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A modeling framework to meta-analyse discard survival experiments illustrated by *Nephrops norvegicus* in European demersal trawl fisheries

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ABSTRACT

Good knowledge of discard survival in commercial or recreational fisheries is key in enabling more accurate stock assessments and supporting evidence-based regulatory measures; however, estimating discard survival is challenging. To improve the estimation of survival rate at regional scales and to identify key drivers, we present here a meta-regression (MR) framework that considers differences in experimental design, quality and context specificity between individual studies. First, studies are filtered through a systematic critical review to exclude results considered too uncertain or potentially biased. Discard survival rates are then corrected to limit estimation method bias, and associated uncertainty is included as a weighting. The MR is finally applied under a hierarchical mixed-effects framework to account for the nested structure of the data and correct for experimentally induced mortality bias. We illustrate how the MR can address methodological and analytical limitations in discard survival studies using Norway lobster (*Nephrops norvegicus*) discarded from European demersal trawl fisheries. While some effects were already identified from single studies, such as the temperature change, the MR highlighted other effects not perceptible at a regional scale, varying more at the scale of the fishing operation, such as tow duration, but also related to the size of experimental subjects and physiology. This flexible framework has applicability to other species or contexts. The case study provided insights to make recommendations for future survival studies to improve the predictive potential of this type of MR, such as the importance of following standardized protocols and analyses, and to report data at the finest resolution.

1. Introduction

Estimating and understanding discard survival plays an essential role in considering unaccounted fishing mortality but also supports recent fisheries management policies aimed at substantially reducing discarding of unwanted catches, such as the landing obligation in the European Union (EU) (Broadhurst et al., 2006; Condie et al., 2014; Karp et al., 2019; Uhlmann and Broadhurst, 2015). In particular, the EU landing obligation allows case-specific exemptions, where scientific evidence demonstrates high survival after capture, handling, and release (EU, 2013; Rihan et al., 2019). Scientifically estimating discard survival can, however, be challenging due to the variability among individual discard survival studies and the operational, environmental, and biological dependency of discard survival rates (Arlinghaus et al., 2007; Bartholomew and Bohnsack, 2005; Braccini and Waltrick, 2019; Broadhurst et al., 2006; Davis, 2002; Raby et al., 2014; STECF, 2020, 2019; Uhlmann and Broadhurst, 2015). Furthermore, accurately estimating discard survival is particularly challenging given financial and logistical

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constraints that almost always result in limited experimental replication and that hinder the capacity to undertake experiments at large scale. To overcome such limitations, meta-analysis (MA) can be used to generalise and strengthen inferences about survival and the factors that affect them (Arnqvist and Wooster, 1995; Gilman et al., 2022; Lièvre et al., 2002; Thorson et al., 2015; van Houwelingen et al., 2002).

MA has significantly enhanced the understanding of the operational, environmental and biological drivers of discard survival, and has led to more robust estimates (Campbell et al., 2014; Dapp et al., 2016; Fox et al., 2020; Musyl et al., 2011; Musyl and Gilman, 2019; Rudershausen et al., 2021). However, important methodological and analytical problems remain. First, the experimental and analytical approach, as well as the quality and robustness of the data, may vary considerably among studies, which then must be differentially weighted in the MA (Ahn and Becker, 2011; Hedges and Olkin, 2014). Second, data from discard survival studies often have a hierarchical or nested structure because observations from the same study or treatment share conditions and experimental specifics that differ from the other studies and/or treatments. Third, discard survival studies do not always provide evidence that all the discard-related mortality has been estimated, either because only the survival fraction at a single point in time after the "discarding" treatment is reported, or the convergence of the survival curve was not correctly checked. Indeed, in absence of other sources of mortality, such curve should converge to an asymptote considered as the expected discard survival fraction (*sensu* Benoît et al., 2012). Fourth, the observation method itself, including transportation and captivity, may induce stress and mortality that may compound and interact with the catch and discard-related stress and mortality expressed by the treatment groups. If disregarded, for instance by not employing experimental control subjects or groups, survival estimates could be biased (Breen and Catchpole, 2021).

In this paper, we propose and evaluate a meta-analysis framework for discard survival estimation based on meta-regression (MR) that addresses the aforementioned issues (Cleophas and Zwinderman, 2017). Following best practice for undertaking meta-analysis, this framework relies on an initial systematic critical review (SCR) of the candidate studies to identify all relevant datasets and to filter these against a minimum acceptable quality standard (Higgins et al., 2024). Such a quality standard includes consideration of factors such as the representativeness of the samples, the duration of the monitoring periods and the use of controls (ICES, 2016b, 2016a, 2015). For some factors affecting quality, it is possible to apply corrections that avoid having to discard too many studies a priori. First, the expected asymptote of the survival curve is estimated for studies that did not provide it using a method developed and described in ICES (2016c, 2016a). Second, we inspect the quality of the control survival, subsequently using the results from controls to correct for biases induced by different experimental designs by modelling survival estimates using a beta regression. Furthermore, we derive an expression for the standard deviation of the adjusted response variable, which accounts for various sources of uncertainty, to include it as a weight in the modelling process. Finally, the MR is applied under a hierarchical mixed-effects framework to account for the nested structure of the data.

To illustrate this framework, we presented the case of Norway lobster (*Nephrops norvegicus*, Nephropidae) caught in European demersal trawl fisheries for which *Nephrops* fisheries are widespread in the North East (NE) Atlantic and are conducted on muddy bottoms in the Greater North Sea region (NS), North Western Waters (NWW, Scottish and Irish waters), and South Western Waters (SWW, the Bay of Biscay and the Iberian Peninsula), as well as the Mediterranean Sea (Ungfors et al., 2013; Vasilakopoulos and Maravelias, 2016). By catch volume, *Nephrops* is mainly caught by single or multi-rig bottom otter trawls in targeted and mixed species fisheries. Among crustaceans, *Nephrops* has the highest landings by weight in the NE Atlantic since early 2000s and the fourth highest in the Mediterranean Sea, with total landings around 55,000 tonnes per year (Vasilakopoulos and Maravelias, 2016). Notably, the NE Atlantic trawl fisheries for *Nephrops* also rank among the largest in the world in terms of discard amounts (in number and weight), commonly composed of damaged or undersized *Nephrops* and fish bycatch (Catchpole and Revill, 2008; Pérez Roda et al., 2019). An existing comprehensive SCR of nineteen relevant studies for *Nephrops* from the primary and grey literature was used as the basis for this study, MR was applied to the eight studies retained from the SCR. The results were used to discuss the ability of such an approach to improve knowledge on the drivers of discard survival and make recommendations to improve the predictive potential of this type of MR.

2. Material and methods

2.1. Selection of discard survival studies and data extraction

The SCR process screens all the relevant studies that could be included in a MA. For the case study, relevant studies were identified in a two-stage literature search using search terms identified based on the collective expertise of our team of authors and from studies known to us beforehand (ICES, 2015). The first stage used 'Web of Science' (WoS), scientific citation search engine whereby applied search terms (Table S1-1) to identify potentially relevant studies. Those containing original discard survival estimates were retained. The second stage was a single-pass reading of the retained articles to identify other sources of original discard survival data (further details in ICES, 2015). Data from the retained articles were either digitized from the tables and figures, or obtained from the authors.

A SCR is especially important in the context of discard survival studies, which are often conducted under different methodological, operational, environmental and biological conditions that may be very specific to the fisheries of interest, or at demonstrating the effect of particular treatments. The comprehensive question-based critical review framework developed by researchers during the ICES WKMEDS (ICES, 2016b, 2016a, 2015) and applied to *Nephrops* enabled us to identify and assess the experimental protocol, the methodology used to analyse the data and the level of reported information (Supplementary 1,2). We examined nineteen relevant studies: the fifteen originally reviewed during the ICES WKMEDS (ICES, 2018, 2015); and four published subsequently (Fox and Albalat, 2018; Visser and van Broekhoven, 2019; Barragán-Méndez et al., 2020; Fox et al., 2020; García-De-Vinuesa et al., 2020). We retained studies providing original estimates of *Nephrops* discard survival, after being caught in commercial fishing conditions, handled on deck by the crew and/or the scientific staff and kept in captivity for survival observations. Then, two exclusion criteria were defined with respect to the absence of control subjects or any sample that could be used as 'pseudo-control', and to the suspicion of biases in the results that cannot be corrected.

Finally, estimates of survival rates, associated standard deviations and data describing fishing conditions may be reported at different observation levels (e.g., fishing operation, treatment, study). The choice of the minimum level at which the variable of interest should be reported and the type of covariates required should be done in a parsimonious manner to constitute the most consistent and relevant dataset. For *Nephrops*, both cross-sectional and longitudinal data, where available, could be compiled at the fishing haul level from eight survival studies (Table 1). Data describing the environmental (fishing depth, sea surface temperature), operational (haul duration, catch weight, air exposure), and biological (carapace length) conditions of the discard experiments were extracted at available levels, from the haul level to the treatment or study level (Supplementary 2).

2.2. Data adjustments for the MR

2.2.1. Treatment survival rates (S^A)

The reported survival rates are not systematically estimated at the survival asymptote because of the cross-sectional nature of the data or

Table 1
Description of the different treatments included in the meta-regression by study: gear type (OTB for Bottom Otter Trawl with single rig, and OTT for Otter Twin Trawl) ('Gear'), codend mesh size in mm and shape with 'D' for diamond and 'S' for square ('Mesh', with NA if the information was missing), square mesh panel (SMP) mesh size if present, practice type (e.g., the presence of selective devices, presence or absence of a discarding chute) ('Practice'), season ('Season'), the part of the catch that was collected with 'All' for landings and discards and 'Discards' for discards only ('Sample'), and the total number of hauls observed ('*N*').

Study	Treatment	Gear	Codend	SMP	Practice	Season	Sample	*N*
Albalat et al. (2016)	Clyde_Short_Winter_(Albalat)	OTB	85D	120	Short haul duration	Winter	<MCRS (<1 %) and unfit for market	2
	Clyde_Short_Spring_(Albalat)					Spring		3
	Clyde_Short_Summer_(Albalat)					Summer		3
Armstrong et al. (2016)	North_Sea_Grid_Winter	OTT	80D	-	Four-panel box with fish escape hole and inclined netting sheet (Netgrid)	Winter	<MCRS (46 %) and unfit for market	12
Fox and Albalat (2018)	Forth_80Diamond_Summer	OTT	80D	200	Standard	Summer	<MCRS (29 %) and unfit for market	6
	Minch_80Diamond_Winter	OTT	80D			Winter	<MCRS (3 %) and unfit for market	6
	Minch_100Diamond_Winter	OTT	100D					6
	Minch_80Diamond_Summer	OTT	80D			Summer		6
	Minch_100Diamond_Summer	OTT	100D					6
Mérillet et al. (2018)	Biscay_Chute_Spring	OTT	80D	100	Discarding chute	Spring	<MCRS and unfit for market	3
	Biscay_Standard_Spring	OTT			Standard (sorting on deck)			3
	Biscay_Chute_Summer	OTT			Discarding chute	Summer		6
	Biscay_Standard_Summer	OTT			Standard (sorting on deck)			6
	Biscay_Chute_Autumn	OTT			Discarding chute	Autumn		6
	Biscay_Standard_Autumn	OTT			Standard (sorting on deck)			5
Oliver et al. (2017)	Aran_SELTRA_Summer	OTB	80D	300	Standard (SELTRA)	Summer	<MCRS (9%) and unfit for market	6
Ridgway et al. (2006)	Clyde_Short_Autumn_(Ridgway)	OTB	NA	NA	Short haul duration	Autumn	>MCRS	2
	Clyde_Long_Autumn_(Ridgway)	OTB			Standard (long haul)			2
	Clyde_Short_Spring_(Ridgway)	OTB			Short haul duration	Spring		2
	Clyde_Long_Spring_(Ridgway)	OTB			Standard (long haul)			2
Valentinsson and Nilsson (2015)	Skagerrak_SELTRA_Summer	OTT	90D	270	SELTRA	Summer	<MCRS and unfit for market ("large majority")	3
	Skagerrak_Grid_Summer	OTT	70S	-	Swedish grid (35 mm bar space)			3
	Skagerrak_SELTRA_Winter	OTT	90D	270	SELTRA	Winter		3
	Skagerrak_Grid_Winter	OTT	70S	-	Swedish grid (35 mm bar space)			3
Wileman et al. (1999)	Minch_70Diamond_Summer	OTB	70D	-	Standard	Summer	< min size	3
	Minch_100Diamond_Summer	OTB	100D	-				3
	Minch_60Square_Summer	OTB	60S	-	More size-selective codend			3

the absence of an appropriate modelling approach. If sufficient longitudinal data were available for the species of concern, the delayed period during which capture- and discard-related mortality can occur, can be assumed to be species-specific and estimated using a cure rate model. Then, the expected discard survival rate at the asymptote, referred to as S^A, can be retrospectively predicted for any cross-sectional data and an associated standard deviation estimated (ICES, 2016c, 2016a). Uncertainty associated with the projection, dependent on both the sample size and the lag between the experimental monitoring duration and time to convergence of the survival curve, was estimated and incorporated into the standard deviation associated with each estimated survival rate.

2.2.2. Control survival rates (C)

Controls are commonly used to measure the potential biases induced by experimental and captivity conditions (Breen and Catchpole, 2021). Control subjects are generally caught and sampled using methods that cause the least harm, such as by pots or trawl using short haul durations. Two of the studies (Albalat et al., 2016; Ridgway et al., 2006) did not include formal controls but survival estimates for individuals in excellent vitality or caught by short hauls that were considered as suitable "pseudo-control" subjects (Supplementary 2). In both cases, the observed proportions of individuals alive at the end of the monitoring period are referred to here as *C*, the control survival. In another study, bias was suspected in the estimation of the controls survival rate (Mérillet et al., 2018). They were kept in captivity for longer than the test individuals without being fed and their survival curves continued to decrease, contrary to the survival curves of experimental subjects which converged to an asymptote within 5 days. To correct for this difference, we only accounted for the mortality that occurred during the first five days, consistent with the treatment subjects. The resulting proportions of alive controls are reported in Supplementary 3.

2.2.3. Ratio of treatment and control survival rates as a response variable (R)

To account for the potential undesirable bias induced by the basic handling and captivity conditions, the survival ratio of S^A to *C*, referred to as *R*, can be used as the response variable instead of S^A. This approach assumes that the observed mortality in the controls is representative of mortality induced by the basic experimental protocol alone, independent of the fishing related treatments. In practice, this is not always the case because it can be quite difficult to perfectly isolate the different sources of mortality, and the mortality of the controls can even be larger than the mortality linked to the experimental conditions in some samples. If the overestimation can be identified (e.g., starvation effect of the control group collected before the test group), we therefore recommend also correcting for the control rate. Alternatively, studies for which potential bias have been identified in the mortalities in control subjects and cannot easily be corrected, should be removed. Then, the clear upper bound formed by the controls indicates that no significant additional source of mortality was introduced to the controls. Finally, the absence or weak correlation of survival rates between controls and tests indicates that the controls were not significantly driven by the process. These elements must be inspected to decide whether or not to make this correction.

2.3. Meta-regression using beta generalized linear mixed models

2.3.1. Variable dispersion beta regression model

The resulting variable of interest R is continuous and expected to be upper bounded by a value of one. In this case, the beta distribution is relevant as it is well suited to model proportions. In the regression model, the response is beta distributed using a parameterization of the beta distribution indexed by mean and dispersion parameters modelled as a function of a set of exogenous variables (Ferrari and Cribari-Neto, 2004; Smithson and Verkuilen, 2006). With this definition the response is restricted to the interval (0, 1). In the event that discard mortality is very low, the sample size is small, and no source of bias is identified for the controls' estimation, ratio values may be slightly above or equal to one. Such a ratio can then be transformed to range in (0,1) using the transformation $R' = (R - u)/(v - u)$, where R and R' are the original and transformed ratios, u the highest possible score on the test and v the smallest possible ratio. If a few values equal 0 are present in the data, a linear transformation of the response can be applied (Smithson and Verkuilen, 2006). If a high proportion of 0 is present in the data, a hierarchical approach is advised (Tang et al., 2023). For each observation i the survival ratio R_i was assumed to follow a beta distribution parameterised by the survival ratio mean π_i and the precision parameter θ_i.

$$R_i \sim Beta(\pi_i,\ \theta_i), \tag{1}$$

where $E(R_i) = \pi_i$ and $V(R_i) = \frac{\pi_i(1-\pi_i)}{\theta_i \quad +1}$.

The precision parameter θ_i was used to account for quantified differences in standard deviation of the survival ratio R_i. This uncertainty arises from both the asymptote projection and the sample size related to the estimation of the individual ratio. To address this, the uncertainty of the response variable was included in the precision parameter as:

$$\log(\theta_i) \quad = a + b * \log \quad \left(\frac{1}{\widehat{V(R_i)}}\right), \tag{2}$$

in which $\widehat{V(R_i)}$ is the estimated variance associated with the estimation of the response variable as a covariate, and a and b are parameters estimated by maximum likelihood.

The variance of R_i, $V(R_i)$, was estimated using the conditional variance formula (Appendix A), with the first two moments of S_i^A and $1/C_i$. The variance and expected value of S_i^A were estimated by a simulation approach, which accounted for the size of the treatment sample, the monitoring period duration and the uncertainty of the parameter estimates used to project the asymptote (ICES, 2016a, 2016c). The estimation of the variance and expected value of the inverse of the controls, $1/C_i$, are detailed in Appendix A.

2.3.2. Fixed effects

A set of covariates expected to influence discard survival was linked to the expected survival ratio as a linear combination of covariates with regression coefficients through a logistic link function. For *Nephrops*, the linear combinations of the six potential covariates were tested as fixed effects: air exposure duration, catch weight, fishing depth, haul duration, sea surface temperature (SST), and carapace length (CL). We also tested the effect of CL as a 3rd degree polynomial as well as the more expected interactions for the covariates: CL*Catch weight, CL*Depth and Air exposure*SST (Miller, 2002). When available, the thermal shock between SST and air or bottom temperature was correlated to SST, so considering SST only was deemed satisfying for this particular dataset. Despite potential nonlinearities in the temperature-response relationship, data exploration (Supplementary 4) supported a linear effect for SST. Several confounded covariates were not considered, such as: gear characteristics (confounded with catch weight); and handling and sorting practices (confounded with air exposure duration); and interaction between CL and SST (confounded with Study*CL). The absence of high collinearity between the six selected covariates was confirmed visually (Supplementary 4). The covariates were rescaled at mean 0 and standard deviation 1 to improve convergence and provide comparable effect sizes.

2.3.3. Random effects

Several studies may have distinctive sampling schemes to test for the effect of study-specific factors on discard survival, such as codend mesh size and shape (Wileman et al., 1999), selectivity devices (Valentinsson and Nilsson, 2015), discarding chute system (Mérillet et al., 2018), haul duration (Ridgway et al., 2006), or seasonal effect (Albalat et al., 2016) (Table 1, Table 2). These specificities relating to each study prevent the direct use of these factors in the MR. Furthermore, studies are generally conducted under different fishing conditions, including specification of the gear, environmental conditions and handling practices specific to each fishery, which are not always exhaustively reported nor all relevant to assess. Based on the structure of the data, effects such as study and treatment, or any other effect that was not of direct interest but expected to induce some correlations between observations, can be introduced as random effects in the regression part of the model:

$$logit(\pi_{i,j,k}) = X_{i,j,k}\beta + \quad T_{j,k} + S_k, \tag{3}$$

where i refers to the finest aggregation level of the data (i.e., the animal or, in our case study, the haul), j to the treatment and k to the study. X is the design matrix, $\boldsymbol{\beta}$ the fixed effects, and T and S are treatment and study-specific random effects, respectively.

Some covariates may still be confounded with study or treatment despite attempts to avoid this. For *Nephrops*, sampled individuals in the Bay of Biscay (Mérillet et al., 2018) were smaller compared to the other studies, because of both different commercial sizes between regions and differences in the experimental objectives (Fig. 2, Table 1). Furthermore, the effect of CL may vary between seasons and regions because of differences in the reproductive status (Milligan et al., 2009), which could not be included in the MR. To address these potential confounding effects, random interactions between CL and the study or treatment effects were also tested. Eight possible random structures were therefore tested: Study, Treatment, Treatment+Study (equivalent to Treatment*Study as the treatments are nested in studies), Study*CL, Study*CL+Treatment, Treatment*CL, Treatment*CL+Study, (Study+Treatment)*CL.

2.3.4. Model selection and validation

With mixed models, the predictions depend on both the fixed effects of covariates and the data structure within clusters. For models of the exponential family, the conditional and the marginal Akaike Information criterion are appropriate to assess the model performance at the cluster and at the population levels, respectively (Donohue et al., 2011; Saefken et al., 2014). The calculation of these criteria in the beta case is not yet solved, but Fang (2021) demonstrated that they are asymptotically equivalent to 'leave-one-observation-out' (conditional approach) and 'leave-one-cluster-out' (marginal approach) cross-validation procedures. Given this, we applied a 'Leave-one-observation-out' Cross-Validation to select the main drivers of discard survival, and we applied a 'Leave-one-cluster-out' Cross-Validation procedure to select the best predictors across the population.

In the case study, the treatment structure was nested in the study structure so we chose the study level as cluster; leaving out one study at each round and predicting their expected marginal response, i.e., with the expected random effects set to 0. The Root Mean Squared Error (RMSE) was used to assess the prediction error for both procedures. The mean squared errors between predictions from the model fitted to the

Table 2
Operational, environmental and biological drivers of discard survival investigated in the different studies. 'Yes' means that the effect of the covariate on discard survival was investigated and significant, 'No' that it was investigated but not significant, and '-' that it was not investigated. Italic font means that the effect of the covariate on discard survival was investigated through an indirect comparison: looking at susceptibility to physical damage to *Nephrops* first, and then at the relationship between damage and discard survival.

Drivers	Covariate	Albalat et al. (2016)	Armstrong et al. (2016)	Fox and Albalat (2018)	Mérillet et al. (2018)	Oliver et al. (2017)	Ridgway et al. (2006)	Valentinsson and Nilsson (2015)	Wileman et al. (1999)
Operational	Gear (codend)	-	-	No	-	-	-	-	No
	Selectivity device	-	-	-	-	-	-	Yes[†]	-
	Discarding chute system	-	-	-	Yes	-	-	-	-
	Haul duration	-	-	No	-	-	No	-	No
	Towing speed	-	-	-	-	-	-	-	No
	Catch weight	-	-	No	-	-	-	-	No
	Catch composition	-	-	Yes	No	-	-	-	No
	Air exposure	-	-	No	Yes	No	-	-	No
Environmental	Fishing depth	-	-	-	-	-	-	-	No
	Season	Yes	-	Yes	Yes	-	-	Yes	-
	Sea state	-	-	-	-	-	-	-	Yes
Biological	Carapace length	No	No	-	No	No	*Yes*	Yes[‡]	No
	Sex	No	-	*No*	No	No	*Yes*	No	Yes
	Moulting	Yes	-	-	-	-	*Yes*	-	-
	Gonad status	No	-	-	-	-	-	-	-
	Vitality/vigor	No	Yes	Yes	-	Yes	-	-	-
	Damage/Injuries	Yes	-	Yes	Yes	-	Yes	-	Yes
	Infection (*Hematodinium*)	Yes	-		-	-	-	-	-

[†] In winter
[‡] In females
[§] Jelly compared to soft and hard

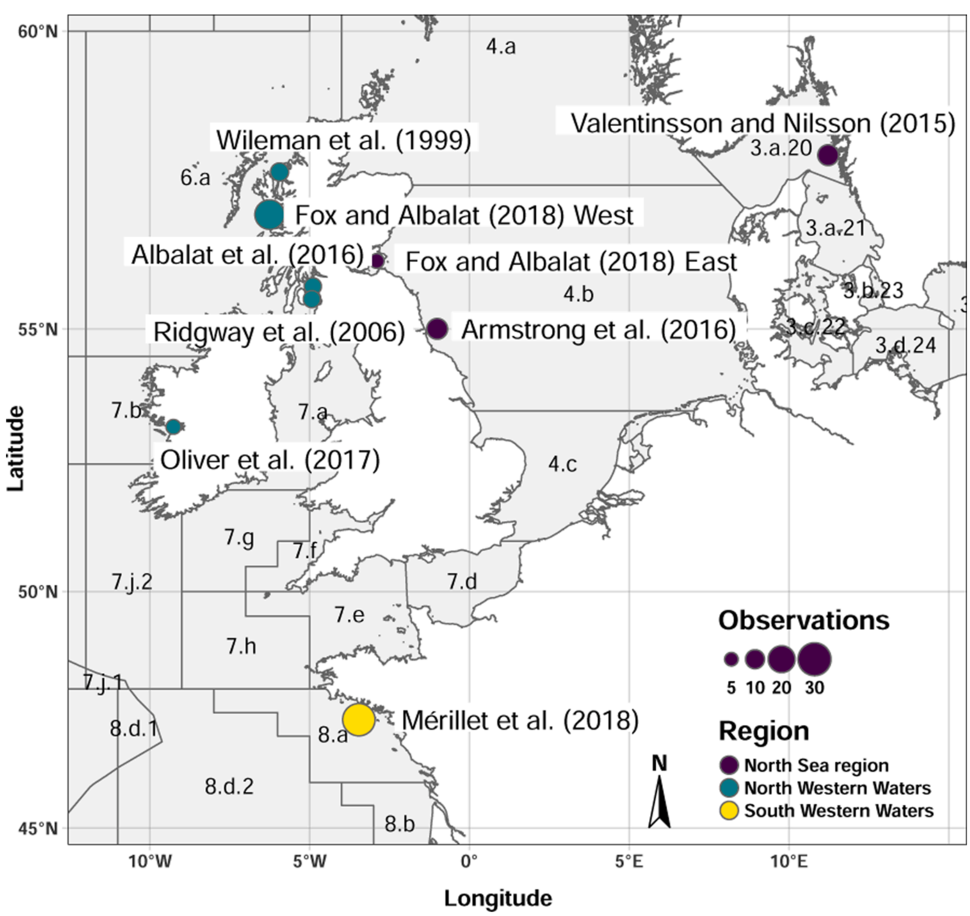

Fig. 1. Location of the studies included in the meta-regression by area (North Sea; Northern Western Waters; Southern Western Waters). The boxes shaded in grey refer to the International Council for the Exploration of the Sea (ICES) statistical areas. For each study, the size of the point is proportional to the number of observations.

Fig. 2. Operational, environmental, and biological drivers selected as covariates by treatment (left) and study (right). 'SST' is the Sea Surface Temperature. For Carapace Length ('CL'), vertical lines are used to indicate the legal Minimum Conservation Reference Size (MCRS, previously Minimum Landing Size or MLS; dotted line) and the experimental minimum size, if different from the legal size (dash-dotted line).

training sample and the observations of the test sample were calculated at each round and averaged (Fang, 2021). Given the relatively small number of covariates and interactions modelled, a full model selection procedure was adopted. The models with an intercept only as fixed effects (no covariate), as well as all the potential linear (and until the 3rd polynomial degree for CL) combinations of the six covariates and the three interactions, were estimated without any random structure, and with the eight possible random structures. Ultimately, 954 model variants were compared.

For each of the two model selection approaches and each of the random effects model variants, the variation rate in RMSE was estimated between the best fixed effects model variant and (1) the 'intercept-only' mixed model with the same random structure ('%$RMSE_1$'), and (2) the 'intercept-only' model with 'no random structure' ('%$RMSE_2$'). '%$RMSE_1$' quantifies the percentage decrease in predictive error due to the included covariates only (the fixed structure), while '%$RMSE_2$' additionally quantifies the percentage decrease in predictive error due to the random structure. For the best predictive model, the RMSE was also calculated between the mean observed and predicted values at the treatment and study levels to explore the influence of these aggregation levels on the predictive performance. The retained models were validated with a test on the normality of the quantile residuals, hereafter called 'Residual test', enabling to detect discrepancies between the model and the data (Dunn and Smyth, 1996; Pereira, 2019). Significance level was set at 5 %.

The R statistical software version 4.2.2 (R Development Core Team, 2022) was used for all data analysis and the beta regression was undertaken with the 'glmmTMB' package (Brooks et al., 2017). All the scripts are available in Supplementary 5.

3. Case study results

3.1. Potential drivers

Data in the SWW (Mérillet et al., 2018) and the Minch (Fox and Albalat, 2018; Wileman et al., 1999) were collected at deeper mean fishing depths compared to the other areas (Fig. 2). As expected, temperatures were dependent on both the area and the season, with a gradient of increasing mean SST from the north to the south, and higher SST in summer than in the winter (Fig. 2). As usually observed when targeting *Nephrops*, haul durations were relatively long (Ungfors et al., 2013), i.e., about three to four hours, but could vary as some studies specifically tested for the effect of shorter haul durations compared to standard commercial practices (Fig. 2, Table 1). The catch weights were similar between studies and treatments in the NS and SWW, whereas larger discrepancies were observed in the NWW (Fig. 2). Mean air exposures during catch sorting on deck were the longest for the standard treatments in the SWW (Fig. 2), in which the catch was sorted directly on deck and then discarded (Mérillet et al., 2018). In contrast for one of the NWW studies (Ridgway et al., 2006), air exposures were close to nil (Fig. 2) as individuals were kept submerged in seawater for the purpose of the experiment.

Differences between ICES areas in either the old Minimum Landing Sizes (MLS) or newer Minimum Conservation Reference Sizes (MCRS) were reflected in the experimental data with larger mean Carapace Length (CL) in discarded *Nephrops* from the Skagerrak (Valentinsson and Nilsson, 2015) compared to the SWW (Mérillet et al., 2018) (Fig. 2). Additionally, a substantial proportion of the discarded *Nephrops* in the considered studies considered were not undersized (Fig. 2, Table 1).

3.2. Survival ratio

At the lowest observation level (haul), 114 separate estimates of survival rate and associated standard deviation were available (Table 1). Overall, survival of controls was not dependent on the fishing conditions, because C and S^A were not directly proportional (ρ=0.15). In some studies this is expected because control animals were either derived from a different source, such as being creel caught, or were survivors in excellent condition from an earlier trial. The survival rate of controls thus corresponded to an upper bound to the maximum survival rate observed in experimental treatments (Fig. 3).

The estimated values for S^A ranged from 0.09 to 0.91, for C from 0.60 to 1.00, and for R from 0.15 to 0.99 (Supplementary 3). As expected, R was noticeably higher than S^A for studies with a lower C (Albalat et al., 2016; Mérillet et al., 2018; Ridgway et al., 2006), but the ranking of values was globally maintained (Spearman's correlation ρ=0.95) (Fig. 4). The range of the survival ratio was more than double between the data from all the studies and the data within each study (Fig. 4).

3.3. Meta-regression of survival ratio

3.3.1. Model validation ('Residuals')

For both model selection approaches, the model associated with the lowest RMSE was validated based on testing their residuals' distributions (Table 3 and Supplementary 6).

3.3.2. Model selection for the best explanatory model ('Leave-one-observation-out' procedure)

The best RMSE (0.149) with the 'Leave-one-observation-out' cross-validation procedure was obtained with 'Treatment * CL + Study' as random effect and 'CL + Haul duration + SST' as fixed effects (Table 3). With this model, the predictive error was reduced by 6 % (Table 3) due to the selected covariates only (compared to the 'intercept-only' mixed

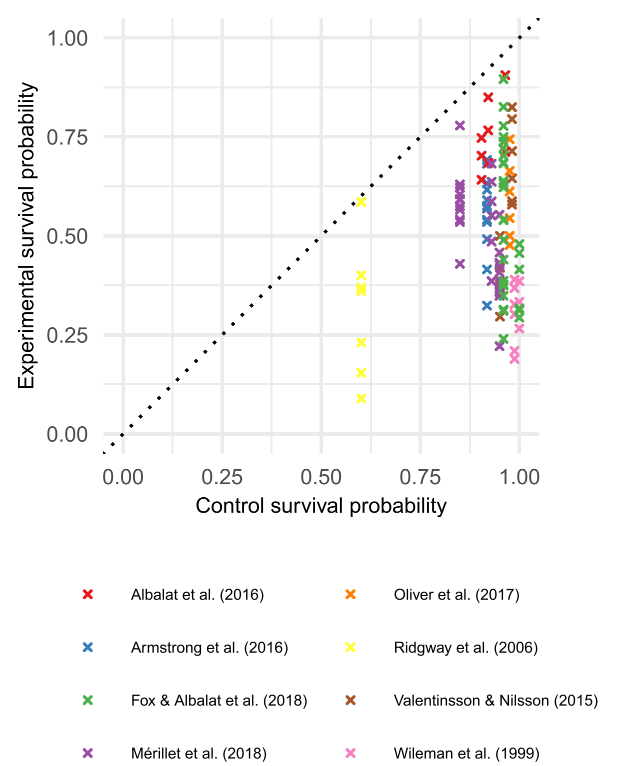

Fig. 3. Relationship between the asymptotic discard survival estimates (experimental treatments) and the control survival probabilities. The dotted line with intercept 0 shows how captivity/control related mortality can potentially cap the observable scope of survival in the treatments. Weights are given as the inverse of the variance (discard survival probability).

Fig. 4. Experimental discard survival (predicted at asymptote) and control survival estimates, and survival ratio R (between discard and control survival estimates) weighted by the inverse of its variance, by area, study and treatment.

model, as documented by $\%RMSE_1$) and by 22 % (Table 3) owing to the correlation within treatments (compared to the 'intercept-only' of the 'no random structure' model, as documented by $\%RMSE_2$).

3.3.3. *Model selection for the best predictive performance ('Leave-one-cluster-out' procedure)*

The best predictive performance calculated by the 'Leave-one-cluster-out' cross-validation was obtained with the model with no random

Table 3
Best fixed effects models (lowest RMSE) for both selection procedures (i.e., Leave-one-Observation-out Cross Validation to select the best explanatory model, and Leave-one-Cluster-out Cross Validation to select the best predictive model. $\%RMSPE_1$ is the percentage reduction in predictive error between the RMSE of the best fixed effects model variant and the corresponding 'intercept-only' model, $\%RMSE_2$ is the percentage reduction in predictive error between the RMSE of the best fixed effects model variant and the corresponding 'intercept-only' with 'no random structure' model. In the 'Residuals' field the p-values of the uniform distribution test for the simulated quantile residuals are reported, to check for model misspecification (in bold when well-specified with a p-value greater than 0.05).

Random structure	Fixed structure	RMSE	$\%RMSE_1$	$\%RMSE_2$	Residuals
Leave-one-observation-out					
Treatment*CL+Study	CL+Haul duration+SST	0.149	−6 %	−22 %	0.40
Leave-one-cluster-out					
None	CL+Depth+Catch_Weight+SST	0.195	−60 %	−60 %	0.90

effect and 'CL + SST + Catch weight + Depth' as fixed effects (RMSE=0.195). This higher predictive error compared to the best explanatory model is expected as more data were removed at each step of the cross-validation procedure. The improvement in prediction performance compared to the 'intercept-only' model (i.e., due to the covariates, as documented by $\%RMSE_1$) was much greater than for the best explanatory model (-60 % versus −6 %, Table 3). The model shows moderate predictive performance at the haul level, and improved performance at the treatment and especially the study level (Fig. 5). The model predictions under any values of these four covariates can be generated with the R script provided in Supplementary 7.

3.3.4. Effects of the covariates on the survival ratio

In both models, the marginal effects of the selected covariates on survival were all negative, with an important effect size for SST and CL, though only SST was significant for the 'Leave-one-observation-out' cross-validation procedure (Table 4). Haul duration and depth show effect size of the same magnitude, but were not selected with both procedures (Table 4). Among the ten best explanatory models, a model with predictive performance very close to the best one (RMSE=0.151) shows the same set of covariates as the best predictive model (Supplementary 6), i.e., including catch weight and fishing depth, suggesting confounding effect between haul duration and the combination of catch weight and depth.

4. Discussion

4.1. Technical capabilities of the modelling framework for meta-regression of discard survival

The proposed modelling framework for MR addresses several of the methodological and analytical limitations that have hindered meta-analysis of discard survival estimates, by accounting for the main identified biases and uncertainties, as well as the complex structure of discard survival data. We identified three crucial aspects that should be considered when applying such MR: 1) the availability of a control or pseudo-control sample to measure the experimental bias, 2) an adequate monitoring protocol to estimate survival after release, i.e. longitudinal data and estimation of the asymptote, 3) the type and level of resolution of factors reported. When applied to *Nephrops* in the European bottom trawl fisheries, the methodology generated consistent and informative results. This case study also highlights that conducting a meta-analysis in the context of discard survival is not straightforward, given the diversity of potential survival covariates and experimental biases, and

Table 4
Estimated fixed effects (and standard deviations) of the variables included in the selected models from both cross-validation procedures, and effect of the $\boldsymbol{log\left(\frac{1}{\widehat{V_{Rijk}}}\right)}$ in the dispersion model.

Selected variables	EFFECT	p-values
Leave-one-observation-out		
CL	−0.15 (0.20)	0.447
SST	−0.30 (0.11)	0.006
Haul duration	−0.19 (0.10)	0.061
Dispersion model	−0.28 (0.07)	$< 10^{-4}$
Leave-one-cluster-out		
CL	−0.24 (0.08)	0.001
Depth	−0.33 (0.08)	$< 10^{-3}$
SST	−0.21 (0.06)	0.278
Catch weight	−0.07 (0.07)	$< 10^{-3}$
Dispersion model	−0.29 (0.06)	$< 10^{-5}$

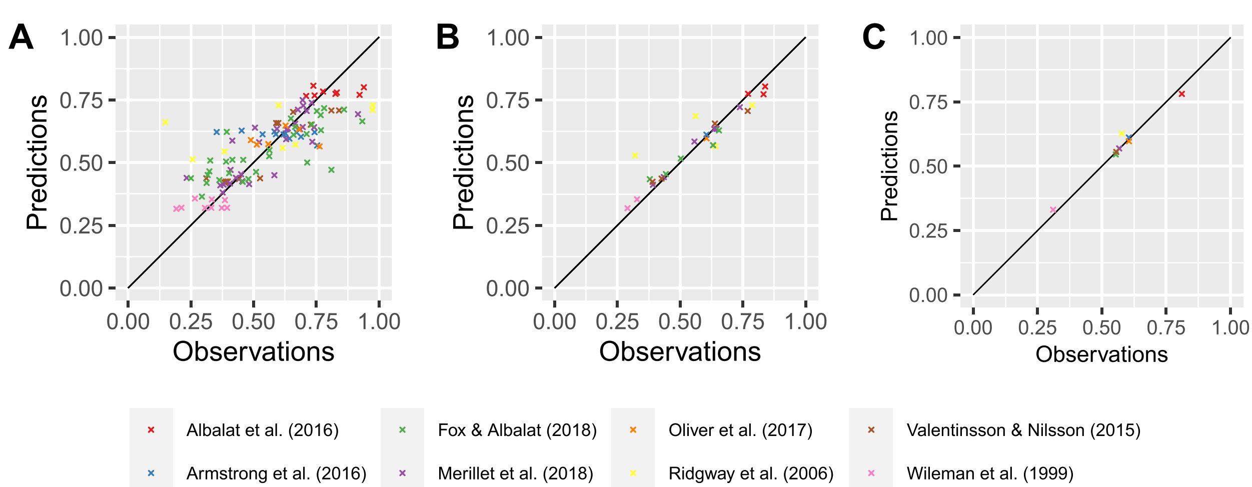

Fig. 5. Predictions of the survival ratio *R* ('Predicted ratio') from the model selected with the Leave-one-cluster-out cross-validation procedure as a function of the observed survival ratio *R* ('Observed ratio'). Values were displayed at the haul (i.e., individual observation) level, and averaged at the treatment and study levels, by study with blue for given study and red for other studies.

methodologies employed. There is therefore no generic method that can be directly applied without a detailed review of the studies and ad hoc post-processing of the data. Nevertheless, the experimental contexts of the papers used in this study are not specific to *Nephrops*, and the framework we propose is at least compatible with any ensemble of survival studies that follow the ICES guidelines (Breen and Catchpole, 2021), provided that sufficient studies are published. For instance, the ICES WKMEDS reviewed or began reviewing studies for several taxa occurring in European waters including skates, flatfish, Atlantic cod, all of which could be candidates for meta-regression based on the framework in this paper. To facilitate implementation to these or other cases, Appendix B shows a diagram highlighting the key steps of this framework and its potential limitations, and the analytical methods are available in Supplementary 1,5,7 and Appendix A.

The projection of survival rates to the asymptote (S^A) avoids overestimation of survival rates, in instances where the asymptote was not reached due to incomplete observation periods in the original studies. Contrary to mortality from natural sources, that resulting from capture and discarding is expected to be expressed over a finite, likely short time interval, resulting in an asymptote for that mortality source, while overall survival will monotonically decrease to zero in the longer term (Benoît et al., 2015, 2020). Given discard survival experiments durations that are typically very short relative to the lifespan of the target species, natural mortality can reasonably be ignored and a simple asymptotic function used, although slightly more complex models accounting for both natural and discard mortality are available (Benoît et al., 2015, 2020). Estimating S^A introduces additional uncertainty to the survival estimate, and is therefore relevant when a sufficiently large longitudinal data set is available and a similar convergence rate between curves can be observed for the species or group studied. This uncertainty will be the greater the shorter the follow-up time of the corrected study, but also the greater the expected bias. In any cases, this uncertainty is accounted for in the MR.

As control survival (C) decreases, the potential for bias in related treatment survival estimates increases (Breen and Catchpole, 2021). In the MR, standardisation across studies with different baseline (control) mortality rates was achieved using the "survival ratio" (R); i.e. treatment survival divided by control survival. Breen and Catchpole (2021) cautioned against such corrections "unless there is evidence of covariance between the method-control and treatment survival estimates", because of the potential to introduce further unknown bias and errors. To this end, Fig. 3 demonstrates that, for the data used in this MR, there is a general reduction in treatment survival probability with decreasing control survival probability. Furthermore, the related control survival rate represents an effective cap for most treatment survival estimates (Fig. 3). However, this correction method may introduce additional errors from several sources. Firstly, if the control group fails to fully characterise the effects of the captivity/observation related stressors in isolation from any capture and handling stressors associated with the treatment (i.e. fishing and discarding), this would manifest as an underestimation of the overall survival in the corrected R estimate. Such errors can be minimised by scrutinising the control methodology in a SCR and filtering out sub-optimal studies, as was done here. Secondly, assuming the control group does correctly characterise any methodological effects, the assumption of a simple linear relationship between the treatment survival rate and the underlying baseline survival (control) may contribute to error. The combined stressors of captivity/observation and treatment may compound to have an interactive rather than additive effect on the treatment survival rate (Breen and Catchpole, 2021). Without empirical evidence of such interactions, such an effect cannot be ruled out or accurately corrected for. If it was evident, however, it would likely manifest as a systematic depression of treatment survival, correlated with decreasing control survival, at levels substantially below the cap formed by the baseline (control) survival rate. This was not evident in the present data (Fig. 3). Thirdly, if the control group fails to properly exclude potential treatment effects, notably related to capture and handling, the method related mortality will be overestimated, resulting in over-estimation in the survival ratio (R), possibly even exceeding 1. In Mérillet et al. (2018) bias was suspected in the estimation of the controls survival rate. They were kept in captivity for a longer time period than for the test individuals without being fed, and their survival curves continued to decrease while the test curves were already stabilised (at 5 days). To correct for this bias, we accounted for the mortality that occurred during the first five days only. Nevertheless, although we recognise that the corrected R estimate has the potential to introduce errors that might be difficult to quantify, the relationship between treatment and control survival in Fig. 3 shows that failing to account for method induced mortality by not using R in this MR would substantially and demonstrably bias the survival estimates.

The modelling of a ratio could have been performed simply from a Gaussian model, but applied to the case of discards survival where the ratio is expected to be bounded between 0 and 1, the beta distribution was deemed more suitable. For our case study, the Gaussian model did not reach the same predictive performance as the Beta model, and was also systematically rejected following the residual analysis. Furthermore, the Beta model allows to account for the differences in uncertainties related to both sample size and estimate corrections, via its precision parameter for which we used the inverse of the variance of the survival ratio as a covariate. Although highly significant, the negative effect was not expected as it attributed more weight to more uncertain values. In our case, this is certainly due to the fact that only part of the data has been corrected, and that the uncertainty associated with the correction is partly linked to the method itself, and therefore not to the heteroscedasticity of the response. One solution would be to include the additional source of uncertainty by weighting the likelihood

The use of a hierarchical mixed-effects framework accounted for the potential correlations induced by specific experimental conditions that could not be controlled and measured. The cross-validation approach used for model selection enables comparison of all the potential structuring in the dataset, which is particularly relevant in such applications for which the potential structuring of the dataset is not necessarily obvious. By conditionally assessing the potential for covariates to influence discard survival at a regional scale, the MR mitigates the fact that catches, treatments and studies are not necessarily representative of the commercial fisheries' discards.

4.2. *Utility of meta-regression for explaining discard survival in Nephrops norvegicus*

The MR confirmed consistent and significant negative effects between the survival ratio and SST. *Nephrops* are poikilotherms, therefore rapid changes in the ambient temperature (i.e., between the capture depth and surface) can disrupt metabolism causing death (Breen et al., 2020; García-De-Vinuesa et al., 2020; Harris and Ulmestrand, 2004). The lower survival in warmer compared to colder seasons had previously been established in individual studies (Albalat et al., 2016; Fox and Albalat, 2018; Mérillet et al., 2018; Valentinsson and Nilsson, 2015). One might test for non-linear relationships between survival and temperature, but there easily can be complex correlations with other factors, especially at the scale of the MR. Indeed, in this case it was difficult to partition the contributions to mortality between sea bottom and air temperatures, or associated temperature shock, air exposure duration and deck setup which is study dependent.

Haul duration also showed a significant negative effect on the survival ratio. Although it was previously suggested that, in contrast with the stress induced by trawling and acute environmental change, increased haul duration did not significantly alter physiological parameters (Ridgway et al., 2006), increased damage and thus decreased survival likelihood has been demonstrated for longer hauls (Albalat et al., 2009; Milligan et al., 2009).

Though no evident correlation was found in the combined data between haul duration and catch weight, our results showed a possible confounding effect with haul duration and the combination of catch weight and fishing depth. Indeed, the MR revealed significant effects of catch weight and fishing depth, which were not perceived in the individual studies. The effect of catch weight implies susceptibility to stressors and physical damage induced by the catch during capture, retrieval and handling (Wileman et al., 1999). Clear links between the survival of *Nephrops* and the presence of physical damage or compromised vitality have been reported in previous studies (Albalat et al., 2016; Campos et al., 2015; Fox et al., 2020; Symonds and Simpson, 1971). Puncture and crush injuries are known to lead to the loss of haemolymph in *Nephrops* that may result in circulatory collapse (Wileman et al., 1999). In the latest comparison of the survival of discarded *Nephrops* across three distinct northern European trawl fisheries, including two studies included in our MR (Armstrong et al., 2016; Valentinsson and Nilsson, 2015), no relationship between physical damage and mean survival was found (Fox et al., 2020). However, the influence of physical damage might have been confounded with that of the air exposure, as they were both related to the catch weight in these two studies (Fox et al., 2020). Reduced survival with increased capture depth suggests a greater susceptibility to physical damage due to the effects of pressure change and temperature shock during the ascent of the trawl. As the southern *Nephrops* populations (e.g. SWW) are generally distributed in much deeper waters than the northern ones, from about 70–700 m (Ungfors et al., 2013), the effect of fishing depth on survival may, however, be associated with a study effect. One might also consider salinity shifts, however rare in *Nephrops* fishery areas with no pronounced low salinity layer reported in any of the studies considered in the MR (Harris and Ulmestrand, 2004; Valentinsson and Nilsson, 2015; Fox et al., 2020).

Whilst the effect of CL on discard survival was uncertain when looking at the studies individually, the MR showed a negative effect of CL on survival (though only significant for the predictive approach). This was unexpected because larger animals are thought to be more robust and resilient to the traumas of capture, handling and release (ICES, 2020). Larger *Nephrops* have a stronger carapace and should be better able to withstand physical damage (Milligan et al., 2009). There might however also be an interactive effect of CL and SST due to the poikilothermic nature of the species. Furthermore, the minimum commercial size varies regionally and may also change in time within a region, e.g., from 40 to 32 mm in Skagerrak and Kattegat in 2016. In addition, many of the sampled individuals were damaged and unfit for market despite being above minimum commercial size (Table 1). The presence of these larger, damaged individuals with lower likely survival could partly explain the unexpected negative marginal effect of size on survival, as the presence of injuries was not directly accounted for in the MR. Reproductive status of individuals is another potential driver associated with individual size that was not accounted for in the MR. While reproductive status was not directly found to influence discard survival (Table 2) (Albalat et al., 2016), it is known to affect the sex ratio in the catch of bottom trawls, as females spend more time hiding in their burrows while incubating eggs and are therefore less available to the gear (Chapman, 1980; Ungfors et al., 2013). The effect of sex on discard survival was more uncertain but interrelated with CL and moult stage (Table 2). Moult status was always found to be significant when investigated in individual studies, with lower survival for recently moulted individuals (Table 2) (Albalat et al., 2016; Bruun Nielsen, 2015; Ridgway et al., 2006). The retained 'Treatment*CL' random structure for the best explanatory model implies that the effect of *Nephrops* size on survival was treatment dependent. This may be due to the variation in size ranges between treatments, but also because the sampling procedure and the minimum commercial size varied between studies and/or treatments.

4.3. Utility of meta-regression for predicting discard survival in Nephrops norvegicus

The MR approach can be adapted to predict discard survival under different fishing conditions, thus limiting the number of animal experiments required in line with the principles of "replacement" and "reduction" in the animal welfare related concept of the 3Rs (Replacement, Reduction and Refinement) (Madsen et al., 2022). This is all the more relevant since this type of studies are often conducted under commercial conditions, thus limiting the ability to perform protocols under a broad range of conditions, and therefore the predictive potential of a single study.

Applied to *Nephrops*, the predictive performance was mostly associated with CL, fishing depth and SST. Fishing depth is a study-related predictor. The CL is not necessarily a study–related factor but averaging at the catch scale resulted in low within-study variability. The predictive performance was also associated with the treatment-related predictor SST. Other factors describing the treatments such as season (Albalat et al., 2016; Mérillet et al., 2018; Ridgway et al., 2006; Valentinsson and Nilsson, 2015) could not be included in the MR, but were partially accounted for through SST. Season reflects changes in sea temperatures and is connected to moult stage and reproductive status of the individual *Nephrops*, as well as sex ratio in the catch. Therefore, season was not directly accounted for in the MR, as its link with the *Nephrops* biological cycle and temperature varies at the European scale. Information on moult stage, reproductive status and sex was also not available for all studies.

The predictive performance was partly impaired by the data resolution. Even though survival data are collected for each individual animal, the minimum level survival data is usually presented is at the haul level. Data in the MR were reported at the haul, treatment or study level in publications, not at the level of each individual. However, some explanatory variables, significant in several individual studies, are individual–dependent, e.g. air exposure duration and moulting. Furthermore, not all potential covariates could be included, as they were not consistently reported across studies. The limited predictive performance also indicates that some factors influencing survival vary more at the individual than the regional or fleet scale, e.g., CL or air exposure. The effects of these factors are masked by the variability between studies, which was not sufficiently balanced by the sample size. This variability is expected as individual studies are generated from separated experiments and most of the individual studies stem regional specificities, e.g., comparing alternative selective gears or catch handling procedures. In order to improve the performance of such MR, we encourage researchers to standardize the protocols of future discard survival experiments as much as possible to provide comparable results (such as guidelines in Breen and Catchpole, 2021), and report data at the finest resolution. Note that for larger species than *Nephrops*, (such as flatfish, and skates and rays), results are commonly reported at the treatment or study levels rather than the fishing operation level. The ideal would of course be to share raw data when the study is published (as Fox et al., 2020 did) as this would allow animal-scale analyses by performing mega-analyses (Eisenhauer, 2021).

4.4. Utility of meta-regression for informing management on Nephrops discard survival and its mitigation

Discard related mortality can best be reduced by "simply" avoiding unwanted catch through strategic and tactical modification of fishing practices, for example improved gear selectivity (Reid et al., 2019), and through better utilisation of the "unwanted catch" (Iñarra et al., 2019). Where unwanted catches cannot be avoided, the welfare and hence survival of any discarded animals could theoretically be improved with changes in fishing practices, including limiting haul duration, reducing catch sizes, and improved handling of the catch during retrieval and sorting on deck (Breen et al., 2020). To this end, information from MR on the effect sizes of the different drivers of discard survival for a given species and fishing practices provides high-value recommendations to improve discard survival.

In the case of trawl fisheries for *Nephrops*, this MR has identified the following factors as influential in the survival of discarded animals: CL, catch weight, fishing depth, haul duration and SST. The reduced survival of large *Nephrops* emphasises the need for stronger measures to encourage better catch utilisation in this fishery. One might note though, that many studies were conducted before the landing obligation, when "high grading" was a legal practice. Improved selectivity, for example by using grids to separate *Nephrops* from bycatch, is likely to substantially reduce catch weight and hence improve resulting discard survival (Catchpole and Revill, 2008; Valentinsson and Ulmestrand, 2008). The importance of fishing depth for survival may inform the selection of locations for temporary or permanently closed conservation areas (Carvalho et al., 2019). Haul duration is an easily adjustable operational tactic, which also affects catch quality and presumably price. However, conducting more but shorter hauls may impact crew rest periods and thus safety. Surface temperature effects could be mitigated by improved handling techniques to minimise air exposure and avoiding direct exposure of the catch to sunlight (Breen et al., 2020), or through seasonal regulation of fishing and discarding practices (García-De-Vinuesa et al., 2020). Technical approaches may also work as for example a 'high survival' exemption to the EU Landing Obligation, was recently included in the discard management plan for the Bay of Biscay (France), but conditional on the use of a discarding chute system (Mérillet et al., 2018).

One important but unresolved factor pointed out in Fox et al. (2020) is that the SST effect on post-discard survival could be partly driven by recovery conditions in the experiments. Captive recovery studies have mostly been undertaken using aquaria where the water temperature is often determined by surface water supplying the aquaria, and is thus elevated in summer experiments. It is possible that poorer survival, especially of injured *Nephrops*, in summer studies might be related to differences in bacterial loading in warmer waters. Thus, Fox et al., (2020) suggested that further studies are required to isolate that factor, with seawater temperatures in the aquaria used for recovery experiments being more tightly controlled, e.g. similar to bottom temperatures.

It is common practice in clinical MA to provide an overall mean estimate with confidence interval for the response variable of interest (Hedges and Olkin, 2014). It is also common to compare two or more treatments, assuming a random contribution of all other factors inherent in the studies. Studies are therefore effectively assumed to be based on independent samples from the same population. In contrast, discard survival is highly dependent on fishing conditions (Benoît et al., 2010) and studies have different experimental aims, such that one cannot assume unconditionally that studies are drawn from a common population. In the present study, treatments were study specific and often chosen by the original studies as means of improving expected survival potential, and so were not always meant to be reflective of conditions in the respective *Nephrops* fisheries. Therefore, it was not meaningful to produce a reliable mean survival estimate in European waters, even at the scale of each ICES area, by simply averaging the survival estimates. Nevertheless, this objective could be achieved without a representative sample using the predictive capacity of the MR. Indeed, survival rates may be predicted by the operational, environmental and biological conditions that are representative of the fisheries of interest, provided that (1) the relationship between fishing conditions and discard survival is well quantified, and (2) the ranges of fishing conditions included in the model are on the whole representative of all the relevant fisheries. Such model predictions can be generated with the R script provided in Supplementary 4. This is especially relevant considering the ongoing efforts to include discard survival in stock assessments, which requires producing rates at the specific scale of the management unit (ICES, 2021).

CRediT authorship contribution statement

Conceptualization and Methodology: Benoît, H.P., Breen, M., Morfin, M., Savina, E., Teixeira Alves, M.

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Data curation: Albalat, A., Breen, M., Catchpole, T., Fox, C., McHugh, M., Mehault, S., Nilsson, H., Oliver, M., Valentinsson, D.

Formal analysis: Morfin, M., Savina, E.

Writing – original draft: Benoît, H.P., Breen, M., Morfin, M., Savina, E., Teixeira Alves, M.

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Declaration of Competing Interest

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.

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Appendix A. Description of the variance calculation for the survival ratio R estimate

The variance of R, V(R), was estimated using the law of total variance, by the sum of the expected conditional variance of R given C, $E_C(V_R(C))$ and the variance of the conditional expectation of R given C, $V_C(E_R(C))$:

$$V(R) = E_C(V_R(C)) + V_C(E_R(C)) \tag{1}$$

As R is the ratio between S^A and C, S^A/C, Eq. (1) becomes:

$$V(R) = E_C(V_R(C)) + V_C(E_R(C)) \tag{2}$$

Conditionally to C, the variable can then be extracted as a constant:

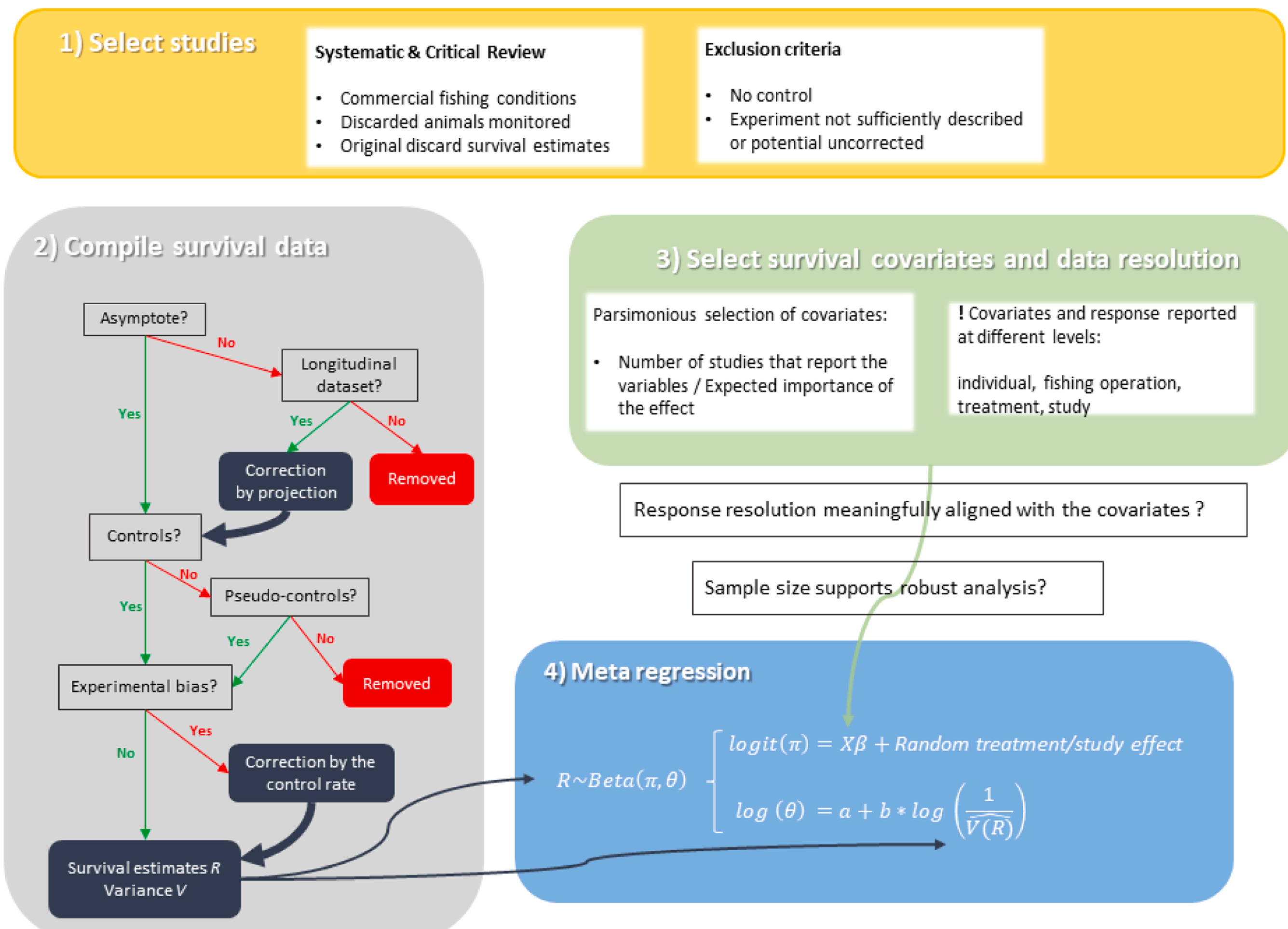
$$V(R) = E_C\left(\frac{1}{C^2}V(S^A)\right) + V_C\left(\frac{1}{C}E(S^A)\right) \tag{3}$$

Finally, the same extraction can be done for the first two moments of S^A:

$$V(R) = V(S^A)E_C\left(\frac{1}{C^2}\right) + E(S^A)^2V_C\left(\frac{1}{C}\right) \tag{4}$$

The variance and expected value of the inverse of the control survival estimate $\frac{1}{C}$, $E\left(\frac{1}{C^2}\right)$ and $V(\frac{1}{C})$, respectively, were estimated by simulating proportions from a binomial distribution with parameters (n_C, C) (Supplementary 2). The mean and variance of the inverse of these simulated proportions were then calculated.

Appendix B. Illustration of the Meta Regression framework



Appendix C. Supporting information

Supplementary data associated with this article can be found in the online version at doi:10.1016/j.fishres.2025.107513.

Data Availability

Data will be made available on request.

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