Feasibility study:
discard survival

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Samenvatting

In het nieuwe Gemeenschappelijk Visserijbeleid (GVB) is een aanlandplicht (*discard ban*) ingevoerd voor (quota) gereguleerde soorten. Artikel 15, paragraaf 2 van het nieuwe GVB bepaalt echter een vrijstelling van de aanlandplicht voor vissoorten waarvoor een hoog overlevingspercentage kan worden aangetoond, zodat verspilling van vis die zou kunnen overleven wordt tegengegaan. Maar wat is een 'hoog overlevingspercentage' en hoe kan dat wetenschappelijk worden aangetoond?

In dit rapport worden de volgende vragen beantwoord:

1) Hoe kan een onderzoek naar de overlevingskans van ongewenste bijvangsten opgezet worden? Wat is daarvoor nodig? Wat is het tijdspad? Wat zijn de kosten?

2) Hoe kan een onderzoek naar het vergroten van overlevingskansen opgezet worden? Wat is daarvoor nodig? Wat is het tijdspad? Wat zijn de kosten?

3) Wat voor resultaten zou deze onderzoeken op kunnen leveren en wat is de betrouwbaarheid daarvan?

4) Is het mogelijk om tot een internationaal gedragen uitspraak over wat een hoge overlevingskans is voor verschillende bijvangstsoorten te komen? Wat is daarvoor nodig? Wat zijn daarvan de kosten? Als het mogelijk is om tot een internationaal gedragen uitspraak te komen, dan is het erg gewenst die uitspraak te hebben voordat het onderzoek naar de overlevingskans start.

Vraag 1 en 2 zijn beantwoord na literatuurstudie. Het resultaat van die studie is een manuscript dat wordt ingediend bij een peer-reviewed wetenschappelijk tijdschrift (bijlage A). Een ander resultaat is een beschrijving van de *best-practices* en richtlijnen; hierbij wordt ingegaan op een aantal algemene principes die in gedachte moeten worden gehouden bij elk overlevingsonderzoek. Deze richtlijnen en *best practices* kunnen gebruikt worden om andere geplande of gepubliceerde onderzoeken naar overlevingskansen te beoordelen op hun wetenschappelijke kwaliteit (Bijlage B geeft een voorbeeld). Vraag 3 is beantwoord na een gedetailleerde statistische power-analyse (Bijlage C). Het antwoord op vraag 4 is een poging om 'hoge overlevingskans' te definiëren, gebaseerd op een internationaal deskundigenrapport dat is opgesteld tijdens een internationale workshop van STECF. De belangrijkste bevindingen van deze STECF workshop zijn bijgevoegd als Bijlage D.

In deze samenvatting worden de belangrijkste conclusies uit de literatuurstudie, de power analyse en de internationale workshop samengebracht. De belangrijkste punten zijn gestructureerd aan de hand van de bovengenoemde vragen.

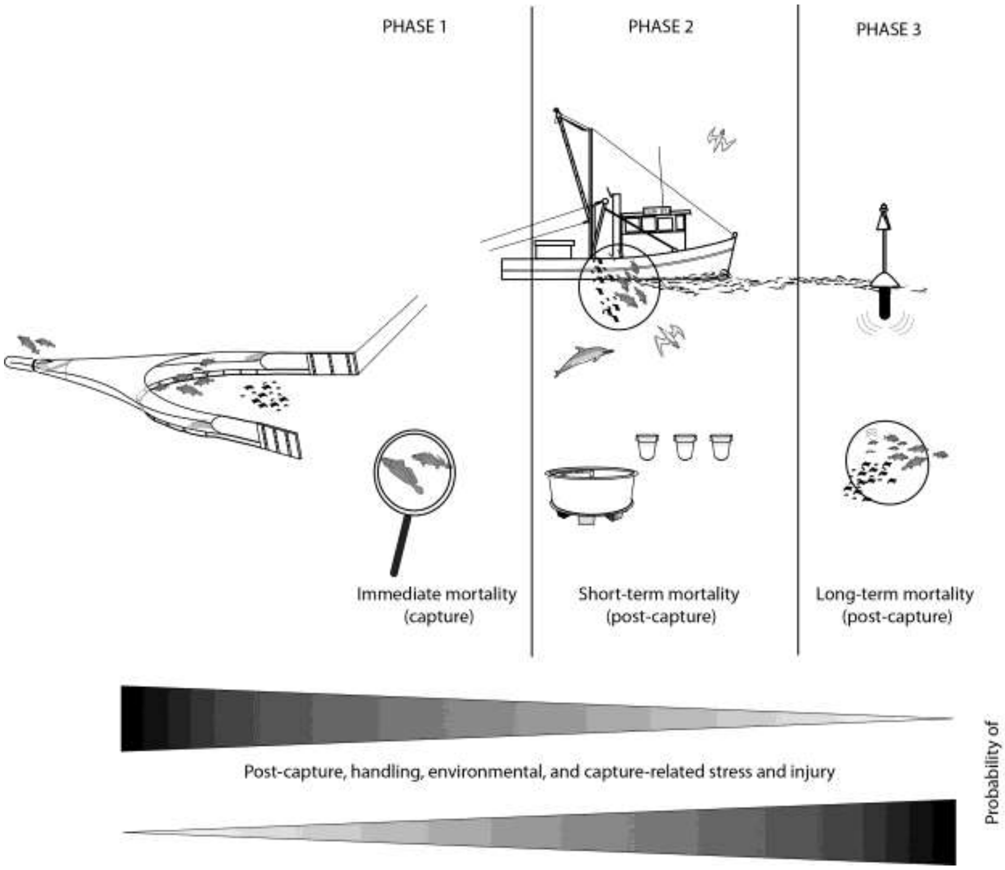
1) Hoe kan een onderzoek naar de overlevingskans van ongewenste bijvangsten opgezet worden? Wat is daarvoor nodig? Wat is het tijdspad? Wat zijn de kosten?

1.i) Opzet

Een *best-practice* benadering van overlevingskans (hierna genoemd naar conventie 'discardsterfte'), zoals bedoeld in artikel 15 van het GVB, bevat drie niveaus of fasen:

- Fase 1: beschrijving van soorten, visserijen en milieuomstandigheden die van belang zijn bij het beoordelen van discardsterfte (een zogenaamde 'meta-analyse', op basis van bestaande gegevens). Deze fase omvat ook een beperkte beoordeling van de directe sterfte of van vitaliteit van dieren die gediscard worden.
- Fase 2: beoordeling van 'uitgestelde sterfte' op korte termijn (dat wil zeggen: dagen) na het discarden
- Fase 3: beoordeling van sterfte op lange termijn (dat wil zeggen: maanden) na het discarden.

Deze *best-practice* aanpak is ontwikkeld op basis van een evaluatie van de kosten en baten van eerdere studies naar discardsterfte. De uitkomst van een fase bepaalt of het zinvol is om de volgende fase in te gaan.



Fase 1: meta-analyse
In een meta-analyse worden de soorten, visserijen en milieuomstandigheden beschreven die van belang zijn bij het beoordelen van discardsterfte. Naast deze analyse kan een aantal kleinschalige directe-sterfte en vitaliteit assessments worden gedaan om te bepalen of er toegevoegde waarde is om een bepaalde vissoort en visserijtype meer in detail te gaan onderzoeken. Zo kan er in deze fase bijvoorbeeld gekeken worden naar een aantal soorten die specifiek zijn voor de vangstsamenstelling in een bepaalde visserijtak.

Er is een aantal biologische, technische en milieu- omstandigheden (Bijlage A - paragraaf 2.1.4 *stressors*) die een lage discardsterfte waarschijnlijk maken voor bepaalde soorten en binnen bepaalde visserijtypen (Bijlage A - paragraaf 2.2 *Guideline*). Belangrijke stressfactoren zijn het vangstvolume, trekduur, vissnelheid, blootstelling aan de lucht, temperatuur en de gradiënten in zoutgehalte. Met de combinatie van de stressfactoren en de biologische kenmerken van een soort kan een inschatting van de te verwachten discardssterfte worden gemaakt. Zo kan men verwachten dat kwetsbare soorten die worden gevangen in lange trekken door gesleepte bodemvistuigen in diep water met een rotsachtige of zandige bodem veel letselschade hebben en als gevolg een hoge sterftekans. De sterftekans zal lager zijn als de milieuomstandigheden en technische omstandigheden milder zijn. Hoe belangrijk stressfactoren zijn, kan in relatief eenvoudige experimenten worden onderzocht door waarnemers aan boord van schepen: zij kunnen inschatten wat de directe sterfte is en aan welk soort stress en lichamelijk letsel de dieren bloot komen te staan. Tijdens zulke reizen kan ook worden gekeken naar sterfte door predatie van vogels op discards. Dit soort experimenten geven, samen met de meta-analyse, een indicatie van de orde van grootte van discardsterfte. Als blijkt dat een groot aandeel van de vissen bij het aan boord komen van een schip al dood is, dan is het weinig zinvol om naar de volgende fase te gaan en te kijken naar de discardsterfte gedurende een paar dagen na de vangst (fase 2). In deze eerste fase kunnen ook *a priori* schattingen worden gedaan van de statistische onzekerheden rond de schattingen. Deze schattingen kunnen vervolgens worden gebruikt in een *power analyse* om de benodigde steekproefomvang van een vervolgexperiment te kunnen bepalen.

Fase 2: beoordeling van uitgestelde sterfte
Als zowel de directe discardsterfte en de sterfte door vogelpredatie laag genoeg zijn, kan in fase 2 een directe beoordeling van uitgestelde sterfte worden uitgevoerd. Dit gebeurt onder meer door het plaatsen van gevangen en nog levende dieren in kooien of bakken en het volgen van hun ontwikkeling gedurende een bepaalde periode. Deze experimenten zullen slechts een gedeeltelijke schatting geven van de totale discardsterfte, omdat bijvoorbeeld het effect van predatie door vogels niet kan worden meegenomen. Als de sterfte in gevangenschap zoals gemeten in fase 2 consistent laag is, kan een beoordeling van de sterfte op langere termijn overwogen worden (fase 3).

Het verdient aanbeveling om in studies met bakken of kooien: 1) een controlegroep te hebben: dit is een groep vissen die bij voorkeur uit dezelfde populatie komen, maar die het vangstproces niet hebben meegemaakt. Zo kunnen eventueel verstorende effecten van de bakken of kooien getoetst worden; 2) individuele vissen niet te beschouwen als statistische herhalingen maar als onderdeel van een experiment. Als er meerdere vissen in een bak of kooi worden geplaatst, kan worden verwacht dat de vissen elkaar beïnvloeden. Daarom is de bak in een dergelijk geval het experiment en maken alle vissen in die bak deel uit van dat experiment; 3) precisiedoelstellingen te formuleren voordat wordt begonnen met de experimentele studie, zodat een geschikt aantal vissen wordt ingezet; en 4) te beoordelen of een bepaalde soort daadwerkelijk kan worden gehouden in gevangenschap en om de condities waaronder ze worden gehouden af te stemmen op de specifieke behoeften.

Fase 3: beoordeling van de lange termijn sterfte
Fase 3 is een langdurige, beoordeling van de discardsterfte op langere termijn, door merkexperimenten. In dit soort experimenten worden levende vissen voorzien van een merkje of een elektronische tags ('bio-telemetrie'), waarna ze zo snel mogelijk weer in zee worden terug gezet. Als de vissen later weer worden opgevist, kan dat informatie geven over de discardsterfte op langere termijn nadat vissen zijn gediscard. In dit soort proeven moet in de conclusies rekening gehouden worden met het verlies van merken en van de sterfte die wordt veroorzaakt door de tags zelf. Het vereist ook een schatting van de terugvangstwaarschijnlijkheden. Dergelijke experimenten zijn Het kan daarom alleen geschikt voor soorten die in grote hoeveelheden worden gevangen, met voldoende stimulans voor het melden van teruggevangen vis.

Naast de directe schattingen van discardsterfte zoals die in fasen 1 en 2 kunnen worden gedaan, zijn ook indirecte schattingen mogelijk. Deze indirecte schattingen worden gebaseerd op factoren die samenhangen met sterfte, zoals vitaliteit, reflexbeoordeling of fysiologische metingen. Deze indirecte schattingen kunnen een indicatie geven van de conditie en overlevingskansen van gevangen en gediscarde vis (Bijlage A, 2.1.1 *Direct assessment of discard mortality* en 2.1.2 *Indirect assessments of discard mortality*). Voor elk van deze methoden zijn de voor-en nadelen besproken in bijlage A, 2.1.1.2.1 *Captivity studies* en 2.1.1.2.2 *Free-range studies*.

1.ii) Tijdsduur
De benodigde tijdsduur van projecten en monitoringperiodes kunnen aanzienlijk variëren, afhankelijk van de keuze van directe of indirecte schattingen en de doelen die worden geformuleerd (Bijlage A, 2.1.1.2.3 *Time period of monitoring*). Projecten in fase 1 kunnen enkele maanden duren om bijvoorbeeld seizoenseffecten op directe sterfte te onderzoeken. Voor het schatten van korte termijnsterfte (fase 2), zal de duur van de monitoring in het algemeen variëren tussen 3 en 20 dagen met een maximum van 60 dagen. De dieren moeten in gevangenschap gevolgd worden tot de bij elkaar opgetelde sterfte afvlakt. Als het experiment te vroeg stopt, wordt de sterftekans onderschat omdat de kans nog groot is dat er in de dagen na het experiment ook nog beesten doodgaan. Korte periodes van waarneming zorgen ervoor dat effecten die indirect tot sterfte kunnen leiden, zoals een afnemende voortplantingsconditie, niet

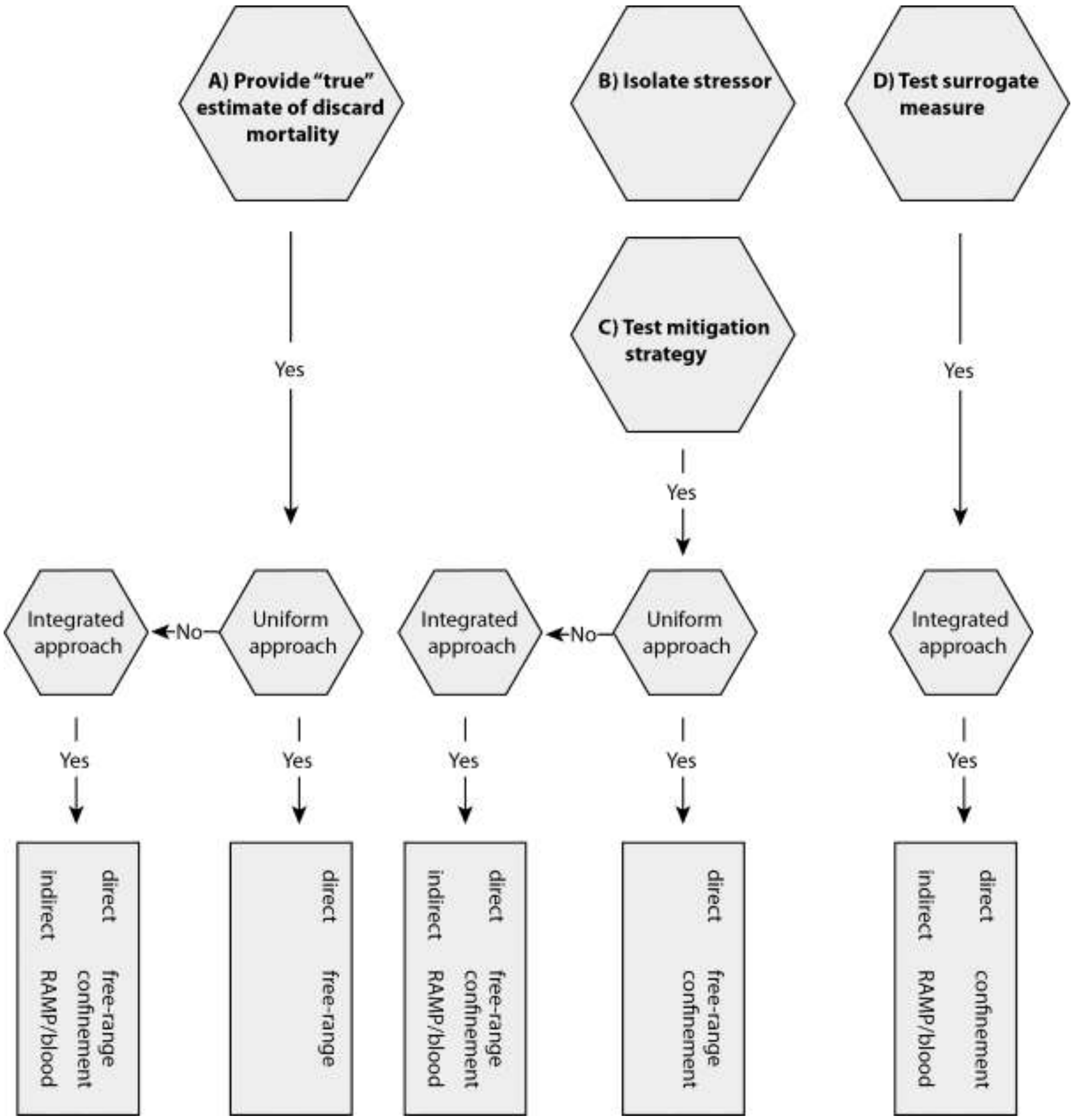
wordt opgemerkt. Beoordelingen op de langere termijn (fase 3), zoals via merkexperimenten kunnen de volledige levenscyclus van de proefdieren overspannen en duren om die reden meestal maanden of jaren.

1.iii) Kosten
De evaluatie van discardsterfte, met name in fase 2 en 3, zijn arbeidsintensief en brengen hoge kosten met zich mee (Bijlage A, 2.1.5 *Costs*). Daarom is het belangrijk om in fase 1 zorgvuldig een kwalitatieve en kwantitatieve inschatting te maken van de directe sterfte en verwondingen, voordat men gaat investeren in fase 2 of 3. Gedetailleerde kosten-baten analyses van deze fasen zijn helaas grotendeels afwezig in de wetenschappelijke literatuur en kunnen dus niet worden gepresenteerd.

2) Hoe kan een onderzoek naar het vergroten van overlevingskansen opgezet worden? Wat is daarvoor nodig? Wat is het tijdspad? Wat zijn de kosten?

2.i) Opzet
Voor het schatten van het effect van een alternatieve behandelingswijze (aanpassingen in de vis- of verwerkingstechniek) op de discardsterfte, is een zogenaamde *integrated assessment* een mogelijke methode (Bijlage A, 2.2 *Guideline*). In een uniforme benadering wordt slechts een methode toegepast. Bij een *integrated assessment* worden binnen verschillende methoden van directe metingen in gevangenschap (zoals in fase 2) gecombineerd met indirecte schattingen op basis van bijvoorbeeld reflexbeoordelingen en bloedanalyse. *Integrated assessments* kunnen ook worden gebruikt om factoren te identificeren die bijdragen aan de waargenomen sterfte. De onderstaande beslisboom schetst de keuze van geschikte methoden (of een combinatie van uniforme en geïntegreerde benadering) in relatie tot het doel van de studie.

Voor het bepalen van stresseffecten en het testen van een alternatieve behandelingswijze, worden zowel uniforme en geïntegreerde benaderingen gebruikt. Een geïntegreerde aanpak kan het effect van een alternatieve behandelingswijze testen door een directe beoordeling (studie met kooien of merkexperiment) te combineren met een indirecte beoordeling door een reflextest als indicator voor sterfte. Het voordeel van zo'n geïntegreerde aanpak is dat een correlatie tussen de indirecte meting met de directe meting kan worden gebruikt als een alternatieve meetmethode. In de toekomst zou dan de eenvoudige reflextest worden gebruikt om inzicht te geven in sterfte en is de kosten- en arbeidsintensieve directe schatting van uitgestelde sterfte niet meer nodig.



2.II) Tijdsduur
De looptijd van het project en de monitoringperiodes bij het testen van alternatieve behandelingswijzen, zijn over het algemeen korter vergeleken met een studie waarin de overlevingskans moet worden vastgesteld. Dit komt omdat de relatieve verbetering van overlevingskans belangrijker is dan de absolute overlevingskans. Het is dan niet nodig om dieren te monitoren totdat een afvlakking in de snelheid waarmee sterfte plaatsvindt.

2.III) Kosten
Proeven voor aanpassingen in vistuig en behandeling hebben ook lagere kosten dan studies waarin de overlevingskans moet worden vastgesteld. Er zijn minder herhalingen van de proef nodig (bijv. visreizen) om veranderingen in overleving aan te tonen. Dit geldt vooral als de aanpassing een groot effect heeft op de discardsterfte .

3) Wat voor resultaten zou dit onderzoek op kunnen leveren en wat is de betrouwbaarheid daarvan?

De haalbaarheid van een discardsterfte studie hangt af van enerzijds de kosten en hoeveelheid werk en anderzijds de te verwachten resultaten (en waarde daarvan) die kunnen worden bereikt. Directe en indirecte beoordeling van de discardsterfte zijn arbeidsintensief en brengen hoge kosten met zich mee. Een belangrijk onderdeel van de kosten wordt bepaald door het aantal benodigde herhalingen van een proef: hoeveel reizen, trekken, kooien/bakken en individuen zijn er nodig. Dit zijn essentiële elementen om tot conclusies te kunnen komen.

Er zit veel variatie in meetbare resultaten door de invloed van biologische, technische en milieuomstandigheden. Bovendien kan de proefopstelling zelf verstorende effecten hebben (bijvoorbeeld extra sterfte door het plaatsen van een kooi in ongunstige omstandigheden voor een bepaalde soort). Daarom is het noodzakelijk om ook controledieren te gebruiken die niet door het normale vangstproces zijn beïnvloed of in ieder geval voldoende tijd hebben gehad voor het herstellen van het vangstproces. Met behulp van controledieren kunnen ongewenste, verstorende en experimentele effecten worden bepaald.

Statistical Power Analysis is een statistische techniek om te bepalen hoeveel informatie nodig is om robuuste uitspraken te kunnen doen over de waargenomen effecten. De vraag is: kun je een effect met een onderzoek aantonen, als dat effect in werkelijkheid bestaat? Idealiter hebben veldexperimenten een hoge statistische power nodig om een effect aan te kunnen tonen. Onderzoeken naar discardsterfte hebben vaak een lage statistische *power*, vanwege de beperkte steekproefomvang en de lage herhaling. Hoe meer variatie er is in discardssterfteschattingen, hoe groter het aantal waarnemingen dat nodig is om effecten te kunnen aantonen. Discardsterfteschattingen kennen vaak hoge onzekerheid door variabele ecologische, technische, biologische en experimentele omstandigheden. Daardoor kan het gemeten effect van een bepaalde behandeling kan klein zijn omdat het wordt gemaskeerd door de variabiliteit. Een voorbeeld van een a priori *power* analyse van discardsterfte-experimenten wordt hier onder gegeven:

> *Men wilde een statistische power van meer dan 0.8, in combinatie met een effectgrootte variërend tussen 0.1 (klein effect) en 1 (groot effect). Er waren twee verschillende behandelingen: A. de omgeving waarin vissen worden bewaard en B. de duur van blootstelling aan lucht. Het vereiste aantal herhalingen per combinatie van behandelingen varieerde tussen de 3 en 600 herhalingen, afhankelijk van het aantal gebruikte vissen en de variantie tussen elke behandeling (zie Bijlage C, Tabel 8). Indien het verschil tussen de behandelingen relatief klein is, en een duidelijk effect van de behandeling wordt waargenomen (effect grootte = 1), dan zijn slechts 3 herhalingen nodig om het effect aan te kunnen tonen. Als er daarentegen weinig bewijs is van een behandelingseffect (effect grootte = 0.1) en er zijn grote verschillen tussen de behandelingen, dan kan het benodigde aantal herhalingen oplopen tot 600 om een significant effect aan te kunnen tonen.*

Dit voorbeeld illustreert dat veel herhalingen nodig kunnen zijn om een statistisch robuust resultaat te bereiken, vooral wanneer het effect klein is en verschil in discardsterfte tussen behandelingcombinaties groot is.

Belangrijke overwegingen die door een discardsterftebeoordeling moet worden aangepakt zijn gebundeld in een checklist. Het kan worden gebruikt om eerdere studies te evalueren op hun robuustheid en wetenschappelijke methodes (Appendix B). Bijvoorbeeld, de keuze van een geschikte controleperiode, het gebruik van onbehandelde controles, de selectie en de meting van de belangrijkste stress-effecten, en het niveau van haalbare en betaalbare herhalingen.

Essential
Clearly defined objective leading towards a coherent design and methodology ☐
Use of valid controls (sourced from the same population than treatment fish) ☐
Realistic monitoring period ☐
Homogenous stress history of study organisms ☐
Plausible fishing conditions ☐
Verifiable endpoints ☐
Replication (rule of thumb): at least 15 observations per explanatory variable ☐

Optional
Measures population impact: link from individual to population level ☐
Manipulative experiments (distinguishable treatments) ☐
Field experiments (for more realistic conditions) ☐
Integrated approach (e.g. direct and indirect assessments) ☐

4) Is het mogelijk om tot een internationaal gedragen uitspraak over wat een hoge overlevingskans is voor verschillende bijvangstsoorten te komen? Wat is daarvoor nodig? Wat zijn daarvan de kosten?

Soorten met een hoge overlevingskans kunnen worden vrijgesteld van de aanlandplicht. Bepaalde morfologische en fysiologische eigenschappen geven sommige soorten meer kans op sterfte dan andere soorten. Generiek gesproken, zijn bijvoorbeeld sommige haaien en roggen, slijmprikken, paling en schorpioen vissen tolerant voor discarden. Een algemene drempelwaarde voor hoge overlevingskans kan echter niet worden gegeven (zie ook appendix A, 2.2.1 *What constitutes high survival?*). Tijdens een internationale werkgroep van STECF (EWG 13-16) werd geconcludeerd dat een definitie van hoge overlevingskans gekoppeld zou moeten zijn aan een expliciet doel van de uitzonderingsbepaling op de aanlandplicht (Bijlage D). Dat expliciete doel is geen onderdeel van de regelgeving. Omdat de uitzonderingsbepaling opgenomen zal moeten worden in regionale discardplannen of lange termijn beheerplannen, zou in die plannen de volgende informatie opgenomen moeten worden: 1) context en de achtergrond waarop de uitzondering van toepassing zou zijn, 2) de onderbouwing van de claim, 3) de gevolgen voor toezicht en naleving en 4) alle andere overwegingen (zoals voordelen en risico's).

Dit onderzoek is uitgevoerd binnen een EZ - programma Beleidsondersteunend Onderzoek (BO) .

Executive Summary

Under an obligation to land all sizes of (quota) regulated species ('discard ban'), Article 15, paragraph 2(b) of the new Common Fisheries Policy (CFP) specifies an exemption allowing the discarding of species for which 'high discard survival' can be demonstrated so as to minimize wastage due to foregone reproductive output and future yield. But what constitutes 'high survival' and how can it be scientifically demonstrated?

Keeping this question in mind, the assignment of this report was split into four clusters:

1) Research into the survival of discards: What are the i) set-up, ii) timeline and iii) costs to do research into the survival of discards?

2) Research into the improvement of survival of discards: What are the i) set-up, ii) timeline and iii) costs to do research into the improvement of survival of discards?

3) Reliability of results: What are the results of such research and their reliability?

4) Definition of high survival: What is 'high survival' according to the best-available international expert opinion?

A literature review addressed questions 1) – 2). This resulted in a manuscript which is going to be submitted to a peer-reviewed journal (Appendix A). It also resulted in a description of a best-practice approach and guideline which can be used to 'rate' each published study of discard mortality for their scientific rigor and robustness (Appendix B gives an example). The literature review teased out some generic principles which should be considered in any study of discard mortality. A detailed statistical power analysis contributed to question 3) (Appendix C). An attempt to define 'high survival' is based on an expert opinion that was formulated during an international STECF workshop and summarized herein to address question 4). Minutes from this workshop are attached as Appendix D.

This executive summary synthesizes all conclusions that came out of the literature review, power analysis and international workshop. The key points are structured addressing the four questions above.

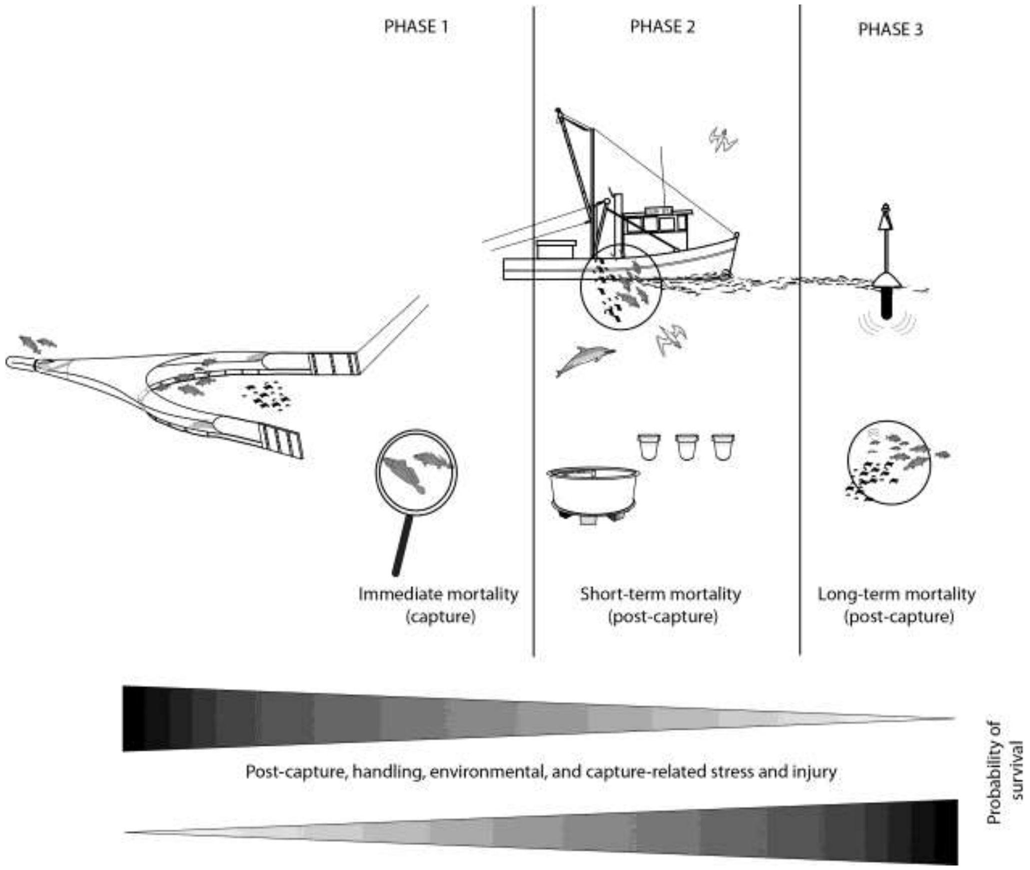
1. Research into the survival of discards

1.i) set-up

A best-practice approach to study discard survival (hereafter termed by convention 'discard mortality'), specifically addressing article 15 of the CFP, contains three tiers or phases:

- Phase 1: meta-analysis of existing evidence and an assessment of immediate mortality or vitality
- Phase 2: direct assessment of delayed short-term mortality (i.e. days) after 'discarding'
- Phase 3: direct assessment of long term mortality (i.e. months) after discarding.

This best-practice approach has been developed based upon an evaluation of the costs and benefits of discard mortality studies.



Phase 1: meta-analysis
In the first phase, a meta-analysis describes the species, fisheries and environment of interest and shall be corroborated by some small scale immediate-mortality/vitality assessments to gauge whether there would be any value in investigating a particular species and fishery. In this phase, it would be sensible to look at a range of species within the catch composition of a particular fishery.

The starting point is to quantitatively or qualitatively describe biological, technical and environmental circumstances (Appendix A - section 2.1.4 'Stressors') under which low mortality is likely for a given species and fishery (Appendix A – section 2.2 'Guideline'). Key stressors such as catch volume, haul duration, fishing speed, air exposure, temperature and salinity gradients, together with life history traits and species-specific physiological tolerances may qualitatively describe the fate of discards. For example, high injury levels and discard mortality can be expected for sensitive species captured by bottom trawls with longer than 1 hour tows in deep water with stony or sandy sediment. Contrary, if environmental and technical circumstances are mild, the probability of mortality may be low for some species. Such a meta-analysis may be corroborated by observer-based onboard quantification of immediate mortalities, stress and physical injury as a consequence of the capture process. Avian predation rates of discarded species can also be included. These experiments together with the meta-analysis would give an indication of the order of magnitude of survival probabilities or an indicator thereof. That is, if the majority does not survive at this stage, there may be no value to proceed to phase 2. Moreover, they would also provide *a priori* estimates of variance which may be used in a power analysis to define the necessary sample size for a more refined, definitive experiment.

Phase 2: direct assessment of delayed mortality
If both immediate mortality and predation rates are sufficiently and consistently low, phase 2 may include a direct assessment of delayed mortality by 'discarding' live animals into cages, containers or tanks and monitor their fate for an appropriate monitoring period. This assessment will generate only

partial estimates of discard mortality because among other the effect of predation cannot be included. However, if mortalities in captivity are consistently low, a direct assessment of free-ranging, tagged individuals could be considered. It has been recommended that in confinement studies 1) always untreated controls stemming from the same population of treatment animals should be used to account for any confounding effects from being held in captivity; 2) individual specimens are not being treated as replicates; This is the case when multiple fish are place in a container and where the container should be treated as the replicate; 3) precision targets are set and explored before beginning with an experimental study; and 4) to evaluate whether a given species can actually be held in captivity and to match the holding conditions to its specific needs (e.g. demersal versus pelagic species; large rays and sharks vs small fish).

Phase 3: direct assessment of long term mortality
Phase 3 may be a long-term, direct assessment of discard mortality by monitoring tagged and discarded individuals using traditional mark-recapture or electronic tags. Such an approach requires quantification of tag loss, and tagging-related mortality and estimation of recapture probabilities. It may be only appropriate for species that are caught in large numbers, with strong incentives for reporting recaptures.

Apart from direct assessments where the measured endpoint is death, phases 1 and 2 above may benefit from indirect assessments of correlates of mortality such as vitality or reflex tests or physiological measurements. These may give an indication of the condition and survival probabilities of captured-and-discarded animals (Appendix A, sections 2.1.1 *Direct assessment of discard mortality* and 2.1.2 *Indirect assessments of discard mortality*). For each of these methods, the advantages and disadvantages have been discussed (Appendix A, sections 2.1.1.2.1 *Captivity studies* and 2.1.1.2.2 *Free-range studies*).

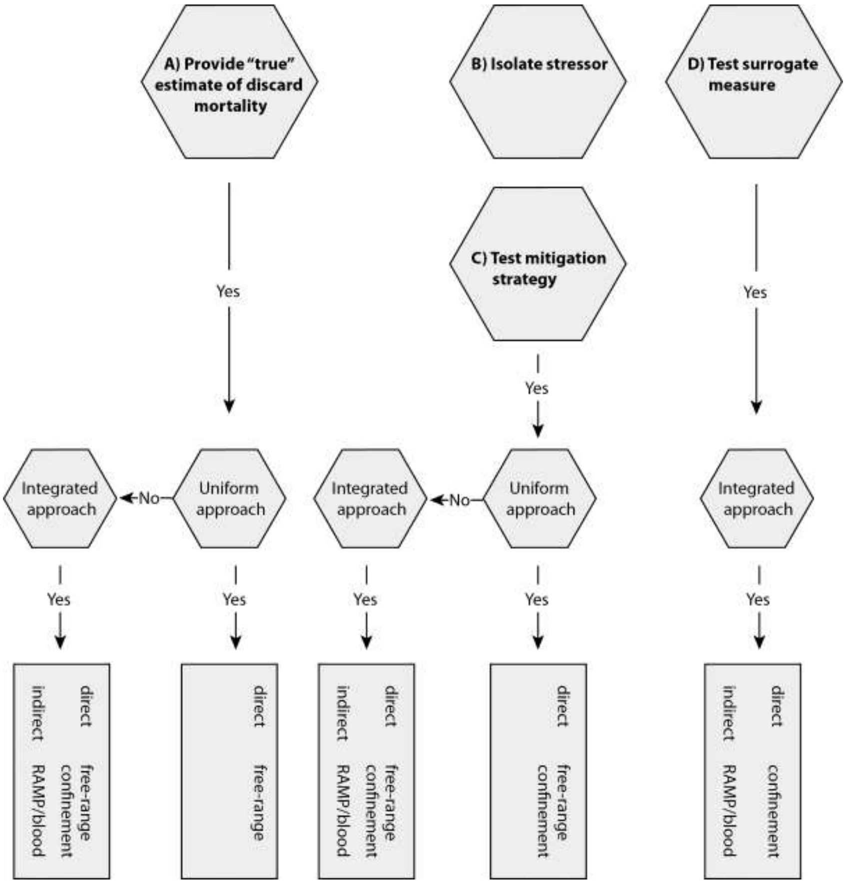
1.ii) Timeline
Project duration and monitoring periods can vary considerably depending on the choice of either direct or indirect assessments and the projects objective (Appendix A, section 2.1.1.2.3 *Time period of monitoring*). Projects may last for several months to capture seasonal effects. For the assessment of short-term mortalities by holding animals in captivity, monitoring periods generally range between 3 and 20 days with a maximum of 60 days. Animals should be monitored in confinement until cumulative mortality curves reach a plateau because otherwise outcomes could be biased and incomparable between studies. Short monitoring periods imply that sub-lethal effects such as diminishing reproductive condition or output are typically not documented. Longer-term assessments such as mark-recapture studies may span across the complete life cycle of the study animals and can therefore last for months or years.

1.iii) Costs
Discard mortality assessments, especially in phase 2 and 3 are cost and labour intensive (Appendix A, section 2.1.5 *Costs*). Therefore, it is important to carefully, qualitatively and quantitatively assess the order of magnitude of immediate mortalities and injuries in phase 1 before proceeding to invest into phase 2 or 3. Unfortunately, detailed cost-benefit analyses of these phases are largely absent from the scientific literature.

2. Research into the improvement of survival of discards

2.i) Set-up
If the objective of a discard mortality study is to assess the potential impacts of changes in gears, practices or handling in mitigating and reducing the mortality of discards, approaches such as integrated assessments could be considered (Appendix A, section 2.2 *Guideline*). In a uniform approach a single method is applied. Integrated assessments combine within the same study methods of direct measurements in captivity with indirect assessments such as reflex tests and blood chemistry measurements. Integrated assessments may also be used to identify factors that contribute to the observed mortality. A decision tree outlines the choice of appropriate approaches (or their combination of uniform and integrated approaches) with respect to a generic objective of a study (a – d). For both objectives b) and c), either a uniform or integrated approach have been used in some of the reviewed

studies. An integrated approach may test the utility of a particular mitigation strategy by combining a direct assessment (study of discards in captivity or in free range) with an indirect assessment using for example reflex tests as an indicator of mortality. The advantage of such an integrated over a uniform approach is, that if the indirect measurement correlates strongly with observed mortalities is, it may be used as a surrogate measure and saves the costs and labour of monitoring discards for delayed mortalities.



2.ii) Timeline

The project duration and monitoring periods to test mitigation devices are typically shorter compared with baseline or reference studies. This is because the relative improvement in survival is more important than the absolute number. Thus it may not be necessary to monitor animals until delayed mortalities have completely ceased for a consecutive number of days.

2.iii) Costs

Mitigation trials generally involve lower costs than baseline mortality studies, because less replication (e.g. fishing trips) is needed to demonstrate improvements in survival. This is especially the case if the mitigation method or device proves to be very effective in reducing mortality rates.

3.) Reliability of results of discard mortality research?

The feasibility of any discard mortality study relies on an evaluation of the required costs against the value to be gained. Direct or indirect assessments of discard mortality are labour- and cost-intensive. A key component of the costs are typically associated with the level of replication of how many trips, hauls, cages or containers and the sample size of how many individuals of a species can be assessed for their discard mortality. These are essential elements to make an inference on a particular outcome. Statistical significance of an outcome is as important as statistical power of the test.

The measurable outcomes, such as a count of fatalities, are likely to be variable due to the associated biological, technical and environmental stressors. In addition, the scientific assessment method may generate confounding effects. Therefore, it is imperative to use control animals that have not gone through the capture-and-discarding process or at least have had sufficient time to recover from it. Using control animals will account for any undesirable, confounding and experimental effects during the monitoring of survivors.

Power analysis is a statistical technique to assess the amount of information that is needed to make robust statements on the observed effects. In any ecological study where the null hypothesis is not rejected for a given level of significance, the probability of committing a type II error (not rejecting a false null hypothesis) should to be calculated. This indicates the statistical power of the test.

Experimental designs of discard mortality studies are prone to low statistical power because of limited sample sizes and low replication. Ideally, field experiments should have a high statistical power to detect an effect if an effect exists. High variability in discard mortality estimates in a treatment, haul or trip could be an effect of variable environmental, technical, biological and experimental conditions. In those situations, effect size for a given treatment may be small.

> *An example of an a priori power analysis of discard mortality experiments was carried out, where a desirable statistical power of >0.8 was aimed for in combination with an effect size between 0.1 and 1. Two different treatments were considered: holding environment and air exposure levels. The required number of replicates per treatment combination ranged between 3 and 600 replicates depending on the number of fish used and the variance between each treatment (Appendix C, Table 8). In scenario 2 of Table 8, if the variance between treatments is relatively small, and a clear effect of the treatment can be observed (effect size = 1), then only 3 replicates in this case are required. In contrast, if there is very little evidence of a treatment effect (effect size = 0.1) and large variation between treatments, up to 600 replicates may be needed per treatment to demonstrate a significant effect.*

This example illustrates that many replicate experimental units may be required to provide a statistically robust result, especially if the effect size is small and variance in mortality rates between treatment combinations is large.

Key considerations that should be addressed by any discard mortality assessment have been compiled in a checklist. It may be used to rank studies for their robustness and scientific rigor (Appendix B). For example, the choice of an appropriate monitoring period, the use of untreated controls, the selection and measurement of key stressors, and the level of achievable and affordable replication.

Essential
- Clearly defined objective leading towards a coherent design and methodology ☐
- Use of valid controls (sourced from the same population than treatment fish) ☐
- Realistic monitoring period ☐
- Homogenous stress history of study organisms ☐
- Plausible fishing conditions ☐
- Verifiable endpoints ☐
- Replication (rule of thumb): at least 15 observations per explanatory variable ☐

Optional
- Measures population impact: link from individual to population level ☐
- Manipulative experiments (distinguishable treatments) ☐
- Field experiments (for more realistic conditions) ☐
- Integrated approach (e.g. direct and indirect assessments) ☐

4. Definition of *High survival*

Species with a high survivability may be exempted from the landing obligation. It is well known that certain morphological and physiological traits make some species more prone to discard mortality than others. Generically speaking, some sharks and rays, hagfishes, eels, and scorpion fishes among others, seem to be quite tolerant to being discarded. However, a general threshold level of survival cannot be given (Appendix A – section 2.2.1 *What constitutes high survival?*). An international STECF workshop (EWG 13-16) concluded that a definition of high survival would require an explicit goal for the exception to the landing obligation (Appendix D). The regulation does not contain such an explicit goal. Because the exceptions on the basis of high survival will need to be embedded in regional discard plans or long term management plans, those plans would need to present the basis for the exceptions as follows: 1) context and background to which the exception rule applies, 2) the evidence base for the claim, 3) the monitoring and surveillance implications and 4) any other considerations (like benefits and risks).

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Justification:

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The scientific quality of this report has been peer reviewed by a colleague scientist and the head of the department of IMARES.

Approved: Prof. dr. Adriaan Rijnsdorp
Senior Researcher

Signature:

Date: 29/1/2014

Approved: Nathalie Steins
Afdelingshoofd Visserij

Signature:

Date: 4/2/2014

Introduction

Under an obligation to land all sizes of (quota) regulated species ('discard ban'), Article 15, paragraph 2(b) of the new Common Fisheries Policy (CFP) specifies an exemption allowing the discarding of species for which 'high discard survival' can be demonstrated to minimize wastage due to foregone reproductive output and future yield. But what constitutes 'high survival' and how can it be scientifically demonstrated? Keeping this question in mind, the assignment of this report was split into four clusters:

1) Research into the survival of discards: What are the i) set-up, ii) timeline and iii) costs to do research into the survival of discards?

2) Research into the improvement of survival of discards: What are the i) set-up, ii) timeline and iii) costs to do research into the improvement of survival of discards?

3) Reliability of results: What are the results of such research and their reliability?

4) Definition of high survival: What is 'high survival' according to the best-available international expert opinion?

Reading guide

The main aim of this report is to add to the scientific research activities regarding the survival of discarded fish. In order to facilitate the uptake of the report, it has been structured as a series of appendices:

Appendix A. Draft scientific paper on a guideline to quantify discards mortality
Appendix B. Example evaluation of a discard mortality study using the above guideline
Appendix C. *A priori* power analysis of a discard mortality experiment
Appendix D. Excerpt from STECF expert group on mortality of discards

The specific research questions for this report are addressed in the following report sections:

Question	Report section
1) Research into the survival of discards: What are the i) set-up, ii) timeline and iii) costs to do research into the survival of discards?	Appendix A: "Draft scientific paper on methods and a guideline to quantify discard mortality" Appendix A: section 2.2 "Guideline" Appendix B: Example evaluation of a discard mortality study using the guideline
2) Research into the improvement of survival of discards: What are the i) set-up, ii) timeline and iii) costs to do research into the improvement of survival of discards?	Appendix A: "Draft scientific paper on methods and a guideline to quantify discard mortality" Appendix A: section 2.2 "Guideline" Appendix B: Example evaluation using discard mortality guidelines
3) Reliability of results: What are the results of such research and their reliability?	Appendix C: "Power analysis"
4) Definition of high survival: What is 'high survival' according to the best-available international expert opinion?	Appendix A: section 2.2.1 "What constitutes high survival?" Appendix D. Excerpt from STECF expert group on survival of discards

Conclusions

The main conclusions to the four research questions are summarized below:

1) Research into the survival of discards: What are the i) set-up, ii) timeline and iii) costs to do research into the survival of discards?

A best-practice approach to study discard survival (hereafter termed by convention 'discard mortality'), specifically addressing article 15 of the CFP, contains three tiers or phases:

- Phase 1: meta-analysis of existing evidence and an assessment of immediate mortality or vitality
- Phase 2: direct assessment of delayed short-term mortality (i.e. days) after 'discarding'
- Phase 3: direct assessment of long term mortality (i.e. months) after discarding.

Project duration and monitoring periods can vary considerably depending on the choice of either direct or indirect assessments, and the projects objective. Projects may last for several months to capture seasonal effects. For the assessment of short-term mortalities by holding animals in captivity, monitoring periods generally range between 3 and 20 days with a maximum of 60 days. Animals should be monitored in confinement until cumulative mortality curves reach a plateau because otherwise outcomes could be biased and incomparable between studies. Longer-term assessments such as mark-recapture studies may span across the complete life cycle of the study animals and can therefore last for months or years.

Discard mortality assessments, especially in phase 2 and 3 are cost and labour intensive. Unfortunately, detailed cost-benefit analyses of these phases are largely absent from the scientific literature.

2) Research into the improvement of survival of discards: What are the i) set-up, ii) timeline and iii) costs to do research into the improvement of survival of discards?

If the objective of a discard mortality study is to assess the potential impacts of changes in gears, practices or handling in mitigating and reducing the mortality of discards, approaches such as integrated assessments could be considered (Appendix A, section 2.2 'Guideline'). Integrated assessments combine within the same study methods of direct measurements in captivity with indirect assessments such as reflex tests and blood chemistry measurements. Integrated assessments may also be used to identify factors that contribute to the observed mortality.

The project duration and monitoring periods to test mitigation devices are typically shorter compared with baseline or reference studies. This is because the relative improvement in survival is more important than the absolute number. Mitigation trials generally involve lower costs than baseline mortality studies, because less replication (e.g. fishing trips) is needed to demonstrate improvements in survival.

3) Reliability of results: What are the results of such research and their reliability?

Power analysis is a statistical technique to assess the amount of information that is needed to make robust statements on the observed effects. Experimental designs of discard mortality studies are prone to low statistical power because of limited sample sizes and low replication. Ideally, field experiments should have a high statistical power to detect an effect if an effect exists. High variability in discard mortality estimates in a treatment, haul or trip could be an effect of variable environmental, technical, biological and experimental conditions. In those situations, effect size for a given treatment may be small.

An example of an *a priori* power analysis of discard mortality experiments was carried out. To reach a desirable statistical power of >0.8 in combination with an effect size between 0.1 and 1 required between 3 and 600 replicates per treatment depending on the number of fish used and the variance between each treatment. This example illustrates that many replicate experimental units may be required to provide a statistically robust result, especially if the effect size is small and variance in mortality rates between treatment combinations is large.

Key considerations that should be addressed by any discard mortality assessment have been compiled in a checklist that may be used to rank studies for their robustness and scientific rigor.

4) Definition of high survival: What is 'high survival' according to the best-available international expert opinion?

Species with a high survivability may be exempted from the landing obligation. It is well known that certain morphological and physiological traits make some species more prone to discard mortality than others. Generically speaking, some sharks and rays, hagfishes, eels, and scorpion fishes among others, seem to be quite tolerant to being discarded. However, a general threshold level of survival cannot be given (Appendix A – section 2.2.1 *What constitutes high survival?*). An international STECF workshop (EWG 13-16) concluded that a definition of high survival would require an explicit goal for the exception to the landing obligation (Appendix D). The regulation does not contain such an explicit goal. Because the exceptions on the basis of high survival will need to be embedded in regional discard plans or long term management plans, those plans would need to present the basis for the exceptions as follows: 1) context and background to which the exception rule applies, 2) the evidence base for the claim, 3) the monitoring and surveillance implications and 4) any other considerations (like benefits and risks).

Appendix A. Draft scientific paper on methods to quantify discard mortality

Towards advancing and standardizing methods to quantify discard mortality

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Running title: Guideline for quantifying discard mortality

Abstract
To continue the discarding of unwanted individuals which are likely to survive their return back to sea, operators of discard-intensive fisheries are motivated to proof their high survivorship via independent scientific assessments. This may allow them to be granted an exemption from the landing obligation of the new European Common Fisheries Policy (CFP) and thus, may reduce unwanted costs associated with sorting and landing them. Article 15 of the CFP emphasizes the need to establish reference values of discard mortality for different species and fisheries, although it can be anticipated that such values may be imprecise considering that external contributing conditions vary greatly among seasons, areas, fisheries, and possibly also due to the assessment method being used. Therefore, objective scientific guidelines are required on how to carry out and standardize discard mortality assessments i) to meet the above objective of the CFP; allow for ii) comparisons of results derived from various national studies; and iii) possibly a pooling of observations to increase sample sizes and replication levels. A three-tiered best practice approach to discard mortality assessment is suggested. In the first phase, key stressors such as catch volume, haul duration and fishing speed, air exposure, temperature and salinity gradients, together with life history traits and species-specific physiological tolerances may qualitatively describe and predict in approximate terms the likelihood of discard survival. To confirm such predictions, on-board observers may quantify capture-related immediate mortalities (and stress and/or physical injury) and avian predation rates. If these are sufficiently and consistently low, the second phase may include an direct assessment of delayed mortality by holding 'discarded' survivors in captivity and monitor their fate until cumulative mortalities cease. Although not accounting for longer-term protracted and predation mortalities, it will indicate whether a follow-up study of free-ranging, tagged individuals is needed. It has been recommended that in captivity studies 1) always untreated controls stemming from the same population of treatment animals are being used to account for any confounding effects from the caging; 2) individual specimens are not being treated as replicates; and 3) precision targets are set and explored before beginning with an experimental study. The third phase may be a long-term, direct assessment of discard mortality by monitoring tagged and discarded individuals using mark-recapture or biotelemetry techniques. Such an approach requires quantification of tag loss, and tagging-related mortality and estimation of recapture probabilities.

Keywords by-catch, discards, European Common Fisheries Policy, mortality, survival

1. Introduction

Most fishing operations inevitably leads to capture of individuals of a certain species, size, shape or gender that are unwanted and will not be retained and are 'discarded' back overboard to sea. Such a catch-and-release potentially induces stress, physical damage and/or mortality which is rarely accounted for in ecological and economic impact and stock assessments (ICES 1995; Chopin and Arimoto 1996). If it occurs persistently in large quantities, whole populations may be impacted (Pollock and Pine 2007; Cooke *et al.* 2013), although direct evidence for this assumption is largely lacking. Discarding occurs in response to regulatory and/or market forces and is considered as a waste of valuable resources and a threat to healthy fish stocks when discards die before being able to reproduce or grow to a harvestable size (EEC 2011; EWG 2013). Because the unaccounted fishing-induced mortality is unknown on a sea-basin scale (Uhlmann *et al.* 2013), it creates a source of uncertainty in stock assessments and fisheries management (Barkley and Cadrin 2012; Sauls 2014).

A cornerstone of the new European Common Fisheries Policy (CFP; ECC 2013) is the obligation to land all catches of (quota) regulated species and which aims to eliminate the practice of discarding at sea. However, a key concern is that in some fisheries overall fishing mortality may increase by also retaining unwanted catch that would otherwise have survived their release back to sea. Therefore, quantification of discard and natural mortalities for different species within variable technical, operational and environmental conditions would be essential to gauge population-level risk of increased total fishing mortality. Another reason for quantifying discard mortality, is to provide robust scientific evidence that some discarded species may rarely die and thus qualify for an exemption rule of the CFP that prescribes "*fishermen should be allowed to continue discarding species which, according to the best available scientific advice, have a high survival rate when released into the sea*" (ECC 2013). For example, hagfishes, eels, scorpion fishes, some sharks and rays among others may fall under this category given their known tolerance to the catch-and-release process (Harris and Ulmestrand 2004; Braccini *et al.* 2012; Benoît *et al.* 2013).

These and some other discarded species may not necessarily die as a consequence of the catch-and-release process. Mortalities of discards is influenced by a range of biological (e.g. species, physiology, size; and catch volume and composition), technical (e.g. gear design, deployment duration and speed) and environmental (e.g. temperature, hypoxia, sea state and availability of light) factors or so called 'stressors' (Table 1; Figure 1; Davis 2002, Broadhurst *et al.* 2006; Broadhurst and Uhlmann 2013). In other words, catch composition, choice of fishing gear, operational procedures and environmental conditions during fishing determine the stress of captured-and-discarded individuals (Davis 2002). Any type of wild capture fisheries inevitably disturbs, stresses and damages organisms. In an attempt to avoid and escape capture, the animal often rapidly modifies its behaviour (e.g. swims faster). If capture is unavoidable, an individual may become internally or externally damaged and/or experience physiological signs of stress (Figure 1). Thereby, mortality may become apparent immediately as soon as the catch is hauled on-board (hereafter termed 'immediate discard mortality') and can be attributed to the capture process (Braccini *et al.* 2012), or occurs after a delay during sorting and return back to sea (termed 'delayed discard mortality').

Following a framework by Warden and Murray (2011), both immediate and delayed discard mortality can be directly observed in experiments in which fish were exposed to conventional stressors. A stressor can be defined as a factor, which either as a consequence of a human activity (i.e. fishing) or natural conditions exceeds an organism's tolerance threshold. But isolating single attributing factors may be difficult, in particular in field environments, due to the need to control for all other influential variables. Laboratory experiments may also be useful to determine whether subsequent fieldwork was necessary (Kennelly *et al.* 1990; Uhlmann *et al.* 2009). Further, unravelling the individual and combined effects of multiple stressors may be challenging, because it will require a certain level of replication to determine whether effects are simple, additive, synergistic or antagonistic. 'Simple' responses to a stressor encompass both additive and multiplicative models (Folt *et al.* 1999), where the effect of all stressors combined is equal to the sum or product, respectively, of single effects. The 'complex' outcomes are synergistic or antagonistic, when the combined effect is respectively larger or smaller than multiplicative. For example, discard mortality of demersal species may be exacerbated due to increased predation where dredging by fishing gears has removed valuable shelters. Potentially, laboratory experiments fail to capture such more complex responses.

As opposed to immediate mortality (which can be registered on-board), delayed discard mortality typically occurs out of detectable ranges below the water surface. Thus, their cryptic nature makes quantifying discard mortalities technically challenging and costly (Broadhurst *et al.* 2006; Warden and Murray 2011; Benoît *et al*. 2013). Also, the complex nature of multiple stressor effects has prevented the development of standardized methods to provide robust and accurate mortality estimates (Gilman *et al.* 2013). Furthermore, the lack of standardized protocols impairs comparisons of mortality estimates among studies (Chopin *et al.* 1996; Pollack and Pine 2007). The combination of the inherent variability of the influencing factors together with the difficulty to accurately quantify discard mortality, makes the interpretation of any experimentally-derived mortality estimates difficult. Unsurprisingly, discard mortality rates for the same species and fishery may vary between 0 and 100% (Broadhurst *et al.* 2006; Revill 2012).

Depending on the objective of a study, the methods to provide the desired assessment may vary. For example, the objective of discard mortality studies may be to (i) provide empirical estimates of mortality (i.e. to address the CFP exemption rule or improve stock assessments), (ii) identify suitable surrogate/indirect indices of mortality (e.g. physiological parameters); (iii) identify factors that contribute to the observed mortality; (iv) comparative trials to assess mitigation measures (e.g. experiments with modified gear and/or operational fishing practices), or (v) understanding the fundamental fatal mechanisms causing discard mortality. Although, in addressing iii) or iv), a combination of methods may be used: measurements of correlative indices of mortality (e.g. physiological parameters) together with counts of mortalities in captivity.

This paper presents a guideline to a best-practice approach of discard mortality assessments by first describing the different methodologies (e.g. captivity, mark-recapture or biotelemetry), outline some pitfalls with these methods (including costs), describe the tools to quantify endpoints and suggest, a framework of standardized experimental procedures meeting the needs of the respective study objectives.

2. Methods

2.1 Review

The types of approaches to quantify discard mortality among both active and passive gears have followed similar patterns (discussed by Broadhurst *et al.* 2006 and Uhlmann and Broadhurst 2013). All methods can be categorised as either direct (i.e. counting or tracing mortality events) or indirect (using surrogate measures) assessments; both of which have been done during field or laboratory studies. In some studies, approaches were combined (e.g. Breen 1987; Kennelly *et al.* 1990; Barkley and Cadrin 2012).

For an assessment of discard mortalities, discarded animals may be (1) traced free-ranging via mark-and-recapture, biotelemetry and/or divers (e.g. Cole *et al.* 2000; Xiao and McShane 2000; Hueter *et al.* 2006; Donaldson *et al*. 2008; Sauls 2014); or (2) observed with specimens held in captivity in specifically-designed cages, tanks or containers on-board vessels, *in situ* and/or in the laboratory (e.g. Hislop and Hemmings 1971; Grant and Hiscock 2013). The common end point for many experimental studies is a count of fatalities or registration of tagged survivors, typically expressed as a probability or simple percentage or proportion of the total count of monitored specimens, or a range; although this varies according to the component in question. Although desirable, following wild fish for an extended period of time to demonstrate their successful reproduction, is often not possible (Cooke *et al.* 2013).

2.1.1 Direct assessment of discard mortality

Direct assessments imply that dead individuals can be counted or that their fate is at least traceable. Such an approach also assumes that a quantifiable endpoint is death (or some degree of incapacity, injury or measurable stress response) within a measured timeframe. Endpoints can be assessed either repeatedly (e.g. Broadhurst *et al.* 2008) or destructively at different intervals following the initial stressor (i.e. interactions with fishing gear, e.g. Broadhurst and Uhlmann 2007). Both lethal and sub-lethal endpoints may result in the same adverse effects: loss of individuals; sub-optimal growth; reduced recruitment success within the exploited population; which all surmounts to a loss of utilisable resource and foregone profits from fishing. Direct assessments often involve complex designs and potentially high costs (Table 2). The fate of discarded individuals may be monitored in captivity or free range (e.g. registration of tagged individuals). Assessments of discard mortality in captivity are typically partitioned into immediate (upon landing the catch) and delayed components (e.g. Broadhurst *et al.* 2008; Braccini *et al.* 2012).

2.1.1.1 Immediate discard mortality

Immediate mortality occurs before the unwanted catch is discarded back overboard and can be attributed to stress and injuries received during capture (Figure 1; Braccini *et al.* 2012). For an assessment of immediate discard mortalities, one of the key tasks should be the classification of the catch into 'live' and 'dead', immediately upon releasing the catch from the gear on deck. Thereby, signs of life such as any perceived movement of the body and/or operculum or responses to reflex tests may be useful criteria (Uhlmann and Broadhurst 2007; Benoît *et al.* 2013). If the majority of a species catch is already dead when brought on-board, an assessment of delayed mortality may be unnecessary. Apart from the classification of 'live' and 'dead', physical injuries such as the extent of scale loss may also be objectively scored and correlated with mortality rates (Main and Sangster 1988; Broadhurst *et al.* 2006).

2.1.1.2 Delayed discard mortality

2.1.1.2.1 Captivity

One of the most common approaches to assess delayed discard mortality is to hold 'discard' animals in cages or tanks for monitoring their post-capture well-being (Pollock and Pine 2007; Rogers *et al.* 2013). Holding these animals in captivity allows for recording of fatalities or other measurable endpoints, but as the effect of predation and infection cannot be accounted for, any mortality estimates are partial and likely to be underestimates (Raby *et al.* 2013). Animals may be held on-board commercial vessels in tanks or *in situ* in underwater cages or net pens; or in combination of field and laboratory holding facilities (Grant and Hiscock 2013). The advantage of the captivity method is its replicability.

The challenge is to minimize any deleterious and confounding captivity-related stress by mimicking natural environmental conditions (e.g. water quality: pH, temperature, salinity), minimizing stress from holding (e.g. minimal swaying movement, sediment, light, choice of housing colour and material), maintaining an optimal stocking density (based on reference values from aquaculture, aquariums or other sources; and replacing fatalities by marked controls, Broadhurst *et al.* 2012), and keeping individuals as independent from each other as possible (Pankhurst and Sharples 1992; Rottlant and Tort 1997; Hilbig *et al.* 2002; Portz *et al.* 2006). For example, captivity facilities for demersal species should allow for natural burrowing behaviour to occur and thus, require non-abrasive surfaces for the animals to rest upon (van Beek *et al.* 1990; Broadhurst *et al.* 2005). If many individuals are being held in large underwater cages, tags or fin clipping may be used to allow for individual recognition (Broadhurst *et al.* 2012; Revill *et al.* 2013), but not without accounting for any additive stress from handling.

Ideally, healthy animals that have not gone through the capture-and-discarding process are used as controls, to attribute any control mortality to the holding and not the fishing treatment (see section 'Controls'). If the prevailing conditions in one tank may elevate stress levels of confined fish compared with another tank, a 'paired design' where treatment and control fish are being held together at similar stocking densities and at independence from other tanks (e.g. no exchange of water) in each tank may be desirable (Pollock and Pine 2007; Rogers *et al*. 2013). While the number of animals held in captivity determine the sample size of the experiment, the number of holding units (e.g. tanks or cages) determines the level of replication of primary sampling units (Pollock and Pine 2007; Rogers *et al.* 2013). In a multi-level, hierarchical study design, days of fishing, and gear deployments may add further levels of replication.

The period and intervals for how long animals are held in captivity is dependent on how protracted experiment-induced mortalities are and whether the measureable endpoint is death. Very rarely have other endpoints such as growth or recruitment impairment have been considered (see section 'Time period of monitoring').

The advantage of field studies is that they reflect more realistic test conditions than can be simulated in the laboratory. The disadvantage is that unlike under laboratory conditions it is more difficult to control non-experimental variables. In either field or laboratory studies, the use of appropriate controls and accurate description of relevant variables are essential, to allow for an interpretation of the observations.

For any delayed assessment of discard mortality, it is important to decide whether a random subset of animals or strictly alive ones are used for post-capture monitoring (Broadhurst and Uhlmann 2007; Uhlmann and Broadhurst 2007). Further, it should be decided whether vitality reflexes, stress levels or external physical damage are also recorded.

2.1.1.2.2 Free-range studies

The distribution and fate of free-ranging individuals may be studied via remote and non-invasive monitoring techniques such as mark-recapture or biotelemetry using ID or electronic tags, respectively. Tagging is used where captivity studies were not an option (Snoddy and Southwood Williard 2010; Kot *et al.* 2012). For example, tags are attached to individuals retained from fishing and ideally to matching numbers of controls (e.g. animals captured from the same population, but with a low-impact method) which should all have a baseline stress history at the beginning of the trials and should be representative of the population. Tracking free-swimming fish in their natural habitat is also possible via biotelemetry using archival tags, passive integrated transponders (PIT), satellite or radio transmitters which can provide insights into (sub-) lethal effects of catch-and-release by describing behaviour, condition and fate of released fish (Donaldson *et al.* 2008). Biotelemetry provides high resolution data on the movement of released fish, but are often based on a small sample size (Neat *et al.* 2009). Some studies have combined conventional tagging (Bacheler *et al*. 2009) or captivity (Roberts *et al.* 2011) with biotelemetry approaches to compare respective estimates of fishing mortality. It has also been suggested to combine mark-recapture with biotelemetry studies under the assumption that the high resolution data of some individuals can be extrapolated to a larger, tagged proportion of a stock (Donaldson *et al.* 2008). It is also important to consider whether special animal ethics approvals and licenses are required for the tagging and handling of animals.

The advantage of tagging studies is a provision of long-term data to estimate natural and fisheries-induced mortality, including predation. But the disadvantages are: that the (i) resolution of the data are relatively low (Donaldson *et al.* 2008), especially when conventional ID tags are used; (ii) recapture/resighting probabilities may need to be adjusted, especially if individuals were tagged over a large area and at different times (Sauls 2014); (iii) individual's behaviour (Bromaghin *et al.* 2007), growth, and survival (Montgomery and Gray 1991) may be impaired (Donaldson *et al.* 2008); (iv) some tags may be lost (Montgomery and Gray 1991). To test for such impairments and also determine the rate of tag loss, appropriate tank experiments should be designed (Brattey and Cadigan 2004). Apart from the high costs of transmitting (and receiving) devices, further limitations of biotelemetry approaches include a short battery life.

For all of the above reasons, tag-and-release studies are expensive programmes that rely on the participation of many parties involved with the recovery of tagged individuals or devices to trace movements often over a wide geographical area. Even though such studies may be valuable for long-term assessments (Vander Haegen *et al.* 2004; Siira *et al.* 2006; Baker and Schindler 2009; Sauls 2014), shorter-term observational studies are deemed to be more appropriate for the quantification of approximate, partial discard mortalities. These can then be used to provide an indication whether follow-up, long-term studies are necessary.

Another non-invasive, but labour-intensive and also costly observation method to trace the fate of released fish is via scuba divers. For example, scuba observations of free ranging blue cod *Parapercis colias* had no stressful effects on the released fish, but were limited by diving times and visibility (Cole *et al.* 2000; Davidson 2001). But adverse effects such as disturbance from underwater scuba noise may occur in some cases (Chapman *et al.* 1974).

2.1.1.2.3 Time period of monitoring

To study short- and/or longer term protracted mortalities, especially in captivity requires the setting of appropriate monitoring intervals (Wassenberg and Hill 1993). Based on the reviewed literature, the temporal monitoring during experiments has varied considerably, depending on the type of experimental approach, and a species-, size, or sex-specific protraction period in response to an impact. More specifically, previous studies with various mobile and stationary gears have indicated that up to 5 days is an acceptable period to monitor confined discards for short-term direct mortalities (Broadhurst *et al.* 2006; Pollock and Pine 2007; Benoît *et al*. 2012); beyond which there is increased potential for confounding impacts when wild animals are held in captivity (Barkley and Cadrin 2012). However, there is also a risk to underestimate the effect of protracted mortalities due to internal injuries and/or infections (e.g. Mensink *et al.* 2000). For example, during experimental trials comparing conventional beam with electric pulse trawls in 2005, discarded animals were monitored for 8 days and no mortalities were recorded within the first 3 days (van Marlen *et al.* 2005). The majority of studies examining the fate of discards have opted for shorter monitoring periods, although have ranged between 3 and 20, up to a maximum of 60 days (Olla *et al.* 1998).

A key consideration is to determine the point in time when mortalities cease, indicating a plateau in the cumulative mortality curve (Benoît *et al.* 2012). However, protracted mortalities may be elusive if the cumulative mortality curve does not level out for a consecutive number of days (Barkley and Cadrin 2012). In reality, the chosen monitoring period is most likely a trade-off between what is biologically optimal and practically feasible (considering available resources such as sea time and on-board or laboratory space), but nevertheless the aim should be to ascertain a point in time when mortalities attributable to the fishing-and-discarding process cease to occur. Predictive functions have proven to be beneficial in calculating this (Benoit *et al*. 2012).

2.1.2 Indirect assessments of discard mortality

In contrast, and although perhaps not as accurate or precise (Davis 2007), indirect assessments generally are easier to achieve because they infer the fate of organisms; often by measuring either physical damage or vitality indices (e.g. Davis 2007; Benoît *et al.* 2012, 2013; Raby *et al.* 2012) or physiological parameters indicative of stress (e.g. Patterson *et al.* 2007; Snoddy and Southwood Williard 2010; Gale *et al.* 2011). This approach has been used mainly to assess discard mortalities of animals caught by trawls, seines, and hooks (Davis 2007; Hochhalter 2012; Raby *et al.* 2012).

Less tangible than direct measurements, indirect assessments rely on the concept that beyond certain thresholds, the condition of a stressed organism is irreversibly disturbed and it will precipitously decline until its death. For example, due to extreme muscular exertion during and/or following capture, metabolic changes (e.g. acidosis – Beamish 1966) may be evident. Vitality and reflex impairment indices, physical damage scores, hormone and metabolite concentrations of blood or plasma samples (e.g. potassium, glucose, and $_{\text{L}}$-lactate) have all been used as surrogates of mortality (e.g. Benoît *et al*. 2012, 2013; Frick *et al.* 2012); although in particular, conventional physiological parameters may not be appropriate to forecast long-term mortality of fisheries-related injuries (Cooke *et al.* 2013). Nevertheless, this approach still requires validation trials where both surrogate parameters and mortalities are measured to construct calibration curves and/or semi-quantitative, predictive functions (Davis 2007, 2009; Benoît *et al.* 2010; Braccini *et al*. 2012; Raby *et al.* 2012). Once validated, these may be cost-effective methods to collect data correlative with mortality on a large scale (e.g. from many individuals and/or multiple trips).

2.1.2.1 Physiological parameters

Stress responses are reflected in primary, secondary and tertiary physiological parameters (Portz *et al*. 2006). The primary stress response of teleost fishes includes increased hormone concentrations such as corticosteroids ('cortisol'). Increased cortisol concentrations are measurable within minutes after the onset of stress and can remain elevated before returning to baseline levels. Secondary stress responses include parameters such as glucose and $_{\text{L}}$-lactate which are measurable over protracted periods (Portz *et al.* 2006). In many species, glucose and $_{\text{L}}$-lactate levels increase in response to the release of hyperglycaemic hormone (Bergmann *et al.* 2001). This occurs as intracellular glycogen is mobilised and becomes available as glucose which is converted to $_{\text{L}}$-lactate via glycolysis. Although concentrations of glucose and $_{\text{L}}$-lactate have been used as physiological indicators of stress in species of crustaceans (e.g. Paterson and Spanoghe 1997) and fishes (e.g. Pankhurst and Sharples 1992; Parker *et al.* 2003; Portz *et* *al.* 2006), their utility as surrogate measures of mortality was not always conclusive (Davis and Schreck 2005).

2.1.2.2 Reflex assessment mortality predictors (RAMP)
Scoring of physical responses to reflex tests provides a simple, and comprehensible method to correlate physiological condition with mortality (Davis 2007). Reflex responses are also independent from the influence of environmental conditions, unlike some blood chemistry parameters (Davis 2007). Reflex tests can be done on both restrained and free-swimming fish and follow the methods described by Davis and Ottmar (2006) and Davis (2007). Based on the proportion of present responses, the reflex action mortality predictor (RAMP) index can be calculated following Davis (2007), whereby the absence of a reflex response is scored with '1' and presence with '0'. Thus, a high RAMP score indicates that the fish showed an absence of responses to several reflex tests. The relationship between RAMP scores and total mortality estimates (proportion or %) can then be calibrated. This method has been proven successful for species where stress responses follow an incremental curve as a function of an applied stressor (Davis 2009). However, it still requires careful testing with the help of a captivity or free-range study, ideally under realistic conditions, to validate its predictability function. For example, in field trials, Barkley and Cadrin (2012) found a positive correlation between RAMP scores of trawl-caught-and-discarded yellowtail flounder (*Limanda ferruginea*) that were exposed to longer deployment durations and subsequent air exposure times.

2.1.2.3 Time to mortality proxy
In contrast to the RAMP method which incorporates several stressors, Benoît *et al.* (2013) focussed on species-specific tolerances to hypoxia (measured as 'time to mortality') and demonstrated that this is a useful proxy for discard mortality. A number of individuals from a variety of trawled teleost species were exposed to air and the point in time when their tolerance level was reached was recorded. Thereby, species-specific variability in time to mortality was related to traits such as presence of deciduous scales, presence and type of gas bladder, high mucus production, sedentariness and body softness (Benoît *et al.* 2013).

2.1.3 Controls
Irrespective of the experimental method, a common prerequisite should be the use of controls to facilitate the attribution of causality (Broadhurst *et al.* 2006; Pollock and Pine 2007; Barkley and Cadrin 2012; Rogers *et al.* 2013). Ideally, they should stem from the same population as the treatment fish, but this is rarely the case, because of logistical constraints (Cooke *et al.* 2013) and, often, a lack of valid methods of obtaining controls. Therefore, some researchers have opted for hatchery-raised specimens, although inherently these fish are genotypically and phenotypically different from wild fish, and thus may be more or less tolerant to environmental stressors than their wild congenerics (Pollock and Pine 2007; Cooke *et al.* 2013), and are likely be accustomed to captivity. Controls should be within the same size, age and physical condition than the treated organisms. The lack of true controls during *in situ* experiments with wild fish may be difficult to overcome (Cooke *et al.* 2013).

2.1.4 Stressors
Reviews by Davies (2002), Broadhurst *et al.* (2006) and Uhlmann and Broadhurst (2013) summarized relevant stressors associated with discard mortality from active and passive gears (Table 1; Figure 1). For many of these gears, key factors included: technical (gear design, tow duration and speed); environmental (water and air temperature, hypoxia, sea state and availability of light); and biological (species physiology, size, catch volume and composition; Broadhurst *et al*. 2006; Uhlmann and Broadhurst 2013). Vulnerability to discard mortality is clearly a function of how sensitive a species is to capture-and-discarding mechanisms. Biological variables, and especially the size, type of exo-skeleton (if any), and physiology to withstand stress and injury are clearly important (Broadhurst *et al*. 2006; Uhlmann and Broadhurst 2013). However, other technical and environmental factors are also influential. Post-capture handling procedures can also have a strong effect on the fate of discards and exceed even the stress levels experienced during capture, especially if force is used to remove captured individuals from the fishing gear (e.g. disentangling crustaceans from gillnet meshes; Uhlmann and Broadhurst 2013). Several of the reviewed studies established a correlation between discard mortality and

deployment duration. While some factors may be of relevance for a specific gear type (e.g. crowding in pelagic trawls or sediment type for demersal trawls), others have been associated with discard mortality across different gears (e.g. air exposure, water and air temperature, Table 1). Any direct or indirect assessment of discard mortality needs to carefully consider and measure all key stressors to allow for a meaningful interpretation of the results. Given the plethora of relevant variables, there is a risk of not including an important variable as part of the monitoring and to over parameterize a regression model exists (Zuur *et al.* 2007).

2.1.5 Costs

Due to the cryptic nature of delayed discard mortality – it occurs out of sight and underwater – any assessment is labour- and cost intensive (Table 2). However, long-term tagging studies may be more costly than short-term captivity studies. No information was available about the initial costs of finding a suitable indicator of mortality from indirect assessments. Between €40,000 and € 100,000 per year costs a direct assessment of discarded animals in captivity on-board commercial vessels during 4 or 5 multi-day trips (Table 2). Sending an observer out on a boat to register immediate mortalities may be expensive, too, considering that an observer-day costs about € 723 and € 1,400 per day in some US and Dutch fisheries, respectively (Hartley 2011; Helmond *et al.* 2011). Detailed cost-benefit analyses of discard mortality studies were lacking among the reviewed literature, but would be essential to relate the required costs against the value to be gained.

2.1.6 Analysis

Over the past 30 years, statistical methods to analyse ecological data including those that are used to process data from discard mortality studies have advanced (Guisan *et al.* 2002; Jaeger *et al.* 2008). Most commonly and given sufficient sample sizes and replication, logistic regression, generalized linear models (GLM) or generalized additive models (GAM) are used nowadays to analyse these data (Broadhurst *et al.* 2008; Benoît *et al*. 2012; Revill *et al.* 2013) and are preferred over traditional ANOVAs (Uhlmann and Broadhurst 2007). Discard mortality data from direct assessments may involve counts of dead individuals, which can be used as either discrete binary (success/failure) or proportional (the number dead over all monitored animals) response variables in these models. The various influencing (categorical or continuous) factors/stressors (see following paragraph) may be considered as either fixed or random explanatory variables (e.g. Broadhurst *et al*. 2008; Revill *et al*. 2013). As with any analyses, it is important that each regression model is carefully validated against any underlying assumption (e.g. homogeneity of variances or independence; Zuur *et al.* 2010), before any conclusions are drawn from seemingly significant results.

Another analytical variant is 'survival analysis' (Cox and Oates 1984; Ibrahim *et al*. 2001) which models survival probability as a function of time (e.g. Benoît *et al.* 2012), with the advantage to consider continuous time intervals of monitoring for fatalities. For example, this technique becomes very relevant if animals from the same trip have been monitored for different periods.

Where mortalities to controls have been assessed, these should be used to adjust those counts, probabilities or proportions of treatments.

2.1.6.1 Sample size, replication and statistical power

Experimental designs of discard mortality studies are prone to low statistical power due to often limited sample sizes and replication. As stated above, charter of fishing vessels may be expensive and thus, the number of trips and/or hauls may be small. In a hierarchical study design, where groups of fish are nested within haul, and hauls within trips and trips within vessel, sufficient replication at each of these levels is necessary.

In statistical tests, there is often a focus on the significance level α (0.05). This signifies a test for a so-called type I error (rejecting a true null hypothesis). The probability of rejecting a true null hypothesis should obviously be low. But if a null hypothesis is not rejected, the risk of committing type II error (not rejecting a false null hypothesis) needs to be explored. This is referred to as the statistical power of the test. For example, suppose the null hypothesis assumes that there is no difference in discard mortality rates of a species caught-and-discarded from conventional compared with a modified gear. The statistical test based on the significance level $\alpha=0.05$ does not reject the null hypothesis. Before accepting this result, the probability of committing a type II error (not rejecting a false null hypothesis) needs to be calculated (Peterman 1990; Beninger *et al.* 2013). The power of the test is the ability to avoid type II error. The probability of committing type II error needs to be low. In general, $\beta < 0.2$ or a power of 0.8 ($1-\beta$) are acceptable. Ideally, field experiments should have a high power or in other words high probability of detecting an effect if an effect exists (i.e. rejecting the null hypothesis if it is not true). But given the naturally high variability in discard mortality estimates that can be introduced from variable environmental and experimental conditions, large effect sizes for a given treatment may not be consistently evident. Thus, to single out impacts may require a high level of replication, especially if measured differences between certain treatments are low.

Following Peterman (1990), power is a function of (α, effect size, N, and s^2), where α is the stated value for P (the calculated probability) below which you reject H_0, effect size is the magnitude of the true effect for which you are testing, N is the sample size, and s^2 is the sample variance (including both natural variability in the sampled parameter as well as measurement error). For example, in the above hypothetical example, the effect size may be the difference between the observed mortality of discards from the conventional versus the modified gear treatment. To be able to calculate statistical power requires a set of alternative values for the same parameter.

2.2 Guideline

The fishing industries of European member states are motivated to produce evidence of species likely to survive discarding which qualifies for an exemption from the landing obligation. Thus, objective scientific guidelines are required on how to carry out discard mortality assessments (STECF 2013). However, the value of involving the industry without comprising the independence of the outcome should be evaluated (Armstrong *et al.* 2013); this may guarantee that the results are accepted by both scientists and stakeholders. The current draft policy of the landing obligation emphasizes the need to establish reference values of discard mortalities for different species and fisheries. These will then be classified as either sufficiently low or not to issue an exemption.

While the choice of an appropriate experimental study design is driven by the study objective, a systematic and best practice approach to discard mortality assessments in light of the exemption rule of the CFP may be split into three phases. In the first phase, biological, technical and environmental conditions of well-defined fishery would be qualitatively described and the rate of immediate discard mortality, stress and extent of visible physical damage are scored on-board (Figure 3). The development of a sensitivity index or matrix may be useful in this process (MacDonald *et al*. 1996; Chuenpagdee *et al.* 2003). Already in this phase it may become evident that due to rather extreme fishing conditions (e.g. offshore operations with heavy gear, >1 hour haul durations in deep water with thermo- and haloclines, and heavy catch volumes), immediate mortality counts and/or physical injury rates may be high. In this case, further monitoring of delayed mortalities may not be necessary, because the bulk of the catch may already be dead, and under the Policy Reform no exemption could be granted purely based on a high mortality rate in this phase. Additionally, observations of avian predation rates may be done to evaluate their potential removal of discards (Figure 3). If a seemingly large proportion of discardable individuals is already dead on-board and/or is being eaten by apex predators soon after being discarded, further assessments may not be necessary.

Otherwise the second phase may include an assessment of the fate of 'discarded' individuals over a relatively short time period by holding animals in captivity, together with true controls (sourced from the same population) in a recommended 'paired design' (Pollock and Pine 2007; Figure 3). If consistently mortalities were below 50% in the second phase, in a third phase, natural and predation mortalities may be assessed via a well-designed tagging study accounting for tag loss, tag mortality and recapture probabilities (Figure 3).

The choice of using direct and/or indirect assessments techniques of discard mortality is going to be dependent on the objective of the study (see Introduction). For example, the aim may be to (i) provide reference estimates of overall mortality, (ii) identify factors that contribute to the observed mortality; and/or (iii) measure relative reduction in mortalities during mitigating trials (e.g. experiments with modified gear and/or operational fishing practices). To increase the inference, direct and indirect assessment techniques may be combined in an integrated approach, for example by identifying suitable surrogate indices of mortality (e.g. physiological parameters; Cooke *et al.* 2013).

For a wider application, a decision tree has been developed based on some of the reviewed literature to indicate which technique (or combination thereof) is most suited to address the specific objective in question (Figure 4; Gilman *et al.* 2013). The most labour and cost-intensive approach is to provide a baseline, reference or definitive estimate of discard mortality for a given species (and fishery - the objective of the exemption rule of the CFP), because it may require a long-term monitoring component of tagged, free-ranging individuals. Captivity studies may be suited in particular for identifying key stressors associated with mortality and which may be mitigated to further reduce mortality (Benoît *et al*. 2012). Such a direct approach may be combined with an indirect approach to identify suitable surrogate measures allowing for rapid data collection on a larger scale as part of conventional observer-based data collection programmes (i.e. different fisheries, areas, and seasons; Barkley and Cadrin 2012).

However, the majority of discard mortality studies up to now include a direct assessment component of confined organisms and were set up to provide an order of magnitude estimate of post-release mortality with the limitation of not being able to account for natural and predation mortalities. Apart from this objective, these studies may also provide an indication which factors contribute towards mortality (e.g. Uhlmann *et al.* 2007; Broadhurst *et al.* 2011; Benoît *et al.* 2012). If the results are promising – in the sense of indicating low mortality or a significant reduction in mortality between treatments, for fisheries managers such studies may already justify the introduction of certain mitigation measurements or a justification for more accurate direct assessments of long-term mortalities.

If the objective is to isolate relevant stressor(s) or test a mitigation strategy (Figure 4), either an integrated or uniform (single) approach may be taken of direct and/or indirect assessment methods (Benoît *et al.* 2012; Braccini *et al.* 2012; Leland *et al.* 2012). Identification of surrogate measures typically asks for an integrated approach combining a captivity study with an indirect assessment to validate any inferences drawn from the analysis of the physiological parameter(s). In some catch-and-release studies of angled fish, both direct assessments techniques (observations under captivity and free-range conditions were combined, Donaldson *et al*. 2008; Roberts *et al.* 2011) to evaluate the utility of mitigation treatments both under captivity and free-range conditions. Integrated approaches increase the power of inference of the results, but are going to be also more costly by employing different techniques to study the same outcome.

2.2.1 What constitutes high survival?

Under the common fisheries policy reform, according to article 15, paragraph 2(b), "*species for which scientific evidence demonstrates high survival rates, taking into account the characteristics of the gear, of the fishing practices and of the ecosystem, shall be exempted from the landing obligation.*" The reasoning behind this may be that for species that are highly likely to survive the discarding process, a landing obligation would imply all these organisms would be retained and die, although otherwise at least some could have survived. However, linking individuals to a population has been difficult to demonstrate (Cooke *et al.* 2013), because high discard mortality may not necessarily explain the status of a stock. For example, in the North Sea, the plaice stock is thriving despite a historically high rate of discarding and associated mortality (van Beek *et al.* 1990). In any case, the exemption rule raises the following issue: what is the definition of 'high survival'?

The actual level of mortality which may have consequences for the productivity of a particular stock may be very specific to a species and very difficult to nominate, also due to above-mentioned experimental limitations (e.g. direct assessments in captivity are often underestimates). Moreover, mortality rates may be size- and age dependent, so proportional shares of discards-at-age are relevant to know and their specific mortality rates. Although some species are *per se* physiologically more tolerant to fishing-induced stressors than others, the impact is also a function of how extreme the stress is. Therefore, a definition of 'high survival' needs to consider the circumstances – when, where and how a certain species was caught-and-discarded (STECF 2013).

Notwithstanding the above, obviously, it would be desirable that a smaller proportion of discarded animals dies and the majority survives, but depending on how many animals are being discarded in relation to the catch and also the population, an attribution of a stock-recruitment-discard mortality relationship will be difficult to make.

3. Conclusions

The feasibility of any discard mortality study relies on an evaluation of the required costs against the value to be gained. Clearly, direct or indirect assessments of discard mortality are labour- and cost-intensive, and the measurable outcomes, such as a count of fatalities, are likely to be variable due to the associated stressors and potential for confounding effects from the scientific assessment method. Furthermore, the choice of an appropriate assessment method is tied to the objective of a study. For example, if the objective is to establish a reference/baseline level of mortality, a direct assessment via a tagging study may provide estimates of delayed mortality including predation, but may be confounded from tag loss and additional mortality related to tagging. In contrast, a captivity study faces other confounding effects, for example from caging animals, and also most likely underestimates mortality by not accounting for predation and protracted effects. Simply, the low stress tolerance of study organisms and more 'extreme' operational and environmental conditions (e.g. in deep water with >1 h haul durations, and large catch volumes) may already indicate that mortality may be high and may question the value of an exhaustive experimental study. Likewise, even during a seemingly mild fishing operation (short duration, low catch volumes) mortalities may be high, if the catch were sorted in air, during direct sunlight and in warm waters.

Any study of discard mortality should address some essential elements (Table 3 summarizes generic pitfalls, some of which have been described in detail in some of the previous sections above). Table 5 may be used as a checklist and scorecard which allows for a convenient ranking of the level of detail and also quality of a discard mortality study (see Appendix B for an example – review of Revill *et al.* 2013). Essential are: i) a clearly defined objective of the study; ii) use of valid controls, iii) involving, if possible and depending of the study's objective, animals with a homogeneous stress history that have gone through a realistic process of capture-and-release (especially for indirect assessments). As a rule of thumb, at least between 10 and 15 replicate observations should be made per explanatory variable (Gotelli and Ellison 2004). Gotelli and Ellison (2004) base this rule not on any theoretical principle or statistical analysis, but on year-long experience in their field of ecological research. Less replicates per treatment level can still be powerful designs, if, for example, variances are small.

Optional elements include whether a link can be made to the population level (e.g. a large-scale tagging study may have tagged a large number of individuals that allows for some inference about the impact on the population). Opting for manipulative treatments over a purely observational study increases also the power of inference (Gotelli and Ellison 2004).

Apart from the associated stressors, the potential for success may also be undermined if, in the case of European Policy Reform, 'success' – as in 'high survival' is not clearly defined. It remains unclear whether high survival is a condition at the species or population level, and how regularly it needs to be demonstrated, for how many different species and within what level of confidence limits? Such a vague policy target allowing a multitude of interpretations and the foreseeable variability among discard mortality estimates questions the feasibility of discard mortality studies with methodologies that cannot rule out confounding effects.

Discard mortality is influenced by technical and operational (e.g. fishing practice, deployment duration), environmental (e.g. air exposure) and biological factors (e.g. composition and size of the catch) and varies within and between species depending in which fisheries and under which conditions these were caught. Although desirable, determining a definitive (reference) estimate of discard mortality is extremely difficult to achieve given the limited replication under conventional fishing conditions (unless a costly and long-term tagging study is done); and also because other unaccounted mortalities such as predation are often not considered in captivity studies. Therefore, estimates are in most cases going to be underestimates of "true" mortality and restricted to such (experimental) conditions and can be extrapolated to entire fishing fleets only if operating under similar conditions. To measure the impact on a population level, larger-scale and costly projects are necessary to trace the fate of a large number of released fish (i.e. mark-recapture, biotelemetry approaches). However, in some cases, it may suffice to develop more cost-effective alternative approaches that facilitate the identification of proxies for mortality. The use of valid controls is important to account for any confounding effects. Pooling of results across individual studies where comparable methods have been used will increase sample size. Validated rapid assessment techniques (i.e. RAMP index) could be used to monitor the impact of capture-and-discarding. Combined approaches of direct (captivity or tagging studies) and indirect assessments (using surrogate measures) can facilitate the link from studying the impact on individuals to a wider population.

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Table 1. List of technical, environmental, and biological factors of a given fishery known to be associated with discard mortality.

	Trawls	Gillnets/Traps	Pelagic	Reference
Technical				
Deployment duration	x	x		1, 2
Gear type, material, or design (configuration)	x	x		1, 2
Gear modification (BRD)		x		1, 2
Gear operation (e.g. crowding)			x	4, 5
Sorting and handling onboard (e.g. drop height)	x	x		1, 2
Towing speed				
Hauling speed				
Environmental				
Air exposure	x	x	x	1, 2
Temperature, air	x	x		1
Temperature, water	x	x	x	4
Depth	x	x		1, 2
Sediment type	x			2
Season	x			1
Water current speed				
Light	x			1
Dissolved oxygen			x	4
Salinity	x	x		1,2
Biological	x			1
Size/age		x		2
Gender	x			1
(Reproductive) condition	x	x		2,3
Catch composition	x	x		1
Catch volume	x			1
Predation	x			1, 2

[1] Broadhurst *et al*. (2006); [2] Uhlmann and Broadhurst (2013); [3] Revill *et al.* (2013); [4] Hall and Roman (2013); [5] Huse and Vold (2010)

Table 2. Approximate, anecdotal cost estimates of discard mortality projects with an indication of the method used and number of sampled trips and hauls. [F], field; [L], laboratory study; --, information was not available.

Location	Period	Method	Trips/hauls	Costs
Australia	3 years	Direct (captivity, With underwater cages)[F]	--/152	€ 396,844
Netherlands	1 year	Direct (captivity, with underwater cages)[F]	4/20	€ 74,800
Netherlands	1 year (proposed)	Direct (captivity with onboard containers)	18	€ 312,000
England	1 year	Direct (captivity with onboard containers)[F]	5/122	€ 39,230
England	1.5 years	Direct (captivity, tagging, avian predation) Indirect (vitality)[F]		€ 298,148
Denmark	1 year (proposed)	Not specified		€ 320,000
Belgium	1.5 years (proposed)	Direct (captivity)[F]	15/--	€ 135,000

Table 3. List of generic pitfalls when quantifying discard mortality.

Generic pitfalls

- Lack of controls:
  - unable to control for treatment effects (to attribute mortality to treatment rather than captivity or tagging effects or natural mortality)
- Short monitoring period
  - Trade-off between long-term impairment and monitoring effects; some fisheries-induced effects may be protracted for some species and result in slow growth and/or lack of reproductive ability
- Insufficient replication:
  - Definitive answer of mortality rate impossible to give (too little replication to cover the range of environmental, biological and technical conditions)
  - Without replicated experiments in for example other environments and/or fisheries, generic extrapolations from the monitored to the total population of vessels is not possible.
  - As a rule of thumb, per relevant explanatory factor at least 15 observations should be available for a rigorous statistically powerful analysis (e.g. mixed effect model)
- Identify robust species-specific criteria to determine immediate mortality endpoints
- Confounding effects:
  - Invasive monitoring
  - Repeated sampling
  - Cryptic factors which may contribute to mortality (e.g. knotted mesh in holding cages, presence of scavengers, strong tidal currents)

Table 5. Checklist of essential and optional considerations which should be addressed in any study of discard mortality.

Essential	
Clearly defined objective leading towards a coherent design and methodology	☐
Use of valid controls (sourced from the same population than treatment fish)	☐
Realistic monitoring period	☐
Homogenous stress history of study organisms	☐
Plausible fishing conditions	☐
Verifiable endpoints	☐
Replication (rule of thumb): at least 15 observations per explanatory variable	☐
Optional	
Measures population impact: link from individual to population level	☐
Manipulative experiments (distinguishable treatments)	☐
Field experiments (for more realistic conditions)	☐
Integrated approach (e.g. direct and indirect assessments)	☐

Figure legends

Figure 1. Stressors influencing captured-and-discarded organisms during fishing resulting in either synergistic, antagonistic or cumulative stress responses (reproduced with permission from Davis 2002).

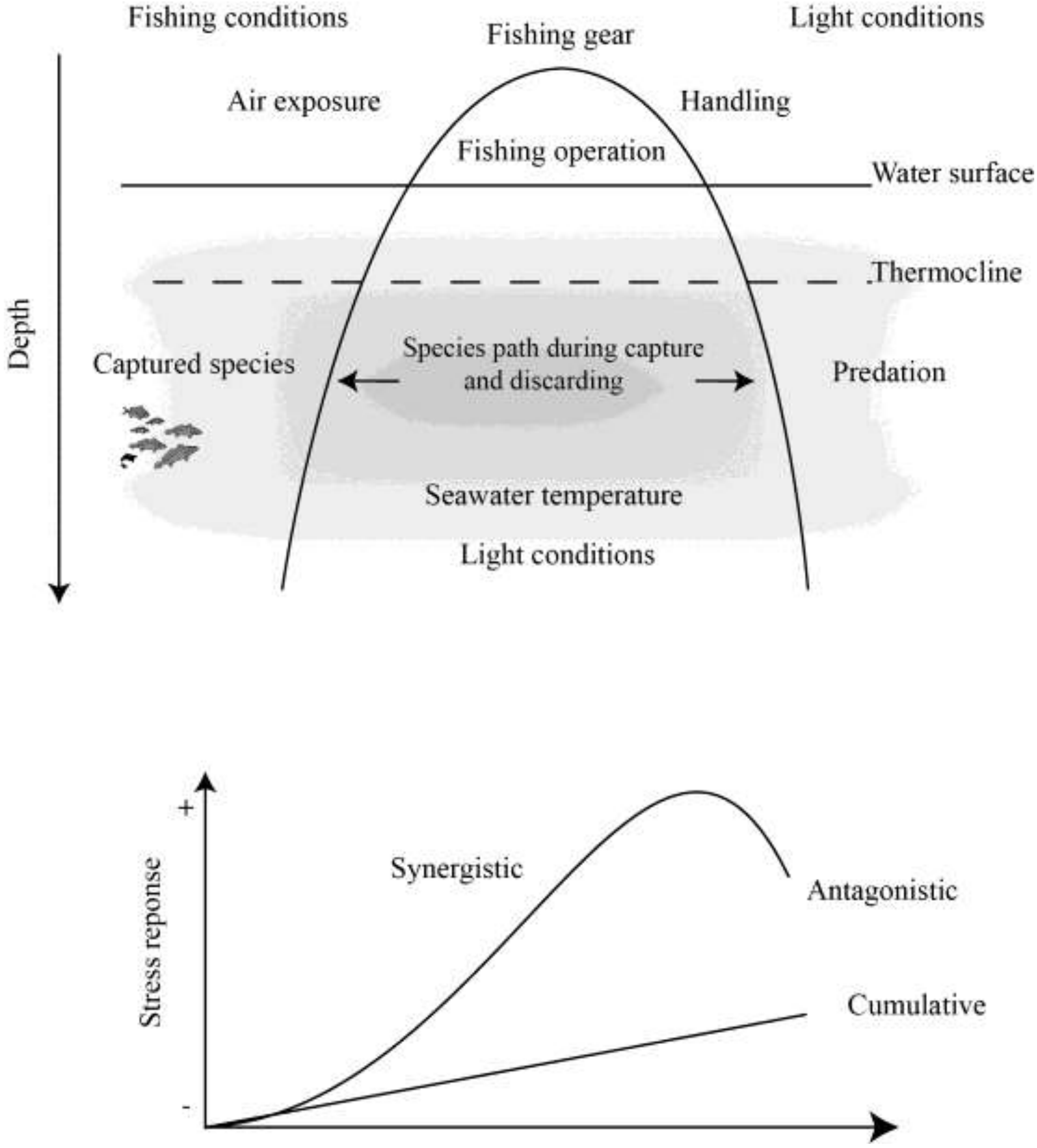


Figure 2. Schematic illustration of the three phases of a best-practice approach to discard mortality assessments in response to the Common Fisheries Policy exemption rule.

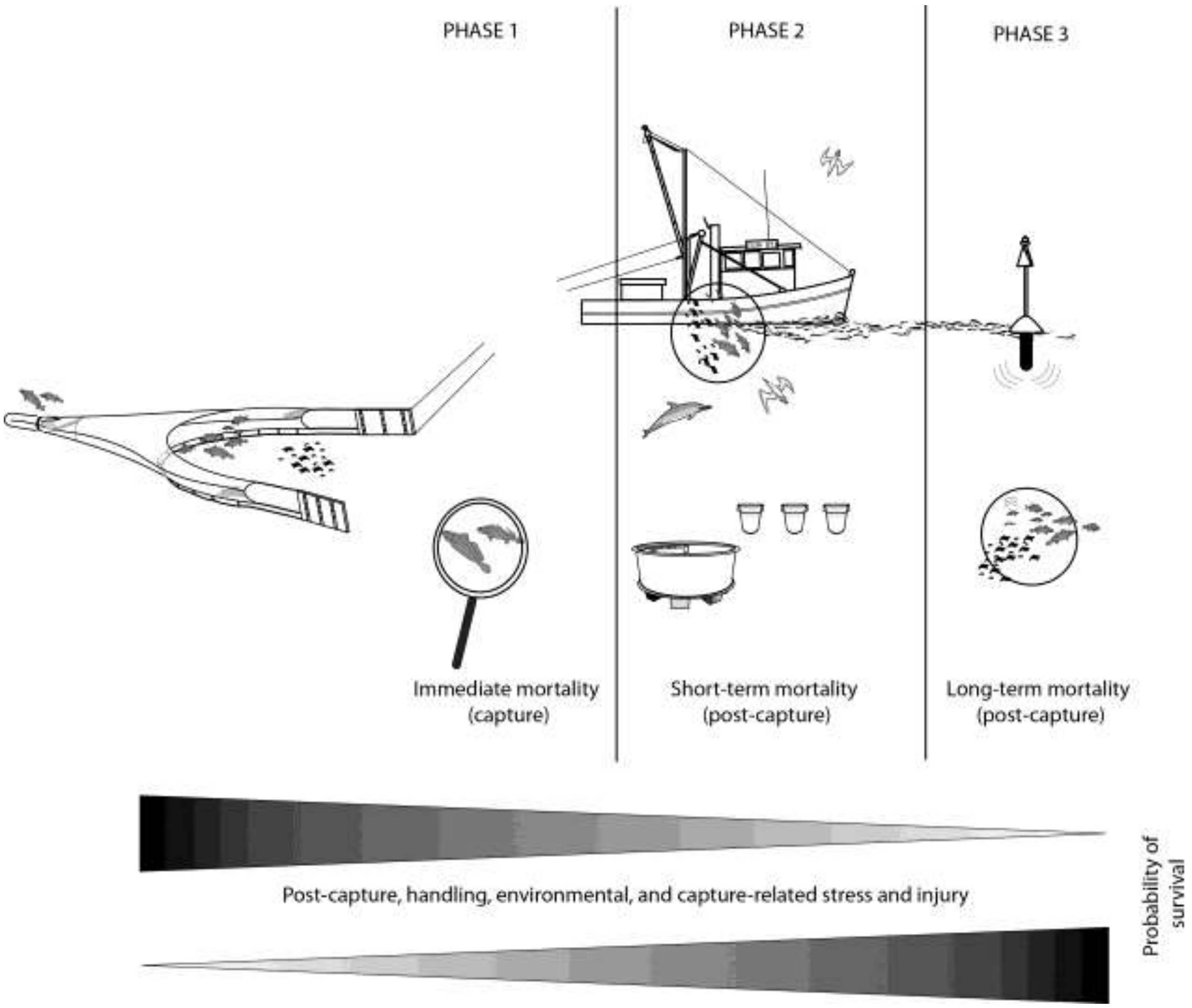
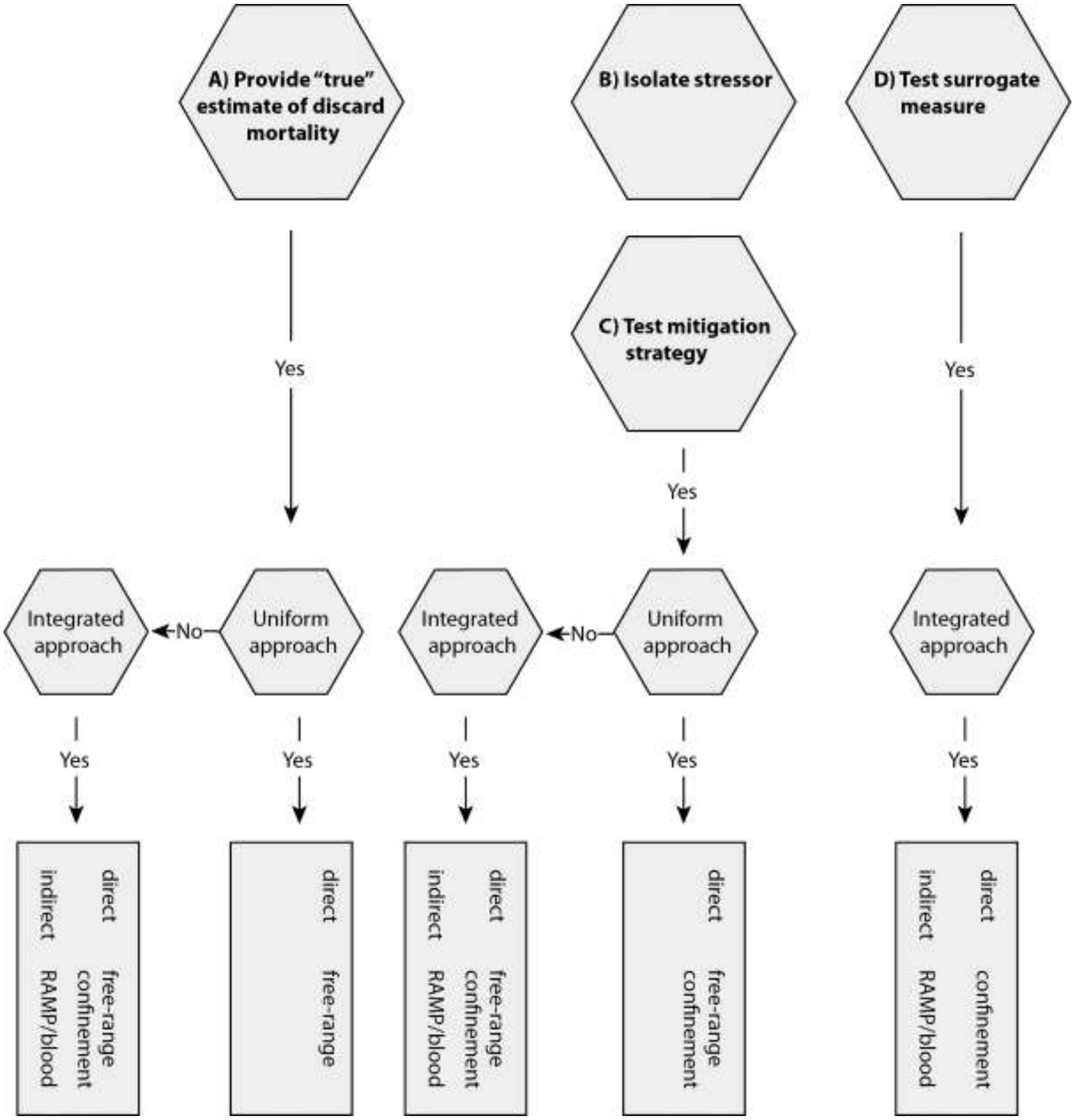


Figure 3. Decision tree to guide the process of choosing an appropriate discard mortality study design. Decisions between uniform or integrated approach may be based case-specific considerations with regards to the budget, logistics and its desired research application.



Appendix B. Example evaluation using discard mortality guidelines

Mortality of adult plaice, *Pleuronectes platessa* and sole, *Solea solea* discarded from English Channel beam trawlers. Fisheries Research. doi http://dx.doi.org/10.1016/j.fishres.2013.07.005

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September 25, 2013

General comments:
One of the cornerstones of the Common Fisheries Reform is to eradicate or at least minimise the discarding of unwanted catches. Apart from a socio-economic justification to prevent the wastage of valuable food resources, an environmental consideration is to minimise potentially high levels of associated mortalities. For the English beam trawlers operating in the Western English Channel, Revill et al. (2013) present, for the first time, estimates of the discard mortalities of adult plaice and sole. The authors concluded that clearly there exists some potential for not all discarded adult plaice or sole to die, although definitive figures cannot be drawn from this research due to experimental design limitations (i.e. confounding effects of using onboard containers, also precluding an assessment of predation; lack of true controls; lack of an understanding of key driving factors contributing to species-specific differences in mortalities).

Specific comments and criticism:
From previous studies it is known that for some species, length is negatively associated with mortality: with smaller individuals having a higher chance than larger ones to die after discarding. Adult plaice and sole above minimum landing size (MLS) were used in this study, because of considerable over-quota discard quantities. However, this implies that in general resulting estimates of adult discard mortality may be lower than those of juveniles.

As in previous studies, mortalities showed considerable variation. There were species-specific differences in partitioned immediate and short-term (3 days) mortalities: plaice seemed more sensitive than sole. This has also been confirmed by van Marlen *et al*. (2013). There were also seasonal differences in mortality rates, which may have been related to reproductive condition. In February during the peak spawning period, plaice were in poor condition, although this was not formally assessed.

The main limitations of this study are that confounding effects from holding fish in captivity onboard, or specific technical or operational configurations may have contributed to the variability in resulting mortality estimates. Cumulative mortalities of the control group were still increasing after the third day of monitoring, indicating that some stressor prevailed.

Given the study-specific experimental design and fishing conditions, as the authors state, it is difficult to compare these with other estimates from the same species. If experiments (with the same design and procedures) are not replicated simultaneously on different vessels and/or areas, there is always going to be a confounding effect of time/season. Furthermore, in this study adult fish were examined which are likely to return a more conservative estimates than more sensitive juveniles, which comprise the majority of discards and which are of central concern under the discard ban/landing obligation. Therefore, the estimates produced by Revill et al. (2013) should be used with caution in a discussion about juvenile discards.

Remarks:
- Adults were deliberately assessed, not juveniles, not possible to compare with others studies were juveniles were monitored.

- Not all trips had controls, but at least some were sourced from short trawls
- Controls may have come from a different population of study animals (and were caught from shallower water). Therefore, potentially their resistance to discard mortality may be different to that of the study animals.
- Confounding effects of technical, operational variables with season. For example, in each season only, discard mortalities were gleaned from a single vessel. Ideally, these trips should have been replicated.
  - Although the monitoring period may be adequate to attribute the observed mortality to capture-and-release stress, mortalities of controls had not reached a plateau, indicating that something was stressing the fish (or that the controls were in poor condition). Considerable mortality of control plaice and sole (pooled): 23%.
  - Mortality estimates are underestimates, because predation is not considered; or equally plausible overestimates, because of the vessel effects (i.e. the control fatalities onboard suggest the containers may have caused deaths?)

Table 6. Number of trips, trawl deployments, and number of fish monitored, and their corresponding percentage (%) immediate (IM) and short-term mortalities from English beam trawlers in the Western English Channel.

Study area	Species	N trips	IM/STM	N deployments	N monitored	% mortality
English Channel	Plaice	5	IM	58	1013	15.2
			STM	7	120	45-66
	Sole	5	IM	75	810	5.6
			STM	11	90	43-73

Table 7. Checklist against essential and optional considerations which should be addressed in a study of discard survival.

Essential	
Clearly defined objective leading towards a coherent design and methodology	☑
Use of valid controls (sourced from the same population than treatment fish)	Maybe
Realistic monitoring period	☑
Homogenous stress history of study organisms	☐
Plausible fishing conditions	☑
Verifiable endpoints	☑
Replication (rule of thumb): at least 15 observations per explanatory variable.	☑
Optional	
Measures population impact: link from individual to population level	☐
Manipulative experiments (distinguishable treatments)	☐
Field experiments (for more realistic conditions)	☑
Integrated approach (e.g. direct and indirect assessments)	☐

Appendix C. *A priori* power analysis of discard mortality experiments

To design a meaningful discard mortality study, it may be relevant to carry out an *a priori* power analysis to calculate the required sample size and/or replication for a desirable level of power (e.g. 0.8), if α, an estimate of variance and an effect size are known or can be assumed.

The following questions may be addressed via a discard mortality experiment under captivity conditions for short-term monitoring of protracted fatalities:

a) a comparison of captivity (underwater cage vs on-board container) of caught-and-discarded fish from conventional fishing practices

b) a comparison of conventional vs modified fishing practices (including operational and handling procedures) using a single method of captivity.

c) a comparison of mild and extreme on-board handling practices and housed in containers versus underwater cages.

d) Treated fish vs untreated controls (this applies to a-c above).

To give an example, we used data from a pilot study (Marlen *et al*. 2013) where discard mortalities of beam-trawled sole were monitored in captivity in either underwater cages or on-board containers (question a, above). The beam trawler fished in the North Sea with pulse-trawl gear. For each housing method (cage vs container), two handling treatments were compared – fish were either directly picked from the hopper (where the codend is emptied into) or from the sorting conveyor belt to test whether the supposedly shorter air exposure times of the hopper-picked fish benefitted their fate (question c). Experiments were conducted during a single commercial fishing trip between 27-31 May 2013. In this pilot study, questions a) and c) were addressed above in a 2x2 experimental design (two treatments with two levels each). The response variable (endpoint) is then the number of alive sole out of the stocked total after 72 hours post-discarding. The explanatory variables included two treatments: captivity method (underwater cage vs. on-board container) and picking location (hopper vs sorting conveyor belt). Each underwater cage and on-board container was stocked with 14-31 and 3 fish, respectively. These numbers were determined so that the density (number of fish per volume) were similar between the underwater cage and the on-board container.

All live treatment fish were collected over several hauls. Some were picked directly from the hopper with minimal (< 3 min) air exposure, and others were picked from the sorting conveyor belt and placed into two separate aerated seawater-filled containers ('bak'). At the end of the first day of fishing, fish from either picking location were placed in either an underwater cage or on-board container. Each cage was compartmentalized to house fish from either picking location within the same cage (cage+hopper/conveyor); whereas separate containers were used (container+hopper, container+conveyor; Table 8). For each treatment combination, or each individual experiment (exp), fish were stocked in groups, either in the underwater cage or in on-board container. Thereby, the two compartments within the same cage were considered as independent, experimental units.

Table 8. Overview of the 2x2 factorial, experimental design used to compare discard mortality rates of fish picked from the hopper vs the conveyor belt and stocked in underwater cages vs on-board containers. Each treatment combination is considered as a separate experimental unit (expID).

ExpID	Captivity method	Picking location	No. alive	No. dead
1	cage	belt	7	10
2	cage	hopper	11	4
3	cage	belt	7	7
4	cage	hopper	13	2
…	…	…	…	…
23	container	belt	2	1
24	container	hopper	1	2
25	container	belt	3	0
26	container	hopper	2	1
27	container	belt	2	1
28	container	hopper	2	1

Analysis
A generalized linear mixed-effects model (GLMM) was used with captivity treatment and picking location as fixed effects and with experimentID (expID) as random intercept.

$$Y_{ij} \sim B(n_{ij}, \pi_{ij})$$
$$E(Y_{ij}) = \pi_{ij} \times n_{ij}$$
$$\mathrm{var}(Y_{ij}) = n_{ij} \times \pi_{ij} \times (1 - \pi_{ij})$$
$$\mathrm{logit}(\pi_{ijk}) = \alpha + \beta_1 \times CM_{ij} + \beta_2 \times PL_{ij} + \exp ID_i \qquad \text{(Equation 1)}$$
$$i = 1, \ldots, N$$
$$j = 1, \ldots, 4$$
$$exp_i \sim N(0, \sigma_{\exp})$$

while CM and PL refer to the captivity treatment and picking location variables, respectively; i indicates the i^{th} replicate of a treatment combination of the two fixed effect (captivity treatment × picking location) with *N* replicates conducted for each treatment combination; j indicates the index of a treatment combination (j=1,…,4). π_{ij} indicates probability of survival; n_{ij} the number of fish in the i^{th} replicate of treatment combination j ; Y_{ij} the number of alive fish. Thus, Y_{ij} follows the binomial distribution $B(n_{ij}, \pi_{ij})$.

Results
The variance of the random factor expID is 0.61 (SD=0.78; Table 9). This implies a large variance among the experiments. Both fixed effects are not significant and very small (Table 9, Figure 4). To detect a significant difference with sufficient statistical power, >230 replicates would be needed in scenario 2) with 17 or 3 fish per cage or container, respectively and relatively small variance in survival between experiments (Table 10).

Although the difference in survival is small, the model output indicates that fish picked from the deck hopper or stocked in an underwater cage experience greater mortality (Figure 4). This was an unexpected result, because fish picked from the conveyor belt were exposed to air for longer and thus, should have been more stressed. Also, fish stocked in the cages were thought to be housed under more natural and less stressful conditions than fish held inside the onboard containers. Possibly, by chance fish were picked from the hopper and stocked in a cage with a more severe stress history, but concrete measurements of relevant explanatory variables (e.g. time of air exposure, physiological stress, physical damage) are lacking to prove a potential correlation.

Table 9. Results of the generalised mixed effects model with caging method and picking location as explanatory variables.

Variable	Estimated effect	SE	*p*-value
Intercept	0.67	0.42	0.11
Hopper (vs belt)	-0.13	0.46	0.78
Cage (vs container)	-0.21	0.46	0.65

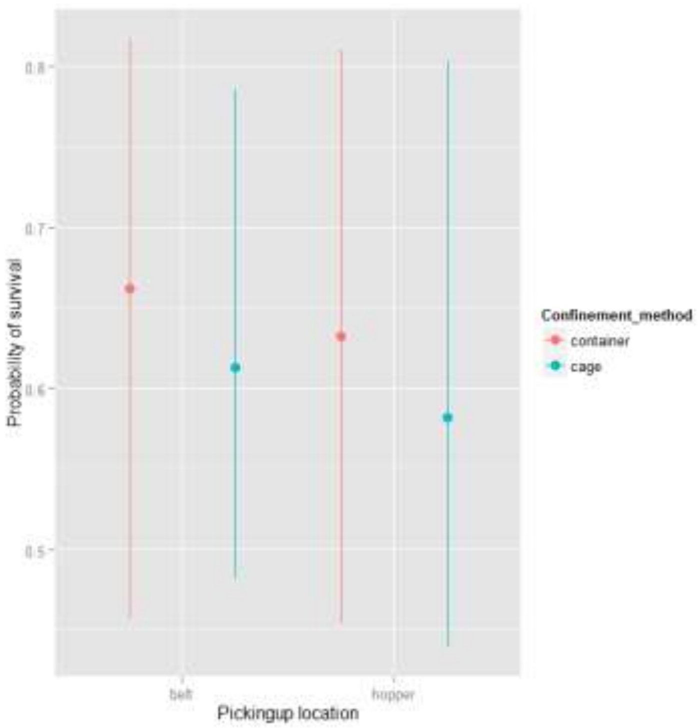

Figure 4. Estimated fixed effects (± 95% confidence interval) by treatment (i.e. picking location – from the hopper vs sorting conveyor belt; and captivity method – underwater cage vs onboard container).

The estimated effect indicates the odds ratio (odds=$\pi/(1-\pi)$) between the two levels. In general, an odds ratio>1 indicates a positive effect to survival, while odds ratio<1 indicates a negative effect to survival (lower survival). For instance the effect of picking location:

$$\text{odds ratio} = \frac{\pi_{\text{last}}}{(1-\pi_{\text{last}})} \bigg/ \frac{\pi_{\text{band}}}{(1-\pi_{\text{band}})} = e^{\beta_2} = e^{-0.13} = 0.88$$

,

when the treatment is fixed at either underwater cage or onboard container.

Similarly, for the effects of treatment:

$$\text{odds ratio} = \frac{\pi_{\text{kooi}}}{(1-\pi_{\text{kooi}})} \bigg/ \frac{\pi_{\text{bak}}}{(1-\pi_{\text{bak}})} = e^{\beta_1} = e^{-0.21} = 0.81,$$

when the picking location is fixed at either hopper or sorting conveyor belt.

Statistical power calculation

Given the number of fish in each treatment combination (e.g. n_{ij}), the variance of the random factor (e.g. σ_{exp}), the expected effect for the treatment factors captivity method and picking location (e.g. β_1 and β_2) and the number of replicates per treatment combination (e.g. N), we could simulate random data sets according to the variable distributions in formula (1). For every simulated data, we fit the GLMM model. Eventually, the proportion of the simulations that gives a significant p-value (e.g. $p <$ 0.05) is computed as the statistical power of the test.

The steps are as follows:

1) Determine the experimental design (e.g. 2×2 treatment factors captivity method × picking location). For each treatment combination, N experiments were conducted. For the underwater cages and onboard container, n_1=17 fish and n_2=3 fish were assigned to each experiment, respectively.
2) Fit the GLMM model on the pilot data to obtain the estimated variance for the random factor expID ($\hat{\sigma}_{\text{exp}}$).
3) Assign the expected effect from the fixed factors treatment (β_1), picking location (β_2) and α (α =0.05). If no value to be assigned, the values obtained from the pilot data may be used (but first to make sure that they are biological useful; for example, do we expect an effect, in which direction, and how big it is going to be?).
4) Simulate a data set according to the parameters above and the distributions in formula (1).
5) Fit the GLMM model to the simulated data and extract the p-value for the treatment variable.
6) Repeat step 4) and 5) 1000 times.
7) Compute the power as the proportion of the simulations that received a significant p-value (e.g. $p < \alpha$). This gives the statistical power of the test when the number of experiments per treatment combination is N.
8) Similarly, by repeating the above process with varying values of N, we could find the required N* that gives a power of ≥0.8.

Table 10 and Figure 5 give the required number of experiments per treatment combination to achieve a power of 0.8, given the expected effect size to detect. Thereby, three different scenarios were tested (Table 10): with greater and smaller variance in discard survival between experiments (scenario 1 and 2; Table 10) and greater stocking density (scenario 3; Table 10). Because captivity method and picking location have the same form in equation (1), the power calculation given a β applies to both variables.

In this case, the sampling variance in a discard mortality experiment comes from two sources: 1) the variance among the experiments or replicates ($\sigma_{\exp}$) and 2) the variance of estimating π_{ij} from the binomial distribution $B(n_{ij}, \pi_{ij})$. Many factors could contribute to the variance among the replicates, for instance, the variance of environmental factors, or the variance of biological characteristics among fish. Thus, the more confounding factors to the survival of fish, the larger the $\sigma_{\exp}$ would be. Consequently, a larger number of replicates per treatment combination would be required from the power calculation. On the other hand, given an individual experimental replicate *ij*, if we assume that the corresponding survival probability π_{ij} is independent of the number of fish n_{ij} in this replicate, thus the survivals of each individual fish are independent, identically distributed (*i.i.d.*), thus each individual fish follows a Bernoulli distribution $B(1, \pi_{ij})$. The estimation of π_{ij} is then varying by n_{ij} as well:

$$SE(\pi_{ij}) = \sqrt{\frac{1}{n_{ij}} \pi_{ij} (1 - \pi_{ij})}$$

The larger the n_{ij} , the smaller the standard error of the estimated π_{ij} is, and the higher statistical power would be achieved. In this case, the statistical power analysis becomes a trade-off between the number of fish (n_{ij}) per replicate and the number of replicates (*N*) per treatment combination. If $\sigma_{\exp}$ dominates the whole sampling variance, N becomes the main consideration in the power analysis. Contrarily, if $\sigma_{\exp}$ is small, namely no confounding factors in the experiment or all confounding factors are well controlled (e.g. all experiments are conducted at exactly the same environmental conditions and all cages/containers are identical, and all fish are exactly in the same condition), then n_{ij} becomes the main consideration in the power analysis. In the extreme case, when every other factors were perfectly controlled, only 1 replicate per treatment combination is needed and the power is increased by increasing n_{ij} .

However, despite the statistical considerations, in practice, the assumption that mortality of each individual fish are *i.i.d.* distributed is often not valid. That is, π_{ij} differs with varying n_{ij} , which is often true, because fish behave in schools. Therefore, it is not advisable to take n_{ij} as a varying factor in the power analysis.

Table 10. The required number of replicates per treatment combination to achieve a power of ≥0.8, given the expected effect size β to be detected, the number of fish for treatment 1 (n_1) and treatment 2 (n_2), the standard deviation among experiments/replicates $\sigma_{\exp}$.

Scenario		β=0.1	0.2	0.3	0.4	0.5	0.6	0.7	0.8	0.9	1
1	*N (n_1=17, n_2=3, $\sigma_{\exp}$ =0.78)*	600	150	70	35	25	16	13	10	9	7
2	*N (n_1=17, n_2=3, $\sigma_{\exp}$ =0.39)*	230	65	28	16	11	7	5	4	4	3
3	*N (n_1=34, n_2=6, $\sigma_{\exp}$ =0.78)*	350	80	40	24	15	14	9	7	5	4

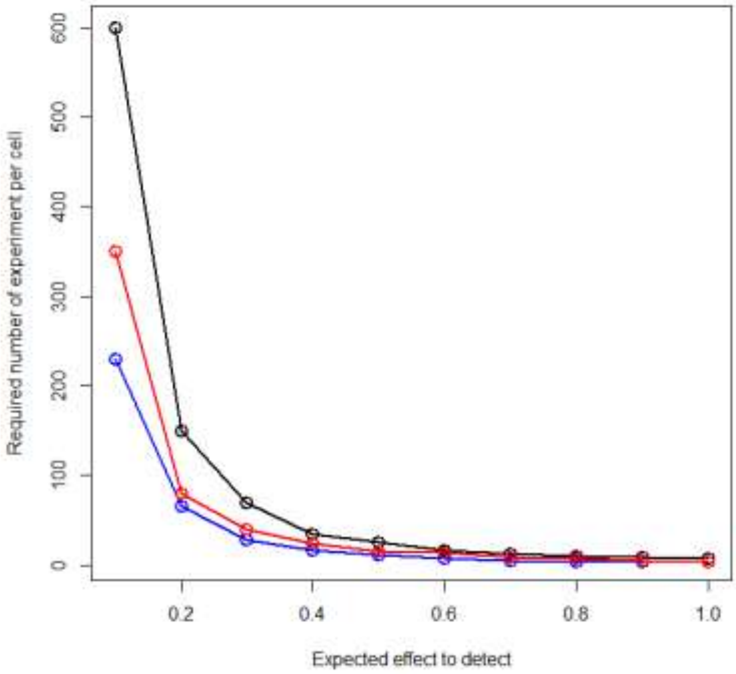

Figure 5. The required number of replicates per treatment combination to achieve a power of ≥0.8, given the expected effect size to be detected, the number of fish for treatment 1 (n_1) and treatment 2 (n_2), the standard deviation among experiments/replicates $\sigma_{\exp}$.

Conclusions
Ideally, discard mortality experiments should have a high statistical power: a high probability of detecting a treatment effect where an effect exists. However, due to the many biological, technical, environmental and experimental variables, the variability in discard mortality estimates is large. When the variability in discard mortality estimates is large, the number of replicates that is required to demonstrate an effect increases sharply. In determining the level of replication it is also important to consider the expected effect size and the stocking density of underwater cages or onboard containers. In the example power analysis, the detected difference in survival between the treatments was very low (β =-0.13 for the effect of picking location and β =-0.21 for captivity method; Table 9). Thus, to detect a significant difference, between 150 and 600 replicates per treatment combination would be needed. If an *a priori* power analysis would be carried out for an experiment with distinctive treatment levels (e.g. short vs long air exposure times) with biologically plausible effect size such as β =0.5, only 25 replicates per treatment combination would be needed (Table 10).

The power analysis considered only two fixed factors (i.e. manipulated treatments) from a single day of fishing. Many more categorical or continuous variables may be associated with discard mortality (Broadhurst *et al*. 2008) which may all introduce variability and make it more difficult to tease out a significant effect between treatments. A study that proposes to monitor discards onboard vessels operating with different gears in different seasons and areas will need a (very) high level of replication to be able to state that a certain modified gear significantly reduces mortality over another gear.

Appendix D. Excerpt from STECF expert group on survival of discards

Excerpt from the 'Scientific, Technical and Economic Committee for Fisheries (STECF) – Landing obligation in EU fisheries (STECF-13-23). 2013. Publications Office of the European Union, Luxembourg, EUR 26330 EN, JRC 86112, 115 pp':

"Survival issues: Research has shown that not all discards die. In some cases, the proportion of discarded fish that survive can be substantial, depending on the species, fishery and other technical, biological and environmental factors. Article 15 paragraph 2(b) of the regulation allows for the possibility of exemptions from the landing obligation for species for which "scientific evidence demonstrates high survival rates". Taking the first element of this – "scientific evidence"- it is important that managers have guidance on protocols and methodologies that should be followed in order to ensure the results of such experiments are scientifically robust. Presently there are no such internationally agreed guidelines. EWG 13-16 therefore has provided guidance on best practice to undertake survival studies. This includes a detailed description of the methodological approaches available, their advantages and disadvantages and what factors need to be considered when undertaking such studies including sample sizes, selection and treatment of specimens and protocols for the various methods. In this regard EWG 13-16 has identified three methodologies for conducting survival experiments i.e. captive observations, vitality/reflex assessments and tagging/biotelemetry experiments. The intention is that this initial analysis will be followed up by an ICES expert group with the express intention of publishing guidelines for the conduct of discard survival studies.

Managers also require guidance on the second element of this provision – "high survival rates". EWG 13-16 was therefore asked provide an objective framework to identify what constitutes "high survival". However, on the basis of the analysis carried out, EWG 13-16 concluded that the term "high survival" is somewhat subjective. From the perspective of waste minimisation, it could be viewed that the minimum level of survival that could be considered as high is where true survival (as opposed to experimentally observed survival) is greater than 50%. Put simply, any value less than this would result in a greater proportion of fish dying than those surviving and simply means that less of the resource is wasted (as dead fish) than is returned alive, to contribute to the stock biomass. However, defining a single value cannot be scientifically rationalised and therefore EWG 13-16 advises that assessing proposed exemptions on the basis of "high survival" need to be considered on a case-by-case basis taking account the specificities of the species and fisheries involved.

EWG 13-16 also looked at the potential impacts of exemptions for survival on fishing mortality, SSB and associated reference points. Obliging fishermen to land catches of fish that would otherwise have survived the discarding process could, in some specific cases, result in negative consequences for the stock, the rationale for excluding species meeting high survival criteria. This is because any surviving discarded fish contributes positively to the stock and landing those individuals therefore removes that benefit. This in effect increases fishing mortality. However, the potential impact is heavily dependent on a number of factors including the age structure of the discards; discard survival rates at age; natural mortality at age; the contribution discards make to the overall catch and; the overall status of the stock. The worked example show that where there is >50% survival across all age groups, then landings these fish would result in ~30% reduction is stock biomass (after 20 years) assuming no change in selectivity. Where discard survival is lower with younger age groups the effect is far less pronounced (~6%). The other example provided shows that in order to maintain catches within predefined targets e.g. Fmsy, then it would be necessary to reduce fishing opportunities in order to compensate for the contribution surviving discards had previously made to the stock. Furthermore, the choice to exempt a particular species is a "trade-off" between the stock benefits of the continued discarding of "high" survivors, which can be estimated through established forecasting models, and the potential removal of incentives to change exploitation pattern by allowing discarding. EWG 13-16 advises that such an evaluation should also consider the potential benefits for other stocks and the broader ecosystem that would arise from changes in exploitation pattern. EWG 13-16 considers that avoidance of unwanted catch should be the primary focus of such considerations and take precedence over the application of exemptions based on high survival. EWG 13-16 notes that in cases where exemptions are provided it will be necessary, to document the weight and age composition of discarded catches for accurate estimation of fishing mortality where discard survival rates are less than 100%."