Biological Conservation

Estimates and drivers of protected species bycatch in the California set gillnet fishery

--Manuscript Draft--

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Abstract:	The identification of efficient management strategies that reduce protected species bycatch while also minimizing impacts on fishing livelihoods is a global conservation challenge. Identifying such strategies requires understanding levels of bycatch relative to management targets as well as the relationship between bycatch risk and potential management levers. In this study, we use ratio estimators to reconstruct bycatch of select marine mammal and seabird species in the California ≥3.5” set gillnet fishery from 1981-2022 and random forest models to identify drivers and hotspots of bycatch risk. We find that bycatch has dropped precipitously since the 1980s as a result of management, but at significant costs to fisheries participation and revenues. Recent marine mammal bycatch ranges from 0.1% to 4.0% of the potential biological removal and marine mammal populations are recovering. Spatial-temporal drivers of bycatch risk were more important than fishing-related drivers of risk, suggesting that spatial-temporal closures would be more effective than mesh size or soak time restrictions at limiting bycatch. For each species, we identified 2-5 hotspots of elevated bycatch risk as candidates for temporary seasonal closures. Bycatch risk for harbor seal (Phoca vitulina) and California sea lion (Zalophus californianus), the species with the greatest bycatch risk, is especially high from April 1st to June 15th, suggesting that hotspot closures during this 2.5-month time period could be particularly efficient. Our study also highlights the value of competing multiple modeling approaches to identify methods that best predict rare bycatch events.
Response to Reviewers:	Please see the attached response to reviewers document

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February 20, 2025

Dear Dr. Bates:

We are grateful for the opportunity to submit our revised manuscript, "Estimates and drivers of protected species bycatch in the California set gillnet fishery", for consideration as an Article in *Biological Conservation*.

We carefully reviewed the comments from you and the reviewers and appreciate this thoughtful feedback. We address each comment individually below with the original comment shown in black text and the response shown in indented blue text. This feedback and the associated revisions have greatly improved the manuscript.

Briefly, we made the following notable changes to the manuscript text:

1. We added an "Overview" paragraph at the top of the methods to better orient the reader to the study objectives, approach, and a few key choices.
2. We added a paragraph before introducing the logbook and observer datasets to orient the reader to their differences and their role in the analysis.
3. We moved the methods and results text describing the estimation of bycatch used the random forest models to the supplemental information to reduce confusion.
4. We substantially edited the paper to clarify how our analysis of drivers of bycatch can help to refine bycatch management to more efficiently meet bycatch objectives while minimizing impacts on fishing opportunities. In particular, we better explained why spatial-temporal hotspot closures could lead to improved efficiency.
5. We clarified why we used a random forest modeling approach and how we selected and categorized the variables assessed as bycatch drivers using this approach.

Thank you for your consideration and please let us know if you have any questions.

On behalf of all authors,
Sincerely,

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Reviewer #1

This is a comprehensive and well-written manuscript on an important topic of relevance to the readership of Biological Conservation.
I liked the comprehensive multi-species, multi-order approach. Most by-catch papers are inappropriately taxonomically restricted.

> We are grateful for your close review of our manuscript, for this kind acknowledgement, and for your thoughtful and constructive feedback. We agree with all of your comments and suggestions and detail how we addressed them in the indented blue text below.

I have only a few minor suggestions to improve the manuscript:

1. The authors make a large number of assumptions scattered through the methods. I suggest adding a table of assumptions, an assessment of the likelihood of each being correct and a comment on the implications for the results if each assumption is not correct to the Supplementary Materials

> We added a new table to the supplement (**Table S4**) to detail the assumptions made in the analysis and their implications for the results. A snapshot is shown below.

Table SX. Assumptions made throughout the analysis, their likelihood, and their potential impact on the results (*assumptions that are expected to be valid or true on average are not likely to impact the results).

Assumption	Likelihood and potential impact of assumption
Data analysis	
Ratio estimation	
1. Ratio estimation assumes that the rate of bycatch for observed fishing trips equals the rate for all fishing trips in a given stratum.	This assumption appears valid as the traits of observed trips are representative of the unobserved trips (**Fig. S7**) and we stratified our analysis using the 7-region scheme recommended in other studies (**Fig. S8**).
2. We assumed that the bycatch rate in years without observer data was equal to the bycatch rate in the closest year with data.	The validity of this assumption is difficult to assess given the inconsistency in the observer program (**Fig. 2BC**); however, it is this inconsistency that also makes the assumption necessary. Ultimately, our finding that recent bycatch is low is not likely sensitive to this assumption, as the effects of substantial decreases in fishing effort overwhelm the effects of changes in bycatch rates.
Bycatch estimation using the random for models*	
1. We assumed that a "pseudo-set" (roughly equivalent to a trip) is equivalent to 3 sets (**Fig. S4AB**).	This assumption is likely to be true on average since it reflects the median value, which has been consistent through time (**Fig. S4AB**).
2. We assumed the number of captures per set is the	This assumption is likely to be true on average since it reflects

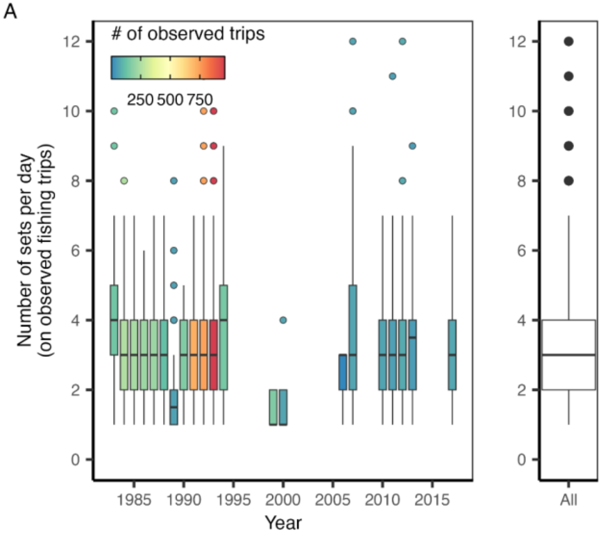
2. Moving Table 1 to the Supplementary Materials to reduce the number of Tables and Figures in the main paper

> We moved **Table 1** to the Supplementary Materials (now **Table S1**) and updated table numbers throughout the manuscript.

3. Clarifying whether all the trips were the same length and involved the same number of sets. If not, does that affect the validity of the analyses?

> We appreciate the question and refer the reviewer to **Fig. S4A** (provided below for ease), which shows that there is a consistent median of three sets per observed trip over time. The level of variability around this central measure is also consistent over time.

A



> The validity of the ratio estimation analysis, which treats trips and not sets as the sampling units, would be compromised if there were temporal trends in these values; fortunately, the lack of trend means that the analysis is appropriate and valid. We added the following underlined sentence to the methods to clarify this point:
>
> > "We used trips rather than sets as the sampling unit given the inability to identify unique sets in the logbook data (**Fig. S4B**). <u>This is valid because the number of gillnet sets per fishing trip (median: 3 sets/trip; IQR: 2-4 sets/trip) has been consistent through time (**Fig. S4A**).</u>"
>
> This is also true for the estimation of bycatch using the random forest models, where we assumed that the average number of sets per trip is time invariant. This assumption is supported by **Fig. S4A**, which was referenced in this methods sentence:
>
> > "We summed the number of pseudo-sets predicted to have bycatch each year, converted this sum to "true sets" assuming three sets per pseudo-set (**Fig. S4AB**), and multiplied this sum by the median number of captures when a capture occurs to generate estimates of the total number of captured animals (**Fig. S4C**)."
>
> The evaluation of drivers of bycatch through the random forest analysis is not sensitive to this variability because the sampling unit for this analysis is gillnet sets not trips.

4. Clarifying how you selected the attributes for inclusion in the forest models.

We added the following underlined text to describe how we selected these variables:

> "We considered nine attributes of fishing as potential drivers of bycatch risk: haul depth (fathoms), mesh size (inches), soak time (hours), latitude (°N), longitude (°W), distance from shore (km), Julian day, sea surface temperature (°C), and whether the fishing occurs near an island (i.e., within 10 km of island coast). <u>These attributes were selected based on their demonstrated relationship to bycatch risk in other papers (e.g., Bettoli et al. 2006, Bjørge et al. 2013, Kroetz et al. 2020) and their availability in the observer data or their ability to be derived through remote sensing (i.e,, distance from shore, temperature, island area). They represent a range of spatial (latitude, longitude, distance from shore, depth, island area), temporal (Julian day), environmental (temperature), and fishing-related (soak time, mesh size) attributes.</u>"

- <u>Kroetz et al. 2020</u>: region, latitude, season, sea surface temperature, depth, net depth, net length, mesh size, gear type, target, and soak time
- <u>Bettoli et al. 2006</u>: mesh size, fish length, water temperature, soak time, and twine type
- <u>Bjørge et al. 2013</u>: Season, areas, depth, soak time

- Kroetz, A.M., Mathers, A.N. and Carlson, J.K., 2020. Evaluating protected species bycatch in the US Southeast Gillnet Fishery. *Fisheries research*, *228*, p.105573.
- Bettoli, P.W. and Scholten, G.D., 2006. Bycatch rates and initial mortality of paddlefish in a commercial gillnet fishery. *Fisheries Research*, *77*(3), pp.343-347.
- Bjørge, A., Skern-Mauritzen, M. and Rossman, M.C., 2013. Estimated bycatch of harbour porpoise (Phocoena phocoena) in two coastal gillnet fisheries in Norway, 2006–2008. Mitigation and implications for conservation. *Biological Conservation*, *161*, pp.164-173.

5. Checking the use of 'only' which I think is misplaced in several sentences.

We deleted "only" in two places in the manuscript where it was used more as a colorful adjective stressing a small value than as an expression of a precise or single value:

> "Since then, participation has continued to decline, with ~~only~~ ~40 vessels active in 2022, and the vast majority (>90%) of landings coming from just 13 vessels (CDFW, 2023) (**Fig. 1B**)."

> "Fleetwide revenues decreased from US$15 million in 1987 to ~~only~~ US$1 million in 2022 (**Fig. 1C**; both values in 2022 dollars)."

Reviewer #2

Bycatch estimation and the identification of factors affecting bycatch are essential for developing effective management measures to reduce bycatch. The aim of the present study was to estimate bycatch in the California set gillnet fishery and identify hotspot areas of bycatch. Despite the relevance of this work, the article requires significant improvement. The main aspects that need attention include a clearer explanation of the dataset used and the statistical methods employed for bycatch estimation. Additionally, a restructuring of all sections is necessary to enhance the overall clarity and flow of the manuscript.

> We are grateful for your close review of our manuscript and for your thoughtful and constructive feedback. We agree with all of your comments and suggestions and detail how we addressed them in the indented blue text below.

Above, I offer the following comments as suggestions to further improve the manuscript:

Introduction

Lines 33-37: The authors highlight that management disruptions can have serious social, cultural, and economic implications. This point is reiterated in another part of the introduction, leading the reader to believe that the present study will propose a management approach that addresses both ecological and social concerns. However, this was not actually presented in the article. I suggest that the authors reconsider how they introduce this topic.

> In response to many of the comments below, we revised the paper to clarify how our analysis of drivers of bycatch can be used to design more efficient management that expands fishing opportunities while keeping bycatch below management targets. We hope that these revisions better show how our paper can support management that addresses both ecological and social concerns. In particular, see the changes we made in response to comments about the spatial-temporal hotspot closures.

Lines 35-37: I am not sure that fishery bycatch, particularly in gillnet fisheries, constitutes an economic constraint for the fishing industry. Bycatch becomes a problem when fishing effort needs to be reduced to mitigate bycatch levels. A better contextualization of this issue is needed here.

> We replaced the word “impacts” with “consequences” to better connect this sentence to the previous two sentences, which articulate that bycatch becomes a problem when bycatch management reduces fishing opportunities:
>
> > “As a result, many countries have established strict mandates to limit bycatch of vulnerable species, which can result in fisheries closures and other severe restrictions (Crowder and Murawski, 1998; Senko et al., 2014). These

> management disruptions can have serious social, cultural, and economic impacts on fishing communities (Smith et al., 2020). Due to the negative ecological, economic, and social consequences of fishery bycatch, bycatch avoidance is an important objective for global fishery management."

The cited papers all show that bycatch management can result in reduced fishing opportunities that result in economic impacts. Further in the introduction, we show how historical bycatch management of the California set gillnet fishery has dramatically reduced fishing opportunities and revenues.

Lines 64-66: To predict bycatch on unobserved trips, the bycatch estimates from observed trips must be representative, even when using a model-based approach.

This is true. If observed trips are not representative of unobserved trips, a model risks being asked to make out-of-sample (OOS) predictions. We added the following underlined text to soften the unintended implication that model-based approaches are not also vulnerable to the challenges faced by ratio estimation:

> "Model-based approaches, which use statistical models to estimate bycatch, can overcome many of these limitations by incorporating a wider suite of covariates and allowing for non-linear impacts and are generally thought to produce better bycatch estimates (Stock et al., 2019)."

We opt not to make more drastic changes given our confirmation that, in our case, the observed trips were representative of the unobserved trips (see **Fig. S7**).

Lines 100-111: A drastic reduction in fishing effort, as well as bycatch, was observed in the study area. This reduction corresponds to a 15-fold decrease in fishing days and a 10-fold decrease in fleet size, representing a significant decline in fishing effort. However, despite these decreases, the authors highlight ongoing calls for further reductions. To support additional reductions in fishing effort, it is important to present evidence, including examples, demonstrating the real need for further limiting fishing activities.

We added the following underlined text to clarify that the ongoing calls are from one of the U.S.'s more agenda-based conservation groups (Oceana). As a result, these calls are less based on empirical scientific evidence and more on organizational mission. Our paper serves to provide the scientific basis for either refuting or supporting Oceana's claims that bycatch is a significant problem in the fishery. After conducting the research, we found no support for Oceana's claims, as current bycatch mortality is below NOAA's "zero mortality rate goal" (ZMRG). To avoid being overly provocative, we are intentionally vague about the specific conservation group, though both of the cited documents are Oceana lobbying materials.

> "Despite declining fishing effort and bycatch, <u>conservation groups are lobbying for</u> additional restrictions, including permanent closure, to further avoid bycatch (Birch et al., 2023; Birch and Shester, 2023). There is thus great need for scientific guidance on management regulations that are likely to provide conservation benefits while also avoiding unnecessary burdens on the fishing industry."

In addition, I suggested that the authors reconsider and restructure the introduction. Below, I provide an example of a potential revised structure and emphasize the need for a more detailed statistical explanation regarding bycatch estimation and bycatch predictors:
1- Fishery bycatch concerns and management actions to reduce bycatch.
2- Description of the management actions implemented to reduce bycatch in the California set gillnet fishery, along with the consequent reduction in fishing effort in the region.
3- Despite the reduction in fishery bycatch, there is substantial evidence suggesting that fishing remains a growing impact.
4- To guide effective bycatch reduction policies, it is crucial to understand the magnitude of both historical and recent bycatch, as well as the key drivers of bycatch in fisheries.
5- Explanation of the models used: Why choose ratio estimation over model-based predictions (e.g., regression models)? Unlike ratio estimators, which are commonly used to extrapolate bycatch estimates by multiplying an observed bycatch rate by the total fishing effort, model-based predictions offer greater efficiency and precision by incorporating a statistical model of bycatch. Additionally, why choose Random Forest over regression models? Regression models can provide both bycatch predictions and identify predictors simultaneously. Please explain the advantages of using different approaches for estimating bycatch and identifying predictors.
6- Objectives.

We took several steps to address this comment.

We incorporated the requested information -- i.e., (1) why we used both design- and model-based approaches to estimate bycatch and (2) why we used random forests, a machine learning approach, instead of classical regression approaches like GLMs or GAMs to evaluate predictors of bycatch -- into the methods section. We opted to put this information in the methods rather than in the introduction to keep the introduction broadly focused on the roles and tradeoffs of the approaches and the methods focused on the technical details and decisions. This information was added to a new methods "Overview" section that address both points, a rewrite of the methods section on estimating bycatch using the random forests that explains both the reason for and problems with this approach (point #1), and small changes to the random forests section to articulate why random forests were used instead of classical regression techniques (point #2).

We also added the following underlined text to the introduction to better introduce the types of classical regression techniques and machine learning techniques that can be used to both estimate bycatch and evaluate drivers:

“Model-based approaches, which use <u>either</u> statistical (<u>e.g., generalized linear models, generalized additive models) or machine learning (e.g., random forests, boosted regression trees)</u> models to estimate bycatch, can overcome many of these limitations by incorporating a wider suite of covariates and allowing for non-linear impacts and are generally thought to produce better bycatch estimates (Stock et al., 2019). Additionally, these approaches can support management by identifying drivers of bycatch risk and by predicting detailed hotspots of risk (Long et al., 2024; Lopez et al., 2024; Stock et al., 2019).”

We note that this text previously explained that statistical models can both estimate bycatch and evaluate drivers.

We appreciate the suggested outline for a potential restructured introduction but opted not to restructure the introduction because (1) the current structure is fairly close to the suggested structure and (2) the current structure better flows from broad (general bycatch issues) to narrow (specific case study). The current structure is as follows:

1. Fishery bycatch concerns and impacts on fishing communities
2. Bycatch management actions and the information needed to guide them
3. Approaches for quantifying the scale/drivers of bycatch to guide management
4. Introduction of the study fishery and its bycatch concerns
5. History of bycatch management in the study fishery
6. Consequences of management on communities and need for greater efficiency
7. Study objectives

The current introduction starts with 3 broad paragraphs followed by 4 narrow paragraphs. The suggested introduction goes 1 broad paragraph then 2 specific paragraphs then 2 broad paragraphs then 1 specific paragraph.

Methods

I missed the "Study Area" subsection. I agree that describing the area in terms of oceanographic conditions and productivity is crucial for a better understanding of the spatial and temporal patterns of both fishery activity and the target bycatch species.

We added a new “Study area” subsection that briefly introduces the section of California coast evaluated in the study and its oceanography, fisheries, and protected species. The new subsection reads as follows:

“Our study area spans southern and central California, U.S.A. from the U.S.-Mexico border (32°N) to Point Reyes (38°N), which is just north of San Francisco Bay (**Fig. 1A**). The study area falls within the California Current Ecosystem, a highly productive eastern boundary upwelling system spanning from the southern tip of the Baja Peninsula, Mexico (28°N) to the U.S.-Canada border (48°N). Seasonal upwelling of cold, nutrient-rich water fuels populations of krill, squid, sardines, and other low trophic species that are fed upon by larger fishes, sea turtles, seabirds, and marine mammals. The ecosystem also supports important commercial and recreational fisheries and other human uses that benefit tens of millions of people living along the U.S. West Coast. We assessed bycatch within seven regions of the study area, which are shown in **Fig. 1A** and explained in detail in *section 2.5*."

Moreover, the subsections about the fishery dataset are confusing. It is unclear whether the authors used two data sources: one from observer programs and another from logbooks. Is the logbook dataset merely complementary information, or does it represent new fishing sets? Additionally, these subsections are lengthy and repetitive, and should be condensed.

We added a new "Overview" subsection to the methods where we now provide an overview of both the analytical approach and the datasets used to support this approach.

This new "Overview" subsection introduces the differences between the logbook and observer datasets and explains their role in the analysis. Briefly, all gillnet vessels are required to submit logbooks that describe ***all fishing trips*** and retained catch. Logbooks do not contain information about protected species bycatch but they do document all fishing trips for which we then predict the bycatch associated with the trip. Observers are trained scientists placed on ***a sample of all fishing trips*** that collect information about discarded catch including catch of protected species. The random forest models are trained on the observer data. The ratio estimators use bycatch rates on observed trips (observer data) to estimate bycatch for all trips (logbook data).

We also added a new opening paragraph to the "Dataset" section to explain the differences and role of the two datasets before they are introduced in detail:

"Our analysis relies on two fisheries-dependent datasets: logbook data and observer data. All gillnet vessels are required to submit logbooks documenting when, where, and how they fished and how much catch was retained. Thus, logbooks characterize all fishing trips. However, because logbooks are self-reported and reported discards are unverifiable, logbooks likely underreport discarded bycatch, especially the bycatch of protected species. As a result, information from observer programs, which place trained observers on a sample of fishing trips (0-16.7% in this fishery; **Fig. 2B**), are required to inform estimates of bycatch for the unobserved trips recorded in vessel logbooks, which constitute the majority of fishing effort (83.3-100% in this fishery; **Fig. 2B**). Thus, the estimation of total bycatch through ratio estimation depends on both the observer and logbook data. In contrast, the evaluation of bycatch drivers and hotspots with the random forest models uses only the observer data, as these are the only data to accurately record protected species bycatch when it occurs. These datasets are described in detail below."

We reduced the length and repetitiveness of the dataset subsections by moving the details on how missing values were imputed to the supplemental methods. We added the following two sentences to refer the reader to this text:

"We developed a series of simple assumptions to impute missing values for a few key variables (GPS coordinates, fishing depth, soak hour, mesh size) used to describe gillnet sets documented in the observer data (**Fig. S3**; see supplemental methods for details)."

"We developed a series of simple assumptions to impute missing or unrealistic values for a few key variables (fishing depth, soak hour, mesh size) used to describe gillnet pseudo-sets documented in the logbook data (**Fig. S5 & S6**; see supplemental methods for details)."

Lines 149-154: It is not necessary to explain how the fishing sets were identified in the federal and state datasets. Furthermore, the distinction between the state and federal datasets is not clearly explained.

We removed the text explaining how unique identifiers for fishing sets in the observer data were constructed. By deleting this text, we also removed the only reference to the "state" and "federal" observer data, thereby preventing the need to clarify this distinction. For reference, CDFW is a state agency and NOAA is a federal agency; this nomenclature was originally intended to distinguish between the data collected by the state-run (CFDW) and federally-run (NOAA) observer programs.

Lines 157-160: I don't think it's necessary to explain that some pieces of the dataset were digitized from sheets. Simply stating the origin of the data should suffice.

We replaced the word "digitized" with the word "recovered" in the following sentence:

"We recovered a small portion of the missing raw data – observations from Monterey Bay from 1987-1989 (**Fig. 2**) – from original CDFW data sheets that were given to a colleague at the Southwest Fisheries Science Center during the late 1990s for a reanalysis of historic bycatch rates in that region (Forney et al., 2001)."

This is helpful because it better aligns the text with the language used in **Fig. 2**.

Line 164: What missing values are being referred to? Are the authors referring to a fishing net set?

We added the following underlined text to clarify that we imputed missing values that describe traits of observed or logged gillnet sets:

> "We developed a series of simple assumptions to impute missing values for a few key variables (GPS coordinates, fishing depth, soak hour, mesh size) <u>used to describe gillnet sets documented</u> in the observer data (**Fig. S3**; see supplemental methods for details)."
>
> "We developed a series of simple assumptions to impute missing or unrealistic values for a few key variables (fishing depth, soak hour, mesh size) <u>used to describe gillnet pseudo-sets documented</u> in the logbook data (**Fig. S5 & S6**; see supplemental methods for details)."

Lines 167-168: What does 'block' mean? This term is introduced for the first time, so a definition is required. Additionally, the authors state that if week-level information is missing, they use month-level data, and if month-level data is unavailable, they use year-level data to determine the position of the fishing net set. Is this correct? If so, it seems complicated to impute missing values based on monthly or yearly averages. I suggesting excluding fishing net sets where no week information is available, as this could lead to more accurate data.

In California, fishing catch and effort is reported to 10x10 minute (~18x18 km) statistical reporting blocks, which we refer to as "blocks" for brevity. We added a definition of these statistical blocks at first use and in the caption of **Fig. 1**, where the blocks are mapped.

> **First use:** "These data describe vessel information (vessel name, unique identifier, permit number); when (date), where (<u>statistical reporting "block"; **Fig. 1A**</u>), and how long (hours) a vessel fished; …."
>
> <u>**Fig. 1:**</u> "Panel **A** shows the spatial history of fishing effort during four regulatory periods. <u>Trips are reported by the 10 x 10 minute (~18 x 18 km) statistical blocks used for fisheries catch and effort reporting.</u> The horizontal lines delineate geographical strata used in the ratio estimation analysis; strata are labeled in the first plot."

Line 183: The distinction between the logbook and observer coverage datasets is unclear. Both datasets cover the same period (1983-2017). Does the information obtained from logbooks complement the observer data, or does it represent new trips that were not monitored by the observer program? Clarification on the relationship between these two datasets is needed.

We added a new "Overview" subsection to the methods where we now provide an overview of both the analytical approach and the datasets used to support this approach.

We also added a new opening paragraph to the "Dataset" section to explain the differences and role of the two datasets before they are introduced in detail.

See our response to the comment where this point is first made and addressed above.

Line 227: What characteristics were evaluated?

We added the following underlined text to list the evaluated characteristics, which we note are also all shown in Fig. S7:

> "This assumption requires that the characteristics of observed trips do not systematically differ from the characteristics of all trips, which was confirmed by a two-sided Kolmogorov-Smirnov test for six key traits (i.e., day of year, depth, latitude, mesh size, distance from shore, soak time) (**Fig. S7)**."

Line 228: The unit used in the article was fishing trips rather than fishing net sets. However, for some imputed data obtained from historical records, the unit was fishing net sets, as shown in Table 1. It is not clear how the authors standardized all the information to the same unit.

We took several actions to address this comment.

First, we added the following underlined text to the methods to clarify that we converted set-level bycatch rates from historical reports to trip-level bycatch rates assuming 3 sets per trip, as indicated by the observer data:

> "We extracted summaries of set-level bycatch rates from historical reports (**Table S1**) for years and regions missing raw data to support the ratio estimation analysis. We converted set-level bycatch rates to trip-level bycatch rates assuming an average of 3 sets per trip (**Table S3**), as indicated by the observer data (**Fig. S4**)."

Second, we added a new supplemental table (**Table S3**) to explicitly provide the bycatch rates extracted from historical reports. A snippet is shown below:

Table S3. Bycatch rates extracted from historical reports (* mark years with raw observer data where the summary values from the historical reports are not needed).

Reference	Species	Region	Year	# sets observed	Estimated # trips observed	Catch	Catch per trip	Raw data available?
Hanan et al. 1988	California sea lion	Channel Islands	1985	180	60.00	44	0.733	*
Hanan & Diamond 1989	California sea lion	Channel Islands	1986	66	22.00	54	2.455	*
Hanan et al. 1988	California sea lion	Monterey Bay	1983	22	7.33	12	1.636	
Hanan et al. 1988	California sea lion	Monterey Bay	1984	126	42.00	19	0.452	
Hanan et al. 1988	California sea lion	Monterey Bay	1985	49	16.33	5	0.306	
Hanan & Diamond 1989	California sea lion	Monterey Bay	1986	36	12.00	4	0.333	
Hanan et al. 1988	California sea lion	Morro Bay	1983	288	96.00	41	0.427	
Hanan et al. 1988	California sea lion	Morro Bay	1984	374	124.67	22	0.176	
Hanan et al. 1988	California sea lion	Morro Bay	1985	317	105.67	25	0.237	
Hanan & Diamond 1989	California sea lion	Morro Bay	1986	137	45.67	13	0.285	*
Hanan et al. 1988	California sea lion	San Francisco	1983	158	52.67	4	0.076	
Hanan et al. 1988	California sea lion	San Francisco	1984	300	100.00	8	0.080	
Hanan et al. 1988	California sea lion	San Francisco	1985	348	116.00	3	0.026	
Hanan & Diamond 1989	California sea lion	San Francisco	1986	419	139.67	2	0.014	
Hanan et al. 1988	California sea lion	Southern California	1983	430	143.33	16	0.112	*
Hanan et al. 1988	California sea lion	Southern California	1984	571	190.33	13	0.068	*
Hanan et al. 1988	California sea lion	Southern California	1985	339	113.00	5	0.044	*
Hanan & Diamond 1989	California sea lion	Southern California	1986	425	141.67	15	0.106	*
Hanan et al. 1988	California sea lion	Ventura	1983	430	143.33	16	0.112	*

Finally, we modified **Figures 2 and S9** to explicitly mark values that were extracted from the historical reports. See the revised figures in a response to a comment further below.

Line 230: What do the authors mean by "stratum i"? Is it related to the "seven-region" division? If so, I suggest modifying the text as follows: "Where ks,i is the total number of individuals of species s captured in observed trips occurring in each region i and…". Additionally, I recommend that the authors include a "Study Area" subsection and describe how the study area was divided into different regions.

We added the following underlined text to tease up that, in this case, strata are defined by years and regions:

> "Under this approach, the bycatch rate for species *s* in stratum *i* ($r_{s,i}$) – where, in this case, strata are defined by years and regions (see next paragraph) – is thus calculated as:"

The next paragraph explains the derivation of the regional strata.

Line 234: What is the number of trips observed and the number of trips not observed?

We added a new supplemental table (**Table S2**) to provide the number of total (observed+unobserved) trips, observed trips, and unobserved trips per year.

Table S2. Number of fishing trips in the California 3.5 inch mesh set gillnet fishery by year.

		# of trips			Percent of trips	
Year	**# of vessels**	**Total**	**Observed**	**Unobserved**	**Observed**	**Unobserved**
1981	162	6139	0	6139	0.0%	100.0%
1982	222	9218	0	9218	0.0%	100.0%
1983	262	9012	143	8869	1.6%	98.4%
1984	357	12374	255	12119	2.1%	97.9%
1985	378	14314	238	14076	1.7%	98.3%
1986	379	13336	217	13119	1.6%	98.4%
1987	290	10667	213	10454	2.0%	98.0%
1988	211	8585	120	8465	1.4%	98.6%
1989	198	7811	58	7753	0.7%	99.3%
1990	182	6836	167	6669	2.4%	97.6%
1991	158	6668	716	5952	10.7%	89.3%
1992	117	5611	744	4867	13.3%	86.7%
1993	115	5754	959	4795	16.7%	83.3%
1994	74	2455	150	2305	6.1%	93.9%
1995	70	2616	0	2616	0.0%	100.0%
1996	59	2654	0	2654	0.0%	100.0%
1997	66	3310	0	3310	0.0%	100.0%

We also note that this information can be visually assessed from **Fig. 2B**, which already plots the number and percentage of trips that are observed trips. **Fig. 1C** provides additional helpful context because it shows the total number of trips over time.

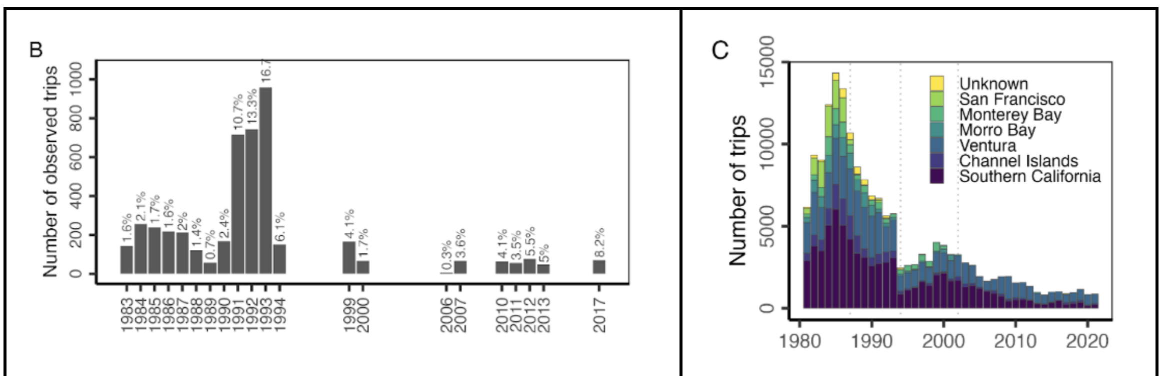


Fig. 2B **Fig. 1C**

We added the following sentence to the "Observer data" subsection to highlight this new table and better highlight the existing figures showing this information:

> "The percentage of fishing trips with onboard observers has varied over time, ranging from 0.3% of trips in 2006 to 16.7% of trips in 1993 (**Fig. 2B; Table S2**)."

Line 241: Did the authors assess the logbooks of all boats? This should be clearly stated in the description of the logbook dataset.

We added the following sentence to the "Logbook data" section to clarify that we assessed all logbooks, which means that we assessed all fishing effort associated with the fishery.

> "All California gillnet vessels are required to submit logbooks that document all of their fishing trips; as a result, these logbooks represent all fishing effort associated with the fishery."

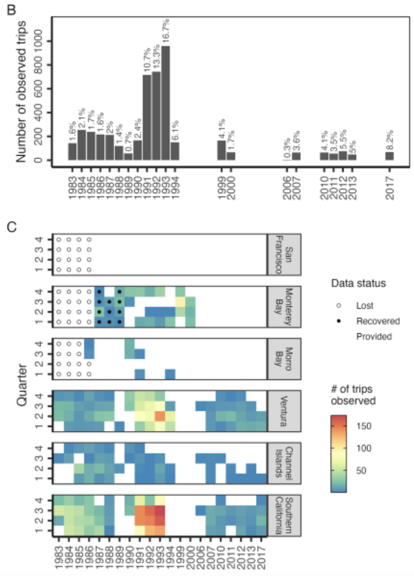
We also added a new "Overview" subsection to the methods and a new opening paragraph to the "Dataset" section that we hope further clarified this point. See our response to related comments above for details.

Lines 248-250: For missing observed coverage, the authors mention that bycatch rates were obtained from historical reports, correct? Are there years where bycatch rates are unavailable from both historical records and observed/logbook records? If so, imputing missing bycatch values by averaging previous and subsequent years may not be appropriate. I suggest the authors verify the best procedure for handling missing values in a time series.

In this response, we (1) answer the two questions posed in the comment; (2) explain how we made the answers to these questions more clear in the revised manuscript; (3) clarify the procedure used to impute missing bycatch rates; and (4) explain why this is the best possible imputation procedure.

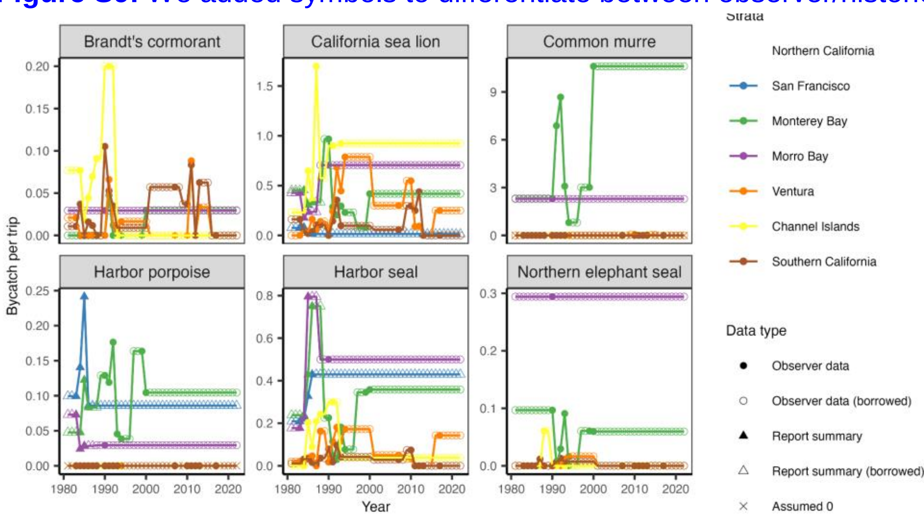
(1) Yes, bycatch rates were obtained from historical reports for strata (regions and years) not covered by the raw observer data and yes, there are strata for which bycatch rates are not available from either historical reports or raw data. These are the strata for which we had to impute bycatch rates. Strata missing bycatch rates can be seen in **Figures 2BC** and **Figure S9** (revised version shown below).

Figure 2BC for reference:



(2) We made this more clear by modifying **Figures S9** to mark which years have bycatch rates from historical reports. Please see the modified figure below.

Figure S9: We added symbols to differentiate between observer/historical report data.



(3) We did not impute missing bycatch rates by averaging previous and subsequent years. This is not possible because, as seen in **Figures 2** and **S9**, observer data ends in 2017 for the southern strata and even earlier for the northern strata; there are no data for "subsequent years" after the observer programs end. This is why we, instead, used the closest available data for the strata, as stated in this sentence in the methods:

> "Stratum-specific bycatch rates for years without observer coverage in the stratum are borrowed from the closest year (forwards or backwards) with observer coverage in the stratum (**Fig. 2C & S9**), as has been the practice in previous studies."

(4) This was our approach because no practicable alternative exists and the use of the most recent year with data is the standard used in the historical reports. We clarified that this has been the approach of current studies by adding the underlined text above.

Lines 252-254: Why was uncertainty not provided for the years in which a raw dataset is available?

We appreciate the encouragement to estimate uncertainty where possible as this is a critical part of rigorous science. However, this is only possible for 6 of the 42 evaluated years (1981-2022): 2006, 2007, 2010, 2011, 2012, and 2017. These are the only years during which the observer program operated in all of the fished strata (see **Fig. 1AC; Fig. 2BC).** 2013 almost qualified, except that fishing occurred in the Morro Bay strata that year (and there was no observer coverage there). We feel that estimating uncertainty for such a small, scattered, and non-continuous number of years would add more confusion than value and opted not to estimate uncertainty for these 6 years.

We added the following underlined text to clarify this in the text:

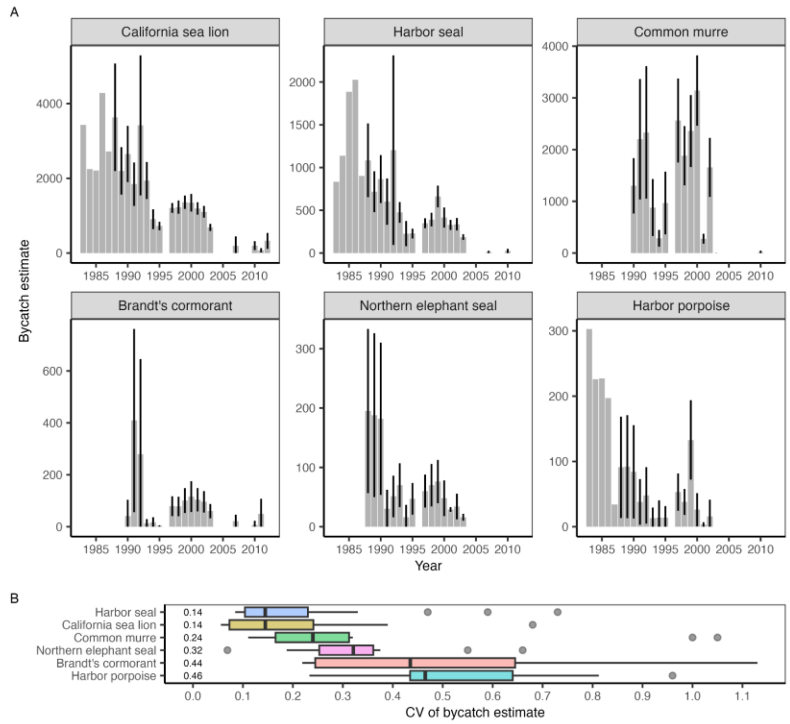
> "Because these procedures require raw observer data, we cannot use them for (1) years where summary values from historical reports are used because the raw data have been lost or (2) years without observer data from within one of the fished strata. As a result, only 6 of the 42 evaluated years had the data required to estimate uncertainty: 2006, 2007, 2010, 2011, 2012, and 2017 (**Fig. 1AC; Fig. 2BC**). An exploration of the uncertainty estimates generated in historical reports with access to the lost data (**Table S1**) suggests that the median coefficient of variation for estimates of annual bycatch estimates ranges from a low of 0.14 for harbor seal to a high 0.47 for harbor porpoise (**Figure S10B**)."

Lines 257-259: It is not clear what the authors intend to convey with this sentence. Why is the estimate provided for only two species?

The original sentence contained a few typos that made it difficult to understand. We corrected these typos and the sentence now reads:

> "An exploration of the uncertainty estimates generated in historical reports (**Table 1**) suggests that the median coefficient of variation for estimates of annual bycatch estimates ranges from a low of 0.14 for harbor seal to a high 0.47 for harbor porpoise (**Figure S10B**)."

In addition to fixing the typos, we added the underlined text to clarify that we were bookending the range of values (i.e., providing the smallest and largest values), rather than listing all five values, which is why only two values were provided. To further clarify this, we printed the median coefficient of variation (CV) for all five species in **Fig. S10B**. Rather than listing all five values, an interested reader can now refer to the figure.



We added the following text to the **Fig. S10** caption to explain this addition: "In **(B)**, the median CV of the bycatch estimates for each species is printed on the far left."

Lines 325-327: The authors state that the ratio estimation method was used to estimate bycatch rates, while Random Forest was employed to evaluate the drivers of bycatch. However, the text also suggests that bycatch estimation was derived using Random Forest, which requires clarification. Additionally, it is unclear how bycatch was estimated using Random Forest with a Bernoulli distribution (0 or 1) rather than a count distribution. Could the authors provide more details on this methodology and clarify how it aligns with the stated objectives?

We took multiple steps to address this comment.

It is correct that we estimated total bycatch using both the design-based ratio estimation approach and the model-based random forest approach; however, we ultimately found the model-based random forest approach to be unsuitable in this particular case study. In the original submission, we documented both approaches in the main text but focused largely on the ratio estimation results, which created understandable confusion.

As a result, we moved the methods and results pertaining to bycatch estimation using the random forest models to the supplemental information. This makes the paper cleaner and easier to understand as only the method used to generate reliable bycatch estimates is now described in the main text. However, we felt it was important not to entirely remove the model-based approach methods and results from the paper, as this approach is generally thought to perform better than the design-based approach, and it would have been a scientific oversight not to pursue the approach that is generally thought to perform best. Thus, moving this work to the supplemental information retains the work while also making the paper less busy and confusing.

In doing this, we added text to the main text to notify the reader that we tried to estimate total bycatch using the random forest models but that the results were unreliable, and we refer the reader to the supplemental information for details. Specifically, we included the following underlined text in the new methods "Overview" section:

> "We used a design-based ratio estimation approach to estimate bycatch of select marine mammal and seabird species in the California set gillnet fishery from 1981-2022. Briefly, the ratio estimation approach estimates total bycatch by applying the bycatch rates (i.e., bycatch per trip) on the sample of fishing trips with trained observers (*section 2.4.1*) to all fishing trips, which are documented in the logbooks maintained by all fishing vessels (*section 2.4.2*). Although model-based approaches, including the random forest approach used in the second portion of our analysis, are generally thought to produce better estimates of bycatch than design-based approaches (Stock et al., 2019), we found model-based approaches to be unsuitable for this specific case study due to data challenges (see *section 2.6.4* and the supplemental information for more details). However, the random forest models were successfully used to evaluate drivers of bycatch and map hotspots of bycatch risk for four of the six evaluated species."

We also replaced the paragraph describing the methods for estimating bycatch using the random forests, which was moved to the supplement, with this paragraph explaining why the method produced unreliable results and is not featured in the main text:

> "We explored using the random forests models to estimate annual bycatch (1981-2022) in the fishery but found them to be unsuitable for this specific case study. Although model-based approaches generally perform better than design-based approaches at estimating bycatch (reference), we found that our random forest models underpredicted bycatch risk in the 1980s and 1990s relative to the ratio estimator (**Fig. S14**). This is most likely because of the loss of observer data from the northern strata during this time period, fishery largely operated in the northern portion of the region. See the supplemental information for details on the estimation of bycatch using the random forest models."

The following text in the now supplemental methods explains how bycatch was estimated using a random forest model that predicts bycatch presence/absence:

> "We summed the number of pseudo-sets predicted to have bycatch each year, converted this sum to "true sets" assuming three sets per pseudo-set (**Fig. S4AB**), and multiplied this sum by the median number of captures when a

capture occurs to generate estimates of the total number of captured animals (**Fig. S4C**)."

Lines 327-328: It is not clear how bycatch can be estimated when the number of fishing sets is under estimation. What are the implications of this for the reliability of bycatch estimation?

We added the following underlined text to tease the answer to this question, which is addressed three sentences later in the paragraph:

Topic sentence with added text: "We used the best fitting model to generate annual estimates of protected species bycatch from 1981 to 2022 by predicting whether "pseudo-sets" recorded in logbooks were likely to have captured each study species and assuming median numbers of sets and caught animals for "pseudo-sets" with bycatch."

Details occurring later in paragraph: "We summed the number of pseudo-sets predicted to have bycatch each year, converted this sum to "true sets" assuming three sets per pseudo-set (**Fig. S4AB**), and multiplied this sum by the median number of captures when a capture occurs to generate estimates of the total number of captured animals (**Fig. S4C**)."

We note that this text now occurs in the supplemental information and not the main text.

Results

Some results presented here were not explicitly stated in the objectives or in the material and methods sections. Additionally, in many instances, the authors are not simply presenting their results but also providing interpretations, which is more appropriate for the discussion section. In the results section, authors typically report findings without offering detailed interpretation. Moreover, the bycatch estimation obtained from Random Forest also requires further explanation to clarify the methodology and its role in the analysis.

We took multiple steps to address this comment.

First, we added the following underlined text to the last paragraph of the introduction to clarify upfront that comparing bycatch levels to management targets is an objective of the study:

"We use ratio estimation methods to reconstruct historical bycatch levels and compare recent bycatch levels to management targets."

Second, we added the methods for comparing recent bycatch levels to management targets to the methods section. See a response to a comment below for details.

Third, we moved the methods and results related to estimating bycatch using the random forest model to the supplemental information. This negates the need to state the use of this method and the comparison of its results to those from the ratio estimation approach as an explicit objective of the paper.

Finally, we carefully reviewed the results and removed many sentences that are more appropriate for the discussion (i.e., they interpret the results).

Many of these points are corrected by the helpful comments below.

Lastly, I think that in the first part of the results, the authors could provide some basic information on bycatch and fishing effort before presenting the bycatch trends. For example, it is only in line 417 that the authors mention that the common murre was the most common bycatch species.

We added the following underlined text to better highlight the most common bycatch species in the 1980s:

> "In general, estimated bycatch peaked in the mid-1980s, steadily declined following the 40-fathom depth restriction implemented in 1987, with a temporary increase in the late-1990s followed by continued decline (**Fig. 3**). <u>Bycatch peaked, in order of decreasing magnitude, at 5,059 common murre in 1984, 3,437 California sea lion in 1987, 2,605 harbor seal in 1986, 560 harbor porpoise in 1985, 453 Brandt's cormorant in 1991, and 432 northern elephant seal in 1986 (**Fig. 3**)</u>. This pattern reflects trends in fishing effort, which also declined after a peak in 1985, with a brief expansion in the late-1990s followed by continued decline (**Fig. 1C**)."

The last sentence provides a brief summary of the fishing effort trends shown in Figure 1, which was presented in detail in the introduction.

Lines 359-360: This information could be included in the discussion section "40-fathom depth restriction implemented in 1987".

We retained the first sentence of the results as originally written because it is a simple reminder of what happened in 1987 that coincides with this decline.

Line 364: Looking at Figure 3, the peak occurred in 1990, not in 1989 as indicated by the authors.

We deleted these sentences to eliminate confusion.

Lines 362-366: The issue lies in the timing of the changes in bycatch estimation described in the two sentences.

1. First sentence: "Estimated bycatch precipitously declines after a peak in 1989 with a temporary increase occurring in the early-2000s." This indicates that after a peak in 1989, bycatch rates declined until the early 2000s.
2. Second sentence: "This pattern is driven by a steep increase in Brandt's cormorant bycatch rates in the Channel Islands region in 1990 that is assumed to have persisted to today." This implies a steep increase in bycatch rates starting in 1990 and continuing to the present.
The contradiction arises because the first sentence describes a decline in bycatch rates after 1989, while the second sentence suggests a steep increase starting in 1990. If there was a decline in bycatch rates from 1989 to 2000 (as the first sentence suggests), it seems inconsistent with the second sentence, which states that there was a steep increase in bycatch rates starting in 1990.

We deleted these sentences to eliminate confusion.

Lines 366-367: Observing Fig. S9, it is also possible to see significant bycatch variability for the California sea lion (yellow line), harbor porpoise (green line), and common murre (green line)

We deleted these sentences to eliminate confusion.

Lines 367-369: A sharp decline is also observed for the elephant seal (Figure 3).

This is a good observation. We added northern elephant seal to this sentence:

> "Estimated bycatch of harbor porpoise, northern elephant seal, and common murre declined especially sharply following the 2002 exclusion of fishing from waters shallower than 60 fathoms."

Lines 369-371: I suggest that the authors present only the results in this section and reserve the discussion of the causes behind these patterns for the Discussion section.

We moved this sentence to the discussion.

Lines 373-378: Until this point in the manuscript, the authors have not stated that one of the objectives of the article is to compare the bycatch estimation with potential biological removal. This objective should be clearly outlined in both the Introduction and Materials and Methods sections.

We added the following underlined text to the last paragraph of the introduction to clarify upfront that comparing bycatch levels to management targets is an objective of the study:

> "We use ratio estimation methods to reconstruct historical bycatch levels and compare recent bycatch levels to management targets."

We added the following paragraph to the ratio estimation methods to explain that we compare recent levels of bycatch to management targets and to explain the reference points used to judge sustainability:

> "We evaluated the sustainability of recent estimated marine mammal bycatch by comparing it to the potential biological removal (PBR) for each stock, which is defined under the MMPA as the maximum number of animals, not including natural mortalities, that may be removed from a marine mammal stock while allowing that stock to reach or maintain its maximum sustainable population. We extracted each PBR from its most recent stock assessment (**Fig. 7**) and compared it to the average estimated catch over the last 10 years (2013-2022). A fishery is managed based on its classification into one of three categories: Category 1 fisheries cause annual mortality and serious injury (M/SI) greater than 50% of the PBR, Category II fisheries cause annual M/SI between 1 and 50% of the PBR, and Category III fisheries cause annual M/SI less than 1% of the PBR. A fishery is considered to be approaching the MMPA's "zero mortality rate goal" (ZMRG) when annual M/SI is below 10% of the PBR."

Lines 390-406: This sentence is more suited for the discussion section, not the results.

We moved this paragraph to the supplemental information.

We added the following text to the end of the first results paragraph to refer the reader to these results in the supplemental info:

> "Slight differences between our estimates of annual bycatch and those from historical studies (**Fig. S11**) are driven by a mixture of differences in our methods and input data (**Fig. S12;** see supplemental information for more details)."

Lines 411-412: Move this sentence: 'The models for two species, Brandt's cormorant and harbor porpoise, were excluded from further consideration because of their poor performance (Table 2)' to after the first sentence of the paragraph.

We moved this sentence to the suggested location. This is much improved.

Line 414-415: This sentence should be removed: "This highlights the importance of evaluating multiple modeling approaches when predicting rare bycatch events."

We agree that this is more appropriate for the discussion and deleted this sentence.

Line 416: What do the authors mean by "strongly correlated".

We rewrote this sentence to clarify the correlation, its magnitude, and implications:

“Model performance was positively correlated with the frequency of bycatch observations, i.e., species with more observed bycatch events produced models with greater skill (**Table 1**; $r^2 = 0.64$ for Cohen’s kappa for training data).”

See the reply to the comment relating to Lines 423-425 for a graphical representation of this correlation. We did not add this visual to the supplement because the supplement is already long and we judged **Table 1** to be sufficient for conveying this minor result.

Lines 420-422: This sentence should be removed, as the threshold value indicating when a model was not used is already provided in the Materials and Methods section and should not be repeated in the Results.

We removed this sentence and adopted the language proposed below.

Lines 422-423: The sentence should be removed, as the Materials and Methods section already states that fair performance models were also used for prediction.

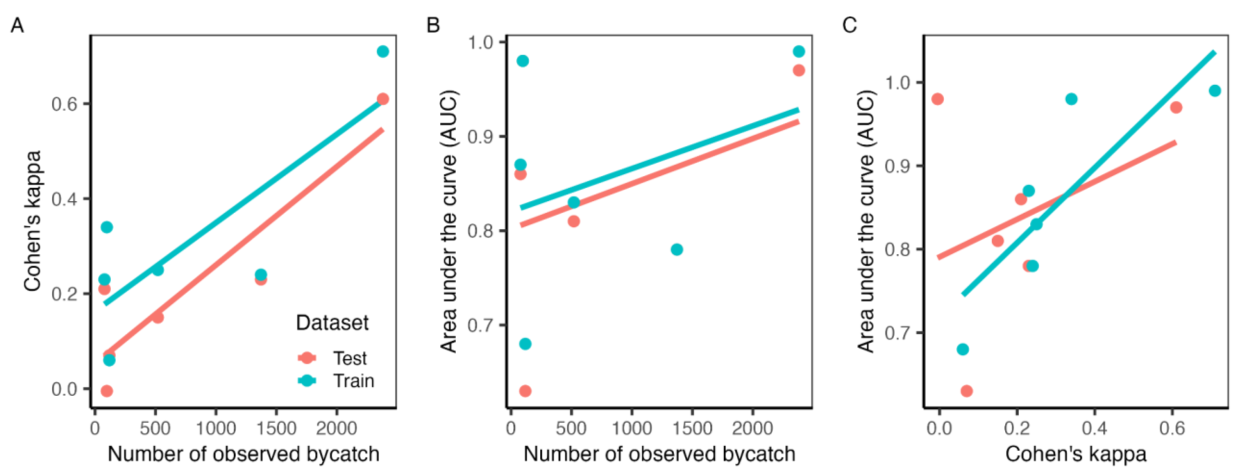
We removed this sentence and adopted the language proposed below.

Lines 423-425: What do the authors mean by 'Cohen's kappa values were correlated with the area under the receiver operator curve (AUC)?' How can the reader identify this correlation?

We added the following underlined text to clarify the correlation and its magnitude:

“Cohen’s kappa was positively correlated with the area under the receiver operator curve (AUC) (r^2=0.68 for the training dataset), indicating minimal tradeoffs in using this metric for model selection (**Table 1**).”

It can be tabularly assessed in **Table 1** but we provide a figure below for graphical reference. We did not add this visual to the supplement because the supplement is already long and we judged **Table 1** sufficient for conveying this minor result.



Here, I've provided a restructured version of the paragraph (409-425):
"The best-fitting model performed well for all species, except for Brandt's cormorant and harbor porpoise, where poor performance was observed (Cohen's kappa values of less than 0.2). Weighted random forest models performed best for California sea lion, common murre, northern elephant seal, and harbor seal with case weights of 25, 25, 25, and 75, respectively (Table 2). For common murre, the Cohen's kappa value was 0.71, indicating 'good' performance, while for harbor seal, California sea lion, and northern elephant seal, Cohen's kappa values were 0.25, 0.24, and 0.23, respectively, indicating 'fair' performance."

> We appreciate this suggestion as it more succinctly describes our results. We adopted this suggested language in the paper with a few small edits. The paragraph now reads:
>
> > "The best-fitting model performed well for all species, except for Brandt's cormorant and harbor porpoise, which exhibited poor performance (Cohen's kappa less than 0.2) and were therefore excluded from further consideration. Weighted random forest models performed best for California sea lion, common murre, northern elephant seal, and harbor seal with case weights of 25, 25, 25, and 75, respectively (**Table 1; Fig. S.13**). For common murre, Cohen's kappa was 0.71, indicating "good" performance, while for harbor seal, California sea lion, and northern elephant seal, Cohen's kappa was 0.25, 0.24, and 0.23, respectively, indicating "fair" performance. Model performance was positively correlated with the frequency of bycatch observations, i.e., species with more observed bycatch events produced models with greater skill (**Table 1**; $r^2 = 0.64$ for Cohen's kappa for training data). Cohen's kappa was positively correlated with the area under the receiver operator curve (AUC) ($r^2 = 0.68$ for the training dataset), indicating minimal tradeoffs in using this metric for model selection (**Table 1**)."

Lines 427-430: I suggest the authors explicitly include the categorization of the driver predictors as environmental, fishing-related, temporal, and spatial in the Materials and Methods section (paragraphs 262-268). Please, the process of predictor selection

> We added the following underlined text to explain variable selection and categorization:
>
> > "We considered nine attributes of fishing as potential drivers of bycatch risk: haul depth (fathoms), mesh size (inches), soak time (hours), latitude (°N), longitude (°W), distance from shore (km), Julian day, sea surface temperature (°C), and whether the fishing occurs near an island (i.e., within 10 km of island coast). <u>These attributes were selected based on their demonstrated relationship to bycatch risk in other papers (e.g., Bettoli et al. 2006, Bjørge et al. 2013, Kroetz et al. 2020) and their availability in the observer data or their ability to be derived through remote sensing (i.e., distance from shore, temperature, island area). They represent a range of spatial (latitude, longitude, distance from shore, depth,</u>

island area), temporal (Julian day), environmental (temperature), and fishing-related (soak time, mesh size) attributes."

- Kroetz et al. 2020: region, latitude, season, sea surface temperature, depth, net depth, net length, mesh size, gear type, target, and soak time
- Bettoli et al. 2006: mesh size, fish length, water temperature, soak time, and twine type
- Bjørge et al. 2013: Season, areas, depth, soak time

- Kroetz, A.M., Mathers, A.N. and Carlson, J.K., 2020. Evaluating protected species bycatch in the US Southeast Gillnet Fishery. *Fisheries research*, *228*, p.105573.
- Bettoli, P.W. and Scholten, G.D., 2006. Bycatch rates and initial mortality of paddlefish in a commercial gillnet fishery. *Fisheries Research*, *77*(3), pp.343-347.
- Bjørge, A., Skern-Mauritzen, M. and Rossman, M.C., 2013. Estimated bycatch of harbour porpoise (Phocoena phocoena) in two coastal gillnet fisheries in Norway, 2006–2008. Mitigation and implications for conservation. *Biological Conservation*, *161*, pp.164-173.

Lines 428-430: How did the authors address potential collinearity between the variables? For example, three spatial variables were included in the same model, which may lead to high collinearity.

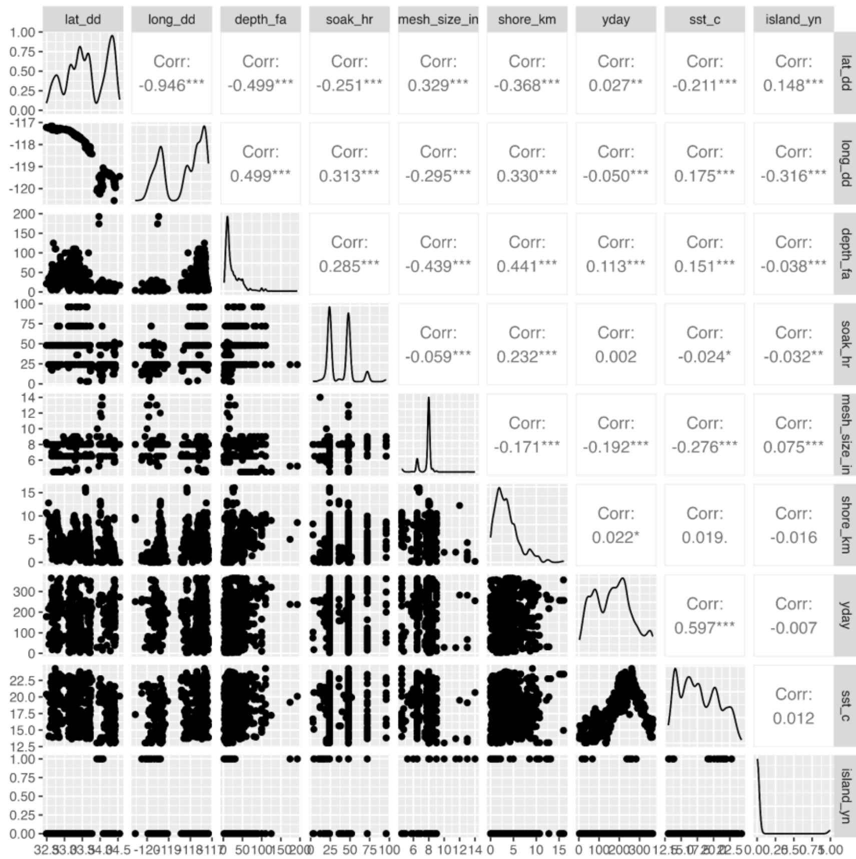
We added the following underlined text to clarify that a strength of the random forest approach is that, compared to classical regression techniques, it is insensitive to collinear and unimportant variables:

> "We used random forest classification models trained on the observer data to identify drivers of bycatch risk for each of the six study species. We used a random forest approach, a machine learning method that ensembles predictions from hundreds of decision trees, rather than a classical regression method (e.g., generalized linear or additive models) because of their comparatively high predictive skill to rare events, ability to model non-linear relationships, and insensitivity to collinear or unimportant predictor variables (Cutler et al., 2007; Prasad et al. 2006)."

Additionally, among the three spatial variables considered, only latitude and longitude showed a strong correlation with each other (r^2 > -0.9) (**see figure below**). The correlation between latitude and distance to shore was -0.368, while longitude had a correlation of 0.33 with distance to shore. Despite the collinearity between longitude and latitude, we chose to include both variables for several reasons: (1) random forest models are generally less sensitive to collinearity issues compared to traditional regression techniques, as noted above; (2) including longitude resulted in higher model skill and improved the model's ability to predict bycatch hotspots observed around the Channel Islands; and (3) other studies using random forest for spatiotemporal bycatch predictions typically incorporate both latitude and longitude (Stock et al., 2020).

Stock, B. C., Ward, E. J., Eguchi, T., Jannot, J. E., Thorson, J. T., Feist, B. E., & Semmens, B. X. (2020). Comparing predictions of fisheries bycatch using multiple spatiotemporal species distribution model frameworks. *Canadian Journal of Fisheries and Aquatic Sciences*, *77*(1), 146–163. https://doi.org/10.1139/cjfas-2018-0281

A correlogram showing the correlation between the predictor variables.



Lines 433-436: This sentence should be moved to the discussion section.

We deleted this sentence. It is already discussed in the discussion.

Lines 438-439: What do the authors mean by 'correlated' and 'no-correlated'?

We replaced “correlated” and “non-correlated” with “similar” and “dissimilar”. We meant that species sometimes show similar responses to drivers and that they sometimes show dissimilar responses:

> “The species exhibit a mixture of <u>similar</u> and <u>dissimilar</u> responses to the explanatory variables (**Fig. 5**).”

Lines 439-441. Simply stating that a peak was observed at 34° latitude does not necessarily indicate that it represents nearshore areas. To confirm this, additional longitude information would be needed to pinpoint the location more accurately and determine its proximity to the shore.

> We added the following underlined text to clarify that the spike at 34° included some deeper offshore areas:
>
> > "California sea lion and harbor seal exhibit similar responses in bycatch risk. Both species have higher bycatch risk in shallower depths in nearshore areas with a spike in risk occurring around 34°N latitude, including some deeper offshore areas (**Fig. 6**)."
>
> We also refer the reader to **Figure 5**, which shows the marginal effects of depth, distance to shore, latitude, and longitude on bycatch risk and **Figure 6**, which shows maps of bycatch risk. Together, these figures substantiate this statement.

Lines 446-448: The dome-shaped relationship is more apparent for the California sea lion and harbor seal, whereas for the elephant seal, a decrease in bycatch risk is observed with increasing temperature.

> We rewrote this sentence, which now reads as follows:
>
> > "For all four species, bycatch risk exhibits an asymptotic relationship with soak time, though the shapes of these relationships differ by species. The species exhibit complex and variable relationships to temperature."

Line 449: The 'Maps of Bycatch Risk' section is lengthy and repetitive. A reduction is required.

> We shortened this section to a single paragraph by moving the sentences that were more related to results interpretation to the discussion. The paragraph now reads:
>
> > "The species exhibited different patterns of spatial bycatch risk. California sea lion bycatch risk is predicted to be highest in four areas: (1) on the northern coasts of the northern Channel Islands, especially on the northern coast of Santa Rosa; (2) a small nearshore area west of Santa Barbara; (3) the eastern coast of Santa Cruz Island; and (4) the northwestern shores of Santa Catalina and San Clemente Islands; (**Fig. 6**). Harbor seal bycatch risk is predicted to be highest in four areas: (1) the sliver of nearshore area stretching from Santa Barbara to Point Sal; (2) the eastern coasts of Santa Cruz Island; (3) a broad coastal area near Point Mugu; and (4) the sliver of nearshore area stretching from Point Mugu to Point Vicente (**Fig. 6**). Common murre bycatch risk is predicted to be negligible throughout most of southern California (**Fig. 6**). It is only predicted to be high in a small patch near Point Sal and even there, the maximum risk index is much lower than for the other evaluated species. Like common murre, northern

elephant seal bycatch risk is also predicted to be negligible throughout most of southern California except in the region near Point Sal (**Fig.6**).”

Line 451: Where are the Channel Islands and Santa Cruz located?

We added a label for the Channel Islands, the name for the group of islands in southern California, in **Figures 1 and 6**. We added the word “Island” to the end of “Santa Rosa”, “Santa Catalina”, “Santa Cruz”, and “San Clemente” in **Figure 6** to clarify that these are the names of islands (thereby clarifying where Santa Cruz Island is).

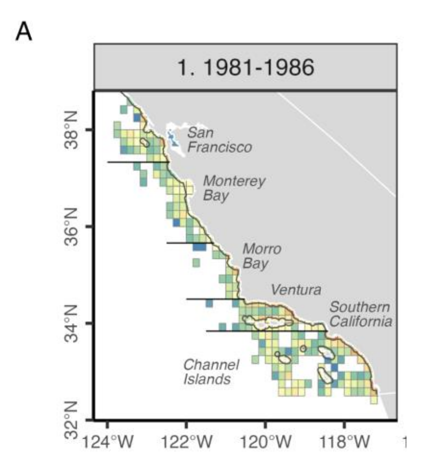


Figure 1 (cropped)

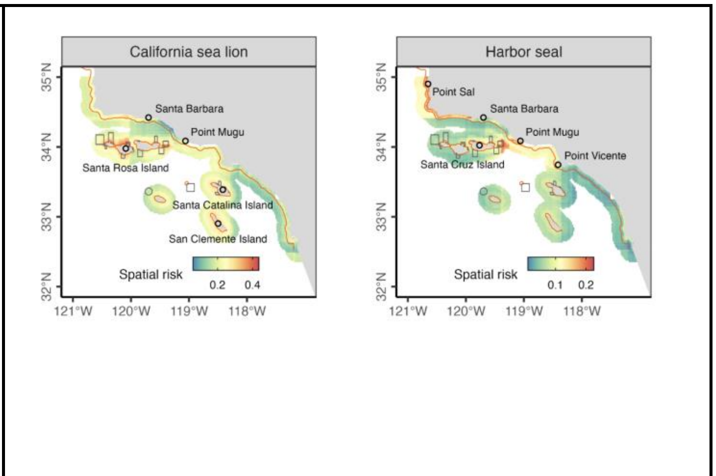


Figure 6 (cropped)

Lines 553-554: This sentence should be moved to the discussion section.
Lines 459-460: This sentence should be moved to the discussion section.
Lines 464-467: This sentence should be moved to the discussion section.
Lines 470-474: This sentence should be moved to the discussion section.

We addressed all four of these comments, which all relate to moving the interpretation of the spatial patterns in bycatch risk to the discussion section, by merging these sentences into a new paragraph in the discussion. The paragraph reads:

“Hotspots of bycatch risk are aligned with the location of known haulouts, breeding colonies, and foraging grounds. High California seal lion bycatch risk around the northern Channel Islands is likely related to the large haulouts of sea lions in that area (**Fig. S15**). Similarly, hotspots of harbor seal bycatch risk correspond to the locations of large harbor seal haulouts on Santa Cruz Island and near Point Mugu (**Fig. S15**). The absence of common murre bycatch risk in southern California is consistent with the distribution of the species, which has no breeding colonies or permanent foraging grounds in southern California (**Fig. S15**). Similarly, the negligible risk for northern elephant seals is consistent with the phenology of their migrations. Although northern elephant seals breed on the Channel Islands and near San Simeon/Cambria (**Fig. S15**) from December to March, they disperse to their distant foraging grounds (males to Alaska and females to oceanic waters far West of California) before the fishing season peaks from April to June, significantly reducing their vulnerability to the gillnet fishery."

We believe that the comment referring to lines 553-554 intended to refer to lines 453-454 based on its order in the review and its similarity to the following comments. Lines 553-554 were in the fourth paragraph of the discussion in the original submission.

Lines 476-477: If random forest models also estimate bycatch, why did the authors choose to use ratio estimation methods for bycatch estimation? What is the rationale for using both methods? This reasoning should be explicitly addressed in the Materials and Methods section

Please see our detailed response where this comment is made for the first time. Briefly, we used both methods because (1) the design-based ratio estimation approach has been used to estimate bycatch in all historical assessments (**Table S1**) but (2) model-based approaches are generally thought to perform better and were therefore worth exploring here. Ultimately, we found the model-based approach to be unsuitable for this case study due to the loss of raw observer data and the inability of this approach to use summarized data. In contrast, the ratio estimation approach was less sensitive to the loss of raw observer data because it can use summarized data. We moved the methods and results associated with the estimation of bycatch using the random forests to the supplemental information to avoid confusion.

Lines 476-493: The authors have conflated results with discussion in this paragraph, as well as throughout the results section. A restructuring of the results section is necessary to clearly distinguish between these components. Additionally, this paragraph repeats information already mentioned in the first paragraph of the results section. Similar topics, such as bycatch trends, should be grouped together to enhance coherence and avoid redundancy

This entire paragraph, which presented the results of bycatch estimation using the random forest models, was moved to the supplemental information. As a result, the blended results and discussion is more appropriate because it now occurs in the supplemental information. However, we have reviewed and edited the rest of the results, including the sentences flagged above, to ensure that results interpretation occurs in the discussion and not in the results section.

Lines 477-479: Agreement between two methods does not inherently validate the observed trend—it only demonstrates consistency between the results produced by the methods. The validity of the trend depends on the robustness of the methodologies, the quality of the data, and the assumptions underlying the analysis.

We deleted this sentence. Furthermore, this entire paragraph, which presents results and discussion related to the use of the random forest models to estimate bycatch, was moved to the supplemental information.

Lines 488-490: What does the author mean by this sentence? It is very confusing, and clarification is needed.

We entirely rewrote the underlined sentence to clarify its meaning. It now reads:

> "<u>Unlike the random forest models, the ratio estimators are able to use summarized observer data for this region and time period from old reports. As a result, the ratio estimators can learn from observations from this region and time period while the random forest models are blind to data from this region and period. Thus, the random forest models are </u>likely to underpredict risk in early years in northern strata because they largely learned from late years in southern California, where risk was lower."

Furthermore, this entire paragraph, which presents results and discussion related to the use of the random forest models to estimate bycatch, was moved to the supplemental information.

Discussion

Lines 495-496: A reference for this sentence is not needed. Remove '(Carretta et al., 2014)'.

We removed the reference for this sentence, which now reads:

> "Our study provides the first update to total estimates of protected species bycatch in the California set gillnet fishery since 2012."

Lines 500-501: Reference for this sentence is required.

This is a novel result of our study; thus, providing a reference is inappropriate and not possible. We could refer to **Fig. 3** but the figure and result was already presented in the results section.

Lines 501-503: What do the authors mean by: 'are more due to reductions in fishing effort than to reductions in bycatch rates (i.e., bycatch per unit effort)', since reducing fishing effort is expected to lead to a decrease in bycatch rates? Please clarify this statement.

We added the following underlined text to clarify this sentence:

> “These advances, while directly attributable to management interventions, are more due to reductions in fishing effort <u>(i.e., fewer fishing trips)</u> than to reductions in bycatch rates (i.e., <u>lower</u> bycatch per fishing trip).”

We hope that this clarifies how bycatch *rates* are different from bycatch *amounts*. Reduced fishing effort results in reduced bycatch amounts assuming bycatch rates are unchanged. Reduced fishing effort does not reduce bycatch rates.

Amount = effort * rate
Amount of total bycatch = [number of trips] * [bycatch per trip]

Line 505: What do the authors mean by 'sustained declines'? Please clarify this statement.

We meant that fishing opportunities have undergone consistent declines while marine mammal populations have undergone consistent growth. We replaced “sustained” with “prolonged” to clarify this meaning. The sentence now reads as follows:

> “This highlights a steep tradeoff between conservation and fisheries objectives under the current management regime: while populations of protected species have undergone sustained growth, fishing opportunities and revenues have undergone <u>prolonged</u> declines.”

Lines 508-510: In the first paragraph of the introduction, the authors state: 'In this study, we reconstruct the bycatch of select marine mammals and seabirds in the California set gillnet fishery from 1981-2022 and identify drivers of bycatch that could be used to design management measures that effectively and efficiently reduce bycatch.' However, in the first paragraph of the discussion section, they state: 'Our results indicate that current fishing operations do not pose a threat to the evaluated species, which suggests that current management is sufficient at limiting bycatch.' If current management is already effective, why is there a need to identify drivers to efficiently reduce bycatch?
The need to identify drivers of bycatch risk, despite the low levels of bycatch, could be more clearly justified. It may be helpful to emphasize that understanding these drivers is important for enhancing knowledge and fine-tuning management practices to ensure that bycatch remains at low levels over time, even as conditions change. This would make the identification of drivers less about urgent management needs and more about ongoing refinement of strategies and maintaining low bycatch levels.
The justification for identifying the drivers could be more explicitly linked to improving long-term sustainability or enhancing the effectiveness of current measures, rather than suggesting that further management interventions are immediately necessary.

We did not know that bycatch levels were low before embarking on this study. In fact, conservation groups were claiming that bycatch levels were high and problematic and were therefore lobbying for increased restrictions on the fishery. As a result, the *a priori* objectives of our study were to estimate current bycatch levels and to identify drivers of bycatch that could be used to inform management strategies that lead to effective and **efficient** bycatch reduction. We bold "efficient" because it is distinct from "effective". An effective strategy avoids or limits bycatch, but might not consider impacts to fishing opportunities. An efficient strategy achieves bycatch reduction goals while minimizing impacts on fishing opportunities. Although we ultimately found that management has been effective at reducing bycatch (i.e., current bycatch levels are below NOAA's net zero mortality rate goal), they have caused great hardship for the fishery: as summarized in the introduction, the fishery has shrunk from generating $15 million annually for 400 vessels in 1987 to generating $1 million annually for 40 vessels today. As a result, we evaluate drivers of bycatch to propose management refinements that expand fishing opportunities while still achieving bycatch reduction goals (i.e., improve efficiency).

We took several actions to make the above explanation in the paper.

First, as described in response to comments above, we clarified that the study was motivated by claims from conservation organizations that bycatch is high and problematic and that the fishery requires more regulation. To evaluate these claims, we naturally asked the questions: *"how much bycatch is there"* and *"what can be done about it"*? We did not know if bycatch was high or low before conducting our study.

Second, we clarified the definition of efficient management and our goal to support more efficient management by adding/editing the following underlined text to the last paragraph of the introduction, where we introduce the study objectives:

> Opening sentence: In this study, we reconstruct the bycatch of select marine mammals and seabirds in the California set gillnet fishery from 1981-2022 and identify drivers of bycatch that could be used to refine management to more efficiently reduce bycatch, where efficient management achieves conservation objectives while minimizing impacts on fishing opportunities.
>
> Final sentence: Based on these results, we make recommendations for how management could more efficiently manage bycatch risk through measures such as seasonal or spatial closures, depth restrictions, or gear restrictions.

We made other small edits throughout the introduction and discussion to clarify this. Collectively, these edits build on other sentences already included in the abstract and introduction including:

> Abstract: "The identification of efficient management strategies that reduce protected species bycatch while also minimizing impacts on fishing livelihoods is a global conservation challenge. Identifying such strategies requires understanding levels of bycatch relative to management targets as well as the relationship between bycatch risk and potential management levers."

> Introduction: “Despite declining fishing effort and bycatch, conservation groups are lobbying for additional restrictions, including permanent closure, to further avoid bycatch (Birch et al., 2023; Birch and Shester, 2023). There is thus great need for scientific guidance on management regulations that are likely to provide conservation benefits while also avoiding unnecessary burdens on the fishing industry.”

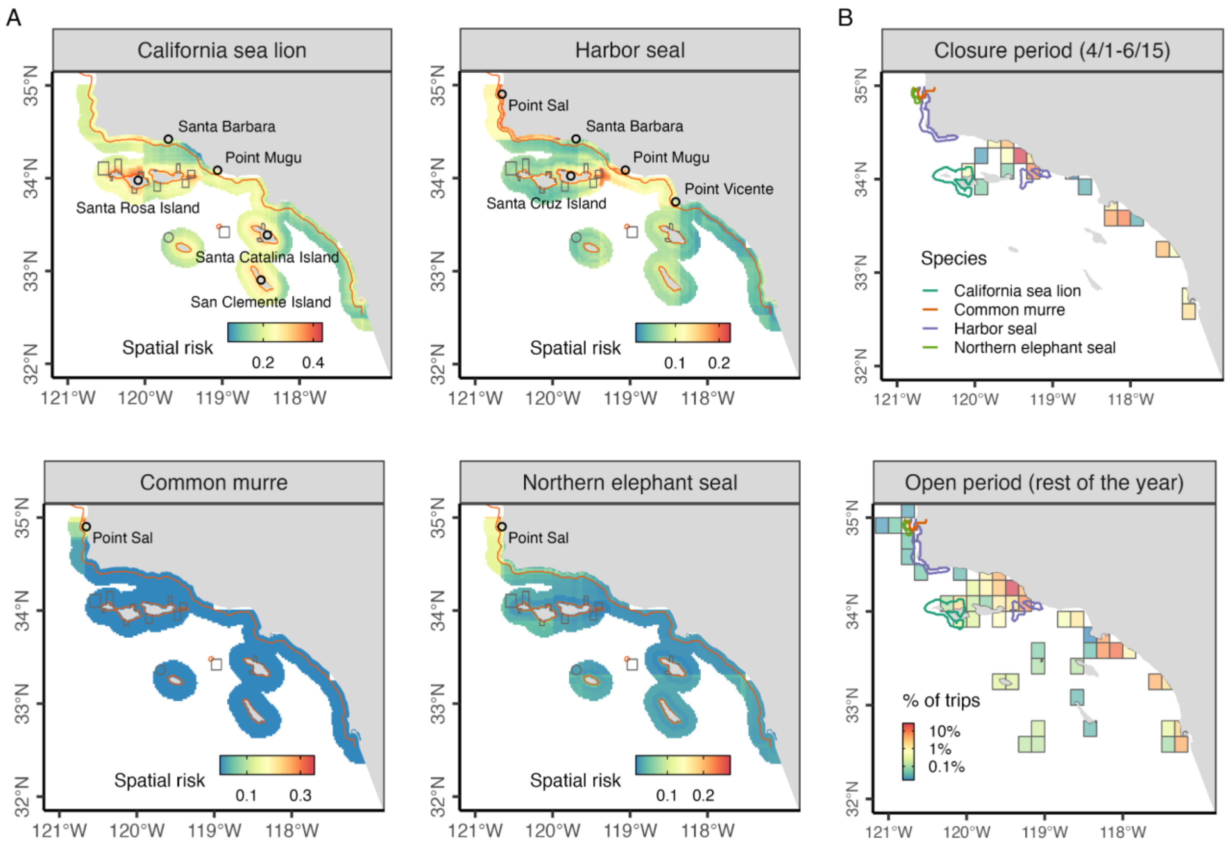
Third, we made all of the changes described in response to the comment below to substantiate why spatial-temporal hotspot closures could empower efficient management that achieves the same level of bycatch reduction while increasing fishing opportunities.

More broadly, these results can be used to anticipate the impact of future changes in management regulations on bycatch, whether they are motivated by bycatch or not.

Lines 508-510: But how could identifying spatial-temporal bycatch hotspots improve economic outcomes? The authors mention this justification in the introduction, but do not explain how.

We took several actions to address this important comment.

First, we expanded **Figure 6** to show the identified bycatch hotspots relative to both the distribution of (1) current closures and (2) recent fishing effort. This illustrates the potential efficiency of seasonal closures by showing that (1) there are areas currently closed to fishing that have low predicted bycatch risk and (2) that bycatch hotspots are located in areas of relatively minor fishing importance, which suggests that the loss of fishing opportunities associated with their brief seasonal closure could be made up by opening areas of low predicted risk that are currently closed to fishing. The figure is shown below for reference.



Second, we added the following underlined text to the discussion to describe this result:

> "Our results suggest that spatial-temporal management could more efficiently and effectively manage bycatch risk than gear modifications or soak time regulations. Specifically, bycatch rates for California sea lions and harbor seals are greatest from April 1 to June 15, suggesting that a 2.5 month seasonal closure of bycatch hotspots for these two species could prevent bycatch while allowing the opening of less risky but currently closed areas to fishing. These hotspots are predicted to disproportionately contribute to bycatch yet are of minor fishing importance (**Fig. 6B**) suggesting that the loss of fishing opportunities in these areas during brief seasonal closures could be easily made up by opening areas of low predicted bycatch risk. As a result, seasonal closures could broaden fishing opportunities while continuing to meet bycatch avoidance objectives. However, we caution that such hotspot closures could exacerbate bycatch problems if fishing effort is displaced and concentrated in areas of secondarily high risk (Free et al., 2023). Therefore, monitoring of fishing effort and bycatch rates are important for verifying that seasonal closures achieve their conservation and fisheries objectives. Additionally, any changes in current management strategies must take into account spatial-temporal patterns of bycatch and relative sensitivity of each species."

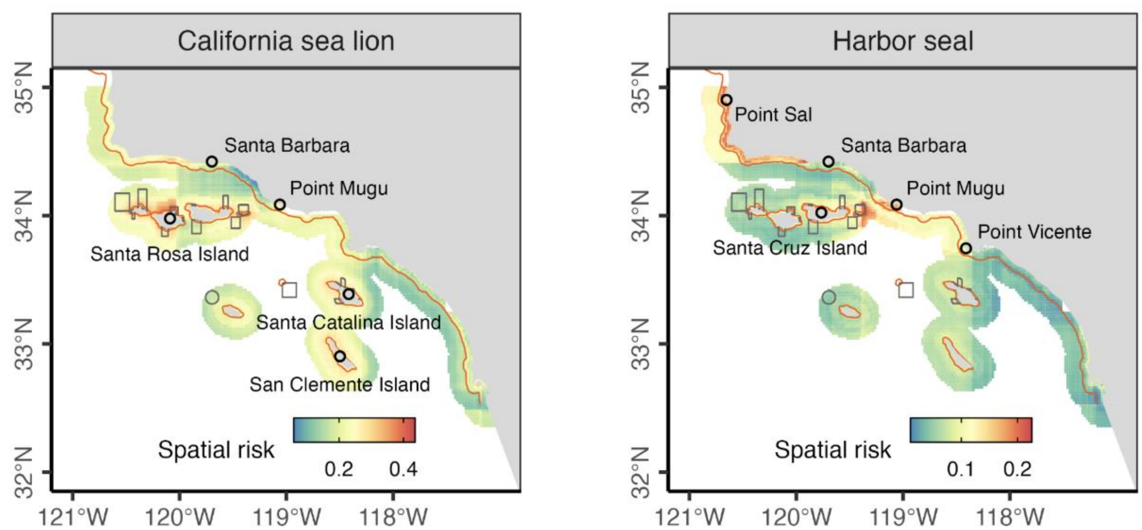
Finally, as described in response to other comments, we revised the introduction to better introduce the concept of efficient management that achieves bycatch objectives while minimizing the impact on fishing opportunities.

Lines 516-524: A figure showing the 0-3 nautical miles offshore area is required, as well as all other restrictions. It is important to visually represent the area using shaded polygons to help the reader understand the coverage of the restricted areas for all fishing regulations.

We added a thin orange line to **Figure 6** to delineate the nearshore areas where gillnet fishing is prohibited: (1) the area 0-3 nautical miles offshore from the mainland, and (2) the area extending 1 nautical mile or 70 fathoms (whichever is closer to shore) around the Channel Islands. Additionally, we mapped Marine Protected Areas, where set gillnet fishing is also excluded, as grey polygons. We added the following sentence to the caption to explain this regulation:

> “The thin orange coastal line marks the nearshore areas from which gillnet fishing is excluded: within 3 nautical miles of the mainland and within 1 nautical mile or shallower than 70 fathoms (whichever is closer to shore) from the Channel Islands. The gray polygons indicate the locations of Marine Protected Areas in Southern California, where all set gillnet fisheries are excluded. Spatial bycatch risk is shown only for southern California, as this is the only area where the fishery can operate under current regulations.”

Figure 6 (cropped):



Lines 516-524: The authors suggest adjusting the current closed area to target regions identified with higher bycatch risk, justifying that this change could potentially open up more

fishing grounds. However, how can the authors be certain that the current closed area has lower bycatch risk than the areas identified with higher bycatch risk, considering that the 0-3 nautical miles offshore closed area was not sampled? I would expect that areas where fishing is prohibited would not have been sampled. If the current closed area has a higher bycatch risk than the areas identified by the authors, bycatch could increase. Furthermore, even if the current closed area has a lower bycatch risk than the areas identified by the authors, but covers a larger area than the areas identified, bycatch could still increase. The authors need to provide a clearer explanation of how the bycatch risk map was constructed and discuss the implications of changing the current fishing restrictions.

We took several actions to address this comment.

First, we added a new subsection header "2.6.3 Mapping spatial bycatch risk" to better highlight the portion of the methods where the method for constructing the bycatch risk maps is presented.

Second, we expanded **Figure 6** to better illustrate the implications for changing current fishing regulations (i.e., closing high risk areas to fishing and opening low risk areas to fishing) and added text to describe these results to the discussion. See our response to two comments above for details.

Finally, we remind the reviewer that our bycatch risk maps are predicted using a random forest model trained using the full observer data, which spans fishing trips from 1983-1995 (coastwide), 1999-2000 (Monterey Bay), 2010-2013 (South of Point Conception), and 2017 (South of Point Conception). The 0-3 nautical mile closure was implemented after 1994, but prior to that year, set gillnet fishing was still allowed in the current closure area, permitting the observer program to collect samples from these regions (as shown in **Figures S3** and **S7**). By including observer data from the period before 1994 in our model training, we account for bycatch events that occurred within the current state water closure. This helps us capture potential high bycatch risks within the 0-3 nautical mile zone in the model predictions. Notably, our model predicts a bycatch hotspot for harbor seals in a nearshore area stretching from Santa Barbara to Point Sal, within the current 0-3 nautical mile closure (**Figure 6**). This prediction provides evidence that our model effectively learns from historical observations to make predictions in areas that may not have been sampled recently.

Lines 528-530: In the introduction, the authors did not mention that a method comparison was intended. An explanation is required to clarify why the comparison of methods is being presented and how it contributes to the study's objectives.

Please see our detailed response where this comment is made for the first time. Briefly, we used both methods because (1) the design-based ratio estimation approach has been used to estimate bycatch in all historical assessments (**Table S1**) but (2) model-based approaches are generally thought to perform better and were therefore worth exploring here. Ultimately, we found the model-based approach to be unsuitable for this case study due to the loss of raw observer data and the inability of this approach to use summarized data. In contrast, the ratio estimation approach was less sensitive to the loss of raw observer data because it can use summarized data. We moved the methods and results associated with the estimation of bycatch using the random forests to the supplemental information to avoid confusion.

Line 527-530: The authors need to clearly highlight in the Introduction and Materials and Methods sections the rationale for using the ratio estimator to estimate bycatch. Additionally, they should specify the period and region for which bycatch estimation was possible using the random forest model. Furthermore, the authors need to clarify that it was not possible to produce spatial bycatch risk assessments for northern regions due to the absence of data.

Please see our detailed response where this comment is made for the first time. Briefly, we used both methods because (1) the design-based ratio estimation approach has been used to estimate bycatch in all historical assessments (**Table S1**) but (2) model-based approaches are generally thought to perform better and were therefore worth exploring here. Ultimately, we found the model-based approach to be unsuitable for this case study due to the loss of raw observer data and the inability of this approach to use summarized data. In contrast, the ratio estimation approach was less sensitive to the loss of raw observer data because it can use summarized data. We moved the methods and results associated with the estimation of bycatch using the random forests to the supplemental information to avoid confusion.

We clarified that the bycatch estimation using the random forest model was done for all years (i.e., same years as the ratio estimation analysis) in both the main text and supplemental information:

> **Main text:** "We explored using the random forests models to estimate annual bycatch (<u>1981-2022</u>) in the fishery but found them to be unsuitable for this specific case study."
>
> **Supplemental information:** "We used the best fitting model to generate annual estimates of protected species bycatch from <u>1981 to 2022</u> by predicting whether "pseudo-sets" recorded in logbooks were likely to have captured each study species and assuming median numbers of sets and caught animals for "pseudo-sets" with bycatch."

We can use the random forest models to make predictions of spatial bycatch risk in the northern regions; we simply chose to limit the predictions to the southern region, the only area where the fishery can operate under current management, to increase the visibility of bycatch hotspots in this priority region. This was stated in the methods section (see underlined text below):

"To generate maps of spatial bycatch risk, we used the best fitting model to predict risk to a 0.02° grid spanning southern California (32-35°N and 117-121°W), the only area where the fishery can operate under current regulations."

However, we added the following underlined text to the caption of **Fig. 6**, which shows the maps of the predicted spatial bycatch risk, to clarify this choice there.

"**Fig. 6.** Average spatial bycatch risk as estimated by the best fitting random forest model for the four study species with acceptable model performance. The spatial bycatch risk represents the probability of bycatch at a given location under recent (2010-2021) average conditions. Key landmarks for delineating bycatch hotspots are labeled in each panel. Spatial bycatch risk is shown only for southern California, as this is the only area where the fishery can operate under current regulations."

Lines 539-543: It is important to discuss that fishery data obtained from logbooks depends on the trustworthiness of fishers to accurately report bycatch. In some cases, data collected by onboard observers may provide more reliable and accurate information.

This is correct and this is why we used bycatch rates from observed trips (observer data) to predict bycatch from unobserved trips (logbook data).

To clarify this, we added a new "Overview" subsection to the methods where we now provide an overview of both the analytical approach and the datasets used to support this approach. We also added a new opening paragraph in the "Dataset" section to further explain the difference and role between the two datasets.

See responses above for more details.

Lines 545-550: This difference should not be discussed, as it is well-established that a model-based approach to estimate bycatch is preferable to a ratio-based estimate. The use of ratio estimation in this study was merely circumstantial, driven by the absence of raw data. This rationale should be explicitly stated in the Materials and Methods.

This sentence was deleted.

Please see our detailed response where this comment is made for the first time. Briefly, we used both methods because (1) the design-based ratio estimation approach has been used to estimate bycatch in all historical assessments (**Table S1**) but (2) model-based approaches are generally thought to perform better and were therefore worth exploring here. Ultimately, we found the model-based approach to be unsuitable for this case study due to the loss of raw observer data and the inability of this approach to use summarized data. In contrast, the ratio estimation approach was less sensitive to the loss of raw observer data because it can use summarized data. We moved the methods and results associated with the estimation of bycatch using the random forests to the supplemental information to avoid confusion.

Lines 550-552: Taking into account the authors' conclusion, if both methods produce different results, could this suggest that the reported low levels of bycatch might not be reliable? The agreement or disagreement between methods does not inherently confirm or discredit the reliability of the results. Instead, it underscores the need for a discussion focused on the strengths, limitations, and assumptions of each method. Moreover, the comparison between the two methods was not stated as an objective of the present study. It is essential to clarify whether the authors used the ratio estimator due to the lack of raw data for certain years or whether the intention was to compare the ratio estimator with the random forest approach.

We removed this sentence and concept from the paper. Furthermore, see the responses above detailing how and why we moved the estimation of bycatch using the random forests to the supplemental information.

Lines 577: This figure is not included in the Results section. If the authors choose to include figures, they should be presented consistently within the Results section.

This figure illustrates data from another study and not data generated by our study. Thus, these data are not presented as results; instead, they are presented as useful context for interpreting our results, which is the reason why the figure is referenced in the discussion section. We see no policy against this in the *Biological Conservation* author guidelines but will remove this figure and reference if the editor advises us that this practice is against the journal guidelines.

Estimates and drivers of protected species bycatch in the California set gillnet fishery

Abstract

The identification of efficient management strategies that reduce protected species bycatch while also minimizing impacts on fishing livelihoods is a global conservation challenge. Identifying such strategies requires understanding levels of bycatch relative to management targets as well as the relationship between bycatch risk and potential management levers. In this study, we use ratio estimators to reconstruct bycatch of select marine mammal and seabird species in the California ≥3.5" set gillnet fishery from 1981-2022 and random forest models to identify drivers and hotspots of bycatch risk. We find that bycatch has dropped precipitously since the 1980s as a result of management, but at significant costs to fisheries participation and revenues. Recent marine mammal bycatch ranges from 0.1% to 4.0% of the potential biological removal and marine mammal populations are recovering. Spatial-temporal drivers of bycatch risk were more important than fishing-related drivers of risk, suggesting that spatial-temporal closures would be more effective than mesh size or soak time restrictions at ~~further~~ limiting bycatch. For each species, we identified 2-5 hotspots of elevated bycatch risk as candidates for temporary seasonal closures. Bycatch risk for harbor seal (*Phoca vitulina*) and California sea lion (*Zalophus californianus*), the species with the greatest bycatch risk, is especially high from April 1st to June 15th, suggesting that hotspot closures during this 2.5-month time period could be ~~especially~~particularly efficient. Our study also highlights the value of competing multiple modeling approaches to identify methods that best predict rare bycatch events.

Keywords: gillnet, bycatch, marine mammals, seabirds, area-based management

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1. Introduction

Bycatch, the accidental capture of non-target species in fisheries, presents a significant conservation and economic challenge (Crowder and Murawski, 1998; Soykan et al., 2008). Bycatch of large-bodied, slow-growing, low-productivity species such as marine mammals, sea turtles, and seabirds (Crowder and Murawski, 1998; Read et al., 2006; Soykan et al., 2008) is of particularly high concern, as the mortality of just a few individuals in these vulnerable populations, often recovering from historical exploitation, can threaten population collapse and even extinction ~~(Geijer and Read, 2013; Read et al., 2006).~~(Geijer and Read, 2013; Read et al., 2006). As a result, many countries have established strict mandates to limit bycatch of vulnerable species, which can result in fisheries closures and other severe restrictions ~~(Crowder and Murawski, 1998; Senko et al., 2014).~~(Crowder and Murawski, 1998; Senko et al., 2014). These management disruptions can have serious social, cultural, and economic impacts on fishing communities ~~(Smith et al., 2020).~~(Senko et al., 2014). Due to the negative ecological, economic, and social ~~impacts~~consequences of fishery bycatch, bycatch avoidance is an important objective for global fishery management. A sustainably managed fishery with low bycatch can not only provide ecological benefits, but also social and economic benefits by providing a sustainable source of income, food, and nutrition.

To guide effective bycatch reduction policies, it is important to understand the magnitude of historical and recent bycatch as well as the drivers of bycatch in a fishery. Estimates of total bycatch are needed to determine whether bycatch exceeds management targets or is on pace to exceed targets in the near future ~~(Bjørge et al., 2013; Geijer and Read, 2013; Read et al., 2006).~~(Bjørge et al., 2013; Geijer and Read, 2013; Read et al., 2006). Historical bycatch estimates ~~also~~ offer insights into the effectiveness of past management interventions, which provide useful benchmarks for adapting management in response to ~~contemporary~~recent bycatch levels and trends. Understanding the drivers of bycatch risk is critical to guiding effective and efficient management adaptations. For example, determining whether bycatch is concentrated within specific areas or seasons can support the design of time-area closures that prevent fishing when and where risk is high while maintaining fishing opportunities elsewhere ~~(Lewison et al., 2014; O'Keefe et al., 2023; Soykan et al., 2008)~~(Lewison et al., 2014; O'Keefe et al., 2023; Soykan et al., 2008). Similarly, gear~~ restrictions~~, soak time~~ restrictions~~, or time of day restrictions can be used to curb bycatch if there are strong relationships between bycatch risk and gear or other characteristics of fishing ~~(O'Keefe et al., 2023)~~(O'Keefe et al., 2023). Without this information, bycatch management must be precautionary to guarantee compliance with protected species legislation, which could forego considerable fisheries yields.

Because observer programs, which place trained scientists on fishing vessels to collect bycatch data, are costly and rarely cover all fishing trips, various analytical approaches have been developed to estimate unobserved bycatch and to evaluate drivers of bycatch risk. Ratio estimation, ~~which~~a design-based approach that assumes that the rate of bycatch in observed fishing trips is proportional to the rate for all fishing trips, is one of the most common strategies ~~(Cochran, 1977; Stock et al., 2019)~~(Cochran, 1977; Stock et al., 2019). The reliability of this simple approach increases if there are sufficient data to support estimates within meaningful spatial or temporal strata (e.g., regions, depth zones, seasons). However, ratio estimation can produce biased estimates if other factors (e.g., gear type, soak time, time of day) influence bycatch rates ~~(ICES, 2007)~~(ICES, 2007), if the observed trips are not representative of the unobserved trips, or if low sample sizes lead to spuriously low or high bycatch rates within a stratum ~~(Martin et al., 2015; McCracken, 2004; Ortiz and Arocha, 2004; Rochet and Trenkel, 2005).~~(Martin et al., 2005; McCracken, 2004; Ortiz and Arocha, 2004; Rochet and Trenkel, 2005). Model-based approaches, which use either statistical (e.g., generalized linear models, generalized additive models) or machine learning (e.g., random forests, boosted regression trees) models to estimate bycatch, can overcome many of these limitations by incorporating a wider suite of covariates and ~~are~~by allowing for non-linear relationships and are generally thought to produce better bycatch estimates ~~(Stock et al., 2019)~~(Stock et al., 2019). Additionally, ~~these~~model-based approaches can support management by identifying drivers of bycatch risk and by predicting detailed hotspots of risk ~~(Long et al., 2024; Lopez et al., 2024; Stock et al., 2019)~~(Long et al., 2024; Lopez et al., 2024; Stock et al., 2019).

The California set gillnet fishery would benefit from updated bycatch estimation due to ~~growing~~ concerns about the fishery's impact on protected marine mammals, which have led some conservation organizations to call for the fishery's closure ~~(Birch et al., 2023; Birch and Shester, 2023).~~(Birch et al., 2023; Birch and Shester, 2023). The fishery occurs in southern California and targets California halibut (*Paralichthys californicus*), white seabass (*Atractoscion nobilis*), and Pacific angel shark (*Squatina californica*), among other species. It is currently listed as a Category II fishery under the U.S. Marine Mammal Protection Act (MMPA), indicating that it presents a medium bycatch threat to protected marine species ~~(NOAA, 2024).~~(NOAA, 2024). Bycatch of marine mammal and seabird species, including harbor porpoise (*Phocoena phocoena*), southern sea otter (*Enhydra lutris nereis*), and common murre (*Uria aalge*) was high during the 1980s and 1990s, prompting ~~significant~~large-scale management interventions ~~(Forney et al., 2001; Julian and Beeson, 1998)~~(Forney et al., 2001; Julian and Beeson, 1998). The fishery has also impacted pinniped species such as California sea lion (*Zalophus californianus*), harbor seal (*Phoca vitulina*), and northern elephant seal (*Mirounga angustirostris*). Total bycatch in the fishery has not been estimated since 2012 ~~(Carretta et al., 2014) and there are concerns that bycatch may remain an~~

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~~issue (Birch et al., 2023; Birch and Shester, 2023)~~(Carretta et al., 2014) and some conservation groups are concerned that bycatch remains an issue (Birch et al., 2023; Birch and Shester, 2023).

A number of management actions have been taken to reduce bycatch in the California set gillnet fishery. During the 1980s, high bycatch of southern sea otters and common murres in central California ~~(Barlow et al., 1994)~~(Barlow et al., 1994) led to a depth restriction that closed fishing inside of 40 fathoms (73 m) in 1987 ~~(Forney et al., 2001)~~(Forney et al., 2001). This restriction shut down the fishery in the San Francisco area, effectively pushing it south of Pigeon Point and into Monterey Bay and Morro Bay (**Fig. 1A-2**). In 1990, the state adopted Proposition 132 ~~(CA Secretary of State, 1990)~~(CA Secretary of State, 1990), which went into effect in 1994 and banned the fishery in mainland state waters (0-3 nautical miles) and in waters within 1 nautical mile or 70 fathoms of depth, whichever is less restrictive, around the Channel Islands to further reduce bycatch of protected ~~marine~~ species (FGC §8610.1-8610.16). In 2002, the state expanded the existing depth restriction, closing fishing inside of 60 fathoms (110 m) to avoid the harbor porpoise population in Central California (14 CCR §104.1). This effectively closed the fishery in Monterey Bay and Morro Bay (**Fig. 1A-4**). Currently, the fishery only operates in ~~Southern~~southern California (south of Point Arguello) outside 3 nautical miles from the mainland and outside 1 nautical mile or shallower than 70 fathoms (whichever is less) from the Channel Islands.

Although these regulations are believed to have reduced bycatch in the set gillnet fishery~~,~~ (Carretta et al., 2014), they have also greatly reduced fishery participation and revenues. The implementation of the 40 fathom depth restriction in 1987 triggered a precipitous decline in participation from ~400 vessels in 1987 to ~100 vessels in 1994. Since then, participation has continued to decline, with ~~only~~ ~40 vessels active in 2022, and the vast majority (>90%) of landings coming from just 13 vessels ~~(CDFW, 2023) (**Fig.**~~(CDFW, 2023) (**Fig. 1B**). Fishery effort has similarly decreased from an estimated ~15,000 ~~days of~~ fishing trips in 1987 to 1,000 ~~days~~trips in 2022 (**Fig. 1C**). This reduction in effort has significantly reduced bycatch levels ~~(Carretta et al., 2014)~~(Carretta et al., 2014) but at large costs to fishery revenues. Fleetwide revenues decreased from US$15 million in 1987 to ~~only~~ US$1 million in 2022 (**Fig. 1C**; both values in 2022 dollars). Despite declining fishing effort and bycatch, ~~there~~conservation groups are ~~calls~~lobbying for additional restrictions, including permanent closure, to further avoid bycatch ~~(Birch et al., 2023; Birch and Shester, 2023)~~(Birch et al., 2023; Birch and Shester, 2023). There is thus great need for scientific guidance on management regulations that are likely to provide conservation benefits while also avoiding unnecessary burdens on the fishing industry.

In this study, we reconstruct the bycatch of select marine mammals and seabirds in the California set gillnet fishery from 1981-2022 and identify drivers of bycatch that could be used to ~~design~~refine management ~~measures that effectively and~~to more efficiently reduce bycatch, where efficient management achieves conservation objectives while minimizing impacts on fishing opportunities. We focus on six protected species encountered in the set gillnet fishery: California sea lion, harbor seal, northern elephant seal, harbor porpoise, common murre, and Brandt's cormorant~~.~~ (*Phalacrocorax penicillatus*). Southern sea otter, despite being one of the original species of management concern, is not included in this analysis due to data limitations (**Fig. 2A**). We use ratio estimation methods to reconstruct historical bycatch levels and compare recent bycatch levels to management targets. These methods, which have been used to estimate bycatch in the fishery at various points in the past~~,~~, (**Table S1**), provide a complete time series of bycatch estimates using methods approved for stock assessment and management. We then use random forest models to evaluate drivers of bycatch risk and to make predictions of spatial bycatch risk for four species of concern; Brandt's cormorant and harbor porpoise were excluded from this portion of the analysis due to poor model performance (**Table ~~2~~1**). Based on these results, we make recommendations for how management could ~~effectively~~more efficiently manage bycatch risk through measures such as seasonal or spatial closures, depth restrictions, or gear restrictions.

2. Methods

2.1 Overview

We used a design-based ratio estimation approach to estimate bycatch of select marine mammal and seabird species in the California set gillnet fishery from 1981-2022. Briefly, the ratio estimation approach estimates total bycatch by applying the bycatch rates (i.e., bycatch per trip) on the sample of fishing trips with trained observers (*section 2.4.1*) to all fishing trips, which are documented in the logbooks maintained by all fishing vessels (*section 2.4.2*). Although model-based approaches, including the random forest approach used in the second portion of our analysis, are generally thought to produce better estimates of bycatch than design-based approaches (Stock et al., 2019)(see *section 2.6.4* and the supplemental information for more details)(Stock et al., 2019)evaluate drivers of bycatch and map hotspots of bycatch risk for four of the six evaluated species. We used a random forest approach, a machine learning method increasingly used in ecology, because of its high predictive skill for rare events, ability to model non-linear relationships, and insensitivity to collinear or unimportant predictor variables relative to classical regression techniques such as generalized linear models or generalized additive models (Cutler et al., 2007; Prasad et al., 2006). All analysis was done in R (R Core Team, 2024) and all code and non-confidential data are available on GitHub here: [add link post double-blind peer review].

2.2 Study area

Our study area spans southern and central California, U.S.A. from the U.S.-Mexico border (32°N) to Point Reyes (38°N), which is just north of San Francisco Bay (**Fig. 1A**). The study area falls within the California Current Ecosystem, a highly productive eastern boundary upwelling system spanning from the southern tip of the Baja Peninsula, Mexico (28°N) to the U.S.-Canada border (48°N). Seasonal upwelling of cold, nutrient-rich water fuels populations of krill, squid, sardines, and other low trophic species that are fed upon by larger fishes, sea turtles, seabirds, and marine mammals. The ecosystem also supports important commercial and recreational fisheries and other human uses that benefit tens of millions of people living along the U.S. West Coast. We assessed bycatch within seven regions of the study area, which are shown in **Fig. 1A** and explained in detail in *section 2.5*.

2.3 The fishery

We defined the fishery using the definition in the MMPA List of Fisheries ~~(NOAA, 2023)~~(NOAA, 2023)**:** the ≥3.5 inch mesh set gillnet fishery targeting California halibut, white seabass, Pacific angel shark, and other species. Although this definition deviates from historical studies, which frequently focused on the portion of the fishery using mesh sizes larger than 8.0 or 8.5 inches (**Table ~~1~~S1**), the MMPA definition provides the legal basis for bycatch management and is more consistent with historical regulations. Specifically, a minimum mesh size of 3.5 inches was set for white seabass in 1941, though it was increased to 6.0 inches in 1988 (FGC §8623(d)). Since 1989, California halibut and Pacific angel shark have been targeted using a minimum mesh size of 8.5 inches (FGC §8625(a)). The set gillnet fishery principally excluded by this definition is that for Pacific herring (*Clupea pallasii*), which occurs in California's four largest herring spawning areas — San Francisco Bay, Tomales Bay, Humboldt Bay, and Crescent City Harbor — using mesh sizes of 2.0 to 2.5 inches (CDFW, 2019).

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2.~~2~~4 Data

Our analysis relies on two fisheries-dependent datasets: logbook data and observer data. All gillnet vessels are required to submit logbooks documenting when, where, and how they fished and how much catch was retained. Thus, logbooks characterize all fishing trips. However, because logbooks are self-reported and reported discards are unverifiable, logbooks likely underreport discarded bycatch, especially the bycatch of protected species. As a result, information from observer programs, which place trained observers on a sample of fishing trips (0-16.7% in this fishery; **Fig. 2B; Table S2**), are required to inform estimates of bycatch for the unobserved trips recorded in vessel logbooks, which constitute the majority of fishing effort (83.3-100% in this fishery; **Fig. 2B; Table S2**). Thus, the estimation of total bycatch through ratio estimation depends on both the observer and logbook data. In contrast, the evaluation of bycatch drivers and hotspots with the random forest models uses only the observer data, as these are the only data to accurately record protected species bycatch when it occurs. These datasets are described in detail below.

2.~~2~~4.1 Observer data

We received observer data from 1983 to 2017 from the California Department of Fish and Wildlife (CDFW). There was observer coverage in the California set gillnet fishery from 1983-1995 (coastwide), 1999-2000 (Monterey Bay area only), 2010-2013 (south of Point Conception only), and in 2017 (south of Point Conception only) (**Fig. 2**). The observer program was run by CDFW from 1983-1989 and by the National Oceanic and Atmospheric Administration (NOAA) from 1990 onwards. The percentage of annual fishing trips with onboard observers has varied over time, ranging from 0.3% of trips in 2006 to 16.7% of trips in 1993 (**Fig. 2B; Table S2**). Observers collected information on the amount and fate of catch (kept, discarded, or damaged), the length composition of the catch, the location and time of the catch, and characteristics of the gear used to target the catch (**Fig. S2**). ~~In the state observer data, we defined individual gillnet sets based on the date of fishing, the vessel, and the set number and built a unique identifier to link set-level information across state datasets (i.e., YYYY-MM-DD-VesselD-Set#). In the federal observer data, we defined individual gillnet sets based on the observer trip number and the set number and built a unique identifier to link set-level information across federal datasets (i.e., TripID-Set#).~~We developed a series of simple assumptions to impute missing values for a few key variables (GPS coordinates, fishing depth, soak hour, mesh size) used to describe gillnet sets documented in the observer data (**Fig. S3**; see supplemental methods for details).

Although historical reports document low levels of observer coverage in Morro Bay, Monterey Bay, and San Francisco in the 1980s (**Table ~~1~~S1**), the data that we received from CDFW excluded most of these observations. We ~~digitized~~recovered a small portion of the missing raw data – observations from Monterey Bay from 1987-1989 (**Fig. 2**) – from original CDFW data sheets that were given to a colleague at the Southwest Fisheries Science Center during the late 1990s for a reanalysis of ~~historic~~historical bycatch rates in that region ~~(Forney et al., 2001).~~(Forney et al., 2001). We extracted summaries of set-level bycatch rates from historical reports (**Table ~~1~~S1**) for years and regions missing raw data to support the ratio estimation analysis (**Table S3**). We converted set-level bycatch rates to trip-level bycatch rates assuming an average of 3 sets per trip (**Table S3**), as indicated by the observer data (**Fig. S4**). **Fig.**~~-~~ ~~**Figure**~~ **2C** illustrates the coverage of the available, recovered, and lost observer data.

~~We developed a series of simple assumptions to impute missing values for a few key variables (GPS coordinates, fishing depth, soak hour, mesh size) reported in the observer data (**Fig. S3A**). We assigned missing GPS coordinates using the median coordinates for observed trips within the statistical block most frequently visited by the vessel – in order of preference – that week, month, or year based on the logbook data (described below). We derived missing fishing depths by extracting depths from 25-meter resolution bathymetry data (CDFW, 2002) (**Fig. S3B**). We reassigned missing soak hours the mode value for a vessel and target species (**Fig S3C**). We reassigned missing mesh sizes the mode for – in order of preference – the vessel and target species, the target species, or all vessels (**Fig. S3DE**). We assigned each GPS coordinate to the nearest statistical reporting block (see **Fig. 1A**), which allows points erroneously falling on land to be assigned a likely statistical block. We derived the distance from shore, a covariate used to explain bycatch rates in the random forest model, as the distance of each set to the nearest point on shore.~~

For each species of marine mammal, seabird, and sea turtle documented in the observer data, we calculated the total number of captures observed in the California set gillnet fishery (**Fig. 2**). Throughout this analysis, we focus on the six species with more than 50 observations: ~~Common~~common murre (2,381), California sea lion (1,372), harbor seal (519), Brandt's cormorant (118), ~~Northern~~northern elephant seal (78), and harbor porpoise (97). Unfortunately, this ~~excludes~~excluded southern sea otter, which was a species of significant conservation concern in the 1980s, but whose bycatch was only documented in the lost observer data.

2.~~2~~4.2 Logbook data

We received logbook data from the commercial gillnet fishery from 1981 to 2022 from CDFW (**Fig. 1**). All California gillnet vessels are required to submit logbooks documenting all of their fishing trips; as a result, these logbooks represent all fishing effort associated with the fishery. These data describe vessel information (vessel name, unique identifier, permit number); when (date), where (statistical reporting block~~ id~~; **Fig. 1A**), and how long (hours) a vessel fished; what fish it targeted; what type of gear it used (drift or set gillnet) and characteristics of this gear (length, mesh size, fishing depth; **Fig. S2**); what species it caught; and the amount (number and/or weight) and fate of this catch (kept, released, or lost, including the identity of predators preying on released fish). We attempted to identify individual fishing sets within the logbook data as the unique combination of vessel administration

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information (vessel name, either vessel id or boat number, permit number), where, when, and how long a vessel fished (block id, date, and fishing hours), and characteristics of the gear (net length, mesh size, and fishing depth). This analysis revealed an average of 1 set per trip, which is inconsistent with the 3 sets per trip documented in the more accurate observer data. We term these unique identifiers "pseudo-sets" and view them as roughly equivalent to a fishing trip (**Fig. S4**). ~~We derived the distance from shore, a covariate used to explain bycatch rates in the random forest model, as the median distance from shore of observed trips within the reported block given that exact locations are not reported.~~

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We developed a series of simple assumptions to impute missing or unrealistic values for a few key variables (fishing depth, soak hour, mesh size) ~~reported in the logbook data (**Fig. S5A**). We reassigned both missing (including 0 values) and unrealistic fishing depths, which we defined as depths exceeding the maximum depth in the reported fishing block, the median depth of the fishing block (**Fig. S5B**). We computed the median and maximum depths of each fishing block using 25-meter resolution bathymetry data (CDFW, 2002). We reassigned missing soak hours (including 0 values) the mode value for a vessel. We capped rare and unlikely soak times exceeding 96 hours (4 days) at 96 hours; however, such soak times could theoretically occur during rough weather when it is unsafe to haul gear (**Fig. S5C**). We reassigned missing (including 0 values) and unrealistic mesh sizes, which we defined as mesh sizes exceeding 20 inches, using a hierarchical procedure (**Fig. S5DE**). For logbooks with both vessel identification and target species information, we assigned the mesh size most commonly used by the vessel when targeting that target species. For logbooks with only target species information (no vessel identification), we assigned the mesh size most commonly used when targeting that target species across all vessels (**Figs. S5 & S6**).~~ used to describe gillnet pseudo-sets documented in the logbook data (**Fig. S5 & S6;** see supplemental methods for details).

2.~~2~~4.3 Sea surface temperature

Because sea surface temperature (SST) is a common driver of the distributions of both target and bycatch species ~~(Hazen et al., 2018)~~(Hazen et al., 2018), we used SST as an environmental covariate in the random forest models described below. We derived the SST associated with each set documented in the observer and trip documented in the logbook data using the NOAA 1/4° Daily Optimum Interpolation Sea Surface Temperature (OISST) dataset, which interpolates observations from different monitoring platforms (e.g., satellites, ships, buoys, and Argo floats) to provide a globally complete grid of SST from September 1, 1981 to present ~~(Huang et al., 2021)~~(Huang et al., 2021). For sets reported in the observer data, we extracted the SST at the reported GPS location on the reported day of fishing. For ~~sets~~trips reported in logbooks, we calculated the average SST in the reported block on the reported day of fishing.

2.~~3~~5 Ratio estimation

We estimated annual bycatch for each study species using ratio estimators. Ratio estimators assume that the rate of bycatch in observed fishing trips is proportional to the rate of bycatch within all fishing trips within a given stratum ~~(Cochran, 1977).~~(Cochran, 1977). This assumption requires that the characteristics of observed trips do not systematically differ from the characteristics of all trips, which was confirmed by a two-sided Kolmogorov-Smirnov test ~~(**Fig.**~~for six key traits (i.e., day of year, depth, latitude, mesh size, distance from shore, soak time) (**Fig. S7**). We used trips rather than sets as the sampling unit given the inability to identify unique sets in the ~~logbooks~~logbook data (**Fig. S4B**). This is valid because the number of gillnet sets per fishing trip (median: 3 sets/trip; interquartile range: 2-4 sets/trip) has been consistent through time (**Fig. S4A**).~~S4).~~ Under this approach, the bycatch rate for species s in stratum i ($r_{s,i}$) – where, in this case, strata are defined by years and regions (see next paragraph) – is thus calculated as:

$$r_{s,i} = \frac{k_{s,i}}{d_{s,i}}$$

Where $k_{s,i}$ is the total number of individuals of species s captured in observed trips occurring in strata i and $d_{s,i}$ is the total number of observed trips occurring in strata i. The total estimate of bycatch of species s in strata i ($m_{s,i}$) is then calculated by multiplying the bycatch rate ($r_{s,i}$) by the total number of trips to have occurred in the strata (D_i):

$$m_{s,i} = D_i * r_{s,i}$$

Where the total number of trips (D_i) is derived from the logbook data.

We calculated annual bycatch estimates using a seven-region stratification scheme (**Figs. 1A, S8**). This stratification scheme combines the scheme by Diamond and Hanan (1986) and Julian (1993) for areas north and south of Point Conception, respectively. Although early efforts to estimate bycatch in the California set gillnet fishery often stratified estimates by region and season (**Table ~~1~~S1**), later efforts found that observer coverage was often too limited to employ complex temporal stratification and that estimates between temporally stratified and unstratified approaches were generally similar (**Table ~~1~~S1**).

Stratum-specific bycatch rates for years without observer coverage in the stratum are borrowed from the closest year (forwards or backwards) with observer coverage in the stratum (**Fig. 2C & S9**~~).~~), as has been the practice in previous studies. We collated annual bycatch estimates from past studies (**Table ~~1~~S1**) for comparison with our updated estimates (**Figs. S10-S12**).

Although there are methods for estimating the uncertainty of bycatch estimates generated through ratio estimation ~~(Julian and Beeson, 1998)~~(Julian and Beeson, 1998), we were unable to implement these methods because they rely on bootstrap procedures that sample from the bycatch rates of observed trips. Because these procedures require raw observer data, we cannot use them for (1) ~~species-strata~~ years where summary values from historical reports are used because the raw data have been lost or (2) ~~species-strata~~ years without observer data~~.~~ from within one of the fished strata. As a result, only 6 of the 42 evaluated years had the data required to estimate uncertainty: 2006, 2007, 2010, 2011, 2012, and 2017 (**Fig. 1AC; Fig. 2BC**). An exploration of the uncertainty estimates ~~from~~generated in historical reports with access to the lost data (**Table ~~1) suggest~~S1**) suggests that the median coefficient of ~~estimate~~variation for estimates of annual bycatch estimates ranges from a low of 0.14 for harbor seal to a high 0.47 for harbor porpoise (~~**Figure**~~**Fig.** **S10B**).

We evaluated the sustainability of recent estimated marine mammal bycatch by comparing it to the potential biological removal (PBR) for each stock, which is defined under the MMPA as the maximum number of animals, not including natural mortalities, that may be removed from a marine mammal stock while allowing that stock to reach or maintain its maximum sustainable population. We extracted each PBR from its most recent stock assessment (**Fig. 7**) and compared it to the average estimated catch over the last 10 years (2013-2022). A fishery is managed based on its classification into one of three categories: Category 1 fisheries cause annual mortality and serious injury (M/SI) greater than 50% of the PBR, Category II fisheries cause annual M/SI between 1 and 50% of the PBR, and Category III fisheries cause annual M/SI less than 1% of the PBR. A fishery is considered to be approaching the MMPA's "zero mortality rate goal" (ZMRG) when annual M/SI is below 10% of the PBR.

2.~~4~~6 Random forest modeling

2.6.1 Model training

We used random forest classification models trained on the observer data to identify drivers of bycatch risk for each of the six study species. ~~2.4.1 Model training~~

~~We used random forest classification models trained on the observer data to identify drivers of bycatch risk for each of the six study species.~~ We used a random forest approach, a machine learning method that ensembles predictions from hundreds of decision trees, rather than a classical regression method (e.g., generalized linear or additive models) because of their comparatively high predictive skill for rare events, ability to model non-linear relationships, and insensitivity to collinear or unimportant predictor variables (Cutler et al., 2007; Prasad et al., 2006). We considered nine attributes of fishing as potential drivers of bycatch risk: haul depth (fathoms), mesh size (inches), soak time (hours), latitude (°N), longitude (°W), distance from shore (km), Julian day, sea surface temperature (°C), and whether the fishing occurs near an island (i.e., within 10 km of island coast). These attributes were selected based on their demonstrated relationship to bycatch risk in other papers (e.g., (Bettoli and Scholten, 2006; Bjørge et al., 2013; Kroetz et al., 2020)) and their availability in the observer data or their ability to be derived through remote sensing (i.e., distance from shore, temperature, island area). They represent a range of spatial (latitude, longitude, distance from shore, depth, island area), temporal (Julian day), environmental (temperature), and fishing-related (soak time, mesh size) attributes. For each species, we classified an observed set as having (1) or not having (0) bycatch, and trained a classification model assuming a Bernoulli distribution in the response variable.

Because bycatch of protected species is rare, observed fishing sets show strong class imbalance towards sets without bycatch compared to sets with bycatch. To illustrate, the percent of observed sets with bycatch is as follows: California sea lion (1.02%), common murre (0.43%), harbor seal (0.43%), Brandt's cormorant (0.09%), northern elephant seal (0.076%), and harbor porpoise (0.074%). Therefore, without a proper sample balancing method, predictions are likely to be biased towards the majority class (sets without bycatch), leading to an underestimation of bycatch risk. For this reason, we considered four approaches for accounting for class imbalance resulting from bycatch rarity.

The first three approaches employ different sample balancing methods: (1) downsampling, (2) upsampling, and (3) synthetic minority over-sampling (SMOTE), which uses a mixture of down and upsampling ~~(More and Rana, 2017).~~(More and Rana, 2017). The downsampling approach randomly removes observations of the majority group (sets without bycatch) to obtain equal representation of the majority and minority (sets with bycatch) group. The upsampling approach randomly samples observations from the minority group with replacement to obtain equal representation with the majority group. The synthetic minority over-sampling (SMOTE) approach both up-samples the minority group and down-samples the majority group. It up-samples the minority class by synthesizing new cases from its nearest five neighbors and down-samples the majority class by randomly drawing samples from that group. We created each balanced dataset using the *themis* package in R ~~(Hvitfeldt, 2023)~~(Hvitfeldt, 2023) and fit random forest models to these datasets using the *randomForests* package in R ~~(Liaw and Wiener, 2002)~~(Liaw and Wiener, 2002).

The fourth approach employs weighted random forests, which use observation weighting rather than sample balancing to elevate the importance of the minority class. In this “cost-sensitive” learning approach ~~(More and Rana, 2017)~~(More and Rana, 2017), higher weights are assigned to minority observations so that the model receives a higher penalty for misclassifying these observations, helping to reduce bias towards the majority class. We evaluated multiple weighting schemes to optimize the predictive skill of this approach. Specifically, we assigned majority observations (sets without bycatch) a weight of 1 and assigned minority observations (sets with bycatch) weights of 25 to 200 in increments of 25. Thus, a total of eight candidate weighted random forest models were evaluated as described below. We fit the weighted random forest model using the *ranger* package in R ~~(Wright and Ziegler, 2017).~~(Wright and Ziegler, 2017).

We trained each of the eleven candidate models (three balanced random forest models, eight weighted random forest models) on 80% of the observer data, withholding the remaining 20% for model testing. In training the models, we performed a grid search to identify the “mtry” hyperparameter – the number of variables to randomly sample as candidates at each node split – that maximizes Cohen’s kappa under 10-fold cross validation (**Fig. S13**). While accuracy measures the proportion of correctly classified categorizations, Cohen’s kappa measures the proportion of correct classifications while accounting for the probability of being correct by chance and is a better measure of predictive skill, especially for imbalanced datasets ~~(Cohen, 1968).~~(Cohen, 1968). Although there are no definitive rules for interpreting Cohen’s kappa, general guidelines suggest that values above 0.7 are ~~‘excellent’,~~“excellent”, 0.4-0.7 are ~~‘good’,~~“good”, 0.2-0.4 are ~~‘fair’,~~“fair”, and below 0.2 are ~~‘poor’~~“poor” ~~(Fleiss et al., 2013; Landis and Koch, 1977)~~(Fleiss et al., 2013; Landis and Koch, 1977). We identified the best fitting model as the model generating the highest Cohen’s kappa on the training data. We applied this model to the test dataset for an independent evaluation of its predictive power. We only evaluated four species (California sea lion, common murre, harbor seal and northern elephant seal) whose best models exhibited “fair” or better performance in their training dataset and close to “fair” performance on the test dataset for the rest of the analysis (**Table ~~2~~1**).

2.~~4~~6.2 Model evaluation~~ and prediction~~

We evaluated the drivers of bycatch risk for each species by inspecting the variable importance and the marginal effects of each variable as estimated in the best fitting model. Variable importance was evaluated as the total decrease in node impurities from splitting on the variable averaged over all trees. The impurity measure is corrected when building the model to reduce its bias towards continuous variables ~~(Nembrini et al., 2018)~~(Nembrini et al., 2018). Marginal effects measure the impact of the changes in one variable on the response variable while all other variables are held constant. The marginal effects plots provide the scientific basis for our discussions of management regulations that could effectively and efficiently reduce bycatch risk.

~~We used the best fitting model to generate annual estimates of protected species bycatch from 1981 to 2022 by predicting whether “pseudo-sets” recorded in logbooks were likely to have captured each study species.~~

2.6.3 Mapping spatial bycatch risk

~~We predict to pseudo-sets rather than trips because the random forest model is trained on set-level covariates in the observer data. We used the best fitting model for each species to estimate the probability that a logged pseudo-set included bycatch of a species then categorized the pseudo-set as with or without bycatch using a species-specific probability threshold. We derived the species-specific probability thresholds as the threshold that maximizes Cohen’s kappa when applied to the training datasets (**Fig. S14**). We selected the probability threshold based on Cohen’s kappa rather than the area under the receiver operator curve (AUC) because (1) the models were tuned and selected based on Cohen’s kappa and (2) simulation work shows that deriving thresholds based on AUC tends to overestimates the prevalence of rare events while it underestimates the prevalence of common events (Freeman and Moisen, 2008; Manel et al., 2001). We summed the number of pseudo-sets predicted to have bycatch each year, converted this sum to “true sets” assuming three sets per pseudo-set (**Fig. S4AB**), and multiplied this sum by the median number of captures when a capture occurs to generate estimates of the total number of captured animals (**Fig. S4C**). We opted not to employ a more complex two-stage or hurdle model approach, where a second model estimates the number of captured individuals when bycatch occurs, given the rarity of bycatch events larger than one for all species but common murre (**Fig. S4C**).~~

To generate maps of spatial bycatch risk, we used the best fitting model to predict risk to a 0.02° grid spanning southern California (32-35°N and 117-121°W), the only area where the fishery can operate under current regulations. Conceptually, our metric of spatial bycatch risk represents the probability of bycatch at a given location under recent average conditions. We derived this metric by first predicting the probability of bycatch on every calendar day (Julian day 1 to 365) for each grid cell. The ~~eight~~nine input variables for making these predictions were derived as follows: (1) *latitude*, (2) *longitude*, (3) *distance from shore*, ~~and~~ (4) *depth*, and (5) *island proximity* (i.e., whether the location was within 10 km of an island) were derived based on the centroid of the grid cell; (~~5~~6) *soak time* was set to 24 hours, the logbook mode; (~~6~~7) *mesh size* was set to 8.5 inches, the logbook mode; (~~7~~8) *Julian day* was the input day; ~~and (8~~(9) *sea surface temperature* (SST) was the average SST for that day and grid cell from 2010-2022. We then calculated the weighted average of the daily risk weighting by the amount of logbook entries (a metric of fishing effort) occurring on that Julian day from 2010-2022. We cropped the predictions to areas within 20 km of shore as this is the farthest offshore that the fishery has operated (**Fig. S7**). We delineated bycatch hotspots as areas falling within the 95th (California sea lion, harbor seal) or 99th (common murre, northern elephant seal) percentile contour of spatial risk for each species. A higher threshold was used for common murre and northern elephant seal because they exhibit a large number of very low risk cells.

2.6.4 Bycatch estimation

We explored using the random forests models to estimate annual bycatch (1981-2022) in the fishery but found them to be unsuitable for this specific case study. Although model-based approaches generally perform better than design-based approaches at estimating bycatch (reference), we found that our random forest models underpredicted bycatch risk in the 1980s and 1990s relative to the ratio estimator (**Fig. S14**). This is most likely because of the loss of observer data from the northern strata during this time period, fishery largely operated in the northern portion of the region. See the supplemental information for details on the estimation of bycatch using the random forest models.

3. Results

3.1 Ratio estimation

In general, estimated bycatch peaked in the mid-1980s, steadily declined following the 40-fathom depth restriction implemented in 1987, with a temporary increase in the late-1990s followed by continued decline (**Fig. 3**). Bycatch peaked, in order of decreasing magnitude, at 5,059 common murre in 1984, 3,437 California sea lion in 1987, 2,605 harbor seal in 1986, 560 harbor porpoise in 1985, 453 Brandt's cormorant in 1991, and 432 northern elephant seal in 1986 (**Fig. 3**). This pattern reflects trends in fishing effort, which also declined after a peak in 1985, with a brief expansion in the late-1990s followed by continued decline (**Fig. 1C**). Estimated bycatch of ~~Brandt's cormorant follows a similar pattern but lags behind the patterns for other species: estimated bycatch precipitously declines after a peak in 1989 with a temporary increase occurring in the early-2000s (**Fig. 3**). This pattern is driven by a steep increase in Brandt's cormorant bycatch rates in the Channel Islands region in 1990 that is assumed to have persisted to today (**Fig. S9**). None of the other species experienced such a pronounced and impactful change in bycatch rates (**Fig. S9**). Estimated bycatch of harbor porpoise~~harbor porpoise, northern elephant seal, and common murre declined especially sharply following the 2002 exclusion of fishing from waters shallower than 60 fathoms. ~~This regulation effectively closed the fishery in Monterey Bay and Morro Bay and restricted it to only Southern California (~~Slight differences between our estimates of annual bycatch and those from historical studies (**Fig. S11**) are driven by a mixture of differences in our methods and input data (**Fig. S12**~~**Fig. 1A**), where harbor porpoise do not occur and where common murre occur only in winter in low densities (**Fig. S15**~~).

The sustainability of recent estimated marine mammal bycatch ~~can be weighed against their~~ was evaluated as a percentage of the potential biological removal (PBR) ~~for~~of each stock~~, which is defined under the MMPA as the maximum number of animals, not including natural mortalities, that may be removed from a marine mammal stock while allowing that stock to reach or maintain its maximum sustainable population (**Fig. 7**).~~. Based on this sustainability reference point, bycatch concerns, in order of decreasing threat, are as follows: harbor seal (65 individuals per year = 4.0% of a PBR of 1,641 individuals), harbor porpoise (1 individual per year = 1.5% of a PBR of 65 individuals in the Morro Bay stock, where the bycatch took place), California sea lion (194 individuals per year = 1.4% of a PBR of 14,011 individuals), and northern elephant seal (7 individuals per year = 0.1% of a PBR of 5,122 individuals) (**Fig. 7**). The assessment that bycatch during the last 10 years poses the greatest risk to harbor seals is supported by the fact that the harbor seal stock size has been stable or declining in recent years while all of the other marine mammal populations have been undergoing sustained population growth (**Fig. 7**). The sustainability of estimated seabird bycatch is more difficult to evaluate given more limited population monitoring data (**Fig. 7**) and the lack of legally binding reference points for defining allowed incidental take. However, increasing Brandt's cormorant nests from 1980 to 2020 (**Fig. 7**) and steeply reduced bycatch of common murre (**Fig. 3**) suggests low risks posed to these species.

~~Our estimates of annual bycatch are generally aligned with estimates from historical studies (**Fig. S11**). Slight differences between our estimates and those from historical studies are driven~~

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~~by a mixture of differences in our methods and input data. While we apply a consistent approach for defining the fishery, stratifying the data, and estimating bycatch, historical studies have employed variable fishery definitions, stratification schemes, and estimation methodologies (**Table 1**). First, we consistently defined the fishery as using ≥3.5" set gillnets, while historical studies have considered set gillnets ≥8", ≥8.5", or of unspecified sizes (**Table 1**). Furthermore, it is unclear whether historical studies filtered out fishing sets based on the reported target species and whether this decision was consistent. We do not define the fishery based on reported target species given extreme heterogeneity in how this information is reported. Differences in fishery definitions, as well as differences in data cleaning methods, likely lead to the slight differences in effort and observed bycatch attributed to the fishery (**Fig. S12**), which inevitably causes differences in bycatch estimates. Second, we used a consistent six-region stratification scheme, while historical studies used a mixture of stratification schemes ranging from no stratification to spatial stratification to spatial-temporal stratification (**Table 1**). This impacts the ratio estimators and the magnitudes of bycatch. Finally, we used vessel days (trips) as the sample unit, whereas historical studies oscillated between trips and sets as the preferred sample unit (**Table 1**). This is problematic not only because of the lack of consistency but also because sets cannot be uniquely identified in the logbook data.~~

3.2 Random forest modeling

3.2.1 Model performance

The best~~-~~-fitting model performed well for ~~four of the six study~~all species~~: common murre, harbor seal, California sea lion, and northern elephant seal. All four species utilized weighted random forests but with different case weights (**Table 2; Fig.**~~, except~~S13). The models~~ for ~~two species, Brandt's~~Brandt's cormorant and harbor porpoise, which exhibited poor performance (Cohen's kappa less than 0.2) and were therefore excluded from further consideration~~ because of their poor performance (**Table 2**).~~, Weighted random forest models performed best for California sea lion, common murre, northern elephant seal, and harbor seal with case weights of 25, 25, 25, and 75, respectively (**Table 1; Fig. S13**). ~~2). This highlights the importance of evaluating multiple modeling approaches when predicting rare bycatch events.~~ For common murre, Cohen's kappa was 0.71, indicating "good" performance, while for harbor seal, California sea lion, and northern elephant seal, Cohen's kappa was 0.25, 0.24, and 0.23, respectively, indicating "fair" performance. Model performance was ~~strongly~~positively correlated with the frequency of bycatch observations~~ (**Table 2**). The best fitting model for common murre, the most common bycatch species, exhibited a~~, i.e., species with more observed bycatch events produced models with greater skill (**Table 1**; r^2 = 0.64 for Cohen's kappa ~~value greater than 0.4 indicating "good" performance while the best~~

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~~models for harbor seal and California sea lion, somewhat common bycatch species, exhibited values greater than 0.2 indicating "fair" performance. The best fitting models~~ for ~~Brandt's cormorant and harbor porpoise, rare bycatch species, exhibited~~training data). Cohen's kappa ~~values less than 0.2 on the test dataset, indicating "poor" performance, and were not evaluated any further. Although northern elephant seal bycatch is also rare, its best fitting model exhibited "fair" performance, and it was evaluated further. Cohen's kappa values were~~was positively correlated with the area under the receiver operator curve (AUC) (r^2 =0.68 for the training dataset), indicating minimal tradeoffs in using this metric for model selection (**Table** ~~2).~~ **1**).

3.2.2 Drivers of bycatch risk

The importance of the evaluated explanatory variables in determining bycatch risk varied by species but some general patterns emerged **(Fig. 4**). In general, spatial (latitude, longitude, depth, and distance from shore) and temporal (Julian day) variables were more influential than variables associated with the environment (sea surface temperature) or the fishing methodology (soak time, mesh size). Whether fishing occurred close to an island (a spatial variable) was the exception, as it was consistently the least important variable. Sea surface temperature, which is closely related to space and time, was generally more important than soak time and always more important than mesh size. ~~These results suggest that spatial-temporal management may have better ability to manage bycatch risk than gear modifications or soak time regulations, though shorter soak times could reduce discard mortality for non-target fish species.~~

The species exhibit a mixture of ~~correlated~~similar and ~~non-correlated~~dissimilar responses to the explanatory variables (**Fig. 5**). California sea lion and harbor seal exhibit similar responses in bycatch risk. Both species have higher bycatch risk in shallower depths in nearshore areas with a spike in risk occurring around 34°N latitude~~.~~, including some deeper offshore areas (**Fig. 6**). They also exhibited a pronounced increase in risk during the spring, lasting approximately from Apr 1 (90th day of the year) to June 15 (166th day of the year). They are infrequently caught in nets with mesh sizes smaller than 8.5 inches, though the use of such nets is rare (**Fig. S5E**). Variability in bycatch risk for common murre and northern elephant seal is most strongly determined by latitude and longitude (**Fig. 5**), with the only area of elevated risk in contemporary fishing grounds occurring just north of Point Conception (**Fig. 6**). For all four species, bycatch risk exhibits an asymptotic relationship with soak time ~~and a dome-shaped relationship with temperature; however,~~, though the shapes of these relationships differ by species. The species exhibit complex and variable relationships to temperature.

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3.2.3 Maps of bycatch risk

The species exhibited different patterns of spatial bycatch risk. California sea lion bycatch risk is predicted to be highest in four areas: (1) on the northern coasts of the northern Channel Islands, especially on the northern coast of Santa Rosa; (2) a small nearshore area west of Santa Barbara; (3) the eastern coast of Santa Cruz Island; and (4) the northwestern shores of Santa Catalina and San Clemente Islands; (**Fig.** ~~**6**). The high risk around the northern Channel Islands is likely related to the large haulouts of sea lions in that area (**Fig. S15**).~~

6). Harbor seal bycatch risk is predicted to be highest in four areas: (1) the sliver of nearshore area stretching from Santa Barbara to Point Sal; (2) the eastern coasts of Santa Cruz Island; (3) a broad coastal area near Point Mugu; and (4) the sliver of nearshore area stretching from Point Mugu to Point Vicente (**Fig.** ~~**6**). Most of these areas correspond to the locations of large harbor seal haulouts on Santa Cruz Island and near Point Mugu (**Fig. S15**).~~

6). Common murre bycatch risk is predicted to be negligible throughout most of southern California (**Fig. 6**). It is only predicted to be high in a small patch near Point Sal and even there, the maximum risk index is much lower than for the other evaluated species. ~~This is consistent with the distribution of the species, which has no breeding colonies or permanent foraging grounds in southern California (**Fig. S15**). The absence of common murre in southern California largely explains the significant drop in common murre bycatch (**Fig. 3**) since the fishery was pushed out of central California (**Fig. 1**).~~

Like common murre, northern elephant seal bycatch risk is also predicted to be negligible throughout most of southern California except in the region near Point Sal (**Fig.6**).

~~Although northern elephant seals breed on the Channel Islands and near San Simeon/Cambria (**Fig. S15**) from December to March, they disperse to their distant foraging grounds (males to Alaska and females to oceanic waters far West of California) before the fishing season peaks from April to June, significantly reducing their vulnerability to the gillnet fishery.~~

~~3.2.4 Temporal trends in estimated bycatch~~

~~The random forest models estimate trends in bycatch that are similar to the estimates from the ratio estimator from 2000-2022 (**Fig. 3**). The agreement between these two different approaches underscores the result that bycatch has decreased to low levels as a result of management and reduced fishing effort. However, the estimates produced by the two approaches diverge from 1981-2000 by~~

~~various extents. While they generally agree for California sea lion back to 2000, the random forest model underpredicts bycatch relative to the ratio estimator in the late 1990s and overpredicts in the 1980s and early 1990s (**Fig. 3**). While the approaches generally agree for harbor seal back to 1995, the random forest model underpredicts bycatch relative to the ratio estimator before 1995, especially in the Channel Islands and Ventura strata (**Fig. S16**). For common murre, the random forest model overpredicts bycatch relative to the ratio estimator in the mid- to late-1990s and underpredicts relative to the ratio estimator in earlier years, especially in Morro and Monterey Bays. These underpredictions likely occur because of the unequal impacts of lost data from the northern strata in the 1980s (**Fig. 2**). While the ratio estimation method leverages summarized observer data from old reports, meaning that it sees data from this time period, the lack of the raw observer data from this time period means that the random forest model does learn from this period. As a result, it is likely to underpredict risk in early years in northern strata because it has largely learned from late years in southern California, where risk has been lower. For this reason, we recommend the use of bycatch estimates from the ratio estimator over the random forest model until a time when the 1980s observer data is rediscovered.~~

4. Discussion

Our study provides the first update to total estimates of protected species bycatch in the California set gillnet fishery since 2012~~ (Carretta et al., 2014).~~. We find that bycatch, once high and unsustainable for some species ~~(Forney et al., 2021, 2001)~~(Forney et al., 2021, 2001), is now well below the “zero mortality rate goal” (ZMRG) of 10% of the potential biological removal (50 C.F.R. § 229.2). Recent marine mammal bycatch estimates range from 0.1-4.0% of their potential biological removals and common murre bycatch has been effectively eliminated. All of the evaluated populations, including the once declining and heavily depleted Morro Bay harbor porpoise population, are growing or stable. These advances, while directly attributable to management interventions, are more due to reductions in fishing effort (i.e., fewer fishing trips) than to reductions in bycatch rates (i.e., lower bycatch per ~~unit effort~~fishing trip). This highlights a steep tradeoff between conservation and fisheries objectives under the current management regime: while populations of protected species have undergone sustained growth, fishing opportunities and revenues have undergone ~~sustained~~prolonged declines. Despite this, there have been calls for more bycatch-motivated restrictions to the fishery ~~(Birch et al., 2023; Birch and Shester, 2023)~~(Birch et al., 2023; Birch and Shester, 2023). Our results indicate that current fishing operations do not pose a threat to the evaluated species, which suggests that current management is sufficient at limiting bycatch. However, targeting management toward spatial-temporal bycatch hotspots could improve economic outcomes while keeping bycatch low.

Our results suggest that spatial-temporal management could more efficiently and effectively manage bycatch risk than gear modifications or soak time regulations. Specifically, bycatch rates for California sea lions and harbor seals are greatest from April 1 to June 15, suggesting that a 2.5 month seasonal closure of bycatch ~~spots for these two species could reduce bycatch risk. Spatially, our findings indicate that the existing state water closure (0-3 nautical miles offshore) in Southern California could be adjusted to target specific bycatch hotspots around the Channel Islands, where both California sea lions and harbor seals face higher bycatch risk compared to nearshore areas (except for the nearshore strip from Santa Barbara to Point Sal). This adjustment could potentially open up more fishing grounds in the nearshore region.~~hotspots for these two species could prevent bycatch while allowing the opening of less risky but currently closed areas to fishing. These hotspots are predicted to disproportionately contribute to bycatch yet are of minor fishing importance (**Fig. 6B**) suggesting that the loss of fishing opportunities in these areas during brief seasonal closures could be easily made up by opening areas of low predicted bycatch risk. As a result, seasonal closures could broaden fishing opportunities while continuing to meet bycatch avoidance objectives. However, we caution that such hotspot closures could exacerbate bycatch problems if fishing effort is displaced and concentrated in areas of secondarily high risk ~~(Free et al., 2023)~~(Free et al., 2023). Therefore, monitoring of fishing effort and bycatch rates are important for verifying that seasonal closures achieve their conservation and fisheries objectives. Additionally, any changes in current management strategies must take into account spatial-temporal patterns of bycatch and relative sensitivity of each species.

Hotspots of bycatch risk are aligned with the location of known haulouts, breeding colonies, and foraging grounds. High California seal lion bycatch risk around the northern Channel Islands is likely related to the large haulouts of sea lions in that area (**Fig. S15**). Similarly, hotspots of harbor seal bycatch risk correspond to the locations of large harbor seal haulouts on Santa Cruz Island and near Point Mugu (**Fig. S15**). The absence of common murre bycatch risk in southern California is consistent with the distribution of the species, which has no breeding colonies or permanent foraging grounds in southern California (**Fig. S15**). Similarly, the negligible risk for northern elephant seals is consistent with the phenology of their migrations. Although northern elephant seals breed on the Channel Islands and near San Simeon/Cambria (**Fig. S15**) from December to March, they disperse to their distant foraging grounds (males to Alaska and females to oceanic waters far West of California) before the fishing season peaks from April to June, significantly reducing their vulnerability to the gillnet fishery. Finally, the elimination of harbor porpoise bycatch is consistent with the confinement of the fishery to southern California where harbor porpoise do not occur (**Fig. S15**).

The completion and safekeeping of accurate fisheries-dependent data is fundamental to producing accurate bycatch assessments and effective management strategies. While summaries of historical data facilitated reliable bycatch estimates through ratio estimation, the loss of raw observer data from the 1980s likely impeded our ability to accurately estimate bycatch in the northern strata using random forests, an approach often thought to be more accurate than ratio estimation ~~(Stock et al., 2019)~~(Stock et al., 2019). Notably, the lost data document a period when fishing was allowed in shallower, more inshore, and more northern waters (**Fig. 1A**). Recovering this data would enhance our ability to assess the drivers of bycatch in the northern region and could provide insights for re-evaluating previous management strategies. Furthermore, missing meta-data on critical gear characteristics (e.g., mesh size, net length, net height, net material; **Fig. S3**) in the available observer data also limited our ability to identify the potential for these management levers to reduce bycatch risk. Ensuring the complete documentation of gear characteristics, perhaps by prioritizing characteristics known to impact bycatch risk in other gillnet fisheries ~~(Northridge et al., 2017)~~(Northridge et al., 2017), is important to maximizing the utility of expensive, and sometimes controversial, observer programs ~~(Suuronen and Gilman, 2020).~~(Suuronen and Gilman, 2020). Finally, the ability to delineate individual sets in the logbooks and improved documentation of the characteristics of logged sets would enhance future bycatch estimates by allowing sets to be the sampling unit and by avoiding assumptions about missing data, respectively. This could be achieved by redesigning logbooks, training fishers on completing logbooks, expanding electronic monitoring, and/or demonstrating that better data can actually lead to fewer restrictions.

Our results highlight the importance of considering multiple modeling approaches when estimating and evaluating rare bycatch events. Although model-based methods (e.g., random forests) for estimating bycatch are often preferred to sample-based methods (e.g., ratio estimators) ~~(Stock et al., 2019)~~(Stock et al., 2019), we find complementary value in using both approaches. While ratio estimation generated more reliable bycatch estimates due to its ability to leverage both raw and summarized data, the random forest model provided the empirical basis for assessing drivers of bycatch risk. ~~Additionally, agreement between the two approaches over the past two decades reinforces predictions that recent bycatch levels have been low for the evaluated species.~~ Furthermore, our results highlight the value of considering multiple sample balancing approaches when evaluating bycatch using model-based methods, as the specification of the best performing model varied by species. Recent efforts to estimate bycatch in West Coast fisheries using random forests have used only a single sample balancing technique (e.g., Carretta, 2023); we encourage future efforts to compete multiple approaches to optimize estimates of rare bycatch events ~~(More and Rana, 2017). Finally, we highlight our derivation of model-specific probability~~

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~~thresholds for classifying logged fishing efforts as having or not having bycatch using Cohen's kappa as being aligned with recommended best practices for classifying rare events (Freeman and Moisen, 2008; Manel et al., 2001). Specifically, research shows that classification based on: (1) the default 0.5 probability threshold underestimates rare events, (2) true skill statistics overestimates rare events; and (3) Cohen's kappa provides the least biased predictions. This best practice should also be considered in future bycatch estimation efforts.~~(More and Rana, 2017).

The continued recovery of protected species will require management of stressors besides fisheries bycatch, some of which may present even larger threats ~~(Avila et al., 2018; Oldach et al., 2022).~~(Avila et al., 2018; Oldach et al., 2022). For example, in Summer 2023, there were over 1,000 statewide strandings of California sea lions and other pinnipeds attributed to domoic acid toxicosis resulting from an intense bloom of harmful diatoms in the *Pseudo-nitzschia* genus ~~(SCCOOS, 2023; Smith et al., 2023).~~(SCCOOS, 2023; Smith et al., 2023). Harmful algal blooms are increasing in frequency, duration, and intensity on the West Coast ~~(Hallegraeff et al., 2021)~~(Hallegraeff et al., 2021) as a result of ocean warming and eutrophication ~~(McKibben et al., 2017)~~(McKibben et al., 2017) suggesting that, in the long-term, curbing climate change and nutrient runoff may be the most important actions for stemming mortality for recovering pinniped populations. Furthermore, harassment and shooting are some of the most commonly observed sources of mortality and serious injury for California sea lions, harbor seals, and northern elephant seals ~~(Carretta, 2023) (**Fig. S17**~~(Carretta, 2023) (**Fig. S16**), suggesting the need for greater outreach and enforcement to prevent these gratuitous forms of mortality. Finally, it is important to understand the bycatch contributions of other fisheries, many of which report higher levels of *observed* bycatch (**Fig. ~~S17~~S16**) yet are not as heavily prosecuted as the California set gillnet fishery. Modeling studies similar to this one are needed to determine whether higher apparent bycatch in these fisheries is due to higher observer coverage or higher bycatch rates. However, the sustained recovery of the evaluated populations suggests that total bycatch across all fisheries is low.

There will always be tradeoffs between maximizing fishing opportunities and minimizing bycatch of protected species ~~(Samhouri et al., 2021).~~(Samhouri et al., 2021). As a result, managers often seek to implement regulations that maximize fishing outcomes while keeping bycatch below legally defined sustainable reference points ~~(Kirby and Ward, 2014).~~(Kirby and Ward, 2014). The identification of such strategies is seldom straightforward and depends on substantial investments in data and scientific enterprises. Notably, they depend on monitoring populations of protected species to support the assessment of their status and levels of allowable incidental take and monitoring bycatch in key fisheries to support assessments of total bycatch, drivers of bycatch, and the effectiveness of past management interventions ~~(Kirby and Ward, 2014; Punt et al., 2021).~~(Kirby and Ward, 2014; Punt et al., 2021). In the absence of such data, management must often be precautionary to ensure compliance with protected species legislation ~~(Punt et al., 2021)~~(Punt et al., 2021). We illustrate the potential return on investment of supporting such scientific enterprises as our results show that past management interventions have been successful at reducing bycatch in the California set gillnet fishery well below target levels, opening the door for more efficient restrictions and negating the need for unnecessary precaution. The continued demonstration that monitoring programs can generate better outcomes for businesses could facilitate increased public support and funding to identify win-win scenarios for fisheries and conservation in more fisheries.

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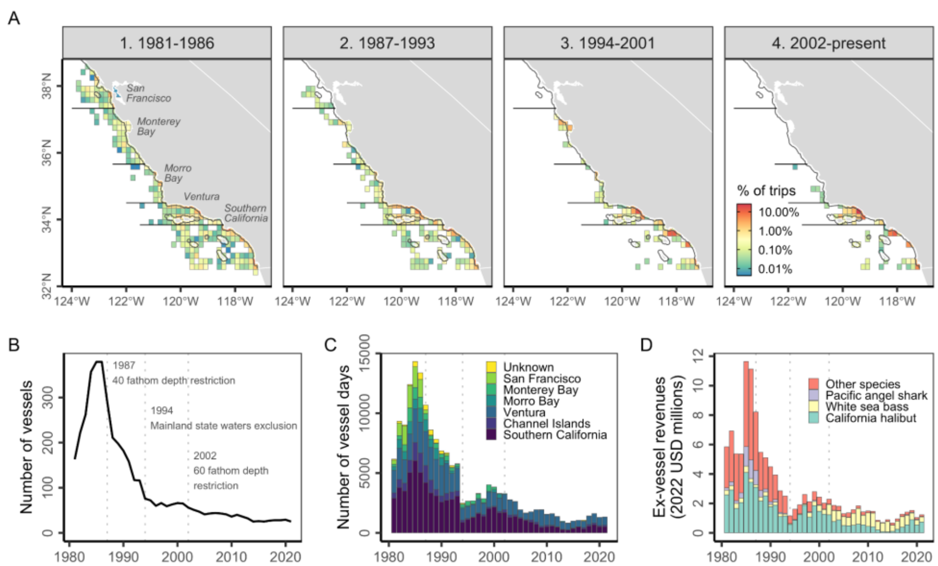
Tables & Figures



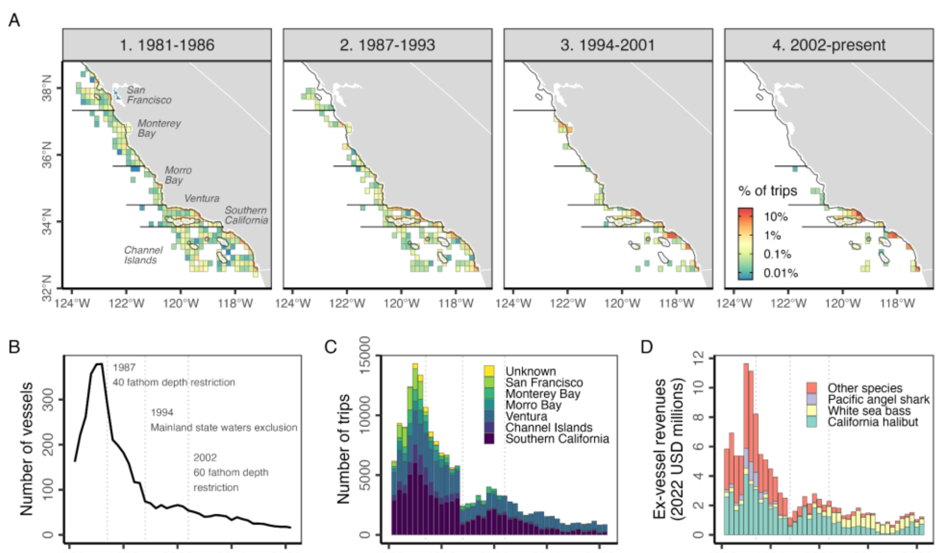
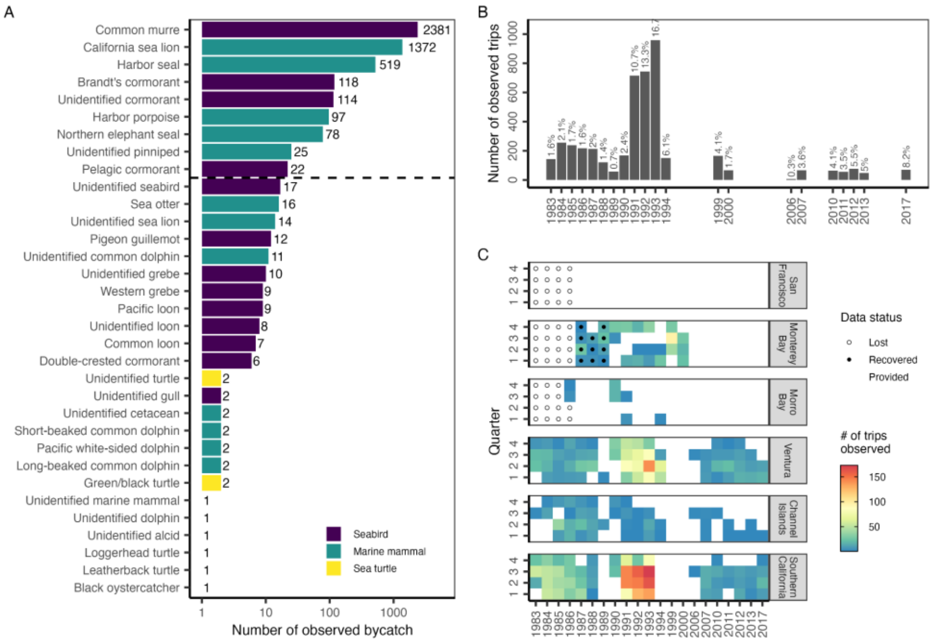


Fig. 1. History of the California ≥3.5” set gillnet fishery. Panel **A** shows the spatial history of fishing effort during four regulatory periods. Trips are reported by the 10 x 10 minute (~18 x 18 km) statistical blocks used for fisheries catch and effort reporting. The horizontal lines delineate geographical strata used in the ratio estimation analysis; strata are labeled in the first plot. The thin coastal line marks state waters (less than 3 nautical miles from the coast). Blocks visited by fewer than three vessels during each regulatory period are hidden to maintain confidentiality and comply with the “rule-of-three.” The other panels show time series of fisheries **(B)** participation, **(C)** effort, and **(D)** revenues. Vertical lines mark years in which major regulations, labeled in Panel B, were implemented; these define the regulatory periods used in Panel **A**. These regulations became operative on April 15, 1987; January 1, 1994; and April 26, 2002. See **Fig. S1** and the supplemental methods for details on estimating ex-vessel revenues from the fishery.


A
Common murre
California sea lion
Harbor seal
Brandt's cormorant
Unidentified cormorant
Harbor porpoise
Northern elephant seal
Unidentified pinniped
Pelagic cormorant
Unidentified seabird
Sea otter
Unidentified sea lion
Pigeon guillemot
Unidentified common dolphin
Unidentified grebe
Western grebe
Pacific loon
Unidentified loon
Common loon
Double-crested cormorant
Unidentified turtle
Unidentified gull
Unidentified cetacean
Short-beaked common dolphin
Pacific white-sided dolphin
Long-beaked common dolphin
Green/black turtle
Unidentified marine mammal
Unidentified dolphin
Unidentified alcid
Loggerhead turtle
Leatherback turtle
Black oystercatcher
2381
1372
519
118
114
97
78
25
22
17
16
14
12
11
10
9
9
8
7
6
2
2
2
2
2
2
2
1
1
1
1
1
1
Seabird
Marine mammal
Sea turtle
1
10
100
1000
Number of observed bycatch
B
Number of observed trips
0
200
400
600
800
1000
1.6%
2.1%
1.7%
1.6%
2%
1.4%
0.7%
2.4%
10.7%
13.3%
16.7
6.1%
4.1%
1.7%
0.3%
3.6%
4.1%
3.5%
5.5%
5%
8.2%
1983
1984
1985
1986
1987
1988
1989
1990
1991
1992
1993
1994
1999
2000
2006
2007
2010
2011
2012
2013
2017
C
Quarter
San Francisco
Monterey Bay
Morro Bay
Ventura
Channel Islands
Southern California
Data status
Lost
Recovered
Provided
# of trips observed
150
100
50

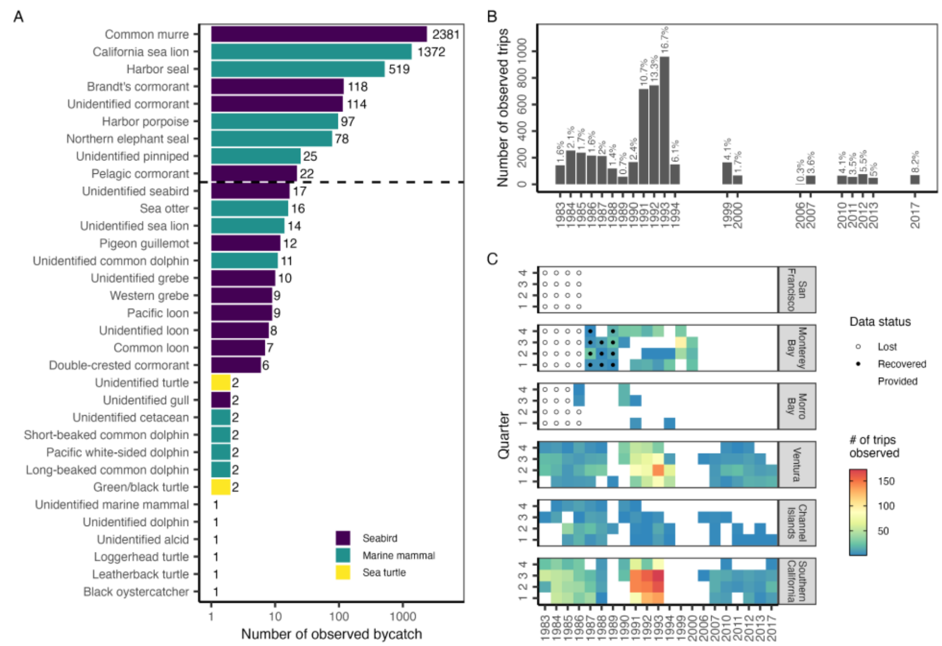
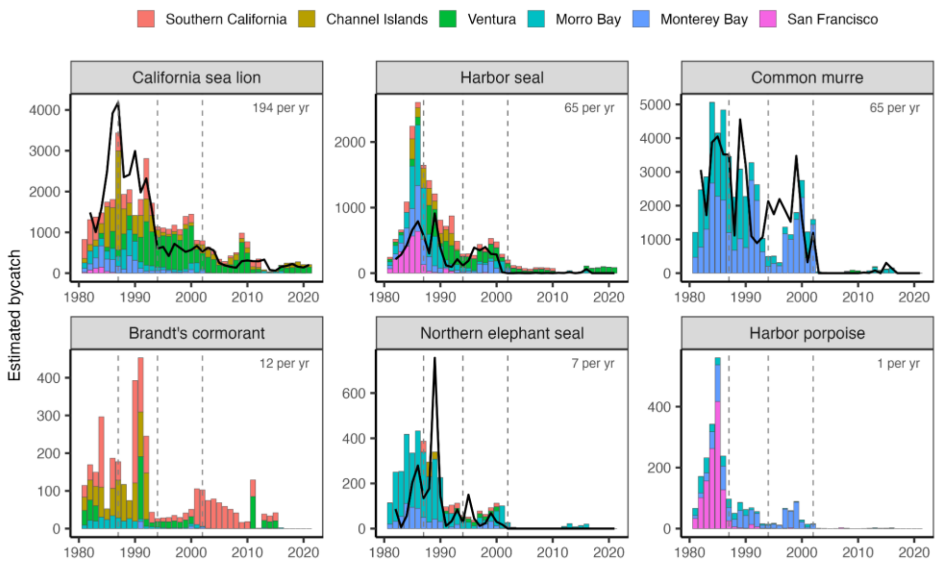


Fig. 2. History of observer coverage in the California ≥3.5” set gillnet fishery. Panel **A** shows the bycatch of marine mammals, seabirds, and sea turtles recorded by observers from 1983-2017. We focus on species with ≥50 observations, which are delineated by the horizontal dashed line. Note log-scale on x-axis. Panel **B** shows the number of observed trips (vessel-days) over time. The dark labels show the estimated percent of trips that were observed. Panel **C** shows the number of observed trips across the spatial (region) and temporal (quarters) strata considered in the ratio estimation analysis. See **Fig. S8** for a map of the spatial strata. Quarters are defined as: 1 = JFM (winter), 2 = AMJ (spring), 3 = JAS (summer), and 4 = OND (fall).



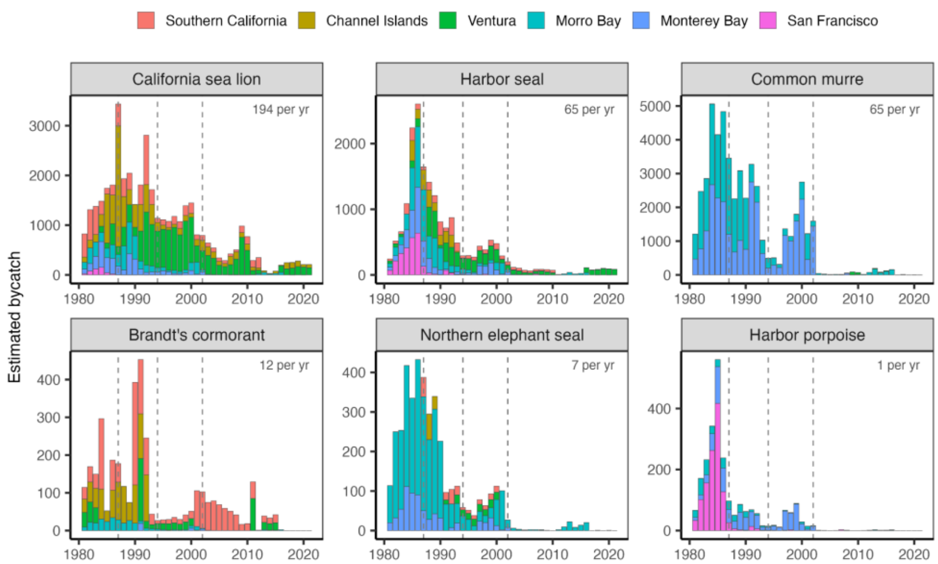
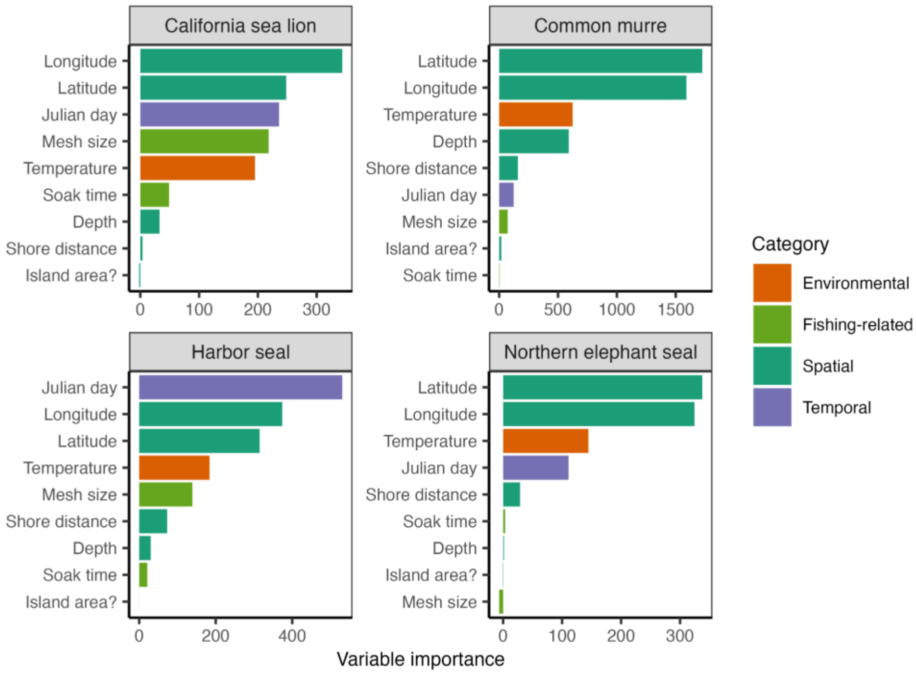


Fig. 3. Estimated bycatch in the California ≥3.5" set gillnet fishery from 1981-2021 predicted ~~by~~using the ratio estimation ~~(bars) and random forest (line) modeling approaches~~approach. Average estimated annual

bycatch rates for the last 10 years (2012-2021) from the ratio estimator are marked in the top-right corner. Species are listed in order of decreasing recent bycatch rates.



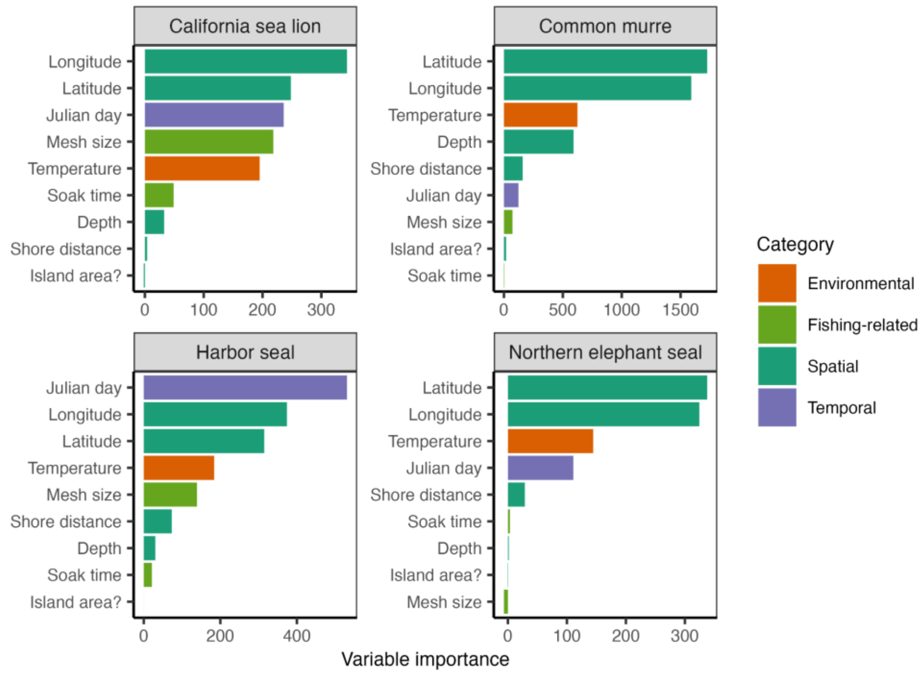
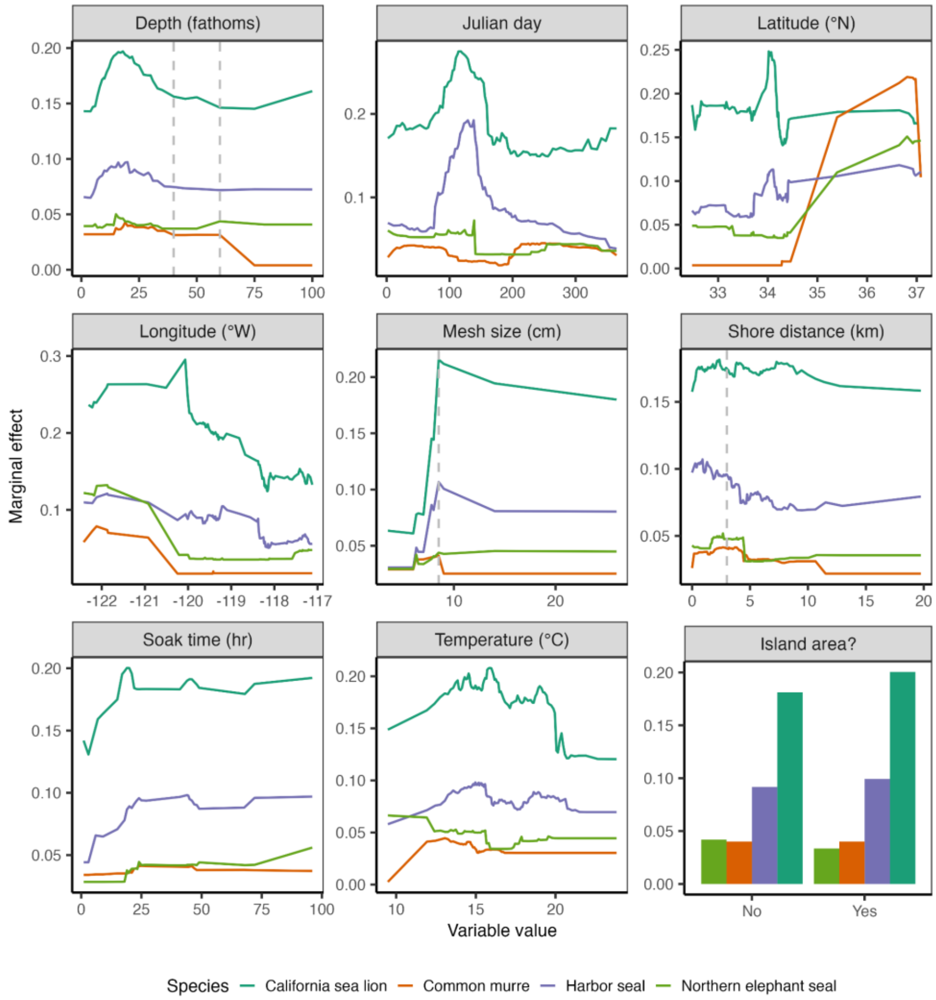


Fig. 4. Importance of the evaluated explanatory variables in the final random forest model for the study species with acceptable model performance. With the bias towards continuous variable corrected, variable importance is measured as the total decrease in node impurities from splitting on the variable averaged over all trees. Explanatory variables are colored based on the category of the variable.


Depth (fathoms)
Julian day
Latitude (°N)
Longitude (°W)
Mesh size (cm)
Shore distance (km)
Soak time (hr)
Temperature (°C)
Island area?
Marginal effect
Variable value
No
Yes
Species
California sea lion
Common murre
Harbor seal
Northern elephant seal

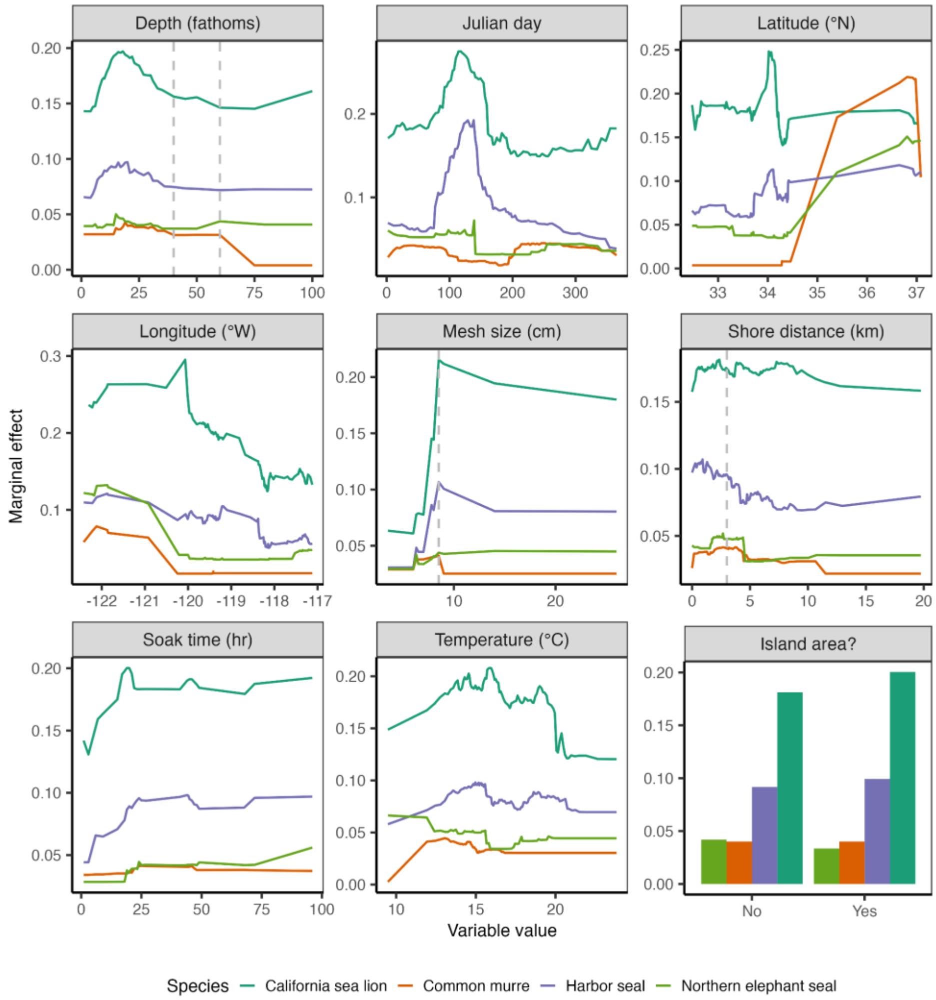


Fig. 5. Marginal effect of the evaluated explanatory variables on bycatch risk as estimated by the best fitting random forest model for the four study species with acceptable model performance. The marginal effect of each variable represents the effect of the variable when the other variables are held at their mean values. The dashed lines indicate 40 and 60 in Depth (fathoms), 8.5 in mesh size (cm), and 3 in shore distance (km).

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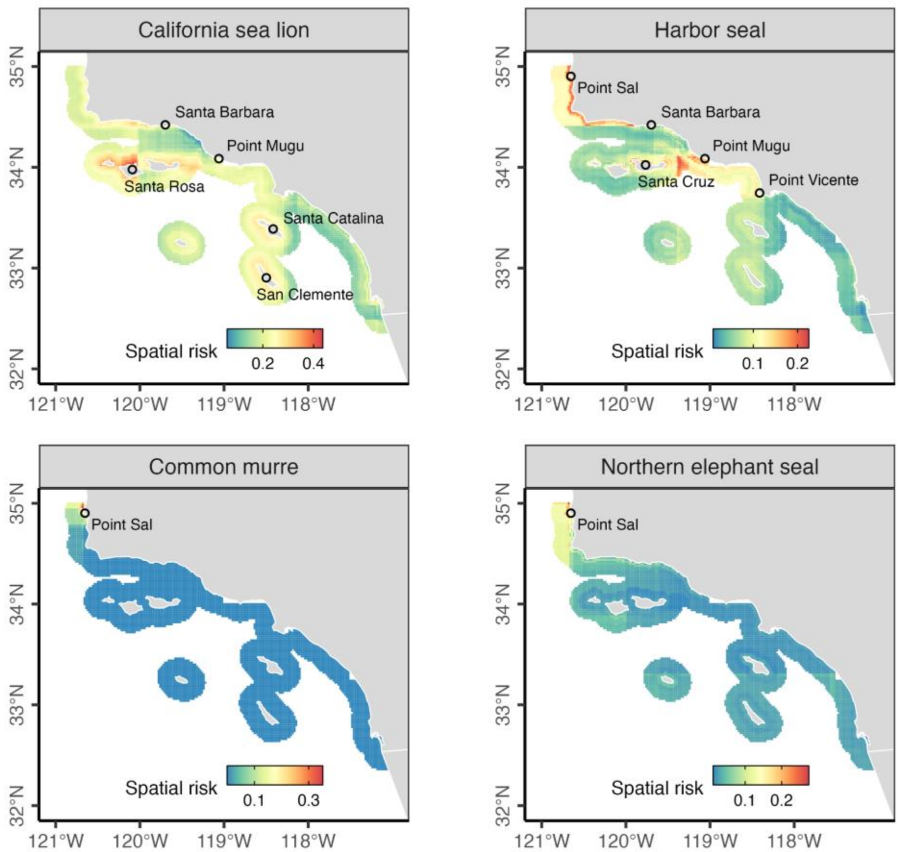

California sea lion
Santa Barbara
Point Mugu
Santa Rosa
Santa Catalina
San Clemente
Spatial risk
0.2
0.4
Harbor seal
Point Sal
Santa Barbara
Point Mugu
Santa Cruz
Point Vicente
Spatial risk
0.1
0.2
Common murre
Point Sal
Spatial risk
0.1
0.3
Northern elephant seal
Point Sal
Spatial risk
0.1
0.2
35°N
34°N
33°N
32°N
121°W
120°W
119°W
118°W

~~**Fig. 6.** Average~~

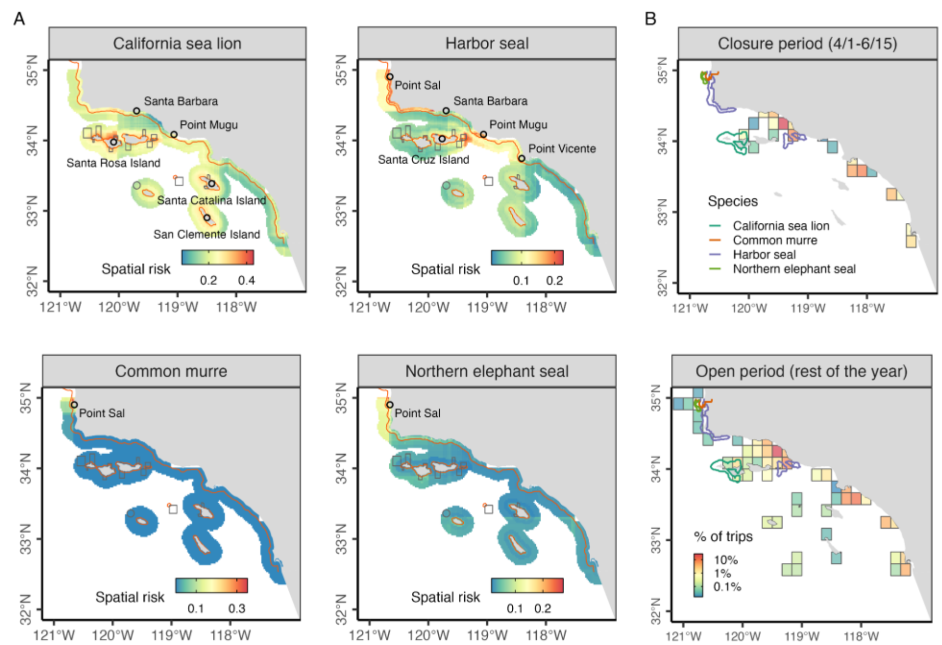
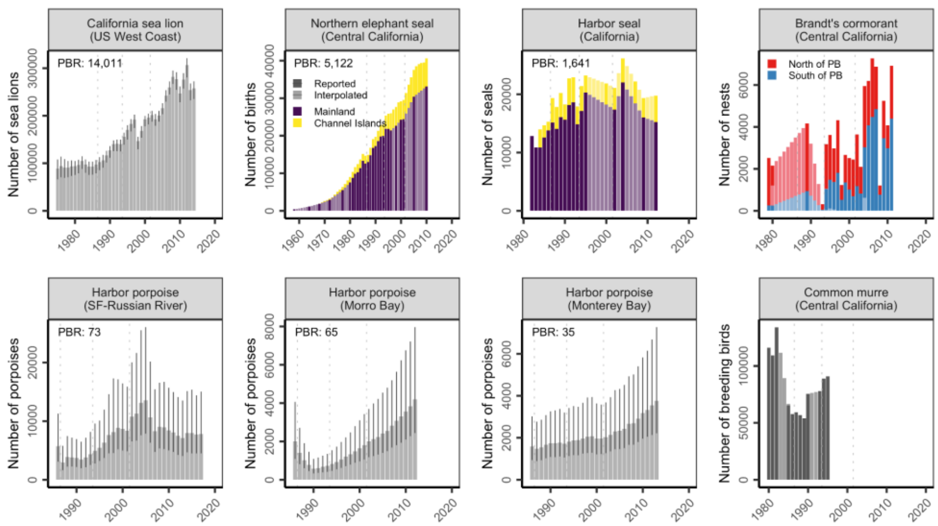


Fig. 6. The **(A)** spatial bycatch risk relative to current management areas and **(B)** spatial bycatch hotspots relative to recent fishing effort. Panel **A** shows the average spatial bycatch risk as estimated by the best fitting random forest model for the four study species with acceptable model performance. The spatial bycatch risk represents the probability of bycatch at a given location under recent (2010-2021) average conditions. Key landmarks for delineating bycatch hotspots are labeled in each panel. The thin orange coastal line marks the nearshore areas from which gillnet fishing is excluded: within 3 nautical miles of the mainland and within 1 nautical mile or shallower than 70 fathoms (whichever is closer to shore) from the Channel Islands. The gray polygons indicate the locations of California Marine Protected Areas, where all set gillnet fisheries are excluded. Spatial bycatch risk is shown only for southern California, as this is the only area where the fishery can operate under current regulations. Panel **B** shows hotspots of bycatch risk relative to recent fishing effort (2002-2022; see **Fig 1A**) during the proposed closed and open periods.



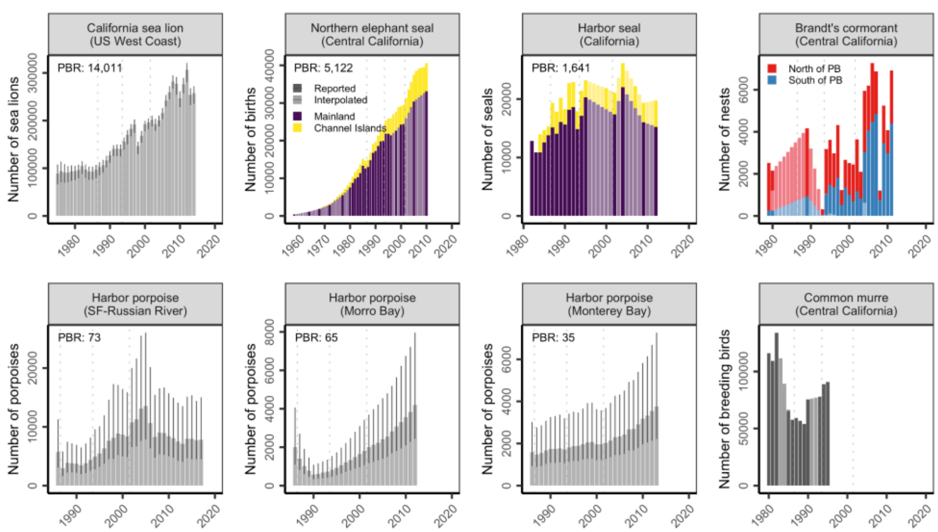


Fig. 7. Estimated abundance of populations of the six study species. The potential biological removal (PBR) indicates the maximum number of animals, not including natural mortalities, that may be removed from a marine mammal stock while allowing the stock to reach or maintain its optimum sustainable population. PBRs are only calculated for marine mammals. Error bars indicate 95% confidence or credible intervals. Years without reported values were filled using linear interpolation. Vertical lines mark

years in which major bycatch regulations were implemented. Population estimates are from the following sources: California sea lion ~~(Laake et al., 2018)~~(Laake et al., 2018), northern elephant seal ~~(Carretta et al., 2022)~~(Carretta et al., 2022), harbor seal ~~(Carretta et al., 2022)~~(Carretta et al., 2022), harbor porpoise ~~(Forney et al., 2021)~~(Forney et al., 2021), Brandt's cormorant ~~(Capitolo et al., 2012)~~(Capitolo et al., 2012), and common murre ~~(Carter, 2001).~~(Carter, 2001). Data from Carretta et al. (2022) were graphically digitized.

Table 1. ~~Historical bycatch estimation studies and their characteristics (the period column indicates month and year as MM/YY).~~

~~Study~~	~~Seasons~~	~~Period~~	~~Species~~	~~Fishery definition~~	~~Stra~~	
~~(Barlow et al., 1994)~~	~~1983-87~~	~~(see papers)~~	~~Pinnipeds/cetaceans~~	~~(see included papers)~~	~~(see~~	
~~(Hanan et al., 1988)~~	~~1983-85~~	~~Apr 1-Mar 31~~	~~Sea lion, harbor seals~~	~~Set nets for halibut/flounder/sharks~~	~~5 reg~~	
~~(Diamond and Hanan, 1986)~~	~~1983~~	~~4/83 - 3/84~~	~~Harbor porpoise~~	~~≥8.0" set nets for halibut/flounder~~	~~3 regions~~	~~Sets~~
~~(Hanan et al., 1986)~~	~~1984~~	~~4/84 - 3/85~~	~~Harbor porpoise~~	~~Set nets for halibut/flounder/sharks~~	~~3 regions~~	~~Sets~~
~~(Hanan et al., 1987)~~	~~1985~~	~~4/85 - 3/86~~	~~Harbor porpoise~~	~~Set nets (but not for croaker)~~	~~3 regions~~	~~Sets~~
~~(Hanan and Diamond, 1989)~~	~~1986~~	~~4/86 - 3/87~~	~~Sea lion, harbor seal, harbor porpoise~~	~~Set nets for halibut/flounder/sharks~~	~~5 regions, seasons~~	~~Sets~~
~~(Konno, 1990)~~	~~1987~~	~~4/87 - 3/88~~	~~Sea lion, harbor seal, harbor porpoise~~	~~Set nets for halibut/flounder/sharks~~	~~5 regions, seasons~~	~~Sets~~
~~(Perkins et al., 1994)~~	~~1988-90~~	~~4/88 - 3/89~~ ~~4/89 - 12/89~~ ~~1/90 - 12/90~~	~~Pinnipeds/cetaceans~~	~~Set nets for halibut/angel shark~~	~~3 regions~~	~~Vessel-day~~
~~(Lennert et al., 1994)~~	~~1990~~	~~7/90 - 12/90~~	~~Marine mammals~~	~~Set nets for halibut/angel shark~~	~~3 regions~~	~~Vessel-day~~
~~(Perkins et al., 1992a)~~	~~1990~~	~~7/90 - 6/91~~	~~Pinnipeds/cetaceans~~	~~≥8.0" nets for halibut/angel shark~~	~~3 regions~~	~~Sets~~
~~(Perkins et al., 1992b)~~	~~1991~~	~~1/91 - 12/91~~	~~Pinnipeds/cetaceans~~	~~Set nets for halibut/angel shark~~	~~SCal/Ventura: Quarters~~ ~~Other strata: Annual~~	~~Vessel-day~~
~~(Julian, 1993)~~	~~1992~~	~~1/92 - 12/92~~	~~Pinnipeds/cetaceans~~	~~Set nets for halibut/angel shark~~	~~4 regions: quarterly~~	~~Vessel-day~~
~~(Julian, 1994)~~	~~1993~~	~~1/93 - 12/93~~	~~Pinnipeds/cetaceans~~	~~Set nets for halibut~~	~~4 regions~~	~~Vessel-day~~
~~(Julian and Beeson, 1998)~~	~~1990-95~~	~~7/90-12/90~~ ~~1-12, 91-95~~	~~Mammals/seabirds/turtles~~	~~≥8.5" nets for halibut/angel shark~~	~~SCal/Ventura: Quarters~~ ~~Other strata: Annual~~	~~Vessel-day~~
~~(Cameron and Forney, 1999)~~	~~1997-98~~	~~1/97 - 12/97~~ ~~1/98 - 12/98~~	~~Cetaceans~~	~~≥8.5" nets for halibut/angel shark~~	~~1997: Geographical+seasonal~~ ~~1998: Geographical only~~	~~Vessel-day~~
~~(Cameron and Forney, 2000)~~	~~1999~~	~~1/99 - 12/99~~	~~Cetaceans~~	~~≥8.5" nets for halibut/angel shark~~	~~Geographical+seasonal~~	~~Vessel-day~~
~~(Carretta, 2001)~~	~~2000~~	~~1/00 - 12/00~~	~~Cetaceans~~	~~≥8.5" nets for halibut/angel shark~~	~~SCal/Ventura: Quarters~~ ~~Other strata: Annual~~	~~Vessel-day~~
~~(Carretta, 2002)~~	~~2001~~	~~1/01 - 12/01~~	~~Cetaceans~~	~~Set nets for halibut/angel shark~~	~~SCal/Ventura: Quarters~~ ~~Other strata: Annual~~	~~Vessel-day~~
~~(Carretta and Chivers, 2003)~~	~~2002~~	~~1/02 - 12/02~~	~~Cetaceans~~	~~Set nets for halibut/angel shark~~	~~SCal/Ventura: Quarters~~ ~~Other regions: Annual~~	~~Vessel-day~~
~~(Carretta and Chivers, 2004)~~	~~2003~~	~~1/03 - 12/03~~	~~Marine mammals~~	~~≥8.5" nets for halibut/angel shark~~	~~SCal/Ventura: Quarters~~ ~~Other regions: Annual~~	~~Vessel-day~~
~~(Carretta and Enriquez, 2009)~~	~~2007~~	~~1/07 - 12/07~~	~~Mammals/seabirds~~	~~Set nets for halibut/white seabass~~	~~No stratification~~	~~Sets~~
~~(Carretta and Enriquez, 2012a)~~	~~2010~~	~~1/10 - 12/10~~	~~Mammals/seabirds~~	~~Set nets for halibut/white seabass~~	~~No stratification~~	~~Vessel-day~~
~~(Carretta and Enriquez, 2012b)~~	~~2011~~	~~1/11 - 12/11~~	~~Mammals/seabirds~~	~~Set nets for halibut/white seabass~~	~~No stratification~~	~~Sets~~
~~(Carretta et al., 2014)~~	~~2012~~	~~1/12 - 12/12~~	~~Mammals/seabirds/turtles~~	~~Set nets for halibut/white seabass~~	~~No stratification~~	~~Vessel-day~~

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Table ~~2~~1. Performance of the best fitting random forest model by species.

				Training data		**Test data**	
Species	**# of bycatch observed**	**Best model**	**Mtry**	**Kappa**	**AUC**	**Kappa**	**AUC**
California sea lion	1372	Weighted-25	3	0.24	0.78	0.23	0.78
Harbor seal	519	Weighted-75	2	0.25	0.83	0.15	0.81
Harbor porpoise	97	Weighted-50	2	0.34	0.98	-0.005	0.98
Common murre	2381	Weighted-25	6	0.71	0.99	0.61	0.97
Brandt’s cormorant	118	Weighted-25	8	0.06	0.68	0.07	0.63
Northern elephant seal	78	Weighted-25	1	0.23	0.87	0.21	0.86

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Supplemental ~~Methods~~Information

Imputing missing values in observed and logbooks data

We developed a series of simple assumptions to impute missing values for a few key variables (GPS coordinates, fishing depth, soak hour, mesh size) reported in the observer data (**Fig. S3A; Table S4**). We assigned missing GPS coordinates using the median coordinates for observed trips within the statistical block most frequently visited by the vessel – in order of preference – that week, month, or year based on the logbook data (described below). We derived missing fishing depths by extracting depths from 25-meter resolution bathymetry data (CDFW, 2002) (**Fig. S3B**). We reassigned missing soak hours the mode value for a vessel and target species (**Fig S3C**). We reassigned missing mesh sizes the mode for – in order of preference – the vessel and target species, the target species, or all vessels (**Fig. S3DE**). We assigned each GPS coordinate to the nearest statistical reporting block (see **Fig. 1A**), which allows points erroneously falling on land to be assigned a likely statistical block. We derived the distance from shore, a covariate used to explain bycatch rates in the random forest model, as the distance of each set to the nearest point on shore.

We developed a series of simple assumptions to impute missing or unrealistic values for a few key variables (fishing depth, soak hour, mesh size) reported in the logbook data (**Fig. S5A; Table S4**). We reassigned both missing (including 0 values) and unrealistic fishing depths, which we defined as depths exceeding the maximum depth in the reported fishing block, the median depth of the fishing block (**Fig. S5B**). We computed the median and maximum depths of each fishing block using 25-meter resolution bathymetry data (CDFW, 2002). We reassigned missing soak hours (including 0 values) the mode value for a vessel. We capped rare and unlikely soak times exceeding 96 hours (4 days) at 96 hours; however, such soak times could theoretically occur during rough weather when it is unsafe to haul gear (**Fig. S5C**). We reassigned missing (including 0 values) and unrealistic mesh sizes, which we defined as mesh sizes exceeding 20 inches, using a hierarchical procedure (**Fig. S5DE**). For logbooks with both vessel identification and target species information, we assigned the mesh size most commonly used by the vessel when targeting that target species. For logbooks with only target species information (no vessel identification), we assigned the mesh size most commonly used when targeting that target species across all vessels (**Figs. S5 & S6**). We derived the distance from shore, a covariate used to explain bycatch rates in the random forest model, as the median distance from shore of observed trips within the reported block given that exact locations are not reported.

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Bycatch estimation using the random forest models

We used the best fitting model to generate annual estimates of protected species bycatch from 1981 to 2022 by predicting whether "pseudo-sets" recorded in logbooks were likely to have captured each study species and assuming median numbers of sets and caught animals for "pseudo-sets" with bycatch. We predict to pseudo-sets rather than trips because the random forest model is trained on set-level covariates in the observer data. We used the best fitting model for each species to estimate the probability that a logged pseudo-set included bycatch of a species then categorized the pseudo-set as with or without bycatch using a species-specific probability threshold. We derived the species-specific probability thresholds as the threshold that maximizes Cohen's kappa when applied to the training datasets (**Fig. S17**). We selected the probability threshold based on Cohen's kappa rather than the area under the receiver operator curve (AUC) because (1) the models were tuned and selected based on Cohen's kappa and (2) simulation work shows that deriving thresholds based on AUC tends to overestimates the prevalence of rare events while it underestimates the prevalence of common events (Freeman and Moisen, 2008; Manel et al., 2001). We summed the number of pseudo-sets predicted to have bycatch each year, converted this sum to "true sets" assuming three sets per pseudo-set (**Fig. S4AB**), and multiplied this sum by the median number of captures when a capture occurs to generate estimates of the total number of captured animals (**Fig. S4C**). We opted not to employ a more complex two-stage or hurdle model approach, where a second model estimates the number of captured individuals when bycatch occurs, given the rarity of bycatch events larger than one for all species but common murre (**Fig. S4C**).

The random forest models estimate trends in bycatch that are similar to the estimates from the ratio estimator from 2000-2022 (**Fig. 3**). However, the estimates produced by the two approaches diverge from 1981-2000 by various extents. While they generally agree for California sea lion back to 2000, the random forest model underpredicts bycatch relative to the ratio estimator in the late 1990s and overpredicts in the 1980s and early 1990s (**Fig. 3**). While the approaches generally agree for harbor seal back to 1995, the random forest model underpredicts bycatch relative to the ratio estimator before 1995, especially in the Channel Islands and Ventura strata (**Fig. S14**). For common murre, the random forest model overpredicts bycatch relative to the ratio estimator in the mid- to late-1990s and underpredicts relative to the ratio estimator in earlier years, especially in Morro and Monterey Bays. These underpredictions likely occur because of the unequal impacts of lost data from the northern strata in the 1980s (**Fig. 2**). Unlike the random forest models, the ratio estimators are able to use summarized observer data for this region and time period from old reports. As a result, the ratio estimators can learn from observations from this region and time period while the random forest models are blind to data from this region and period. Thus, the random forest models are likely to underpredict risk in early years in northern strata because they largely learned from late years in southern California, where bycatch risk was lower. For this reason, we recommend the use of bycatch estimates from the ratio estimators over the random forest models until a time when the 1980s observer data is rediscovered.

Comparison to bycatch estimates from historical reports

Our estimates of annual bycatch are generally aligned with estimates from historical studies (**Fig. S11**). Slight differences between our estimates and those from historical studies are driven by a mixture of differences in our methods and input data. While we apply a consistent approach for defining the fishery, stratifying the data, and estimating bycatch, historical studies have employed variable fishery definitions, stratification schemes, and estimation methodologies (**Table S1**). First, we consistently defined the fishery as using ≥3.5" set gillnets, while historical studies have considered set gillnets ≥8", ≥8.5", or of unspecified sizes (**Table S1**). Furthermore, it is unclear whether historical studies filtered out fishing sets based on the reported target species and whether this decision was consistent. We do not define the fishery based on reported target species given extreme heterogeneity in how this information is reported. Differences in fishery definitions, as well as differences in data cleaning methods, likely lead to the slight differences in effort and observed bycatch attributed to the fishery (**Fig. S12**), which inevitably causes differences in bycatch estimates. Second, we used a consistent six-region stratification scheme, while historical studies used a mixture of stratification schemes ranging from no stratification to spatial stratification to spatial-temporal stratification (**Table S1**). This impacts the ratio estimators and the magnitudes of bycatch. Finally, we used vessel days (trips) as the sample unit, whereas historical studies oscillated between trips and sets as the preferred sample unit (**Table S1**). This is problematic not only because of the lack of consistency but also because sets cannot be uniquely identified in the logbook data.

Identifying set gillnet landing receipts and revenues

We used landing receipts (a.k.a., fish tickets) to estimate ex-vessel revenues generated by the California ≥3.5" set gillnet fishery from 1981-2022. Among other information, landing receipts report the date, value, species, and gear of commercial landings. We identified landing receipts associated with the ≥3.5" set gillnet fishery through a multi-step filtering process. First, we filtered the landing receipts to the five gear types that could include ≥3.5" mesh set gillnets: trammel nets, set gillnets, small-mesh set gillnets, large-mesh set gillnets, or entangling nets (**Fig. S1A**). Entangling nets, which encompass both set and drift gillnets, were a widely used gear type from 1984-1993. As a result, this filter retained many swordfish landings and other landings associated with drift gillnets. It also retained many herring landings and other landings associated with set gillnets with mesh sizes smaller than 3.5 inches. To remove landing receipts associated with drift gillnets and set gillnets with mesh sizes smaller than 3.5 inches, we used gillnet logbooks to identify landing receipts associated with known set gillnet vessels and logged set gillnet trips. We began by further filtering to only include vessels documented as using ≥3.5” set gillnets in the gillnet logbooks (**Fig. S1B**). After this filter, a large amount of swordfish and herring landings remained, indicating that many ≥3.5” set gillnet vessels use other gears. Thus, to further tie landing receipts with known ≥3.5” set gillnet trips, we explored four related approaches for linking landing receipts to logged ≥3.5” set gillnet trips. The first approach was the most strict and only considered landing receipts reported on the exact day of logged ≥3.5” set gillnet trips (**Fig. S1C**). This filter eliminated swordfish and herring landings but is likely to be overly restrictive. The date of landing may differ from the date of fishing because of misreporting, multi-day trips, or delayed sales. Thus, we explored three progressively less restrictive rules, which attributed landing receipts recorded within one (**Fig. S1D**), two (**Fig. S1E**), or three (**Fig. S1F**) days of logged ≥3.5” set gillnet trips to the fishery. We selected the landing receipts associated with the 3-day buffer as the final set of landing receipts associated with the fishery because it effectively eliminated landings of species not associated with the ≥3.5” set gillnet fishery (i.e., swordfish and herring) while being inclusive-within-reason of potential ≥3.5” set gillnet fishery landings. Finally, we adjusted daily ex-vessel landings values for inflation by converting all values to January 1, 2022 US dollars using the *priceR* package in R ~~(Condylios, 2023)~~(Condylios, 2023).

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Mapping species ranges

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We mapped the range of the study species using range maps from the California Wildlife Habitat Relationships (CWHR) System ~~(CDFW, 2021).~~(CDFW, 2021) (**Fig. S15**). The CWHR ranges were developed by species-specific experts. Range maps were not developed for harbor porpoise as part of the CWHR effort. We developed a range map for harbor porpoise assuming that harbor porpoise occur primarily in waters shallower than 50 fathoms (92 meters) north of Point Conception ~~(Forney et al., 2014).~~(Forney et al., 2014). Harbor seal haulouts were mapped using the CDFW Harbor Seal Haulout GIS dataset ~~(CDFW, 2014).~~(CDFW, 2014). CDFW conducted aerial surveys of all known haulout sites in 2001, 2002, and 2003 and counted the number of harbor seals observed in aerial photographs of each site. We mapped northern elephant seal rookery size in 2010 using a database of counts developed by ~~(Lowry et al., 2014)~~(Lowry et al., 2014). Counts were generated through a review of ground and aerial photographic surveys. We mapped California sea lion haulouts using data from ~~(Lowry, 2021).~~(Lowry, 2021). Haulouts were mapped in the Channels Islands between 2016-2019 using aerial photographic surveys. Sea lion haulouts occur along the California coast but were not mapped to single sites in this study and therefore not plotted in our range maps. We mapped seabird colonies using the 2010 CDFW Seabird Colonies Database ~~(CDFW, 2010).~~(CDFW, 2010). These data were collected as part of the

Marine Life Protection Act (MLPA) planning process and report the maximum number of seabirds of 26 species at all known colonies.

Supplemental Tables

Table S1. Historical bycatch estimation studies and their characteristics (the period column indicates month and year as MM/YY).

Study	Seasons	Period	Species	Fishery definition	Stratification scheme	Sample unit
(Barlow et al., 1994)	1983-87	(see papers)	Pinnipeds/cetaceans	(see included papers)	(see included papers)	(see papers)
(Hanan et al., 1988)	1983-85	Apr 1-Mar 31	Sea lion, harbor seals	Set nets for halibut/flounder/sharks	5 regions	Sets
(Diamond and Hanan, 1986)	1983	4/83 - 3/84	Harbor porpoise	≥8.0” set nets for halibut/flounder	3 regions	Sets
(Hanan et al., 1986)	1984	4/84 - 3/85	Harbor porpoise	Set nets for halibut/flounder/sharks	3 regions	Sets
(Hanan et al., 1987)	1985	4/85 - 3/86	Harbor porpoise	Set nets (but not for croaker)	3 regions	Sets
(Hanan and Diamond, 1989)	1986	4/86 - 3/87	Sea lion, harbor seal, harbor porpoise	Set nets for halibut/flounder/sharks	5 regions, seasons	Sets
(Konno, 1990)	1987	4/87 - 3/88	Sea lion, harbor seal, harbor porpoise	Set nets for halibut/flounder/sharks	5 regions, seasons	Sets
(Perkins et al., 1994)	1988-90	4/88 - 3/89 4/89 - 12/89 1/90 - 12/90	Pinnipeds/cetaceans	Set nets for halibut/angel shark	3 regions	Vessel-day
(Lennert et al., 1994)	1990	7/90 - 12/90	Marine mammals	Set nets for halibut/angel shark	3 regions	Vessel-day
(Perkins et al., 1992a)	1990	7/90 - 6/91	Pinnipeds/cetaceans	≥8.0” nets for halibut/angel shark	3 regions	Sets
(Perkins et al., 1992b)	1991	1/91 - 12/91	Pinnipeds/cetaceans	Set nets for halibut/angel shark	SCal/Ventura: Quarters Other strata: Annual	Vessel-day
(Julian, 1993)	1992	1/92 - 12/92	Pinnipeds/cetaceans	Set nets for halibut/angel shark	4 regions: quarterly	Vessel-day
(Julian, 1994)	1993	1/93 - 12/93	Pinnipeds/cetaceans	Set nets for halibut	4 regions	Vessel-day
(Julian and Beeson, 1998)	1990-95	7/90-12/90 1-12, 91-95	Mammals/seabirds/turtles	≥8.5” nets for halibut/angel shark	SCal/Ventura: Quarters Other strata: Annual	Vessel-day
(Cameron and Forney, 1999)	1997-98	1/97 - 12/97 1/98 - 12/98	Cetaceans	≥8.5” nets for halibut/angel shark	1997: Geographical+seasonal 1998: Geographical only	Vessel-day
(Cameron and Forney, 2000)	1999	1/99 - 12/99	Cetaceans	≥8.5” nets for halibut/angel shark	Geographical+seasonal	Vessel-day
(Carretta, 2001)	2000	1/00 - 12/00	Cetaceans	≥8.5” nets for halibut/angel shark	SCal/Ventura: Quarters Other strata: Annual	Vessel-day
(Carretta, 2002)	2001	1/01 - 12/01	Cetaceans	Set nets for halibut/angel shark	SCal/Ventura: Quarters Other strata: Annual	Vessel-day
(Carretta and Chivers, 2003)	2002	1/02 - 12/02	Cetaceans	Set nets for halibut/angel shark	SCal/Ventura: Quarters Other regions: Annual	Vessel-day
(Carretta and Chivers, 2004)	2003	1/03 - 12/03	Marine mammals	≥8.5” nets for halibut/angel shark	SCal/Ventura: Quarters Other regions: Annual	Vessel-day
(Carretta and Enriquez, 2009)	2007	1/07 - 12/07	Mammals/seabirds	Set nets for halibut/white seabass	No stratification	Sets
(Carretta and Enriquez, 2012a)	2010	1/10 - 12/10	Mammals/seabirds	Set nets for halibut/white seabass	No stratification	Vessel-day
(Carretta and Enriquez, 2012b)	2011	1/11 - 12/11	Mammals/seabirds	Set nets for halibut/white seabass	No stratification	Sets

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(Carretta et al., 2014)	2012	1/12 - 12/12	Mammals/seabirds/turtles	Set nets for halibut/white seabass	No stratification	Vessel-day

1448 **Table S2.** Number of fishing trips in the California 3.5 inch mesh set gillnet fishery by year.

		# of trips			Percent of trips	
Year	**# of vessels**	**Total**	**Observed**	**Unobserved**	**Observed**	**Unobserved**
1981	162	6139	0	6139	0.0%	100.0%
1982	222	9218	0	9218	0.0%	100.0%
1983	262	9012	143	8869	1.6%	98.4%
1984	357	12374	255	12119	2.1%	97.9%
1985	378	14314	238	14076	1.7%	98.3%
1986	379	13336	217	13119	1.6%	98.4%
1987	290	10667	213	10454	2.0%	98.0%
1988	211	8585	120	8465	1.4%	98.6%
1989	198	7811	58	7753	0.7%	99.3%
1990	182	6836	167	6669	2.4%	97.6%
1991	158	6668	716	5952	10.7%	89.3%
1992	117	5611	744	4867	13.3%	86.7%
1993	115	5754	959	4795	16.7%	83.3%
1994	74	2455	150	2305	6.1%	93.9%
1995	70	2616	0	2616	0.0%	100.0%
1996	59	2654	0	2654	0.0%	100.0%
1997	66	3310	0	3310	0.0%	100.0%
1998	59	2889	0	2889	0.0%	100.0%
1999	63	4026	165	3861	4.1%	95.9%
2000	66	3828	66	3762	1.7%	98.3%
2001	63	3289	0	3289	0.0%	100.0%
2002	54	3395	0	3395	0.0%	100.0%
2003	51	2779	0	2779	0.0%	100.0%
2004	47	2627	0	2627	0.0%	100.0%
2005	40	1930	0	1930	0.0%	100.0%
2006	41	1658	5	1653	0.3%	99.7%
2007	44	1797	65	1732	3.6%	96.4%
2008	43	1936	0	1936	0.0%	100.0%
2009	41	1934	0	1934	0.0%	100.0%
2010	33	1544	64	1480	4.1%	95.9%
2011	39	1575	55	1520	3.5%	96.5%
2012	35	1374	76	1298	5.5%	94.5%
2013	32	968	48	920	5.0%	95.0%
2014	25	819	0	819	0.0%	100.0%
2015	25	1014	0	1014	0.0%	100.0%
2016	24	1077	0	1077	0.0%	100.0%
2017	21	840	69	771	8.2%	91.8%
2018	19	948	0	948	0.0%	100.0%
2019	18	1151	0	1151	0.0%	100.0%
2020	18	841	0	841	0.0%	100.0%
2021	16	870	0	870	0.0%	100.0%

Table S3. Bycatch rates extracted from historical reports (* mark years with raw observer data where the summary values from the historical reports are not needed).

Reference	Species	Region	Year	# sets observed	Estimated # trips observed	Catch	Catch per trip	Raw data available?
Hanan et al. 1988	California sea lion	Channel Islands	1985	180	60.00	44	0.733	*
Hanan & Diamond 1989	California sea lion	Channel Islands	1986	66	22.00	54	2.455	*
Hanan et al. 1988	California sea lion	Monterey Bay	1983	22	7.33	12	1.636	
Hanan et al. 1988	California sea lion	Monterey Bay	1984	126	42.00	19	0.452	
Hanan et al. 1988	California sea lion	Monterey Bay	1985	49	16.33	5	0.306	
Hanan & Diamond 1989	California sea lion	Monterey Bay	1986	36	12.00	4	0.333	
Hanan et al. 1988	California sea lion	Morro Bay	1983	288	96.00	41	0.427	
Hanan et al. 1988	California sea lion	Morro Bay	1984	374	124.67	22	0.176	
Hanan et al. 1988	California sea lion	Morro Bay	1985	317	105.67	25	0.237	
Hanan & Diamond 1989	California sea lion	Morro Bay	1986	137	45.67	13	0.285	*
Hanan et al. 1988	California sea lion	San Francisco	1983	158	52.67	4	0.076	
Hanan et al. 1988	California sea lion	San Francisco	1984	300	100.00	8	0.080	
Hanan et al. 1988	California sea lion	San Francisco	1985	348	116.00	3	0.026	
Hanan & Diamond 1989	California sea lion	San Francisco	1986	419	139.67	2	0.014	
Hanan et al. 1988	California sea lion	Southern California	1983	430	143.33	16	0.112	*
Hanan et al. 1988	California sea lion	Southern California	1984	571	190.33	13	0.068	*
Hanan et al. 1988	California sea lion	Southern California	1985	339	113.00	5	0.044	*
Hanan & Diamond 1989	California sea lion	Southern California	1986	425	141.67	15	0.106	*
Hanan et al. 1988	California sea lion	Ventura	1983	430	143.33	16	0.112	*
Hanan et al. 1988	California sea lion	Ventura	1984	571	190.33	13	0.068	*
Hanan et al. 1988	California sea lion	Ventura	1985	339	113.00	5	0.044	*
Hanan & Diamond 1989	California sea lion	Ventura	1986	425	141.67	15	0.106	*
Diamond & Hanan 1986	Harbor porpoise	Monterey Bay	1983	22	7.33	2	0.273	
Hanan et al. 1986	Harbor porpoise	Monterey Bay	1984	126	42.00	2	0.048	
Hanan et al. 1987	Harbor porpoise	Monterey Bay	1985	49	16.33	2	0.122	
Hanan & Diamond 1989	Harbor porpoise	Monterey Bay	1986	36	12.00	1	0.083	
Diamond & Hanan 1986	Harbor porpoise	Morro Bay	1983	288	96.00	7	0.073	
Hanan et al. 1986	Harbor porpoise	Morro Bay	1984	374	124.67	3	0.024	
Hanan et al. 1987	Harbor porpoise	Morro Bay	1985	317	105.67	3	0.028	
Hanan & Diamond 1989	Harbor porpoise	Morro Bay	1986	137	45.67	3	0.066	*
Diamond & Hanan 1986	Harbor porpoise	San Francisco	1983	151	50.33	5	0.099	
Hanan et al. 1986	Harbor porpoise	San Francisco	1984	299	99.67	14	0.140	
Hanan et al. 1987	Harbor porpoise	San Francisco	1985	348	116.00	28	0.241	
Hanan & Diamond 1989	Harbor porpoise	San Francisco	1986	419	139.67	12	0.086	
Hanan et al. 1988	Harbor seal	Channel Islands	1985	180	60.00	13	0.217	*
Hanan et al. 1988	Harbor seal	Monterey Bay	1983	22	7.33	0	0.000	
Hanan et al. 1988	Harbor seal	Monterey Bay	1984	126	42.00	10	0.238	
Hanan et al. 1988	Harbor seal	Monterey Bay	1985	49	16.33	7	0.429	
Hanan & Diamond 1989	Harbor seal	Monterey Bay	1986	36	12.00	9	0.750	
Hanan et al. 1988	Harbor seal	Morro Bay	1983	288	96.00	17	0.177	
Hanan et al. 1988	Harbor seal	Morro Bay	1984	374	124.67	29	0.233	
Hanan et al. 1988	Harbor seal	Morro Bay	1985	317	105.67	84	0.795	
Hanan & Diamond 1989	Harbor seal	Morro Bay	1986	137	45.67	25	0.547	*
Hanan et al. 1988	Harbor seal	San Francisco	1983	158	52.67	11	0.209	
Hanan et al. 1988	Harbor seal	San Francisco	1984	300	100.00	22	0.220	
Hanan et al. 1988	Harbor seal	San Francisco	1985	348	116.00	38	0.328	
Hanan & Diamond 1989	Harbor seal	San Francisco	1986	419	139.67	60	0.430	
Hanan et al. 1988	Harbor seal	Southern California	1983	430	143.33	0	0.000	*
Hanan et al. 1988	Harbor seal	Southern California	1984	571	190.33	4	0.021	*
Hanan et al. 1988	Harbor seal	Southern California	1985	339	113.00	2	0.018	*
Hanan & Diamond 1989	Harbor seal	Southern California	1986	425	141.67	8	0.056	*
Hanan et al. 1988	Harbor seal	Ventura	1983	430	143.33	0	0.000	*
Hanan et al. 1988	Harbor seal	Ventura	1984	571	190.33	4	0.021	*
Hanan et al. 1988	Harbor seal	Ventura	1985	339	113.00	2	0.018	*
Hanan & Diamond 1989	Harbor seal	Ventura	1986	425	141.67	8	0.056	*

Table S4. Assumptions made throughout the analysis, their likelihood, and their potential impact on the results (*assumptions that are expected to be valid or true on average are not likely to impact the results).

Assumption	**Likelihood and potential impact of assumption**
Data analysis	
Ratio estimation	
1. Ratio estimation assumes that the rate of bycatch for observed fishing trips equals the rate for all fishing trips in a given stratum.	This assumption appears valid as the traits of observed trips are representative of the unobserved trips (**Fig. S7**) and we stratified our analysis using the 7-region scheme recommended in other studies (**Fig. S8**).
2. We assumed that the bycatch rate in years without observer data was equal to the bycatch rate in the closest year with data.	The validity of this assumption is difficult to assess given the inconsistency in the observer program (**Fig. 2BC**); however, it is this inconsistency that also makes the assumption necessary. Ultimately, our finding that recent bycatch is low is not likely sensitive to this assumption, as the effects of substantial decreases in fishing effort overwhelm the effects of changes in bycatch rates.
Bycatch estimation using the random for models*	
1. We assumed that a “pseudo-set” (roughly equivalent to a trip) is equivalent to 3 sets (**Fig. S4AB**).	This assumption is likely to be true on average since it reflects the median value, which has been consistent through time (**Fig. S4AB**).
2. We assumed the number of captures per set is the median number of captures when a capture occurs (**Fig. S4C**).	This assumption is likely to be true on average since it reflects the median value and that for most of the evaluated species, deviations from the median have been rare (**Fig. S4C**).
Data imputation	
Observer data	
1. We assigned missing GPS coordinates using the median coordinates for observed trips within the statistical block most frequently visited by the vessel – in order of preference – that week, month, or year based on the logbook data.	This assumption is likely to be true on average given that it uses vessel-specific median values.
2. We derived missing fishing depths by extracting depths from 25-meter resolution bathymetry data (**Fig. S3B**).	If the GPS coordinates were accurately reported, then the extracted depth estimates are accurate.
3. We reassigned missing soak hours the mode value for a vessel and target species.	This assumption is likely to be true on average given that it uses vessel-specific median values.
4. We reassigned missing mesh sizes the mode for – in order of preference – the vessel and target species, the target species, or all vessels (**Fig. S3DE**).	This assumption is likely to be true on average given that it uses mode values that are vessel-specific to the greatest extent possible.
5. We assigned each GPS coordinate to the nearest statistical reporting block, which allows points erroneously falling on land to be assigned a likely statistical block.	If the GPS coordinates were close to accurate, then this procedure is accurate on average.
Logbook data	
1. We reassigned both missing and unrealistic fishing depths, defined as depths exceeding the maximum depth in the reported fishing block, the median depth of the fishing block (**Fig. S5B**).	This assumption is likely to be true on average given it uses the median value.
2. We reassigned missing soak hours the mode value for a vessel.	This assumption is likely to be true on average given that it uses vessel-specific mode values.
3. We capped unlikely soak times exceeding 96 hours at 96 hours.	The validity of this assumption is unclear because the values could be typos; however, values >96 hours were rare and exert little leverage on the results.

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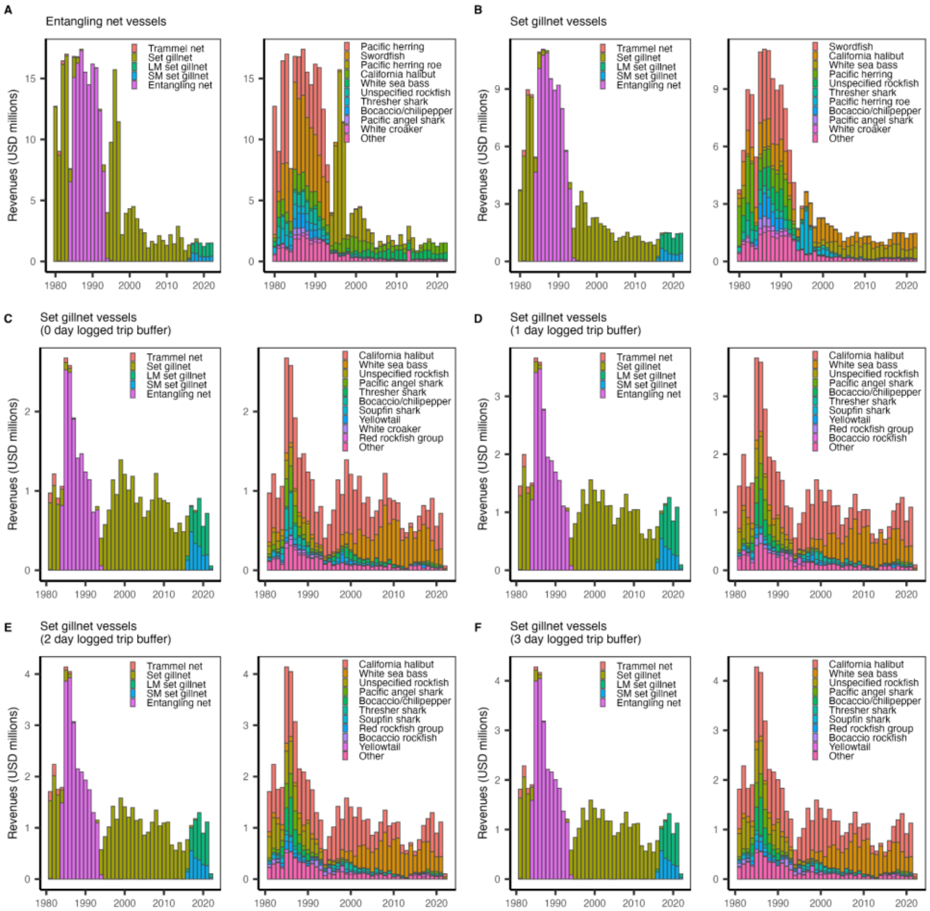
4. We reassigned missing and unrealistic (>20 inch) mesh sizes using a hierarchical procedure (**Fig. S5DE**). For logbooks with both vessel id and target species info, we assigned the mesh size most commonly used by the vessel when targeting that target species. For logbooks with only target species info (no vessel id), we assigned the mesh size most commonly used when targeting that target species across all vessels (**Figs. S5 & S6**).	This assumption is likely to be true on average given that it uses mode values that are vessel-specific to the greatest extent possible.

*** This analysis is informational and only included in the supplemental information.*

Supplemental Figures


A
Entangling net vessels
Trammel net
Set gillnet
LM set gillnet
SM set gillnet
Entangling net
Pacific herring
Swordfish
Pacific herring roe
California halibut
White sea bass
Unspecified rockfish
Thresher shark
Bocaccio/chilipepper
Pacific angel shark
White croaker
Other
B
Set gillnet vessels
Trammel net
Set gillnet
LM set gillnet
SM set gillnet
Entangling net
Swordfish
California halibut
White sea bass
Pacific herring
Unspecified rockfish
Thresher shark
Pacific herring roe
Bocaccio/chilipepper
Pacific angel shark
White croaker
Other
C
Set gillnet vessels
(0 day logged trip buffer)
Trammel net
Set gillnet
LM set gillnet
SM set gillnet
Entangling net
California halibut
White sea bass
Unspecified rockfish
Pacific angel shark
Thresher shark
Bocaccio/chilipepper
Soupfin shark
Yellowtail
White croaker
Red rockfish group
Other
D
Set gillnet vessels
(1 day logged trip buffer)
Trammel net
Set gillnet
LM set gillnet
SM set gillnet
Entangling net
California halibut
White sea bass
Unspecified rockfish
Pacific angel shark
Bocaccio/chilipepper
Thresher shark
Soupfin shark
Yellowtail
Red rockfish group
Bocaccio rockfish
Other
E
Set gillnet vessels
(2 day logged trip buffer)
Trammel net
Set gillnet
LM set gillnet
SM set gillnet
Entangling net
California halibut
White sea bass
Unspecified rockfish
Pacific angel shark
Bocaccio/chilipepper
Thresher shark
Soupfin shark
Red rockfish group
Bocaccio rockfish
Yellowtail
Other
F
Set gillnet vessels
(3 day logged trip buffer)
Trammel net
Set gillnet
LM set gillnet
SM set gillnet
Entangling net
California halibut
White sea bass
Unspecified rockfish
Pacific angel shark
Bocaccio/chilipepper
Thresher shark
Soupfin shark
Red rockfish group
Bocaccio rockfish
Yellowtail
Other
Revenues (USD millions)
1980
1990
2000
2010
2020

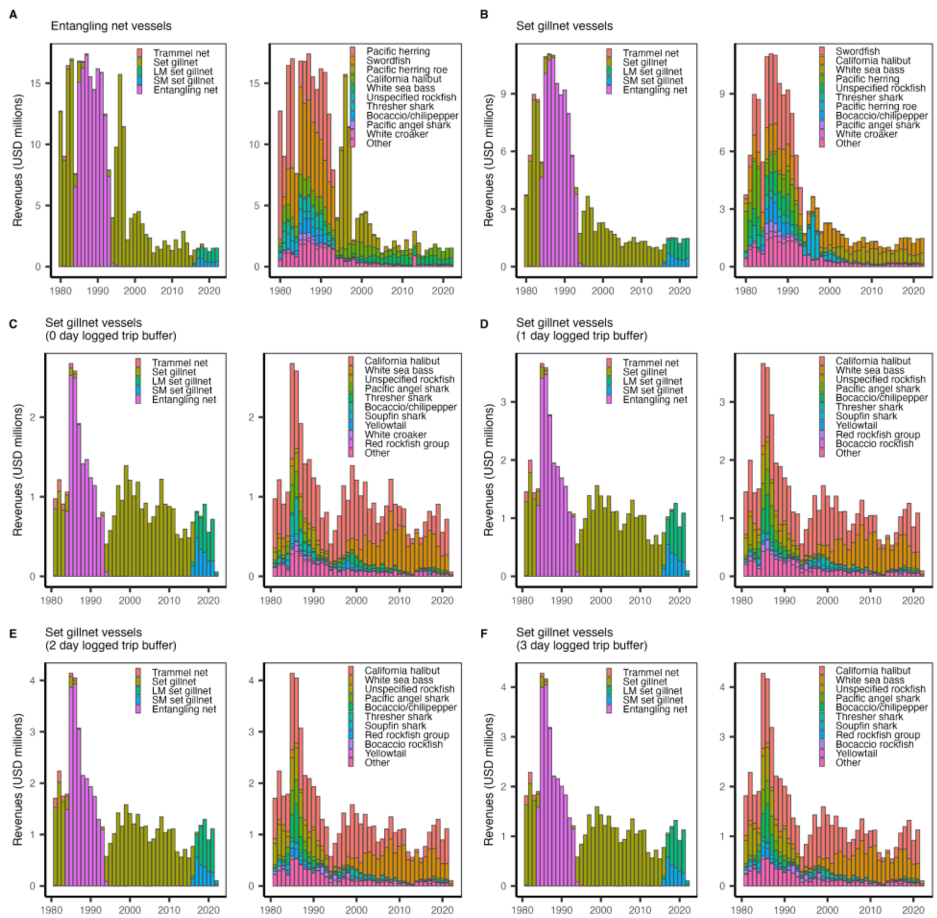
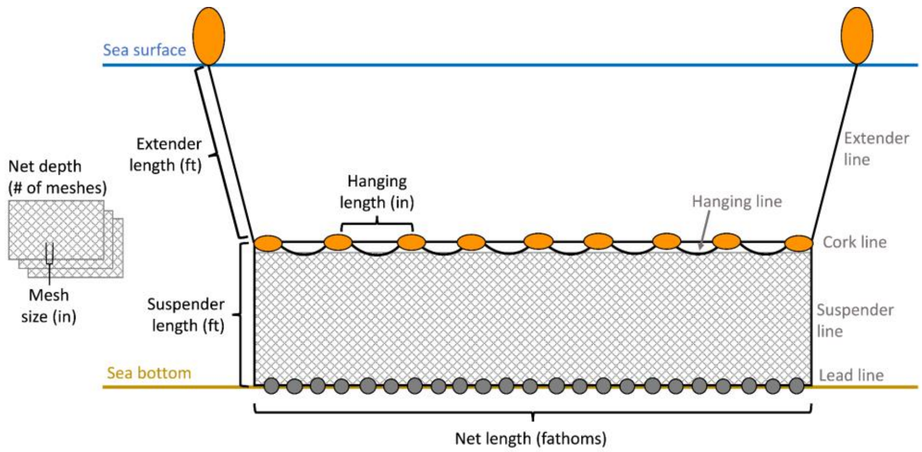


Fig. S1. Estimated ex-vessel revenues generated by the California ≥3.5” set gillnet fishery as estimated through six different filtration procedures. The filtration procedures examine the sum annual ex-vessel revenues reported on landing receipts from **(A)** vessels using various reported entangling net gears; **(B)** vessels using various entangling net gears that are known to use set gillnets based on logbooks; and **(C-F)** vessels known to use set gillnets based on logbooks that are dated within various buffers of a logged set gillnet trip. We adopted the final filter, which sums landing receipts date within 3 days of logged set gillnet trip, as the best estimate of ex-vessel revenues for the fishery. Revenues have not been adjusted for inflation (see **Fig. 1D** for the inflation adjusted ex-vessel revenues).



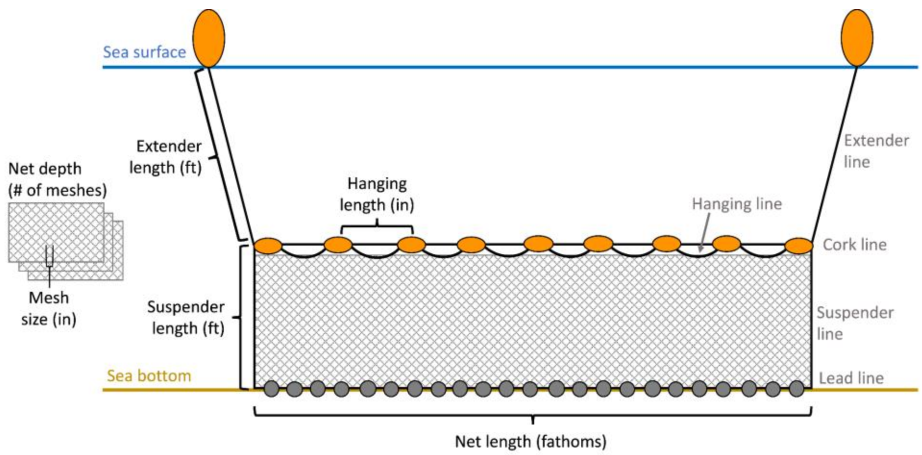


Fig. S2. Diagram of a typical California set gillnet illustrating the measurements reported in the logbook and observer data. Grey text indicates parts of the gillnet and black text indicates the reported measurements. The material, strength, and color of the net is also reported. Finally, the hanging ratio, a percentage that is calculated as the length of the mesh web divided by the length of the cork line, is reported.

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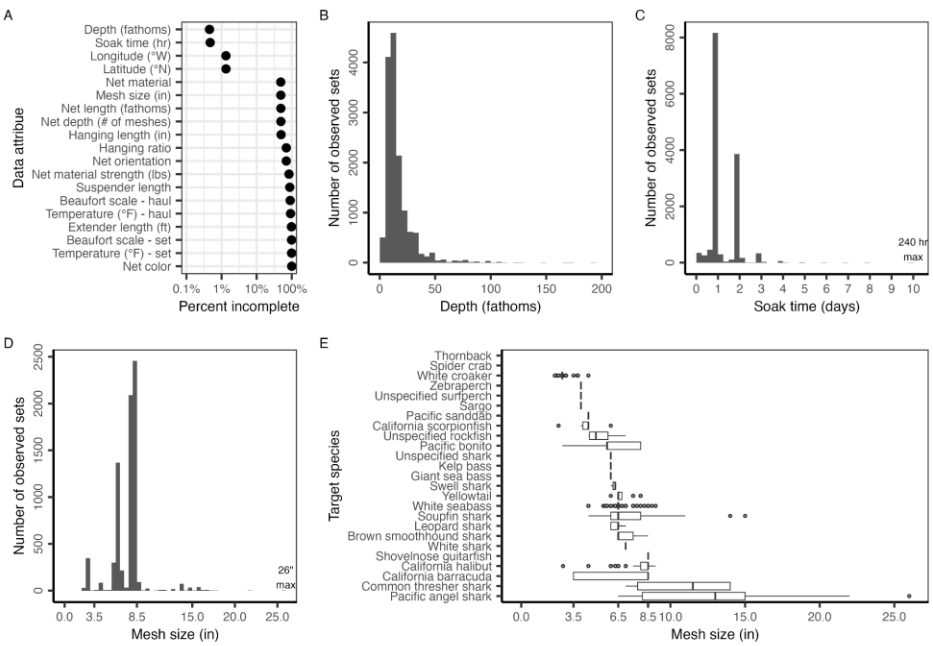

A
Depth (fathoms)
Soak time (hr)
Longitude (°W)
Latitude (°N)
Net material
Mesh size (in)
Net length (fathoms)
Net depth (# of meshes)
Hanging length (in)
Hanging ratio
Net orientation
Net material strength (lbs)
Suspender length
Beaufort scale - haul
Temperature (°F) - haul
Extender length (ft)
Beaufort scale - set
Temperature (°F) - set
Net color
Data attribue
0.1% 1% 10% 100%
Percent incomplete
B
Number of observed sets
0 1000 2000 3000 4000
0 50 100 150 200
Depth (fathoms)
C
Number of observed sets
0 2000 4000 6000 8000
240 hr
max
0 1 2 3 4 5 6 7 8 9 10
Soak time (days)
D
Number of observed sets
0 500 1000 1500 2000 2500
26"
max
0.0 3.5 8.5 15.0 20.0 25.0
Mesh size (in)
E
Thornback
Spider crab
White croaker
Zebraperch
Unspecified surfperch
Sargo
Pacific sanddab
California scorpionfish
Unspecified rockfish
Pacific bonito
Unspecified shark
Kelp bass
Giant sea bass
Swell shark
Yellowtail
White seabass
Soupfin shark
Leopard shark
Brown smoothhound shark
White shark
Shovelnose guitarfish
California halibut
California barracuda
Common thresher shark
Pacific angel shark
Target species
0.0 3.5 6.5 8.5 10.0 15.0 20.0 25.0
Mesh size (in)

1477

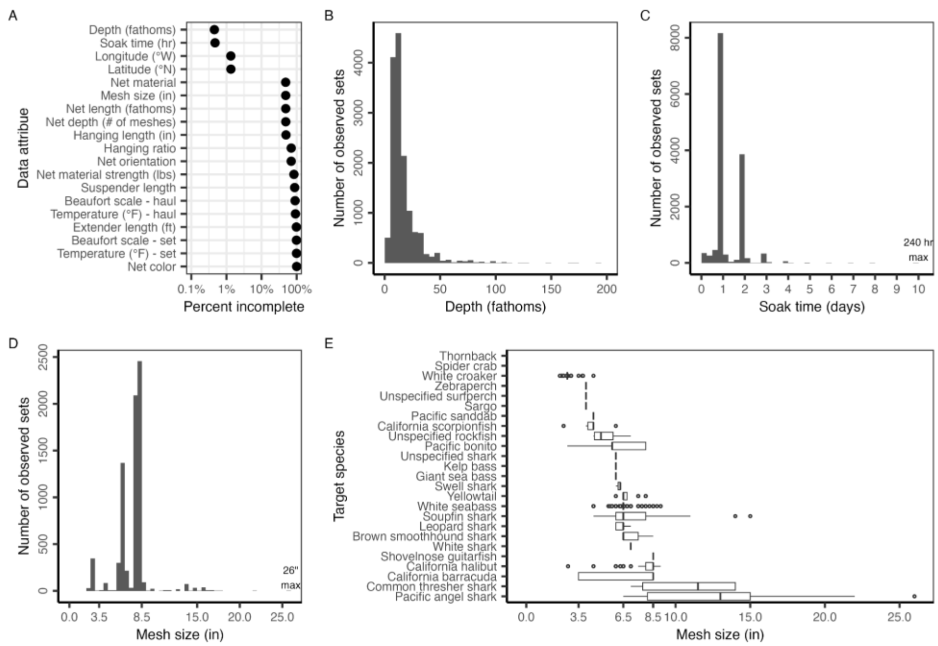
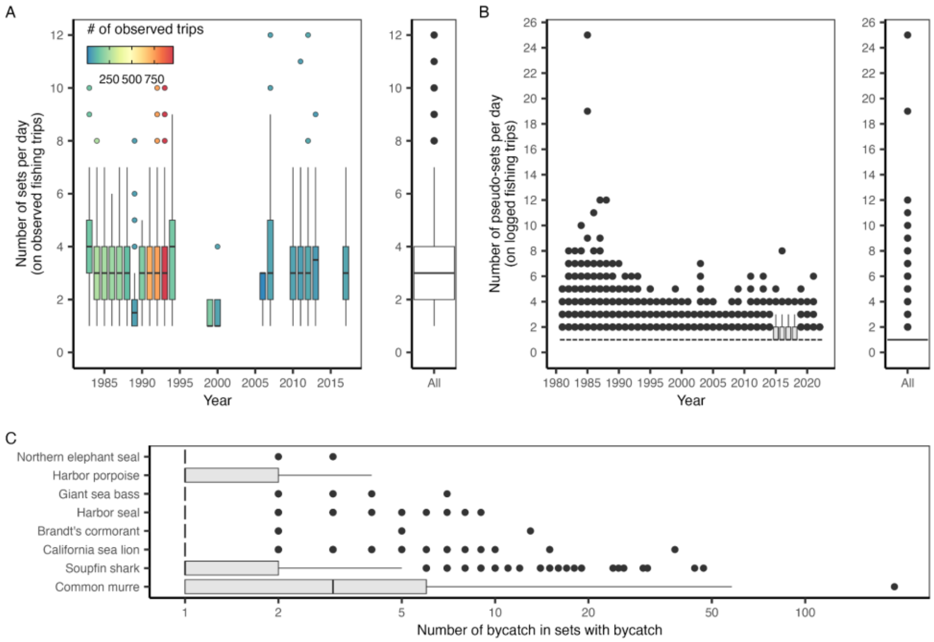


Fig. S3. Traits of the observed set gillnet metadata. Panel **A** shows the level of completeness of gillnet metadata. Panel **B** shows the distribution of reported depths. Panel **C** shows the distribution of reported soak times; the maximum reported soak time is 240 hours (10 days). Panel **D** shows the distribution of reported mesh sizes; the maximum reported mesh size is 26 inches. Panel **E** shows the distribution of reported mesh sizes by reported target species. In the boxplots, the solid line indicates the median, the box indicates the interquartile range (IQR; 25th to 75th percentiles), the whiskers indicate 1.5 times the IQR, and points indicate outliers.



A
# of observed trips
250 500 750
Number of sets per day
(on observed fishing trips)
Year
All
B
Number of pseudo-sets per day
(on logged fishing trips)
Year
All
C
Northern elephant seal
Harbor porpoise
Giant sea bass
Harbor seal
Brandt's cormorant
California sea lion
Soupfin shark
Common murre
Number of bycatch in sets with bycatch

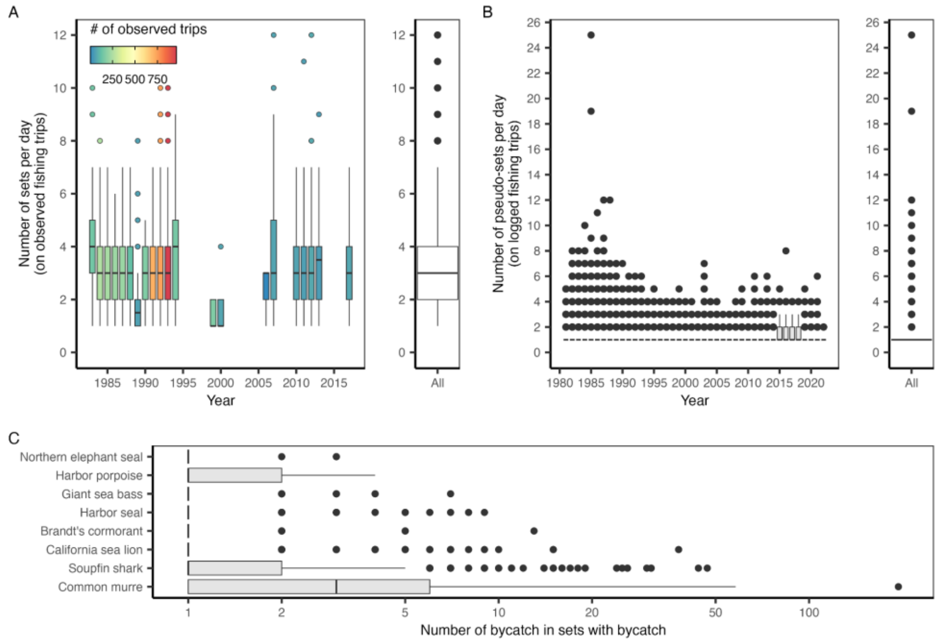
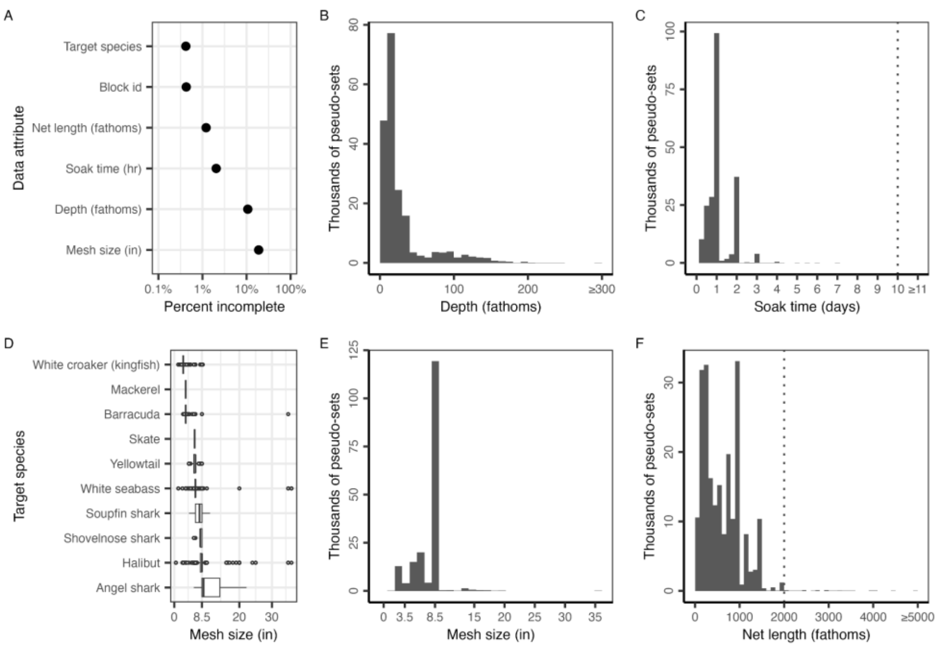


Fig. S4. The **(A)** number of sets per observed fishing trip by year and overall; **(B)** number of pseudo-sets per logged fishing trip by year and overall; and **(C)** number of bycatch in sets with bycatch of each of the species of interest. In the boxplots, the solid line indicates the median, the box indicates the interquartile range (IQR; 25th to 75th percentiles), the whiskers indicate 1.5 times the IQR, and points indicate outliers. In **(A)**, the fill color indicates the number of observed fishing trips contributing to the annual distribution.


A
Data attribute
Target species
Block id
Net length (fathoms)
Soak time (hr)
Depth (fathoms)
Mesh size (in)
0.1%
1%
10%
100%
Percent incomplete
B
Thousands of pseudo-sets
Depth (fathoms)
≥300
C
Thousands of pseudo-sets
Soak time (days)
≥11
D
Target species
White croaker (kingfish)
Mackerel
Barracuda
Skate
Yellowtail
White seabass
Soupfin shark
Shovelnose shark
Halibut
Angel shark
Mesh size (in)
E
Thousands of pseudo-sets
Mesh size (in)
F
Thousands of pseudo-sets
Net length (fathoms)
≥5000

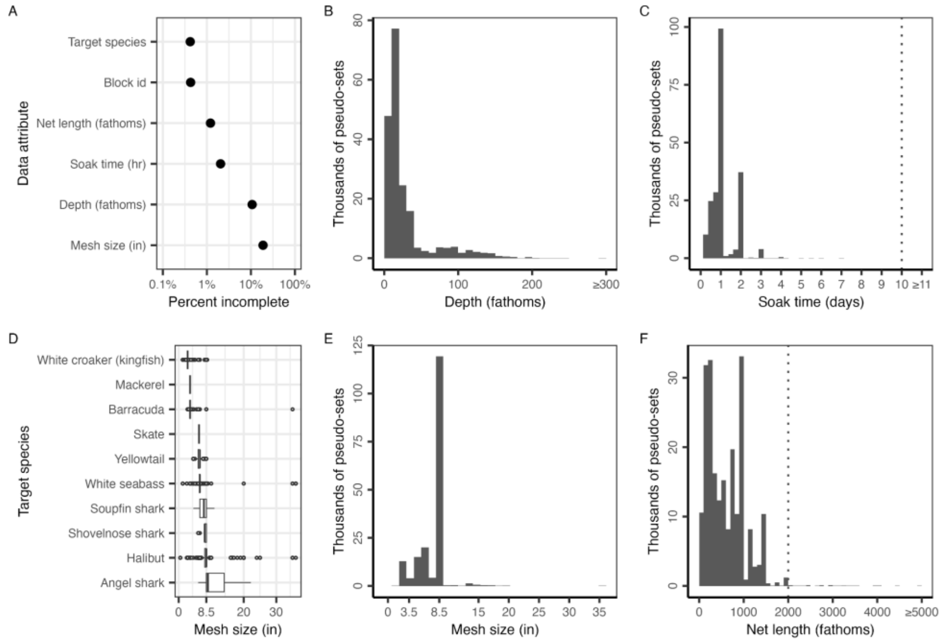
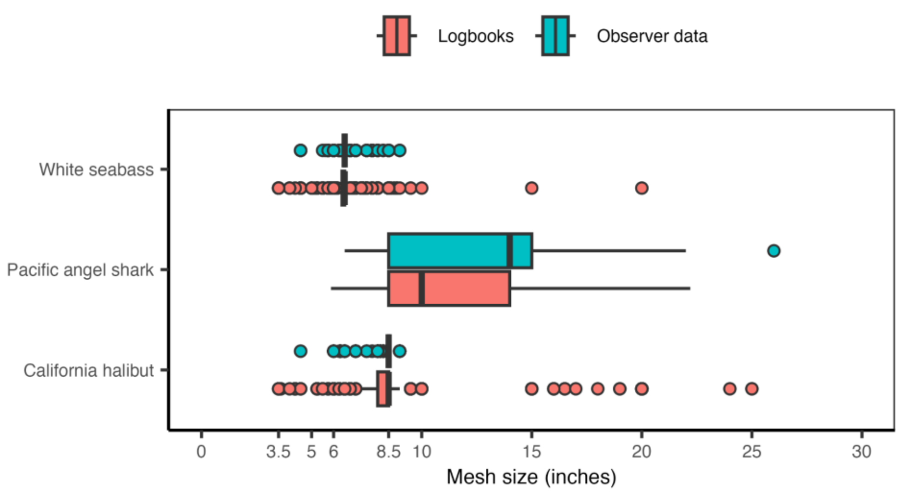


Fig. S5. Traits of the set gillnet sets documented in the logbook data. Panel **A** shows the level of completeness of gillnet logbook metadata. Panel **B** shows the distribution of reported depths. Panel **C** shows the distribution of reported soak times. The maximum reported soak time in the observer data is 10 days; rare values larger than this value were assumed to be unrealistic and were capped at the maximum. Panel **D** shows the distribution of reported mesh sizes by reported target species. In the boxplots, the solid line indicates the median, the box indicates the interquartile range (IQR; 25th to 75th percentiles), the whiskers indicate 1.5 times the IQR, and points indicate outliers. Panel **E** shows the distribution of reported mesh sizes. Panel **F** shows the distribution of reported net lengths.



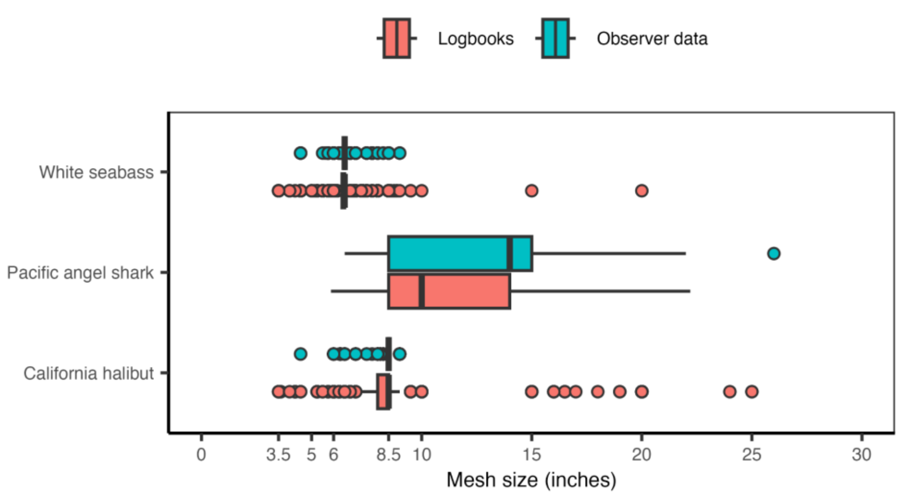
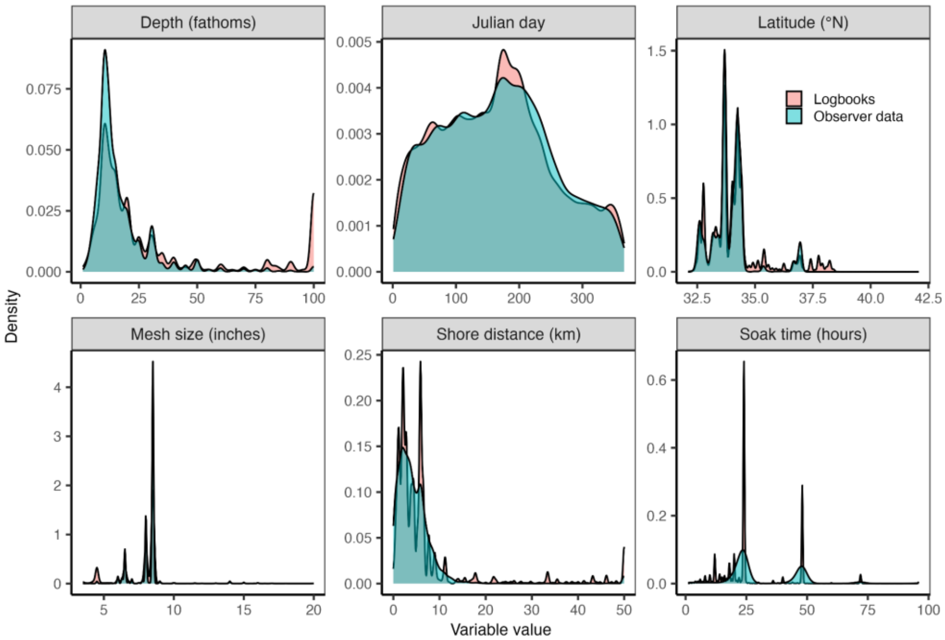


Fig. S6. Mesh size (inches) by target species in the logbook (red) and observer (blue) data. Since 1989, California halibut and Pacific angel shark can only be targeted using mesh sizes larger than 8.5 inches. White seabass are typically targeted using a minimum mesh size of 6.0 inches; however, a small amount of incidental take (<20% of catch and ≤10 individuals) in mesh sizes between 3.5 to 6.0 inches is allowed from June 16 to March 14 (14, § 155.10).


Depth (fathoms)
Julian day
Latitude (°N)
Logbooks
Observer data
Mesh size (inches)
Shore distance (km)
Soak time (hours)
Density
Variable value

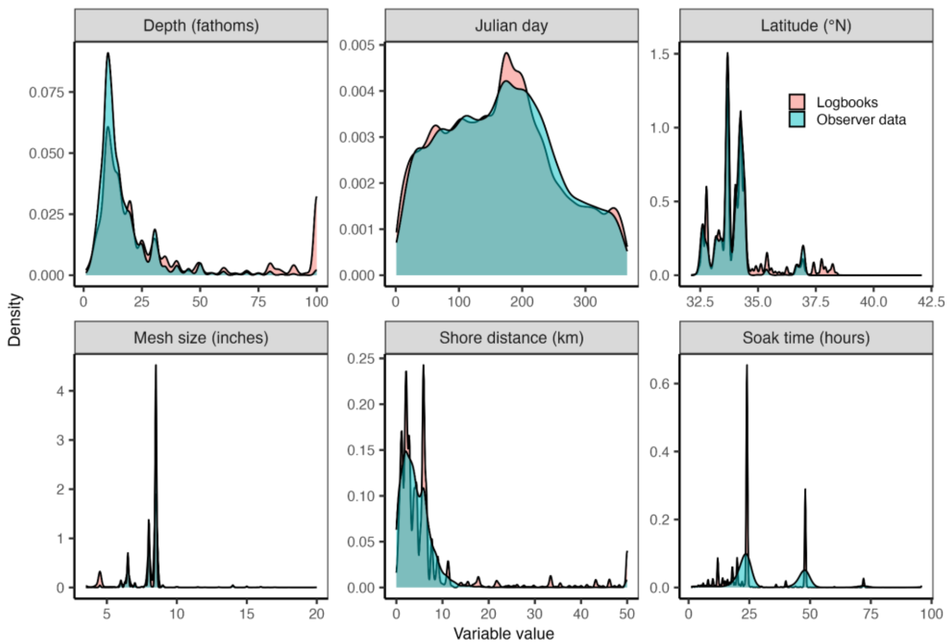
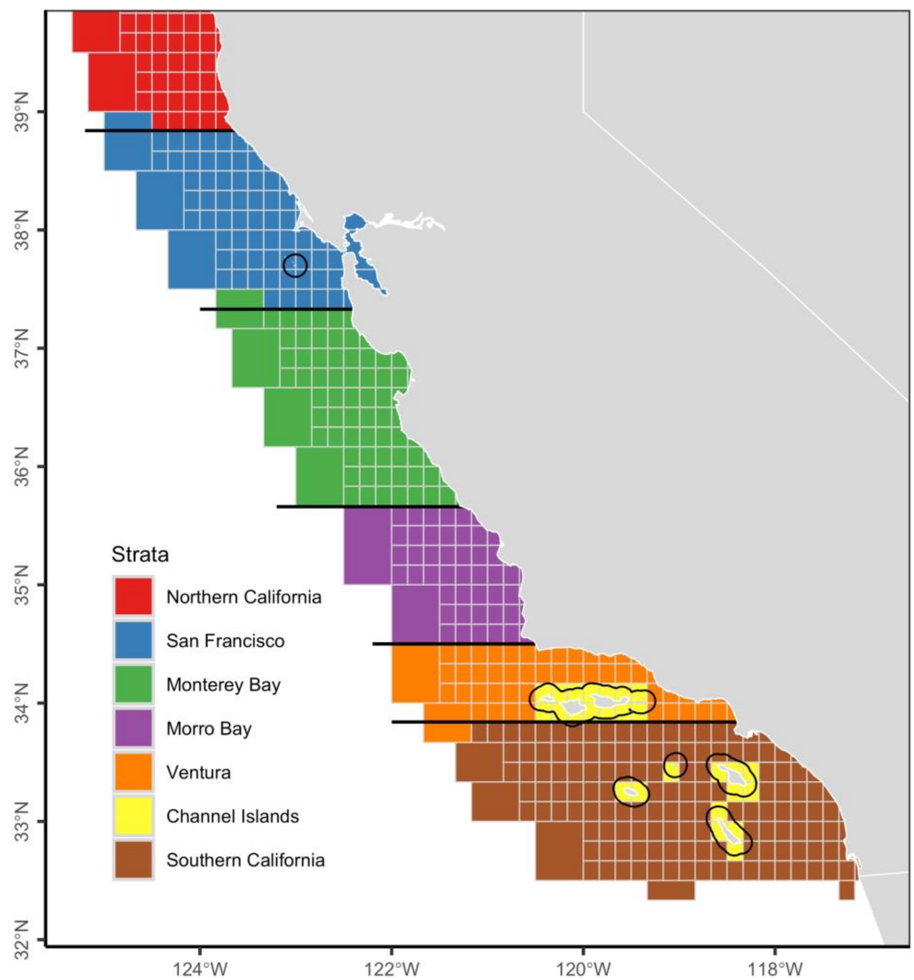


Fig. S7. Comparison of key set traits in the logbook (red) and observer (blue) data. We used a two-sided Kolmogorov-Smirnov test to confirm that the traits of the logbook and observer data could have come from the same probability distribution (all p-values < 0.001) could have come from the same probability distribution (all p-values < 0.001).


Strata
Northern California
San Francisco
Monterey Bay
Morro Bay
Ventura
Channel Islands
Southern California
39°N
38°N
37°N
36°N
35°N
34°N
33°N
32°N
124°W
122°W
120°W
118°W

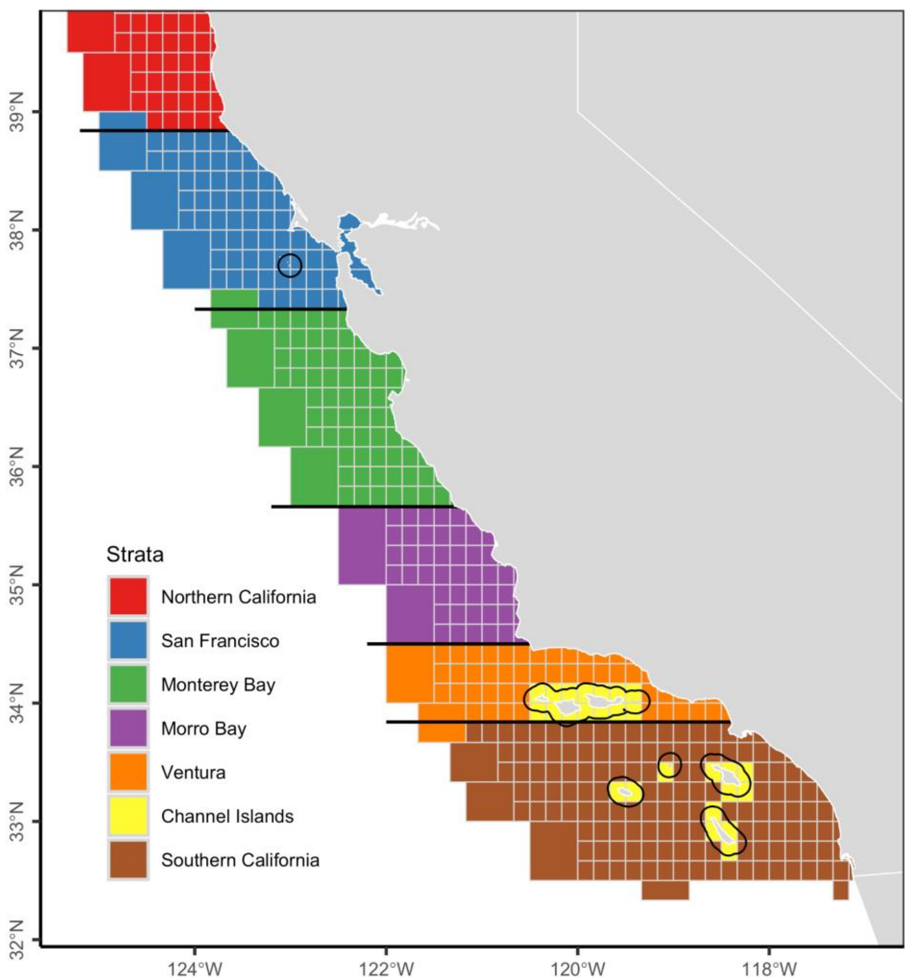


Fig. S8. The regional stratification scheme used throughout the analysis. The stratification scheme north of Point Conception was originally proposed by Diamond and Hanan (1986). The stratification scheme south of Point Conception was originally proposed by Julian (1993). The dark black lines around the Channel Islands show the 10 km buffer used to identify island-associated fishing trips.

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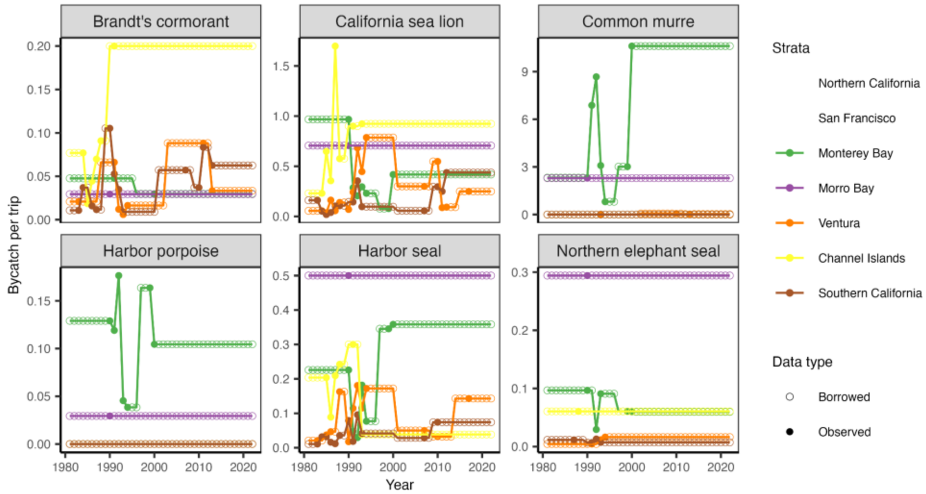


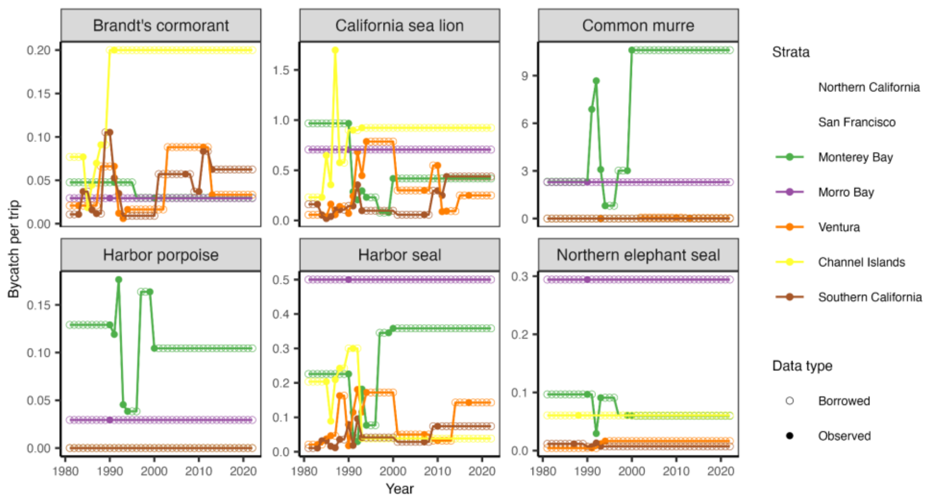
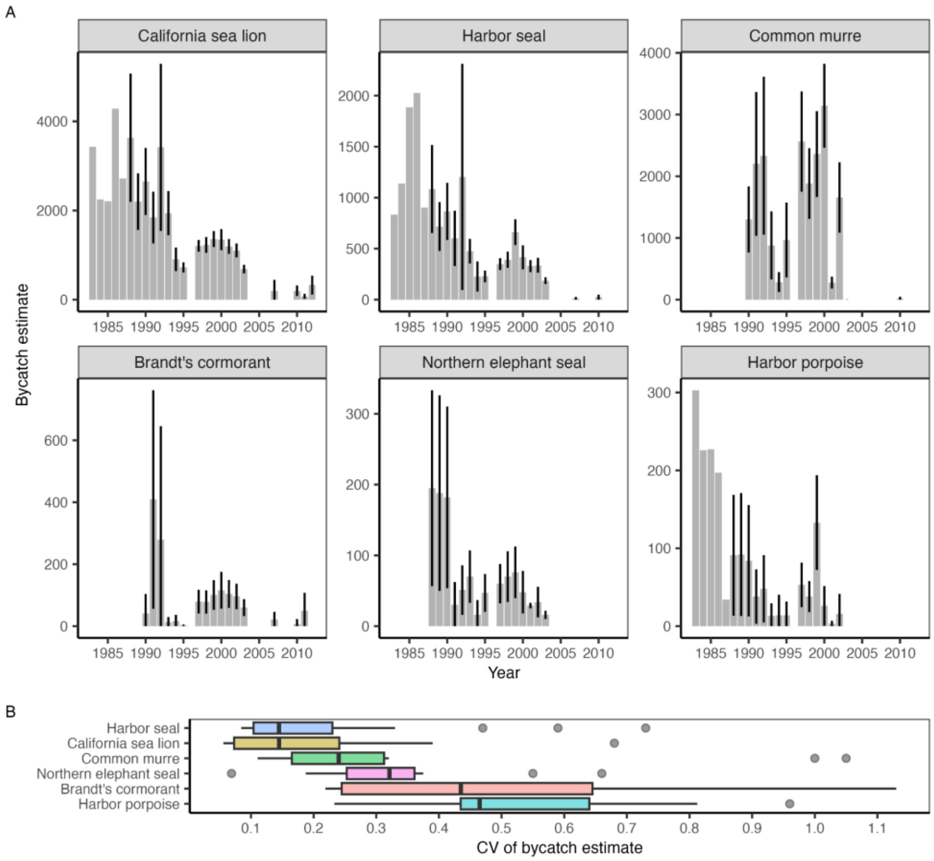


Fig. S9. Annual bycatch rates by species and regional strata as assumed in the ratio estimation analysis. Solid circles indicate years with observer data and open circles indicate years whose bycatch rates are borrowed from the closest year with observer data. See **Figs. 1A and S8** for maps of the regional strata.


A
California sea lion
Harbor seal
Common murre
Brandt's cormorant
Northern elephant seal
Harbor porpoise
Bycatch estimate
Year
B
Harbor seal
California sea lion
Common murre
Northern elephant seal
Brandt's cormorant
Harbor porpoise
CV of bycatch estimate

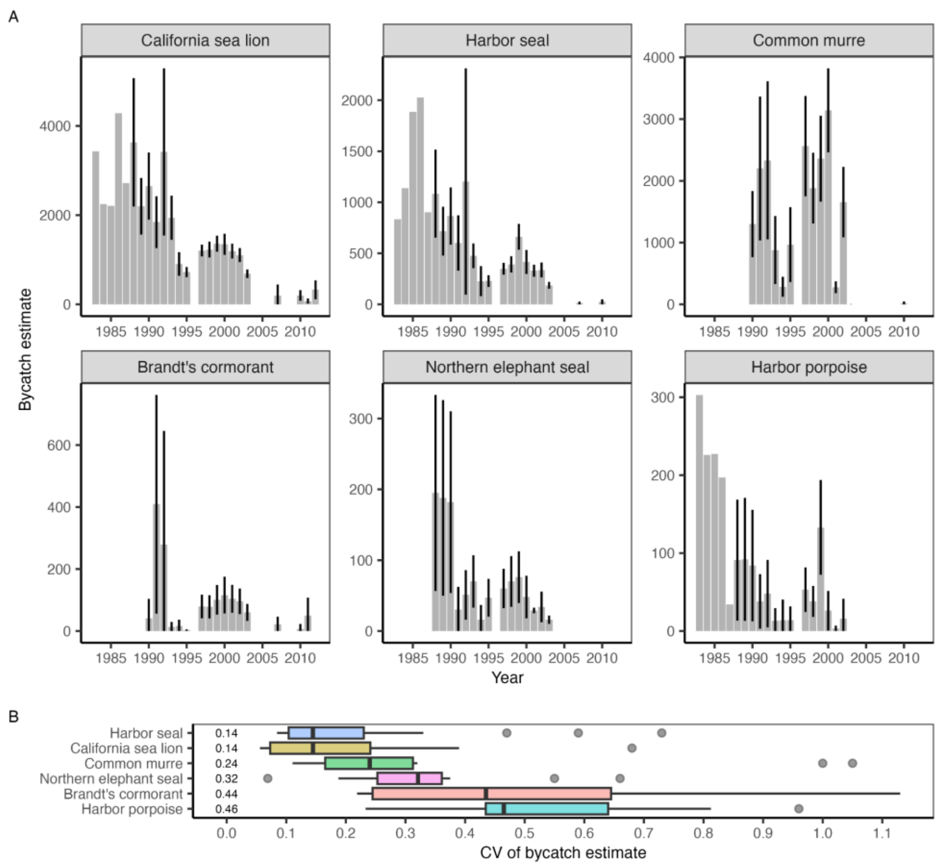


Fig. S10. Estimates of **(A)** annual bycatch from historical studies and **(B)** the uncertainty of these estimates expressed as the coefficient of variation (CV). In **(A)**, error bars indicate 95% confidence intervals. See **Table ~~1~~S1** for the sources of these estimates. In **(B)**, the median CV of the bycatch estimates for each species is printed on the far left. In the boxplots, the solid line indicates the median, the box indicates the interquartile range (IQR; 25th to 75th percentiles), the whiskers indicate 1.5 times the IQR, and points indicate outliers.

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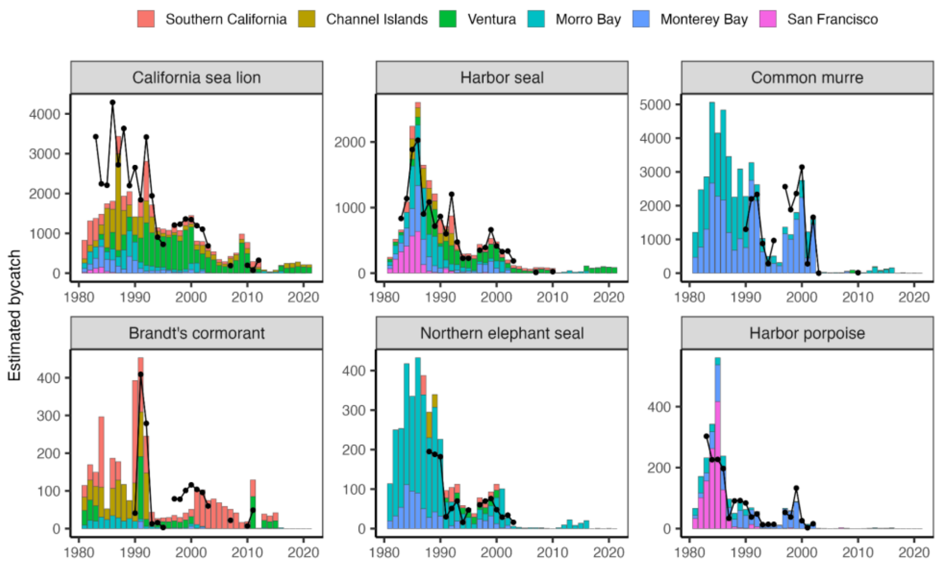

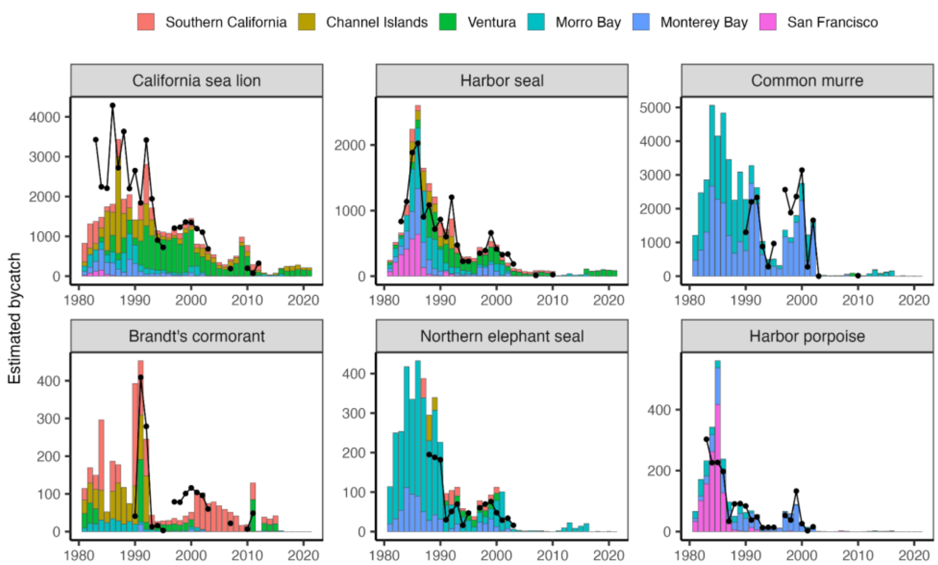

Fig. S11. A comparison of estimates of annual bycatch from our study (bars) and historical studies (points and lines). Potential reasons for these differences are explored in **Fig. S12**.

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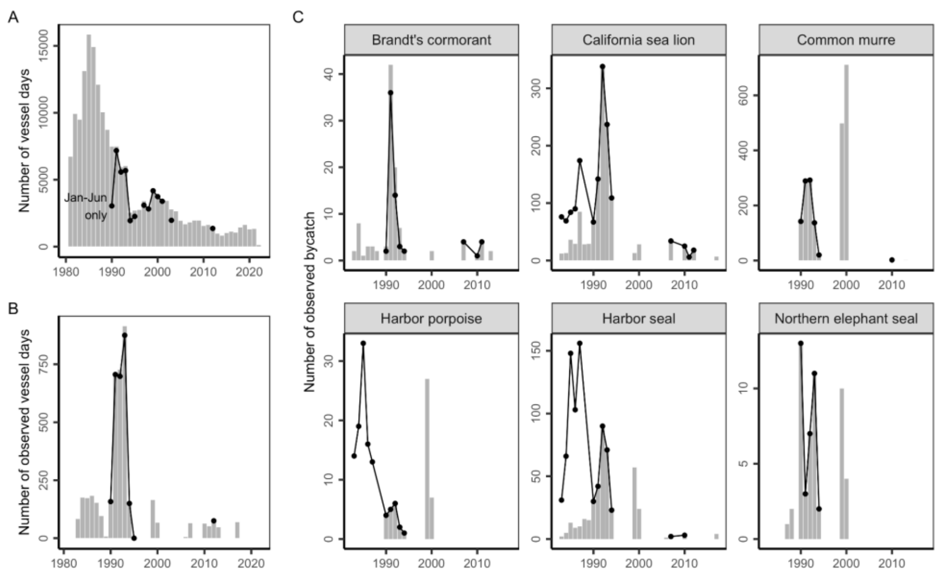


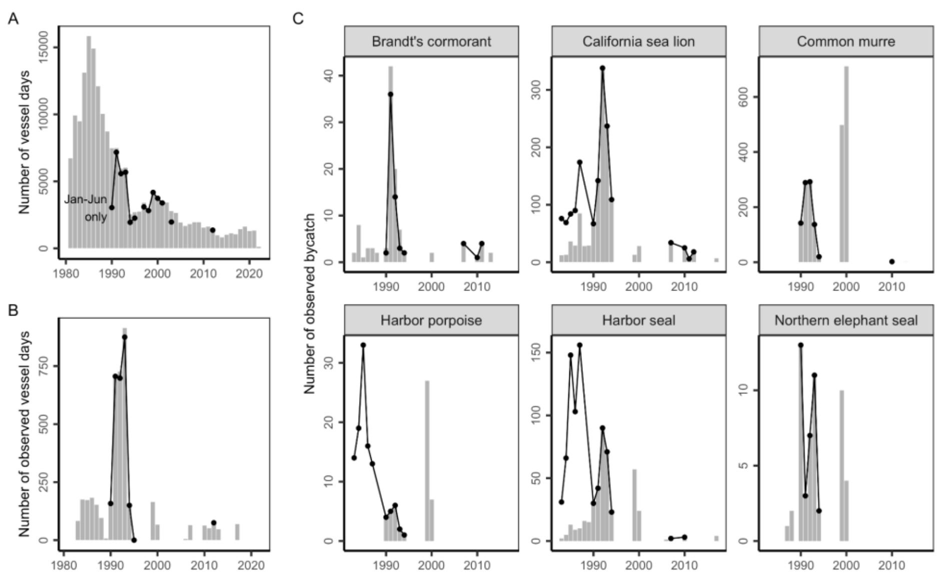
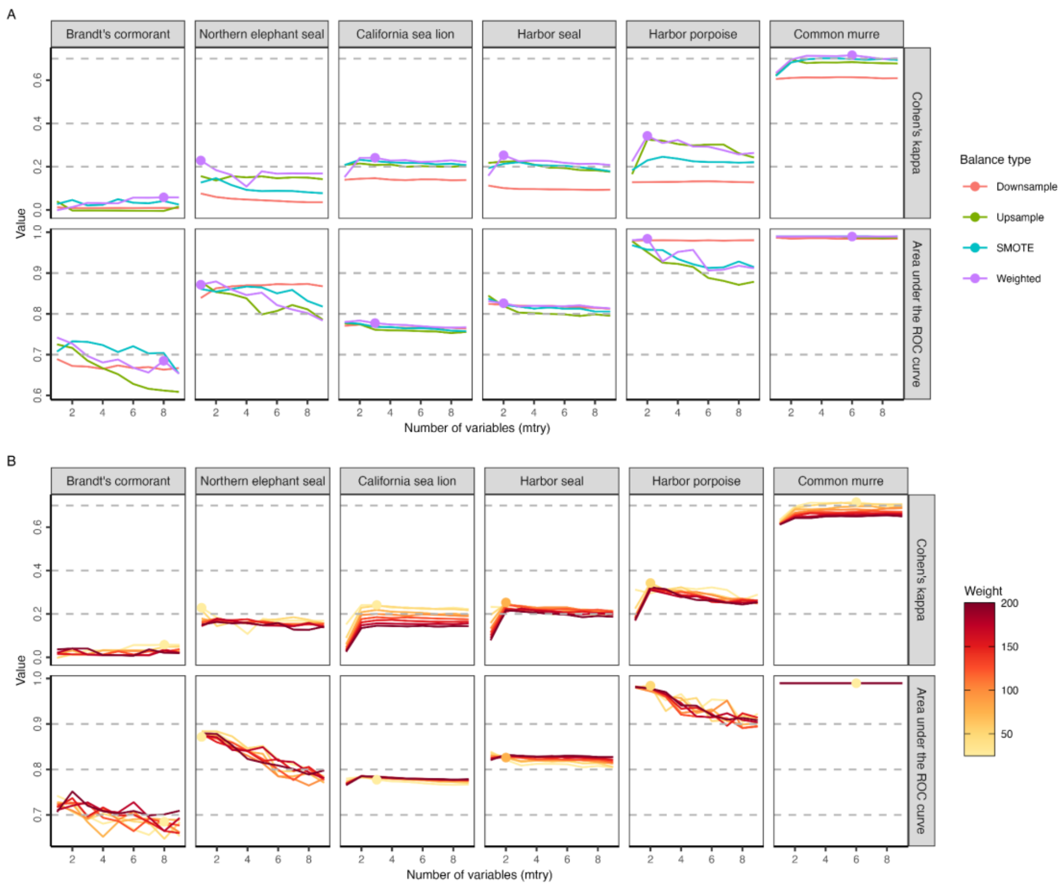


Fig. S12. A comparison of historical **(A)** fishing effort (number of vessel days); **(B)** observer coverage (number of observed vessel days); and **(C)** observer records (number of observed bycatch) derived in our analysis (bars) and reported in historical studies (points and lines). These time series represent the key inputs into the ratio estimation analysis and help to explain the differences between the estimates of annual bycatch derived in our study as compared to historical studies (see **Fig. S11**). See **Table ~~1~~S1** for additional details on historical studies.



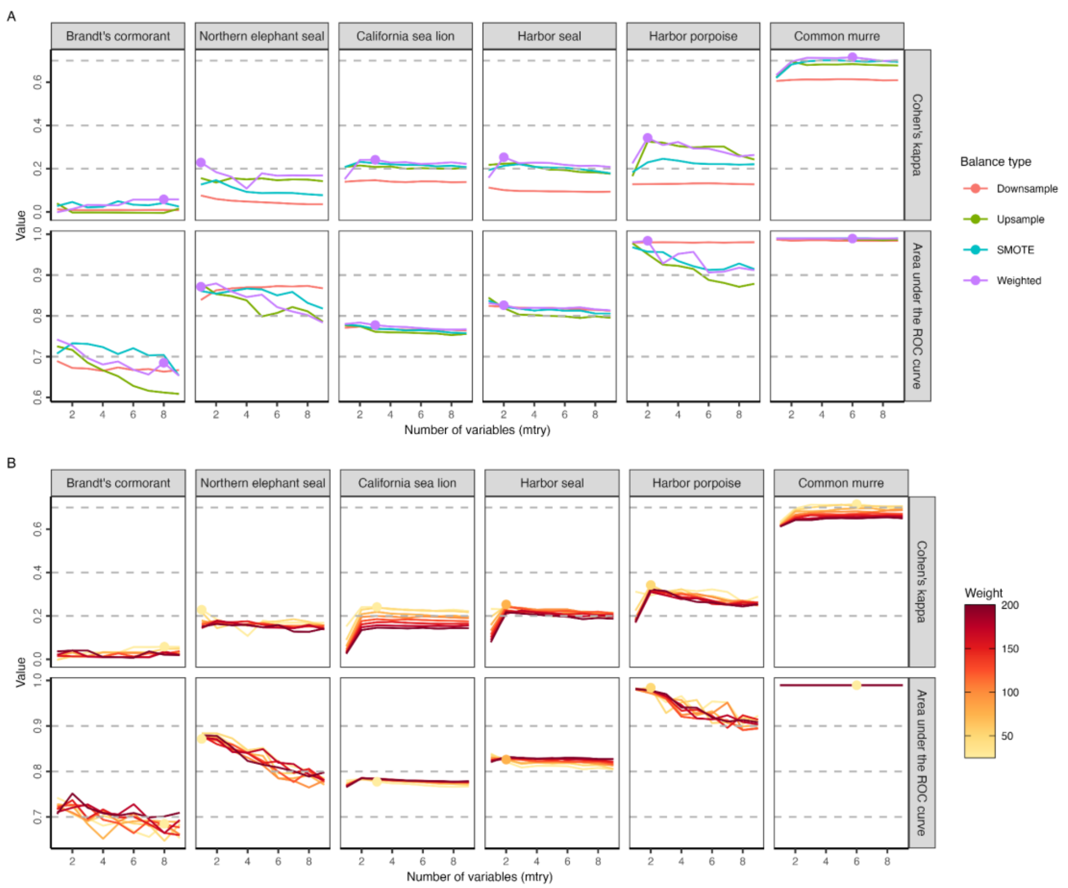
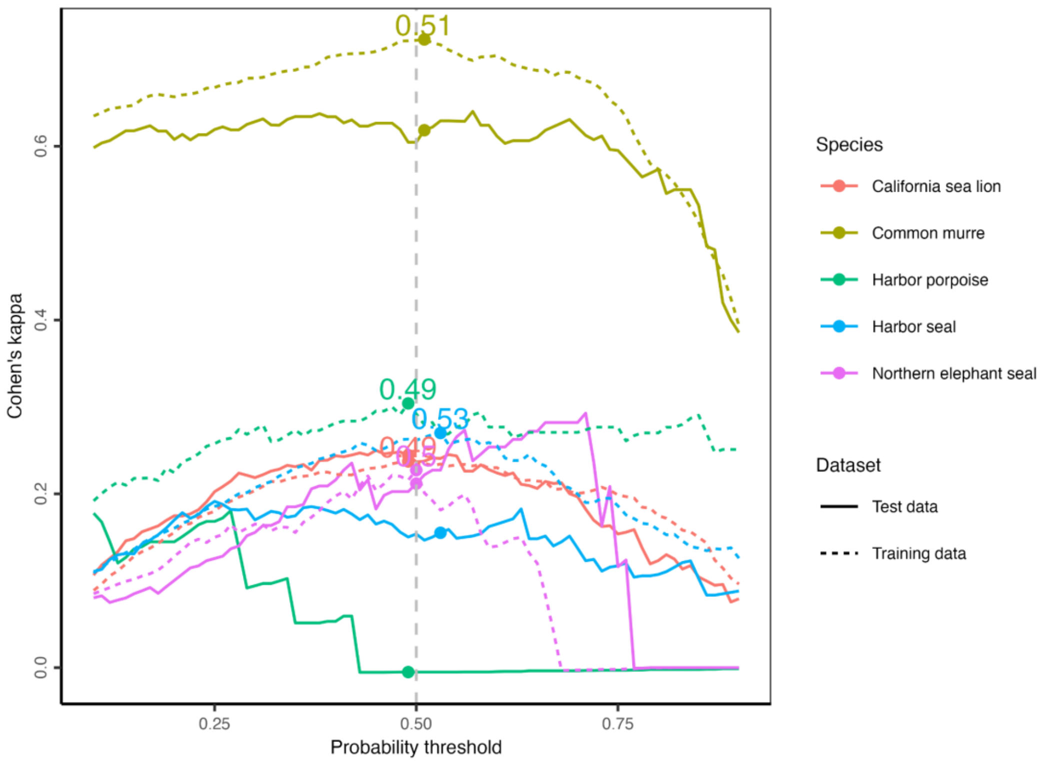


Fig. S13. A comparison of the model performance as measured through cross-validation on the training dataset between (**A**) balanced random forest modeling approaches and (**B**) weighted random forest modeling approaches for all candidate bycatch species (i.e., observer records > 50). We evaluated the model performance using Cohen's kappa and area under the receiver operator curve (ROC) and selected the model with the highest Cohen's kappa as the best performing model (labeled in the plot). We excluded Brandt's cormorant in the model prediction as their Cohen's kappa (<0.1) is too low to produce reliable predictions.



0.51
0.49
0.53
0.49
Cohen's kappa
0.0
0.2
0.4
0.6
0.25
0.50
0.75
Probability threshold
Species
California sea lion
Common murre
Harbor porpoise
Harbor seal
Northern elephant seal
Dataset
Test data
Training data

~~**Fig. S14.**~~

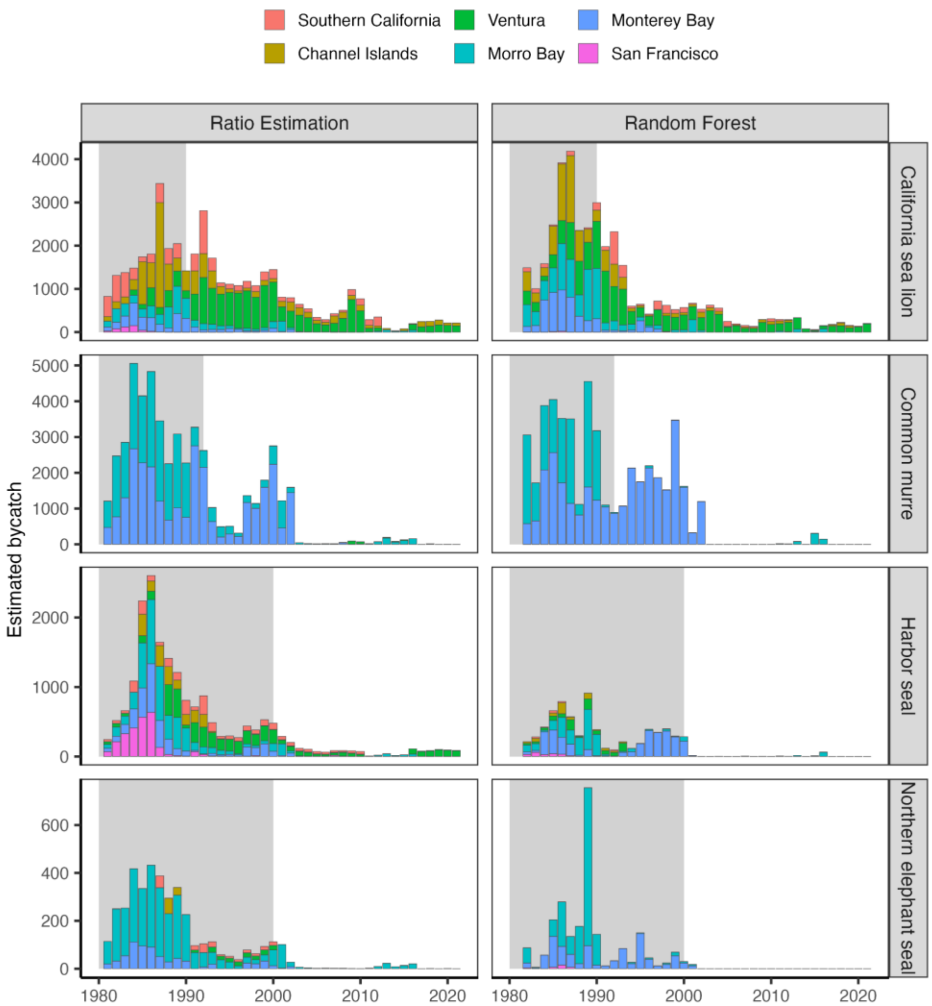
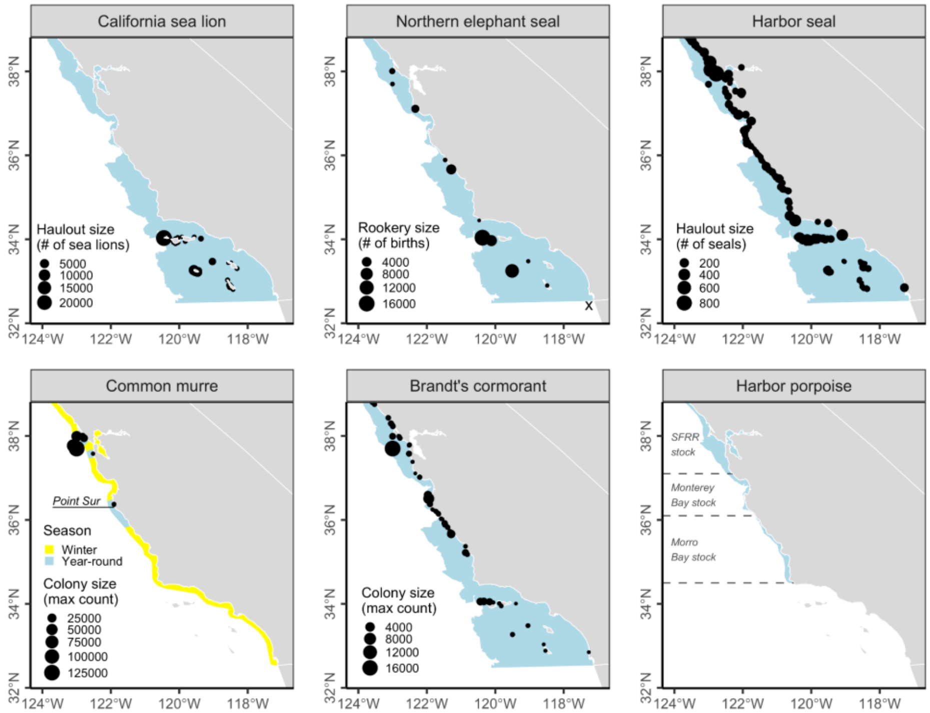


Fig. S14. A comparison of estimated bycatch numbers between ratio estimation and random forest stratified by regions (**Fig. S8**). The major differences between years are highlighted in gray.



~~Illustration of the methods used to select a probability threshold for classifying a logged set as having or not having bycatch. We selected the probability threshold that maximizes Cohen's kappa when applied to the training data as the optimal threshold (labeled in plot). We highlight the performance of this threshold when used on the independent test data to illustrate performance on out-of-sample data.~~

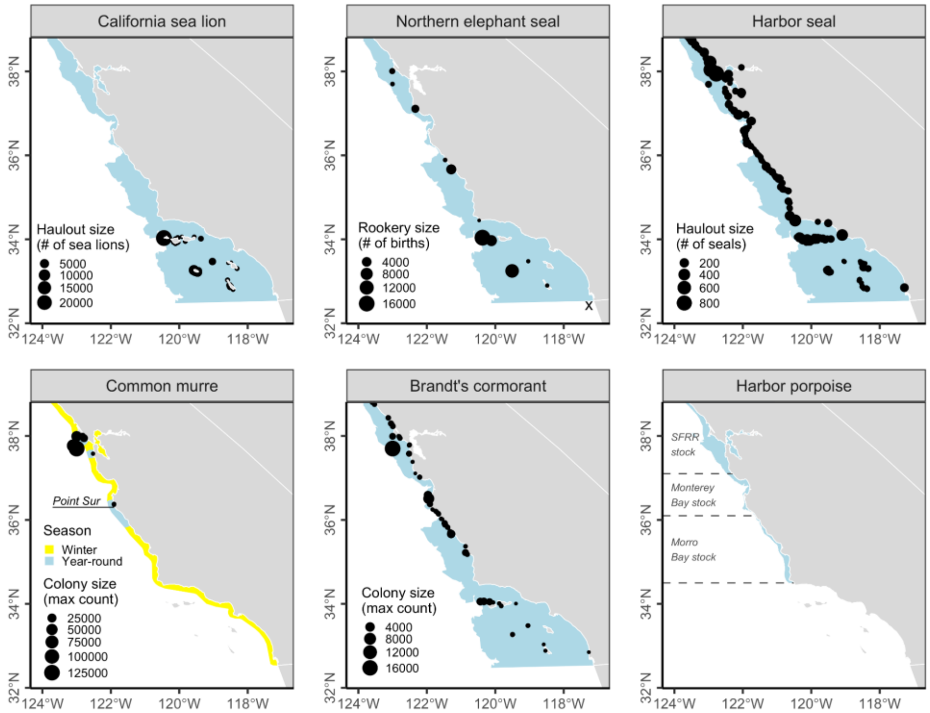


Fig. S15. Ranges of the six study species and information on haulout and colony size for selected species. Blue colors indicate year-round ranges and yellow colors (common murre only) indicate winter ranges. The range maps for all species except harbor porpoise are from the California Wildlife Habitat Relationship System ~~(CDFW, 2021).~~(CDFW, 2021). Harbor porpoises occur in waters less than 50 fathoms (92 meters) deep north of Point Conception ~~(Forney et al., 2014)~~(Forney et al., 2014). SFRR indicates the San Francisco-Russian River harbor porpoise stock. Common murre and Brandt's cormorant colony counts are from the California Seabird Colony Database ~~(CDFW, 2010).~~(CDFW, 2010). There are no historical breeding records for common murre south of Point Sur except for at the Prince Island colony in Cuyler Harbor of San Miguel Island. Harbor seal haulout counts are from the CDFW Harbor Seal GIS dataset (CDFW, 2014).

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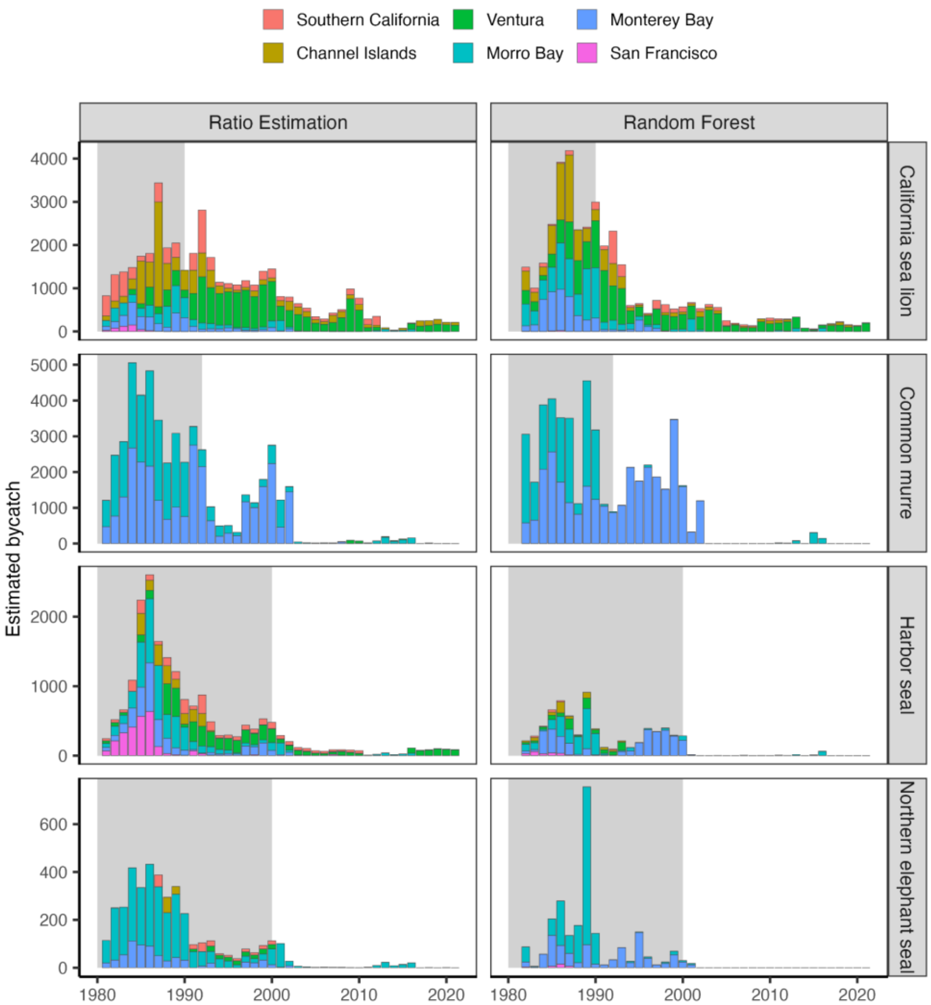


1580

1581 **~~Fig. S16.~~**

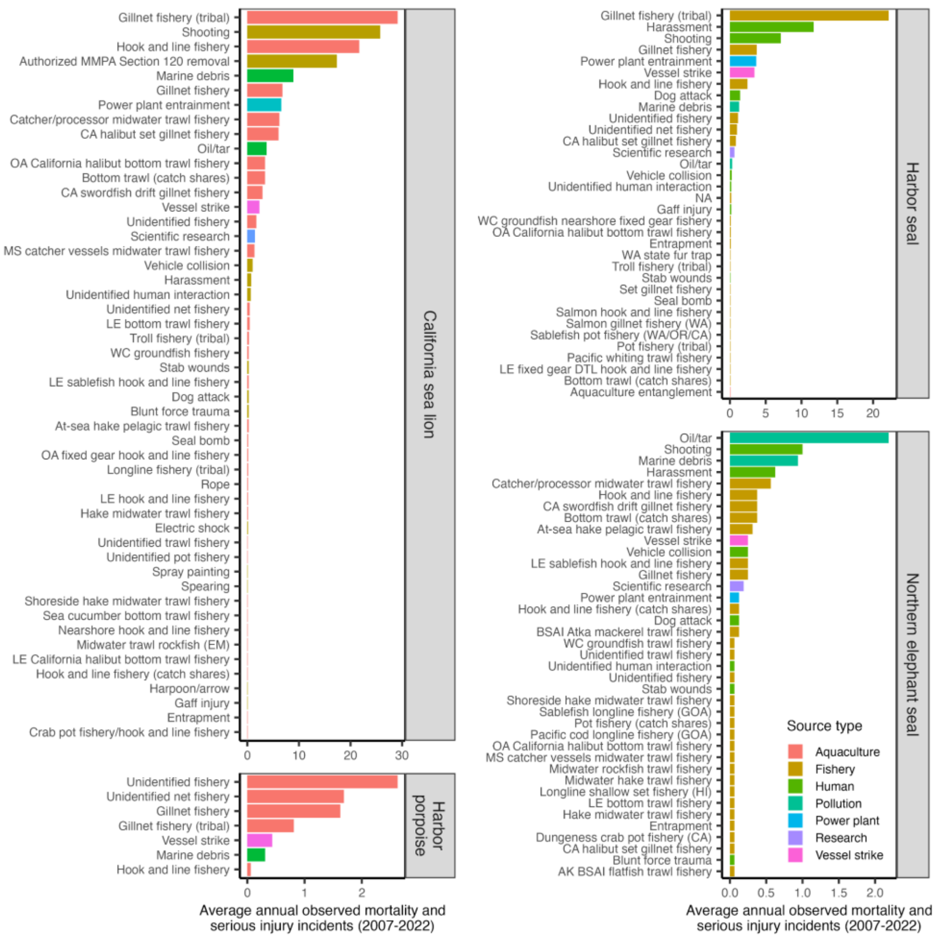
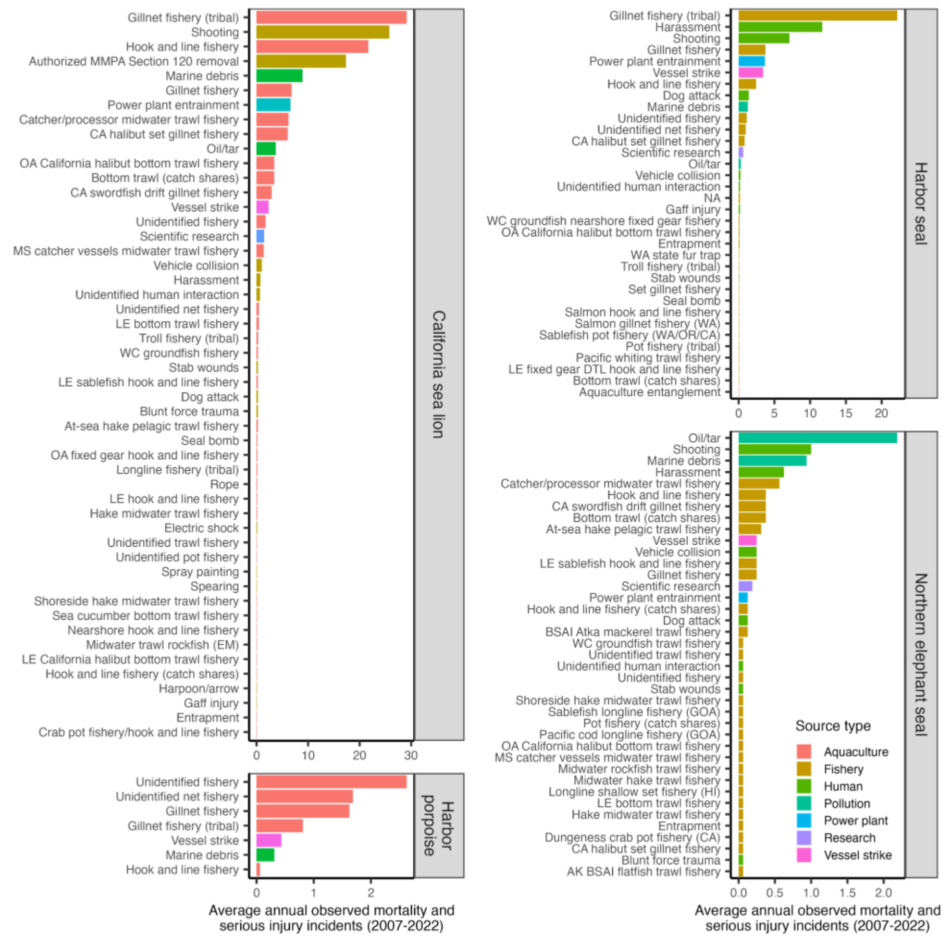


Fig. S16. ~~A comparison of estimated bycatch numbers between ratio estimation and random forest stratified by regions (**Fig. S8**). The major differences between years are highlighted in gray.~~



~~Fig. S17.~~ Average annual observed mortality and serious injury incidents by source on the entire U.S. West Coast between 2007-2022 ~~(Carretta, 2023).~~(Carretta, 2023).

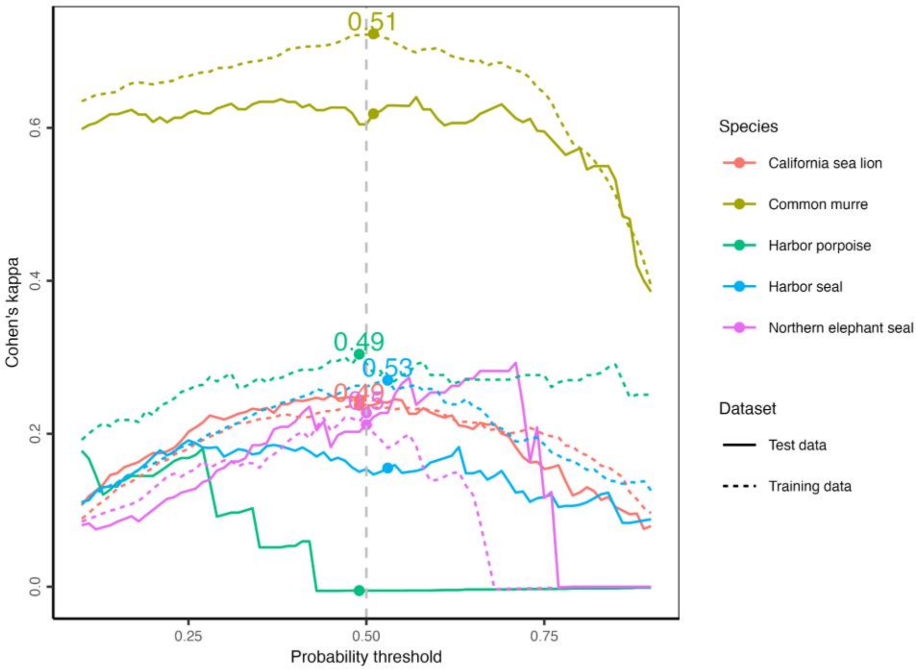


Fig. S17. Illustration of the methods used to select a probability threshold for classifying a logged set as having or not having bycatch. We selected the probability threshold that maximizes Cohen's kappa when applied to the training data as the optimal threshold (labeled in plot). We highlight the performance of this threshold when used on the independent test data to illustrate performance on out-of-sample data.

Estimates and drivers of protected species bycatch in the California set gillnet fishery

Abstract

The identification of efficient management strategies that reduce protected species bycatch while also minimizing impacts on fishing livelihoods is a global conservation challenge. Identifying such strategies requires understanding levels of bycatch relative to management targets as well as the relationship between bycatch risk and potential management levers. In this study, we use ratio estimators to reconstruct bycatch of select marine mammal and seabird species in the California ≥3.5” set gillnet fishery from 1981-2022 and random forest models to identify drivers and hotspots of bycatch risk. We find that bycatch has dropped precipitously since the 1980s as a result of management, but at significant costs to fisheries participation and revenues. Recent marine mammal bycatch ranges from 0.1% to 4.0% of the potential biological removal and marine mammal populations are recovering. Spatial-temporal drivers of bycatch risk were more important than fishing-related drivers of risk, suggesting that spatial-temporal closures would be more effective than mesh size or soak time restrictions at limiting bycatch. For each species, we identified 2-5 hotspots of elevated bycatch risk as candidates for temporary seasonal closures. Bycatch risk for harbor seal (*Phoca vitulina*) and California sea lion (*Zalophus californianus*), the species with the greatest bycatch risk, is especially high from April 1st to June 15th, suggesting that hotspot closures during this 2.5-month time period could be particularly efficient. Our study also highlights the value of competing multiple modeling approaches to identify methods that best predict rare bycatch events.

Keywords: gillnet, bycatch, marine mammals, seabirds, area-based management

1. Introduction

Bycatch, the accidental capture of non-target species in fisheries, presents a significant conservation and economic challenge (Crowder and Murawski, 1998; Soykan et al., 2008). Bycatch of large-bodied, slow-growing, low-productivity species such as marine mammals, sea turtles, and seabirds (Crowder and Murawski, 1998; Read et al., 2006; Soykan et al., 2008) is of particularly high concern, as the mortality of just a few individuals in these vulnerable populations, often recovering from historical exploitation, can threaten population collapse and even extinction (Geijer and Read, 2013; Read et al., 2006). As a result, many countries have established strict mandates to limit bycatch of vulnerable species, which can result in fisheries closures and other severe restrictions (Crowder and Murawski, 1998; Senko et al., 2014). These management disruptions can have serious social, cultural, and economic impacts on fishing communities (Senko et al., 2014). Due to the negative ecological, economic, and social consequences of fishery bycatch, bycatch avoidance is an important objective for global fishery management. A sustainably managed fishery with low bycatch can not only provide ecological benefits, but also social and economic benefits by providing a sustainable source of income, food, and nutrition.

To guide effective bycatch reduction policies, it is important to understand the magnitude of historical and recent bycatch as well as the drivers of bycatch in a fishery. Estimates of total bycatch are needed to determine whether bycatch exceeds management targets or is on pace to exceed targets in the near future (Bjørge et al., 2013; Geijer and Read, 2013; Read et al., 2006). Historical bycatch estimates offer insights into the effectiveness of past management interventions, which provide useful benchmarks for adapting management in response to recent bycatch levels and trends. Understanding the drivers of bycatch risk is critical to guiding effective and efficient management adaptations. For example, determining whether bycatch is concentrated within specific areas or seasons can support the design of time-area closures that prevent fishing when and where risk is high while maintaining fishing opportunities elsewhere (Lewison et al., 2014; O'Keefe et al., 2023; Soykan et al., 2008). Similarly, gear, soak time, or time of day restrictions can be used to curb bycatch if there are strong relationships between bycatch risk and gear or other characteristics of fishing (O'Keefe et al., 2023). Without this information, bycatch management must be precautionary to guarantee compliance with protected species legislation, which could forego considerable fisheries yields.

Because observer programs, which place trained scientists on fishing vessels to collect bycatch data, are costly and rarely cover all fishing trips, various analytical approaches have been developed to estimate unobserved bycatch and to evaluate drivers of bycatch risk. Ratio estimation, a design-based approach that assumes that the rate of bycatch in observed fishing trips is proportional to the rate for all fishing trips, is one of the most common strategies (Cochran, 1977; Stock et al., 2019). The reliability of this simple approach increases if there are sufficient data to support estimates within meaningful spatial or temporal strata (e.g., regions, depth zones, seasons). However, ratio estimation can produce biased estimates if other factors (e.g., gear type, soak time, time of day) influence bycatch rates (ICES, 2007), if the observed trips are not representative of the unobserved trips, or if low sample sizes lead to spuriously low or high bycatch rates within a stratum (Martin et al., 2005; McCracken, 2004; Ortiz and Arocha, 2004; Rochet and Trenkel, 2005). Model-based approaches, which use either statistical (e.g., generalized linear models, generalized additive models) or machine learning (e.g., random forests, boosted regression trees) models to estimate bycatch, can overcome many of these limitations by incorporating a wider suite of covariates and by allowing for non-linear relationships and are generally thought to produce better bycatch estimates (Stock et al., 2019). Additionally, model-based approaches can support management by identifying drivers of bycatch risk and by predicting detailed hotspots of risk (Long et al., 2024; Lopez et al., 2024; Stock et al., 2019).

The California set gillnet fishery would benefit from updated bycatch estimation due to concerns about the fishery's impact on protected marine mammals, which have led some conservation organizations to call for the fishery's closure (Birch et al., 2023; Birch and Shester, 2023). The fishery occurs in southern California and targets California halibut (*Paralichthys californicus*), white seabass (*Atractoscion nobilis*), and Pacific angel shark (*Squatina californica*), among other species. It is currently listed as a Category II fishery under the U.S. Marine Mammal Protection Act (MMPA), indicating that it presents a medium bycatch threat to protected marine species (NOAA, 2024). Bycatch of marine mammal and seabird species, including harbor porpoise (*Phocoena phocoena)*, southern sea otter *(Enhydra lutris nereis)*, and common murre *(Uria aalge)* was high during the 1980s and 1990s, prompting large-scale management interventions (Forney et al., 2001; Julian and Beeson, 1998). The fishery has also impacted pinniped species such as California sea lion (*Zalophus californianus*), harbor seal (*Phoca vitulina*), and northern elephant seal (*Mirounga angustirostris*). Total bycatch in the fishery has not been estimated since 2012 (Carretta et al., 2014) and some conservation groups are concerned that bycatch remains an issue (Birch et al., 2023; Birch and Shester, 2023).

A number of management actions have been taken to reduce bycatch in the California set gillnet fishery. During the 1980s, high bycatch of southern sea otters and common murres in central California (Barlow et al., 1994) led to a depth restriction that closed fishing inside of 40 fathoms (73 m) in 1987 (Forney et al., 2001). This restriction shut down the fishery in the San Francisco area, effectively pushing it south of Pigeon Point and into Monterey Bay and Morro Bay (**Fig. 1A-2**). In 1990, the state adopted

Proposition 132 (CA Secretary of State, 1990), which went into effect in 1994 and banned the fishery in mainland state waters (0-3 nautical miles) and in waters within 1 nautical mile or 70 fathoms of depth, whichever is less restrictive, around the Channel Islands to further reduce bycatch of protected species (FGC §8610.1-8610.16). In 2002, the state expanded the existing depth restriction, closing fishing inside of 60 fathoms (110 m) to avoid the harbor porpoise population in Central California (14 CCR §104.1). This effectively closed the fishery in Monterey Bay and Morro Bay (**Fig. 1A-4**). Currently, the fishery only operates in southern California (south of Point Arguello) outside 3 nautical miles from the mainland and outside 1 nautical mile or shallower than 70 fathoms (whichever is less) from the Channel Islands.

Although these regulations are believed to have reduced bycatch in the set gillnet fishery (Carretta et al., 2014), they have also greatly reduced fishery participation and revenues. The implementation of the 40 fathom depth restriction in 1987 triggered a precipitous decline in participation from ~400 vessels in 1987 to ~100 vessels in 1994. Since then, participation has continued to decline, with ~40 vessels active in 2022, and the vast majority (>90%) of landings coming from just 13 vessels (CDFW, 2023) (**Fig. 1B**). Fishing effort has similarly decreased from an estimated ~15,000 fishing trips in 1987 to 1,000 trips in 2022 (**Fig. 1C**). This reduction in effort has significantly reduced bycatch levels (Carretta et al., 2014) but at large costs to fishery revenues. Fleetwide revenues decreased from US$15 million in 1987 to US$1 million in 2022 (**Fig. 1C**; both values in 2022 dollars). Despite declining fishing effort and bycatch, conservation groups are lobbying for additional restrictions, including permanent closure, to further avoid bycatch (Birch et al., 2023; Birch and Shester, 2023). There is thus great need for scientific guidance on management regulations that are likely to provide conservation benefits while also avoiding unnecessary burdens on the fishing industry.

In this study, we reconstruct the bycatch of select marine mammals and seabirds in the California set gillnet fishery from 1981-2022 and identify drivers of bycatch that could be used to refine management to more efficiently reduce bycatch, where efficient management achieves conservation objectives while minimizing impacts on fishing opportunities. We focus on six protected species encountered in the set gillnet fishery: California sea lion, harbor seal, northern elephant seal, harbor porpoise, common murre, and Brandt’s cormorant (*Phalacrocorax penicillatus*). Southern sea otter, despite being one of the original species of management concern, is not included in this analysis due to data limitations (**Fig. 2A**). We use ratio estimation methods to reconstruct historical bycatch levels and compare recent bycatch levels to management targets. These methods, which have been used to estimate bycatch in the fishery at various points in the past (**Table S1**), provide a complete time series of bycatch estimates using methods approved for stock assessment and management. We then use random forest models to evaluate drivers of bycatch risk and to make predictions of spatial bycatch risk for four species of concern; Brandt's cormorant and harbor porpoise were excluded from this portion of the analysis due to poor model performance (**Table 1**). Based on these results, we make recommendations for how management could more efficiently manage bycatch risk through measures such as seasonal or spatial closures, depth restrictions, or gear restrictions.

2. Methods

2.1 Overview

We used a design-based ratio estimation approach to estimate bycatch of select marine mammal and seabird species in the California set gillnet fishery from 1981-2022. Briefly, the ratio estimation approach estimates total bycatch by applying the bycatch rates (i.e., bycatch per trip) on the sample of fishing trips with trained observers (*section 2.4.1*) to all fishing trips, which are documented in the logbooks maintained by all fishing vessels (*section 2.4.2*). Although model-based approaches, including the random forest approach used in the second portion of our analysis, are generally thought to produce better estimates of bycatch than design-based approaches (Stock et al., 2019)(see *section 2.6.4* and the supplemental information for more details)(Stock et al., 2019)evaluate drivers of bycatch and map hotspots of bycatch risk for four of the six evaluated species. We used a random forest approach, a machine learning method increasingly used in ecology, because of its high predictive skill for rare events, ability to model non-linear relationships, and insensitivity to collinear or unimportant predictor variables relative to classical regression techniques such as generalized linear models or generalized additive models (Cutler et al., 2007; Prasad et al., 2006). All analysis was done in R (R Core Team, 2024) and all code and non-confidential data are available on GitHub here: [add link post double-blind peer review].

2.2 Study area

Our study area spans southern and central California, U.S.A. from the U.S.-Mexico border (32°N) to Point Reyes (38°N), which is just north of San Francisco Bay (**Fig. 1A**). The study area falls within the California Current Ecosystem, a highly productive eastern boundary upwelling system spanning from the southern tip of the Baja Peninsula, Mexico (28°N) to the U.S.-Canada border (48°N). Seasonal upwelling of cold, nutrient-rich water fuels populations of krill, squid, sardines, and other low trophic species that are fed upon by larger fishes, sea turtles, seabirds, and marine mammals. The ecosystem also supports important commercial and recreational fisheries and other human uses that benefit tens of millions of people living along the U.S. West Coast. We assessed bycatch within seven regions of the study area, which are shown in **Fig. 1A** and explained in detail in *section 2.5*.

2.3 The fishery

We defined the fishery using the definition in the MMPA List of Fisheries (NOAA, 2023): the ≥3.5 inch mesh set gillnet fishery targeting California halibut, white seabass, Pacific angel shark, and other species. Although this definition deviates from historical studies, which frequently focused on the portion of the fishery using mesh sizes larger than 8.0 or 8.5 inches (**Table S1**), the MMPA definition provides the legal basis for bycatch management and is more consistent with historical regulations. Specifically, a minimum mesh size of 3.5 inches was set for white seabass in 1941, though it was increased to 6.0 inches in 1988 (FGC §8623(d)). Since 1989, California halibut and Pacific angel shark have been targeted using a minimum mesh size of 8.5 inches (FGC §8625(a)). The set gillnet fishery principally excluded by this definition is that for Pacific herring (*Clupea pallasii*), which occurs in California's four largest herring spawning areas — San Francisco Bay, Tomales Bay, Humboldt Bay, and Crescent City Harbor — using mesh sizes of 2.0 to 2.5 inches (CDFW, 2019).

2.4 Data

Our analysis relies on two fisheries-dependent datasets: logbook data and observer data. All gillnet vessels are required to submit logbooks documenting when, where, and how they fished and how much catch was retained. Thus, logbooks characterize all fishing trips. However, because logbooks are self-reported and reported discards are unverifiable, logbooks likely underreport discarded bycatch, especially the bycatch of protected species. As a result, information from observer programs, which place trained observers on a sample of fishing trips (0-16.7% in this fishery; **Fig. 2B; Table S2**), are required to inform estimates of bycatch for the unobserved trips recorded in vessel logbooks, which constitute the majority of fishing effort (83.3-100% in this fishery; **Fig. 2B; Table S2**). Thus, the estimation of total bycatch through ratio estimation depends on both the observer and logbook data. In contrast, the evaluation of bycatch drivers and hotspots with the random forest models uses only the observer data, as these are the only data to accurately record protected species bycatch when it occurs. These datasets are described in detail below.

2.4.1 Observer data

We received observer data from 1983 to 2017 from the California Department of Fish and Wildlife (CDFW). There was observer coverage in the California set gillnet fishery from 1983-1995 (coastwide), 1999-2000 (Monterey Bay area only), 2010-2013 (south of Point Conception only), and in 2017 (south of Point Conception only) (**Fig. 2**). The observer program was run by CDFW from 1983-1989 and by the National Oceanic and Atmospheric Administration (NOAA) from 1990 onwards. The percentage of annual fishing trips with onboard observers has varied over time, ranging from 0.3% of trips in 2006 to 16.7% of trips in 1993 (**Fig. 2B; Table S2**). Observers collected information on the amount and fate of catch (kept, discarded, or damaged), the length composition of the catch, the location and time of the catch, and characteristics of the gear used to target the catch (**Fig. S2**). We developed a series of simple assumptions to impute missing values for a few key variables (GPS coordinates, fishing depth, soak hour, mesh size) used to describe gillnet sets documented in the observer data (**Fig. S3**; see supplemental methods for details).

Although historical reports document low levels of observer coverage in Morro Bay, Monterey Bay, and San Francisco in the 1980s (**Table S1**), the data that we received from CDFW excluded most of these observations. We recovered a small portion of the missing raw data – observations from Monterey Bay from 1987-1989 (**Fig. 2**) – from original CDFW data sheets that were given to a colleague at the Southwest Fisheries Science Center during the late 1990s for a reanalysis of historical bycatch rates in that region (Forney et al., 2001). We extracted summaries of set-level bycatch rates from historical reports (**Table S1**) for years and regions missing raw data to support the ratio estimation analysis (**Table S3**). We converted set-level bycatch rates to trip-level bycatch rates assuming an average of 3 sets per trip (**Table S3**), as indicated by the observer data (**Fig. S4**). **Fig. 2C** illustrates the coverage of the available, recovered, and lost observer data.

For each species of marine mammal, seabird, and sea turtle documented in the observer data, we calculated the total number of captures observed in the California set gillnet fishery (**Fig. 2**). Throughout this analysis, we focus on the six species with more than 50 observations: common murre (2,381), California sea lion (1,372), harbor seal (519), Brandt's cormorant (118), northern elephant seal (78), and harbor porpoise (97). Unfortunately, this excluded southern sea otter, which was a species of significant conservation concern in the 1980s, but whose bycatch was only documented in the lost observer data.

2.4.2 Logbook data

We received logbook data from the commercial gillnet fishery from 1981 to 2022 from CDFW (**Fig. 1**). All California gillnet vessels are required to submit logbooks documenting all of their fishing trips; as a result, these logbooks represent all fishing effort associated with the fishery. These data describe vessel information (vessel name, unique identifier, permit number); when (date), where (statistical reporting block; **Fig. 1A**), and how long (hours) a vessel fished; what fish it targeted; what type of gear it used (drift or set gillnet) and characteristics of this gear (length, mesh size, fishing depth; **Fig. S2**); what species it caught; and the amount (number and/or weight) and fate of this catch (kept, released, or lost, including the identity of predators preying on released fish). We attempted to identify individual fishing sets within the logbook data as the unique combination of vessel administration information (vessel name, either vessel id or boat number, permit number), where, when, and how long a vessel fished (block id, date, and fishing hours), and characteristics of the gear (net length, mesh size, and fishing depth). This analysis revealed an average of 1 set per trip, which is inconsistent with the 3 sets per trip documented in the more accurate observer data. We term these unique identifiers "pseudo-sets" and view them as roughly equivalent to a fishing trip (**Fig. S4**). We developed a series of simple assumptions to impute missing or unrealistic values for a few key variables (fishing depth, soak hour, mesh size) used to describe gillnet pseudo-sets documented in the logbook data (**Fig. S5 & S6;** see supplemental methods for details).

2.4.3 Sea surface temperature

Because sea surface temperature (SST) is a common driver of the distributions of both target and bycatch species (Hazen et al., 2018), we used SST as an environmental covariate in the random forest models described below. We derived the SST associated with each set documented in the observer and trip documented in the logbook data using the NOAA 1/4° Daily Optimum Interpolation Sea Surface Temperature (OISST) dataset, which interpolates observations from different monitoring platforms (e.g., satellites, ships, buoys, and Argo floats) to provide a globally complete grid of SST from September 1, 1981 to present (Huang et al., 2021). For sets reported in the observer data, we extracted the SST at the reported GPS location on the reported day of fishing. For trips reported in logbooks, we calculated the average SST in the reported block on the reported day of fishing.

2.5 Ratio estimation

We estimated annual bycatch for each study species using ratio estimators. Ratio estimators assume that the rate of bycatch in observed fishing trips is proportional to the rate of bycatch within all fishing trips within a given stratum (Cochran, 1977). This assumption requires that the characteristics of observed trips do not systematically differ from the characteristics of all trips, which was confirmed by a two-sided Kolmogorov-Smirnov test for six key traits (i.e., day of year, depth, latitude, mesh size, distance from shore, soak time) (**Fig. S7)**. We used trips rather than sets as the sampling unit given the inability to identify unique sets in the logbook data (**Fig. S4B**). This is valid because the number of gillnet sets per fishing trip (median: 3 sets/trip; interquartile range: 2-4 sets/trip) has been consistent through time

(**Fig. S4A**). Under this approach, the bycatch rate for species *s* in stratum *i* ($r_{s,i}$) – where, in this case, strata are defined by years and regions (see next paragraph) – is thus calculated as:

$$r_{s,i} = \frac{k_{s,i}}{d_{s,i}}$$

Where $k_{s,i}$ is the total number of individuals of species *s* captured in observed trips occurring in strata *i* and $d_{s,i}$ is the total number of observed trips occurring in strata *i*. The total estimate of bycatch of species *s* in strata *i* ($m_{s,i}$) is then calculated by multiplying the bycatch rate ($r_{s,i}$) by the total number of trips to have occurred in the strata (D_i):

$$m_{s,i} = D_i * r_{s,i}$$

Where the total number of trips (D_i) is derived from the logbook data.

We calculated annual bycatch estimates using a seven-region stratification scheme (**Figs. 1A, S8**). This stratification scheme combines the scheme by Diamond and Hanan (1986) and Julian (1993) for areas north and south of Point Conception, respectively. Although early efforts to estimate bycatch in the California set gillnet fishery often stratified estimates by region and season (**Table S1**), later efforts found that observer coverage was often too limited to employ complex temporal stratification and that estimates between temporally stratified and unstratified approaches were generally similar (**Table S1**). Stratum-specific bycatch rates for years without observer coverage in the stratum are borrowed from the closest year (forwards or backwards) with observer coverage in the stratum (**Fig. 2C & S9)**, as has been the practice in previous studies. We collated annual bycatch estimates from past studies (**Table S1**) for comparison with our updated estimates (**Figs. S10-S12**).

Although there are methods for estimating the uncertainty of bycatch estimates generated through ratio estimation (Julian and Beeson, 1998), we were unable to implement these methods because they rely on bootstrap procedures that sample from the bycatch rates of observed trips. Because these procedures require raw observer data, we cannot use them for (1) years where summary values from historical reports are used because the raw data have been lost or (2) years without observer data from within one of the fished strata. As a result, only 6 of the 42 evaluated years had the data required to estimate uncertainty: 2006, 2007, 2010, 2011, 2012, and 2017 (**Fig. 1AC; Fig. 2BC**). An exploration of the uncertainty estimates generated in historical reports with access to the lost data (**Table S1**) suggests that the median coefficient of variation for estimates of annual bycatch estimates ranges from a low of 0.14 for harbor seal to a high 0.47 for harbor porpoise (**Fig. S10B**).

We evaluated the sustainability of recent estimated marine mammal bycatch by comparing it to the potential biological removal (PBR) for each stock, which is defined under the MMPA as the maximum number of animals, not including natural mortalities, that may be removed from a marine mammal stock while allowing that stock to reach or maintain its maximum sustainable population. We extracted each PBR from its most recent stock assessment (**Fig. 7**) and compared it to the average estimated catch over the last 10 years (2013-2022). A fishery is managed based on its classification into one of three categories: Category 1 fisheries cause annual mortality and serious injury (M/SI) greater than 50% of the PBR, Category II fisheries cause annual M/SI between 1 and 50% of the PBR, and Category III fisheries cause annual M/SI less than 1% of the PBR. A fishery is considered to be approaching the MMPA's "zero mortality rate goal" (ZMRG) when annual M/SI is below 10% of the PBR.

2.6 Random forest modeling

2.6.1 Model training

We used random forest classification models trained on the observer data to identify drivers of bycatch risk for each of the six study species. We used a random forest approach, a machine learning method that ensembles predictions from hundreds of decision trees, rather than a classical regression method (e.g., generalized linear or additive models) because of their comparatively high predictive skill for rare events, ability to model non-linear relationships, and insensitivity to collinear or unimportant predictor variables (Cutler et al., 2007; Prasad et al., 2006). We considered nine attributes of fishing as potential drivers of bycatch risk: haul depth (fathoms), mesh size (inches), soak time (hours), latitude (°N), longitude (°W), distance from shore (km), Julian day, sea surface temperature (°C), and whether the fishing occurs near an island (i.e., within 10 km of island coast). These attributes were selected based on their demonstrated relationship to bycatch risk in other papers (e.g., (Bettoli and Scholten, 2006; Bjørge et al., 2013; Kroetz et al., 2020)) and their availability in the observer data or their ability to be derived through remote sensing (i.e., distance from shore, temperature, island area). They represent a range of spatial (latitude, longitude, distance from shore, depth, island area), temporal (Julian day), environmental (temperature), and fishing-related (soak time, mesh size) attributes. For each species, we classified an observed set as having (1) or not having (0) bycatch, and trained a classification model assuming a Bernoulli distribution in the response variable.

Because bycatch of protected species is rare, observed fishing sets show strong class imbalance towards sets without bycatch compared to sets with bycatch. To illustrate, the percent of observed sets with bycatch is as follows: California sea lion (1.02%), common murre (0.43%), harbor seal (0.43%), Brandt's cormorant (0.09%), northern elephant seal (0.076%), and harbor porpoise (0.074%). Therefore, without a proper sample balancing method, predictions are likely to be biased towards the majority class (sets without bycatch), leading to an underestimation of bycatch risk. For this reason, we considered four approaches for accounting for class imbalance resulting from bycatch rarity.

The first three approaches employ different sample balancing methods: (1) downsampling, (2) upsampling, and (3) synthetic minority over-sampling (SMOTE), which uses a mixture of down and upsampling (More and Rana, 2017). The downsampling approach randomly removes observations of the majority group (sets without bycatch) to obtain equal representation of the majority and minority (sets with bycatch) group. The upsampling approach randomly samples observations from the minority group with replacement to obtain equal representation with the majority group. The synthetic minority over-sampling (SMOTE) approach both up-samples the minority group and down-samples the majority group. It up-samples the minority class by synthesizing new cases from its nearest five neighbors and down-samples the majority class by randomly drawing samples from that group. We created each balanced dataset using the *themis* package in R (Hvitfeldt, 2023) and fit random forest models to these datasets using the *randomForests* package in R (Liaw and Wiener, 2002).

The fourth approach employs weighted random forests, which use observation weighting rather than sample balancing to elevate the importance of the minority class. In this "cost-sensitive" learning approach (More and Rana, 2017), higher weights are assigned to minority observations so that the model receives a higher penalty for misclassifying these observations, helping to reduce bias towards the majority class. We evaluated multiple weighting schemes to optimize the predictive skill of this approach. Specifically, we assigned majority observations (sets without bycatch) a weight of 1 and assigned minority observations (sets with bycatch) weights of 25 to 200 in increments of 25. Thus, a total of eight candidate weighted random forest models were evaluated as described below. We fit the weighted random forest model using the *ranger* package in R (Wright and Ziegler, 2017).

We trained each of the eleven candidate models (three balanced random forest models, eight weighted random forest models) on 80% of the observer data, withholding the remaining 20% for model testing. In training the models, we performed a grid search to identify the "mtry" hyperparameter – the number of variables to randomly sample as candidates at each node split – that maximizes Cohen's kappa under 10-fold cross validation (**Fig. S13**). While accuracy measures the proportion of correctly classified categorizations, Cohen's kappa measures the proportion of correct classifications while accounting for the probability of being correct by chance and is a better measure of predictive skill, especially for imbalanced datasets (Cohen, 1968). Although there are no definitive rules for interpreting Cohen's kappa, general guidelines suggest that values above 0.7 are "excellent", 0.4-0.7 are "good", 0.2-0.4 are "fair", and below 0.2 are "poor" (Fleiss et al., 2013; Landis and Koch, 1977). We identified the best fitting model as the model generating the highest Cohen's kappa on the training data. We applied this model to the test dataset for an independent evaluation of its predictive power. We only evaluated four species (California sea lion, common murre, harbor seal and northern elephant seal) whose best models exhibited "fair" or better performance in their training dataset and close to "fair" performance on the test dataset for the rest of the analysis (**Table 1**).

2.6.2 Model evaluation

We evaluated the drivers of bycatch risk for each species by inspecting the variable importance and the marginal effects of each variable as estimated in the best fitting model. Variable importance was evaluated as the total decrease in node impurities from splitting on the variable averaged over all trees. The impurity measure is corrected when building the model to reduce its bias towards continuous variables (Nembrini et al., 2018). Marginal effects measure the impact of the changes in one variable on the response variable while all other variables are held constant. The marginal effects plots provide the scientific basis for our discussions of management regulations that could effectively and efficiently reduce bycatch risk.

2.6.3 Mapping spatial bycatch risk

To generate maps of spatial bycatch risk, we used the best fitting model to predict risk to a 0.02° grid spanning southern California (32-35°N and 117-121°W), the only area where the fishery can operate under current regulations. Conceptually, our metric of spatial bycatch risk represents the probability of bycatch at a given location under recent average conditions. We derived this metric by first predicting the probability of bycatch on every calendar day (Julian day 1 to 365) for each grid cell. The nine input variables for making these predictions were derived as follows: (1) *latitude*, (2) *longitude*, (3) *distance from shore*, (4) *depth*, and (5) *island proximity* (i.e., whether the location was within 10 km of an island) were derived based on the centroid of the grid cell; (6) *soak time* was set to 24 hours, the logbook mode; (7) *mesh size* was set to 8.5 inches, the logbook mode; (8) *Julian day* was the input day; (9) *sea surface temperature* (SST) was the average SST for that day and grid cell from 2010-2022. We then calculated the weighted average of the daily risk weighting by the amount of logbook entries (a metric of fishing effort) occurring on that Julian day from 2010-2022. We cropped the predictions to areas within 20 km of shore as this is the farthest offshore that the fishery has operated (**Fig. S7**). We delineated bycatch hotspots as areas falling within the 95th (California sea lion, harbor seal) or 99th (common murre, northern elephant seal) percentile contour of spatial risk for each species. A higher threshold was used for common murre and northern elephant seal because they exhibit a large number of very low risk cells.

2.6.4 Bycatch estimation

We explored using the random forests models to estimate annual bycatch (1981-2022) in the fishery but found them to be unsuitable for this specific case study. Although model-based approaches generally perform better than design-based approaches at estimating bycatch (reference), we found that our random forest models underpredicted bycatch risk in the 1980s and 1990s relative to the ratio estimator (**Fig. S14**). This is most likely because of the loss of observer data from the northern strata during this time period, fishery largely operated in the northern portion of the region. See the supplemental information for details on the estimation of bycatch using the random forest models.

3. Results

3.1 Ratio estimation

In general, estimated bycatch peaked in the mid-1980s, steadily declined following the 40-fathom depth restriction implemented in 1987, with a temporary increase in the late-1990s followed by continued decline (**Fig. 3**). Bycatch peaked, in order of decreasing magnitude, at 5,059 common murre in 1984, 3,437 California sea lion in 1987, 2,605 harbor seal in 1986, 560 harbor porpoise in 1985, 453 Brandt's cormorant in 1991, and 432 northern elephant seal in 1986 (**Fig. 3**). This pattern reflects trends in fishing effort, which also declined after a peak in 1985, with a brief expansion in the late-1990s followed by continued decline (**Fig. 1C**). Estimated bycatch of harbor porpoise, northern elephant seal, and common murre declined especially sharply following the 2002 exclusion of fishing from waters shallower than 60 fathoms. Slight differences between our estimates of annual bycatch and those from historical studies (**Fig. S11**) are driven by a mixture of differences in our methods and input data (**Fig. S12**).

The sustainability of recent estimated marine mammal bycatch was evaluated as a percentage of the potential biological removal (PBR) of each stock. Based on this sustainability reference point, bycatch concerns, in order of decreasing threat, are as follows: harbor seal (65 individuals per year = 4.0% of a PBR of 1,641 individuals), harbor porpoise (1 individual per year = 1.5% of a PBR of 65 individuals in the Morro Bay stock, where the bycatch took place), California sea lion (194 individuals per year = 1.4% of a PBR of 14,011 individuals), and northern elephant seal (7 individuals per year = 0.1% of a PBR of 5,122 individuals) (**Fig. 7**). The assessment that bycatch during the last 10 years poses the greatest risk to harbor seals is supported by the fact that the harbor seal stock size has been stable or declining in recent years while all of the other marine mammal populations have been undergoing sustained population growth (**Fig. 7**). The sustainability of estimated seabird bycatch is more difficult to evaluate given more limited population monitoring data (**Fig. 7**) and the lack of legally binding reference points for defining allowed incidental take. However, increasing Brandt's cormorant nests from 1980 to 2020 (**Fig. 7**) and steeply reduced bycatch of common murre (**Fig. 3**) suggests low risks posed to these species.

3.2 Random forest modeling

3.2.1 Model performance

The best-fitting model performed well for all species, except for Brandt's cormorant and harbor porpoise, which exhibited poor performance (Cohen's kappa less than 0.2) and were therefore excluded from further consideration. Weighted random forest models performed best for California sea lion, common murre, northern elephant seal, and harbor seal with case weights of 25, 25, 25, and 75, respectively (**Table 1; Fig. S13**). For common murre, Cohen's kappa was 0.71, indicating "good" performance, while for harbor seal, California sea lion, and northern elephant seal, Cohen's kappa was 0.25, 0.24, and 0.23, respectively, indicating "fair" performance. Model performance was positively correlated with the frequency of bycatch observations, i.e., species with more observed bycatch events produced models with greater skill (**Table 1**; $r^2 = 0.64$ for Cohen's kappa for training data). Cohen's kappa was positively correlated with the area under the receiver operator curve (AUC) ($r^2 = 0.68$ for the training dataset), indicating minimal tradeoffs in using this metric for model selection (**Table 1**).

3.2.2 Drivers of bycatch risk

The importance of the evaluated explanatory variables in determining bycatch risk varied by species but some general patterns emerged **(Fig. 4**). In general, spatial (latitude, longitude, depth, and distance from shore) and temporal (Julian day) variables were more influential than variables associated with the environment (sea surface temperature) or the fishing methodology (soak time, mesh size). Whether fishing occurred close to an island (a spatial variable) was the exception, as it was consistently the least important variable. Sea surface temperature, which is closely related to space and time, was generally more important than soak time and always more important than mesh size.

The species exhibit a mixture of similar and dissimilar responses to the explanatory variables (**Fig. 5**). California sea lion and harbor seal exhibit similar responses in bycatch risk. Both species have higher bycatch risk in shallower depths in nearshore areas with a spike in risk occurring around 34°N latitude, including some deeper offshore areas (**Fig. 6**). They also exhibited a pronounced increase in risk during the spring, lasting approximately from Apr 1 (90th day of the year) to June 15 (166th day of the year). They are infrequently caught in nets with mesh sizes smaller than 8.5 inches, though the use of such nets is rare (**Fig. S5E**). Variability in bycatch risk for common murre and northern elephant seal is most strongly determined by latitude and longitude (**Fig. 5**), with the only area of elevated risk in contemporary fishing grounds occurring just north of Point Conception (**Fig. 6**). For all four species, bycatch risk exhibits an asymptotic relationship with soak time, though the shapes of these relationships differ by species. The species exhibit complex and variable relationships to temperature.

3.2.3 Maps of bycatch risk

The species exhibited different patterns of spatial bycatch risk. California sea lion bycatch risk is predicted to be highest in four areas: (1) on the northern coasts of the northern Channel Islands, especially on the northern coast of Santa Rosa; (2) a small nearshore area west of Santa Barbara; (3) the eastern coast of Santa Cruz Island; and (4) the northwestern shores of Santa Catalina and San Clemente Islands; (**Fig. 6**). Harbor seal bycatch risk is predicted to be highest in four areas: (1) the sliver of nearshore area stretching from Santa Barbara to Point Sal; (2) the eastern coasts of Santa Cruz Island; (3) a broad coastal area near Point Mugu; and (4) the sliver of nearshore area stretching from Point Mugu to Point Vicente (**Fig. 6**). Common murre bycatch risk is predicted to be negligible throughout most of southern California (**Fig. 6**). It is only predicted to be high in a small patch near Point Sal and even there, the maximum risk index is much lower than for the other evaluated species. Like common murre, northern elephant seal bycatch risk is also predicted to be negligible throughout most of southern California except in the region near Point Sal (**Fig.6**).

4. Discussion

Our study provides the first update to total estimates of protected species bycatch in the California set gillnet fishery since 2012. We find that bycatch, once high and unsustainable for some species (Forney et al., 2021, 2001), is now well below the “zero mortality rate goal” (ZMRG) of 10% of the potential biological removal (50 C.F.R. § 229.2). Recent marine mammal bycatch estimates range from 0.1-4.0% of their potential biological removals and common murre bycatch has been effectively eliminated. All of the evaluated populations, including the once declining and heavily depleted Morro

Bay harbor porpoise population, are growing or stable. These advances, while directly attributable to management interventions, are more due to reductions in fishing effort (i.e., fewer fishing trips) than to reductions in bycatch rates (i.e., lower bycatch per fishing trip). This highlights a steep tradeoff between conservation and fisheries objectives under the current management regime: while populations of protected species have undergone sustained growth, fishing opportunities and revenues have undergone prolonged declines. Despite this, there have been calls for more bycatch-motivated restrictions to the fishery (Birch et al., 2023; Birch and Shester, 2023). Our results indicate that current fishing operations do not pose a threat to the evaluated species, which suggests that current management is sufficient at limiting bycatch. However, targeting management toward spatial-temporal bycatch hotspots could improve economic outcomes while keeping bycatch low.

Our results suggest that spatial-temporal management could more efficiently and effectively manage bycatch risk than gear modifications or soak time regulations. Specifically, bycatch rates for California sea lions and harbor seals are greatest from April 1 to June 15, suggesting that a 2.5 month seasonal closure of bycatch hotspots for these two species could prevent bycatch while allowing the opening of less risky but currently closed areas to fishing. These hotspots are predicted to disproportionately contribute to bycatch yet are of minor fishing importance (**Fig. 6B**) suggesting that the loss of fishing opportunities in these areas during brief seasonal closures could be easily made up by opening areas of low predicted bycatch risk. As a result, seasonal closures could broaden fishing opportunities while continuing to meet bycatch avoidance objectives. However, we caution that such hotspot closures could exacerbate bycatch problems if fishing effort is displaced and concentrated in areas of secondarily high risk (Free et al., 2023). Therefore, monitoring of fishing effort and bycatch rates are important for verifying that seasonal closures achieve their conservation and fisheries objectives. Additionally, any changes in current management strategies must take into account spatial-temporal patterns of bycatch and relative sensitivity of each species.

Hotspots of bycatch risk are aligned with the location of known haulouts, breeding colonies, and foraging grounds. High California seal lion bycatch risk around the northern Channel Islands is likely related to the large haulouts of sea lions in that area (**Fig. S15**). Similarly, hotspots of harbor seal bycatch risk correspond to the locations of large harbor seal haulouts on Santa Cruz Island and near Point Mugu (**Fig. S15**). The absence of common murre bycatch risk in southern California is consistent with the distribution of the species, which has no breeding colonies or permanent foraging grounds in southern California (**Fig. S15**). Similarly, the negligible risk for northern elephant seals is consistent with the phenology of their migrations. Although northern elephant seals breed on the Channel Islands and near

San Simeon/Cambria (**Fig. S15**) from December to March, they disperse to their distant foraging grounds (males to Alaska and females to oceanic waters far West of California) before the fishing season peaks from April to June, significantly reducing their vulnerability to the gillnet fishery. Finally, the elimination of harbor porpoise bycatch is consistent with the confinement of the fishery to southern California where harbor porpoise do not occur (**Fig. S15**).

The completion and safekeeping of accurate fisheries-dependent data is fundamental to producing accurate bycatch assessments and effective management strategies. While summaries of historical data facilitated reliable bycatch estimates through ratio estimation, the loss of raw observer data from the 1980s likely impeded our ability to accurately estimate bycatch in the northern strata using random forests, an approach often thought to be more accurate than ratio estimation (Stock et al., 2019). Notably, the lost data document a period when fishing was allowed in shallower, more inshore, and more northern waters (**Fig. 1A**). Recovering this data would enhance our ability to assess the drivers of bycatch in the northern region and could provide insights for re-evaluating previous management strategies. Furthermore, missing meta-data on critical gear characteristics (e.g., mesh size, net length, net height, net material; **Fig. S3**) in the available observer data also limited our ability to identify the potential for these management levers to reduce bycatch risk. Ensuring the complete documentation of gear characteristics, perhaps by prioritizing characteristics known to impact bycatch risk in other gillnet fisheries (Northridge et al., 2017), is important to maximizing the utility of expensive, and sometimes controversial, observer programs (Suuronen and Gilman, 2020). Finally, the ability to delineate individual sets in the logbooks and improved documentation of the characteristics of logged sets would enhance future bycatch estimates by allowing sets to be the sampling unit and by avoiding assumptions about missing data, respectively. This could be achieved by redesigning logbooks, training fishers on completing logbooks, expanding electronic monitoring, and/or demonstrating that better data can actually lead to fewer restrictions.

Our results highlight the importance of considering multiple modeling approaches when estimating and evaluating rare bycatch events. Although model-based methods (e.g., random forests) for estimating bycatch are often preferred to sample-based methods (e.g., ratio estimators) (Stock et al., 2019), we find complementary value in using both approaches. While ratio estimation generated more reliable bycatch estimates due to its ability to leverage both raw and summarized data, the random forest model provided the empirical basis for assessing drivers of bycatch risk. Furthermore, our results highlight the value of considering multiple sample balancing approaches when evaluating bycatch using model-based methods, as the specification of the best performing model varied by species. Recent efforts to estimate bycatch in West Coast fisheries using random forests have used only a single sample balancing technique (e.g., Carretta, 2023); we encourage future efforts to compete multiple approaches to optimize estimates of rare bycatch events (More and Rana, 2017).

The continued recovery of protected species will require management of stressors besides fisheries bycatch, some of which may present even larger threats (Avila et al., 2018; Oldach et al., 2022). For example, in Summer 2023, there were over 1,000 statewide strandings of California sea lions and other pinnipeds attributed to domoic acid toxicosis resulting from an intense bloom of harmful diatoms in the *Pseudo-nitzschia* genus (SCCOOS, 2023; Smith et al., 2023). Harmful algal blooms are increasing in frequency, duration, and intensity on the West Coast (Hallegraeff et al., 2021) as a result of ocean warming and eutrophication (McKibben et al., 2017) suggesting that, in the long-term, curbing climate change and nutrient runoff may be the most important actions for stemming mortality for recovering pinniped populations. Furthermore, harassment and shooting are some of the most commonly observed sources of mortality and serious injury for California sea lions, harbor seals, and northern elephant seals (Carretta, 2023) (**Fig. S16**), suggesting the need for greater outreach and enforcement to prevent these gratuitous forms of mortality. Finally, it is important to understand the bycatch contributions of other fisheries, many of which report higher levels of *observed* bycatch (**Fig. S16)** yet are not as heavily prosecuted as the California set gillnet fishery. Modeling studies similar to this one are needed to determine whether higher apparent bycatch in these fisheries is due to higher observer coverage or higher bycatch rates. However, the sustained recovery of the evaluated populations suggests that total bycatch across all fisheries is low.

There will always be tradeoffs between maximizing fishing opportunities and minimizing bycatch of protected species (Samhouri et al., 2021). As a result, managers often seek to implement regulations that maximize fishing outcomes while keeping bycatch below legally defined sustainable reference points (Kirby and Ward, 2014). The identification of such strategies is seldom straightforward and depends on substantial investments in data and scientific enterprises. Notably, they depend on monitoring populations of protected species to support the assessment of their status and levels of allowable incidental take and monitoring bycatch in key fisheries to support assessments of total bycatch, drivers of bycatch, and the effectiveness of past management interventions (Kirby and Ward, 2014; Punt et al., 2021). In the absence of such data, management must often be precautionary to ensure compliance with protected species legislation (Punt et al., 2021). We illustrate the potential return on investment of supporting such scientific enterprises as our results show that past management interventions have been successful at reducing bycatch in the California set gillnet fishery well below target levels, opening the door for more efficient restrictions and negating the need for unnecessary precaution. The continued demonstration that monitoring programs can generate better outcomes for businesses could facilitate increased public support and funding to identify win-win scenarios for fisheries and conservation in more fisheries.

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Tables & Figures

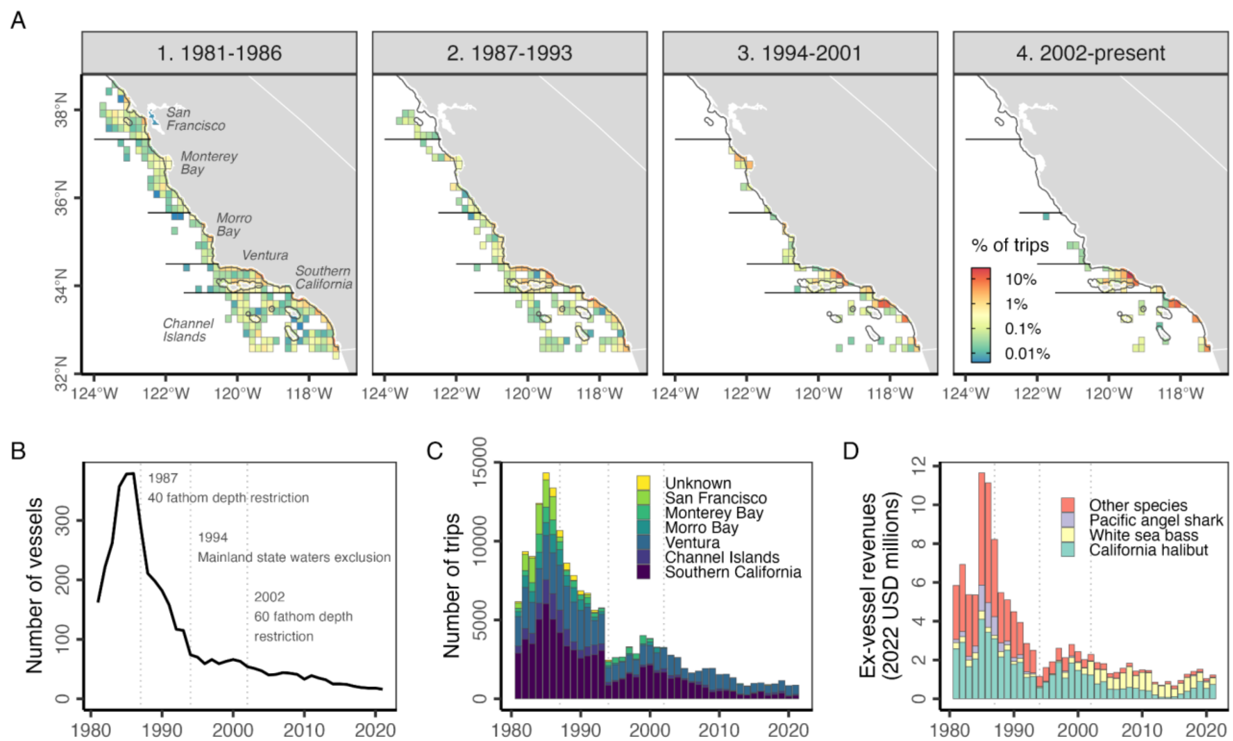

Fig. 1. History of the California ≥3.5” set gillnet fishery. Panel **A** shows the spatial history of fishing effort during four regulatory periods. Trips are reported by the 10 x 10 minute (~18 x 18 km) statistical blocks used for fisheries catch and effort reporting. The horizontal lines delineate geographical strata used in the ratio estimation analysis; strata are labeled in the first plot. The thin coastal line marks state waters (less than 3 nautical miles from the coast). Blocks visited by fewer than three vessels during each regulatory period are hidden to maintain confidentiality and comply with the “rule-of-three.” The other panels show time series of fisheries **(B)** participation, **(C)** effort, and **(D)** revenues. Vertical lines mark years in which major regulations, labeled in Panel B, were implemented; these define the regulatory periods used in Panel **A**. These regulations became operative on April 15, 1987; January 1, 1994; and April 26, 2002. See **Fig. S1** and the supplemental methods for details on estimating ex-vessel revenues from the fishery.

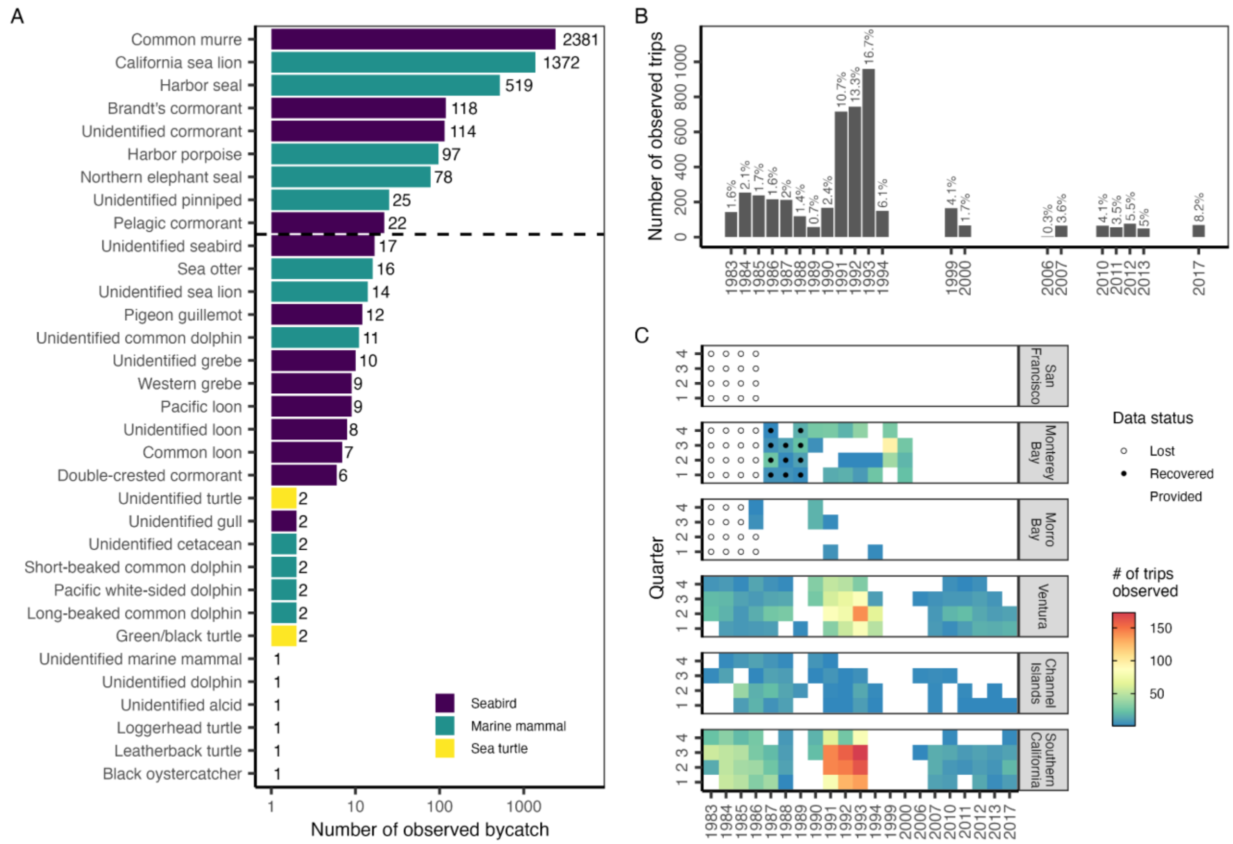

Fig. 2. History of observer coverage in the California ≥3.5" set gillnet fishery. Panel **A** shows the bycatch of marine mammals, seabirds, and sea turtles recorded by observers from 1983-2017. We focus on species with ≥50 observations, which are delineated by the horizontal dashed line. Note log-scale on x-axis. Panel **B** shows the number of observed trips (vessel-days) over time. The dark labels show the estimated percent of trips that were observed. Panel **C** shows the number of observed trips across the spatial (region) and temporal (quarters) strata considered in the ratio estimation analysis. See **Fig. S8** for a map of the spatial strata. Quarters are defined as: 1 = JFM (winter), 2 = AMJ (spring), 3 = JAS (summer), and 4 = OND (fall).

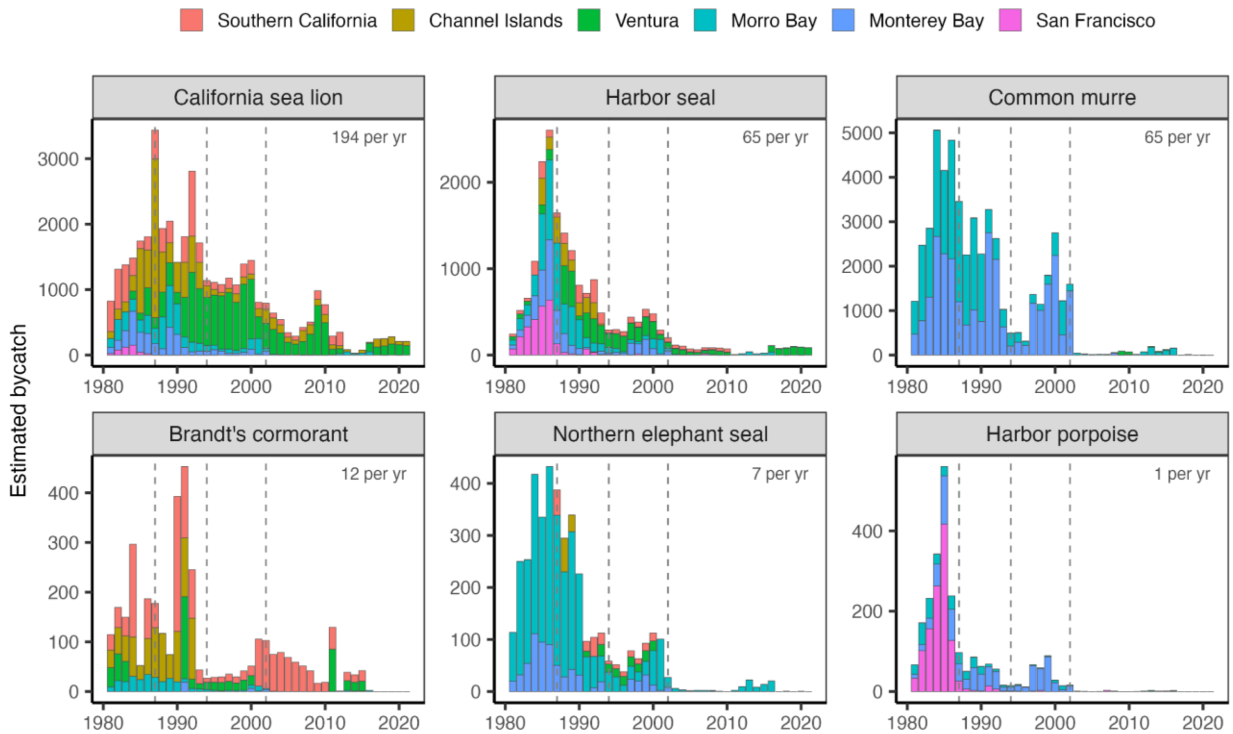

Fig. 3. Estimated bycatch in the California ≥3.5” set gillnet fishery from 1981-2021 predicted using the ratio estimation approach. Average estimated annual bycatch rates for the last 10 years (2012-2021) from the ratio estimator are marked in the top-right corner. Species are listed in order of decreasing recent bycatch rates.

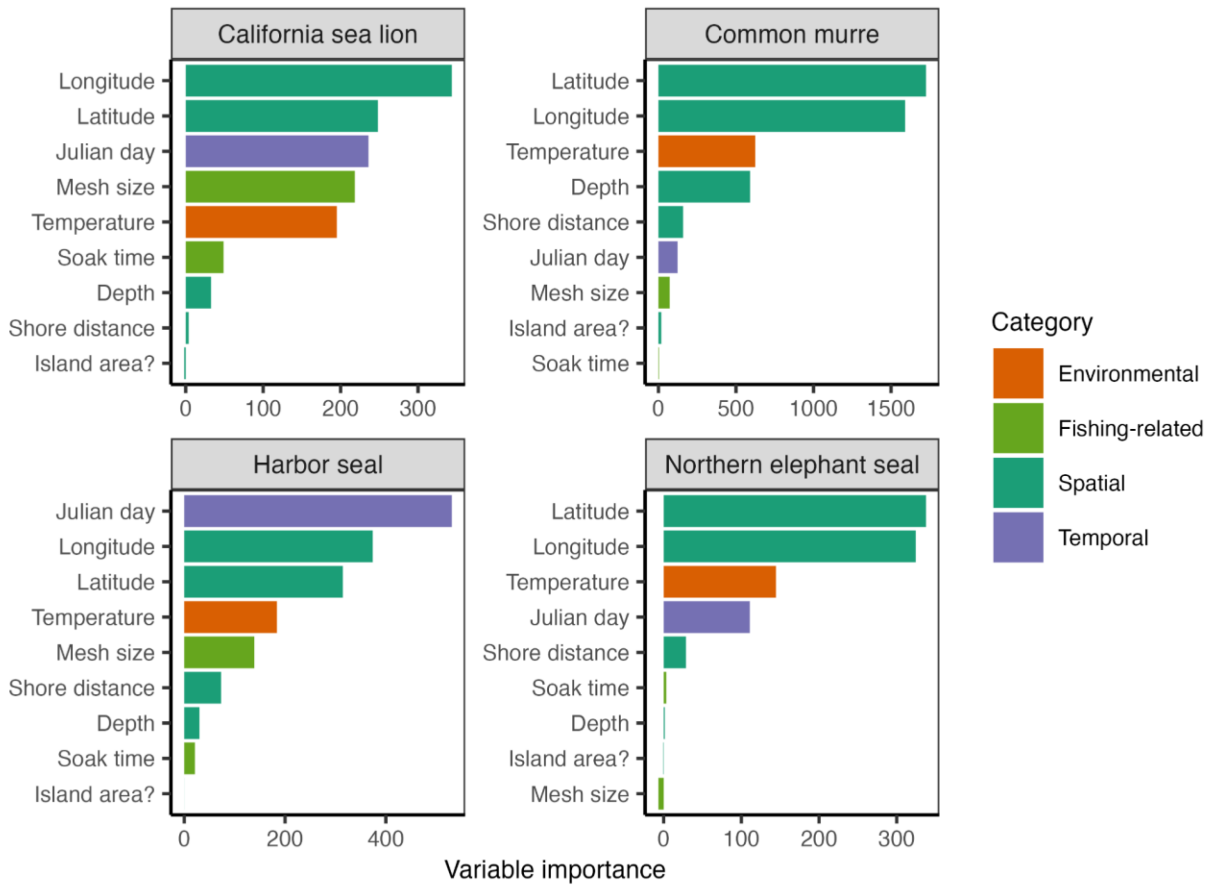

Fig. 4. Importance of the evaluated explanatory variables in the final random forest model for the study species with acceptable model performance. With the bias towards continuous variable corrected, variable importance is measured as the total decrease in node impurities from splitting on the variable averaged over all trees. Explanatory variables are colored based on the category of the variable.

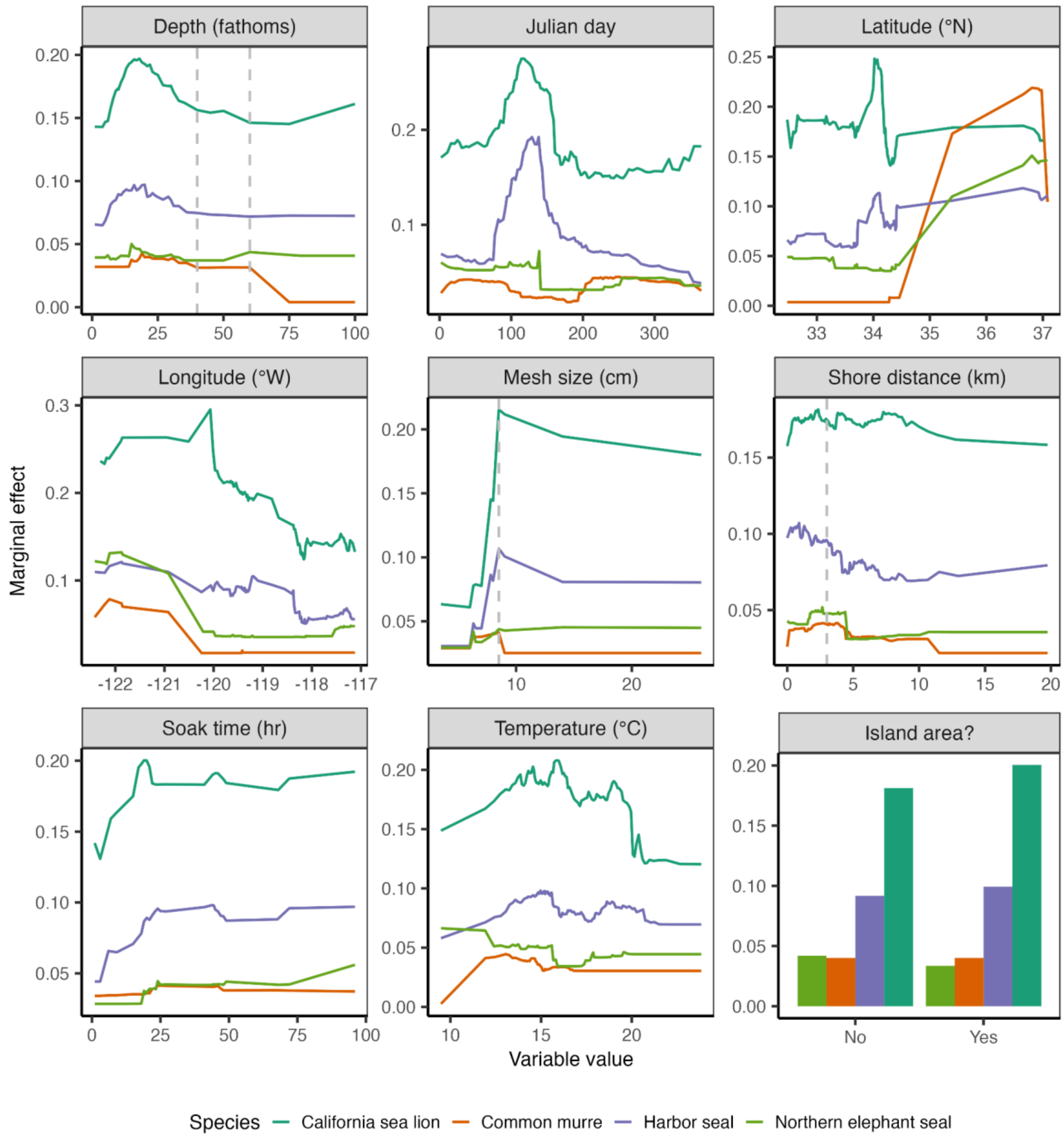

Fig. 5. Marginal effect of the evaluated explanatory variables on bycatch risk as estimated by the best fitting random forest model for the four study species with acceptable model performance. The marginal effect of each variable represents the effect of the variable when the other variables are held at their mean values. The dashed lines indicate 40 and 60 in Depth (fathoms), 8.5 in mesh size (cm), and 3 in shore distance (km).

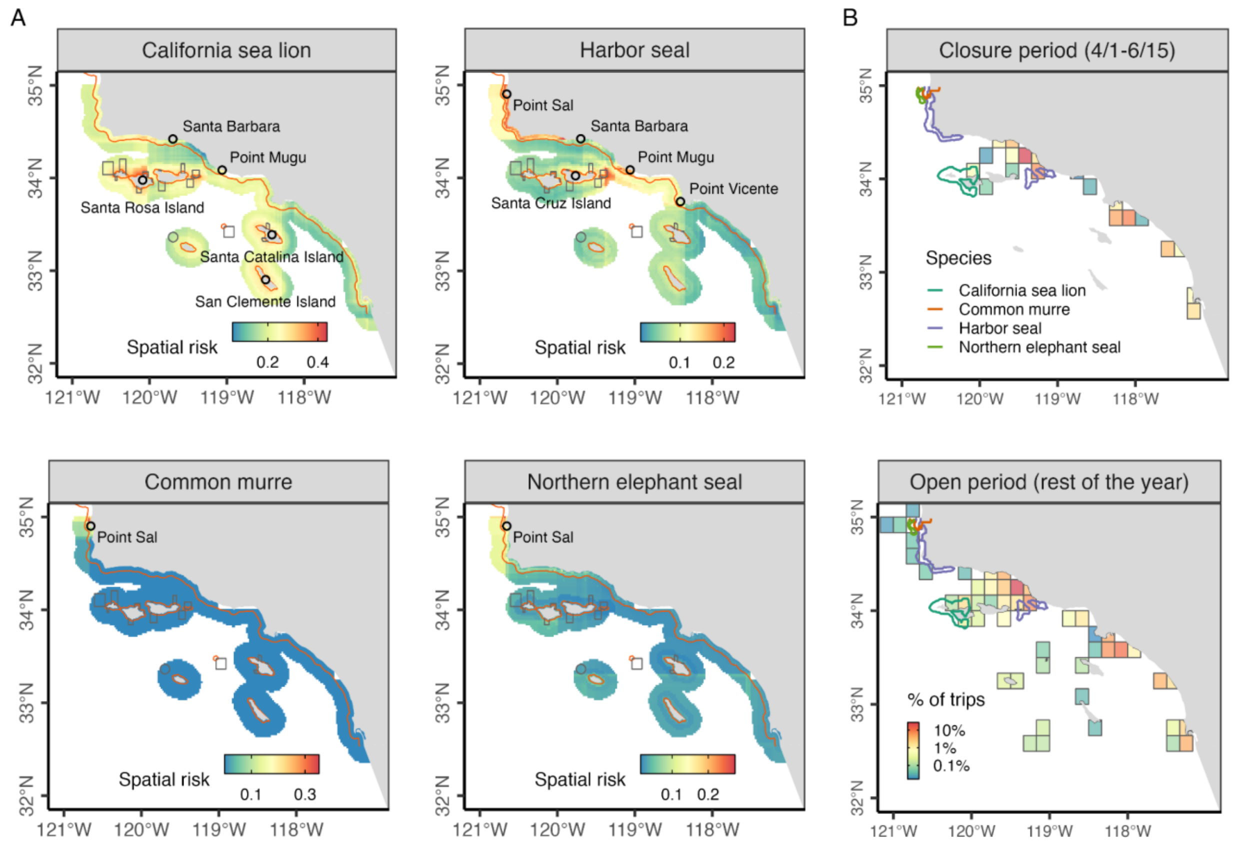

Fig. 6. The **(A)** spatial bycatch risk relative to current management areas and **(B)** spatial bycatch hotspots relative to recent fishing effort. Panel **A** shows the average spatial bycatch risk as estimated by the best fitting random forest model for the four study species with acceptable model performance. The spatial bycatch risk represents the probability of bycatch at a given location under recent (2010-2021) average conditions. Key landmarks for delineating bycatch hotspots are labeled in each panel. The thin orange coastal line marks the nearshore areas from which gillnet fishing is excluded: within 3 nautical miles of the mainland and within 1 nautical mile or shallower than 70 fathoms (whichever is closer to shore) from the Channel Islands. The gray polygons indicate the locations of California Marine Protected Areas, where all set gillnet fisheries are excluded. Spatial bycatch risk is shown only for southern California, as this is the only area where the fishery can operate under current regulations. Panel **B** shows hotspots of bycatch risk relative to recent fishing effort (2002-2022; see **Fig 1A**) during the proposed closed and open periods.

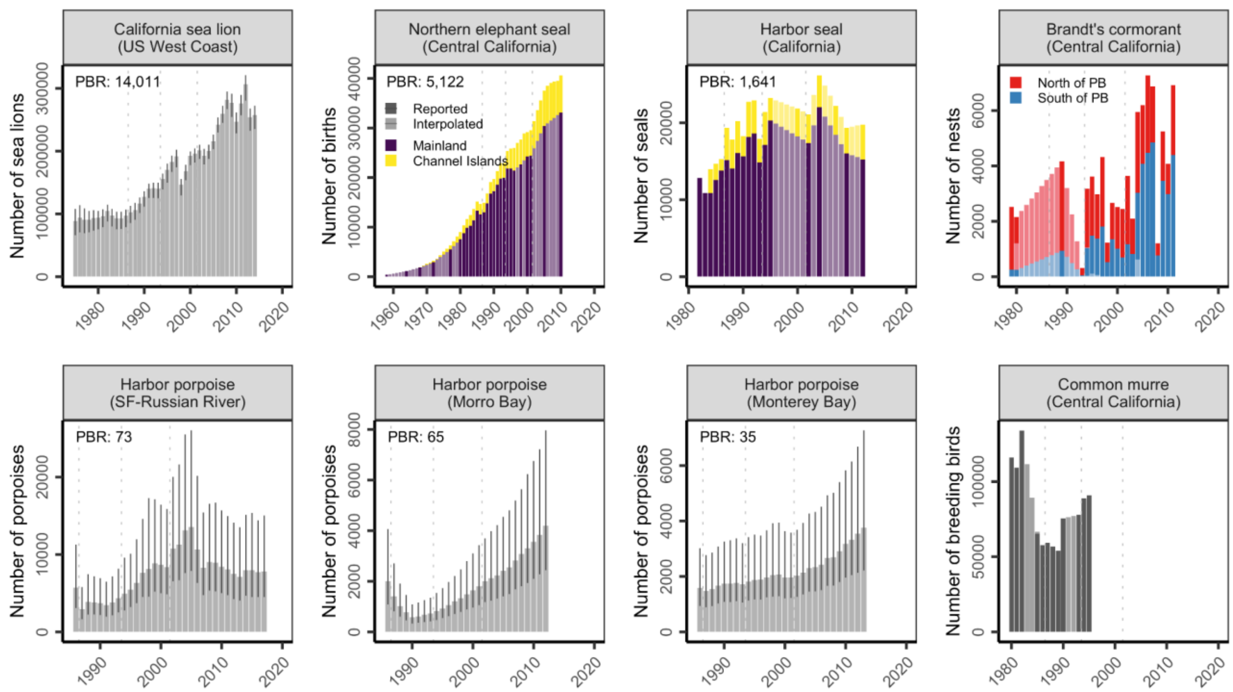

Fig. 7. Estimated abundance of populations of the six study species. The potential biological removal (PBR) indicates the maximum number of animals, not including natural mortalities, that may be removed from a marine mammal stock while allowing the stock to reach or maintain its optimum sustainable population. PBRs are only calculated for marine mammals. Error bars indicate 95% confidence or credible intervals. Years without reported values were filled using linear interpolation. Vertical lines mark years in which major bycatch regulations were implemented. Population estimates are from the following sources: California sea lion (Laake et al., 2018), northern elephant seal (Carretta et al., 2022), harbor seal (Carretta et al., 2022), harbor porpoise (Forney et al., 2021), Brandt's cormorant (Capitolo et al., 2012), and common murre (Carter, 2001). Data from Carretta et al. (2022) were graphically digitized.

Table 1. Performance of the best fitting random forest model by species.

				Training data		Test data	
Species	**# of bycatch observed**	**Best model**	**Mtry**	**Kappa**	**AUC**	**Kappa**	**AUC**
California sea lion	1372	Weighted-25	3	0.24	0.78	0.23	0.78
Harbor seal	519	Weighted-75	2	0.25	0.83	0.15	0.81
Harbor porpoise	97	Weighted-50	2	0.34	0.98	-0.005	0.98
Common murre	2381	Weighted-25	6	0.71	0.99	0.61	0.97
Brandt’s cormorant	118	Weighted-25	8	0.06	0.68	0.07	0.63
Northern elephant seal	78	Weighted-25	1	0.23	0.87	0.21	0.86

Supplemental Information

Imputing missing values in observed and logbooks data

We developed a series of simple assumptions to impute missing values for a few key variables (GPS coordinates, fishing depth, soak hour, mesh size) reported in the observer data (**Fig. S3A; Table S4**). We assigned missing GPS coordinates using the median coordinates for observed trips within the statistical block most frequently visited by the vessel – in order of preference – that week, month, or year based on the logbook data (described below). We derived missing fishing depths by extracting depths from 25-meter resolution bathymetry data (CDFW, 2002) (**Fig. S3B**). We reassigned missing soak hours the mode value for a vessel and target species (**Fig S3C**). We reassigned missing mesh sizes the mode for – in order of preference – the vessel and target species, the target species, or all vessels (**Fig. S3DE**). We assigned each GPS coordinate to the nearest statistical reporting block (see **Fig. 1A**), which allows points erroneously falling on land to be assigned a likely statistical block. We derived the distance from shore, a covariate used to explain bycatch rates in the random forest model, as the distance of each set to the nearest point on shore.

We developed a series of simple assumptions to impute missing or unrealistic values for a few key variables (fishing depth, soak hour, mesh size) reported in the logbook data (**Fig. S5A; Table S4**). We reassigned both missing (including 0 values) and unrealistic fishing depths, which we defined as depths exceeding the maximum depth in the reported fishing block, the median depth of the fishing block (**Fig. S5B**). We computed the median and maximum depths of each fishing block using 25-meter resolution bathymetry data (CDFW, 2002). We reassigned missing soak hours (including 0 values) the mode value for a vessel. We capped rare and unlikely soak times exceeding 96 hours (4 days) at 96 hours; however, such soak times could theoretically occur during rough weather when it is unsafe to haul gear (**Fig. S5C**). We reassigned missing (including 0 values) and unrealistic mesh sizes, which we defined as mesh sizes exceeding 20 inches, using a hierarchical procedure (**Fig. S5DE**). For logbooks with both vessel identification and target species information, we assigned the mesh size most commonly used by the vessel when targeting that target species. For logbooks with only target species information (no vessel identification), we assigned the mesh size most commonly used when targeting that target species across all vessels (**Figs. S5 & S6**). We derived the distance from shore, a covariate used to explain bycatch rates in the random forest model, as the median distance from shore of observed trips within the reported block given that exact locations are not reported.

Bycatch estimation using the random forest models

We used the best fitting model to generate annual estimates of protected species bycatch from 1981 to 2022 by predicting whether "pseudo-sets" recorded in logbooks were likely to have captured each study species and assuming median numbers of sets and caught animals for "pseudo-sets" with bycatch. We predict to pseudo-sets rather than trips because the random forest model is trained on set-level covariates in the observer data. We used the best fitting model for each species to estimate the probability that a logged pseudo-set included bycatch of a species then categorized the pseudo-set as with or without bycatch using a species-specific probability threshold. We derived the species-specific probability thresholds as the threshold that maximizes Cohen's kappa when applied to the training datasets (**Fig. S17**). We selected the probability threshold based on Cohen's kappa rather than the area under the receiver operator curve (AUC) because (1) the models were tuned and selected based on Cohen's kappa and (2) simulation work shows that deriving thresholds based on AUC tends to overestimates the prevalence of rare events while it underestimates the prevalence of common events (Freeman and Moisen, 2008; Manel et al., 2001). We summed the number of pseudo-sets predicted to have bycatch each year, converted this sum to "true sets" assuming three sets per pseudo-set (**Fig. S4AB**), and multiplied this sum by the median number of captures when a capture occurs to generate estimates of the total number of captured animals (**Fig. S4C**). We opted not to employ a more complex two-stage or hurdle model approach, where a second model estimates the number of captured individuals when bycatch occurs, given the rarity of bycatch events larger than one for all species but common murre (**Fig. S4C**).

The random forest models estimate trends in bycatch that are similar to the estimates from the ratio estimator from 2000-2022 (**Fig. 3**). However, the estimates produced by the two approaches diverge from 1981-2000 by various extents. While they generally agree for California sea lion back to 2000, the random forest model underpredicts bycatch relative to the ratio estimator in the late 1990s and overpredicts in the 1980s and early 1990s (**Fig. 3**). While the approaches generally agree for harbor seal back to 1995, the random forest model underpredicts bycatch relative to the ratio estimator before 1995, especially in the Channel Islands and Ventura strata (**Fig. S14**). For common murre, the random forest model overpredicts bycatch relative to the ratio estimator in the mid- to late-1990s and underpredicts relative to the ratio estimator in earlier years, especially in Morro and Monterey Bays. These underpredictions likely occur because of the unequal impacts of lost data from the northern strata in the 1980s (**Fig. 2**). Unlike the random forest models, the ratio estimators are able to use summarized observer data for this region and time period from old reports. As a result, the ratio estimators can learn from observations from this region and time period while the random forest models are blind to data from this region and period. Thus, the random forest models are likely to underpredict risk in early years in northern strata because they largely learned from late years in southern California, where bycatch risk was lower. For this reason, we recommend the use of bycatch estimates from the ratio estimators over the random forest models until a time when the 1980s observer data is rediscovered.

Comparison to bycatch estimates from historical reports

Our estimates of annual bycatch are generally aligned with estimates from historical studies (**Fig. S11**). Slight differences between our estimates and those from historical studies are driven by a mixture of differences in our methods and input data. While we apply a consistent approach for defining the fishery, stratifying the data, and estimating bycatch, historical studies have employed variable fishery definitions, stratification schemes, and estimation methodologies (**Table S1**). First, we consistently defined the fishery as using ≥3.5" set gillnets, while historical studies have considered set gillnets ≥8", ≥8.5", or of unspecified sizes (**Table S1**). Furthermore, it is unclear whether historical studies filtered out fishing sets based on the reported target species and whether this decision was consistent. We do not define the fishery based on reported target species given extreme heterogeneity in how this information is reported. Differences in fishery definitions, as well as differences in data cleaning methods, likely lead to the slight differences in effort and observed bycatch attributed to the fishery (**Fig. S12**), which inevitably causes differences in bycatch estimates. Second, we used a consistent six-region stratification scheme, while historical studies used a mixture of stratification schemes ranging from no stratification to spatial stratification to spatial-temporal stratification (**Table S1**). This impacts the ratio estimators and the magnitudes of bycatch. Finally, we used vessel days (trips) as the sample unit, whereas historical studies oscillated between trips and sets as the preferred sample unit (**Table S1**). This is problematic not only because of the lack of consistency but also because sets cannot be uniquely identified in the logbook data.

Identifying set gillnet landing receipts and revenues

We used landing receipts (a.k.a., fish tickets) to estimate ex-vessel revenues generated by the California ≥3.5" set gillnet fishery from 1981-2022. Among other information, landing receipts report the date, value, species, and gear of commercial landings. We identified landing receipts associated with the ≥3.5" set gillnet fishery through a multi-step filtering process. First, we filtered the landing receipts to the five gear types that could include ≥3.5" mesh set gillnets: trammel nets, set gillnets, small-mesh set gillnets, large-mesh set gillnets, or entangling nets (**Fig. S1A**). Entangling nets, which encompass both set and drift gillnets, were a widely used gear type from 1984-1993. As a result, this filter retained many swordfish landings and other landings associated with drift gillnets. It also retained many herring landings and other landings associated with set gillnets with mesh sizes smaller than 3.5 inches. To remove landing receipts associated with drift gillnets and set gillnets with mesh sizes smaller than 3.5 inches, we used gillnet logbooks to identify landing receipts associated with known set gillnet vessels and logged set gillnet trips. We began by further filtering to only include vessels documented as using ≥3.5” set gillnets in the gillnet logbooks (**Fig. S1B**). After this filter, a large amount of swordfish and herring landings remained, indicating that many ≥3.5” set gillnet vessels use other gears. Thus, to further tie landing receipts with known ≥3.5” set gillnet trips, we explored four related approaches for linking landing receipts to logged ≥3.5” set gillnet trips. The first approach was the most strict and only considered landing receipts reported on the exact day of logged ≥3.5” set gillnet trips (**Fig. S1C**). This filter eliminated swordfish and herring landings but is likely to be overly restrictive. The date of landing may differ from the date of fishing because of misreporting, multi-day trips, or delayed sales. Thus, we explored three progressively less restrictive rules, which attributed landing receipts recorded within one (**Fig. S1D**), two (**Fig. S1E**), or three (**Fig. S1F**) days of logged ≥3.5” set gillnet trips to the fishery. We selected the landing receipts associated with the 3-day buffer as the final set of landing receipts associated with the fishery because it effectively eliminated landings of species not associated with the ≥3.5” set gillnet fishery (i.e., swordfish and herring) while being inclusive-within-reason of potential ≥3.5” set gillnet fishery landings. Finally, we adjusted daily ex-vessel landings values for inflation by converting all values to January 1, 2022 US dollars using the *priceR* package in R (Condylios, 2023).

Mapping species ranges

We mapped the range of the study species using range maps from the California Wildlife Habitat Relationships (CWHR) System (CDFW, 2021) (**Fig. S15**). The CWHR ranges were developed by species-specific experts. Range maps were not developed for harbor porpoise as part of the CWHR effort. We developed a range map for harbor porpoise assuming that harbor porpoise occur primarily in waters shallower than 50 fathoms (92 meters) north of Point Conception (Forney et al., 2014). Harbor seal haulouts were mapped using the CDFW Harbor Seal Haulout GIS dataset (CDFW, 2014). CDFW conducted aerial surveys of all known haulout sites in 2001, 2002, and 2003 and counted the number of harbor seals observed in aerial photographs of each site. We mapped northern elephant seal rookery size in 2010 using a database of counts developed by (Lowry et al., 2014). Counts were generated through a review of ground and aerial photographic surveys. We mapped California sea lion haulouts using data from (Lowry, 2021). Haulouts were mapped in the Channels Islands between 2016-2019 using aerial photographic surveys. Sea lion haulouts occur along the California coast but were not mapped to single sites in this study and therefore not plotted in our range maps. We mapped seabird colonies using the 2010 CDFW Seabird Colonies Database (CDFW, 2010). These data were collected as part of the Marine Life Protection Act (MLPA) planning process and report the maximum number of seabirds of 26 species at all known colonies.

Supplemental Tables

Table S1. Historical bycatch estimation studies and their characteristics (the period column indicates month and year as MM/YY).

Study	Seasons	Period	Species	Fishery definition	Stratification scheme	Sample unit
(Barlow et al., 1994)	1983-87	(see papers)	Pinnipeds/cetaceans	(see included papers)	(see included papers)	(see papers)
(Hanan et al., 1988)	1983-85	Apr 1-Mar 31	Sea lion, harbor seals	Set nets for halibut/flounder/sharks	5 regions	Sets
(Diamond and Hanan, 1986)	1983	4/83 - 3/84	Harbor porpoise	≥8.0” set nets for halibut/flounder	3 regions	Sets
(Hanan et al., 1986)	1984	4/84 - 3/85	Harbor porpoise	Set nets for halibut/flounder/sharks	3 regions	Sets
(Hanan et al., 1987)	1985	4/85 - 3/86	Harbor porpoise	Set nets (but not for croaker)	3 regions	Sets
(Hanan and Diamond, 1989)	1986	4/86 - 3/87	Sea lion, harbor seal, harbor porpoise	Set nets for halibut/flounder/sharks	5 regions, seasons	Sets
(Konno, 1990)	1987	4/87 - 3/88	Sea lion, harbor seal, harbor porpoise	Set nets for halibut/flounder/sharks	5 regions, seasons	Sets
(Perkins et al., 1994)	1988-90	4/88 - 3/89 4/89 - 12/89 1/90 - 12/90	Pinnipeds/cetaceans	Set nets for halibut/angel shark	3 regions	Vessel-day
(Lennert et al., 1994)	1990	7/90 - 12/90	Marine mammals	Set nets for halibut/angel shark	3 regions	Vessel-day
(Perkins et al., 1992a)	1990	7/90 - 6/91	Pinnipeds/cetaceans	≥8.0” nets for halibut/angel shark	3 regions	Sets
(Perkins et al., 1992b)	1991	1/91 - 12/91	Pinnipeds/cetaceans	Set nets for halibut/angel shark	SCal/Ventura: Quarters Other strata: Annual	Vessel-day
(Julian, 1993)	1992	1/92 - 12/92	Pinnipeds/cetaceans	Set nets for halibut/angel shark	4 regions: quarterly	Vessel-day
(Julian, 1994)	1993	1/93 - 12/93	Pinnipeds/cetaceans	Set nets for halibut	4 regions	Vessel-day
(Julian and Beeson, 1998)	1990-95	7/90-12/90 1-12, 91-95	Mammals/seabirds/turtles	≥8.5” nets for halibut/angel shark	SCal/Ventura: Quarters Other strata: Annual	Vessel-day
(Cameron and Forney, 1999)	1997-98	1/97 - 12/97 1/98 - 12/98	Cetaceans	≥8.5” nets for halibut/angel shark	1997: Geographical+seasonal 1998: Geographical only	Vessel-day
(Cameron and Forney, 2000)	1999	1/99 - 12/99	Cetaceans	≥8.5” nets for halibut/angel shark	Geographical+seasonal	Vessel-day
(Carretta, 2001)	2000	1/00 - 12/00	Cetaceans	≥8.5” nets for halibut/angel shark	SCal/Ventura: Quarters Other strata: Annual	Vessel-day
(Carretta, 2002)	2001	1/01 - 12/01	Cetaceans	Set nets for halibut/angel shark	SCal/Ventura: Quarters Other strata: Annual	Vessel-day
(Carretta and Chivers, 2003)	2002	1/02 - 12/02	Cetaceans	Set nets for halibut/angel shark	SCal/Ventura: Quarters Other regions: Annual	Vessel-day
(Carretta and Chivers, 2004)	2003	1/03 - 12/03	Marine mammals	≥8.5” nets for halibut/angel shark	SCal/Ventura: Quarters Other regions: Annual	Vessel-day
(Carretta and Enriquez, 2009)	2007	1/07 - 12/07	Mammals/seabirds	Set nets for halibut/white seabass	No stratification	Sets
(Carretta and Enriquez, 2012a)	2010	1/10 - 12/10	Mammals/seabirds	Set nets for halibut/white seabass	No stratification	Vessel-day
(Carretta and Enriquez, 2012b)	2011	1/11 - 12/11	Mammals/seabirds	Set nets for halibut/white seabass	No stratification	Sets
(Carretta et al., 2014)	2012	1/12 - 12/12	Mammals/seabirds/turtles	Set nets for halibut/white seabass	No stratification	Vessel-day

Table S2. Number of fishing trips in the California 3.5 inch mesh set gillnet fishery by year.

		# of trips			Percent of trips	
Year	**# of vessels**	**Total**	**Observed**	**Unobserved**	**Observed**	**Unobserved**
1981	162	6139	0	6139	0.0%	100.0%
1982	222	9218	0	9218	0.0%	100.0%
1983	262	9012	143	8869	1.6%	98.4%
1984	357	12374	255	12119	2.1%	97.9%
1985	378	14314	238	14076	1.7%	98.3%
1986	379	13336	217	13119	1.6%	98.4%
1987	290	10667	213	10454	2.0%	98.0%
1988	211	8585	120	8465	1.4%	98.6%
1989	198	7811	58	7753	0.7%	99.3%
1990	182	6836	167	6669	2.4%	97.6%
1991	158	6668	716	5952	10.7%	89.3%
1992	117	5611	744	4867	13.3%	86.7%
1993	115	5754	959	4795	16.7%	83.3%
1994	74	2455	150	2305	6.1%	93.9%
1995	70	2616	0	2616	0.0%	100.0%
1996	59	2654	0	2654	0.0%	100.0%
1997	66	3310	0	3310	0.0%	100.0%
1998	59	2889	0	2889	0.0%	100.0%
1999	63	4026	165	3861	4.1%	95.9%
2000	66	3828	66	3762	1.7%	98.3%
2001	63	3289	0	3289	0.0%	100.0%
2002	54	3395	0	3395	0.0%	100.0%
2003	51	2779	0	2779	0.0%	100.0%
2004	47	2627	0	2627	0.0%	100.0%
2005	40	1930	0	1930	0.0%	100.0%
2006	41	1658	5	1653	0.3%	99.7%
2007	44	1797	65	1732	3.6%	96.4%
2008	43	1936	0	1936	0.0%	100.0%
2009	41	1934	0	1934	0.0%	100.0%
2010	33	1544	64	1480	4.1%	95.9%
2011	39	1575	55	1520	3.5%	96.5%
2012	35	1374	76	1298	5.5%	94.5%
2013	32	968	48	920	5.0%	95.0%
2014	25	819	0	819	0.0%	100.0%
2015	25	1014	0	1014	0.0%	100.0%
2016	24	1077	0	1077	0.0%	100.0%
2017	21	840	69	771	8.2%	91.8%
2018	19	948	0	948	0.0%	100.0%
2019	18	1151	0	1151	0.0%	100.0%
2020	18	841	0	841	0.0%	100.0%
2021	16	870	0	870	0.0%	100.0%

Table S3. Bycatch rates extracted from historical reports (* mark years with raw observer data where the summary values from the historical reports are not needed).

Reference	Species	Region	Year	# sets observed	Estimated # trips observed	Catch	Catch per trip	Raw data available?
Hanan et al. 1988	California sea lion	Channel Islands	1985	180	60.00	44	0.733	*
Hanan & Diamond 1989	California sea lion	Channel Islands	1986	66	22.00	54	2.455	*
Hanan et al. 1988	California sea lion	Monterey Bay	1983	22	7.33	12	1.636	
Hanan et al. 1988	California sea lion	Monterey Bay	1984	126	42.00	19	0.452	
Hanan et al. 1988	California sea lion	Monterey Bay	1985	49	16.33	5	0.306	
Hanan & Diamond 1989	California sea lion	Monterey Bay	1986	36	12.00	4	0.333	
Hanan et al. 1988	California sea lion	Morro Bay	1983	288	96.00	41	0.427	
Hanan et al. 1988	California sea lion	Morro Bay	1984	374	124.67	22	0.176	
Hanan et al. 1988	California sea lion	Morro Bay	1985	317	105.67	25	0.237	
Hanan & Diamond 1989	California sea lion	Morro Bay	1986	137	45.67	13	0.285	*
Hanan et al. 1988	California sea lion	San Francisco	1983	158	52.67	4	0.076	
Hanan et al. 1988	California sea lion	San Francisco	1984	300	100.00	8	0.080	
Hanan et al. 1988	California sea lion	San Francisco	1985	348	116.00	3	0.026	
Hanan & Diamond 1989	California sea lion	San Francisco	1986	419	139.67	2	0.014	
Hanan et al. 1988	California sea lion	Southern California	1983	430	143.33	16	0.112	*
Hanan et al. 1988	California sea lion	Southern California	1984	571	190.33	13	0.068	*
Hanan et al. 1988	California sea lion	Southern California	1985	339	113.00	5	0.044	*
Hanan & Diamond 1989	California sea lion	Southern California	1986	425	141.67	15	0.106	*
Hanan et al. 1988	California sea lion	Ventura	1983	430	143.33	16	0.112	*
Hanan et al. 1988	California sea lion	Ventura	1984	571	190.33	13	0.068	*
Hanan et al. 1988	California sea lion	Ventura	1985	339	113.00	5	0.044	*
Hanan & Diamond 1989	California sea lion	Ventura	1986	425	141.67	15	0.106	*
Diamond & Hanan 1986	Harbor porpoise	Monterey Bay	1983	22	7.33	2	0.273	
Hanan et al. 1986	Harbor porpoise	Monterey Bay	1984	126	42.00	2	0.048	
Hanan et al. 1987	Harbor porpoise	Monterey Bay	1985	49	16.33	2	0.122	
Hanan & Diamond 1989	Harbor porpoise	Monterey Bay	1986	36	12.00	1	0.083	
Diamond & Hanan 1986	Harbor porpoise	Morro Bay	1983	288	96.00	7	0.073	
Hanan et al. 1986	Harbor porpoise	Morro Bay	1984	374	124.67	3	0.024	
Hanan et al. 1987	Harbor porpoise	Morro Bay	1985	317	105.67	3	0.028	
Hanan & Diamond 1989	Harbor porpoise	Morro Bay	1986	137	45.67	3	0.066	*
Diamond & Hanan 1986	Harbor porpoise	San Francisco	1983	151	50.33	5	0.099	
Hanan et al. 1986	Harbor porpoise	San Francisco	1984	299	99.67	14	0.140	
Hanan et al. 1987	Harbor porpoise	San Francisco	1985	348	116.00	28	0.241	
Hanan & Diamond 1989	Harbor porpoise	San Francisco	1986	419	139.67	12	0.086	
Hanan et al. 1988	Harbor seal	Channel Islands	1985	180	60.00	13	0.217	*
Hanan et al. 1988	Harbor seal	Monterey Bay	1983	22	7.33	0	0.000	
Hanan et al. 1988	Harbor seal	Monterey Bay	1984	126	42.00	10	0.238	
Hanan et al. 1988	Harbor seal	Monterey Bay	1985	49	16.33	7	0.429	
Hanan & Diamond 1989	Harbor seal	Monterey Bay	1986	36	12.00	9	0.750	
Hanan et al. 1988	Harbor seal	Morro Bay	1983	288	96.00	17	0.177	
Hanan et al. 1988	Harbor seal	Morro Bay	1984	374	124.67	29	0.233	
Hanan et al. 1988	Harbor seal	Morro Bay	1985	317	105.67	84	0.795	
Hanan & Diamond 1989	Harbor seal	Morro Bay	1986	137	45.67	25	0.547	*
Hanan et al. 1988	Harbor seal	San Francisco	1983	158	52.67	11	0.209	
Hanan et al. 1988	Harbor seal	San Francisco	1984	300	100.00	22	0.220	
Hanan et al. 1988	Harbor seal	San Francisco	1985	348	116.00	38	0.328	
Hanan & Diamond 1989	Harbor seal	San Francisco	1986	419	139.67	60	0.430	
Hanan et al. 1988	Harbor seal	Southern California	1983	430	143.33	0	0.000	*
Hanan et al. 1988	Harbor seal	Southern California	1984	571	190.33	4	0.021	*
Hanan et al. 1988	Harbor seal	Southern California	1985	339	113.00	2	0.018	*
Hanan & Diamond 1989	Harbor seal	Southern California	1986	425	141.67	8	0.056	*
Hanan et al. 1988	Harbor seal	Ventura	1983	430	143.33	0	0.000	*
Hanan et al. 1988	Harbor seal	Ventura	1984	571	190.33	4	0.021	*
Hanan et al. 1988	Harbor seal	Ventura	1985	339	113.00	2	0.018	*
Hanan & Diamond 1989	Harbor seal	Ventura	1986	425	141.67	8	0.056	*

Table S4. Assumptions made throughout the analysis, their likelihood, and their potential impact on the results (*assumptions that are expected to be valid or true on average are not likely to impact the results).

Assumption	Likelihood and potential impact of assumption
Data analysis	
Ratio estimation	
1. Ratio estimation assumes that the rate of bycatch for observed fishing trips equals the rate for all fishing trips in a given stratum.	This assumption appears valid as the traits of observed trips are representative of the unobserved trips (**Fig. S7**) and we stratified our analysis using the 7-region scheme recommended in other studies (**Fig. S8**).
2. We assumed that the bycatch rate in years without observer data was equal to the bycatch rate in the closest year with data.	The validity of this assumption is difficult to assess given the inconsistency in the observer program (**Fig. 2BC**); however, it is this inconsistency that also makes the assumption necessary. Ultimately, our finding that recent bycatch is low is not likely sensitive to this assumption, as the effects of substantial decreases in fishing effort overwhelm the effects of changes in bycatch rates.
Bycatch estimation using the random for models*	
1. We assumed that a "pseudo-set" (roughly equivalent to a trip) is equivalent to 3 sets (**Fig. S4AB**).	This assumption is likely to be true on average since it reflects the median value, which has been consistent through time (**Fig. S4AB**).
2. We assumed the number of captures per set is the median number of captures when a capture occurs (**Fig. S4C**).	This assumption is likely to be true on average since it reflects the median value and that for most of the evaluated species, deviations from the median have been rare (**Fig. S4C**).
Data imputation	
Observer data	
1. We assigned missing GPS coordinates using the median coordinates for observed trips within the statistical block most frequently visited by the vessel – in order of preference – that week, month, or year based on the logbook data.	This assumption is likely to be true on average given that it uses vessel-specific median values.
2. We derived missing fishing depths by extracting depths from 25-meter resolution bathymetry data (**Fig. S3B**).	If the GPS coordinates were accurately reported, then the extracted depth estimates are accurate.
3. We reassigned missing soak hours the mode value for a vessel and target species.	This assumption is likely to be true on average given that it uses vessel-specific median values.
4. We reassigned missing mesh sizes the mode for – in order of preference – the vessel and target species, the target species, or all vessels (**Fig. S3DE**).	This assumption is likely to be true on average given that it uses mode values that are vessel-specific to the greatest extent possible.
5. We assigned each GPS coordinate to the nearest statistical reporting block, which allows points erroneously falling on land to be assigned a likely statistical block.	If the GPS coordinates were close to accurate, then this procedure is accurate on average.
Logbook data	
1. We reassigned both missing and unrealistic fishing depths, defined as depths exceeding the maximum depth in the reported fishing block, the median depth of the fishing block (**Fig. S5B**).	This assumption is likely to be true on average given it uses the median value.
2. We reassigned missing soak hours the mode value for a vessel.	This assumption is likely to be true on average given that it uses vessel-specific mode values.
3. We capped unlikely soak times exceeding 96 hours at 96 hours.	The validity of this assumption is unclear because the values could be typos; however, values >96 hours were rare and exert little leverage on the results.

4.	We reassigned missing and unrealistic (>20 inch) mesh sizes using a hierarchical procedure (**Fig. S5DE**). For logbooks with both vessel id and target species info, we assigned the mesh size most commonly used by the vessel when targeting that target species. For logbooks with only target species info (no vessel id), we assigned the mesh size most commonly used when targeting that target species across all vessels (**Figs. S5 & S6**).	This assumption is likely to be true on average given that it uses mode values that are vessel-specific to the greatest extent possible.

** *This analysis is informational and only included in the supplemental information.*

Supplemental Figures

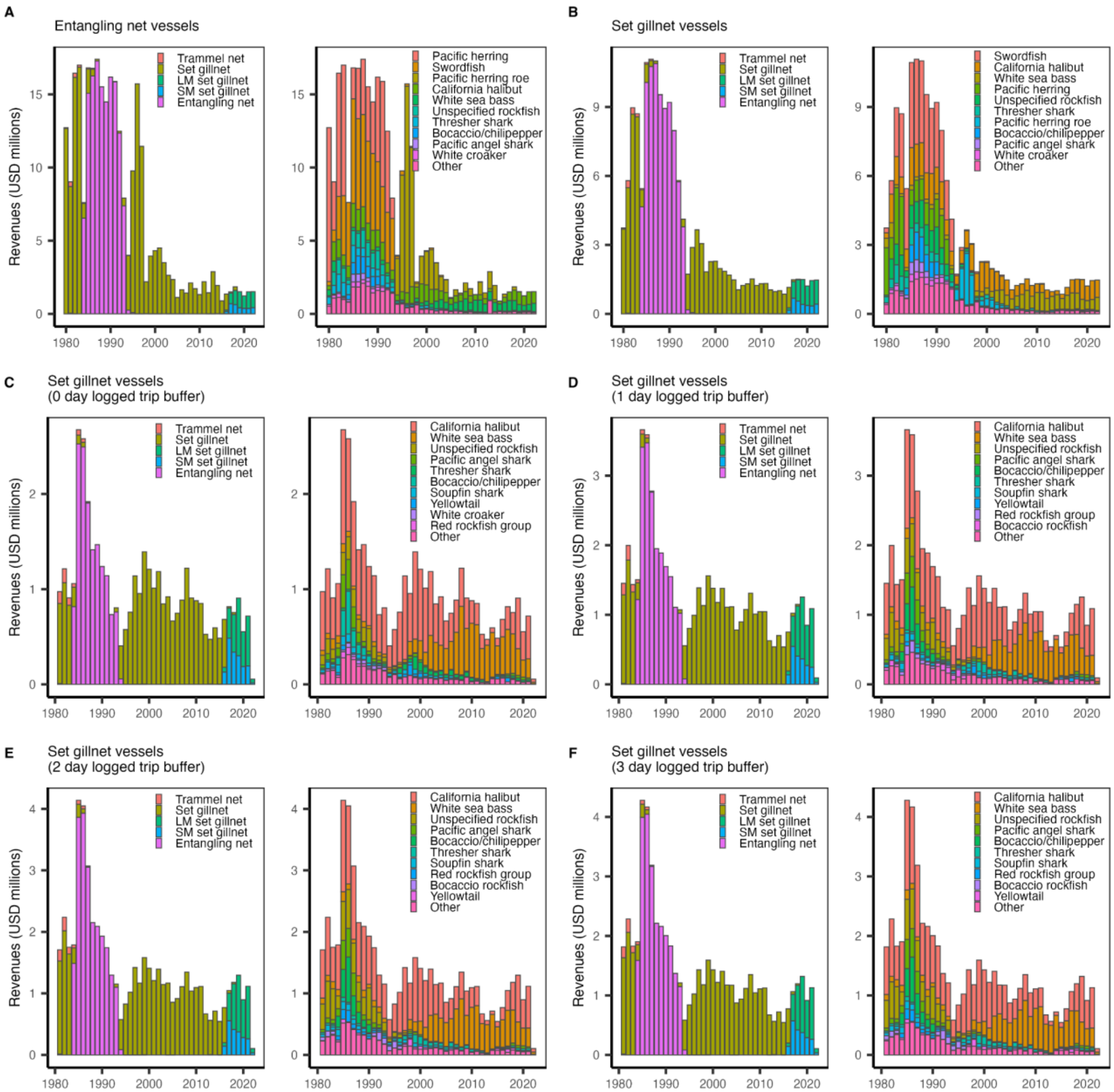

Fig. S1. Estimated ex-vessel revenues generated by the California ≥3.5” set gillnet fishery as estimated through six different filtration procedures. The filtration procedures examine the sum annual ex-vessel revenues reported on landing receipts from **(A)** vessels using various reported entangling net gears; **(B)** vessels using various entangling net gears that are known to use set gillnets based on logbooks; and **(C-F)** vessels known to use set gillnets based on logbooks that are dated within various buffers of a logged set gillnet trip. We adopted the final filter, which sums landing receipts date within 3 days of logged set gillnet trip, as the best estimate of ex-vessel revenues for the fishery. Revenues have not been adjusted for inflation (see **Fig. 1D** for the inflation adjusted ex-vessel revenues).

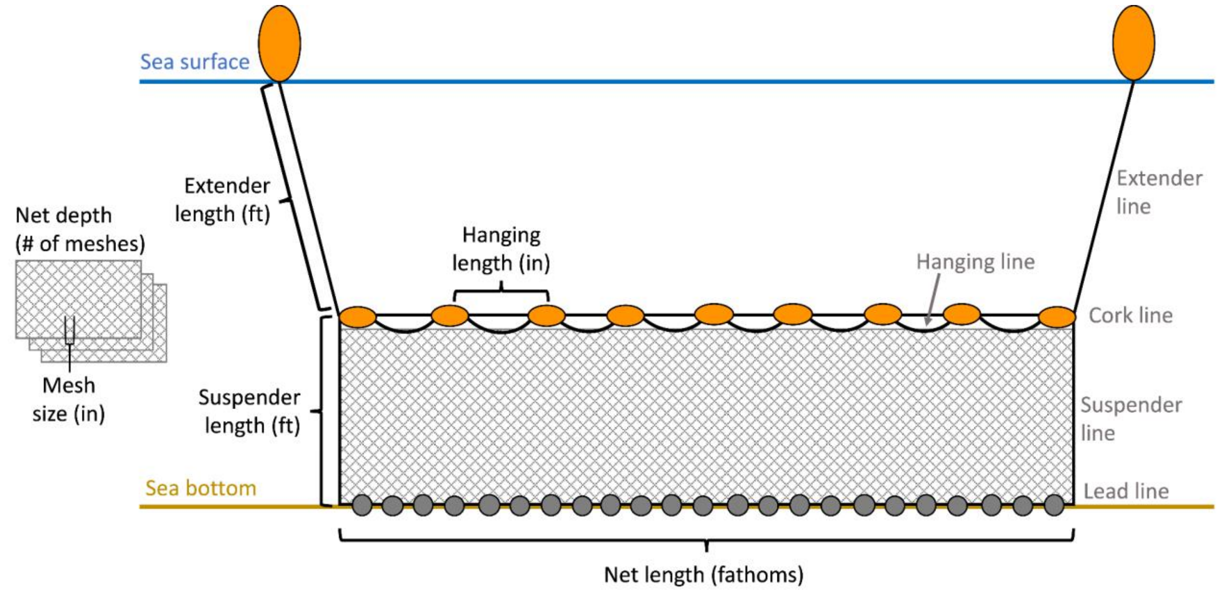

Fig. S2. Diagram of a typical California set gillnet illustrating the measurements reported in the logbook and observer data. Grey text indicates parts of the gillnet and black text indicates the reported measurements. The material, strength, and color of the net is also reported. Finally, the hanging ratio, a percentage that is calculated as the length of the mesh web divided by the length of the cork line, is reported.

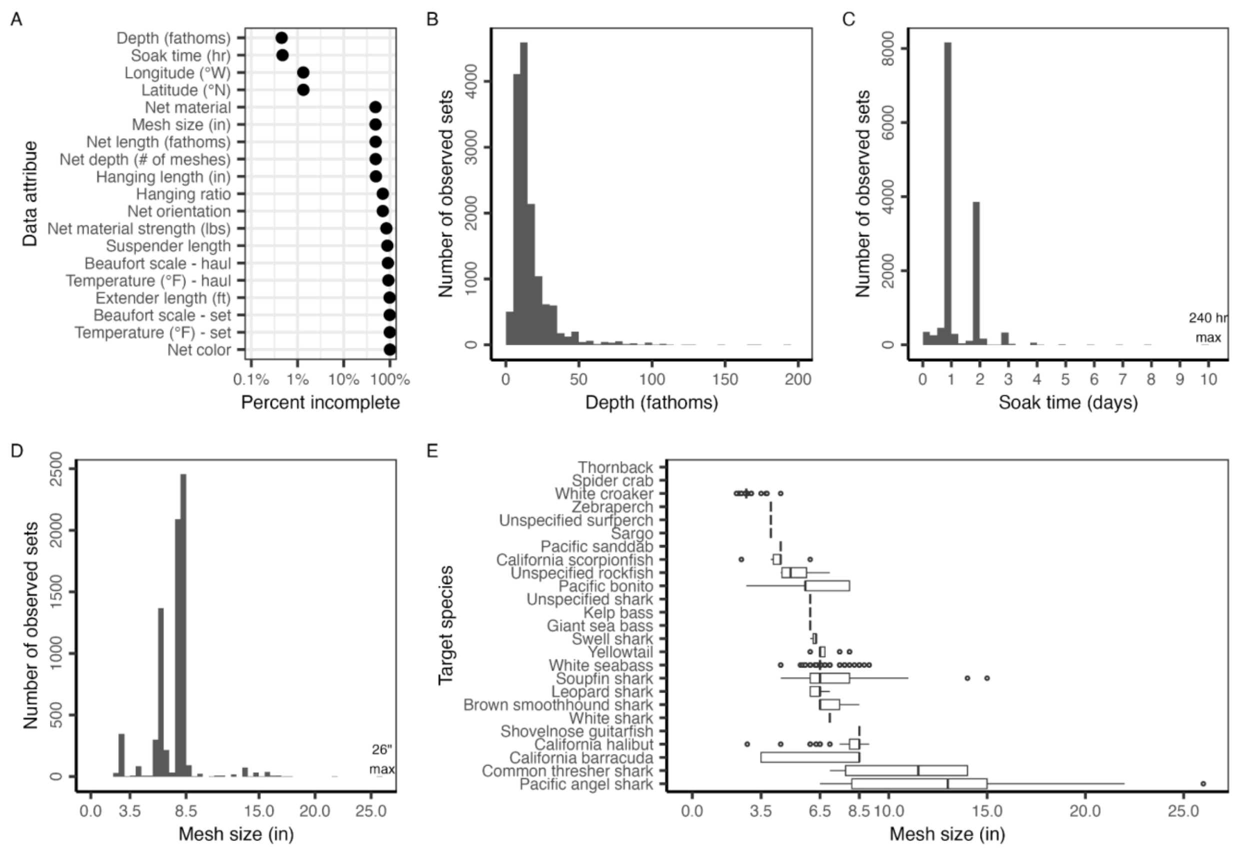

Fig. S3. Traits of the observed set gillnet metadata. Panel **A** shows the level of completeness of gillnet metadata. Panel **B** shows the distribution of reported depths. Panel **C** shows the distribution of reported soak times; the maximum reported soak time is 240 hours (10 days). Panel **D** shows the distribution of reported mesh sizes; the maximum reported mesh size is 26 inches. Panel **E** shows the distribution of reported mesh sizes by reported target species. In the boxplots, the solid line indicates the median, the box indicates the interquartile range (IQR; 25th to 75th percentiles), the whiskers indicate 1.5 times the IQR, and points indicate outliers.

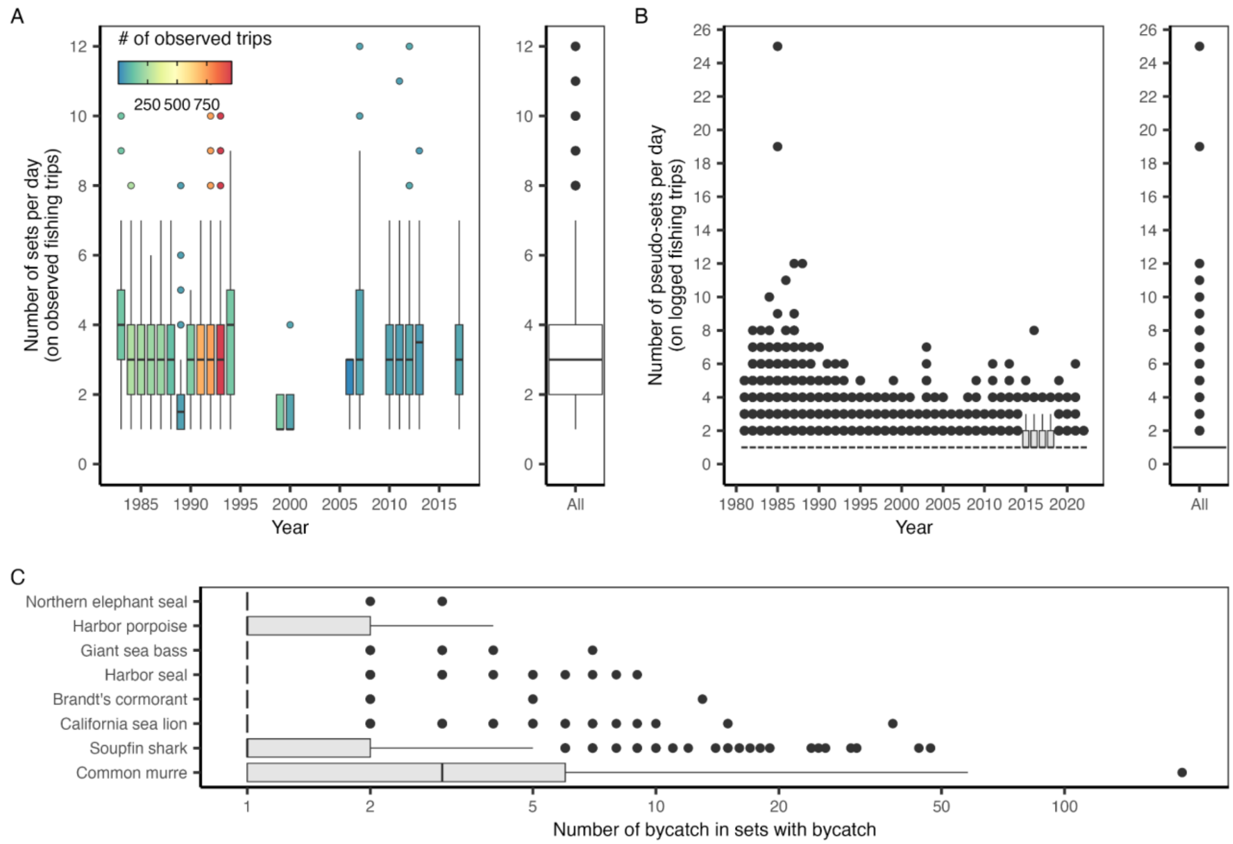

Fig. S4. The **(A)** number of sets per observed fishing trip by year and overall; **(B)** number of pseudo-sets per logged fishing trip by year and overall; and **(C)** number of bycatch in sets with bycatch of each of the species of interest. In the boxplots, the solid line indicates the median, the box indicates the interquartile range (IQR; 25th to 75th percentiles), the whiskers indicate 1.5 times the IQR, and points indicate outliers. In **(A)**, the fill color indicates the number of observed fishing trips contributing to the annual distribution.

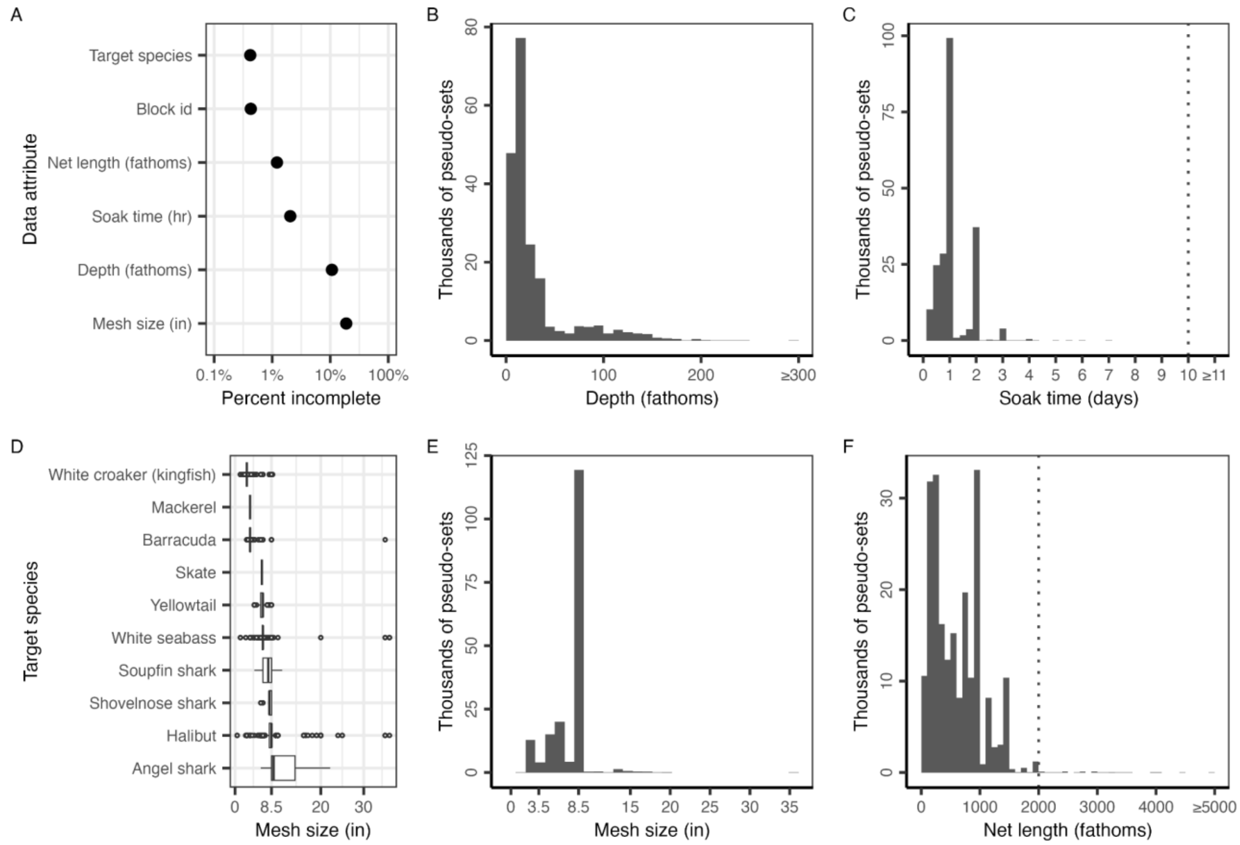

Fig. S5. Traits of the set gillnet sets documented in the logbook data. Panel **A** shows the level of completeness of gillnet logbook metadata. Panel **B** shows the distribution of reported depths. Panel **C** shows the distribution of reported soak times. The maximum reported soak time in the observer data is 10 days; rare values larger than this value were assumed to be unrealistic and were capped at the maximum. Panel **D** shows the distribution of reported mesh sizes by reported target species. In the boxplots, the solid line indicates the median, the box indicates the interquartile range (IQR; 25th to 75th percentiles), the whiskers indicate 1.5 times the IQR, and points indicate outliers. Panel **E** shows the distribution of reported mesh sizes. Panel **F** shows the distribution of reported net lengths.

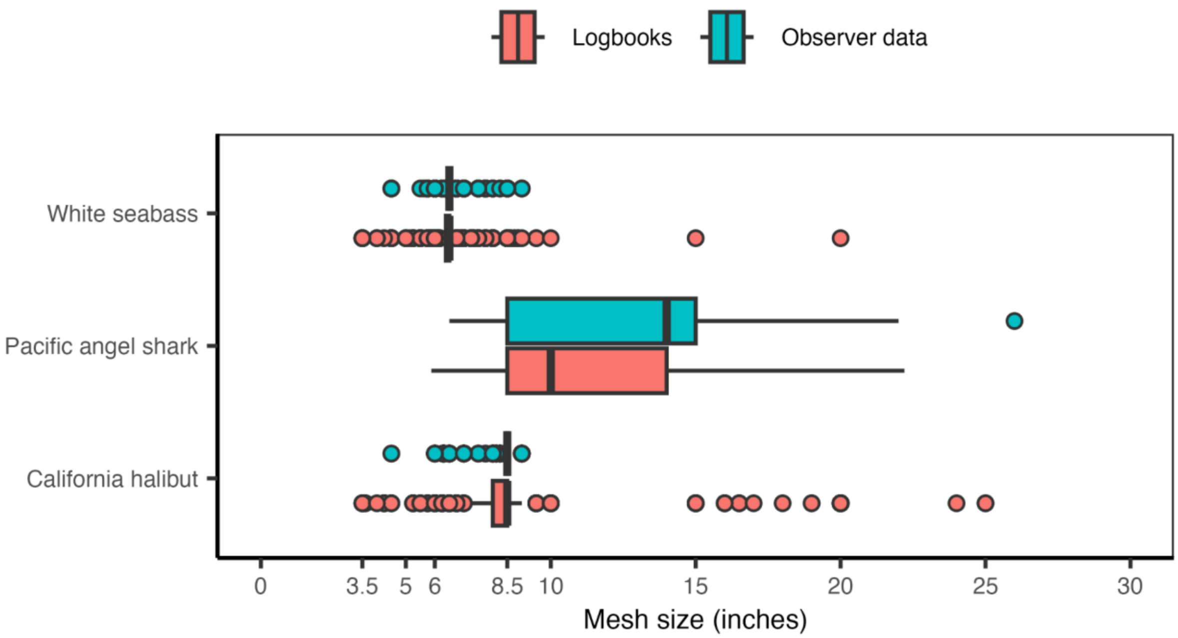

Fig. S6. Mesh size (inches) by target species in the logbook (red) and observer (blue) data. Since 1989, California halibut and Pacific angel shark can only be targeted using mesh sizes larger than 8.5 inches. White seabass are typically targeted using a minimum mesh size of 6.0 inches; however, a small amount of incidental take (<20% of catch and ≤10 individuals) in mesh sizes between 3.5 to 6.0 inches is allowed from June 16 to March 14 (14, § 155.10).

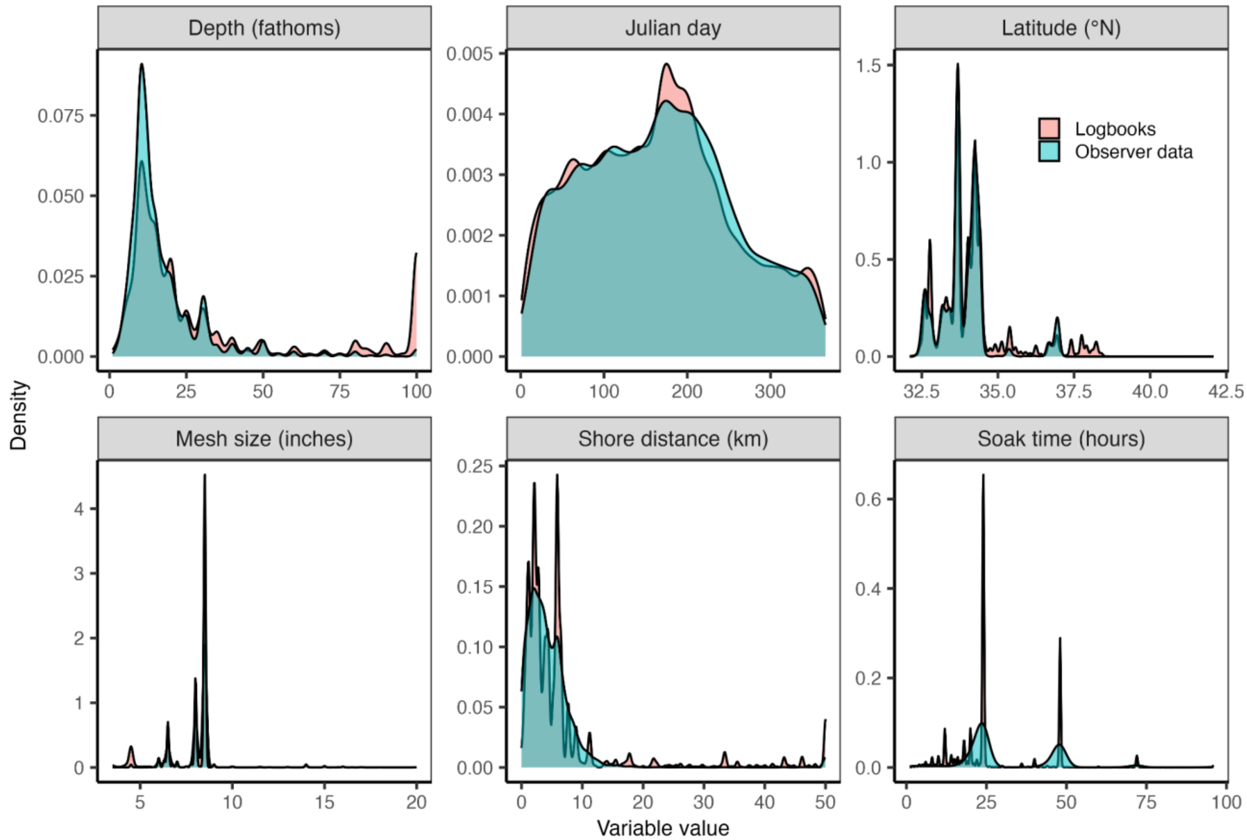

Fig. S7. Comparison of key set traits in the logbook (red) and observer (blue) data. We used a two-sided Kolmogorov-Smirnov test to confirm that the traits of the logbook and observer data could have come from the same probability distribution (all p-values < 0.001) could have come from the same probability distribution (all p-values < 0.001).

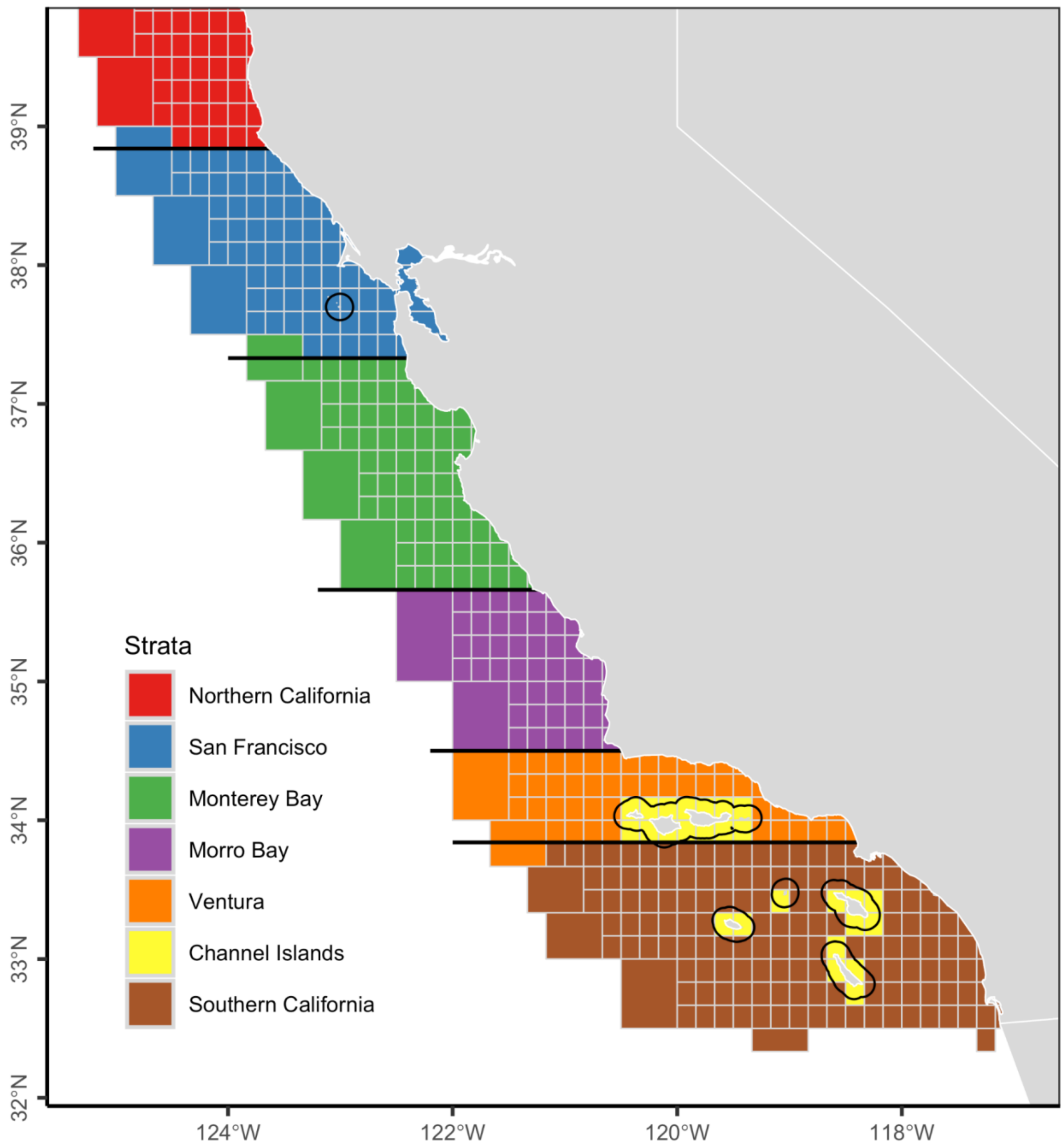

Fig. S8. The regional stratification scheme used throughout the analysis. The stratification scheme north of Point Conception was originally proposed by Diamond and Hanan (1986). The stratification scheme south of Point Conception was originally proposed by Julian (1993). The dark black lines around the Channel Islands show the 10 km buffer used to identify island-associated fishing trips.

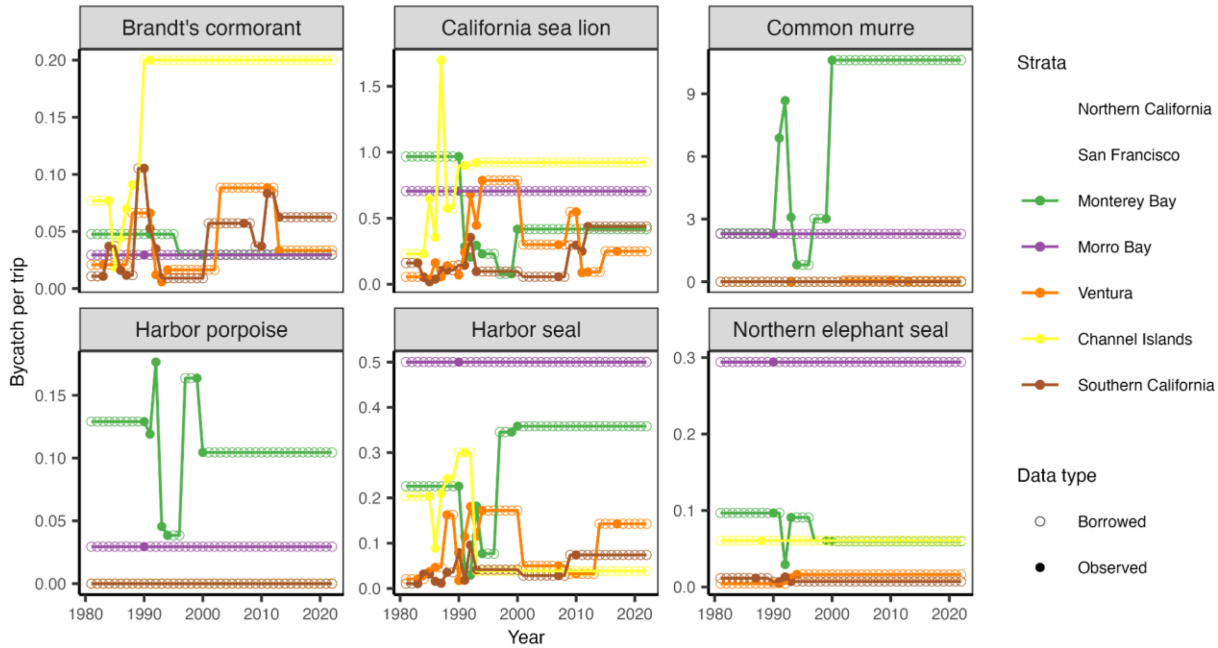

Fig. S9. Annual bycatch rates by species and regional strata as assumed in the ratio estimation analysis. Solid circles indicate years with observer data and open circles indicate years whose bycatch rates are borrowed from the closest year with observer data. See **Figs. 1A and S8** for maps of the regional strata.

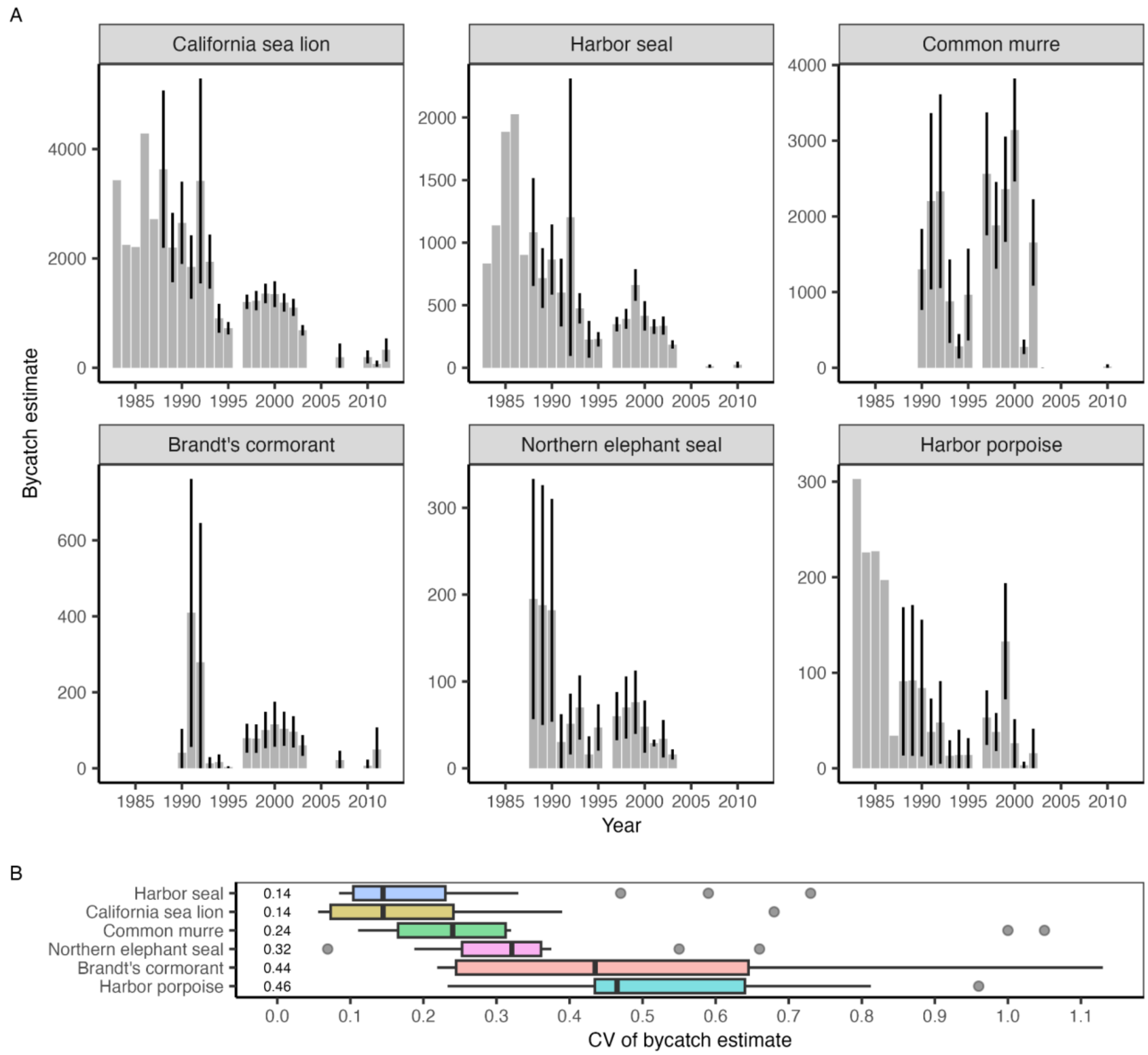

Fig. S10. Estimates of **(A)** annual bycatch from historical studies and **(B)** the uncertainty of these estimates expressed as the coefficient of variation (CV). In **(A)**, error bars indicate 95% confidence intervals. See **Table S1** for the sources of these estimates. In **(B)**, the median CV of the bycatch estimates for each species is printed on the far left. In the boxplots, the solid line indicates the median, the box indicates the interquartile range (IQR; 25th to 75th percentiles), the whiskers indicate 1.5 times the IQR, and points indicate outliers.

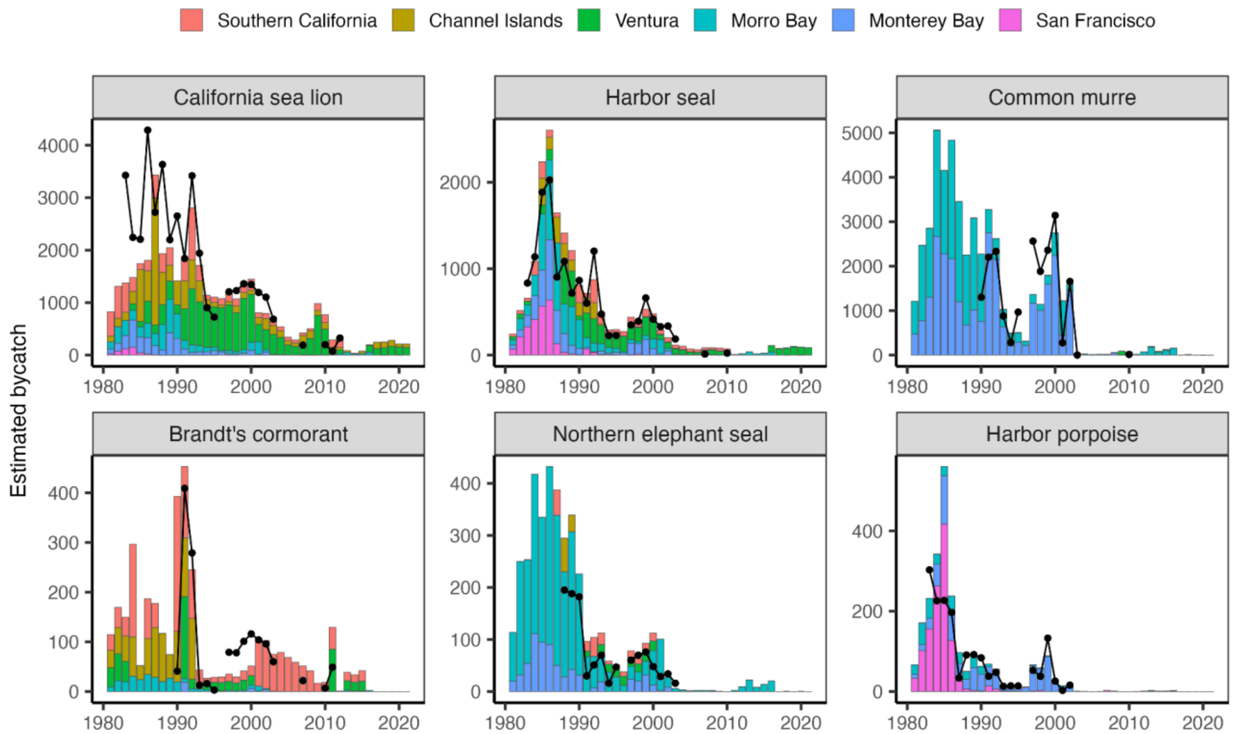

Fig. S11. A comparison of estimates of annual bycatch from our study (bars) and historical studies (points and lines). Potential reasons for these differences are explored in **Fig. S12**.

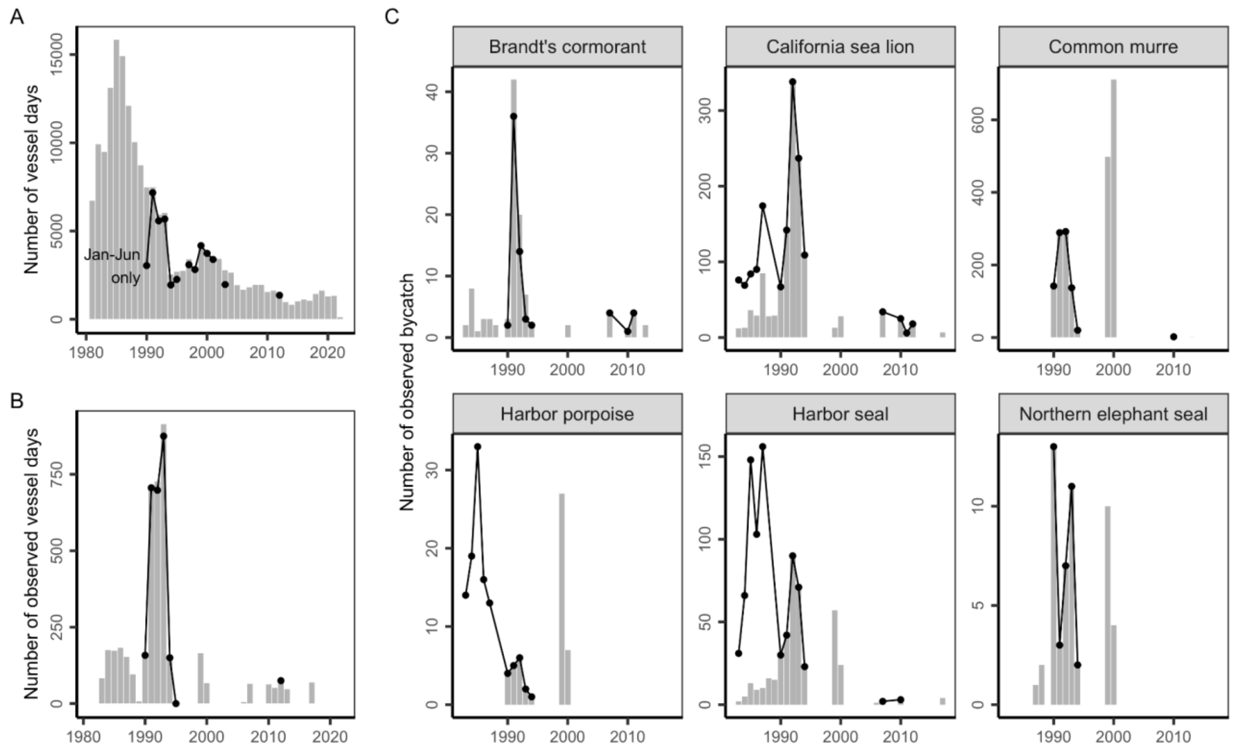

Fig. S12. A comparison of historical **(A)** fishing effort (number of vessel days); **(B)** observer coverage (number of observed vessel days); and **(C)** observer records (number of observed bycatch) derived in our analysis (bars) and reported in historical studies (points and lines). These time series represent the key inputs into the ratio estimation analysis and help to explain the differences between the estimates of annual bycatch derived in our study as compared to historical studies (see **Fig. S11**). See **Table S1** for additional details on historical studies.

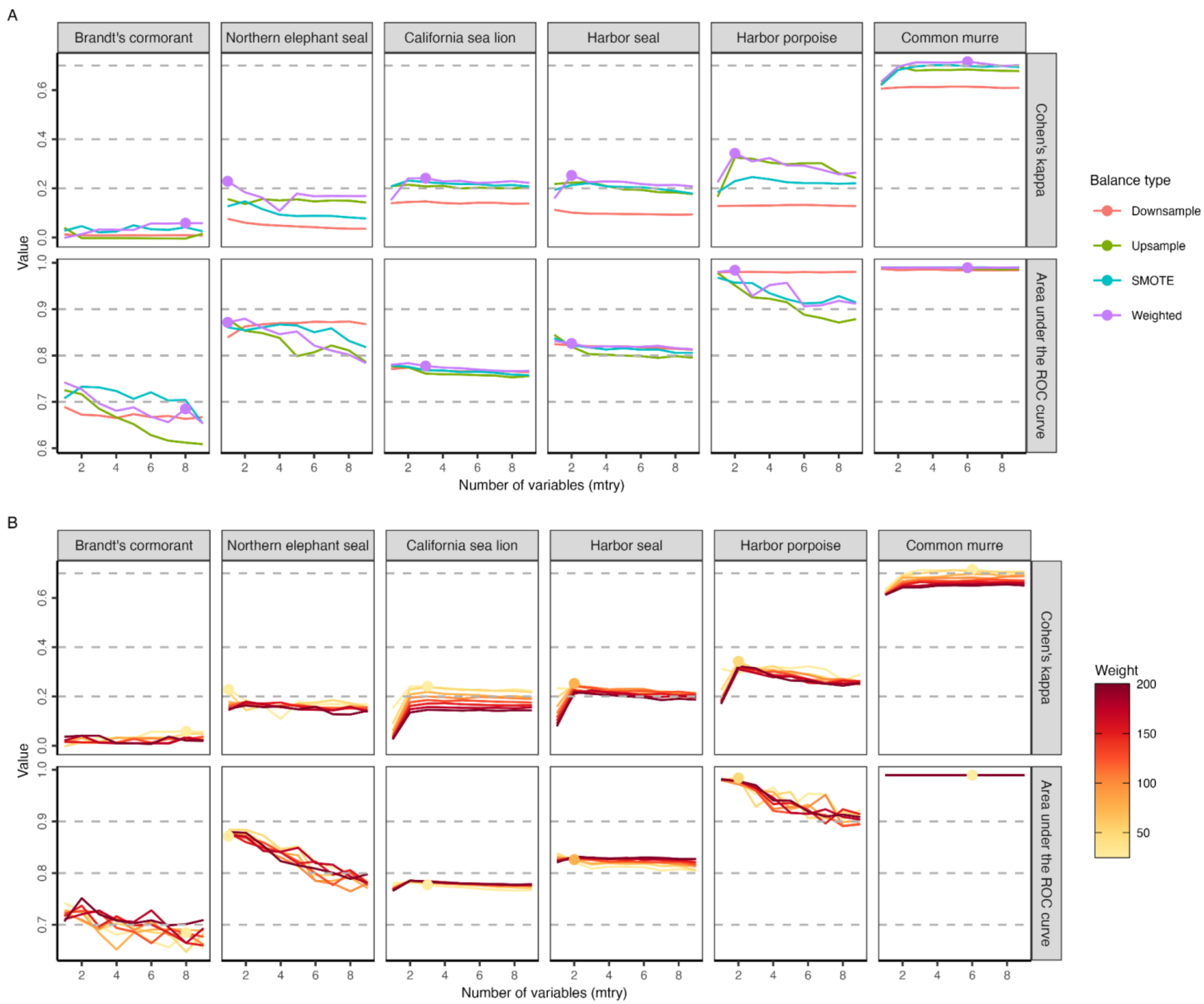

Fig. S13. A comparison of the model performance as measured through cross-validation on the training dataset between (**A**) balanced random forest modeling approaches and (**B**) weighted random forest modeling approaches for all candidate bycatch species (i.e., observer records > 50). We evaluated the model performance using Cohen's kappa and area under the receiver operator curve (ROC) and selected the model with the highest Cohen's kappa as the best performing model (labeled in the plot). We excluded Brandt's cormorant in the model prediction as their Cohen's kappa (<0.1) is too low to produce reliable predictions.

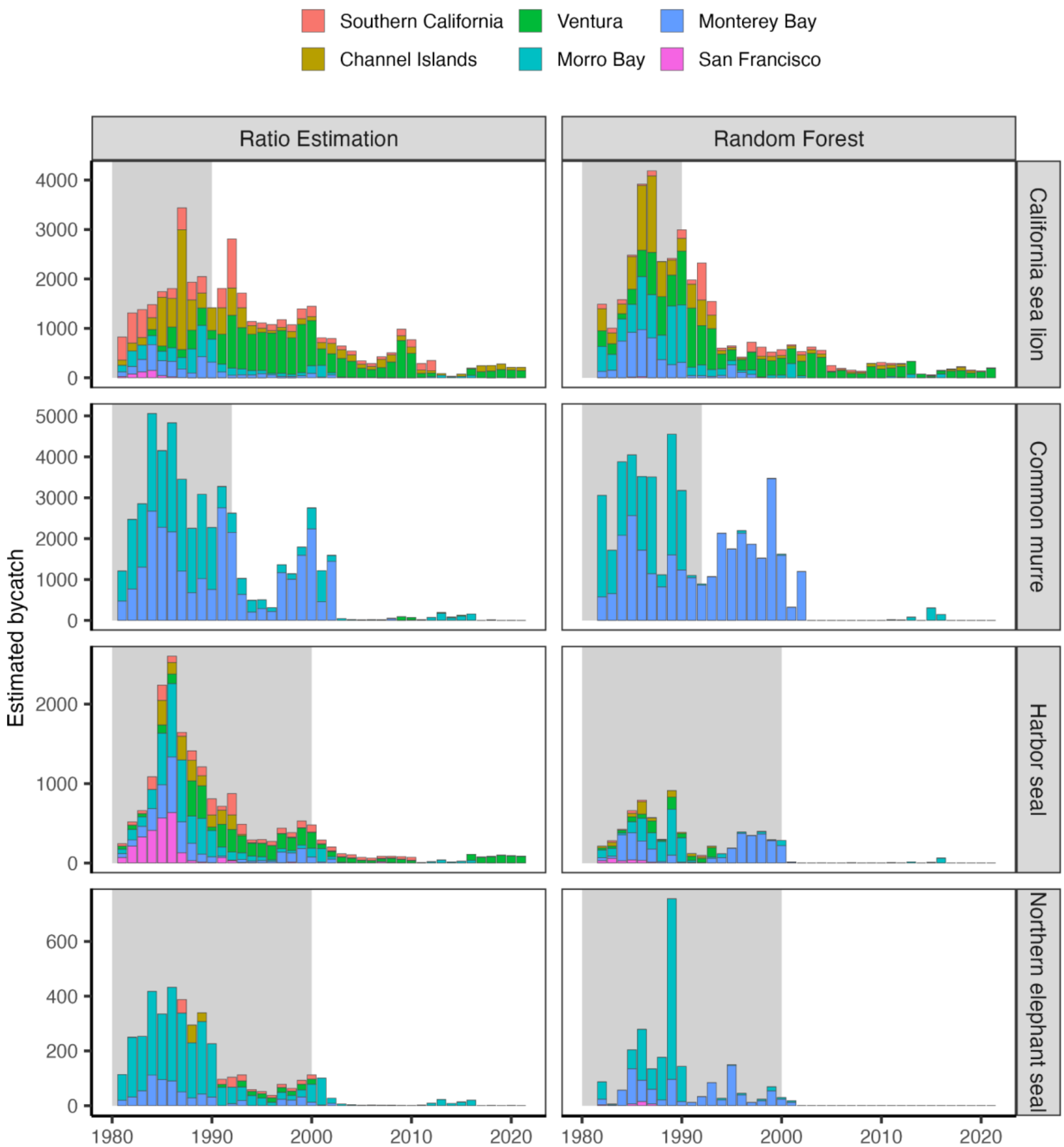

Fig. S14. A comparison of estimated bycatch numbers between ratio estimation and random forest stratified by regions (**Fig. S8**). The major differences between years are highlighted in gray.

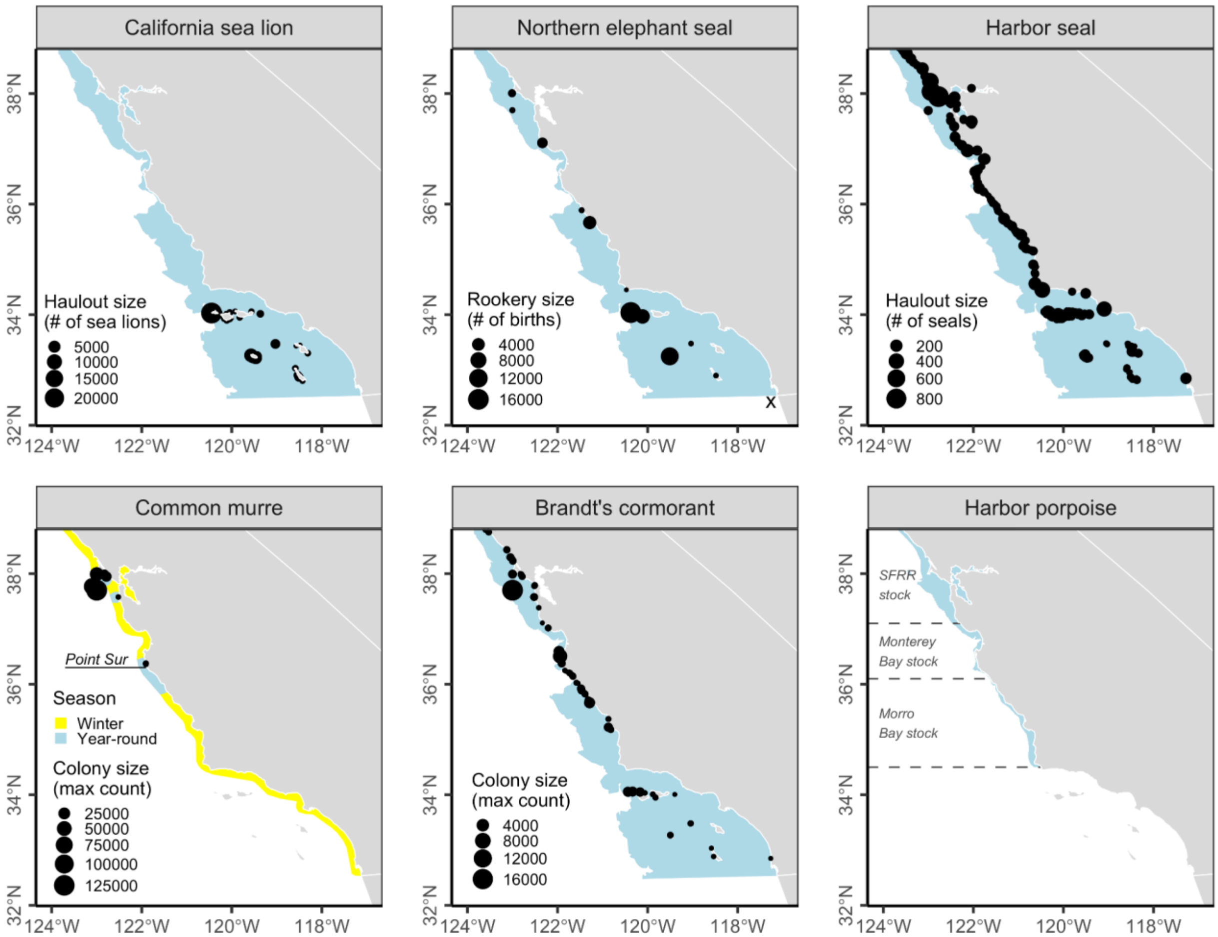

Fig. S15. Ranges of the six study species and information on haulout and colony size for selected species. Blue colors indicate year-round ranges and yellow colors (common murre only) indicate winter ranges. The range maps for all species except harbor porpoise are from the California Wildlife Habitat Relationship System (CDFW, 2021). Harbor porpoises occur in waters less than 50 fathoms (92 meters) deep north of Point Conception (Forney et al., 2014). SFRR indicates the San Francisco-Russian River harbor porpoise stock. Common murre and Brandt’s cormorant colony counts are from the California Seabird Colony Database (CDFW, 2010). There are no historical breeding records for common murre south of Point Sur except for at the Prince Island colony in Cuyler Harbor of San Miguel Island. Harbor seal haulout counts are from the CDFW Harbor Seal GIS dataset (CDFW, 2014).

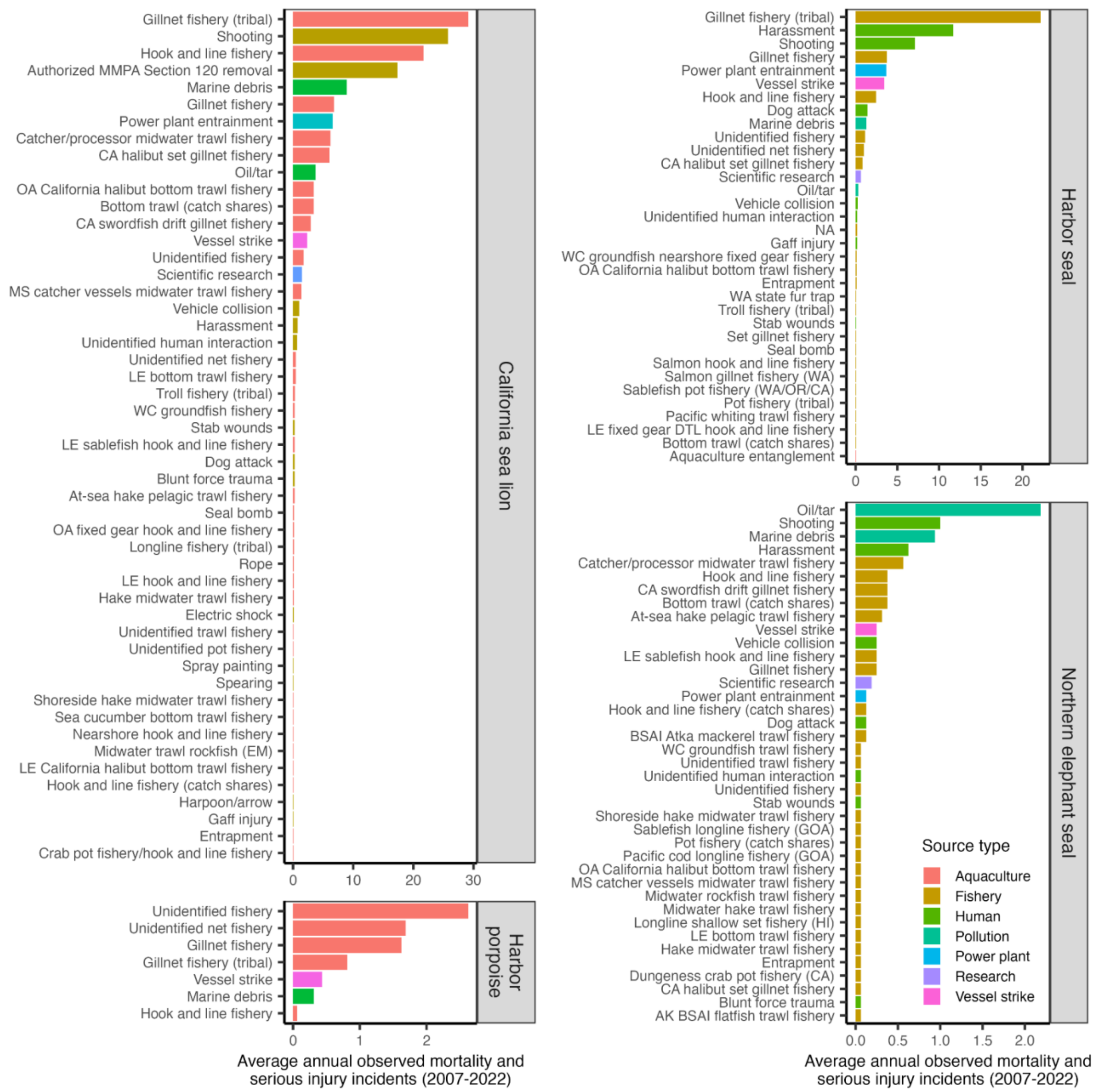

Fig. S16. Average annual observed mortality and serious injury incidents by source on the entire U.S. West Coast between 2007-2022 (Carretta, 2023).

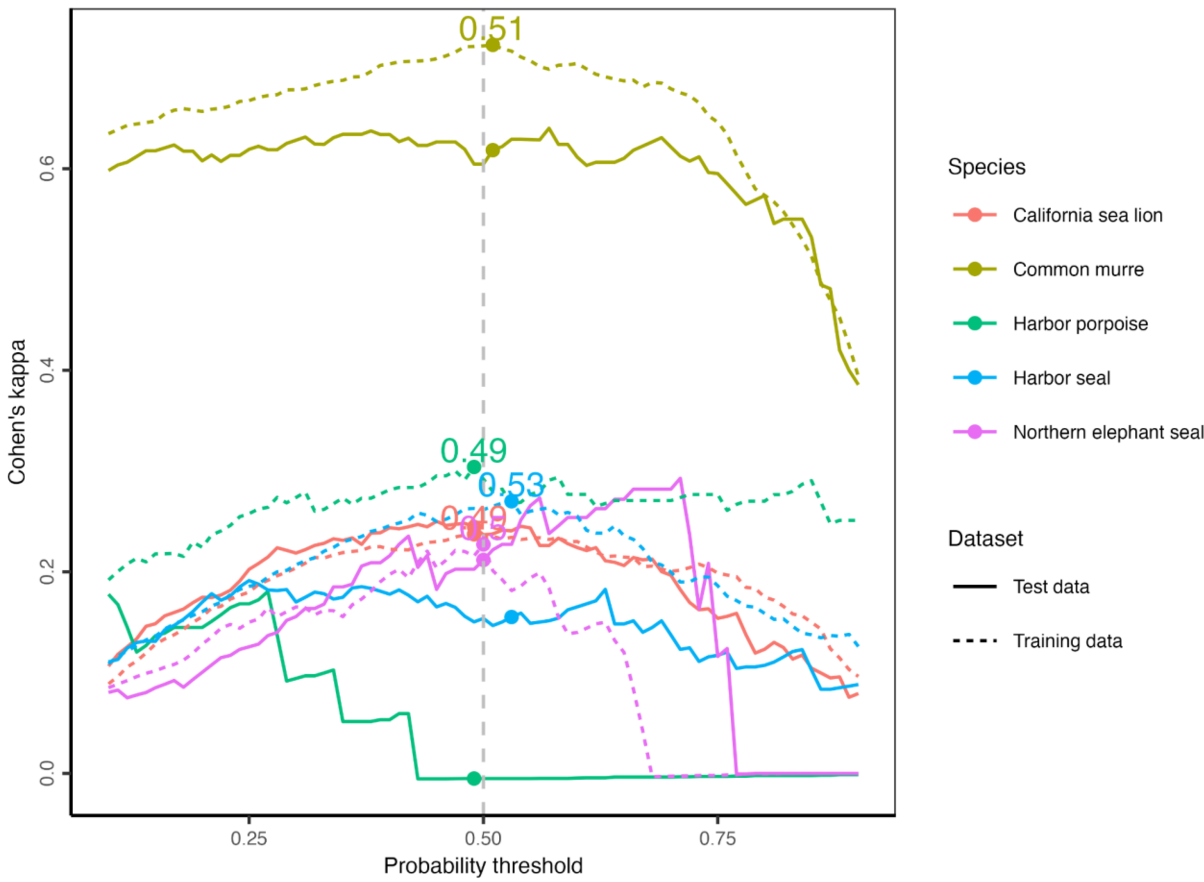

Fig. S17. Illustration of the methods used to select a probability threshold for classifying a logged set as having or not having bycatch. We selected the probability threshold that maximizes Cohen's kappa when applied to the training data as the optimal threshold (labeled in plot). We highlight the performance of this threshold when used on the independent test data to illustrate performance on out-of-sample data.

Highlights

- We evaluate protected species bycatch in the California ≥3.5 inch set gillnet fishery
- Bycatch has declined precipitously due to management and reduced fishing effort
- Recent marine mammal bycatch ranges from 0.1-4.0% of the potential biological removal
- Targeted time-area closures reduce bycatch better than mesh size or soak time limits
- Competing multiple modeling approaches improves predictions of rare bycatch events

Estimates and drivers of protected species bycatch in the California set gillnet fishery

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Declaration of Interest Statement

The authors declare no conflicts of interest related to this work.

Declaration of interests

☐The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

☒The authors declare the following financial interests/personal relationships which may be considered as potential competing interests:

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CRediT author statement

Yutian Fang: Conceptualization; Methodology; Formal analysis; Investigation; Data Curation; Writing - Original Draft; Writing - Review & Editing; Visualization. **James V. Carretta:** Methodology; Data Curation; Writing - Review & Editing. **Christopher M. Free:** Conceptualization; Methodology; Formal analysis; Investigation; Data Curation; Writing - Original Draft; Writing - Review & Editing; Visualization; Supervision; Project administration; Funding acquisition.