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Are Bans Effective under Limited Monitoring? Evidence from High Seas Management

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To mitigate environmental and social harm, policy-makers often provide incentives or impose sanctions to discourage harmful behavior. Such policies are usually implemented with limited monitoring capabilities, which may cause strategic behavior that leads to unintended consequences. Three related questions for any policy are therefore: do targeted agents comply on elements that are visible (visible compliance), do agents behave strategically to undermine the policy (effectiveness), and are raw material prices affected (economic cost)? We study these questions empirically in the context of a zero-tolerance policy (a ban) on seafood transshipments on the high seas — a ban imposed because seafood transshipments are associated with illegal fishing and widespread forced labor. Novel satellite-based datasets, available ex-post several years after implementation of the ban, offer a unique opportunity to study the effect of the ban in hindsight. Combining satellite-based and economic datasets, and exploiting variation across regions and over time, we find that a ban reduces the yearly growth in transshipment rates by an estimated 58% despite significant monitoring challenges, and does not cause appreciable strategic behavior. A difference-in-differences analysis of landing prices suggests that this reduction comes at an estimated cost of 3% higher raw material prices.

Key words: zero-tolerance policy, transshipment, remote sensing, strategic behavior, limited monitoring

1. Introduction

To mitigate environmental and social harm, public and private policy-makers design policies that discourage harmful behavior. The simplest version of such a policy is zero tolerance toward harmful practices, *i.e.,* a ban. This includes bans imposed by global buyers (*e.g.,* the Amazon Soy Moratorium, where companies banned the purchase of soy from suppliers that harvested soy from Amazon forest after 2006) and by government decrees (*e.g.,* the government of Uzbekistan banned child labor in cotton fields). Such bans set clear guidelines on what behavior is prohibited.

Most bans face important challenges with monitoring and enforcement, with policy-makers often relying on audits for actionable insights. Under such limitations, targeted agents may respond through a continuum of actions, depending on their perceived costs of (non-)compliance. Some will comply with the ban, and others will simply ignore the ban. Yet others will respond strategically by increasing efforts to hide harmful behavior (*e.g.,* exerting effort to pass an audit rather than prevent harm, as in Plambeck and Taylor 2015, or further reducing transparency by sub-contracting, as in Caro et al. 2020) or by evading the ban by shifting harmful behavior to a different location (*e.g.,* moving operations in response to a ban on deforestation in some countries or provinces, as in Le Polain de Waroux et al. 2016). If the vast majority of suppliers respond strategically by masking harmful behavior or by evading the ban's jurisdiction, then although targeted agents *appear* to comply with the ban, the ban does not reduce the targeted behavior, *i.e.,* it is ineffective.

For any ban, there are therefore three pertinent, related questions: first, do targeted agents comply on elements that are visible (*visible compliance*)? Second, is there a significant reduction in the targeted behavior (*effectiveness*)? Third, how does the ban affect raw materials prices (*economic cost*)? The response depends on a large array of context-specific parameters and is therefore inevitably an empirical question. However, studying these questions empirically is often difficult due to the limited monitoring capabilities and, therefore, limited data; the lack of data is a natural consequence of limited monitoring capabilities. As a result, previous empirical studies have — to the best of our knowledge — only been able to study one or two of these questions of interest, offering an incomplete response. For example, while Gibbs et al. (2016) find that zero-deforestation cattle agreements significantly increased visible compliance by suppliers, the authors caution that suppliers may behave strategically by "leaking" cattle to slaughterhouses without full monitoring systems or by laundering cattle through compliant ranches. Unlike our setting, it was not possible for the authors to observe or quantify the extent of such strategic behavior that they warn about.

In this paper, we exploit a unique setting to empirically study these questions *jointly*. In our setting, historical monitoring data became available several years *after* implementation of the ban. This allows us to study the response to a ban that had limited monitoring *at the time of implementation*, and about which historical data has now become available. While specific to our context, this can shed light on these three pertinent dimensions of a ban with limited monitoring: visible compliance, effectiveness, and economic cost.

Specifically, we study these dimensions for a ban on *reefer-to-fishing vessel transshipments* on the high seas — the open ocean that does not fall within any country's jurisdiction. A reefer-to-vessel transshipment (referred to as a transshipment hereafter) is the act of offloading fish catch from a fishing vessel to a refrigerated cargo vessel (often called a reefer) (UN FAO 2011). After a transshipment on the high seas, the reefer brings the frozen catch back to port, ensuring that freshly-caught fish does not spoil; this allows the fishing vessel to continuously fish on the high seas (for months or even years) without repeatedly returning to shore to offload catch.

The practice of transshipments has obvious economic benefits, allowing fishing vessels to maximize catch as well as minimize fuel costs, and these benefits have been increasing in response to economic and ecological pressures faced by fishing vessels (Tickler et al. 2018a). Fish stocks close to shore are becoming increasingly depleted due to overfishing, *i.e.*, fish continue to be harvested at rates higher than they can replenish themselves. Consequently, fishing vessels increasingly have to adopt "distant-water" fishing models — fishing further away, into the deep ocean, and for longer periods of time, to obtain sufficient catch (Tickler et al. 2018b, Mongabay 2018, Swartz et al. 2010, Pauly et al. 2003, Gianni and Simpson 2009). Transshipments reduce costs by allowing fishing vessels to save fuel costs on trips back to port, and therefore, the practice of transshipments has been increasing over time (see Fig. 1).

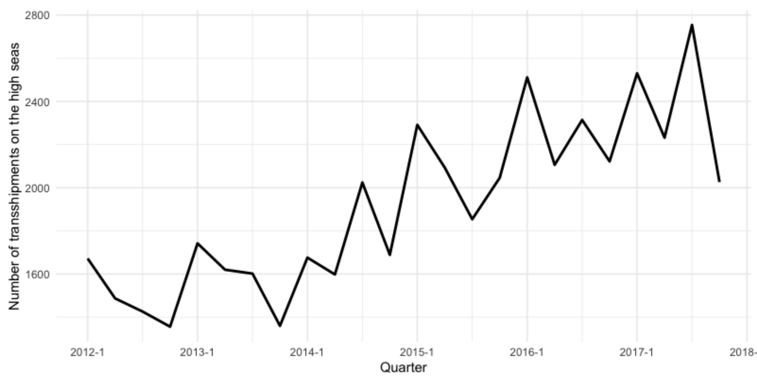

Figure 1 Number of inferred transshipments (see §2 for details) on the high seas per quarter, 2012-2017.

However, transshipments are undesirable because they significantly reduce transparency in the seafood supply chain by masking where, how, and by whom the fish are caught. Reefers can pick up catch from many fishing vessels along their way, enabling them to launder contraband catch into poorly-monitored ports as legally-caught catch (Zimmer 2017). As a result, transshipments are correlated with Illegal, Unreported and Unregulated (IUU) fishing (Kroodsma et al. 2017, Gianni and Simpson 2009). Critically, transshipments enable fishing vessels to stay at sea for months or years at a time, allowing fishing vessels to evade monitoring, enforcement and civil society. These conditions pave the way for human rights abuses (see, *e.g.,* Urbina 2015 for a full story). Indeed, Issara Institute (2017) finds that physical abuse of fishermen is three times more likely to occur on vessels that transshipped catch on the high seas. The growth in transshipments has therefore come with a reduction in supply chain transparency and more opportunities for human trafficking.

Hence, similar to the practice of unauthorized subcontracting in Caro et al. (2020), the practice of transshipments increased in response to economic pressures in the supply chain, but consequently reduced transparency. While prior literature (*e.g.,* Caro et al. 2020 and Levi et al. 2019) focuses on understanding the circumstances under which such undesirable practices might arise (*e.g.,* supply chain dispersion, price pressure, or workload pressure), we focus on understanding what happens if global buyers or policy-makers ban such practices despite challenges with monitoring.

The high seas are international waters, and so no single country has jurisdiction over these waters. However, groups of countries become signatory members to Regional Fisheries Management Organizations (RFMOs) that govern large bodies of these international waters — *e.g.,* the West and Central Pacific Fisheries Commission (WCPFC) regulates fishing in the Pacific Ocean— and regulations can be passed with certain levels of consensus among all signatory members.

In response to growing concerns associated with transshipments on the high seas, several RFMOs instituted a ban on transshipments in the late 2000s for certain types of fishing vessels in waters that they govern. We refer to such bans as *"geographic bans"* because they apply to geographically-delineated regions of the high seas. For instance, the WCPFC instituted a ban in 2009 that applied to purse seine vessels in the Pacific Ocean (Ewell et al. 2017).

Key questions are therefore whether reefers visibly comply with the ban, whether the ban is effective, and how the ban affects raw material prices.

On one hand, non-compliance with the ban comes with steep penalties; most countries seize and forfeit the reefer and its cargo, levy a penalty that can be up to twice the value of the cargo, and impose jail sentences for those involved (see, *e.g.,* U.S. Code 2020, UN FAO 2018a, Jakarta Post 2018). On the other hand, as mentioned above, transshipments significantly reduce economic costs of high-seas fishing (Zimmer 2017), and compliance with the ban on the high seas is very difficult to monitor; as a result, the probability of being detected can be perceived as too low to meaningfully change behavior. We highlight three specific challenges with monitoring below.

First, while reefer *movements* can be monitored remotely — all reefers are required to be equipped with Automatic Identification System (AIS) transponders for real-time collision avoidance — they can "go dark" when captains turn off the transponders. It is often unclear if a missing signal is due to poor satellite coverage or strategic behavior by the vessel captain, making it difficult to enforce that AIS transponders are "on" at all times (GFW 2018, Windward 2014). Second, until the very recent emergence of the Global Fishing Watch (GFW) platform, AIS signals could not be used to detect *behaviors* suggestive of transshipment; this is because doing so required a concerted effort by GFW in processing over 20 million raw AIS messages per day to infer vessel behavior (see §2.2). Thus, until very recently, vessels could transship without the risk of being detected through their AIS transponders (Pintassilgo et al. 2010, O'Leary et al. 2012). Finally, to improve monitoring, several RFMOs began requiring observers on board a small percentage of fishing vessels; these observers monitor transshipments and report violations (Ewell et al. 2017). However, there is limited protection for observer safety on board the vessel s/he is tasked to monitor; consequently, observers can easily be bribed, harassed, threatened or obstructed (Zimmer 2017, ABC 2017, WCPFC 2016).

Understanding the compliance, the effectiveness, and the economic cost of the ban in the presence of such limited monitoring is inevitably an empirical question. We use recent technological developments to analyze vessels' historical AIS signals *ex-post* (GFW 2018) as well as satellite imagery of fishing pressure (Elvidge et al. 2015) and economic datasets on landing prices of fish (Daniel and Zeller 2015, Cullis-Suzuki and Pauly 2010) to study these questions for geographic transshipment bans implemented by RFMOs. We exploit variation over time and across regions to identify the effect of a transshipment ban on inferred transshipments and strategic behavior of vessels. Specifically, we address the following research questions:

- What is the effect of a geographical ban on the growth in transshipment rates?
- Do vessels evade the ban's jurisdiction by shifting transshipments to regions without a ban?
- Do vessels try to mask transshipments by "going dark" in regions with a ban?
- Does the ban lead to reduced fishing activity?
- Does the ban lead to higher raw material prices?

We find that the geographical bans reduce yearly growth in transshipment rates by an estimated 58%. It is important to note that this reduction is relative to transshipment rates in locations without a ban; in fact, while transshipment bans appear to successfully dampen the increase in transshipment rates, they do not eliminate them.

We also find that there is minimal strategic response to the ban, including evading the ban's jurisdiction or investing effort in masking transshipments. This may be because evasion is too costly and masking may not have been necessary due to a lack of monitoring.

We find further evidence of the substantial impact of the ban on high-seas fishing activity through its impact on raw material prices. Specifically, we find that the reduction in transshipment growth is primarily driven by changes to the number of transshipments per unit of fishing activity, rather than a reduction in the growth of fishing activity. Correspondingly, a difference-in-differences analysis estimates that the ban led to a 3% increase in fish landing prices.

Overall, our analysis provides evidence that bans can ensure significant visible compliance by targeted agents even under limited monitoring. More importantly, it can significantly reduce the targeted behavior, *i.e.,* be effective. In other words, not only does the growth rate of non-compliance go down, but targeted agents are also not simply shifting undesirable activities to regions that allow for non-compliance. In the case of the transshipment bans we study, monitoring of compliance will continue to improve – especially since the introduction of Global Fishing Watch – and in several years it will be possible to study how this improved monitoring affects the effectiveness of bans.

The remainder of this paper is structured as follows: §2 describes the datasets, §3 describes our methods, and §4 shares our results. We provide concluding remarks in §5.

2. Datasets

This section describes the data sources that we use in our analyses.

2.1. Regional Fisheries Management Organizations (RFMOs) Data

Fisheries on the high seas are managed by 17 RFMOs, which are international governance bodies comprised of signatory member countries. The number of member countries, the geographic delineation of the open ocean, and the number of fish species managed differ across RFMOs. We obtained spatial data on RFMO borders, as well as RFMO signatory country memberships from the UN FAO's Regional Fishery Bodies Map Viewer (see UN FAO 2018b).

We obtained data about the presence of a transshipment ban in each RFMO from Ewell et al. (2017), who manually classified transshipment-related policies in each RFMO by reviewing official documents and websites. In particular, we use the criterion "transshipments prohibited for some vessels," which refers to whether transshipment at-sea is completely prohibited for at least some types of fishing vessels in the RFMO (see Table 7 in Appendix A.1 for the specific vessel types targeted by transshipment bans in each RFMO). Transshipment bans were passed in 6 of 17 RFMOs in the late 2000s (see Table 1 below).

RFMO	SEAFO	ICCAT	GFCM	IATTC	IOTC	WCPFC
Ban implementation year	2006	2006	2007	2008	2008	2009

Table 1 Six of seventeen RFMOs implemented a ban on transshipments on the high seas in the late 2000s.

2.2. Automatic Identification System (AIS) Data

The International Maritime Organization (IMO) requires all international voyaging ships weighing over 300 tons to be equipped with an AIS transponder, primarily to avoid collisions and promote maritime safety. Reefers typically weigh at least 300 tons, and as a result, 97% of reefers are equipped with AIS transponders (Miller et al. 2018). This stands in contrast to fishing vessels; only 7% of registered fishing vessels meet the weight criteria and so they are unlikely to be equipped with AIS transponders. AIS data can therefore reliably be used to study reefer behavior but much less so to study fishing vessel behavior, motivating us to focus on reefer behavior.

AIS transponders transmit a vessel's unique identifier, position, course and speed every 2 to 3 minutes via VHF radio. Vessels fitted with AIS transponders can be tracked by AIS base stations located along coast lines or through satellites that are fitted with AIS receivers.

The open-access GFW platform was launched in 2016 to monitor global fishing activities by tracking vessels at scale using AIS data collected since 2012. The vast amount of data generated by AIS transponders (over 20 million raw messages per day) was not analyzed or used for monitoring and surveillance purposes until the advent of GFW (Dunn et al. 2018). This required significant data pre-processing effort by a team of data scientists to infer realistic vessel positions and tracks over time from noisy positional messages (see Kroodsma et al. 2018 for details). We obtained data on both transshipments and gaps in AIS signals constructed by GFW. We explain these two datasets in more detail next.

2.2.1. Inferred Transshipments. GFW identified and tracked 694 unique reefers capable of transshipping at sea and transporting fish. While one cannot directly observe whether a reefer is engaged in a transshipment using AIS data, one can identify *vessel behaviors* that strongly suggest transshipment. In particular, by analyzing confirmed observer-reported transshipments from the IOTC and consulting with domain experts, Kroodsma et al. (2017) defined a behavioral proxy for a reefer engaged in transshipment — moving at less than 2 knots for longer than 8 hours on the high seas. While many reefers loiter near the shore (*e.g.,* while waiting for entry to a port, or for cargo arrival), loitering for many hours on the *high seas* is highly suggestive of transshipment to a fishing vessel. In fact, Kroodsma et al. (2017) note that reefers that were loitering on the high seas exhibited behaviors such as "distinctive C-shaped tracks and abrupt shifts in course following a period of slow speeds," which likely signify rendezvous with another vessel. Using this definition, Miller et al. (2018) identify 46,131 inferred transshipments between 2012 and 2017.

We study the effect of transshipment bans on inferred transshipments, with the caveat that these events are only a proxy for transshipment of fish at sea and may not represent a one-to-one relationship. However, to the best of our knowledge, any errors in inferred transshipments are not correlated with the presence of a transshipment ban. This is because transshipment data was made available ex-post (so vessels in this time period would not have invested effort to systematically evade AIS detection while transshipping in regions with a ban), and satellite coverage of AIS signals has remained constant and similar across regions with and without a ban during this time period (see Appendix A.2). One may be concerned that there are differences in the *length* of transshipment events in regions with a ban (*e.g.,* if vessels in regions with a ban engage in shorter transshipments to evade detection by marine patrol); thus, we test the sensitivity of our main results by allowing events where a reefer is loitering for shorter periods of time to qualify as an inferred transshipment, and find qualitatively similar results (see Appendix B.2).

We performed two key pre-processing steps on this data. First, since we are only interested in transshipments on the high seas, we exclude any inferred transshipments that occurred within an Exclusive Economic Zone (EEZ). EEZs are areas in the ocean that typically stretch out 200 nautical miles from a country's coastline; countries have special rights to fish in these zones as prescribed by the UN Convention on the Law of the Sea. Inferred transshipments in EEZs are also unreliable because reefers may loiter for legitimate reasons near the shore as discussed above; furthermore, the RFMO ban may not be applicable in EEZs since country-specific regulations supercede RFMO regulations. We obtained EEZ boundaries from Marine Regions (2018). Second, not all transshipments are illegal, even in regions with a ban. In particular, vessels can receive prior authorization to tranship on the high seas. The Western & Central Pacific Fisheries Commission publishes a list of vessels that are authorized to transship in areas governed by RFMOs (see WCPFC 2018). Authorization periods start in 2008 and sometimes go until 2023. We exclude 1,079 transshipments involving reefers that were authorized to transship at the time of the event.

After pre-processing, we obtained 20,546 inferred unauthorized transshipments on the high seas; their locations are shown in Fig. 2. EEZ regions are shaded grey, ocean regions that are part of an RFMO with no transshipment ban are shaded light blue, and remaining ocean regions are shaded dark blue. Note that transshipment activity appears to be clustered in certain locations, either around EEZ borders or due to location-specific factors (*e.g.,* environmental conditions and catch density vary regionally). It is unclear what drives this clustering behavior (Kroodsma et al. 2017), so we account for location-specific fixed effects by studying the time trend of inferred transshipment behavior conditioned on location, in regions with and without a ban (see §3.2).

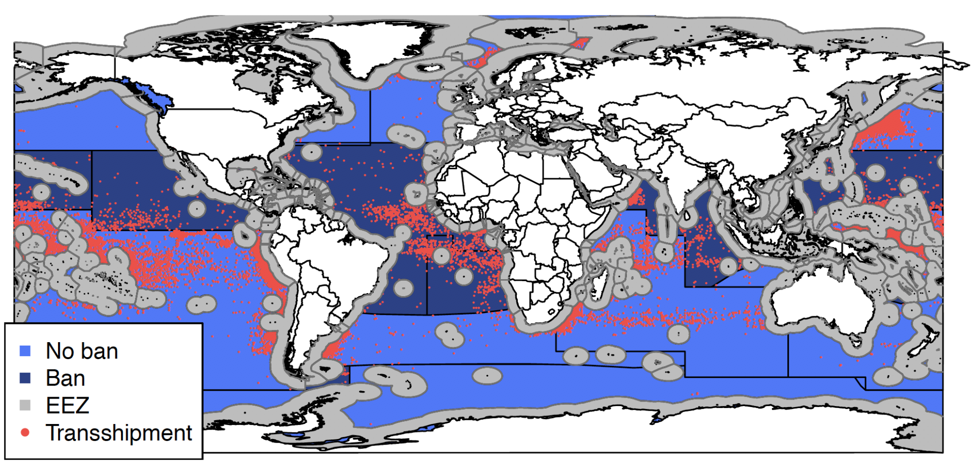

Figure 2 RFMO regions with or without a transshipment ban, overlaid with inferred unauthorized transshipment events (2012-2017). Grey regions indicate Exclusive Economic Zones.

2.2.2. Gaps in AIS signals. There may be "gaps" in the transmission of AIS signals, in which case vessels "go dark." Such a gap occurs when the vessel operator turns off its AIS transponder for a period of time, or when there is lapse in satellite coverage at that time and location. The first is illegal strategic behavior, while the second is a natural cause, and it is currently not possible to tell the difference. The former introduces a systematic bias in the inferred transshipments data —

if a vessel strategically turns off its AIS transponder during a transshipment, this transshipment will not be captured as an inferred transshipment since there was no AIS signal.

To assess the extent to which this bias may affect our results, we obtained a database of gaps in AIS signals emitted by reefers at-sea from GFW. This data allows us to observe the position of the vessel when it lost and re-gained signal.

It is common for vessels to have small gaps in their AIS signals due to temporary lapses in satellite coverage. However, a vessel needs to go dark for at least several hours to perform a transshipment entirely in the "dark." We primarily consider AIS gaps that last 4.75 hours or longer, since these may plausibly mask transshipments. This cutoff was chosen based on conversations with GFW — observer reports of transshipments in ICCAT suggest that the active transfer of substantial fish catch requires at least 3 hours (GFW 2017), and another 2 hours may be required for vessel maneuvering (*i.e.,* setting up and removing cranes) prior to and after transshipment. This timeframe is likely to be an under-estimate of the amount of time a reefer loiters during a rendezvous, since it does not consider multiple transshipments conducted in immediate succession; to address this concern, we perform a robustness check with a larger cutoff of 7.5 hours and find qualitatively similar results (see Appendix C.2).

2.3. Spatial Fishing Activity Data

While AIS data allows us to reliably study reefer behavior, it cannot reliably be used to study fishing activity because, as mentioned previously, fishing vessels are rarely equipped with AIS transponders. Instead, we use satellite images to study fishing activity from space. The Visible Infrared Imaging Radiometer Suite (VIIRS) day/night band on the the Suomi National Polar Partnership satellite collects low-light satellite images at night. Elvidge et al. (2015) pre-processed these images to detect lit fishing vessels at night, noting that "monthly summary data can be used to track spatial and temporal shifts in fishing grounds." This data does not allow us to track or assign unique identifiers to vessels, but it allows us to detect aggregated fishing vessel presence to analyze relative fishing activity over time in regions with and without transshipment bans.

Fig. 3 shows the density of VIIRS-detected vessels in Asia in 2016. We study this specific region from 2012-2016, because this is the only location where VIIRS time-series data was available.

2.4. Landing Price Data

We obtained data on the volume and value of fish landings for each RFMO over time for different fish species from the Sea Around Us project (Daniel and Zeller 2015, Cullis-Suzuki and Pauly 2010). The Sea Around US project assembles this data based on data from the UN Food and Agriculture Organization (UN FAO). While data from the UN FAO is imperfect, it is considered the most reliable data source on food and agriculture at the global scale. Landing value is measured

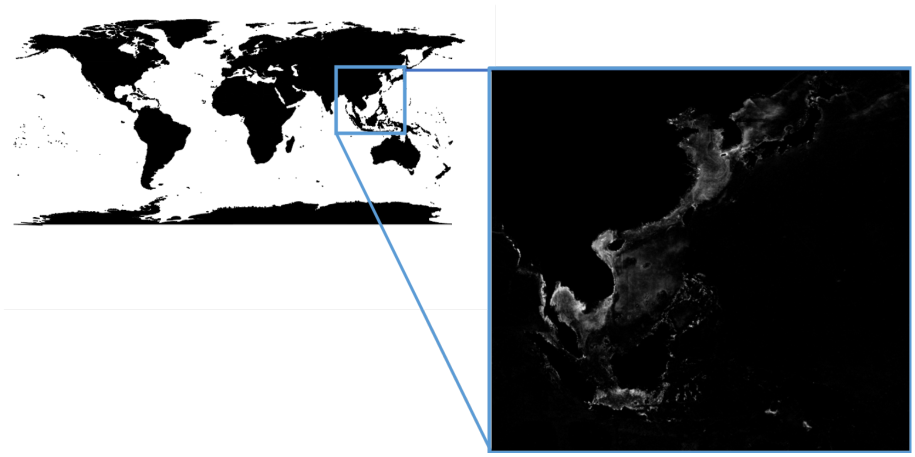

Figure 3 **VIIRS-based boat detection in Asia in 2016.**

in 2010 US dollar equivalents (Sumaila et al. 2007). We compute average landing prices by dividing the total landing value by total landing volume, and consider prices by RFMO across 30 different functional groups (based on fish species and size of catch) in 2000-2014. Importantly, unlike the satellite datasets discussed so far, this data allows us to study prices both before and after the implementation of transshipment bans.

3. Methods

Our analysis relies on observational data, and our methods therefore seek to correct for location-specific effects and alleviate potential endogeneity concerns.

3.1. Unit of Analysis

In most of our analyses, we partition the high seas into discrete locations.

Since transshipment events appear geographically clustered (see Fig. 2), our primary approach defines relevant locations on the high seas using k-means clustering (MacQueen et al. 1967) on our inferred transshipment events. In doing so, we aim to obtain a clustering that balances the dual objectives of (1) the total number of clusters (as each cluster serves as a unit of observation), and (2) the number of events within each cluster (as having too few events in a cluster would make estimates within each cluster unreliable). We therefore set the desired number of clusters to be the square root of the total number of events. In a post-processing step, we remove clusters that are unreliable because they either have too few points or cover too large a distance. Specifically, we drop clusters that have fewer than 30 events between 2012-2017, as well as clusters where the average squared distance between the centroid and any point in the cluster is larger than 50 degrees squared. Using this definition and the `cluster` package in R, we obtain 159 eligible clusters, where 44 clusters are in regions of the high seas governed by a transshipment ban and 115 clusters are

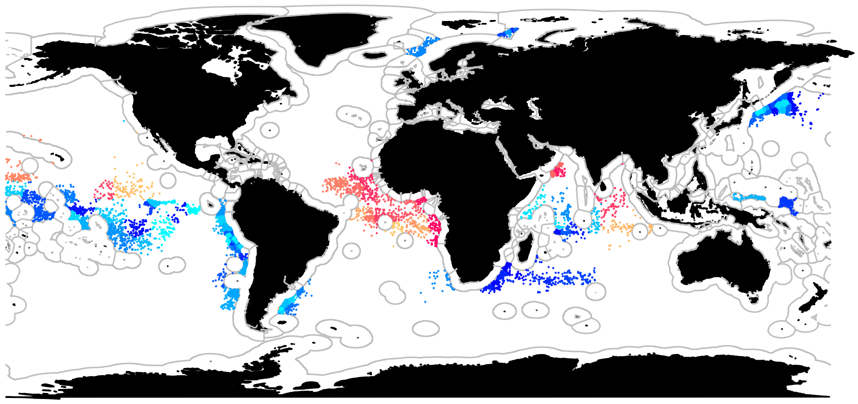

Figure 4 Clusters of inferred unauthorized transshipments on the high seas, in regions where a transshipment ban is in effect (orange palette) and where no ban is in effect (blue palette).

not. These clusters are mapped in Fig. 4. In Appendix B.1, we test the sensitivity of our main result to different clustering choices, and find similar results.

An alternative and more traditional approach is to define locations on the high seas using grid cells. This may lead to noisier estimates since it does not account for the natural clustering of transshipment activity. In Appendix D, we reproduce all our results using discrete grid cells of 0.5×0.5 degrees in latitude and longitude, and find similar results as well.

It is important to note that clusters or grid cells in the same RFMO may have correlated heteroskedastic error due to unobserved RFMO-specific variables. To account for this, we use cluster-robust standard errors (clustered at the RFMO level) in all relevant regressions to allow arbitrary heteroskedasticity and within-RFMO correlation.

3.2. Treatment Variable and Outcome

Given a location ℓ on the high seas, our treatment variable B_ℓ is an indicator of whether that location is governed by a transshipment ban. However, RFMO boundaries are not mutually exclusive, and so many locations on the high seas may fall under the jurisdiction of multiple RFMOs, of which one may have a ban while the others may not. In these cases, if a vessel's flag country is a signatory member of the RFMO without a ban (and not a signatory member of the RFMO with a ban), then the vessel may be free to legally transship in that location (Ewell et al. 2017). Thus, we define our treatment variable $B_\ell = 1$ if and only if *every* RFMO that governs location ℓ has a ban. Our earlier depictions of ban regions in Fig. 2 and Fig. 4 are based on this definition. Note that this implies that our estimated treatment effect of a transshipment ban is likely conservative; some vessels in no-ban regions may actually be subject to a ban in these regions (depending on the flag country) — thus, no-ban regions may in practice partially benefit from the treatment (ban), thereby under-estimating any improvements in outcomes gained through the treatment.

Most of our outcome variables are based on transshipment activity, AIS gap activity or fishing activity. These responses are all functions of the location, the year, and the presence of a ban. For instance, transshipment or fishing activity may depend on the location due to environmental conditions, and AIS gaps may vary due to location-dependent satellite coverage. Ideally, our regressions would include location fixed effects; however, since we only have access to post-ban data (with the exception of our analysis of landing prices), location fixed effects would be collinear with our treatment variable. We therefore instead study the impact of ban status on the *yearly change* in activity rates for any *fixed* location. Note that we have pre-ban data for landing prices, and so we take a more traditional differences-in-differences approach for our analysis of landing prices.

3.3. Endogeneity

Performing our analyses on observational data may be problematic if the ban is an endogenous variable, *e.g.,* the ban was passed in regions where the transshipment rates were already decreasing over time. A standard differences-in-differences analysis is infeasible in this context because data prior to the enactment of transshipment bans is unavailable — data on inferred transshipments only exists for years after 2012, but no new geographic bans have been enacted since 2009. We therefore use instrumental variables to alleviate endogeneity concerns.

Our instruments are national personal income and sales tax rates averaged across member countries of an RFMO. Taxation-related instruments are a common instrument for regulation, since they affect the likelihood of increased governance without directly correlating with the specific policy being considered (see, *e.g.,* Bastani et al. 2019). We obtained country-level tax data from `https://tradingeconomics.com/`. Higher personal income tax rates (*i.e.,* progressive tax) and lower sales tax rates (*i.e.,* regressive tax) signify stronger government regulation and enforcement. We find that RFMOs with member countries that have lower income tax rates and higher sales tax rates are less likely to institute a transshipment ban. However, the tax rates of member countries are unlikely to have a direct relationship with (i) the unobserved variations in environmental factors or fishing patterns that make transshipments more or less necessary in certain RFMOs, or (ii) the outcome of transshipment rates on the high seas. Thus, we argue that our instruments satisfy the exclusion restriction. To provide support to our chosen instruments, we perform the standard validity test for weak identification under robust RFMO-level clustering (we report the Craag-Donald Wald F-statistic based on a 5% Wald test); in all cases, the result is well above the Stock-Yogo critical values for the maximal IV size (19.93 at the 10% level), indicating that our instruments are not weak. When possible, we also perform an over-identification test of our instruments (we report the χ^2 p-value corresponding to the Hansen J statistic), and an endogeneity test of our treatment variable; in all cases, we do not find evidence that the exclusion restrictions are violated, or that the treatment variable is endogenous. The latter result suggests that the transshipment ban may not be endogenous, which matches our finding of similar treatment effects for both the instrumented and non-instrumented regressions.

4. Results

Our main findings are as follows.

4.1. Transshipment bans significantly reduce growth in transshipment rates

Fig. 5 plots unauthorized inferred transshipments on the high seas over time.[1] The trend lines suggest that transshipments are steeply increasing in regions without a transshipment ban, and only mildly increasing in regions with a ban.

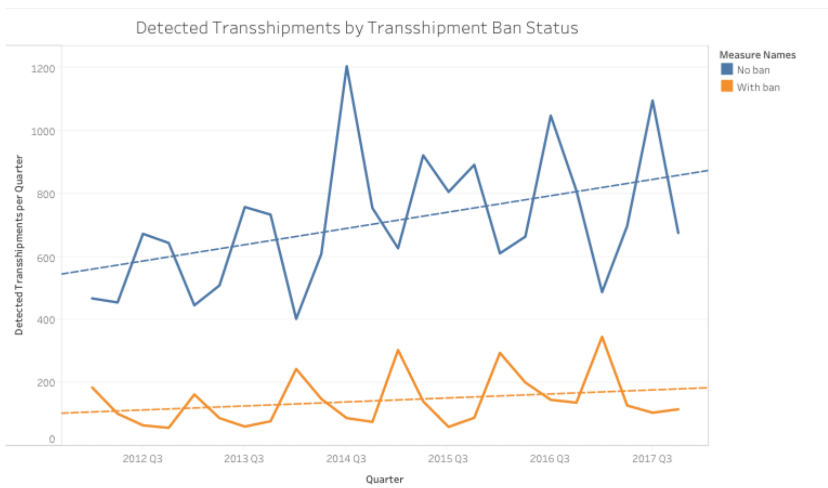

Figure 5 Number of inferred unauthorized transshipments on the high seas from 2012-2017, in regions where a transshipment ban is in effect (orange) and where no ban is in effect (blue). Dashed lines depict the linear trends.

Specification: Let $T(\ell, y)$ denote the number of inferred transshipments in location ℓ in year y. Our outcome is the yearly change in transshipment rates: $C^T_{\ell,y} \equiv \frac{T(\ell,y)-T(\ell,y-1)}{T(\ell,y-1)}$. We then regress

$$C^T_{\ell,y} = \beta B_\ell + \overline{\beta}_{FE}\overline{Y} + \beta_0 + \varepsilon_{\ell,y} \,,$$

where B_ℓ is our treatment variable, the vector $\overline{Y}$ contains yearly fixed effect dummies, β_0 is an intercept term, and $\varepsilon_{\ell,y}$ is the error term. The coefficient of interest β represents the effect of a transshipment ban on the yearly change in transshipment rates. Let the vector $\overline{Z}$ denote our

[1] Note that seasonal trends in the number of inferred transshipments are anti-correlated across regions with and without a ban. This is likely because fishing activity shifts spatially during the year based on fish migration patterns (see Fig. 3 in Kroodsma et al. 2018), and ban regions are located along the equator while no-ban regions span locations closer to the North and South poles (see Fig. 2). This does not affect our analyses, which are at an annual level.

instruments. We also perform a two-stage least squares regression (2-SLS), where we instrument our potentially endogenous treatment variable B_ℓ:

$$\text{1st stage: } B_\ell = \overline{\beta}_z \overline{Z} + \overline{\beta}^0_{FE}\overline{Y} + \beta^0_0 + \varepsilon^0_{\ell,y}\,, \qquad \text{2nd stage: } C^T_{\ell,y} = \beta \hat{B}_\ell + \overline{\beta}_{FE}\overline{Y} + \beta_0 + \varepsilon_{\ell,y}\,.$$

The results are shown in Table 2. Matching the trends observed in Fig. 5, we find that the presence of a transshipment ban reduces the yearly growth in inferred transshipment rates in a given location by 59% under the non-instrumented regression, and 58% under the instrumented regression. This result is consistent when varying the length of a loitering event required to qualify as an inferred transshipment (Appendix B.2), the clusterings that define our unit of analysis (Appendix B.1), and when using gridcells rather than clusters as the unit of analysis (Appendix D).

It is important to note that this reduction is relative to the growth in transshipment rates in locations without a ban. In fact, the general trend shows increasing transshipment rates over time in regions with and without a ban, since the coefficients of the intercept and yearly fixed effects are positive and, jointly, larger in magnitude than the treatment coefficient. In other words, transshipment bans appear to successfully dampen the growth in inferred transshipment rates, but do not eliminate it.

Outcome: Yearly growth in inferred transshipment rates

Variable	**(1) Regression**		**(2) 2-SLS IV Regression**	
	Estimate	**Std Error**	**Estimate**	**Std Error**
(Intercept)	0.61**	0.12	0.61**	0.10
Is 2014	0.22	0.22	0.22	0.22
Is 2015	0.19	0.21	0.20	0.20
Is 2016	0.36*	0.16	0.36*	0.15
Is 2017	0.32	0.26	0.32	0.25
Ban	**− 0.59***	**0.24**	**−0.58****	**0.13**
*$p<0.05$, **$p<0.01$	$N=670,\ R^2=0.01$		$N=670,\ R^2=0.01$	

IV Weak Identification Test: Cragg-Donald Wald F-statistic = 611
IV Over-Identification Test: $p=0.2$, Treatment Endogeneity Test: $p=0.3$

Table 2 Regression results with cluster-robust standard errors for yearly growth in inferred transshipment rates per cluster as a function of transshipment ban status.

4.2. Bans do not lead to geographical evasion

Instead of foregoing transshipment, a reefer could respond to a geographic ban through geographical evasion, *i.e.,* shift transshipments to a region without a ban. Either response would lead to a finding of reduced growth in transshipment rates in regions with a ban relative to regions without a ban. However, they yield very different conclusions: if vessels are simply geographically evading the ban, it is unclear whether the ban is effective.

We refer to the borders between regions of the high seas where our treatment variable differs as the *ban border*. Under geographical evasion, transshipments originally intended to occur in regions with a ban would shift across a ban border to a region without a ban. These vessels would naturally prefer to travel shorter distances, and so the resulting additional transshipments in no-ban regions are far more likely to occur closer to the ban border, inducing a change in the distribution of transshipment activity in no-ban regions. Thus, conditioned on finding reduced growth in transshipment rates in regions with a ban, if vessels are responding through geographical evasion, then we would additionally observe increasing growth in transshipment rates near ban borders in regions without a ban.

We test this hypothesis by adding an interaction term between no-ban status for a location and its distance to a ban border; if vessels are geographically evading the ban, this interaction term would have a large negative coefficient, as inferred transshipment rates would be increasing near the ban border in no-ban regions, and decaying away from the border. Since this effect should only be present in the vicinity of a ban border, we only apply this term to inferred transshipment clusters within a pre-defined number of grid cells of the ban border. A distance of 5 grid cells (≈ 550 km for clusters near the equator) would take a reefer about 24 hours to travel, since reefers tend to travel at about 12 knots; thus, we argue that this distance is likely to be an *upper bound* on how far a reefer would be willing to travel past the ban border to geographically evade the ban. We find qualitatively similar results with smaller cutoffs (Appendix B.3).

Specification: For inferred transshipment clusters within x grid cells of a ban border, let D_ℓ^x denote the shortest distance between the cluster and the ban border. For clusters farther than x grid cells of a ban border, we set $D_\ell^x = 0$. Using the same notation as before, we regress

$$C_{\ell,y}^T = \beta_1 B_\ell + \beta_2 [D_\ell^x \times (1 - B_\ell)] + \beta_3 D_\ell^x + \overline{\beta}_{FE} \overline{Y} + \beta_0 + \varepsilon_{\ell,y}\,.$$

The parameters of interest are β_1 and β_2, where β_2 represents the extent of geographical evasion.

Table 3 shows our regression results for $x = 5$, and Appendix B.3 reports qualitatively similar results for smaller x. While the coefficient β_2 is nominally negative, this effect is not statistically significant, suggesting that geographic evasion is likely insignificant.[2] We also find consistent results when using gridcells rather than clusters as the unit of analysis (Appendix D).

One may be concerned that geographical evasion may be *economically significant* even if it is not statistically significant in our regressions. To this end, we performed our analysis from §4.1 on visible compliance with the ban, excluding all inferred transshipment clusters that are potentially

[2] Table 3 also reports an instrumented version of this regression, but we caution that these 2-SLS estimates are numerically unstable since there are very few transshipment clusters near the ban border. However, the lack of inferred transshipment activity near the ban border supports the hypothesis that geographic evasion is likely insignificant.

Outcome: Yearly growth in inferred transshipment rates

Variable	**(1) Regression**		**(2) 2-SLS IV Regression**	
	Estimate	**Std Error**	**Estimate**	**Std Error**
(Intercept)	0.76**	0.14	0.78**	0.13
Is 2014	0.24	0.15	0.24	0.14
Is 2015	-0.02	0.20	-0.02	0.18
Is 2016	0.20	0.18	0.20	0.16
Is 2017	0.11	0.17	0.11	0.16
Dist	-0.05*	0.02	-0.03	0.04
Dist × No-Ban	**−0.14**	**0.07**	**−0.16**	**0.08**
Ban	**−0.53****	**0.19**	**−0.62****	**0.18**
*$p<0.05$, **$p<0.01$	$N=623,\ R^2=0.01$		$N=623,\ R^2=0.01$	

IV Weak Identification Test: Cragg-Donald Wald F-statistic = 521
IV Over-Identification Test: $p=0.2$

Table 3 Regression results with cluster-robust standard errors for yearly growth in inferred transshipment rates per cluster as a function of transshipment ban status and distance from ban border ($x=5$).

a by-product of geographical evasion (*i.e.,* clusters in no-ban areas within five grid cells from a ban border). In Appendix B.3, we find that our results remain qualitatively similar, supporting the conclusion that geographic evasion does not erode the effectiveness of the ban. We believe that this is because transshipments often occur far from the ban border (see Fig. 2), and as a consequence, it may not be economically worthwhile for vessels to voyage across a ban border, especially under limited monitoring.

4.3. Vessels do not evade the ban by going dark

The previous regressions examined *visible* transshipments. However, vessel operators can strategically "go dark" while performing a transshipment by turning off their AIS transponders. Such transshipments will not be captured in our dataset of inferred transshipments, since they would be masked by a (long) gap in AIS signals. Thus, we may be concerned about increased AIS gap rates — as a consequence of vessel operators deliberately masking transshipments — in regions with a transshipment ban.

Recall that *short* AIS gaps are common due to temporary lapses in satellite coverage. Since we are interested in gaps that may plausibly mask a transshipment, we examine gaps that are at least 4.75 hours long (see discussion in §2.2). Since this cutoff is likely to be an under-estimate of the length of a loitering event required to mask a transshipment, we examine longer AIS gap lengths in Appendix C.2 and find qualitatively similar results.

We can perform this analysis in two ways: consider the *starting* location of the vessel at the time when it went dark, or the *ending* location when it started transmitting an AIS signal again; in reality, the vessel is likely somewhere between these two locations while it is dark. Again, our results are qualitatively similar regardless of which location we use (see Appendix C.1).

Fig. 6 shows the time trend for these AIS gaps, stratified based on whether the starting location is in a RFMO with a transshipment ban. The trends suggest that gaps in AIS signals are decreasing over time, which contrasts with increasing transshipment rates.

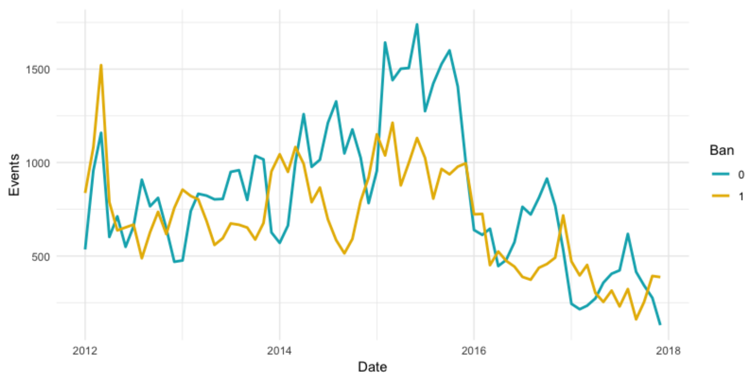

Figure 6 AIS gap events (gap $\geq$ 4.75 hours), in RFMOs with (yellow) and without (blue) transshipment ban.

Specification: Let $G(\ell, y)$ denote the number of AIS gap events in location ℓ in year y. Our outcome is the yearly change in AIS gap rates: $C^G_{\ell,y} \equiv \frac{G(\ell,y)-G(\ell,y-1)}{G(\ell,y-1)}$. Using the same notation as before, we regress

$$C^G_{\ell,y} = \beta B_\ell + \overline{\beta}_{FE}\overline{Y} + \beta_0 + \varepsilon_{\ell,y}\,.$$

The coefficient of interest β represents the effect of a ban on the yearly change in AIS gap rates. We also use 2-SLS to instrument our potentially endogenous treatment variable:

$$\text{1st stage: } B_\ell = \overline{\beta}_z\overline{Z} + \overline{\beta}^0_{FE}\overline{Y} + \beta^0_0 + \varepsilon^0_{\ell,y}\,, \qquad \text{2nd stage: } C^G_{\ell,y} = \beta\hat{B}_\ell + \overline{\beta}_{FE}\overline{Y} + \beta_0 + \varepsilon_{\ell,y}\,.$$

We find that there is no significant effect of the presence of a ban on the yearly growth in AIS gap rates (see Table 4). In fact, there is a nominal but statistically insignificant reduction in AIS gap rates in regions with a ban, which suggests that vessels are not strategically going dark to mask transshipments in regions with a ban. This regression examined vessels' starting locations and short AIS gaps, but we find qualitatively similar results when using vessels' ending locations (see Table 12 in Appendix C.1) and when studying longer gaps that are over 7.5 hours (see Table 13 in Appendix C.2). We also find consistent results when using gridcells rather than clusters as the unit of analysis (Appendix D).

4.4. Bans lead to fewer transshipments per unit of fishing activity

We use VIIRS satellite images in the Asia region (see Fig. 3) to study the time trend of fishing activity in regions with and without a transshipment ban. Unlike our previous datasets which reported discrete events at certain locations on the high seas, this data is reported as an intensity per pixel. Correspondingly, we define our locations ℓ at the pixel level.

Outcome: Yearly growth in AIS gap rates

Variable	(1) Regression		(2) 2-SLS IV Regression	
	Estimate	Std Error	Estimate	Std Error
(Intercept)	0.77**	0.18	0.76**	0.16
Is 2014	−0.23	0.17	−0.23	0.17
Is 2015	−0.08	0.33	−0.08	0.32
Is 2016	−0.98**	0.15	−0.98**	0.15
Is 2017	−0.82**	0.15	−0.82	0.14
Ban	**−0.24**	**0.15**	**−0.23**	**0.26**
*$p<0.05$, **$p<0.01$	$N=2,047,\ R^2=0.03$		$N=2,047,\ R^2=0.03$	

IV Weak Identification Test: Cragg-Donald Wald F-statistic = 401
IV Over-Identification Test: $p=0.4$, Treatment Endogeneity Test: $p=0.8$

Table 4 Regression results with cluster-robust standard errors for yearly growth in AIS gaps per starting cluster as a function of transshipment ban status.

Specification: Let $F(\ell,y)$ denote the fishing activity in location ℓ in year y. Our outcome is the yearly change in fishing activity: $C^F_{\ell,y} \equiv \frac{F(\ell,y)-F(\ell,y-1)}{F(\ell,y-1)}$. Using the same notation as before, we regress

$$C^F_{\ell,y} = \beta B_\ell + \overline{\beta}_{FE}\overline{Y} + \beta_0 + \varepsilon_{\ell,y}\,.$$

The coefficient of interest β represents the effect of a ban on the yearly change in fishing activity. We also use 2-SLS to instrument our potentially endogenous treatment variable:

$$\text{1st stage: } B_\ell = \overline{\beta}_z\overline{Z} + \overline{\beta}^0_{FE}\overline{Y} + \beta^0_0 + \varepsilon^0_{\ell,y}\,, \qquad \text{2nd stage: } C^F_{\ell,y} = \beta\hat{B}_\ell + \overline{\beta}_{FE}\overline{Y} + \beta_0 + \varepsilon_{\ell,y}\,.$$

We find that the presence of a transshipment ban reduces the yearly growth of fishing activity by 6% under the non-instrumented regression, and 7% under the instrumented regression. Note that this reduction is *relative* to fishing activity in locations without a ban. In fact, the general trend shows increasing fishing activity over time in regions with and without a ban, since the coefficient of the intercept term in Table 5 is positive and larger in magnitude than the treatment coefficient β. In other words, transshipment bans dampen increasing fishing activity on the high seas.

These results suggest that concerns that the ban may raise fishing vessel costs (which would explain the reduction in fishing activity) may be well-founded. However, the 7% reduction in the yearly growth of fishing activity is far smaller than the 58% reduction in yearly growth in inferred transshipment rates from §4.1. A simple computation yields that the normalized transshipment rate per unit of fishing pressure (averaged across years) is 3.3 in regions without a ban and 1.5 in regions with a ban; this difference is statistically significant, suggesting that the ban significantly changed reliance on the practice of transshipments on the high seas.

4.5. Bans lead to higher raw material prices

Unlike our other data sources, we have price data from before and after the implementation of bans. Therefore, instead of examining change over time post-ban, we take a difference-in-differences

Outcome: Yearly growth in Fishing Activity

Variable	**(1) Regression**		**(2) 2-SLS IV Regression**	
	Estimate	**Std Error**	**Estimate**	**Std Error**
(Intercept)	0.15**	0.01	0.15**	0.01
Is 2014	0.14**	0.01	0.14**	0.00
Is 2015	0.16**	0.01	0.16**	0.00
Is 2016	0.03	0.01	0.03	0.01
Ban	**$-$0.06****	**0.01**	**$-$0.07****	**0.01**
*$p < 0.05$, **$p < 0.01$	$N = 3,1001,\ R^2 = 0.01$		$N = 3,1001,\ R^2 = 0.01$	

IV Weak Identification Test: Cragg-Donald Wald F-statistic $= 3.4 \times 10^4$

Table 5 Regression for yearly growth in fishing activity per grid cell as a function of transshipment ban status.

approach and include fixed effects for RFMOs. Fig. 7 shows the median price per ton of catch across functional groups and RFMOs with and without a transshipment ban. The grey shaded area represents the years during which transshipment bans went into effect for RFMOs that implemented them. The trends suggest that catch prices were very similar across RFMOs before the implementation of bans (thereby satisfying the parallel trends assumption prior to treatment); however, after implementation, RFMOs with a transshipment ban have higher median landing prices.

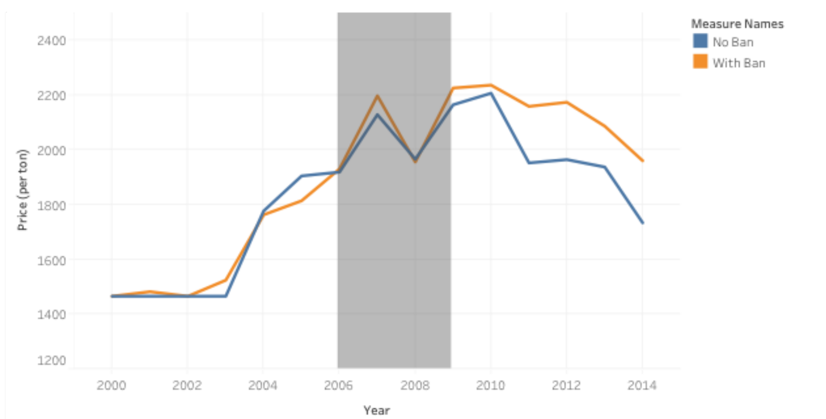

Figure 7 The median price per ton of catch across RFMOs without (blue) and with (orange) a transshipment ban. RFMOs implemented transshipment bans during the year range 2006-2009 (shaded area).

Specification: Let $P_{s,r,y}$ denote the median landing price of catch in functional group s in RFMO r and year y. We then regress

$$P_{s,r,y} = \beta B_{r,y} + \overline{\beta}^{s}_{FE}\overline{S} + \overline{\beta}^{r}_{FE}\overline{R} + \overline{\beta}_{FE}\overline{Y} + \beta_0 + \epsilon_{s,r,y}\,,$$

where $B_{r,y}$ is the presence of a ban in RFMO r in year y (our treatment variable), the vectors $\{\overline{S}, \overline{R}, \overline{Y}\}$ contain fixed effect dummies for the functional group, RFMO and year respectively, and $\varepsilon_{s,r,y}$ is the error term. Note that different RFMOs implemented bans in different years, as captured in $B_{r,y}$. The coefficient of interest β represents represents the effect of a ban on landing prices.

The summarized results are shown in Table 6 (see Table 17 in the Appendix for results showing all fixed effects). We find that a transshipment ban increases landing prices of catch by \$161/ton, which is a 3.2% price increase compared to the intercept of \$5,040/ton. These results match the trends in Fig. 7.

Outcome: Median Landing Prices of Fish Catch

Variable	**Estimate**	**Std Error**
(Intercept)	$5,040^{**}$	532
Ban Implementation	161^{**}	59
Fixed effects for functional group, RFMO, year: Yes		
$^{*}p<0.05$, $^{**}p<0.01$	$N=6,078, R^2=0.43$	

Table 6 Difference-in-differences regression results and cluster-robust standard errors for yearly fish catch landing prices under the treatment of transshipment ban implementation.

5. Discussion & Concluding Remarks

We set out to understand whether a ban with severe limitations on monitoring would (1) induce visible compliance, (2) be effective, and (3) lead to a significant increase in raw material prices. Because the response depends on a vast array of context-specific parameters, this is an empirical question. We study this question in the context of vessel-to-reefer transshipment bans, which provide a unique setting to study these questions jointly, by exploiting a unique dataset about *past* vessel behavior that became available *after* the fact.

We find that while the ban does not stop the practice of vessel-to-reefer transshipments, it is able to significantly dampen growth in transshipments relative to regions without a ban. Hence, while one will continue to see problematic cases arise, the ban is able to induce a significant amount of *visible compliance*. In addition, we find that there is limited evidence that vessels mask their behavior (by turning off their AIS devices) or evade the jurisdiction of the ban (by shifting to other geographical regions). Hence, the ban also seems to be *effective*.

Finally, we find that the ban leads to an increase of 3% in raw material landing prices. However, it is likely that this difference is insufficient to offset the increased costs from inefficiencies due to reduced transshipment rates. Hence, while the increase in landing prices seems significant in a low-margin industry, we cannot rule out the possibility that the transshipment ban may have come at the cost of increasing other harmful behavior like labor exploitation to improve vessel profits. Understanding the impact of transshipment bans on forced labor is a priority for future research.

Our results are specific to the context of transshipments on the high seas. To understand its external validity, it is important to consider why the policy appears effective, and the circumstances under which this may no longer be the case.

One hypothesis for why the policy works is that the transshipment ban may provide a simple, clear guideline that delineates good and bad behavior for vessel owners. Legal ambiguities, coupled with poor education, can confuse supply chain actors about what constitutes problematic behavior, and lack of knowledge drives them to adopt "convenient" interpretations of the law in practice. In a study of initiatives to improve working conditions in Indonesian garment factories, Amengual and Chirot (2016) discuss how interventions can play an important role by *"diffus[ing] information about legal processes so that factory managers and unions have knowledge about the formal rules of the game... reducing information costs for factories sorting out complex and shifting policies, especially when local officials are unreliable... [these] mechanisms correspond to instances of unresolved ambiguities in rules, due either to actors' self-servingly amplifying conflicting interpretations to advance their interests or to genuine legal fuzziness."* Similarly, fishing on the high seas is subject to a complex and dynamic set of regulations, especially because no single country has jurisdiction on these waters and there are multiple stakeholders involved. The simplicity and clarity of the transshipment ban may have aided vessel owners in distinguishing and avoiding the behavior.

Second, the fact that few vessels have been caught and penalized for transshipping is frequently used as evidence for a ban's failure. For example, Zimmer (2017) criticizes the effectiveness of one regulation that proposes to blacklist fishing vessels that transship, arguing that very few vessels have been blacklisted despite significant transshipping rates (WCPFC 2018). However, the game-theoretic literature (see, *e.g.,* Dionne et al. 2009) shows that regulations can act as a deterrent to bad behavior, thereby reducing the probability of catching bad behavior. This does not mean that the policy is ineffective. In fact, we find that despite increasing incidences of transshipments worldwide, there is a significant reduction in transshipment growth rate in regions with a transshipment ban.

Third, the marginal cost of not complying with the ban may be high. For example, Dizon-Ross et al. (2017) study subsidy programs for bed nets in sub-Saharan Africa, where there is widespread concern that poor governance (limited health worker accountability) may undermine the effectiveness of such programs. In particular, there were concerns that workers may attempt to extort additional payments, leak subsidies to ineligible participants, or shirk their responsibilities of distribution. Yet, Dizon-Ross et al. (2017) empirically find that these policies are indeed effective, and that the majority of subsidies are distributed as intended despite ex-ante expectations that health workers may perform poorly under limited monitoring and enforcement. The authors argue that small frictions can significantly decrease corruption when marginal benefits are low. In the case of bed nets, there were low gains to financial corruption and low health worker effort required to abide by the rules. Similarly, given that vessels were already far away from ban borders, the marginal cost of of evading the ban by traveling to an area without a ban seemed high. Instead, it seems to have made more sense to adapt the fishing business model by reducing reliance on the practice of transshipments. Eliminating transshipments entirely, however, may be more costly, and would require further study.

Understanding under what circumstances the ban may remain effective will require further theoretical analyses that are outside the scope of this paper. Such a study could examine, for example, different restrictions on cost ratios, economic conditions, geographies, the extent of available monitoring, etc. In a few years, it will also become possible to empirically study the impact of the new monitoring abilities created by GFW. This would improve our understanding of the impact of improved real-time monitoring through satellite remote sensing capabilities.

Pressure has also been shifting to seafood buyers to stop sourcing from suppliers that use transshipments on the high seas, effectively imposing a *"supply chain ban."* Major buyers such as Thai Union (parent to the U.S. brand Chicken of the Sea) and Nestle have become early adopters of a supply chain ban throughout their supply chain (Thai Union Group 2017, Nestle 2017). Future theoretical work could analyze in more detail the relationships between a geographical ban — as we study here — and a supply chain ban. Such work could provide insight on the extent to which our results from geographical bans inform the potential effectiveness of supply chain bans imposed by seafood buyers.

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Appendix

A. Background

A.1. Vessel Types Targeted by Bans

Transshipment bans in most RFMOs only prohibited transshipments for certain types of fishing vessels. Table 7 below shows the specific vessel types targeted by transshipment bans in each RFMO.

RFMO	**Banned vessels**
SEAFO	All vessels
IATTC	All purse seine vessels & small longline vessels
ICCAT	All vessels except large-scale pelagic long-line vessels
IOTC	All vessels except large-scale pelagic long-line vessels
GFCM	All vessels except large-scale pelagic long-line vessels & all transshipments at sea of bluefin tuna
WCPFC	All purse seine vessels.

Table 7 Summary of vessel types targeted by different transshipment bans.

A.2. Satellite Coverage

Fig. 8 depicts GFW's satellite coverage for AIS transponder signals used by reefers (class A transponders), which has remained mostly constant in the period 2012–2017. The index (from 1% to 100%) reflects how good the reception was for vessels in grid cells of 2 degrees × 2 degrees. All areas with poor coverage (shown in red) coincide with EEZs, which are excluded from our analysis. Satellite coverage is largely similar on the high seas in regions with and without a ban. Furthermore, as discussed in §3, since we are evaluating the change in transshipment rates over time in a *fixed* location, regional but *stationary* variations in satellite coverage should not bias our results.

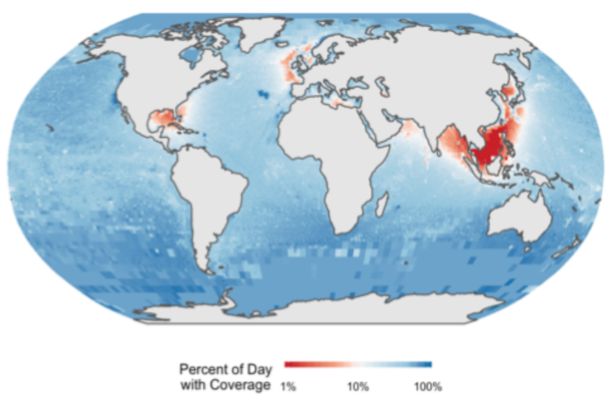

Figure 8 Reception quality of Class A AIS transponders (source: Global Fishing Watch).

B. Robustness Checks

In this section, we report a number of robustness checks on our choice of clustering locations on the high seas, the length of a loitering event required to qualify as an inferred transshipment, and the choice of distance cutoff for geographical evasion.

B.1. Robustness to Choice of Clustering Parameters

We vary the criteria for eligible clusters after running the k-means clustering algorithm. We allow clusters that contain at least $\{30, 40, 50\}$ points and that have an average within-cluster squared distance of $\{10, 50, 100\}$ from the centroid. Naturally, we obtain significantly more eligible clusters when we allow for a smaller minimum number of points and a larger total average within-cluster squared distance; however, allowing clusters with very few points may increase the variance of our outcome variable within a cluster, and allowing clusters that cover very large regions may increase the bias of our outcome variable within a cluster. The resulting treatment effects under the OLS regression are shown in Table 8, and indicate consistent results.

Outcome: Yearly growth in transshipment rates

Min Points per Cluster	**Max Avg Distance**	**Effect Size**	**Std Error**	**# Ban Clusters**	**# No Ban Clusters**
30	10	-0.62*	0.26	32	94
30	50	-0.59*	0.24	44	115
30	100	-0.60*	0.24	45	117
40	10	-0.61*	0.26	29	91
40	50	-0.58*	0.24	39	109
40	100	-0.58*	0.24	39	111
50	10	-0.64*	0.27	27	85
50	50	-0.59*	0.25	34	100
50	100	-0.59*	0.25	34	102

$^{*}p < 0.05$, $^{**}p < 0.01$

Table 8 Summary of treatment effects obtained when varying the criteria for eligible clusters.

B.2. Robustness to Duration of Detected Event

As discussed in §2.2, we may be concerned that there are differences in the length of transshipment events in regions with a ban (*e.g.,* if vessels in regions with a ban engage in shorter transshipments to evade detection by marine patrol). To this end, we obtained additional data from GFW that allows us to vary the definition of an inferred transshipment event by considering different duration cutoffs. Specifically, we allow events where a reefer is traveling at a median speed of less than 2 knots for at least $\{2, 4, 6, 8\}$ hours to qualify as an inferred transshipment event; note that our main definition in the paper — based on the publicly released transshipment dataset by Miller et al. (2018) — uses a cutoff of 8 hours. The resulting treatment effects are shown in Table 9, and indicate consistent results.

B.3. Geographical evasion

As discussed in §4.2, vessels would prefer to travel shorter distances when evading the ban's jurisdiction, and thus, we would expect the additional transshipment activity arising from geographical evasion to only occur in the vicinity of a ban border. Thus, we perform the regression with the same specification but a smaller cutoff of $x = 4$ in Table 10, and find consistent results. We cannot perform the regression with $x \leq 3$ since there are no relevant inferred transshipment clusters in no-ban regions.

Table 11 reports the results of our main regression from §4.1 on visible compliance with the ban, excluding all inferred transshipment clusters that are potentially a by-product of geographical evasion (*i.e.,* clusters in no-ban areas within five grid cells from a ban border). We find that our results remain qualitatively similar, supporting the conclusion that geographic evasion does not erode the effectiveness of the ban.

Outcome: Yearly growth in inferred transshipment rates

Duration	**Effect Size**	**Std Error**	**# Ban Clusters**	**# No Ban Clusters**
2 hours	-0.53**	0.13	110	194
4 hours	-0.43*	0.21	86	137
6 hours	-0.79**	0.10	61	101
8 hours	-0.59**	0.16	39	72

*$p < 0.05$, **$p < 0.01$

Table 9 Summary of treatment effects obtained when varying the minimum duration required for an event to qualify as an inferred transshipment.

Outcome: Yearly growth in inferred transshipment rates

Variable	**(1) Regression**		**(2) 2-SLS IV Regression**	
	Estimate	**Std Error**	**Estimate**	**Std Error**
(Intercept)	0.72**	0.14	0.75**	0.12
Is 2014	0.24	0.15	0.24	0.14
Is 2015	-0.01	0.20	-0.01	0.19
Is 2016	0.21	0.17	0.21	0.16
Is 2017	0.11	0.17	0.11	0.16
Dist	-0.08*	0.02	-0.06	0.04
Dist × No-Ban	**−0.02**	**0.17**	**−0.05**	**0.16**
Ban	**−0.50***	**0.19**	**−0.58****	**0.16**
*$p < 0.05$, **$p < 0.01$	$N = 623$, $R^2 = 0.01$		$N = 623$, $R^2 = 0.01$	

IV Weak Identification Test: Cragg-Donald Wald F-statistic = 563
IV Over-Identification Test: $p = 0.2$

Table 10 Regression results with cluster-robust standard errors for yearly growth in inferred transshipment rates per cluster as a function of transshipment ban status and distance from ban border ($x = 4$).

Outcome: Yearly growth in inferred transshipment rates

Variable	**(1) Regression**		**(2) 2-SLS IV Regression**	
	Estimate	**Std Error**	**Estimate**	**Std Error**
(Intercept)	0.80**	0.12	0.83**	0.10
Is 2014	0.24	0.15	0.24	0.14
Is 2015	-0.10	0.29	-0.10	0.27
Is 2016	0.19	0.18	0.19	0.17
Is 2017	0.04	0.17	0.04	0.16
Ban	**−0.54****	**0.19**	**−0.64****	**0.18**
*$p < 0.05$, **$p < 0.01$	$N = 563$, $R^2 = 0.01$		$N = 563$, $R^2 = 0.01$	

IV Weak Identification Test: Cragg-Donald Wald F-statistic = 566
IV Over-Identification Test: $p = 0.2$, Treatment Endogeneity Test: $p = 0.2$

Table 11 Regression results with cluster-robust standard errors for yearly growth in inferred transshipment rates per cluster as a function of transshipment ban status, after removing transshipments within five grid cells of the ban border in no-ban regions.

C. Dark Vessels

We perform two robustness checks on our results from Table 4, by defining gaps based on their ending (rather than starting) location and defining a longer cutoff for AIS gaps that may mask a transshipment.

C.1. Ending Location

We perform the same regression as in the main paper, but we define the treatment variable (ban status) based on the *ending* location of the AIS gap. Table 12 shows the corresponding results.

Outcome: Yearly growth in AIS gap rate

Variable	**(1) Regression**		**(2) 2-SLS IV Regression**	
	Estimate	**Std Error**	**Estimate**	**Std Error**
(Intercept)	0.57**	0.16	0.47**	0.12
Is 2014	0.08	0.18	0.08	0.17
Is 2015	0.28	0.38	0.28	0.36
Is 2016	−0.78**	0.13	−0.78**	0.13
Is 2017	−0.63**	0.13	−0.63**	0.10
Ban	**−0.30****	**0.11**	**−0.10**	**0.22**
$*p<0.05$, $**p<0.01$	$N=2,062$, $R^2=0.05$		$N=2,062$, $R^2=0.05$	

IV Weak Identification Test: Cragg-Donald Wald F-statistic = 374
IV Over-Identification Test: $p=0.5$, Treatment Endogeneity Test: $p=0.3$

Table 12 Regression results with cluster-robust standard errors for yearly growth in AIS gap rates per ending cluster as a function of transshipment ban status.

C.2. Longer Gaps

Fig. 9 shows descriptive trends for longer AIS gaps (at least 7.5 hours long) as a function of transshipment ban status. We again observe that there does not appear to be increased occurrences of dark events in ban areas. The resulting regression results (Table 13) yield qualitatively similar insights as Table 4.

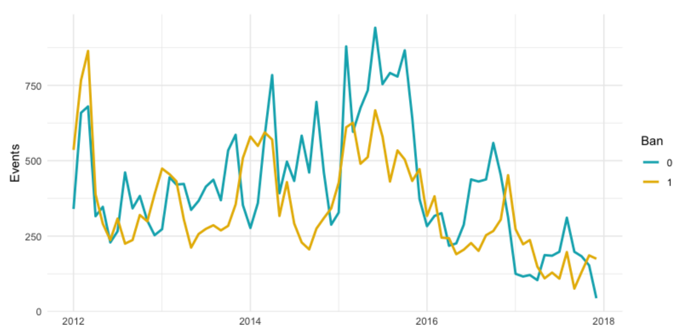

Figure 9 Events where a vessel's AIS signal went dark over 7.5 hours in RFMOs over time in regions where a transshipment ban is in effect (yellow) and where no ban is in effect (blue).

D. Grid-Cell Based Analysis

We can alternatively define locations on the high seas using the more standard unit of grid cells (*i.e.,* equally-sized partitions of the high seas based on latitude and longitude) rather than using clustering. We now report all our main regressions with grid cells that are 0.5×0.5 degrees in latitude and longitude, giving us a total of 5,228 grid cells, where 1,504 cells are under a transshipment ban and 3,724 cells are not. We use the same

Outcome: Yearly growth in long AIS gap rates

Variable	**(1) Regression**		**(2) 2-SLS IV Regression**	
	Estimate	**Std Error**	**Estimate**	**Std Error**
(Intercept)	0.48**	0.19	0.49**	0.18
Is 2014	−0.09	0.18	−0.09	0.18
Is 2015	0.19	0.41	0.19	0.39
Is 2016	−0.74**	0.16	−0.74**	0.15
Is 2017	−0.63**	0.11	−0.64**	0.11
Ban	**−0.08**	**0.13**	**−0.10**	**0.23**
$*p<0.05$, $**p<0.01$	$N=1,371,\ R^2=0.04$		$N=1,371,\ R^2=0.04$	

IV Weak Identification Test: Cragg-Donald Wald F-statistic = 183
IV Over-Identification Test: $p=0.7$, Treatment Endogeneity Test: $p=1.0$

Table 13 Regression results with cluster-robust standard errors for yearly growth in long AIS gaps (>7.5 hours) per starting cluster as a function of transshipment ban status.

regression specifications described in the main paper, and as before, we use cluster-robust standard errors (clustered at the RFMO level) and 2-SLS to instrument our potentially endogenous treatment variable.

Transshipment bans significantly reduce growth in transshipment rates. Table 14 below shows results for the same regression specifications as Table 2, but with locations on the high seas defined by grid cells rather than clustering. We find very similar results using both approaches.

Outcome: Yearly growth in inferred transshipment rates

Variable	**(1) Regression**		**(2) 2-SLS IV Regression**	
	Estimate	**Std Error**	**Estimate**	**Std Error**
(Intercept)	0.86**	0.18	0.85**	0.10
Is 2014	0.66**	0.25	0.64**	0.17
Is 2015	-0.14	0.25	−0.10	0.27
Is 2016	-0.26	0.24	−0.25	0.17
Is 2017	-0.13	0.24	−0.11	0.14
Ban	**−0.54***	**0.25**	**−0.53****	**0.15**
$*p<0.05$, $**p<0.01$	$N=1,666,\ R^2=0.01$		$N=1,666,\ R^2=0.01$	

IV Weak Identification Test: Cragg-Donald Wald F-statistic = 449
IV Over-Identification Test: $p=0.2$, Treatment Endogeneity Test: $p=0.4$

Table 14 Regression results with cluster-robust standard errors for yearly growth in inferred transshipment rates per grid cell as a function of transshipment ban status.

Bans do not lead to geographical evasion. Table 15 shows the same results as Table 3 but with locations on the high seas defined by grid cells rather than clustering. We find very similar results using both approaches.

Vessels do not evade the ban by going dark. Table 16 below shows results for the same regression specifications as Table 4, but with locations on the high seas defined by grid cells rather than clustering. We find very similar results using both approaches.

Outcome: Yearly growth in inferred transshipment rates

Variable	**(1) Regression**		**(2) 2-SLS IV Regression**	
	Estimate	**Std Error**	**Estimate**	**Std Error**
(Intercept)	0.91**	0.12	0.91**	0.10
Is 2014	0.63**	0.18	0.63**	0.16
Is 2015	-0.1	0.30	-0.10	0.28
Is 2016	-0.25	0.19	-0.26	0.18
Is 2017	-0.06	0.16	-0.06	0.14
Dist	-0.09	0.19	0.12	0.18
Dist × No-Ban	**−0.37**	**0.19**	**−0.41**	**0.17**
Ban	**−0.64****	**0.17**	**−0.74****	**0.18**
$^*p<0.05$, $^{**}p<0.01$	$N=1,666$, $R^2=0.02$		$N=1,666$, $R^2=0.02$	

IV Weak Identification Test: Cragg-Donald Wald F-statistic = 427
IV Over-Identification Test: $p=0.2$

Table 15 Regression results with cluster-robust standard errors for yearly growth in inferred transshipment rates as a function of transshipment ban status and distance from border for grid cells ($x=5$).

Outcome: Yearly growth in AIS gap rates

Variable	**(1) Regression**		**(2) 2-SLS IV Regression**	
	Estimate	**Std Error**	**Estimate**	**Std Error**
(Intercept)	0.54**	0.14	0.45**	0.13
Is 2014	0.03	0.10	0.03	0.10
Is 2015	−0.04	0.13	−0.03	0.11
Is 2016	−0.32**	0.11	−0.30**	0.10
Is 2017	−0.09	0.16	−0.08	0.15
Ban	**−0.22****	**0.01**	**−0.06**	**0.15**
$^*p<0.05$, $^{**}p<0.01$	$N=13550$, $R^2=0.01$		$N=13550$, $R^2=0.01$	

IV Weak Identification Test: Cragg-Donald Wald F-statistic = 2999
IV Over-Identification Test: $p=0.6$, Treatment Endogeneity Test: $p=0.3$

Table 16 Regression results with cluster-robust standard errors for yearly growth in AIS gap rates per starting grid cell as a function of transshipment ban status.

E. Full Results on Landing Prices

Table 17 shows the full difference-in-differences regression results for yearly fish catch landing prices under the treatment of transshipment ban implementation. The summarized results were shown in Table 6.

Variable	**Estimate**	**Std Error**
(Intercept)	5039**	532
Is 2000	-519**	57
Is 2001	-333**	65
Is 2002	-439**	74
Is 2003	-318**	61
Is 2004	-7	82
Is 2005	44	73
Is 2006	59	87
Is 2007	197	117
Is 2008	42	111
Is 2009	174	99
Is 2010	230**	75
Is 2011	90	57
Is 2012	84	53
Is 2013	100**	32
CCAMLR	963**	37
CCBSP	-536**	92
CCSBT	268**	34
GFCM	728**	9
IATTC	226**	12
ICCAT	79**	16
IOTC	-2	24
IPHC	139**	28
NAFO	640**	20
NASCO	282**	22
NEAFC	425**	21
NPAFC	-140*	55
PSC	228**	38
SEAFO	125*	63
SIOFA	432**	43
SPRFMO	2	37
Jellyfish	-2538**	416
Krill	-3072**	740
Large bathydemersals ($\geq$ 90 cm)	-688	404
Large bathypelagics ($\geq$ 90 cm)	-3787**	486
Large benthopelagics ($\geq$ 90 cm)	-2470**	612
Large rays ($\geq$ 90 cm)	-4176**	545
Large reef assoc. fish ($\geq$ 90 cm)	-2630**	716
Lobsters, crabs	-1177	693
Medium bathypelagics (30 - 89 cm)	-3464**	504
Medium benthopelagics (30 - 89 cm)	-3574**	621
Medium demersals (30 - 89 cm)	-3966**	554
Medium pelagics (30 - 89 cm)	-4020**	514
Small bathydemersals (< 30 cm)	-3931**	538
Small demersals (< 30 cm)	-2991**	499
Small pelagics (< 30 cm)	-4533**	502
Small to medium flatfishes (< 90 cm)	-3267**	680
Cephalopods	-3043**	616
Small to medium rays (< 90 cm)	-3289**	585
Large pelagics ($\geq$ 90 cm)	-2614**	570
Large sharks ($\geq$ 90 cm)	-3627**	526
Medium bathydemersals (30 - 89 cm)	-2561**	634
Other demersal invertebrates	-2364**	527
Shrimps	-2471**	739
Small benthopelagics (< 30 cm)	-3554**	594
Small to medium sharks (< 90 cm)	-3913**	513
Small bathypelagics (< 30 cm)	-3385**	539
Medium reef assoc. fish (30 - 89 cm)	-2675**	602
Large demersals ($\geq$ 90 cm)	-2547**	570
Small reef assoc. fish (< 30 cm)	-2529**	461
Ban	**161****	**59**

$^*p < 0.05$, $^{**}p < 0.01$ $N = 6078, R^2 = 0.43$

Table 17 Full difference-in-differences regression results and cluster-robust standard errors for yearly fish catch landing prices under the treatment of transshipment ban implementation.