A statistical censoring approach accounts for hook competition in abundance indices from longline surveys

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Abstract

Fishery-independent longline surveys provide valuable data to monitor fish populations. However, competition for bait on the finite number of hooks leads to biased estimates of relative abundance when using simple catch-per-unit-effort methods. Numerous bias-correcting instantaneous catch rate methods have been proposed, modelling the bait removal times as independent random variables. However, experiments have cast doubts on the many assumptions required for these to accurately infer relative abundance. We develop a new approach by treating some observations as right-censored, acknowledging that observed catch counts are lower bounds of what they would have been in the absence of hook competition. Through simulation experiments we confirm that our approach consistently outperforms previous methods. We demonstrate performance of all methods on longline survey data of 11 species. Accounting for hook competition leads to large differences in relative indices (often −50% to +100%), with effects of hook competition varying among species (unlike other methods). Our method can be applied using existing statistical packages and can include environmental influences, making it a general and reliable method for analyzing longline survey data.

Key words: abundance index, relative abundance estimation, statistical censoring, species distribution modelling, stock assessment

Introduction

Monitoring changes in population abundance is essential for the successful management of fisheries (Hilborn and Walters 2013; Kuriyama et al. 2019). Estimates of abundance trends are frequently made using data collected from fishery-independent surveys and these estimates are subsequently used as inputs for formal stock assessments to advise fisheries management on harvest controls (Gunderson 1993; Hilborn and Walters 2013). Longline surveys play a crucial role in the management of many demersal fish species, such as rockfish species that frequently inhabit rocky and untrawlable habitats (Rodgveller et al. 2011; Obradovich 2018). For longline surveys, the commonly used catch-per-unit-effort (CPUE) method typically considers catch counts scaled by the number of baited hooks deployed (e.g., Anderson et al. 2019). Additional factors such as hook spacing, hook type, and soak time may also need to be considered (Yamanaka et al. 2008; Monnahan and Stewart 2018). However, using CPUE to estimate the local abundances of fish species is associated with numerous challenges (Obradovich 2018).

The challenges with longline fishing data originate from processes that sequentially occur between the deployment of longline gear and the subsequent capture of each fish. In particular, captured individuals must be (1) close to the baited hooks, (2) sufficiently attracted by the bait odour to move towards the hooks (Løkkeborg et al. 1995; Sigler 2000), (3) able to reach a baited hook before it is taken by another animal and before the end of the soak time (Rothschild 1967; Somerton and Kikkawa 1995), (4) unable to escape capture by the hook, and (5) able to avoid predation by scavengers for the remainder of the soak time (Ward et al. 2004). Each of these processes can vary between deployments. For example, bait attractiveness is linked to bait type and size (Løkkeborg and Bjordal 1995), hunger levels of the fish (Løkkeborg et al. 1995), temperature (Stoner et al. 2006), availability of natural prey (Stoner 2004), light intensity (Stoner 2003), time of day (Fernö et al. 1986; Ward et al. 2004), and currents (Fernö et al. 1986). Therefore, to link changes in observed catch counts from longline surveys to true changes in the abundance of a species, one must account for the variability due to heterogeneous bait attractiveness, heterogeneous soak times, hook escape, the removal of captured fish by scavengers, interspecific and intraspecific competition for finite hooks, and gear saturation (Sigler 2000). Although carefully designed long-

line surveys with standardized sampling protocols (e.g., fixed gear and bait types used from year to year) can help control for some of these factors (Gunderson 1993; Hilborn and Walters 2013), survey design alone cannot remove the biasing effects that hook competition and gear saturation have on current estimators of abundance (Rothschild 1967; Ricker 1975; Somerton and Kikkawa 1995; Kuriyama et al. 2019).

A desirable method for estimating relative abundance should simultaneously have two fundamental properties. First, when the true abundance is stationary, the long-run frequency in which the method falsely infers a change in abundance should be controllable by the fisheries scientist (a controllable type I error rate, hereafter referred to as the type I property). Second, it should identify, with high probability, a change in abundance when it occurs, and accurately estimate the magnitude of the change (related to the type II error rate and thus called the type II property hereafter).

Theoretical examples demonstrate why unadjusted CPUE indices of relative abundance exhibit neither the type I nor the type II properties. For the type I property, consider a standardized annual longline survey that catches species A and species B. For all fishing events, suppose that the same number of baited hooks are deployed and that all baits are taken by the end of each soak time. Both species are caught throughout the soak times of the fishing events. If the absolute abundance of species A is constant from year to year but species B increases, the expected CPUE of species A will decrease with time (violating the type I property), due to the increased competition from species B for the finite hooks during the fishing events in the later years. Indeed, the longline CPUE of a species can be highly sensitive to the density of competing species (Rothschild 1967). The magnitude of the expected decline in CPUE of a species will depend upon its ability to locate baited hooks ahead of the competing species. Numerous biological characteristics, such as its aggressiveness and olfactory sensitivity, will determine this ability (Sigler 2000).

For the type II property, consider a standardized longline survey over 2 years for which only one species is caught. Ignoring variability for simplicity, suppose each fishing event deploys 1000 baited hooks and catches 800 fish in year 1. Suppose the abundance of the species doubles in year 2. Despite the true abundance having increased by 100%, the limited number of baited hooks available to the fish prevents the estimated abundance from an unadjusted CPUE index from increasing by more than 20%. This is called gear saturation and as a consequence, relative abundance estimates in year 2 will be negatively biased (less than the true change in abundance).

The processes leading to the negative biases in the above two examples are called hook competition and gear saturation (hereafter simply both referred to as hook competition). Not all baits need to be removed during a fishing event for biases due to hook competition to creep into CPUE estimates of relative abundance. The ability of an attracted fish to locate a baited hook may decrease as baits are removed from the fishing gear, possibly once a threshold proportion of baits has been removed (Sigler 2000). The threshold is ex-

pected to be species-specific and depends on numerous biological factors of the fish species (Sigler 2000). Thus, CPUE estimates of relative abundance may poorly correlate with the true species abundance in regions where total fish density is high (relative to the number of baited hooks deployed) and for species that travel in large groups. This result has been determined through mathematical derivation (Rothschild 1967; Somerton and Kikkawa 1995), observed in simulation studies (Kuriyama et al. 2019), observed in experimental settings with observations taken from submersible remote operated vehicles (ROVs; Obradovich 2018), and confirmed through the careful analysis of longline survey data (Rodgveller et al. 2008).

Numerous estimators of relative abundance have been put forward with the aim of better controlling for bias due to hook competition (Rothschild 1967; Ricker 1975; Somerton and Kikkawa 1995). All of these methods focus on modelling the fate of each baited hook present in a longline fishing event. In particular, by deriving a system of fixed-rate ordinary differential equations (ODEs) that closely follows the intuition behind the Baranov catch equation, the bait removal times from each species are modelled (Gulland 1955). The target of inference for each species becomes their fixed instantaneous catch rate (ICR) parameter, defined as their expected rate of bait removal from a hook per unit time.

Various statistical distributions have been proposed to account for the random variability observed in the data that is not explained by the system of ODEs. These include models for the bait removal times when hook timer data are available, and models for binary catch/no-catch events, useful in standard longline fishing scenarios (see Obradovich 2018 for a detailed discussion). Maximum likelihood and Bayesian approaches have been derived and methods have been developed to control for correlations between hooks on the same longline skate and to adjust for hook-level covariates (Somerton and Kikkawa 1995; Obradovich 2018). Finally, a link between a species' ICR and its underlying abundance is made by assuming a functional relationship between the ICR and its abundance, often assumed to be linear (Obradovich 2018).

Three assumptions are crucial for ICR-based abundance indices to successfully describe the relative abundance of a species through time (Obradovich 2018), but are generally not met in practice. First, the ICR is assumed to be constant throughout the soak time of a fishing event. However, studies where the exact bait removal times were recorded (e.g., using hook timers or ROVs) demonstrated time-varying rates in numerous species, including sablefish (*Anoplopoma fimbria*; Løkkeborg et al. 1995; Sigler 2000), pelagic armorhead (*Pentaceros richardsoni*; Somerton and Kikkawa 1995), yelloweye rockfish (*Sebastes ruberrimus*), and quillback rockfish (*Sebastes maliger*; Obradovich 2018). Second, the expected catch count is assumed to be linearly proportional to the number of hooks (required due to the assumed independence between the bait-removal events). However, experiments have demonstrated that increasing the number of hooks on a longline significantly decreases the average catch per hook for both Pacific halibut (*Hippoglosus stenolepis*, Skud and Ham-

ley 1978) and sablefish (Sigler 2000). Third, the ICR is assumed to be linearly proportional to the true species abundance locally around the longline gear. However, results from experiments that compared direct estimates of abundance from ROV-mounted cameras with ICR-based abundance indices have been inconsistent with the assumption of linearity (Rodgveller et al. 2011; Obradovich 2018).

Under the above three assumptions, and assuming that bait removal times are exponentially distributed, the maximum likelihood estimator of the relative abundance equals the CPUE value multiplied by a nonlinear scale factor that increases with the total proportion of baits removed by all sources. The scale factor attempts to account for all the bait removals from fish of the target species that were "lost to observation" due to hook competition by enumerating the expected number of "missed" catches. Crucially, the scale factor inflates the ICR estimator of relative abundance away from the CPUE value by an amount that increases with the total catch of nontarget species. This ICR estimator has been proposed as a corrective offset term within a modelling framework (Webster et al. 2011). We refer to this method as the ICR method.

When species' true ICRs are time-varying within the soak time, the ICR method is biased and fails to attain the type I property. To demonstrate why, suppose standardized longline fishing gear with 1000 hooks is deployed in each of 2 years. Let there be two species present, with 990 fish of species A caught each year and 0 and 9 fish of species B caught in years 1 and 2, respectively. Suppose no hook escape or mechanical bait loss occurs and consider time-varying ICRs whereby species A always reaches a bait ahead of species B during a soak time. Given such perfect knowledge, we would not infer a change in species A's abundance from year 1 to year 2. However, from the catch data an ICR method's estimate of relative abundance assuming time-constant ICRs would estimate species A's abundance to increase by 50% (see Supplementary Material).

These demonstrations of the CPUE and ICR methods failing to attain the type I and type II properties motivate us to derive a new censored method whose performance is robust. The censoring effectively treats a subset of the observed catch counts (deemed to have been affected by hook competition) as lower bounds of what the catch counts would have been in the absence of hook competition. The prevalence and effect of hook competition are dependent on the species of interest. We conduct extensive simulation experiments covering multiple realistic data-generating scenarios and find that (i) the CPUE and ICR methods fail to satisfy the type I property in many realistic settings and (ii) our proposed method satisfies the type I and type II properties in most settings. We apply our method to real data collected for multiple species on an annual longline survey. We estimate relative abundance indices (often used as inputs to formal stock assessment models), finding large differences between indices from the CPUE and ICR methods and from our new method (often by −50% to 100%, sometimes up to 150%). Thus, we recommend use of our new method in future applications.

For reproducibility, we supply all simulation and case study code in R (R Core Team 2022), plus the data files, as Supplementary Material (also available at https://github.com/joeno middlename/Censored_Longline_RCode). We provide short template code for implementing our method using each of three commonly used R packages: R-INLA (Rue et al. 2009; Lindgren et al. 2011; Lindgren and Rue 2015), TMB (Kristensen et al. 2016) via sdmTMB (Anderson et al. 2022), and Stan (Carpenter et al. 2017) via brms (Bürkner 2017). The R-INLA and sdmTMB R packages were updated specifically to implement our censored method.

Methods

To develop our new modelling approach, we first build a flexible process model to describe the spatial density of fish. Next, we develop an observer model for linking observed catch counts collected using longline gear to the process model in an idealized setting without hook competition. We then modify the resulting likelihood function by treating hook competition as a data-quality issue. The catch counts from fishing events experiencing sufficiently high bait-removal levels are treated as right-censored (i.e., treated as lower bounds to the truth). This allows us to account for hook competition without needing to specify its precise nature or magnitude. We finish the section with descriptions of the simulation experiments and the case study using real data.

Mechanism describing the densities of fish

Let Ω denote our spatial region, $\mathbf{s}$ a point in the region, ω a subregion within Ω, and $t = 1, 2, ..., T$ denote years (notation is summarized in Table 1). The density $\lambda_{it}(\mathbf{s})$ defines the expected number of individuals of species i found per unit area of the seabed around location $\mathbf{s}$ at an arbitrary moment in year t. As is frequently done in the fisheries literature (Thorson et al. 2015, 2016; Grüss and Thorson 2019), we use the following general model:

$$(1) \qquad \log \lambda_{it}(\mathbf{s}) = \alpha_{it}(\mathbf{s}) + Z_{it}(\mathbf{s}),$$

where $\alpha_{it}(\mathbf{s})$ captures the overall effect of measured covariates (such as temperature and other environmental influences) affecting the population density across space and time, and $Z_{it}(\mathbf{s})$ is a stochastic term that captures the overall effect of unmeasured (perhaps unknown) sources of variability. Specifically, $Z_{it}(\mathbf{s})$ captures residual spatial, temporal, and spatiotemporal correlations due to, for example, unmeasured changes in predator abundance.

Next, consider K_t fishing events in year t, indexed by $k = 1, 2, ..., K_t$, with associated spatial regions ω_{tk}. Define random variable N_{itk} as the number of fish of species i present within region ω_{tk} during fishing event k in year t, with mean, Λ_{itk}, therefore given by

$$(2) \qquad \Lambda_{itk} = \text{mean}(N_{itk}) = \int_{\omega_{tk}} \lambda_{it}(\mathbf{s}) d\mathbf{s}.$$

A statistical model must now be specified on N_{itk} and $Z_{it}(\mathbf{s})$, with N_{itk} conditional upon Λ_{itk} and $Z_{it}(\mathbf{s})$. We assume that the

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Table 1. List of main notation used.

Indices (subscripts)	
i	species index; $i = 1, ..., I$
t	year index; $t = 1, ..., T$
k	fishing event index (indexes a unique event each year); $k = 1, ..., K_t$
m	index of covariates affecting the expected densities of fish; $m = 1, ..., M$
r	index of covariates affecting response function of fish; $r = 1, ..., R$
Space–time quantities	
Ω	study region
$\mathbf{s}$	vector giving a point-reference location; $\mathbf{s} \in \Omega$
$\mathbf{s}_{tk}$	the point-reference location of fishing event k in year t
ω	a subregion of the study region; $\omega \subset \Omega$
ω_{tk}	an area associated with fishing event k in year t
ω_{itk}	area around $\mathbf{s}_{tk}$ within which bait odour may attract species i
Known quantities	
c_{itk}	observed number of species i caught during fishing event k of year t
$c_{\cdot tk}$	observed number of bait removals by all causes during fishing event k of year t
h_{tk}	number of baited hooks deployed during fishing event k of year t
p_{tk}	proportion of baits removed in fishing event k of year t
$p_i^*, \widehat{p}_i^*$	true and assumed breakdown point for species i; when a proportion of baited hooks $\geqslant$ the breakdown point is removed during a fishing event, the observed catch count of species i is assumed to be low quality
p^*	constant value of p_i^* used for all species in the experiments
$\widehat{p}^*$	shorthand for $\widehat{p}_3^*$ in the experiments, the only value of $\widehat{p}_i^*$ of interest
x_{itkm}	value of the measured covariate m for the density model (eq. 10)
w_{itkr}	value of the measured covariate r for the response function model (eq. 11)
Estimated quantities and random variables	
$\lambda_{it}(\mathbf{s})$	expected density of species i at location $\mathbf{s}$ at an arbitrary moment in year t
$\alpha_{it}(\mathbf{s})$	captures overall effect of known sources of variability on $\lambda_{it}(\mathbf{s})$
$Z_{it}(\mathbf{s})$	captures overall effect of unknown sources of variability on $\lambda_{it}(\mathbf{s})$
N_{itk}	number of fish of species i present within region ω_{tk}
Λ_{itk}	expected value of N_{itk}
e_{itk}	lognormal random noise to account for overdispersion in N_{itk}
C_{itk}	catch count of species i during fishing event k in year t, assuming no hook competition
μ_{itk}	expected value of C_{itk} (so assuming no hook competition)
$R_{itk}(\mathbf{s})$	response function of species i at location $\mathbf{s}$ during fishing event k of year t, loosely interpreted as the probability that a fish of species i located at $\mathbf{s}$ at the beginning of the soak time of fishing event k is attracted to the baits in the absence of hook competition
β_{i0}, β_{itr}	intercept (β_{i0}) and effect size (β_{itr}) for the response function model (eq. 11)
α_{itm}	effect of measured covariate m on $\alpha_{it}(\mathbf{s})$ in eq. 10, with intercept α_{it0}

N_{itk} are conditionally independent across the fishing events k (i.e., local depletion due to the fishing event does not occur).

The simplest choice of conditional count distribution is the Poisson distribution. For species that exhibit schooling behaviour, the variance of their catch counts N_{itk} may be far higher than expected from a Poisson distribution (and the signal-to-noise ratio far lower) due to increased overdispersion and (or) right-skewness. To account for both, a popular choice in ecology is to scale Λ_{itk} by an independent identically distributed (IID) lognormal white noise term, which we denote as e_{itk}, that is unique for every fishing event and species (Bulmer 1974; Engen et al. 2002). Incorporating this term is equivalent to adding IID normal random effects to eq. 1. Then

N_{itk} is conditionally distributed as

$$(3) \qquad [N_{itk} | Z_{it}(\mathbf{s}), e_{itk}] \backsim \text{Poisson}\left(\Lambda_{itk} e_{itk}\right),$$

where the square brackets denote "the probability distribution of" and | is "conditional upon".

Finally, we are often interested in an index of abundance of species i over the whole region Ω for years $t = 1, 2, ..., T$. This index can be calculated as

$$(4) \qquad \Lambda_{it\Omega} = \int_{\Omega} \lambda_{it}(\mathbf{s}) d\mathbf{s}.$$

Since longline fishing events catch an unknown fraction of the fish present around the longline gear, absolute values of $\Lambda_{it\Omega}$ will be unidentifiable. Thus, our inferential target will be relative values of $\Lambda_{it\Omega}$.

Observation model that accounts for fishing but not hook competition

To account for the longline fishing process, we link the underlying fish density model to a novel observation process. Fishing event k of year t deploys h_{tk} baited hooks at location $\mathbf{s}_{tk}$. Around $\mathbf{s}_{tk}$ is believed to be a species-specific region, ω_{itk}, where the bait odour is sufficiently high that a fish of species i located within ω_{itk} may be stimulated to seek a baited hook during the soak time.

Let $R_{itk}(\mathbf{s}) \in [0, 1]$ denote a response function, loosely representing the probability that an individual of species i present at location $\mathbf{s}$ at the beginning of the soak time of longline event k in year t will seek and remove a bait (and be caught by the gear) in the absence of any other fish or other sources of bait removal; $R_{itk}(\mathbf{s}) = 0$ for $\mathbf{s}$ outside ω_{itk}.

Assume the area of ω_{itk}, denoted $|\omega_{itk}|$, is sufficiently small such that throughout ω_{itk} the species density $\lambda_{it}(\mathbf{s})$ can be assumed constant, such that $\lambda_{it}(\mathbf{s}) = \lambda_{it}(\mathbf{s}_{tk})$. Then, from eq. 2 we have $\lambda_{it}(\mathbf{s}_{tk}) = \Lambda_{itk}/|\omega_{itk}|$. Next, we can model the observed longline catch count of species i from longline fishing event k in year t in the *absence of hook competition*, denoted C_{itk}, as

(5) $\qquad [C_{itk}|Z_{it}(\mathbf{s}), e_{itk}] \backsim \text{Poisson}\left(\mu_{itk} e_{itk}\right),$

where the expected catch count is

(6) $\qquad \mu_{itk} = \int_{\omega_{itk}} \lambda_{it}(\mathbf{s}) R_{itk}(\mathbf{s}) \mathrm{d}\mathbf{s}$

(7) $\qquad = \lambda_{it}\left(\mathbf{s}_{tk}\right) \int_{\omega_{itk}} R_{itk}(\mathbf{s}) \mathrm{d}\mathbf{s}$

(8) $\qquad = \dfrac{\Lambda_{itk}}{|\omega_{itk}|} \int_{\omega_{itk}} R_{itk}(\mathbf{s}) \mathrm{d}\mathbf{s}.$

If every fish of species i within ω_{itk} is caught, then we have $R_{itk}(\mathbf{s}) = 1$ within ω_{itk}, such that $\mu_{itk} = \Lambda_{itk}$ and eq. 5 reduces to eq. 3 with $N_{itk} = C_{itk}$; i.e., if every fish is caught then the number of fish within the region equals the number caught within the region. The function $R_{itk}(\mathbf{s})$ is known as a thinning function, with the resulting counts arising from a thinned point process (Watson et al. 2021). The function $R_{itk}(\mathbf{s})$ is therefore similar to catchability.

We assume that the true response function of species i does not change during the soak time of the fishing event as baits are removed from hooks and for now assume that there are enough baited hooks available for each attracted fish of species i (i.e., no hook competition). The function $R_{itk}(\mathbf{s})$ can depend on covariates, including various biotic and abiotic factors affecting the fish behaviours or the characteristics of the odour plumes (Sigler 2000), such as average hunger levels (Rodgveller et al. 2008), numbers of baited hooks deployed and soak times (Sigler 2000), and currents (Fernö et al. 1986).

Extending the observation model to account for hook competition

To account for hook competition, we assume that fishing events with high proportions of baits removed are likely to suffer from hook competition, and label them as "low quality". We replace the catch counts from all low-quality fishing events with inequalities that imply the "true" observed catch counts could have been greater than the actual observed ones if hook competition was absent (some fish that would have been caught were not caught due to hook competition). In statistical terms, we are treating all low-quality fishing events as right-censored.

Specifically, we define the quantity p_i^* as the minimum proportion of the h_{tk} deployed baits that need to be taken during a fishing event for us to deem it low quality for species i. For simplicity, p_i^* is allowed to be unique for each species but is fixed across t and k. The term p_i^* denotes a breakdown point at which the assumed model (eq. 5) and (or) its assumptions no longer adequately describe the observed catch counts. Hook competition strictly *reduces* the chances of attracted fish of species i from being caught once p_i^* is reached. So the observed catch counts (which are affected by hook competition) will be underestimates of the C_{itk} from eq. 5, which does not consider hook competition. We do not need to know the causes of the bait removals that led to the breakdown (e.g., they can be due to combinations of fish, invertebrates, and mechanical failure), the reasons behind the decrease in species i's ability to locate a bait (e.g., they can be due to changes in the bait odour plume or avoidance of captured fish), nor the magnitude of divergence between eq. 5 and the expected catch count. Values of p_i^* are not estimable within the model and are assigned values $\hat{p}_i^*$; a method for assigning $\hat{p}_i^*$ is later developed, tested, and used in the case study.

For all high-quality fishing events, we assume that we perfectly observe the C_{itk} described by our assumed conditional Poisson model (eq. 5). Conversely, for low-quality fishing events, we assume that the observed catch counts of species i are right-censored and hence we only observe lower bounds on C_{itk}. Denoting the observed catch counts as c_{itk}, the high-quality observations can be written as events $C_{itk} = c_{itk}$ and the low-quality observations as $C_{itk} \geqslant c_{itk}$. Right-censoring reflects the failure of the measuring device (longline gear) to measure the local abundance of species i when hook competition is present.

To model the right-censoring, we keep track of the number of baits removed in each fishing event. Let $c_{\cdot tk} = \sum_i c_{itk}$ denote the known number of deployed baits removed in fishing event k of year t by all species, with $p_{tk} = c_{\cdot tk}/h_{tk}$ denoting the corresponding proportion of baits removed (and so $1 - p_{tk}$ is the proportion of hooks returned with bait). To account for the empty hooks (i.e., hooks where the baits were removed during the soak time, but retrieved without fish attached), we consider them as a unique species. Then, the conditional likelihood function for the observed catch count of species i in fishing event k of

year t is

$$
(9) \quad \pi(c_{itk}|Z_{it}(\mathbf{s}), e_{tk}) = \begin{cases} P(C_{itk} = c_{itk}) = f(c_{itk}; \mu_{itk}e_{itk}), & p_{tk} < p_i^* \text{ (high quality)}, \\ P(C_{itk} \geq c_{itk}) = 1 - F(c_{itk} - 1; \mu_{itk}e_{itk}), & p_{tk} \geq p_i^* \text{ (low quality)}, \end{cases}
$$

with $f(x; \mu)$ denoting the probability mass function, $P(X = x)$, and $F(x; \mu)$ the cumulative distribution function, $P(X \leq x)$, of a Poisson random variable X with mean parameter μ. Note that the likelihood function for censored zeros is exactly 1 (because $c_{itk} = 0$ and $P(C_{itk} \geq 0) = 1$ by definition) and so contributes no information to the overall likelihood. Thus, low-quality zero catch counts are effectively discarded.

In settings with very high levels of censorship and limited data, the above model may not converge numerically. To improve convergence in such situations, we can define a species-specific upper bound u_{itk} and consider an interval-censored event by modifying the second line of eq. 9 to $P(c_{itk} \leq C_{itk} \leq u_{itk}) = F(u_{itk}; \mu_{itk}e_{itk}) - F(c_{itk} - 1; \mu_{itk}e_{itk})$. In addition, we can also set the largest observed catch counts each year as "high quality" in eq. 9, regardless of the values of p_{tk}. We outline both approaches in the Supplementary Material and implement them in the case study; however, convergence issues appear to be rare if the model-fitting algorithm is started with initial values selected from a converged CPUE-based model with equivalent specification (considering all data as high quality and using eq. 9). Note that with or without these modifications, the response variables for all censored (low quality) observations are changed from (scalar) catch counts c_{itk} to intervals.

Fitting the models

The following choices of model components enable the above framework to be fit using existing software. First, a linear model for $\alpha_{it}(\mathbf{s}_{tk})$ is defined as

$$
(10) \quad \alpha_{it}(\mathbf{s}_{tk}) = \alpha_{it0} + \sum_{m=1}^{M} \alpha_{itm} x_{itkm},
$$

where α_{it0} is an intercept term and α_{itm} denotes the effect of measured covariate m (where $m = 1, 2, ..., M$) that takes value x_{itkm}, on the log mean density of species i around the fishing gear of event k in year t. The inclusion of measured covariates is optional, and if none are available then eq. 10 reduces to $\alpha_{it}(\mathbf{s}_{tk}) = \alpha_{it0}$.

Next, a log-linear model for the response function is defined as

$$
(11) \quad \log \int_{\omega_{itk}} R_{itk}(\mathbf{s})d\mathbf{s} = \beta_{i0} + \sum_{r=1}^{R} \beta_{itr} w_{itkr},
$$

where β_{i0} is an intercept term and β_{itr} denotes the effect of covariate r (where $r = 1, 2, ..., R$), that takes value w_{itkr}, on the log of the integrated response function of species i for fishing event k in year t.

Thus, taking the log of the expected catch count μ_{itk} in eq. 7 and using eqs. 1, 10, and 11 gives

$$
(12) \quad \log \mu_{itk} = \alpha_{it}(\mathbf{s}_{tk}) + Z_{it}(\mathbf{s}) + \log \int_{\omega_{itk}} R_{itk}(\mathbf{s})d\mathbf{s}
$$

$$
(13) \quad = \alpha_{it0} + \beta_{i0} + \sum_{m=1}^{M} \alpha_{itm} x_{itkm} + \sum_{r=1}^{R} \beta_{itr} w_{itkr} + Z_{it}(\mathbf{s}_{tk}).
$$

Lastly, $Z_{it}(\mathbf{s})$ is specified as a Gaussian process and e_{itk} as IID lognormal random effects. This leads to the catch counts arising from a log-Gaussian Cox process model (Watson et al. 2021). Then, given observed catch counts c_{itk} and known proportions of baits removed p_{tk}, the censored Poisson model defined by eqs. 9 and 13 can be fit using any preferred software package for fitting generalized linear mixed effects models. Estimates of the number of species i over region Ω in year t, $\Lambda_{it\Omega}$, can then be obtained by predicting $\lambda_{it}(\mathbf{s})$ from eq. 1 across a dense grid of points covering Ω, using either a maximum likelihood or Bayesian approach, and then approximating the spatial integral (eq. 4) with finite sums.

Other choices of distributions for $Z_{it}(\mathbf{s})$ and e_{itk} can be made, but custom code would need to be written. Finally, eq. 13 implies that the two intercept terms will not be identifiable from the model. Only their sum will be identifiable. Thus, as long as the response function intercept β_{i0} does not change over time and also the covariates x_{itkm} and w_{itkr} are unique, relative values of $\Lambda_{it\Omega}$ (up to a constant multiplicative factor) will remain identifiable.

For brevity, we refer to our new method, that involves fitting the censored Poisson-lognormal regression model defined by eqs. 9 and 13, as simply the "censored method".

Simulation study

To assess the relative performances of the CPUE and ICR methods against our censored method at inferring relative abundance, we simulate fish populations and data collected from a fishery-independent longline survey using standardized sampling protocols. Longline fishing events are performed at 30 or 100 stations across 6 years, each deploying 800 hooks with soak times of 5 h. No systematic interstation variability or spatial correlation is present, but there are important temporal trends. We create different experiments that explore a range of potential fish behaviour (e.g., schooling, aggressiveness, correlation in abundance between species), as well as whether only the target species is considered or two additional species groups remove the baits: a main competitor whose abundance increases in time, and an "other" species group that includes multiple species (and can include bait loss, etc.). In each experiment, we explore the type I and type II properties by either keeping the abundance of the target species constant or increasing it through time.

Bait-removal events are simulated by (i) sampling numbers of each of the three species groups from their abundance distributions, (ii) sampling the bait removal time of each fish from group-specific bite-time distributions, (iii) matching fish to the next available baited hook in ascending order of their sampled bite times. Each matched fish–hook pair results in a successful capture and bait-removal event with a capture probability that depends on the group-specific sensitivity to hook competition.

In particular, once the proportion of baits removed exceeds p_i^*, we decrease the probability of capture for species i. We then estimate relative abundance indices for only the target species, for which we specify $\widehat{p}_i^*$ in model fitting. Indices for years 2–6 are computed relative to year 1's abundance. For simplicity, we set the true value p_i^* to be the same for all species, however, a second parameter for each species group can change its sensitivity to hook competition. Note that typically $\widehat{p}_i^* \neq p_i^*$.

Assumptions and parameter values are varied to test the performance of the methods under multiple settings. Each assumption–parameter combination is repeated 100 times. The resulting six simulation experiments (which each contain various combinations of settings) are summarized in Table 2 and briefly described when we discuss their results; full details are given in the Supplementary Material, including model fitting details.

The CPUE, ICR, and censored methods' estimators are the posterior medians from fitted Poisson-lognormal models. We use identical model specifications to make the comparisons between the estimators fair (e.g., identical linear predictors, priors, software). The only exceptions are the conditional likelihood function and (or) ICR scale factor. Our above formulation of the censored method allows for inclusion of co-variates and spatial effects, but here we ignore those to fairly compare models, such that eq. 13 reduces to $\log \mu_{itk} = \alpha_{it0}$ (see Supplementary Material). We assess the performance of the methods by comparing their absolute estimation bias, estimation median squared error (MSE), and observed coverage of their posterior credible intervals. Bias close to zero is desired to demonstrate accuracy. But being an average from simulations, bias is not a guarantee of precision, for which MSE is shown, with low values preferred. Observed coverage is the percentage of the 100 95% credible intervals that cover the known value, with values close to 95% indicating accurate model-based uncertainty quantification. Robust 95% confidence intervals for bias and MSE (where the uncertainty arises from the 100 repeated simulations) are computed using the median absolute deviance from the median values to reduce the influence of outliers (Maronna et al. 2019), while 95% intervals of coverage are calculated using a normal approximation to the binomial probability (binomial because each interval either contains the true value or not).

Case study: International Pacific Halibut Commission fishery-independent setline survey

The International Pacific Halibut Commission (IPHC) fishery-independent setline survey is an annual longline survey spanning waters from California to Alaska, USA (IPHC 2022). While the survey's main goal is to provide data on Pacific halibut for stock assessment purposes, non-halibut species have been enumerated within Canadian waters off British Columbia since 1995 (see Supplementary Material for map). The survey provides valuable information on many non-halibut species in these waters, and for 32 species it yields the longest ongoing time series from all types of surveys (Anderson et al. 2019). Hook competition is believed to be high, with over 11% of the IPHC fishing events returning zero baited hooks (Anderson et al. 2019), but is not currently accounted for in Canadian analyses which generate inputs to stock assessments used to provide advice to fisheries managers.

We restrict our analysis to data collected on 11 species from fishing events between 1998 and 2021 at 171 stations within British Columbia waters, 109 of which were fished at every year in the time period. The IPHC data are amalgamated using the R package gfiphc (Edwards et al. 2022).

The stations are located at the intersections of a 10×10 nautical mile square grid. At each station, fishing gear consisting of a set of up to eight skates is deployed, with each skate containing around 100 hooks. For each set, the IPHC calculates an effective skate number to account for variations in numbers of hooks, hook spacing, or hook type (Yamanaka et al. 2008). An effective skate of one attempts to represent a skate of 100 circle hooks with 18 ft spacing (Yamanaka et al. 2008). Consistent baits are used every year except 2012, so for our case study (but not in applications such as Anderson et al. 2019) we remove catch data from 2012. We consider all catch count data at the set level. Species-specific values for $\widehat{p}_i^*$ are determined using methods suggested by the simulation study's results (detailed in the Supplementary Material).

The 11 species are chosen due to their high average catch counts. There are three rockfish species: redbanded rockfish (*Sebastes babcocki*), shortspine thornyhead (*Sebastolobus alascanus*), and yelloweye rockfish; three roundfish species: lingcod (*Ophiodon elongatus*), Pacific cod (*Gadus macrocephalus*), and sablefish; three elasmobranchs: big skate (*Beringraja binoculata*), longnose skate (*Raja rhina*), and North Pacific spiny dogfish (*Squalus suckleyi*); and two flatfish species: arrowtooth flounder (*Atheresthes stomias*) and Pacific halibut.

Current analyses for British Columbia groundfish compute a CPUE value for each fishing event at each set by calculating the number of individuals of that species caught divided by the effective skate of the set (Edwards et al. 2017; Yamanaka et al. 2018; Anderson et al. 2019). Relative abundance indices with 95% confidence intervals are computed by bootstrapping the observed CPUE values within each year (Efron 1987). Preliminary investigation of hook competition, based on the methods of Clark (2008) and Webster et al. (2011), showed it to be important in the data for British Columbia groundfish (Anderson et al. 2019), providing further motivation for the present study.

We compute Canadian coastwide relative abundance indices of the 11 species using the CPUE, ICR, and censored methods, plus bootstrapped raw CPUE values (similar to methods currently used). The focus is to produce annual relative abundance indices that could be used as inputs for for-

Table 2. Brief descriptions of the type I and II properties, methods, and simulation experiments.

Desirable properties	
Type I	Correctly infers a stationary abundance when the true abundance is stationary.
Type II	Correctly infers a changing abundance when the true abundance is changing and estimates the magnitude of change with high accuracy.
Methods	
CPUE	Catch-per-unit-effort, typically the catch count divided by the number of hooks. For the simulation study and case study, estimators and intervals quantifying uncertainty are derived from Poisson-lognormal regression models.
ICR	Instantaneous-catch-rate based. Raw CPUE values multiplied by the hook competition scale factor, assuming baits are removed by each species at independent exponentially distributed times. For the simulation study and case study, estimators and intervals quantifying uncertainty are derived from Poisson-lognormal regression models fit to adjusted CPUE values.
Censored	Estimates and intervals of uncertainty derived from Poisson-lognormal regression models. For high-quality fishing events exact CPUE values are used, and for low-quality events inequalities are used (lower bound set to CPUE value).
Experiments	
1	One schooling species is caught, sometimes present in very large groups in numbers far exceeding the number of available hooks (giving a right-skewed catch count distribution). The relative abundance is fixed through time to assess the type I property of the methods, and increased to assess the type II property. Having all baits removed ($p_{tk} = 1$) is commonplace.
2	Many species are caught (simplified as a target, a main competitor, and other species). The target species is nonschooling and never present in large groups. A main schooling competitor, whose abundance increases through time, is able to reach the baits ahead of the target species on average (under the "differing aggressiveness" setting). The other species group's abundance remains constant. The relative abundance of the target species is fixed through time to assess the type I property of the methods, and increased to assess the type II property.
3	The same setup as 2, but the target species' abundance distribution is overdispersed to represent occasional schooling behaviour, and allowed to be positively and negatively correlated with a competitor's. Settings are more realistic.
4	Similar to 2, but the breakdown point $\widehat{p}^*$ is very poorly specified to investigate robustness of the censored method to the misspecification of $\widehat{p}^*$.
5	The same setup as 3, but the target species now reaches the baits (on average) ahead of the competing species. The goal is to investigate the performance of the censored method in settings where it is not expected to perform well because the target species is largely immune to interspecies hook competition.
6	Regression models are fit to the species' catch counts to investigate the trend in the mean catch count as the proportion of baits removed during a fishing event increases from $p_{tk} \approx \widehat{p}^* $ to $p_{tk} \approx 1$. Multiple data-generating settings are considered. The goal is to identify an empirical approach for determining a suitable $\widehat{p}^*$ value.

mal stock assessments, and so we assume independence between years (rather than making assumptions such as random walks, smoothing splines, or polynomials). The bootstrapped CPUE values are computed using only the data collected at the 107 stations that recorded data each year, while the other methods use data from all 171 stations and attempt to correct for the differences in stations used each year with random effects, again with no covariates or spatial correlation. Each resulting time series is divided by its geometric mean to produce comparable relative abundances indices (see Supplementary Material for details).

Results

Simulation study results

Experiment 1 conducts proof-of-concept trials, simulating longline catch data of a single, highly abundant species where all baits are commonly removed during fishing events. Results confirm that the censored method is able to infer stationary and nonstationary abundance trends with high accuracy, high precision, and with coverage levels close to the target, even with frequently saturated fishing gear (see Supplementary Material). Results also confirm our earlier claim that the CPUE method fails the type II property when the de-

mand for baited hooks from only a single species frequently exceeds the number supplied by the longline gear. The ICR method performed worse.

Experiment 2 simulates multiple species (Figs. 1 and 2). The target species is nonschooling, its distribution is independent of the other species, and its local abundance is less than the number of hooks (800). The main competitor increases in abundance through time. Annual changes in abundance are larger than might be expected for real populations, to provide sufficient contrast to test the methods (slower changes can be inferred by simply considering units of time to be decades instead of years). A large increase in the main competitor in year 6 is designed to test the sensitivity of the methods to large changes in hook competition. The competitor individuals reach the baits, on average, at the same time as individuals from the target species when all species' bite times are simulated from uniform distributions, and ahead of the target species when the competitor's bite times are simulated from an exponential distribution. Thus, hook competition occurs and is sometimes severe. Values of p^* equal to 0.85 and 1 are used to generate the data, while $\widehat{p}^*$ values of 0.85 and 0.95 are assumed throughout.

Figures 1A–1H show that on average, the CPUE method underestimates the relative abundance of the constant target

Fig. 1. Bias results for experiment 2. Leftmost column shows the relative abundances for the target species and the main competitor in the simulations. The relative abundance of the main competitor determines the average proportion of baits removed for each fishing event. Whether the target species' abundance remains constant or increases establishes whether the simulations are investigating the methods' type I (red panels A–H) or type II property (blue panels I–P). The "other" species group's abundance remains constant (not shown). The four right columns indicate whether (i) the true breakdown point, which represents the proportion of baited hooks removed needed to trigger a decline in each species' ability to locate the remaining baits, is $p^* = 1$ or $p^* = 0.85$, and (ii) fish of all species arrive to the hooks at the same rate on average across the soak time ("Identical Aggressiveness"), or the target species is typically slowest to reach the hooks ("Differing Aggressiveness"). Results are shown for the CPUE and ICR methods, and for the censored method with assumed values for p^* of $\hat{p}^* = 0.85$ (Censored 0.85) and $\hat{p}^* = 0.95$ (Censored 0.95). Plotted are the median values of absolute bias, with values close to 0 (solid black line) desired; 95% confidence intervals are omitted since they are almost always smaller than the plotting symbols. Average levels of bias in estimates of relative abundance from the CPUE and ICR methods are high under most scenarios. On average, the level of bias in estimates from the censored method is close to 0 in all scenarios, even when p^* is misspecified ($\hat{p}^* \neq p^*$).
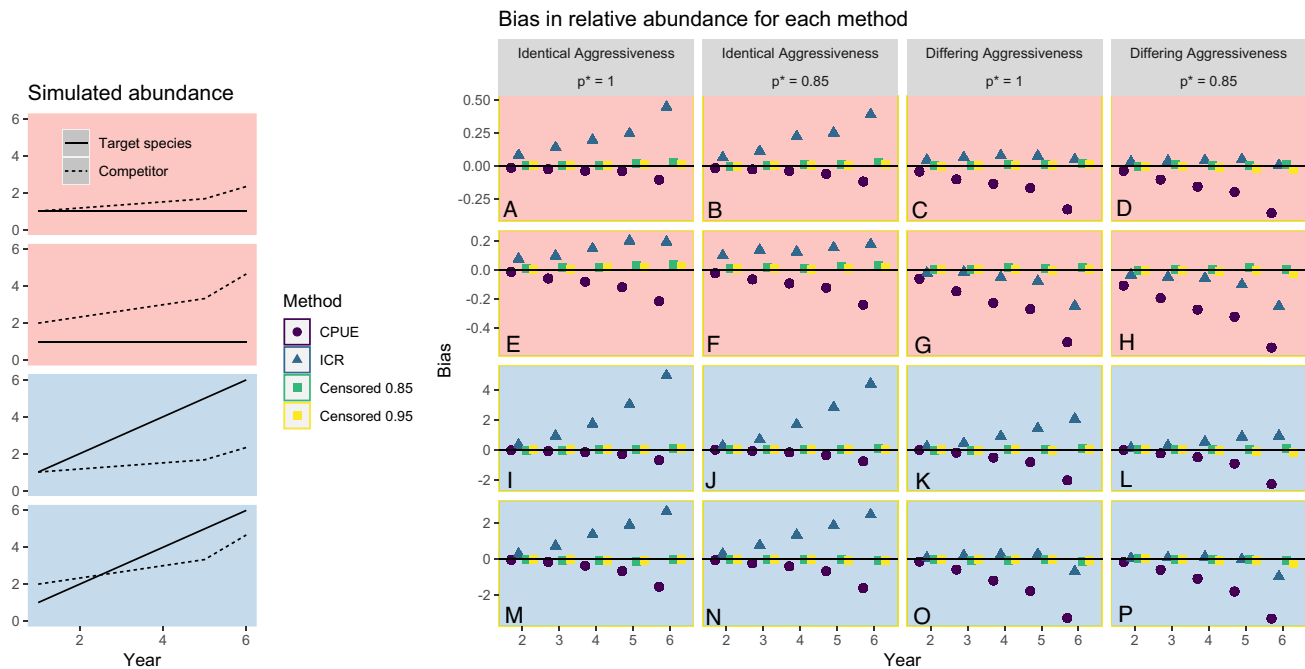
species and the ICR method overestimates it. Biases from the censored method are negligible. Furthermore, Figs. 2A–2H shows that the credible intervals of the censored method attain nominal coverage levels close to the target of 95%, while the coverage for the other two methods can fall below 50%. Thus, the censored method correctly identifies that the relative abundance of the target species is stationary (the type I property), whereas the CPUE and ICR methods frequently infer incorrect trends in relative abundance with high, but misplaced, confidence. The confidence is high because many credible intervals of relative change do not cover zero (the known true value because the target species' abundance is held constant), and so such intervals are confidently inferring (incorrectly) that the species' abundance is changing. Incorrectly specifying $\hat{p}^*$ (as 0.85 when $p^* = 1$, or as 0.95 when $p^* = 0.85$ or 1) appears to have a negligible impact on the type I property of the censored method.

Similarly, Figs. 1I–1P (investigating type II properties) show that the CPUE method typically overestimates the true change in relative abundance by up to 80% (+4.8 in year 6), and the ICR method underestimates it by up to 50%

(−3 in year 6). Again, the bias from the censored method is negligible (satisfying the type II property). Figures 2I–2P demonstrate that the censored method attains close to 95% coverage, unlike the other two methods where coverage can fall to 0%. Thus, the correct trends in abundance are rarely captured within the credible intervals from the CPUE and ICR methods. Again, misspecifying $\hat{p}^*$ for the target species does not negatively impact the performance of the censored method. For all scenarios considered, up to 100-fold reductions in MSE are observed for the censored method compared to the others (see Supplementary Material), further corroborating the censored method's improved performance.

Experiment 3 better exhibits the complexities of real-world data (Figs. 3 and 4). The target species' abundance distribution is overdispersed such that moderate schooling can occur (yielding occasional very large catches) and is allowed to be correlated with the abundance distributions of other species within fishing events. While $p^* = 0.85$ is used to generate the data, $\hat{p}^* = 0.95$ is incorrectly assumed for fitting the censored method. When the experiment was performed, the censored

Fig. 2. As for Fig. 1 but evaluating the nominal coverage of the 95% credible intervals for experiment 2. The mean coverage and its approximate 95% confidence interval is plotted for each scenario. Values close to 95% (the solid black line) are desired. Only the censored method attains close to 95% coverage, with the CPUE and ICR methods failing to satisfy both the type I property (red panels A–H) and the type II property (blue panels I–P) with respect to coverage.
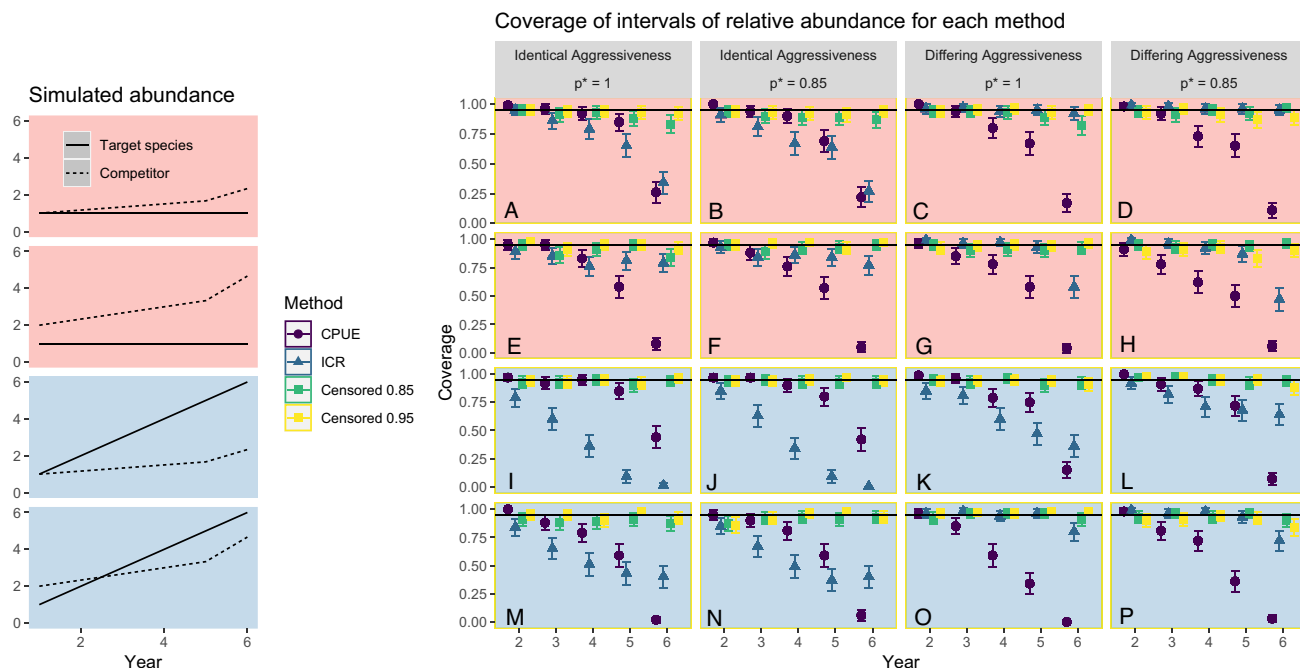

Fig. 3. Bias results for experiment 3. Leftmost column and the rows are interpreted as in Fig. 1. Remaining columns indicate the levels of correlation between the catch counts of the target species and the "other" species group. Plotted are the median values of absolute bias (confidence intervals are again omitted for clarity) from the 100 repetitions. The "Censored 0.95" method assumes the incorrect value $\widehat{p}^* = 0.95$ (while $p^* = 0.85$). The censored method infers changes in relative abundance with less bias than the other methods in almost all settings.
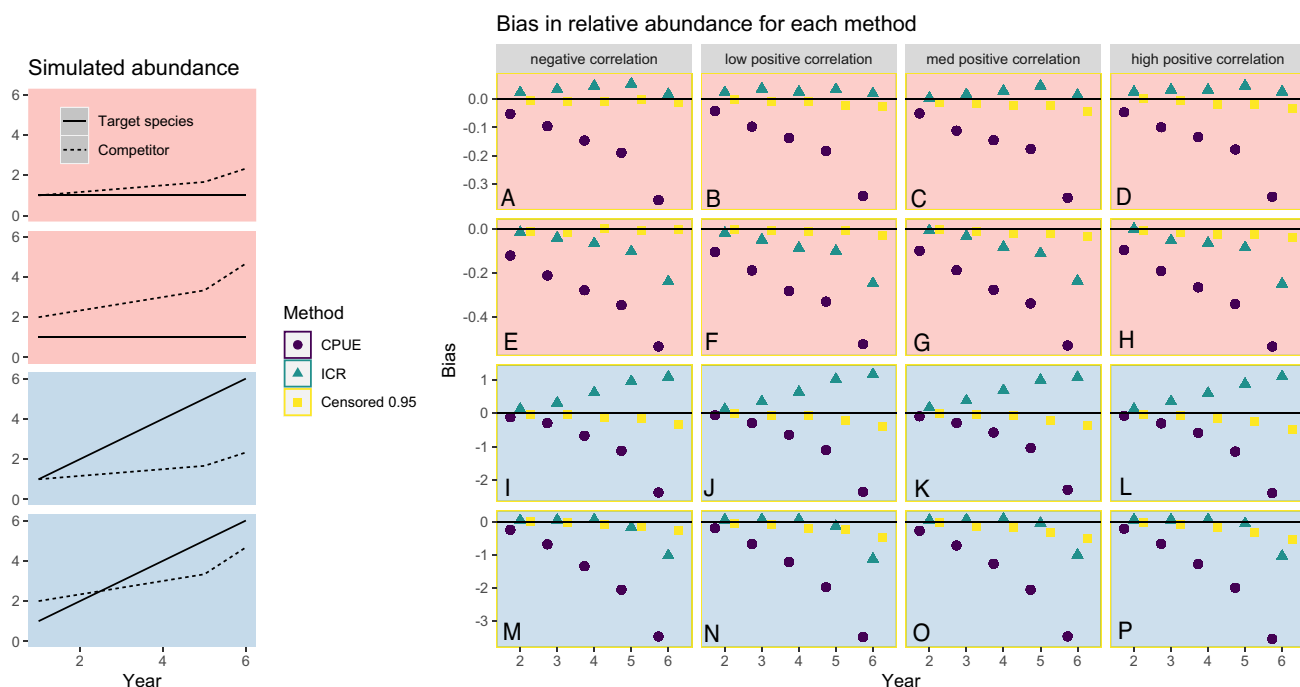
Fig. 4. As for Fig. 3 but evaluating the nominal coverage of the 95% credible intervals for experiment 3. The mean coverage and its approximate 95% confidence interval is plotted for each scenario. Only the censored method satisfies the type I property (red panels A–H). The censored method outperforms the other two with respect to the type II property (blue panels I–P), but falls short of fully satisfying it.
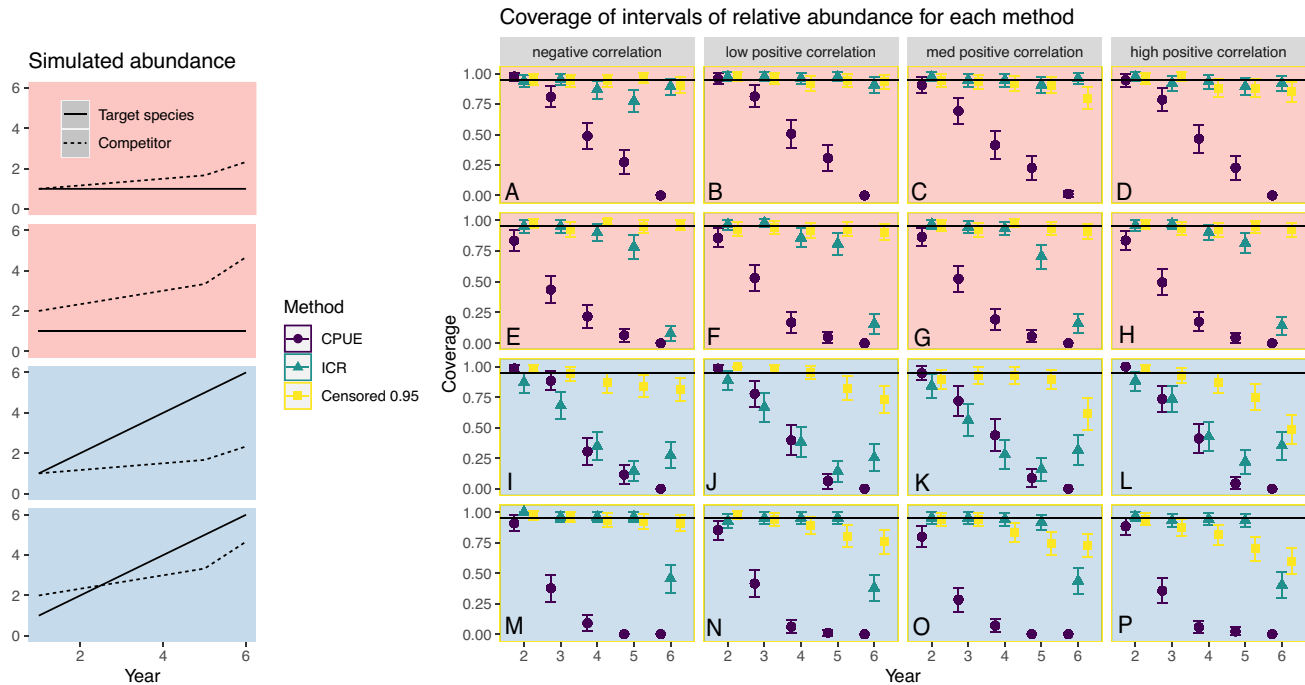
method failed to converge in over 20% of the runs. When this happened, we implemented additional modifications that assisted with the convergence and only slightly impacted performance. However, these convergence issues are much less problematic if the model-fitting routine is started with initial values chosen from the CPUE-based model, a fact only discovered after the simulation study was conducted. We present results only for the converged runs using the unmodified censored method (see Supplementary Material for modified censored results).

The censored method again outperforms the CPUE and ICR methods in terms of bias and coverage (Figs. 3 and 4). Crucially, the censored method is found to have superior type I (panels A–H) and type II (panels I–P) properties in these settings. Again, the magnitude of improvement in MSE offered by the censored method can be dramatic, especially when fishing gear is frequently saturated (see Supplementary Material).

A deterioration in the performance of the censored method occurs for experiment 3 when the target species' abundance distribution is positively correlated with the abundance distribution of the "other" species group (Figs. 4D, 4H, 4L, and 4P). This is to be expected—in such scenarios, the fishing events that would have recorded the largest catch counts of the target species are more frequently right-censored due to the higher probability of larger numbers of competitors being present around the longline gear (i.e., increased hook competition). This stochastic dependence between the probability of a fishing event being right-censored and the value

of the target species' abundance distribution is similar to the problem of informative censoring studied in survival analysis (Ranganathan and Pramesh 2012).

For experiment 4, we repeat experiment 2 but use $p^* = 0.5$ to generate the data. The assumed values of $\hat{p}^* = 0.85$ and 0.95 are thus greatly misspecified. The target species is acutely impacted by hook competition (unbeknownst to the scientist). The performance of the censored method is robust to this misspecification. Coverage close to 95% is attained for the censored method when the target species' abundance does not change over time (satisfying the type I property). When the target species' abundance changes over time, the coverage of the censored method declines, but never below 50%, whereas for the other two methods the coverage can fall below 10%; thus, the type II property remains superior for the censored method (though is not completely satisfied). The bias and MSE of the censored method are never worse than the CPUE and ICR methods and far better in most settings (see Supplementary Material).

Experiment 5 is purposefully designed to provide a scenario where the target species should be relatively unaffected by hook competition, and so the CPUE method should perform well (and the censored method's assumption of hook competition is violated). The target species (i) occurs in numbers far below the number of hooks (i.e., is nonschooling) and (ii) is, on average, able to reach the baited hooks ahead of the competing species groups. Thus, there are often enough hooks available for the target fish. We indeed find that the CPUE method performs best when fish numbers are low

(see Supplementary Material). However, this performance improvement is only slight when $\widehat{p}^* = 1$. Furthermore, as fish numbers increase (the proportion of baits removed increases), the censored method outperforms across all metrics once again. However, despite the censored method attaining good bias and MSE values in all settings, the coverage can fall below 30% in the most challenging settings (and lower still for the CPUE method). In summary, the censored method performs at least as well as the CPUE method for species that are largely "immune" to hook competition. However, reported estimates of uncertainty from all methods should be treated with caution as they are likely to be underestimates.

So, across experiments 1–5, the CPUE method is unsuitable for use in almost all simulation settings tested due to its high sensitivity to changes in the relative abundance of competing species. The one exception occurs when the target species is both nonschooling and able to reach baits ahead of competing species (experiment 5). The overall performance of the ICR method is worse in many settings. Most concerning is its consistent failure to satisfy the type I property; in some settings a change in abundance was incorrectly, but confidently, inferred over 85% of the time (namely when coverage falls below 15% in Figs. 4A–4H). Again, this is in agreement with the earlier theoretical example which highlighted the sensitivity of the ICR method to changing levels of nontarget species' abundances.

Conversely, the censored method performs well in almost every setting tested, consistently attaining both the type I and type II properties. Bias, MSE, and coverage are frequently improved, even when the data-generating mechanism is inconsistent with the modelling assumptions (e.g., when strong correlations exist between the species' abundance distributions). Perhaps most promisingly, the relative improvements in performance offered by the censored method appear to be highly robust to $\widehat{p}^*$ being poorly chosen. Although we have only considered a constant or increasing target species, the conclusions also hold for a decreasing target species, whether or not the main competitor is increasing or decreasing (see Supplementary Material).

We now investigate how a sensible value of $\widehat{p}_i^*$ could be chosen. We explore trends between the mean observed catch counts, c_{itk}, and the proportion of baits removed, p_{tk}, after controlling for time effects, in data simulated under numerous scenarios. The top panels of Fig. 5 show (i) a strongly decreasing trend between c_{itk} and p_{tk} for the target species of experiments 2–4, (ii) a negligible trend for the hook-competition-immune target species of experiment 5, (iii) a strongly increasing trend for the main competitor species under experiments 2–4. A line is drawn at $p^* = 0.85$ used to generate the data which shows that for (i), the decreasing trend begins close to p^*.

These observed trends between the mean observed catch counts and the proportion of baits removed motivate experiment 6. To assess whether the conclusions from the exploratory analysis generalize more broadly, we sample longline fishing events under a range of scenarios (see Supplementary Material). For each simulated dataset, we compare the mean observed catch count at high levels of p_{tk} ($p_{tk} \approx 1$) versus medium levels of p_{tk} ($p_{tk} \approx 0.85$).

Experiment 6 shows that a $>10\%$ decline in mean catch count from medium to high levels of p_{tk} almost always occurs for a species that is very slow to reach the baits and (or) is outcompeted by large schools of a competing species (bottom panels in Fig. 5). The exploratory analysis suggests choosing $\widehat{p}_i^*$ at the value of p_{tk} on the curve where the decreasing trend begins. Experiments 2 and 3 suggest that the censored method should perform very well across all metrics for such a species. Experiment 4 implies that the performance is robust to errors in p_i^* estimates. Conversely, experiment 6 shows that $>20\%$ increases typically only occur for strongly schooling species that frequently saturate the fishing gear (Fig. 5). Experiment 1, which considered that a species was solely responsible for saturating the fishing gear, suggests that the censored method will outperform competing methods with $\widehat{p}_i^*$ set to 1. However, estimates of uncertainty may be moderately underestimated. Lastly, a species with a small trend is expected to be largely immune to hook competition, either because it can reach baits in small numbers ahead of its competitors, or because there is an absence of a strongly schooling competitor. Experiment 5 considered such a species, and results suggest that $\widehat{p}_i^*$ be set to 1. However, levels of uncertainty may be greatly underestimated here. We later use these decision rules for selecting $\widehat{p}_i^*$ in the case study.

In all cases, if the censored method fails to converge numerically for a chosen $\widehat{p}_i^*$, then additional steps can be taken. First, if $\widehat{p}_i^*$ is set so low as to cause model identifiability issues, then $\widehat{p}_i^*$ can be increased (assuming $\widehat{p}_i^* < 1$). Experiments 2–5 suggest that small modifications (≈ 0.1) will have little impact on the quality of inference. Second, an upper bound can be placed on the catch counts (see Supplementary Material). Finally, experiment 3 (see Supplementary Material) suggests that relabelling the largest "low-quality" catch counts as "high-quality" can dramatically improve the convergence with little impact on the quality of inference. In practice, combinations of the above three approaches can be made. We explore the latter two in the case study.

Case study results

For the case study, 9 of the 11 species show statistically significant declines (exceeding 10%) of c_{itk} as p_{tk} increases, after controlling for the effects of time and for the differences in mean abundance across the fishing stations (Fig. 6). Of these, the decline appears to begin at $p_{tk} \approx 0.7$, $p_{tk} \approx 0.85$, and $p_{tk} \approx 0.95$ for 1, 3, and 5 species, respectively. The conclusions of experiment 6 suggest fixing $\widehat{p}_i^* = 0.7$, $\widehat{p}_i^* = 0.85$, and $\widehat{p}_i^* = 0.95$. Results from experiments 2–4 suggest the censored method will be able to infer the relative abundance of these species with high accuracy, high precision, and with accurate uncertainty estimates.

Shortspine thornyhead showed no significant trend between c_{itk} and p_{tk}, suggesting they may be relatively unaffected by hook competition in this data set. North Pacific spiny dogfish (hereafter just "dogfish") show a steady increase in c_{itk}, even accelerating as p_{tk} reaches 1, in agreement with our knowledge of them as a highly mobile schooling species and with the large increasing trends seen in schooling species' catch counts in experiment 6 (Fig. 5). Experiment

Fig. 5. Top: model-estimated trends between the mean observed catch count and proportion of baits removed in each of three representative datasets simulated under scenarios described in the panel titles. Vertical lines show true values of p^*; a change-point beginning at p^* can be seen in the leftmost trajectory. The two intervals with the darker shading define the medium and high hook saturation levels used in experiment 6. Bottom: boxplots of all the results in experiment 6, approximately computed as the relative values of the trend lines within the two shaded intervals. The horizontal dashed line shows where a 10% decline in mean catch count occurs (at the value 0.9 due to the multiplicative scale used). Colours point to the representative trajectories above. The linetype maps the scenario to the corresponding experiment (or lack of). Only the scenarios in the leftmost panel have been simulation tested. Declines of >10% are seen under scenarios matching experiments 2–4, whereas only small declines are seen under experiment 5's settings. Both the speed of a species to reach bait and its relative degree of schooling impact the trend.
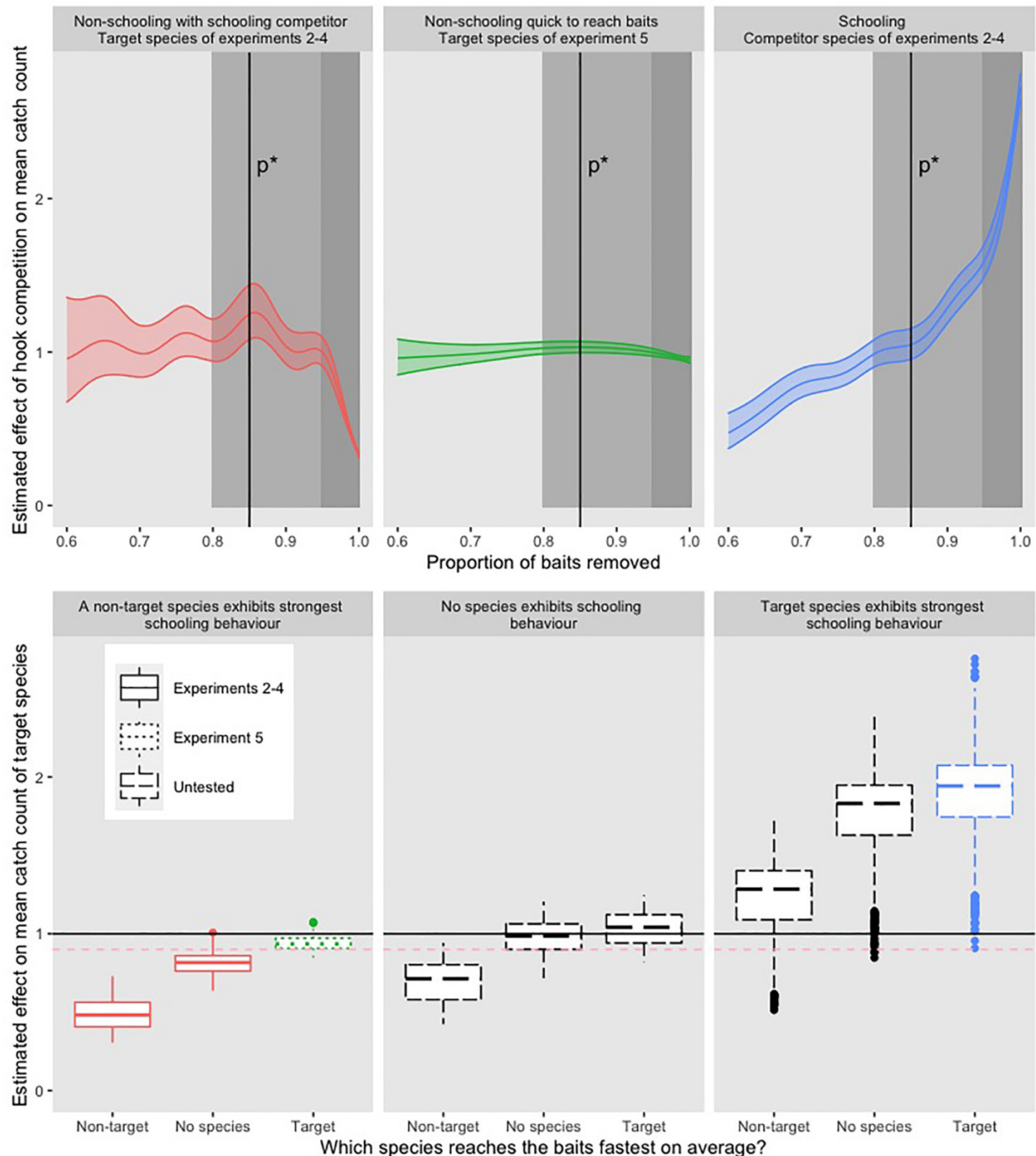
Fig. 6. Estimated effects of hook competition in the IPHC data (after controlling for temporal effects), shown as the relative change in the the mean catch count of each species as the proportion of baits removed increases. Effects are shown on the multiplicative scale to match those from Fig. 5. Models are fit to the catch counts from all of the fishing events. Vertical lines indicate the species-specific $\widehat{p}_i^*$ values chosen using the methods outlined in simulation experiment 6. All species experience a statistically significant decline in the mean catch count exceeding 10% (approximately a drop larger than 0.1 on the y-axis), except for dogfish, which experiences a statistically significant increase in mean catch count exceeding 10%, and shortspine thornyhead, whose estimated 1% decline is nonsignificant. For these two species results from experiment 6 suggest $\widehat{p}_i^* = 1$. Focussing on where the decline commences for the other nine species suggests values of $\widehat{p}_i^* = 0.7$ for big skate, $\widehat{p}_i^* = 0.85$ for lingcod, redbanded rockfish, and yelloweye rockfish, and $\widehat{p}_i^* = 0.95$ for the remaining species.
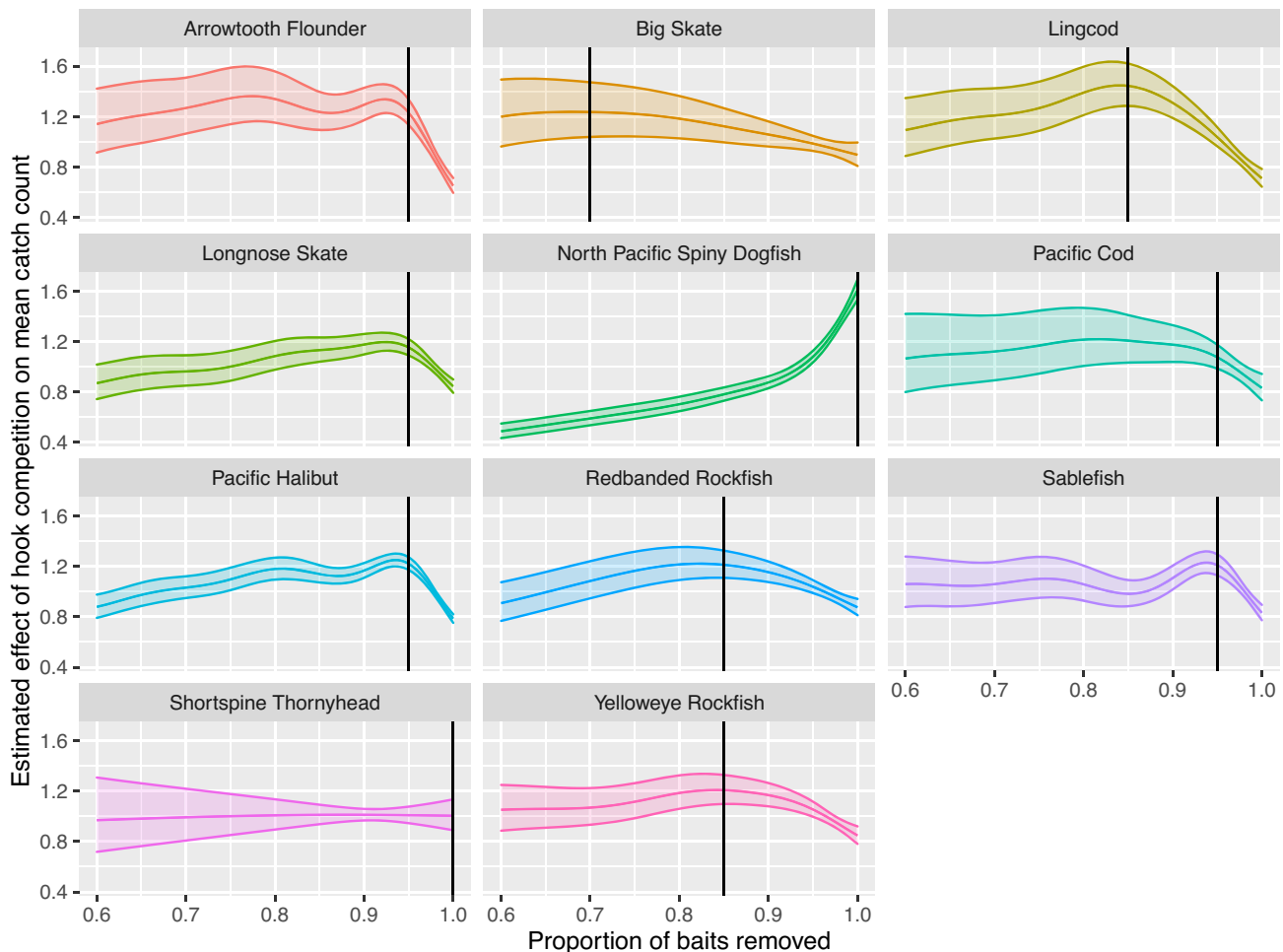
6's results recommend fixing $\widehat{p}_i^* = 1$ for both species and experiments 1 and 5 advise that reported levels of uncertainty be viewed with caution.

The $\widehat{p}_i^*$ values for each species are then used to calculate the relative indices for the censored method, shown alongside the other methods in Fig. 7 for lingcod, arrowtooth flounder, and dogfish (see Supplementary Material for remaining species). Numerical convergence issues occurred for three species (yelloweye rockfish, big skate, and redbanded rockfish). For these, we consider the largest 5% of catch counts each year to be "high quality" and treat any remaining "low-quality" catch counts as interval-censored (imposing an upper bound, see Supplementary Material).

Interestingly, the CPUE and ICR methods' estimates are in close agreement for all species, albeit with the ICR method estimating fluctuations with greater magnitude. The censored method's estimates deviate greatly from the CPUE and ICR methods' estimates for lingcod and arrowtooth flounder, most notably during the time periods of 1998–1999 and 2019–2021 when censorship levels oscillated. Figure 6 outlines why. Both species show a decline in mean catch counts exceeding 40% between fishing events with a proportion of baits removed around $\widehat{p}_i^*$ and those with all baits removed. Furthermore, Fig. 7 shows that the average proportion of censored fishing events (i.e., fishing events with $p_{tk} \geq \widehat{p}_i^*$) is close to 75% and 50% for lingcod and arrowtooth flounder, respectively. Thus, it is estimated that fishing events are frequently

Fig. 7. Plots of normalized (each time series is divided by its geometric mean) bootstrapped percentile intervals of CPUE, plus normalized estimates of relative abundance from the CPUE, ICR, and censored methods, for lingcod ($\widehat{p}_i^* = 0.85$), arrowtooth flounder ($\widehat{p}_i^* = 0.95$), and dogfish ($\widehat{p}_i^* = 1$). Right column shows the proportions of fishing events each year considered censored (i.e., mean proportion of events with $p_{tk} \geq \widehat{p}_i^*$). Large differences between the methods' estimates are seen for lingcod and arrowtooth flounder, whereas estimates show good agreement for dogfish. Bootstrapped estimates consider only the subset of 109 stations for which data were successfully collected each year, all other methods use data from all 171 stations. The censored method's trends differ greatly from those of the CPUE and ICR methods, especially in the time periods with the greatest variation in the proportion of censored events: 1998–1999 and 2019–2021. Data from 2012 are omitted due to the mixture of baits.
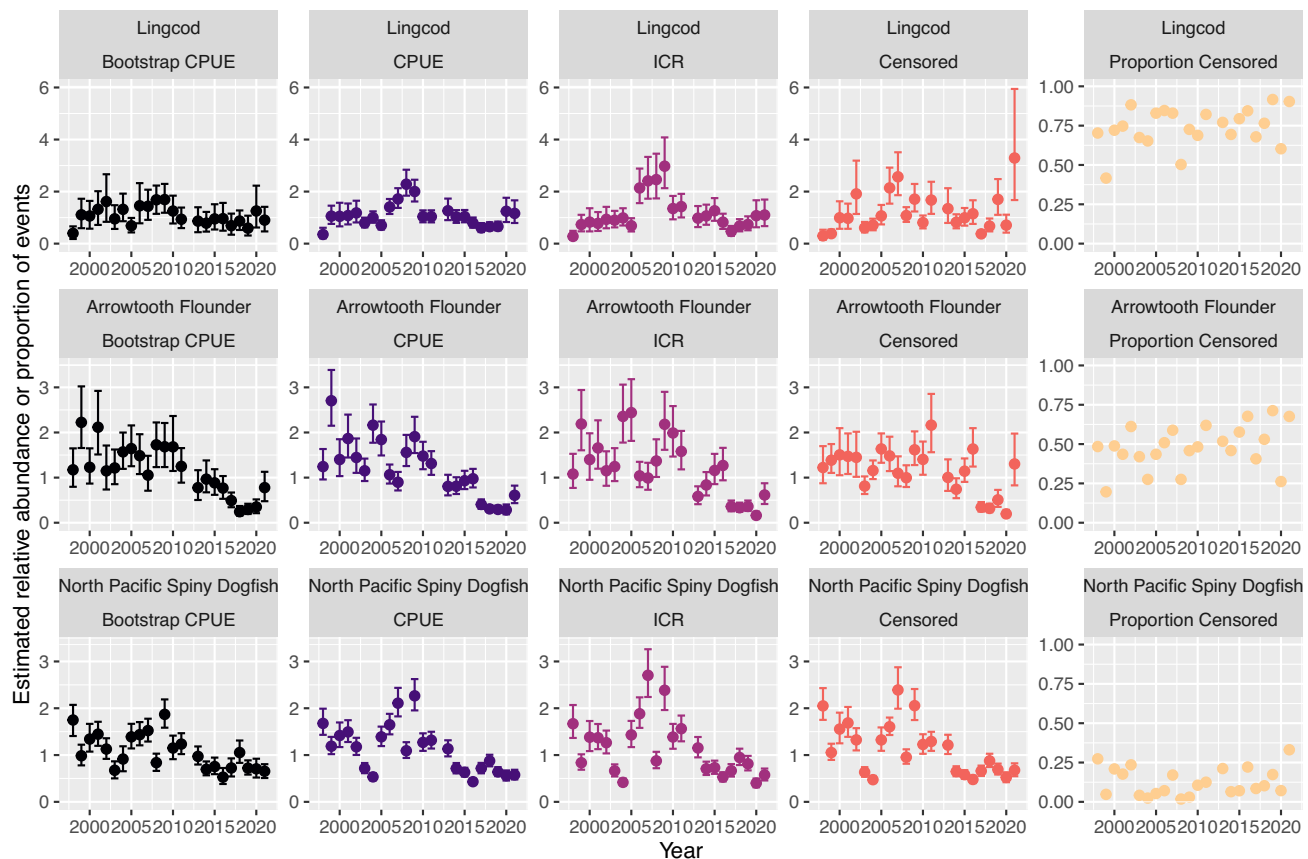
impacted by hook competition bias, and that the magnitude of this bias is very high for both species.

Conversely, although Fig. 6 suggests that estimates of dogfish abundance are sensitive to hook competition bias caused by complete bait depletion, Fig. 7 shows that it is relatively rare for fishing events to return zero baits (zero because $\widehat{p}_i^* = 1$), averaging around 10% of fishing events. Consequently, estimates of relative dogfish abundance from the censored method are in close agreement with those from the CPUE method. See Supplementary Material for full details.

Figure 8 shows the magnitude of the differences between the estimates from the CPUE and the censored method, and how these change with the annual proportion of fishing events that are censored. Strikingly, for lingcod the censored method can yield relative abundance estimates that are deflated by 50% compared to those from the CPUE method in years with low censorship, but inflated by 150% in years with high censorship. Note that although the censored method will not deflate catch counts for individual fishing events,

deflation of the annual relative abundance estimates occurs because of the scaling of each multiyear time series by its geometric mean. The magnitude of inflation/deflation at a given level of censorship varies greatly from species to species (see Supplementary Material). Thus, the censored method identifies a unique level of hook competition sensitivity for each species. Lingcod is estimated to have been most affected by hook competition. Dogfish and Pacific halibut are estimated to be the least affected, with maximum inflations in their relative abundance estimates of around 20% and 50%, respectively, compared to the CPUE method. The species-specific effects of hook competition is unlike the ICR method which assumes the same impact of hook competition on all species.

Discussion

The censored Poisson method introduced here represents a novel approach for handling the biasing effects of hook

Fig. 8. The percentage change in normalized relative abundance estimates from the CPUE method to those from the censored method (one dot for each year) against the annual proportion of censored fishing events, for the three species with $\widehat{p}_i^* =$ 0.85. Horizontal lines indicate no change between the methods, and vertical lines show the average annual proportion of fishing events experiencing at least 85% bait removal. Clearly (i) relative abundance estimates from the censored method are significantly deflated compared to the CPUE method in years with lower levels of censorship, and significantly inflated in years with higher levels, (ii) the inflation/deflation trend is nonlinearly related to the proportion of censored observations, and (iii) the magnitude of inflation/deflation varies across species (even with the same value of $\widehat{p}_i^*$) and can be very large. The censored method infers that lingcod is more severely affected by hook competition than redbanded rockfish or yelloweye rockfish.
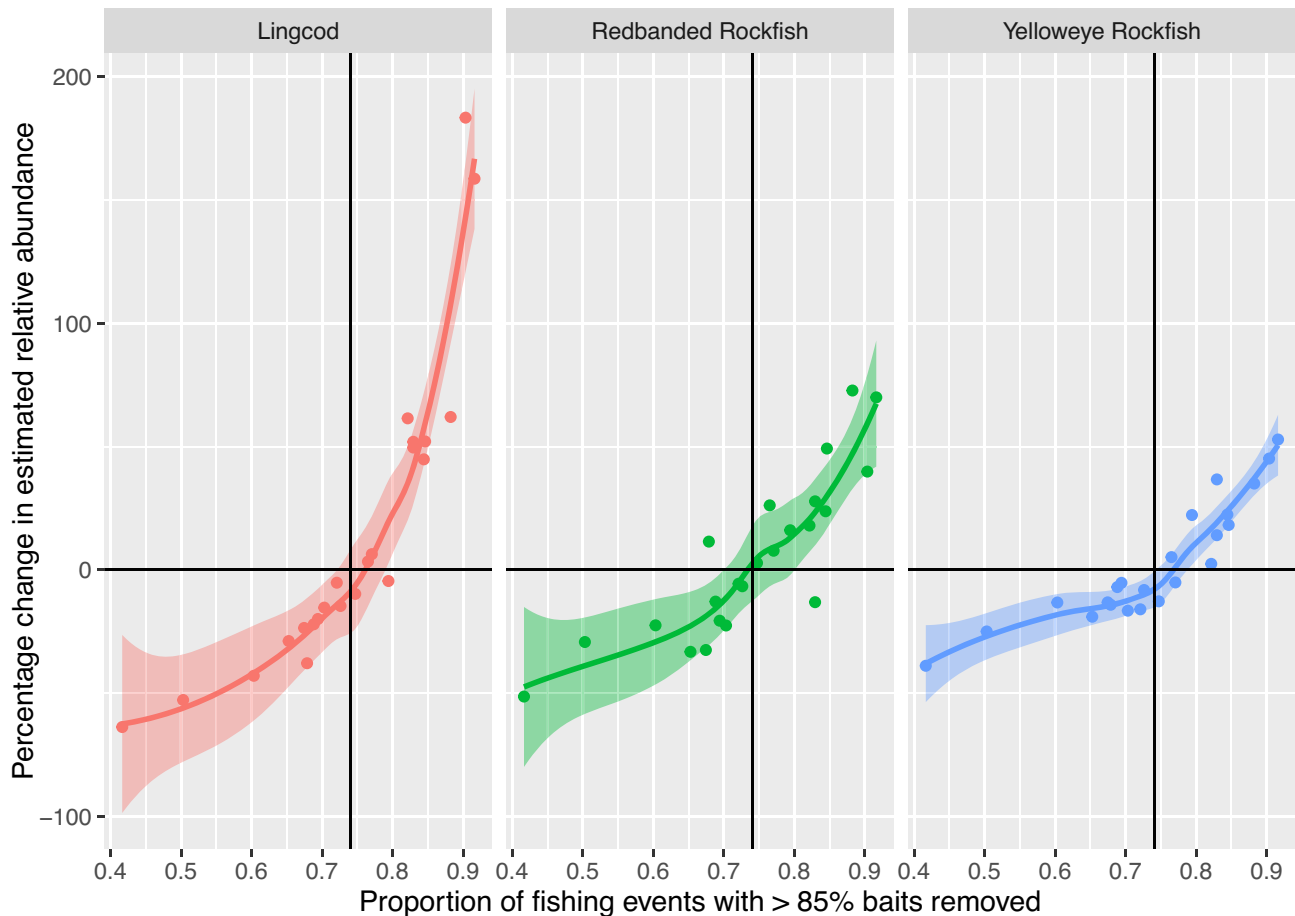
competition on estimates of relative species abundance collected with longline gear. We showed that our method generally outperformed the CPUE and ICR methods currently used. Unlike current methods, our method allows researchers to correctly identify when fish populations appear stable through time, and when they show marked changes. Crucially, the new approach moves away from the earlier mathematical approaches that modelled hook competition with fixed-rate ODEs and towards a purely statistical treatment of the problem by viewing hook competition as a data-quality issue.

Treating the catch counts of a species from longline fishing events that experienced the highest levels of bait removal as right-censored allows us to appropriately express our ignorance with the true underlying processes driving hook competition, such as species-specific bait location abilities, mechanical bait loss, and bait removal due to scavengers. Furthermore, the censoring approach also provides a natural

way of linking the observed catch counts, which are upper bounded by the number of deployed baited hooks, with the somewhat unbounded density of each species. Conversely, attempting to mathematically describe the numerous interacting processes that govern the hook competition within a system of ODEs is a mammoth challenge and it is unclear how to link the ODE rate parameters to the underlying species densities without requiring additional strong assumptions (e.g., independent bait-removal events, nonoverlapping hook-level bait odour concentration plumes).

While the simulation study demonstrated the robustness of the censored method to gross misspecifications of the breakdown point $\widehat{p}^*$, it also clearly showed that improvements in prediction accuracy and uncertainty quantification are both possible if this breakdown point can be better estimated. We introduced an empirical approach to estimate the breakdown point. However, better estimation may be possible by conducting hook-spacing experiments (Sigler 2000).

One can experimentally increase the interhook spacing until a significant decrease is observed in the catch rate, and then map this spacing level to an equivalent proportion of baited hooks removed. Sigler (2000) found that sablefish exhibited an 8% decline in catch rate once the hook spacing was 42 m—equivalent to around 5% of the baits remaining for a standard survey skate with 2 m spacing. Thus, an experimentally justified estimate for the sablefish-specific breakdown point would be when 95% of the hooks have been taken, which is the same value found by our empirical approach. Future research on other species could validate this experimental approach and use it to further develop empirical approaches for estimating p_i^*; see Monnahan and Stewart (2018) for modelling that accounts for hook spacing.

Species-specific breakdown points could be linked to the presence of one or more problematic species. A species may be problematic due to many reasons, including it scaring the target species away from the baited hooks (e.g., predators). If only a few problematic individuals present around the longline gear are able to cause large decreases in the observed catch counts of a target species, then even small fluctuations in the abundances of these problematic species could strongly confound the relationship between the catch counts of a target species and its abundance. The approach outlined here for choosing the breakdown point could be extended for such a scenario, by conducting simulation studies to develop methods for identifying the existence of problematic species and choosing a suitable breakdown point.

While the censored method performed well in all settings tested, uncertainty levels were underestimated when applied to species that were immune to hook competition. Knowing which species are insensitive to hook competition would allow researchers to acknowledge that while the censored method likely estimates the relative abundance of these species with low error, the corresponding estimates of uncertainty are likely too low. Identifying immune species should be possible with experimentation, such as identifying the species with the fastest arrival times with hook timers or ROVs (e.g., Obradovich 2018), and future methodological developments could provide methods to correct for these biases.

While the case study focused on longline data collected by the IPHC for capturing demersal species, the method should apply equally well to midwater or surface longlines for estimating the relative abundances of pelagic species, requiring only a conceptual change towards density quantities and integrals in 3D space with respect to volumes of water instead of surface areas of seafloor. Computational implementation remains the same. For longline surveys that record biomass rather than numbers, the censored likelihood function can be modified to one that handles zero-inflated continuous data.

Mechanical bait loss and bait removal/blocking due to large invertebrates have both been identified as possible sources of bias in abundance indices (High 1980; Obradovich 2018). For example, Obradovich (2018) observed sea stars physically blocking baited hooks in underwater ROV experiments, but rarely observed the offenders on deck upon gear retrieval. Similarly, Grimes et al. (1982) observed crabs and sea stars

remove and (or) block 50% of longline bait within 4 h and Obradovich (2018) also observed giant Pacific octopuses (*Enteroctopus dofleini*) and multiple crab species remove large quantities of bait, despite only a single octopus and no crabs ever being observed on deck. Large annual fluctuations in the population sizes of both crabs and giant Pacific octopuses have been observed (Hartwick et al. 1984; Zhang and Dunham 2013). While the censored method can easily account for bait removal, it will fail to account for any biases caused by baits being blocked but not removed. To correct for this: (i) the local average proportion of blocked baits could be estimated and used to appropriately decrease the breakdown point, (ii) a unique breakdown point could be estimated each year following the empirical approach outlined in this paper—an earlier decline in mean catch count should be visible in years with high levels of blocking, and (iii) a sensitivity analysis could be conducted, averaging conclusions across a range of plausible breakdown point values.

Spatiotemporal methods are being increasingly proposed and implemented in fisheries science (Thorson et al. 2015, 2016). For example, the IPHC analyzes the halibut catch counts from their survey using a spatiotemporal approach (Webster et al. 2020). The censored Poisson method, derived in eqs. 1, 3, and 8, was explicitly set up to be extended by allowing for Gaussian Markov random fields to be included within the linear predictor. Taking a spatiotemporal approach to estimating relative abundance can lead to improved precision and it can simplify the construction of joint models using multiple data sources with data sampled at disjoint spatial locations (such as bottom trawl data and longline data; Grüss and Thorson 2019; Webster et al. 2020). In fact, since the mean of the censored Poisson model (eq. 8) describes a species' density instead of a rate from differential equations, as is the case with other hook competition approaches, such joint models are more easily and justifiably constructed. Two interesting future research questions arise. First, can the breakdown point can be sensibly chosen by maximizing a measure of agreement between longline and trawl (or other hook-competition-free fishing method) subcomponents within a joint model? Second, does a spatiotemporal censored method improve performance when there are regional differences in hook competition?

Additional improvements to the relative abundance indices computed in the case study could be incorporated, including covariates believed to describe the species' habitat preferences, survey timing adjustments (as done by the IPHC for halibut, see Webster and Stewart 2013), and nonlinear effects of effective skate on the mean catch counts. The purpose of the indices presented in the case study is to provide the reader with an understanding of the magnitudes of changes possible between the competing methods.

Relative abundance indices are frequently produced to monitor populations and to use as inputs to formal stock assessment models. Changes on the order of 50%–150%, as found here, could lead to substantial differences in the resulting outputs from such models and in the consequent advice to fisheries managers. Thus, we recommend our simulation-tested censored method be considered for such applications.

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Article information

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Data availability

All R code and data used in this study are available in Supplementary Material (file cjfas-2022-0159supplb.zip) and at https://github.com/joenomiddlename/Censored_Longline_RCode. Further methods and results are in Supplementary Material file: cjfas-2022-0159suppla.pdf.

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