ICES Journal of
Marine Science

ICES Journal of Marine Science (2014), 71(8), 2300–2306. doi:10.1093/icesjms/fst215

Contribution to the Special Issue: 'Commemorating 100 years since Hjort's 1914 treatise on fluctuations in the great fisheries of northern Europe'

Food for Thought

Hazard warning: model misuse ahead

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Dickey-Collas, M., Payne, M. R., Trenkel, V. M., and Nash, R. D. M. Hazard warning: model misuse ahead. – ICES Journal of Marine Science, 71: 2300–2306.

Received 30 May 2013; accepted 14 November 2013; advance access publication 9 January 2014.

The use of modelling approaches in marine science, and in particular fisheries science, is explored. We highlight that the choice of model used for an analysis should account for the question being posed or the context of the management problem. We examine a model-classification scheme based on Richard Levins' 1966 work suggesting that models can only achieve two of three desirable model attributes: realism, precision, and generality. Model creation, therefore, requires trading-off of one of these attributes in favour of the other two: however, this is often in conflict with the desires of end-users (i.e. mangers or policy developers). The combination of attributes leads to models that are considered to have empirical, mechanistic, or analytical characteristics, but not a combination of them. In fisheries science, many examples can be found of models with these characteristics. However, we suggest that models or techniques are often employed without consideration of their limitations, such as projecting into unknown space without generalism, or fitting empirical models and inferring causality. We suggest that the idea of trade-offs and limitations in modelling be considered as an essential first step in assessing the utility of a model in the context of knowledge for decision-making in management.

Keywords: climate, fisheries, GAM, management, prediction, projection, recruitment, time-series analysis.

Introduction

Models are a key tool to build understanding and provide insight in our exploration of the marine ecosystem. The ambition to gain understanding is in part stimulated by our inherent curiosity and in part by our societies' need to manage human impacts. Hjort (1914) typified both of these ambitions; the urge to improve scientific understanding and the need to understand the dynamics of fish stocks to improve the yield of fisheries. A century later, we are still being challenged to understand the "drivers" of marine productivity and thereby inform the management of human impact and ensure both sustainable exploitation and conservation of our seas and oceans. Models can provide the information base for the ecosystem dynamics and human activities and also inform us about the likely consequences of our actions.

In their influential article, Walters and Collie (1988) argued that the evidence base in fisheries was not being fully utilized as a result of an over-reliance on correlative and biological process studies. They dwelt on the impact of spurious correlations that misdirect research and suggested that a lack of experimental control confounds our research, especially when basic statistical precautions are ignored. Myers (1998) continued the theme, showing that most reported correlations between recruitment and environmental explanatory variables did not hold once retested on a longer time-series. Similarly, Ulltang (1998) highlighted the drift away from incorporating biological knowledge into stock assessments in favour of a focus that is solely statistical in its nature. We felt motivated to write this manuscript as we feel that despite the warnings of Walters and Collie (1988), Myers (1998), and Ulltang (1998), much of the marine science community still seems to assume that a correlative relationship provides evidence for causality, which can then be used in advising management. This principle of "covariance over time demonstrates causality and thus can inform management" is being used to push inappropriate modelling approaches to increase our scientific understanding and inform management. Such an approach is clearly flawed: taken to its extreme, it would encourage the widespread consumption of chocolate to increase society's cognitive powers (Messerli, 2012; Figure 1). The central issue, however, is not that the over-reliance on correlative analysis is bad practice, but that models should be appropriate for the question that is being posed.

The question of scale, for example, is central to the appropriate application of a model (Hastings, 2010). All processes can be considered linear over a sufficiently small scale, i.e. any non-linear function can be approximated by piecewise linear regression (Seber and Wild, 2003). However, beyond that characteristic scale, deviations from linearity start to become important and must be considered in the core structure of the model. This property will be further compounded when the dynamics of the system occur at broader scales than that for which we have data (Levin, 1992), e.g. short time-series, or when proposing to project beyond the known range of states (Carpenter, 2002). However, both scientific curiosity and management applications often require extrapolation beyond the known space (such as new regimes or different scales). Obvious examples of this in the fisheries context are the projection of stock recoveries, future recruitment in new regimes, losses due to predator–prey interactions and exploring future climate scenarios.

Importantly, modelling for knowledge building is often substantially different from modelling for management advice (Levin, 1992; Starfield, 1997). The former provides information on function alone while the later must provide information for action, based on our understanding of the underlying processes. Thus, the context of the modelling is different, as ideally the model should be considered as an entity within the management framework. This is further explored by Rose and Cowan (2003) who explore six lessons for ecologists from fisheries management.

The use of frameworks, within which models are imbedded, is commonplace in many disciplines. In assessing climate change impacts, vulnerability and adaptation in human populations, for example, Klein and Juhola (2013) adapt previously presented complex "frameworks" into a framework based on a series of clearly identified principal issues and approaches. The modelling is focused specifically on the issues identified at the outset.

In this manuscript, we explore the appropriate choices about the utility of models in various manifestations within a framework for

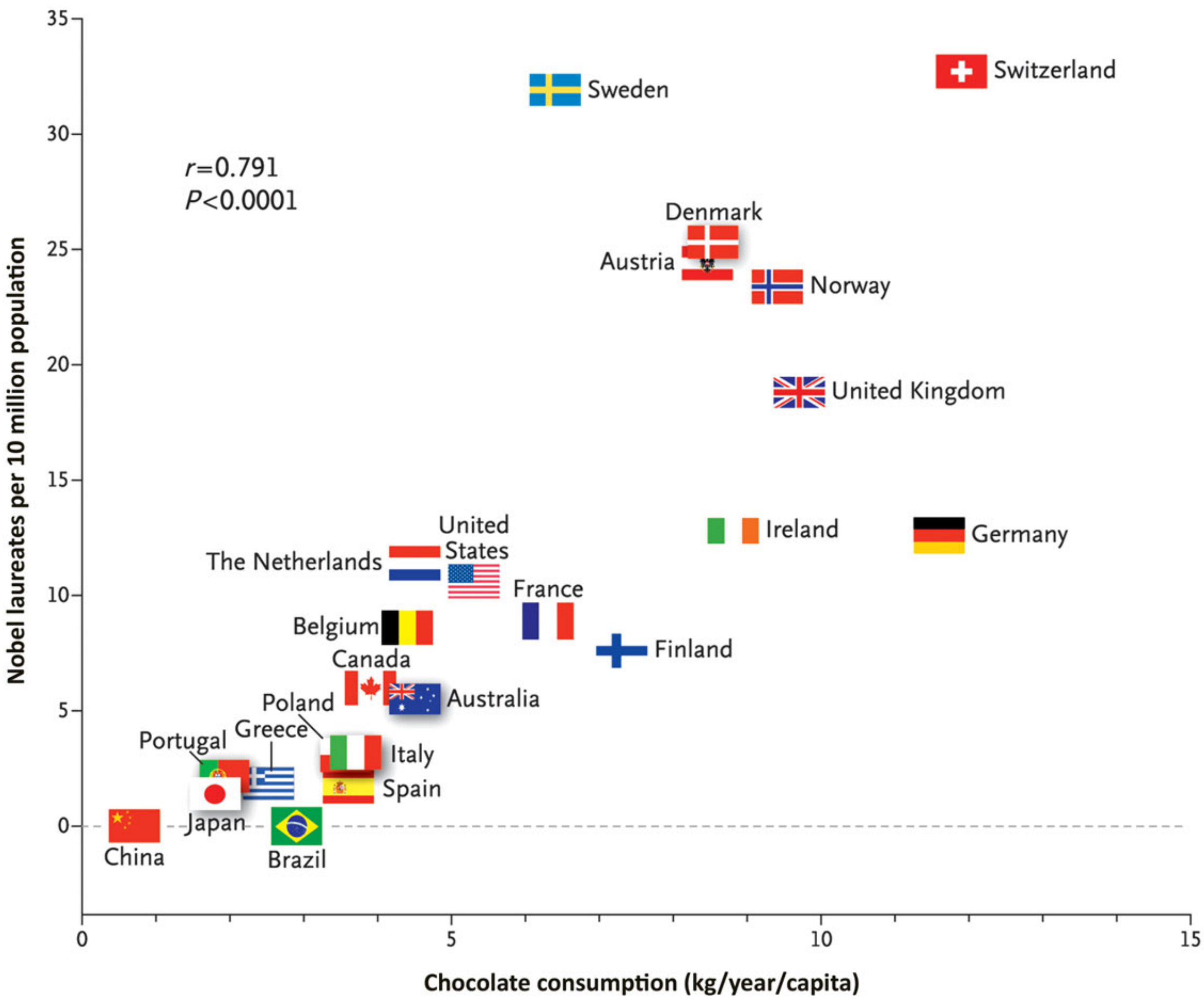

Figure 1. Taken from Messerli (2012). Correlation between countries' annual per capita chocolate consumption and the number of Nobel laureates per 10 million population. Copyright "The New England Journal of Medicine".

providing management advice. We focus primarily on applied marine science and the areas of interest to Johan Hjort. We highlight the traps that commonly ensnare researchers. We propose a framework enabling researchers to consider the nature of the model within the application of the research or management question. We demonstrate this framework with some relevant examples.

Selecting the modelling approach

No model is, or can be, a perfect representation of nature. Models, both in the mathematical and conceptual usage of the word, express the human understanding of the subject under study and thereby reduce complexity to a manageable and accessible form. In the process of creating a model, a reduced fidelity to "truth" is the price paid for simplification. All models are therefore "wrong", to paraphrase Box and Draper (1987), so the question becomes "how useful are they?" This key question, we propose, can only be answered in the context of the application for which the model is intended.

One way to approach the question is by recognizing the existence of the trade-offs inherent in modelling. In the introduction to his 1966 paper, "The Strategy of Model Building in Population Biology", Richard Levins proposed that a model can be characterized in terms of three desiderata: "realism", "precision", and "generality" (Levins, 1966). Generality refers to the ability of the model to represent multiple situations and therefore implicitly includes the ability to extrapolate beyond the domain in which the model was developed (Levins, 1993). Precision refers to the degree of exactness of the measurements or predictions and incorporates the statistical meaning of the word (spread about the mean; Levins, 1993). Reality refers to the number of underlying processes giving rise to the observations that are incorporated into the model (Sharpe, 1990; Korzukhin *et al.*, 1996). Levins further proposed that any given model can only maximize two of these three attributes. Model formulation therefore includes a trade-off of one of these attributes in favour of the other two.

These trade-offs can be used as the basis of a trichotomous model classification system (Levins, 1966; Guisan and Zimmermann, 2000). The "empirical" models (Type I) focus on statistical descriptions of relationships in a precise and realistic manner, but in doing so sacrifice generality. As Levins notes, "this is the approach ... of many fishery biologists" (Levins, 1966) and examples include time-series (e.g. Gröger and Fogarty, 2011) and other statistical-based (e.g. Cardinale *et al.*, 2009) approaches to recruitment studies, particularly those incorporating large-scale climatic indices (e.g. Stenseth *et al.*, 2002). The second class of models (Type II) are the so-called "analytical" models, where theoretical processes (the "laws" of science) are expressed and solved in terms of mathematics: such models are by their nature general and make precise predictions, but by their abstracted and simplified nature do not represent the full complexity of reality. Examples from marine science include size-spectra models (e.g. Andersen and Beyer, 2006), Lotka–Volterra predator–prey dynamics (Wangersky, 1978), and analyses based on the optimization of Darwinian fitness (e.g. Visser and Fiksen, 2013). Finally, "mechanistic" models (Type III) are process-based and integrate the individual processes at one scale up to a higher scale: examples include bioenergetic and individual-based models (e.g. Strand *et al.*, 2005), end-to-end models (Rose *et al.*, 2010), oceanographic circulation models and IPCC-class climate models. Such models tend to be general in their nature and contain realistic representations of their systems, but may not necessarily be precise.

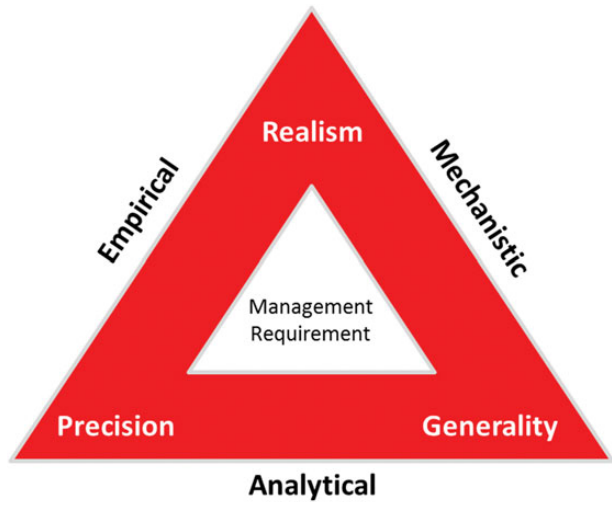

Figure 2. The trichotomous model classification scheme based on Levins (1966) and adapted from Guisan and Zimmermann (2000) and Sharpe (1990). Models are assumed to have two of the three attributes and can be considered empirical, mechanistic, or analytical. Management requirements, however, often lie at the intersection of these attributes, an area which Levins (1966) proposes to be inaccessible.

Based on this trichotomy, the trade-offs inherent in any modelling problem can be visualized in the form of a triangle (Figure 2; Guisan and Zimmermann, 2000). Each vertex of the triangle represents one of the model attributes (realism, precision, generality), and each edge therefore represents one of the model classifications given above. Following the classification of Levins (1966) and Guisan and Zimmermann (2000), model formulations can only exist along the edges of the triangle: the middle of the triangle, where a model combines all three attributes, is inaccessible in the real world. However, it is in this inaccessible area where the expectations of managers often lie.

The Levins trichotomy is particularly attractive in this context as it encapsulates several concepts that we have already touched upon. In particular, causality is closely associated with the generality axis: if the causal mechanisms and processes underlying the question at hand are understood, then generalizing to other situations is possible (Levin, 1992). The mechanistic and analytical classes explicitly incorporate our understanding of causality into their models and therefore use it to achieve generality. In contrast, the empirical models either ignore causal mechanisms or attempt to infer them from correlation: in doing so, they sacrifice generality. Similarly, the use of direct predictors (e.g. food abundance) is common in mechanistic models, whereas indirect predictors (e.g. NAO, AMO, and other large-scale climatic indices) are more common in empirical models.

Levins' work has been highly influential and not without controversy. The paper has been cited more than 580 times (Web of Science, October 2013), including both strong criticisms (Orzack and Sober, 1993; Orzack, 2005, 2012) and robust defences (Levins, 1993; Odenbaugh, 2003) of the work (see references in Orzack, 2012, for a broader overview). Much of this debate has been plagued by semantic differences: Levins did not explicitly define realism, precision, and generality in his original paper, as he believed them to be self-evident [the definitions that we use here are from

Sharpe (1990), Levins (1993), and particularly Guisan and Zimmermann (2000)] but there are also legitimate criticisms of the thesis (see particularly Orzack and Sober, 1993).

Similarly, the model-classification scheme is, of course, imperfect. It can be difficult at times to fit a given model into the classification (Korzukhin *et al.*, 1996). Examples can readily be found that blur the distinctions between the attributes, e.g. the Ricker and Beverton–Holt stock–recruitment relationships have an "analytical" origin (Ricker, 1954; Beverton and Holt, 1957): however, the modern application, especially when modified to include environmental variables (e.g. Mantzouni *et al.*, 2010), is essentially "empirical" in nature. Levins himself, however, did recognize this fluidity (Levins, 1993), describing it as a "delightful" feature, and used it to illustrate that the nature of the model is inseparably linked to its application.

Nevertheless, despite its weaknesses, we assert that the essence of Levins' argument is sound and that it represents a useful way to think about modelling. It is self-evident that no model can be all things to all people. Modelling involves a simplification of the "truth" to make it comprehendible and manageable, and there will naturally arise a bias, conscious or not, towards one aspect of model performance over another (i.e. a trade-off) as a result of this simplification. The important point is that there is a trade-off involved in all modelling. The Levins framework is imperfect and controversial ("wrong"), but, for the sake of the discussions here, we still believe it to be useful.

Creating models

Certain schools in marine fisheries science appear wedded to their techniques and there are fashions in the application of methods and approaches (Johnson and Omland, 2004). This often results in the application of "pet techniques", rather than careful consideration of the appropriate type of model, especially when considering the application to management needs. We argue that the appropriateness of a model is intimately linked to the question that is being posed [a point also stressed by Starfield (1997) for the related field of wildlife management and, as noted above, also by Levins (1993)] and that a model cannot be separated from its intended application. We see three primary applications to which models are put in marine fisheries science:

(i) Knowledge acquisition: the process of trying to understand the characteristics of the system.

(ii) State estimation: attempting to determine the state of the system based on available observations.

(iii) Extrapolation: the use of existing knowledge and/or observations to make statements about scenarios beyond the bounds of the known domain.

It is a combination of 2 and 3 which are used in a management situation, i.e. estimates of the current and predictions of future states of the resource either within or beyond the bounds of the known domain.

Given these types of applications, the Levins trichotomy can be used to identify the model class that should be used to answer these questions. As an example, we consider how the data available to a fish-stock assessment working group could be analysed. If the task was the evaluation of the abundance of a fish stock (e.g. for the generation of management advice), this is a state estimation problem, where accuracy and precision are more important than generality: an empirical model (e.g. time-series model) would therefore be appropriate. However, an analysis of the same dataset to infer the processes influencing recruitment (a classic knowledge acquisition exercise) places value on realism and generality and therefore requires a mechanistic modelling approach. Alternatively, questions about the long-term impacts of climate change (e.g. under various warming scenarios) involve an extrapolation beyond the known range of conditions, and therefore, generality is critical: in this case, mechanistic or analytical models are the most appropriate. The choice of model is therefore determined, in the first instance, by the question that is being posed and not by the datasets or modelling platforms available. System structure and data availability come in the second instance.

As the model and the application need to be considered together, a model can therefore only be judged in the context of its application (Sharpe, 1990; Levins, 1993; Starfield, 1997; Guisan and Zimmermann, 2000). Applying a model in a context outside for which it was designed ultimately risks a mismatch between the (fixed) trade-offs incorporated in the model, and the (shifted) application to which it is put (Levin, 1992). Such mismatches may be the result of a series of active decisions ("mission creep"), or alternatively, they may result from poor model formulation in the first place. In both cases, the outcome is the same: degraded model performance and potentially erroneous conclusions and management advice.

Using models

Unfortunately, the modern application and use of models in applied marine science often does not reflect the pragmatism and common sense encapsulated by Levins (1966). Problems can be identified in both the expectations of the end-users, and the results delivered by the scientific community.

The basic precept of the Levins trichotomy is that the centre of the triangle (Figure 2), where precision, reality, and generality are maximized, is inaccessible. Requests for scientific advice about management options, however, often assume that scientists can deliver all three characteristics; a typical example might be recruitment projections or future distributions and interactions of fish under climate change that could be used to inform harvest strategies. Such expectations are, according to Levins, unrealistic. Of course, the expectations of managers can be made more realistic through a constructive dialogue and iterative process of developing the applied science questions. Many of us though are finding that the work load of managers is too great or that the turnover of managers is too fast to build a constructive working conversation.

The scientific community, however, all too often overlooks the limitations of its work in satisfying such requests. For example, "data mining", with its mantra of "let the data speak" and where the results lead to *post hoc* hypotheses generation, is a classic example of the answer shaping the question. Such empirically derived models are often then used to project outside the known space, as in climate scenarios or projections of recruitment dynamics. Similarly, where two species co-vary in abundance over time, researchers often assume some causal mechanism or some interaction (e.g. sardine and anchovy off California or cod and herring abundance). This simplistic conclusion is often undermined, or at least made more complex when a longer or different time frame is considered or mechanistic or analytical approaches explore the space (Barange *et al.*, 2009; Finney *et al.*, 2010; Speirs *et al.*, 2010; Denderen and Kooten, in press; Hosack *et al.*, 2013). Medium-term (5–10 years) fisheries advice is offered, accounting for interacting species and fleet dynamics, based on the empirically assessed trends in the recent past. This is although human behaviour changes and often finds novel solutions to management restrictions (Rijnsdorp *et al*., 2008; Fulton *et al*., 2011). Recruitment strength can also change rapidly.

There are many challenges when trying to interpret time-series analyses, especially when considering the implications for management action. We feel that this is not strengthened by the *post hoc* use of time-lags, and a failure to consider the appropriate length of the series in relation to the frequency of the signal. Similarly, some theoretical concepts, which have not or cannot be rigorously tested through empirical hypothesis testing (e.g. fisheries-induced evolution, balanced fishing, etc.), have been concluded by induction for specific fisheries without regard to the generality of the approaches.

Furthermore, the willingness to deliver and the unquestioning usage of "pet models" not only leads to inappropriate model choice, but often results in researchers ignoring the particular system structure as well as properties of the data or model parameters. Where some of this comes to the forefront is a researcher with a favoured suite of models or a specific modelling approach that searches for datasets and applies the favoured methodology without due consideration for the question, system and data structure, properties, or implications of the model fits.

Taking heed of some best practice rules and approaches should help to avoid at least some of the pitfalls associated with using models. When fitting empirical statistical models, a basic issue is to ensure that the assumptions underlying the subsequent model fitting are not violated and hence inference can be drawn; carrying out model fitting and selection at the same time causes problems for inference (Chatfield, 1995). A major concern with implementing analytical and mechanistic models is model validation or determination of model skill. Generally, there is no model selection, and thus inference, on process structure (other than via parameter values), though there are exceptions (e.g. Sugihara *et al*., 2012). Ecological inference (or biological inference), i.e. deriving knowledge regarding processes that occurred in the past, based on model selection or comparison is subject to a number of statistical pitfalls. Structure in process errors, such as autocorrelation in space and time will degrade the power of statistical tests, as will structure in measurement (observation) errors. A well-known but often neglected aspect is the measurement error in explanatory variables of regression-type methods—the "errors-in-x" problem (Davies and Hutton, 1975). For example, errors in spawning stock estimates can go as far as masking stock–recruit relationships if they are not accounted for appropriately (Walters and Ludwig, 1981).

Discussion

We propose that the appropriateness of the modelling approach be considered as a first step in the assessment of the utility of models in the context of knowledge for decision making in management. In ICES, the only quality standards for the application of models in management are the peer review process and the lack of strong opposition (rather than consensus). Resources and attention are limited, and this leads to many models being applied inappropriately: fisheries science, however, is not alone in making this mistake, e.g. wildlife management (Starfield, 1997). Society has chosen that fisheries be managed using evidence-based policies. Pragmatic choices, such as selecting a model that is appropriate for the question, are part of the trade-off required to provide advice in an operational manner.

However, we feel that the drive to reanalyse existing datasets (without new data collection or new process investigations) and the widespread ease of use of statistics packages, without sufficient conceptual understanding, is pushing fisheries science into some very uncomfortable corners. In the fisheries science community, the ability to apply state-of-the-art analytical techniques is often seen as a greater skill than investigating ecological or population dynamics (Orr, 1996; Rose, 1997). The use of advanced statistics does not circumvent the basic fact that linear covariation (correlation) does not necessarily equal causality. Often the models applied have many underlying constraints that, when used by the unwary, are readily violated. There is, unfortunately, insufficient education of marine scientists in statistical techniques or alternately, there is insufficient collaboration between well-trained and knowledgeable statisticians and ecologists. The drive of certain schools to push their "pet" model also reduces the space for considering the appropriateness of model choice and application. Hjort himself was not totally above this tendency as he was a proponent of the newly implemented sigmoidal relationship (Hardy, 1950). Looking beyond fisheries science, the increasing analysis of human activities leading to pressures that impact the state of the marine environment and the linearity assumptions associated with certain integrated ecosystem assessment approaches (Samhouri *et al*., 2010; Jennings and Le Quesne, 2012) pose similar challenges and place further demands for knowledge of the research community.

Hjort was very much a practical scientist working with the fishing industry to open up new fishing grounds and concerned about the welfare of fishers (Hardy, 1950). In addition, he applied methods for population statistics applicable for accident insurance for fishers to fish stocks and his seminal paper (Hjort, 1914) identified relationships between abundances and environmental factors along with structural aspects of the populations (intraspecific factors). Statistics at this time was in its infancy and therefore many of the tools that we take for granted today [e.g. Student's *t*-test (1908), ANOVA (1920)] were simply not available. Instead, Hjort dealt primarily with "conceptual models" which formed the basis of deductions. In a subsequent paper (Hjort, 1926), he revisited his "model predictions" and "tested the theories" that he put forward in 1914 by observing what did occur. It was only later that modern statistical techniques and models could be utilized to mathematically "model" relationships between stock metrics and drivers of change in the stock. Thus, there has been a significant paradigm shift in the way that stock fluctuations are predicted: we doubt, for example, that Hjort would have countenanced the projection of fish catches 50–100 years in the future as a way to inform current decision-making. Within the framework of the Levins trichotomy, Hjort's work was essentially conceptualising empirical knowledge. In the absence of a mathematical basis, his work tended to search for mechanistic understanding by using the support of empirical evidence. The objective was to derive some semblance of realism in short-term projections.

Since Hjort (1914), the fish stocks of the world have been heavily exploited. In addition, there is clear evidence that the environment is undergoing a rapid change due to anthropogenic impacts, i.e. climate change, coastal reclamation and construction, etc. Rather than considering variability in future catches alone, as in Hjort's time, we must now consider the wider stewardship of the marine system, and we must acknowledge that the system is always in flux and that ecosystems regularly change. Also, rather than documenting the change in populations and simply understanding the dynamics the emphasis now is on models which have the ability to encapsulate the current knowledge of the principal drivers of fish stock dynamics and the prediction of short (years to decades) and long-term (century) trends in abundance.

In this article, we provide a warning that often a sense of realism about the limitations of our work needs to prevail. Some could say that we are just stating the obvious, or just regurgitating the process for good scientific practice. However, in our opinion, there are too many examples of the misuse of models which claim to provide information to inform management decisions. The message therefore needs to be repeated: the structure of a model must take its application and utilization into account and the model must be fitted using best practice methods (by this we definitely do not mean easiest or most familiar methods). Conversely, a model cannot be separated from its intended application. We have not highlighted a "hall of shame" by identifying specific studies but instead urge the research community to take a step back from their technique-driven approaches to problem solving and ask themselves what is the research question or management problem and what is the most appropriate way to address the challenge. We have not in this "food for thought" moved into the discussion of using multiple models, as widely advocated in multidisciplinary fields such as climate analysis. However, we would encourage all to look at the developments stemming from Schneider and Dickinson (1974) when they encouraged researchers to think across methods. As alluded to above, managers expect that the scientific community can deliver models that are realistic, precise, and general. The onus therefore lies on scientists to be honest about the limitations of our models and thereby ensure that the expectations of managers are pragmatic and not utopian. Clearly dialogue between policy developers, scientists, and stakeholders is needed when shaping research questions. Our management challenges are far more complex than those raised a century ago and require far more sophisticated analytical tools. In Hjort's time, they used the available tools wisely and within their limits: can the same be said of the present? The lessons of Hjort's work therefore mirror the implicit message of Levins: that we must put the focus on the research question and its context first and consider the data or knowledge base available to answer that question, and the tools to be used in doing so, second.

Acknowledgements

We thank the handling editor, our reviewers, and Nigel Yoccoz for their comments which led to a greatly improved manuscript.

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Handling editor: Gary Griffith