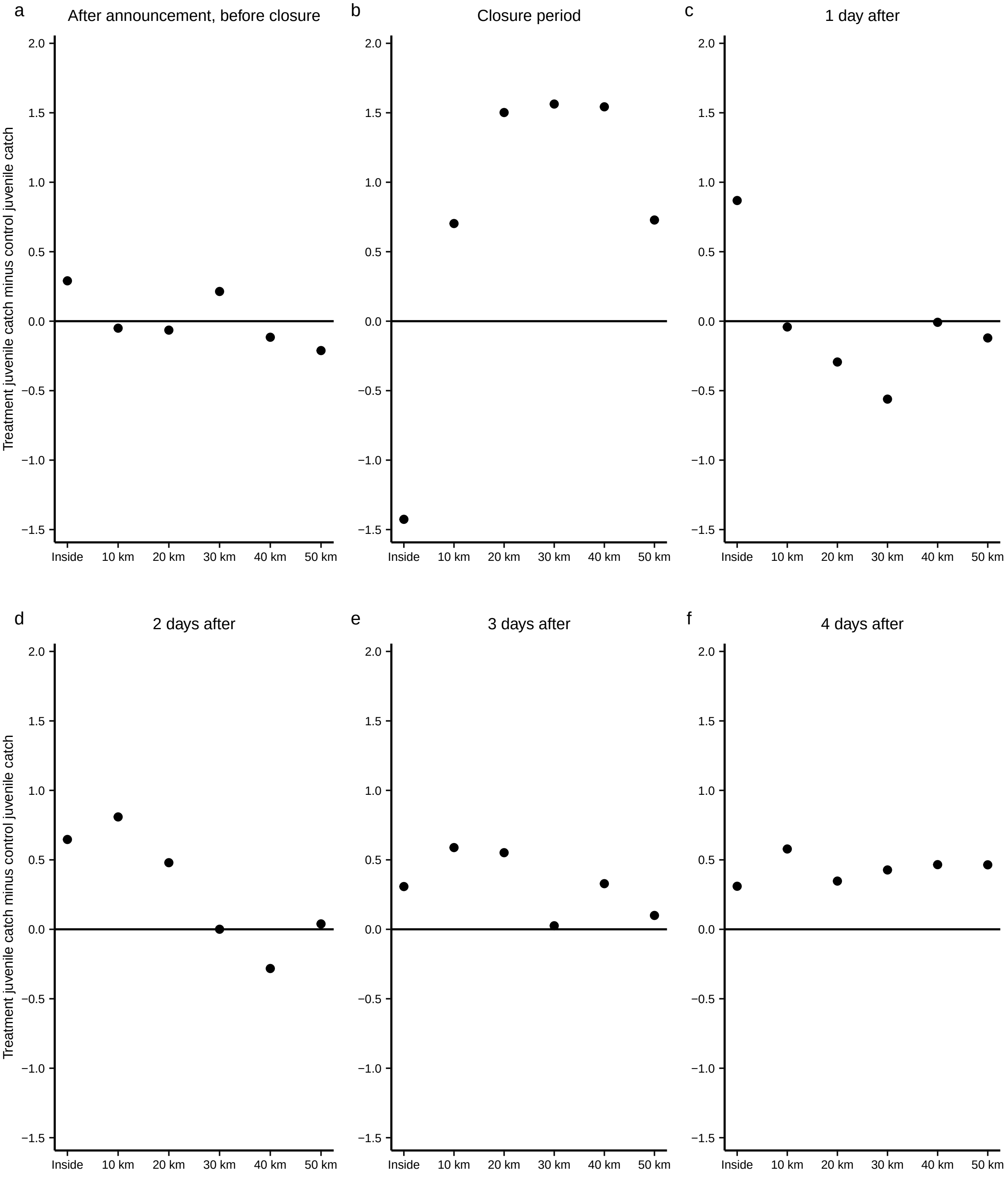
This result is similar to my preferred estimate of the effect of the temporary spatial closures policy—an increase in juvenile catch of 46 billion juveniles, or 48%—even though I obtained it with a different identification strategy.

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.[39] 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 $C = 1$.[40] 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 ℓ denote a generic fishing location. Profit π 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 μ_ℓ. Vessels know that draws of μ_ℓ are independent across locations conditional on C. But vessels do not observe the vector of true productivity $\vec{\mu}$ in the possible fishing locations before making their location choice. For the base case suppose that vessels are identical and that they have the same beliefs $\tilde{\vec{\mu}}$ regarding mean productivity of each location (e.g., the value of $\tilde{\mu_k}$ is the same across vessels).

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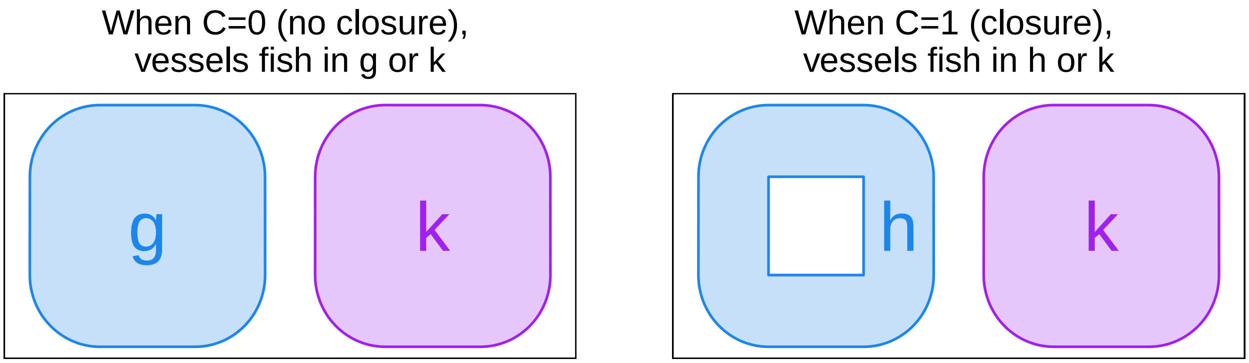
[39] This model abstracts away from important institutional details, such as heterogeneity among vessels and dynamic decision-making. Its purpose is to provide a simple, single framework for understanding the three main empirical results of this paper.

[40] 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

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

Vessel i chooses to fish in g if the expected profit from doing so exceeds the expected profit from fishing in k; $\mathrm{E}[\pi_{i,g}(\mu_g, I_{-i,g})|\tilde{\mu_g}, \tilde{\mu_k}, C = 0] > \mathrm{E}[\pi_{i,k}(\mu_k, I_{-i,k})|\tilde{\mu_g}, \tilde{\mu_k}, C = 0]$. When part of location g is closed ($C = 1$), vessels choose between h and k to maximize their expected profit, yielding a similar decision rule. Let I_ℓ denote the number of vessels who choose location ℓ and let $TotJuv(C)$ denote total juvenile catch given the value of C. Suppose total juvenile catch is the product of the number of vessels who fish in each location, productivity, and percentage juvenile, summed over locations.[41] Then $TotJuv(C = 0)$ equals

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[41] 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 γ is a constant and ρ_ℓ is percentage juvenile.

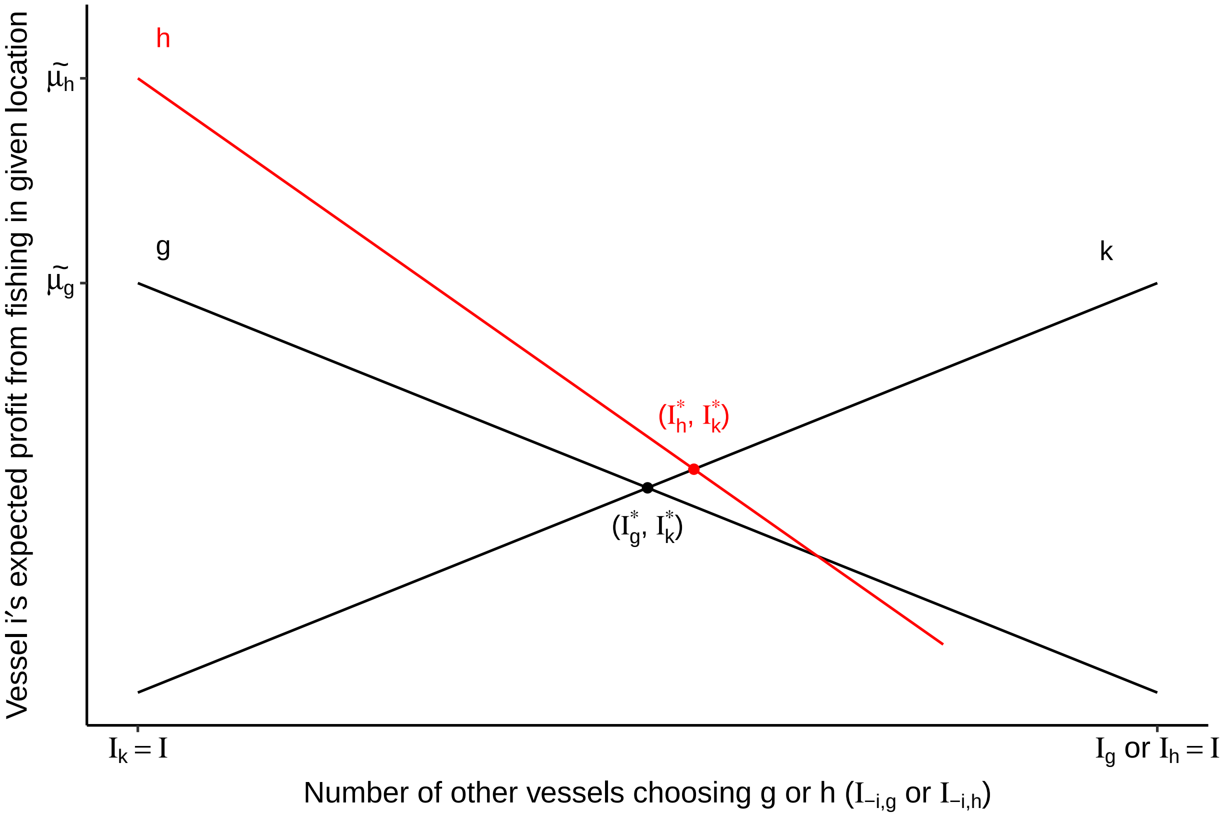
There exists unique Bayes-Nash equilibria (I_g^*, I_k^*) and (I_h^*, I_k^*) such that:

> **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; $\mathrm{E}[\pi_{i,g}(\mu_g, I_{-i,g})|\tilde{\mu_g}, \tilde{\mu_k}, C = 0] =$ $\mathrm{E}[\pi_{i,k}(\mu_k, I_{-i,k})|\tilde{\mu_g}, \tilde{\mu_k}, C = 0]$ $\forall i$. The same is true when $C = 1$; $\mathrm{E}[\pi_{i,h}(\mu_h, I_{-i,h})|\tilde{\mu_h}, \tilde{\mu_k}, C = 1] = \mathrm{E}[\pi_{i,k}(\mu_k, I_{-i,k})|\tilde{\mu_h}, \tilde{\mu_k}, C = 1]$ $\forall i$.
>
> **Proposition 2b.** However, profit from fishing in g exceeds profit from fishing in k if true, unobservable productivity is higher in g than in k ($\mu_g > \mu_k$) and vessels believe mean productivity is the same in both locations ($\tilde{\mu_g} = \tilde{\mu_k}$); $\pi_{i,g}(\mu_g, I_{-i,g}) > \pi_{i,k}(\mu_k, I_{-i,k})$ $\forall i$.

The proofs are in Section B.1. Figure B2 displays the equilibria when $\mathrm{E}[\pi_{i,\ell}(\mu_\ell, I_{-i,\ell})|\vec{\tilde{\mu}}, C] = \tilde{\mu_\ell} - \alpha_\ell I_{-i,\ell}$, where α_ℓ is the cost to vessel i from one additional vessel fishing in location ℓ. Vessel i's expected profit (y-axis) depends on its choice (lines) and the choices of the other I_{-i} vessels (x-axis). Consider Proposition 2a first. When $C = 0$, the equilibrium (I_g^*, I_k^*) is given by the intersection of the two black lines, which represent vessel i's expected profit from fishing in g and expected profit from fishing in k. At this point, no vessel can increase their expected profit by changing their location choice. Similarly, when $C = 1$ the equilibrium (I_h^*, I_k^*) is given by the intersection of the red line (expected profit from fishing in location h) and the upward-sloping black line (expected profit from fishing in location k).

Now consider Proposition 1 as illustrated in Figure 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 $\mathrm{E}[\pi_{i,\ell}(\mu_\ell, I_{-i,\ell})|\tilde{\vec{\mu}}, C] = \tilde{\mu_\ell} - \alpha_\ell I_{-i,\ell}$. The x-axis is the number of other vessels who choose g when $C = 0$ and the number of other vessels who choose h when $C = 1$. The black and red lines indicate vessel i's expected profit from fishing in a given location. The black point is the Nash equilibrium when $C = 0$ and the red point is the Nash equilibrium when $C = 1$. In this parametric example, $I_h^* > I_g^*$ because the closure announcement is a sufficiently 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.[42] 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 ($\tilde{\vec{\mu}}$), but not true productivity $\vec{\mu}$ because $\vec{\mu}$ is unobserved. Then $\pi_{i,g}(\mu_g, I_{-i,g}) > \ \pi_{i,k}(\mu_k, I_{-i,k})\ \ \forall i$ because vessels are identical and profit is increasing in true productivity. If the closure announcement contains valuable information in that it informs vessels that the true productivity of g is higher than k, then vessels that happen to fish in g when $C = 0$ have higher profits because there is no closure announcement that vessels can use to change their fishing location decisions.

The information mechanism proposed in this model also implies a prediction regarding treatment effect heterogeneity. Instead of assuming identical vessels, now suppose there are two types of vessels. When $C = 0$, type a vessels already have the signal regarding location g, but type $-a$ vessels do not: $\tilde{\mu_{g,a}} > \tilde{\mu_{g,-a}}$. When $C = 1$, both types receive the positive signal from the closure announcement, so $\tilde{\mu_{h,a}} = \tilde{\mu_{h,-a}}$. The closures policy treatment effect τ 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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[42] 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 $\tilde{\mu_{g,a}} > \tilde{\mu_{g,-a}}$ when $C = 0$, $\tilde{\mu_{h,a}} = \tilde{\mu_{h,-a}}$ when $C = 1$, juvenile catch per vessel is higher in location g than in location k ($\mu_g \rho_g > \mu_k \rho_k$), and an interior Bayes-Nash equilibrium when $C = 0$ ($I_g^*, I_k^* > 0$) and when $C = 1$ ($I_h^*, I_k^* > 0$). Then type $-a$ vessels have a larger percentage treatment effect than type a vessels; $\frac{\tau_{-a}}{I_{-a}} > \frac{\tau_a}{I_a}$.

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

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

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

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

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

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

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

Together, the fact that vessels believe mean productivity in h is higher but know that marginal congestion costs are also higher in h means that the effect of the closures policy on fishing location choice is ambiguous. The closure announcement must be a sufficiently 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 τ of the closures policy on total juvenile catch is

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

If $I_h^* > I_g^*$, the third term in the expression, $\mu_k\rho_k(I_{k|C=1}^* - I_{k|C=0}^*)$, is negative because $I_{k|C=1}^* < I_{k|C=0}^*$. In order for the closures policy to increase total juvenile catch ($\tau > 0$), the number of vessels fishing, productivity, and percentage juvenile in location h must be sufficiently high relative to location g when there is no closure ($I_h^*\mu_h\rho_h >> I_g^*\mu_g\rho_g$), as well as 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 $\mathrm{E}[\pi_{i,\ell}(\mu_\ell, I_{-i,\ell})|\tilde{\vec{\mu}}, C] = \tilde{\mu_\ell} - \alpha_\ell I_{-i,\ell}$, where α_ℓ is the cost to vessel i from one additional vessel fishing in location ℓ. The equilibrium when $C = 0$ results from setting $\tilde{\mu_g} - \alpha_g I_{-i,g} = \tilde{\mu_k} - \alpha_k I_{-i,k}$. 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 α represent this value. The equilibrium when $C = 0$, (I_g^*, I_k^*), is $(\frac{\tilde{\mu_g} - \tilde{\mu_k}}{2\alpha} + \frac{1}{2}I,\ \frac{\tilde{\mu_k} - \tilde{\mu_g}}{2\alpha} + \frac{1}{2}I)$. The equilibrium when $C = 1$, (I_h^*, I_k^*), is $(\frac{\tilde{\mu_h} - \tilde{\mu_k}}{\alpha_h + \alpha} + \frac{\alpha}{\alpha_h + \alpha}I,\ \frac{\tilde{\mu_k} - \tilde{\mu_h}}{\alpha_h + \alpha} + \frac{\alpha_h}{\alpha_h + \alpha}I)$. Substituting these values into Equation 5 gives the change in total juvenile catch due to the policy.

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

Since type a and type $-a$ vessels are identical when $C = 1$ ($\tilde{\mu_{h,a}} = \tilde{\mu_{h,-a}}$), the same percentage of each type choose locations h and k ($\frac{I^*_{h,a}}{I_a} = \frac{I^*_{h,-a}}{I_{-a}}$ and $\frac{I^*_{k,a}}{I_a} = \frac{I^*_{k,-a}}{I_{-a}}$). Since both types of vessels catch the same number of juveniles when they fish in the same location, $\frac{TotJuv(C=1)^*_{-a}}{I_{-a}} = \frac{TotJuv(C=1)^*_a}{I_a}$. Then the percentage difference in treatment effects between the two types of vessels can be written as

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

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 $.0036 length^{3.238}$ (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.[43]

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 (SNP, 2020).

To calculate juvenile catch for each set, I first use the length distribution values from sets in the SNP electronic logbook data to impute length distributions for non-SNP sets, based on

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[43] 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.[44] My results are robust to imputing length distributions at the two-

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[44] 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%).[45] 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.[46]

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.

[45] 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).

[46] 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.[47]

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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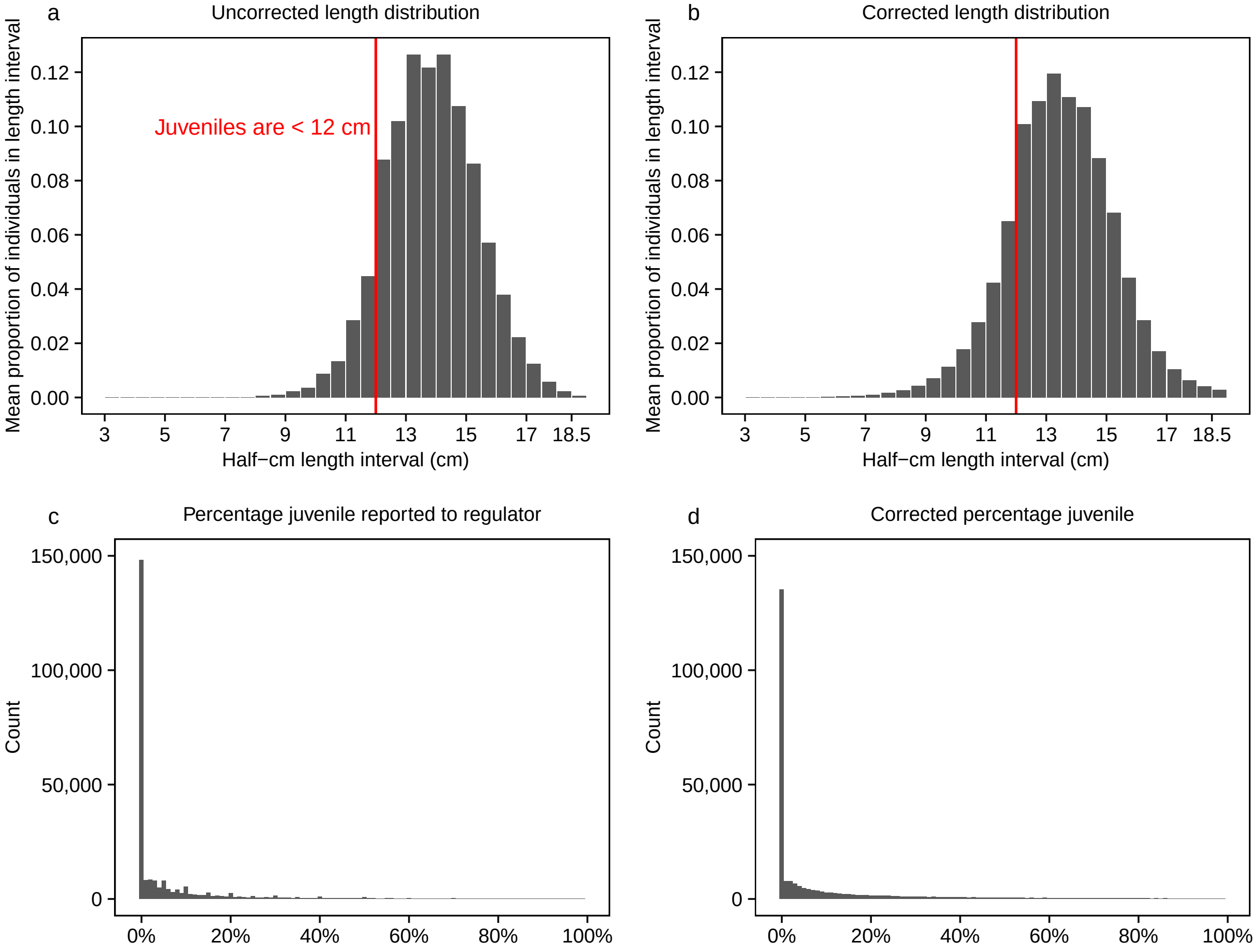
[47] I re-estimate Equation 1 with a single treatment fraction variable (rather than one for each treatment bin) and a new outcome variable: an indicator that equals 1 if any sets inside the potential-closure treatment bin were not matched to a landing event. I choose an indicator variable rather than 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.

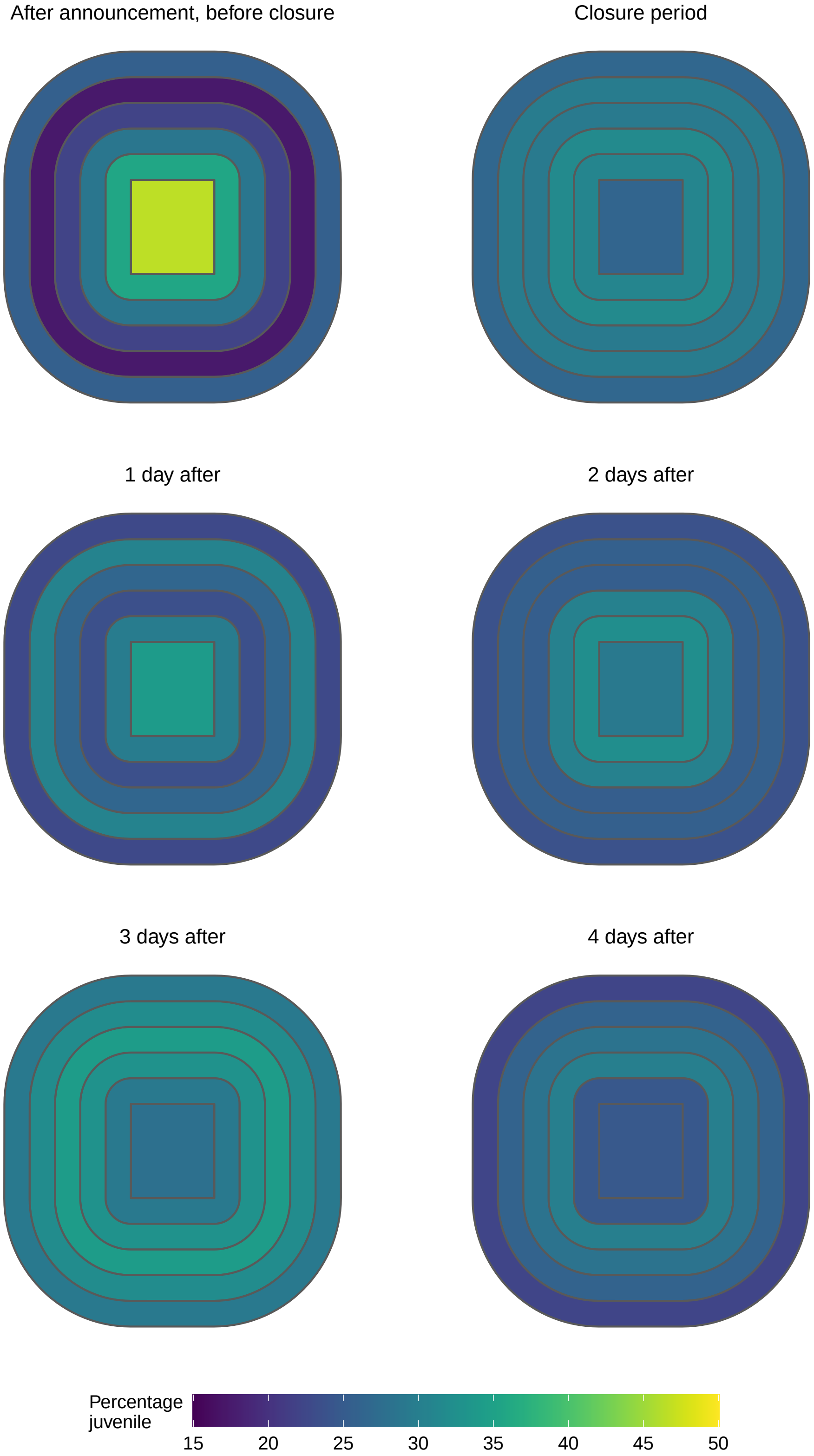
Finally, I shift the length distribution of each set up or down in half-cm increments until the absolute difference between the implied percentage juvenile (updated percentage of individuals that are less than 12 cm) and the corrected percentage juvenile is minimized. I use the resulting length distribution 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^2, or 36 by 36 km for a square closure. A school of anchoveta can swim 20 to 30 km in a day (Peraltilla and Bertrand, 2014). If juvenile anchoveta swim outside the closed area during the closure period, then closures might not be large enough to prevent 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:

$$\begin{aligned} JuvenileCatch_{ist} =& \alpha_{sth} + \beta_{sth} TreatFraction_{isth} + \sum_{\ell=[3,3.5)}^{[18.5,19)} \xi_\ell Prop_{i\ell} + \\ & \gamma_1 Sets_i + \gamma_2 Tons_i + \gamma_3 Area_i + \gamma_4 DistToCoast_i + \\ & \gamma_5 TonsPerSet_i + \gamma_6 TonsPerArea_i + \sigma_{wg} + \delta_d + \epsilon_{isth} \end{aligned} \tag{6}$$

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 0.55). 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 (PRODUCE, 2020). 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 (1)	Large-firm vessels (2)	Medium-firm vessels (3)	Singleton vessels (4)
A. Average tons landed per season				
Minimum	3.11	105.45	14.64	3.11
Mean	2607.41	6311.44	1842.81	903.56
Median	1324.49	6004.34	1303.62	722.86
Max	22261.64	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, 2023).

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	Δ % 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. Δ % 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 (p-value is 0.0005). 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. ℓ 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, α 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).

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

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

$$N_{c,t+1} = N_{c,t}e^{\frac{-.8}{365}} \tag{9}$$

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

First, individuals grow in length and weight according to growth equations from IMARPE (2019) and Salvatteci 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 α 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 Salvatteci 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

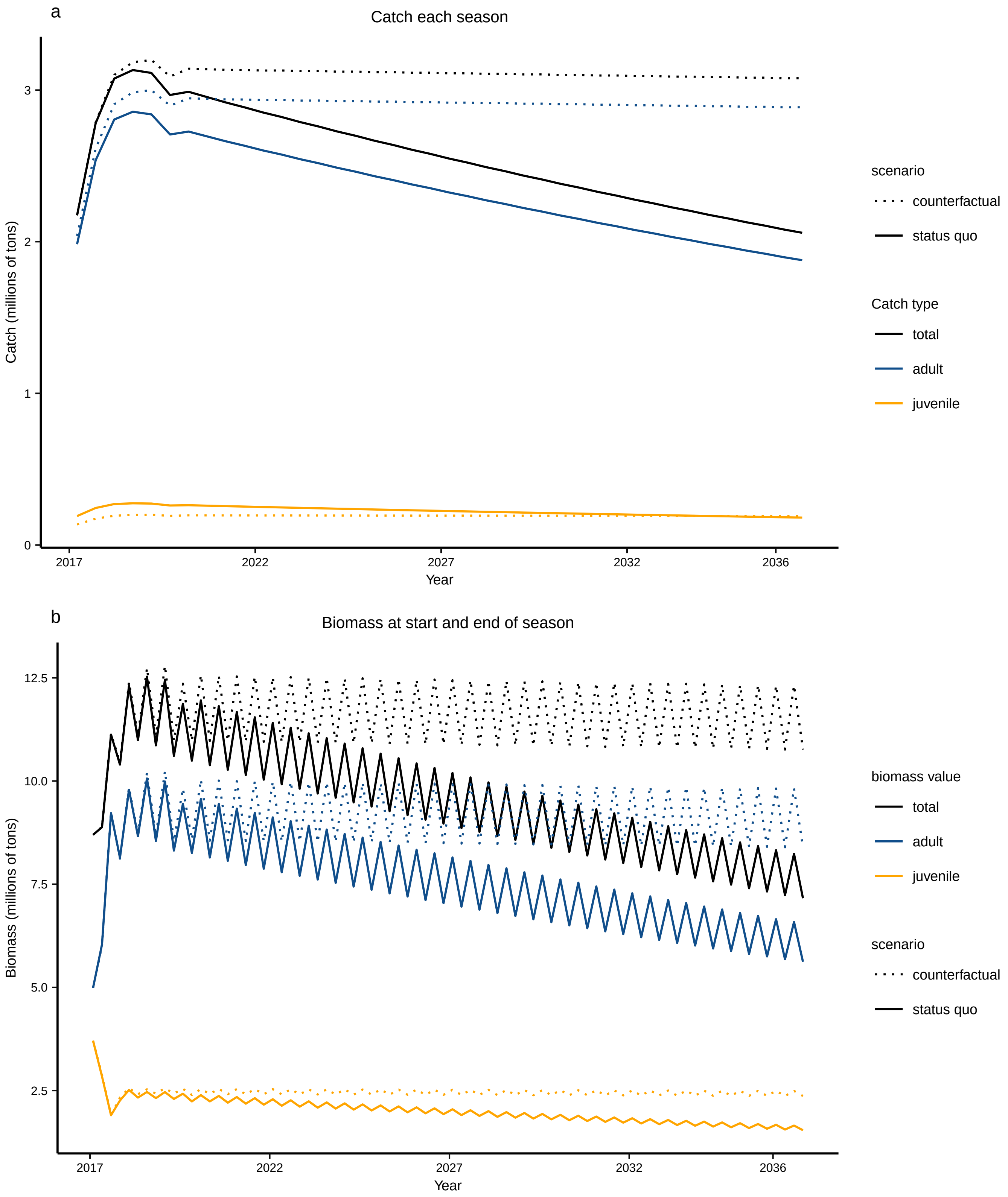
$$\text{(11)} \qquad H_{c,t+1} = \begin{cases} \frac{\sum B_{c,\bar{t}}}{4\cdot 91} \cdot 0.0878 \cdot \frac{B_{c,t}}{\sum_{\ell<12} B_{c,t}}, & \text{if } S = \text{status quo and } \ell_{c,t+1} < 12 \\ \frac{\sum B_{c,\bar{t}}}{4\cdot 91} \cdot (1 - 0.0878) \cdot \frac{B_{c,t}}{\sum_{\ell\geq 12} B_{c,t}},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

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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