

Towards a balanced presentation and objective interpretation of acoustic and trawl survey data, with specific reference to the eastern Scotian Shelf

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Abstract: A debate has developed over the ecosystem consequences following the collapse of Atlantic cod throughout the coastal waters of eastern Canada. The explosive increase in pelagic fish abundance in scientific bottom-trawl catches on the eastern Scotian Shelf has been interpreted as being due to either (i) a “pelagic outburst” of forage fish abundance resulting from predator release or conversely (ii) a change in pelagic fish vertical distribution leading to a “suprabenthic habitat occupation” thereby increasing their availability to bottom trawls. These two interpretations have diametrically opposing ecological consequences and suggest different management strategies for these important forage fish species. We argue that an objective evaluation of the available evidence supports the hypothesis that the abundance of forage fish has not increased in response to the demise of cod and other top predators, and the reliance on a single sampling gear with low catchability has biased and will continue to bias the interpretation of demographic trends of pelagic fish populations. We advocate that multiple sampling technologies providing alternative perspectives are needed for the monitoring and management of the various trophic levels if we are to achieve a balanced and objective understanding of marine ecosystems.

Résumé : Un débat a vu le jour concernant les conséquences écosystémiques de l'effondrement des stocks de morue au travers les eaux côtières de l'est du Canada. L'augmentation fulgurante de l'abondance des poissons pélagiques dans les prises scientifiques au chalut de fond dans la partie est de la plate-forme Néo-Écossaise a été interprétée comme étant le fait soit (i) d'une « explosion pélagique » de l'abondance de poissons fourrage résultant de la baisse des prédateurs ou, à l'inverse, (ii) d'un changement de la répartition verticale des poissons pélagiques ayant mené à leur « occupation des habitats suprabenthiques », accroissant leur disponibilité pour les chaluts de fond. Ces deux interprétations ont des conséquences écologiques diamétralement opposées et nécessiteraient des stratégies de gestion différentes pour ces espèces de poissons fourrage importantes. Nous arguons qu'une évaluation objective des données probantes disponibles appuie l'hypothèse selon laquelle l'abondance des poissons fourrage n'a pas augmenté en réponse à l'effondrement de la morue et d'autres prédateurs de niveau trophique supérieur, et que le recours à un seul engin d'échantillonnage caractérisé par une faible capturabilité a faussé et continuera de fausser l'interprétation des tendances démographiques des populations de poissons pélagiques. Nous soutenons que plusieurs techniques d'échantillonnage offrant différentes perspectives sont nécessaires à la surveillance et la gestion des différents niveaux trophiques et pour en arriver à une compréhension équilibrée et objective des écosystèmes marins. [Traduit par la Rédaction]

Introduction

The ecological aftermath ensuing from the collapse of Atlantic cod (*Gadus morhua*) and other predatory ground fish populations in many eastern Canadian coastal ecosystems in the early 1990s has been intensely studied in the subsequent decades (e.g., Bundy 2005; Dawe et al. 2012; Savenkoff et al. 2007b; Worm and Myers 2003). Regardless of the causes of the collapse, the lack of recovery of east coast cod populations to their former numbers despite over 25 years of low fishing effort and moratoria has been well documented (Bundy and Fanning 2005; Smedbol and Wroblewski 2002; Swain and Mohn 2012) and its causes and consequences extensively debated in terms of regime shifts, top-down versus bottom-up trophic dynamics, and more recently “oscillatory runaway consumption dynamics of the forage fish complex” (Frank et al. 2005, 2011; Greene 2013; Greene et al. 2012; Greene and Pershing 2007; Grubbs et al. 2016; Pershing et al. 2015; Sinclair et al. 2015).

Numerous investigations have taken advantage of this large-scale ecological experiment to examine the role of trophic interactions in the control of demographic trends (Bundy 2005; Bundy and Fanning 2005; Dawe et al. 2012; Savenkoff et al. 2007a). In a series of papers, Ken Frank and colleagues (Frank et al. 2005, 2006, 2011) have argued that the eastern Scotian Shelf (ESS) ecosystem, as well as most Northwest Atlantic ecosystems, have undergone a restructuring due to the release of the top-down control exerted by apex predators, principally cod, a control that has had a cascading effect through four trophic levels (six if the proposed effects of phytoplankton on nutrients, as well as the human role in the cod collapse, are included). A key element of the ESS trophic-cascade hypothesis is the contention that the release of predation pressure on small pelagic fishes such as Atlantic herring (*Clupea harengus*), sand lance (*Ammodytes dubius*), and capelin (*Mallotus villosus*) has led to a pelagic outburst (PO) sensu Cushing (1980) on the ESS, as well as the Labrador shelf – northern Grand Banks, the

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southern Grand Banks, and the northern and southern Gulf of St. Lawrence (Frank et al. 2006). On the ESS, it has been estimated that population biomass of forage fishes had increased by 900% in less than a decade. Frank et al. (2011) further argue that the severe reduction in cod numbers and explosion in small pelagic numbers have led to a predator–prey role reversal whereby forage species, e.g., herring and sand lance, “control” the diminished predators through the consumption of or direct competition with the early life stages of cod, which has been the leading impediment to the cod recovery.

The ESS trophic-cascade hypothesis was based on inverse trends among selected time series of annual abundance indices chosen to represent population trends of major trophic levels within Northwest Atlantic ecosystems. While being a simple and enticing concept, it has been contested on several levels; for example, the proposed top-down control of nutrients by phytoplankton and phytoplankton by zooplankton has been challenged based on independent data (Greene 2013; Greene et al. 2012; Greene and Pershing 2007; Head and Sameoto 2007) and will not be addressed here. The PO hypothesis is not only a critical element in the trophic-cascade concept as it applies to eastern Canada, but indeed has had major repercussions concerning resource exploitation and management, not the least of which was the gearing up of the pelagic fishing industry to harvest the estimated biomass of ESS herring that in the end, fishers and researchers have had difficulty finding (Power et al. 2006). The alleged 900% increase in pelagic biomass was based on a bottom-trawl index (BTI) used by Frank et al. (2005) for population trends of pelagic fish species (principally herring, capelin, and sand lance), trends that have yet to be confirmed by independent means.

McQuinn (2009) refuted the PO hypothesis on the grounds that pelagic-fish BTIs are unreliable, even in a relative sense, because the availability of small pelagics to bottom trawls is extremely low, highly variable, and ultimately biased over time due to changes in their vertical distribution relative to the effective trawl opening. McQuinn (2009) provided evidence from two regions adjacent to the ESS showing that standard BTIs did not follow herring abundance trends determined from formal stock assessments and were instead related to the presence or absence of a major demersal predator, Atlantic cod. Further, McQuinn (2009) showed acoustic evidence that the increase in the ESS pelagic BTI could be explained by a suprabenthic habitat occupation (SHO) by small pelagic fishes, habitat that had been vacated by their principal suprabenthic predators.

In their rebuttal to McQuinn’s conclusions, Frank et al. (2013), hereafter Frank et al., disagreed with the SHO hypothesis while leaving many of the observations raised by McQuinn (2009) unaddressed. At the same time, they engaged in an “argumentum ad methodum sonicam” of unbalanced criticisms of fisheries acoustic methods in general and their application by McQuinn specifically. In so doing, one is left with the impression that the credibility of the acoustic method was put into question, while the potential and evidence-based biases of their own selected datasets were left unchallenged.

In the present paper, we maintain that Frank et al. have not effectively addressed the arguments raised by McQuinn (2009) and argue that their critique of the acoustic method is both unsubstantiated and indeed immaterial in this debate. In our response, we present plausible alternatives to their interpretations, address specifically the unbalanced criticisms of acoustic sampling methods, and highlight case studies demonstrating how trawl and acoustic data can be used in concert to address sampling bias issues. Our response is structured as a brief description of the context followed by an issue-by-issue discussion of the elements raised by Frank et al.

Context

Our perceptions and ultimate understanding of the structure of aquatic ecosystems and the interdependencies between predators and prey species, in both middle and lower trophic levels, are largely based on our ability to measure and monitor the demographic dynamics of the biota, their pelagic and demersal habitats, and potentially the processes and mechanisms underlying those dynamics. Every measurement is biased (accuracy) with some level of uncertainty (precision) and every collection of measurements has variability. The catchability (q) of a net is defined by the species’ availability, i.e., how the net interacts with the animals before they enter the net opening, and selectivity, i.e., the interaction with the net once the organisms have entered it (Wileman et al. 1996). Note that this definition refers specifically to the net and is not equivalent to the q used in stock assessment, which incorporates survey design, e.g., areal distribution, and is a direct scalar between relative trends and true (aka absolute) abundance. Detectability is the analogous trait of underwater acoustic technology (Rose et al. 2000) in which the availability of the target is its relationship to the acoustic beam and proximity to physical boundaries, e.g., the seabed and sea surface, and selectivity is the target strength of the target, i.e., the interaction of the acoustic frequency with the size, shape, orientation, and anatomy of the animal. These definitions are not constrained to acoustic or trawling methods as they apply to all sampling techniques, e.g., continuous plankton recorder (CPR), plankton nets, remote sensing, etc. Nets and acoustics are effectively independent methods in that using traditional deployment procedures, the catchability of one does not affect the catchability of the other. Ideally, multiple sampling methods are used to measure and monitor the ecosystem, and given careful calibration and characterization of each method while acknowledging and compensating for the inherent biases of each, corresponding or matching estimates can be produced to provide validation of the overall assessment as well as a measure of confidence in each method (e.g., McQuinn et al. 2005; Yule et al. 2008; Trenkel et al. 2011).

The reliance on a single data source, especially one that has low catchability for the targeted species, e.g., Atlantic herring (Edwards 1968; Harley et al. 2001), is not only a high risk endeavour, because changes in catchability are undetectable over time, but trends from a time series of such measurements can create the impression that the data are an unbiased representation of the state of these species within the ecosystem. This impression appears to have been taken to the extreme by Frank et al. where they state: “Temporal trends in the biomass of herring, sand lance, and cod were assessed from annual fishery-independent, bottom trawl surveys [...] This is the **only systematically developed, scientifically rigorous** source of data available with which to conduct such an evaluation” (our emphasis). Clearly, it must be acknowledged that a bottom trawl survey — no matter how systematic or rigorously executed — is an indirect sampler for monitoring pelagic fishes and, being the only source of data, does not de facto make it reliable.

The debate

Survey bottom-trawl catches of cod, herring, and sand lance from 1970 to 2010 (fig. 2 in Frank et al.) clearly show changes in catch rates from about 1994, when cod catches were high and herring and sand lance catches were low to nil prior to 1994, i.e., before the collapse of the cod stock (hereafter pre-collapse), and catch trends reversed after 1994, i.e., after the collapse of the cod stock (hereafter post-collapse). The debate is not centered on this observation; both the PO and SHO hypotheses use this observation as a starting point. The debate is whether herring and sand lance abundances increased (PO) or whether the catchability of herring and sand lance increased (SHO). The PO hypothesis fundamentally treats the bottom-trawl catches as representative of

stock abundance with no change in catchability, i.e., a stable q . On the other hand, the SHO hypothesis argues that bottom-trawl catches were not representative of stock abundance of these pelagic fishes because q was not constant over time, i.e., their availability dramatically increased between the pre- and post-collapse periods. The PO hypothesis suggests a dramatic increase in abundance, whereas the SHO hypothesis does not suggest anything about abundance, only that herring and sand lance changed vertical distribution and habitat. Without any direct measurement of the vertical distribution of small pelagic species on the ESS over time, we must rely on measurements of the vertical distribution of small pelagic fishes in adjacent regions and indirect, corroborating information to weigh these two opposing explanations.

Diel vertical migration (DVM)

Frank et al. used the average annual day-to-night (D/N) ratio of survey bottom-trawl catch rates of herring and sand lance in the ESS as an index of diel vertical migration (DVM) behaviour to address the relationship between small pelagic fish catches and cod abundance. According to their reasoning, a DVM value < 1 indicates that a species is predominately demersal during the night, while DVM > 1 indicates that a species is predominately demersal by day, and fish are vertically distributed somewhere in the water column during the opposing sun phase. Frank et al. maintain that DVM > 1 is indicative of herring with “normal” vertical migratory behavior whereby individuals are closer to the sea floor during the day and shallower at night. In this regard, Frank et al. make two predictions of this DVM index based on their interpretation of the SHO hypothesis: (i) the DVM index will be positively correlated with cod biomass and (ii) this DVM index will not deviate significantly from 1 following the collapse of cod.

The predictions of Frank et al. stem from their argument that at high cod biomass, herring and sand lance will perform a normal DVM (near the bottom in the day and shallower at night) resulting in DVM > 1 , while in the absence of cod, there should be no difference between day and night catches, i.e., DVM = 1, resulting in the DVM index being positively correlated with cod biomass. However, such a prediction assumes a very particular, and we suggest unlikely, behavioural change given the observations.

Firstly, we contend that this DVM index does not measure actual DVM behaviour but rather relative availability of fish to the trawl. For example, one can obtain a DVM = 1 in bottom-trawl data by a complete cessation of vertical migration or by a change in residence depth in both night and day in such a way that the number of individuals remains, on average, constant in the bottom layer. In the second case, there is still DVM but it is not detectable from bottom-trawl data. Thus, the second prediction does not follow from SHO and therefore investigating the hypothesis of DVM > 1 is irrelevant as it cannot provide any evidence for or against SHO. However, that said, we will examine the data for the patterns that they present regarding species behaviour.

Visual inspection of fig. 5 in Frank et al. shows that in 6 of the 8 years (75%) prior to the cod collapse, the DVM index was < 1 , suggesting that herring were more apt to be caught in bottom trawls during the night, which is consistent with a shallower residence depth during the day at high cod densities. In support of this observation, McQuinn et al. (2005) have shown through a series of day and night acoustic surveys that cod off western Newfoundland also perform DVM, occupying the suprabenthic habitat more intensively in the day than at night. This would make the demersal habitat less favourable for herring during the day, which is supported by both the low catches in the day and the night and the DVM values < 1 before the cod collapse, despite what one considers “normal” DVM behaviour for herring.

Further, a consistent DVM > 1 after the cod collapse is not evident in the data that Frank et al. present. After the cod collapse, the DVM index was split equitably, with 8 of the 19 years (42%) < 1 ,

10 of the 19 years (53%) > 1 , and 1 year ≈ 1 , suggesting that occupation of the demersal zone by herring after the cod collapse has been more variable than before the cod collapse, resulting in no consistent pattern of the day–night presence of herring in the trawl zone. The nearly equal distribution of DVM values above and below DVM = 1 after the collapse supports the contention that over time, the DVM index does fluctuate around 1 in the absence of a predator such as cod.

The two predictions by Frank et al. used to refute the SHO hypothesis are either not supported by the data or do not follow from the SHO hypothesis. We contend that on a behavioural basis, a DVM value = 1 in the post-collapse era is not a suitable criterion to test the SHO hypothesis and, from our interpretation of the data, is in fact consistent with the SHO hypothesis.

Finally, Frank et al. imply that their observation of equivalent length frequency distributions of herring and sand lance caught in bottom trawls between day and night supports the PO hypothesis. We are unsure how this is related to their DVM index or relevant to their argument. Equivalent length frequencies only suggest that the day and night catches are not length-based, and it appears to be unrelated to the presence or absence of cod.

Simply put, it is impossible to directly measure vertical migration with a bottom trawl. First, DVM is a very specific behaviour related to systematic changes in vertical distribution that occur on a daily cycle. Only the average D/N catch-rate ratios in the trawl zone can be measured with a bottom trawl, from which vertical distribution is inferred. However, the reasons for any measured changes in D/N catch-rate ratios are multiple, given that a species' availability to a trawl can depend on many factors, of which a true DVM is only one. Frank et al.'s DVM index is calculated on an annual basis for July and therefore is influenced by a variety of behaviours, e.g., prey availability, environmental conditions, and the presence of predators. In addition, a bottom trawl most effectively samples the few metres directly above the bottom during the haul, potentially collects fish in the water column that dive in front of it or during retrieval, i.e., haul back, and is least effective during deployment and at the surface. Unless the trawl has multiple opening–closing codends or some other independent sampling gear such as acoustics or optics are used, e.g., Handegard and Tjøstheim (2005), where along the trawl path the specimens were caught cannot be determined directly from the catch data alone. In addition, the DVM index cannot be used as a surrogate for abundance, as it only reflects the availability of the fish to the bottom trawl. In other words, the same abundance of fish could produce different DVM index values if the species is modulating its vertical distribution in response to environmental or biological factors, e.g., predator–prey interactions.

In addition, trawl catches inform even less about what vertical migration behaviour is happening in the remaining 90+% of the water column. For example, a species could be performing a perfectly regular DVM behaviour throughout the water column while completely avoiding the trawl zone, which is most likely the case with herring when cod biomass is high. Ideally, a combination of bottom trawling, midwater trawling, and active acoustics (downward- and (or) upward-oriented transducers) would be substantially better at monitoring vertical migration behaviour — whether diel or otherwise — and reducing the uncertainty as to whether the animals are migrating or abundance has changed.

Given the available acoustic and trawl evidence, we conclude that (i) herring almost completely avoided the bottom in daytime in the presence of cod, (ii) herring either avoided or occupied the bottom more or less equally in daytime or nighttime in the absence of cod, both expected under the SHO hypothesis if herring avoid cod when cod are abundant, but rely on other cues in the absence of cod, and (iii) survey bottom-trawl q must have changed over time as suggested by the change in DVM index values between pre- and post-collapse. However, implicit in the PO hypothesis is a consistent vertical migratory behaviour of herring and

sand lance throughout pre- and post-collapse, as a direct result of the explicit assumption of a constant q .

Proportion of tows with herring

Frank et al. suggest that the proportion of bottom-trawl catches with herring and sand lance (or presumably any species) is indicative of stock abundance, with greater proportion equating to greater abundance. Their metric is the “proportion of tows occupied” with herring or sand lance from 1970 to 2010 (fig. 3 in Frank et al.), which they equate to “area occupied based on presence/absence data” and consider as a “metric of population abundance”. The reader is left to assume that the tows were consistently distributed over the full area from year to year and that an increase or decrease in the number of occupied tows is directly proportional to area occupied, i.e., there was no clumping of catches in some areas. The proportion of tows was lowest in about 1980 and then steadily increased from 1984 to 2010 for both species. Frank et al. suggest a threefold increase in proportion between pre- and post-collapse, although it is not clear from their fig. 3 how this was calculated.

Frank et al. equate a catch metric with abundance but do not present data or evidence that the catchability of the bottom trawl remained constant over those years or area. Similar to equating catch density to stock abundance, using the proportion of tows with herring or sand lance present, i.e., occupied tows, as an index of abundance will depend on the catchability remaining constant over time and space. An increase in catchability, as we suggest, could show an identical temporal and spatial pattern.

Commercial catch

Frank et al. show that herring catches in western Newfoundland by purse seines declined slightly from 1988 to 2002 (their fig. 6), but apparently not to the extent that they expected from their interpretation of the SHO hypothesis. In this case, Frank et al. are interpreting a change in catch as a change in vertical distribution rather than a change in abundance, which is opposite to their position for the bottom-trawl data. However, similar to sampling only near the bottom, sampling only near the surface with a purse seine, or any other gear, is equally unsuited for unraveling trends in vertical distribution from trends in population biomass. This is due to the substantial evidence that commercial purse seine catch rates (catch per unit effort), much less raw catches as are considered here, are not reflective of population or stock abundances (Winters and Wheeler 1985) and for that reason have not been used in herring stock assessments in eastern North America since the mid-1980s (Canadian Atlantic Fisheries Scientific Advisory Committee (CAFSAC) 1986). Commercial fisheries target the species of interest and can continue to catch consistent numbers even while the stock is declining (constant catch paradox), often by exploiting fish behaviour, e.g., by using lights to draw the fish up to the surface when they are too deep.

It is worth noting that Frank et al. also mention that the biomass of snow crab and shrimp, which can be prey for cod, increased since the decline in cod and have sustained lucrative fisheries for many years. It seems that they are suggesting that because two invertebrate species have apparently increased due to reduced predation, it is likely that Atlantic herring and sand lance abundances have done the same. However, this link between snow crab abundance and cod predation has also been challenged (Chabot et al. 2008).

Larval herring abundance

In an apparent contradiction to their assertion that the research bottom-trawl time series was “the only systematically developed, scientifically rigorous source of data available with which to conduct an evaluation of [pelagic fish abundance]”, Frank et al. assembled an amalgam of larval density estimates from diverse larval

surveys and fixed station studies from the early 1980s and the early and late 1990s to construct a time series of “trends in herring larval abundances”. These larval surveys were by no means standardized (Table 1). Some were concentrated on the offshore bank spawning area (Dal/DFO); some covered the whole Scotian shelf and Bay of Fundy except for the offshore bank spawning area (H045); most covered the months before spawning (of the eight SSIP surveys, only two were conducted after spawning); some were after spawning (of these two, only H028 covered the suspected spawning bank); and some were from single fixed stations, one on the bank and one off (OPEN). For the purposes of this investigation, only two annual estimates are at all comparable, Dal/DFO97 and Dal/DFO98, one of which gave a very low estimate ($0.08 \text{ g}\cdot\text{m}^{-3}$) and one of which gave a considerably higher estimate ($1.2 \text{ g}\cdot\text{m}^{-3}$).

However, the fact that these data are presented at all with virtually no discussion of possible biases or uncertainties underlines an interesting juxtaposition. Frank et al. have no reservations about using a nonstandardized spatial and temporal mixture of individual estimates from unrelated studies to construct a larval abundance time series, the results of which are used in a six-point regression with four points at the origin and one highly influential leverage point (1998) as support for their hypothesis (Fig. 1). At the same time, they underline insignificant logistical issues during a standardized acoustic survey to dismiss the results of an entire survey time series.

Furthermore, Sinclair et al. (2015) have examined the relationship of these larval datasets with formally assessed, peer-reviewed adult fish biomass estimates and found no correspondence between spawner abundance and larval abundance for four ground-fish species, which in theory should be sampled much more efficiently than pelagic fishes.

Argumentum ad Methodum Sonicam

In their critique of McQuinn (2009), Frank et al. ultimately present a series of issues that ostensibly address what they feel are deficiencies in the acoustic surveys conducted by McQuinn specifically and the acoustic methodology in general. A critical examination of the surveys and methods is certainly warranted but should be applied equally among all surveys and methods, i.e., those used by Frank et al. in their construction of the PO hypothesis. It is unfortunate that Frank et al. only critique the acoustic methods, as this gives the impression that the bottom-trawl and larval surveys are infallible while calling into question only the acoustic methodology. We wish to provide a more balanced assessment.

Day and night sampling

Frank et al. point out that all of the acoustic surveys described by McQuinn were conducted at night, except for the first in 1989, which was conducted during both day and night, and state that this “may have contributed to the apparent behavioural shift reflected in the post-1989 survey data”. They highlight the differences in vertical distribution between the 1989 and subsequent surveys that McQuinn suggests were in response to the decrease in cod abundance. They imply that the change in sampling regime biased the data and interpretation of the observed changes in vertical distribution and was “the genesis of McQuinn’s (2009) formulation of the SHO hypothesis”.

However, according to Frank et al., herring exhibiting normal DVM behaviour should be deeper in daytime, i.e., more available to the bottom trawl during the day than at night. This then argues against any upward bias in 1989 as the average vertical distribution should be deeper when including daytime data, not shallower as implied by Frank et al. We recalculated the median height off the bottom of herring using the 1989 nighttime data only to be 47.0 m above the bottom, as opposed to 43.0 m using both day and night data. Thus, if anything, the median height off

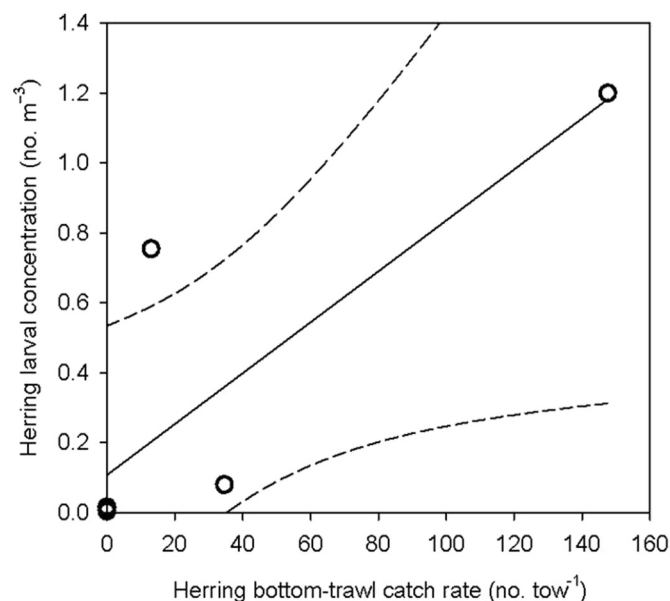
Table 1. Sampling conditions from ichthyoplankton surveys conducted periodically throughout the eastern Scotian Shelf region since the late 1970s: the Scotian Shelf Ichthyoplankton Program (SSIP), the Ocean Production Enhancement Network (OPEN) larval surveys, and the joint DFO/Dalhousie University larval surveys.

Program	Year	Survey	Month	Timing re: spawning	Area covered	Gear	Sampling protocol ^a	Tow speed (m·s ⁻¹)	Survey design	Comments
SSIP	1979	H025	9–10	Before	ESS–WSS	Bongo: 61 cm; 333 µm	1	1	Grid: 150 stations	Outside spawning season
		H028	11–12	After	ESS–WSS	Bongo: 61 cm; 333 µm	1	1	Grid: 150 stations	Outside spawning season
	1980	LE02	9	Before	ESS–WSS	Bongo: 61 cm; 333 µm	1	1	Grid: 150 stations	Outside spawning season
		AR03	10	Before	ESS–WSS	Bongo: 61 cm; 333 µm	1	1	Grid: 150 stations	On-bank stations not sampled
	1981	H045	11–12	After	ESS–WSS–Fundy	Bongo: 61 cm; 333 µm	1	1	Grid: 150 stations	Outside spawning season
		EK01	8–9	Before	ESS–WSS	Bongo: 61 cm; 333 µm	1	1	Grid: 150 stations	Outside spawning season
OPEN	1991	EK02	9–10	Before	ESS–WSS	Bongo: 61 cm; 333 µm	1	1	Grid: 150 stations	Outside spawning season
		NO91	11	After	n/a ^b	Bongo: 50 cm; 153–252 µm	2	1.3	Fixed: 2 stations	1 station on, 1 off the bank
	DE91	12	After	n/a	Bongo: 50 cm; 153–252 µm	2	1.3	Fixed: 2 stations	1 station on, 1 off the bank	
	1992	NO92	11	After	n/a	Bongo: 50 cm; 153–252 µm	2	1.3	Fixed: 2 stations	1 station on, 1 off the bank
		DE92	12	After	n/a	Bongo: 50 cm; 153–252 µm	2	1.3	Fixed: 2 stations	1 station on, 1 off the bank
	Dal/DEO	1997	Dal/DEO97	11	After	Central ESS	Bongo: 61 cm; 333 µm	2	1	Grid: 88 stations
1998		Dal/DEO98	11	After	Central ESS	Bongo: 61 cm; 333 µm	2	1	Grid: 88 stations	

^a1 = Single oblique, bottom to surface; 2 = double oblique, surface to bottom – 5 m.

^bn/a indicates not applicable.

Fig. 1. Bottom-trawl catch rates of ESS herring and herring larval concentration (open circles), linear regression line (solid line), and 95% confidence intervals (broken lines) for larval surveys conducted in 1979, 1980, 1981, 1991, 1997, and 1998 as chosen by Frank et al. (2013). Note the high leverage point at the upper right. Individual survey sampling conditions are described in Table 1.



the bottom was shallower when using nighttime data only, contrary to the prediction of Frank et al.

Examination of day–night differences in acoustic data collection is a valid pursuit, but simply collecting data day and night will not automatically alleviate any biases. Most acoustic surveys are initially conducted day and night to examine behavioral differences in target species. Many species undergo DVM and this can significantly affect detectability to acoustic instrumentation. In many cases, after careful examination of the data, acoustic and midwater trawl surveys are often conducted either only during day or only during night light conditions (e.g., Honkalehto et al. 2011; Stockwell et al. 2007). If the acoustic and midwater trawl data are collected simultaneously, then the data can be stratified and analyzed to account for any differences. Bottom-trawl surveys, or any other survey, are no different and are affected by the same factors. For example, light has been shown to effect bottom-trawl catchability (Kotwicki et al. 2009). Careful examination of acoustic and bottom-trawl data must be done to understand diurnal effects.

Technical issues

Frank et al. address the evidence used to construct the SHO hypothesis by challenging the quality of the acoustic data presented in McQuinn (2009). However, none of the “significant technical issues” mentioned is relevant to this debate. Most of the identified technical issues refer to the 1990 survey, which was not used in the McQuinn (2009) analyses. The rest pertain to either the acoustic system calibration of the 1989 survey, which is why this survey’s results were not used in the time series for the stock assessment, or the “data loss” from the 1993 survey and variable survey coverage. These irregularities in no way affect the vertical distribution measurements presented in McQuinn (2009). The vertical distribution estimates are from relative backscatter measurements and therefore do not depend on the calibration, while the data loss issue in 1993 involved less than 10% of the total data collected.

Frank et al. also imply that significant “evolution” in the survey vessel employed for the acoustic survey was somehow detrimen-

tal to the success of the survey. However, they neglect to disclose that during the ESS bottom-trawl time series, there were vessel and gear changes in 1982 from the side-trawler *A.T. Cameron* with a Yankee 36 trawl (wingspread, 11 m; headline height, 2.0 m) between 1970 and 1981 to the stern-trawler *Alfred Needler* with a Western Ila trawl (wingspread, 12.5 m; headline height, 3.5 m) since 1983. This gear change had measurable effects on catchability for several demersal species (Fanning 1985). Therefore, the possibility that availability changed between the two vessels for pelagic species as well cannot be discounted. Although it is more likely that availability did change, there is no way to determine this from the bottom-trawl catch data alone without doing a comparison. The availability could have increased, decreased, or remained stable, but without independent data, it is impossible to know. Thus the cause of the increase in pelagic species catches is equivocal without further information. It is therefore not reasonable to equate changes in the catch rate of these species only with changes in abundance.

Seabed and surface dead zones

Acoustic “dead zones” are locations not well sampled by acoustic methods and tend to be near boundaries such as the sea surface and seabed but also near reefs or steep slopes (Patel et al. (2009) and references therein). Frank et al. suggest that the “surface blind zone effect [...] may have contributed to the apparent behavioural shift reflected in the post-1989 survey data”. However, in the post-1989 period, virtually no herring were detected close to the surface, indicating that either all of the surface-dwelling herring were above the estimated surface blind zone of 5 m or that an insignificant proportion of the annual herring biomass was found at the surface at night. In a similar study of the vertical distribution of capelin, one of Frank’s co-authors concluded that “the extent of diel patterns [of capelin was] not obscured [by] the transducer depth” (Shackell et al. 1994), thereby dismissing the possibility that fish in the surface dead zone was a significant issue for capelin in Newfoundland waters. The same conclusion applies here given that no herring were registered near the face of the transducers in the western Newfoundland herring dataset.

Echoes from the seabed will mask echoes from individuals or even aggregations of individuals, rendering targets up to some height above the seabed effectively undetectable to the acoustic system. The height of the seabed dead zone can be variable depending on rugosity and bathymetric features, as well as the acoustic pulse length. However, if significant amounts of herring were hidden in the seabed dead zone in McQuinn’s study, the estimates of the near-bottom densities would have been even greater, i.e., this would have accentuated the contrast in vertical distribution between the pre- and post-collapse years.

On another level, it is curious that Frank et al. chose to highlight this particular limitation of echosounders as being detrimental to monitoring a pelagic species such as Atlantic herring. By any measure, the “dead zone” for a bottom trawl is nearly the entire water column, so it seems to stretch credibility to think that a bottom trawl is more effective at quantifying vertical distribution, e.g., DVM, than an echosounder. It is common to scrutinize acoustic data so that only 0.5 m above the bottom is excluded. Cases in which we cannot get that close are on steep slopes and over bathymetric features such as rocky outcrops, but these are also “untrawlable bottoms” where bottom trawls are not able to sample at all.

Integration of seabed echo

Echoes from the seabed are orders of magnitude greater, i.e., 10s of dB, than the echoes from biological targets. Even when those targets are very densely packed, the seabed echo is much larger than those from the targets. For this reason, eliminating the seabed echo is critical for high-quality abundance estimates. However, because the seabed echo is so strong, inclusion of it is easily

detected and mitigation of the effect is simple, albeit time consuming, and is not an issue to anyone who has had experience with scrutinizing acoustic data.

Outstanding issues

Finally, there remain several significant unresolved issues raised by McQuinn (2009) and others that Frank et al. have not addressed. Far from being details, these issues are fundamental to the mounting evidence against the PO hypothesis. These issues include, but are not limited to the following:

1. Why was there no increase in the proportion of small pelagics in the offshore seal diet (Bowen et al. 2006) between the pre- and post-collapse periods? Clearly, if these pelagic stocks grew from virtually undetectable in the 1980s to such great abundances, there would have been a measurable signal found in seal stomachs in the ESS.
2. When structured, peer-reviewed stock assessments were available for comparison with the pelagic species’ BTIs, they were found to be uncorrelated. Further, in Canada, no indices of abundance from bottom-trawl surveys have ever been used for quantitatively assessing pelagic fish stock abundance because they were deemed “not useful” (CAFSAC 1982). If they are not reliable for assessing stock size for commercial fishery management, why would they be reliable for ecological studies?
3. Although the presumed herring biomass from Frank et al. would place the estimate of the ESS herring stock at 900 000 tonnes (t), i.e., the largest herring stock in eastern North America, neither commercial nor acoustic research efforts have been successful, despite several attempts, in finding commercially sustainable abundances of this stock. One would expect that some obvious sign of this large accumulated biomass would be measurable, other than from the bottom-trawl survey, after 25 years. Nonetheless, fisheries management allocated a 12 000 t quota on this stock in 1996 based almost entirely on increases in the research survey bottom-trawl catches. This quota has been reached in only 5 of the last 20 years (Department of Fisheries and Oceans Canada (DFO) 2015).

Exemplars

It is well known that the vertical distribution of pelagic and semipelagic species ranges from very near the sea (or lake) bed to many metres above the bottom, and yet surprisingly, management of these species often relies on relative trends based on either bottom-trawl or acoustic (in conjunction with midwater trawling) data alone, but not both. However, there are examples of when these data sets have been used in concert to improve understanding of ecosystems, trophic dynamics, and stock assessment. In Lake Superior, the pelagic and semipelagic fish species are managed using abundance estimates from bottom-trawl surveys, but observations using midwater trawling and acoustic methods emphasized inconsistencies in these estimates between bottom-trawl and acoustic methodologies and potentially deficiencies in the estimated population trends. In a series of papers by Stockwell et al. and Yule et al. (Stockwell et al. 2006, 2007, 2009, 2010; Yule et al. 2007, 2008), the quantitative process of evaluating different trawl gear and acoustic instrumentation for detecting and enumerating pelagic fish species in the Laurentian Great Lakes and then combining these data streams is exemplified. The process began with quantifying differences in estimates between bottom-trawl and acoustic methods, continued with studies and experiments comparing and assessing the sampling methodologies, and eventually concluded with the development of sampling and analytical techniques to combine the seemingly disparate data streams in ways to effectively sample the ecosystem and improve understanding of trophic interactions and management strategies.

Conversely, the eastern Bering Sea walleye pollock (*Gadus chalcogrammus*) stock abundance is estimated primarily with acoustic midwater trawl survey data, but a separate, fisheries-independent bottom-trawl survey can have substantive catches of walleye pollock, suggesting that some proportion of walleye pollock are located near the seabed. Analogous to the Great Lakes example, research on walleye pollock focused on how environmental factors affect pollock catch in bottom trawls (Kotwicki et al. 2005, 2009) and how to effectively combine bottom-trawl and acoustic data (von Szalay et al. 2007; Honkalehto et al. 2011; Lauffenburger et al. 2016) for improved population trend estimates of pollock. In the walleye pollock case, there is a direct connection to the population models used for generating time series and stock assessments. Unfortunately, this link is missing in the Great Lakes case, which highlights the need for continued development of strategies and tactics to incorporate multiple data streams in stock assessment.

Summary

There is a growing list of studies that question the viability of the trophic-cascade hypothesis in the marine environment (e.g., Grubbs et al. 2016; Sinclair et al. 2015) and those that refute the arguments of Frank et al., providing evidence against top-down forcing by cod on adult snow crab (Chabot et al. 2008; Émond et al. 2015), showing the lack of evidence for pelagic fish affecting the recovery of cod (Swain and Mohn 2012), presenting evidence for bottom-up control of phytoplankton by herbivorous zooplankton (Head and Pepin 2010; Head and Sameoto 2007), and even contesting the methodology used to detect the ESS trophic cascade (Pershing et al. 2015). We are unaware of any studies providing independent support of the PO hypothesis, although many studies have relied on its conclusions.

The fundamental issue that concerns us is whether bottom-trawl catch rates are an unbiased measure of pelagic fish abundance over time, and not whether the evidence against this assumption is unequivocal, although we suggest that it is. Our conclusion is that there is no evidence that supports the assumption that pelagic BTIs are unbiased and plenty of evidence, both direct and indirect, suggesting that they are. Given the evidence, we believe that it is not reasonable to assume that the q of research bottom-trawl surveys have not changed for pelagic fishes, especially as there is no evidence supporting the assumption that they have not. Even Frank et al. presented evidence via their DVM index that indeed q had changed between the pre- and post-collapse periods, showing that the relative proportion of catches had increased in the daytime through a behavioural change. Further, Sinclair et al. (2015) showed that the bottom-trawl survey catchability did change for the adjacent WSS herring stock, and when this “ q correction” was applied to the ESS BTI index, ESS herring showed no increase in abundance over the time series, but indeed a decrease.

In conclusion, studying and monitoring ecosystems and developing an ecosystem-based approach to management will require sampling at multiple trophic levels, which can only be achieved with multiple sampling tools. These tools need to be selected judiciously and the data need to be used in an objective manner in which the strengths and limitations are clearly identified and ideally quantified so that data analyses can be applied in unprejudiced ways. When multiple tools can be used to sample the same trophic levels or ecosystem components, the tools should provide data that are “independent” of each other such that collecting measurements with one tool has minimal influence on measurements from the other. This provides multiple perspectives on the ecosystem that increases confidence in the methods when the measurements are similar but, potentially more importantly, can highlight deficiencies in sampling and can set limits on how the data are interpreted. Many perspectives are needed to provide an

unbiased interpretation of the data and to gain a clear picture of the ecosystem.

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