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**Proceedings of a Meeting on  
Scallop Production Area (SPA) 4 Scallops**

**Regional Advisory Process  
Maritimes Region**

**13 September 2000  
Polaris Boardroom  
Bedford Institute of Oceanography**

R. O'Boyle, Chairperson  
Office of the Regional Advisory Process  
Bedford Institute of Oceanography  
P.O. Box 1006  
Dartmouth, Nova Scotia  
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**January 2001**

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**Canada**

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### **Foreword**

The purpose of this proceedings is to archive the activities and discussions of the meeting, including research recommendations, uncertainties, and to provide a place to formally archive official minority opinions. As such, interpretations and opinions presented in this report may be factually incorrect or mis-leading, but are included to record as faithfully as possible what transpired at the meeting. No statements are to be taken as reflecting the consensus of the meeting unless they are clearly identified as such. Moreover, additional information and further review may result in a change of decision where tentative agreement had been reached. Therefore, only the Stock Status Report(s), which contain the consensus decisions of the meeting, should be used as sources of information on the status of the resource assessed. Additionally, any summary on the stock status presented in this proceedings should not be referenced. The Stock Status Reports are supported by Research Documents which will be finalized from the working papers presented at the meeting.

### **Avant-propos**

Le présent compte rendu fait état des activités et des discussions qui ont eu lieu à la réunion, notamment en ce qui concerne les recommandations de recherche et les incertitudes; il sert aussi à consigner en bonne et due forme les opinions minoritaires officielles. Les interprétations et opinions qui y sont présentées peuvent être incorrectes sur le plan des faits ou trompeuses, mais elles sont intégrées au document pour que celui-ci reflète le plus fidèlement possible ce qui s'est dit à la réunion. Aucune déclaration ne doit être considérée comme une expression du consensus des participants, sauf s'il est clairement indiqué qu'elle l'est effectivement. En outre, des renseignements supplémentaires et un plus ample examen peuvent avoir pour effet de modifier une décision qui avait fait l'objet d'un accord préliminaire. Par conséquent, ce sont uniquement les Rapports sur l'état des stocks, reflétant les décisions consensuelles prises à la réunion, qui doivent être les sources de renseignements au sujet de l'état des ressources évaluées. Les brefs sommaires de rapport sur l'état des stocks présentés dans le présent compte rendu ne doivent pas non plus être considérés comme des textes de référence. Les Rapports sur l'état des stocks sont appuyés par les Documents de recherche, qui seront établis définitivement à partir des documents de travail présentés à la réunion.

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**ABSTRACT**

A meeting of the Maritimes Regional Advisory Process was held on 13 September 2000 at the Bedford Institute of Oceanography to review the status of the Scallop Production Area (SPA) 4 scallop stock and provide harvest advice for the 2000 fall fishing season. A new population model was reviewed and was used as the basis for the harvest advice. A Stock Status Report and associated Canadian Stock Assessment Secretariat Research Document were produced.

**RÉSUMÉ**

Dans le cadre du Processus consultatif régional des Maritimes, on a tenu une réunion le 13 septembre 2000, à l'Institut océanographique de Bedford, pour examiner l'état du stock de pétoncle de la Zone de production de pétoncle (ZPP) 4 et formuler un avis sur la pêche pour la saison de l'automne 2000. Cet avis a été fondé sur l'examen d'une nouvelle modèle de population. Un Rapport sur l'état des stocks et un Document de recherche connexe du Secrétariat canadien pour l'évaluation des stocks ont été produits.

## INTRODUCTION

A meeting of the Maritimes Regional Advisory Process was held at the Bedford Institute of Oceanography on 13 September 2000 to review the status of the SPA 4 scallop stock and to provide harvest advice for the fall 2000 fishing season. The list of attendees is given in Appendix 1 while the meeting remit is given in Appendix 2. R. Claytor, E. Kenchington, R. Mohn and D. Swain were designated as external reviewers for the meeting and received a copy of the working paper by S. Smith and M. Lundy prior to the meeting. Both E. Kenchington and D. Swain prepared written reviews of the working paper (Appendix 3), which were discussed at the meeting. Unfortunately, D. Swain could not attend the meeting in person. The meeting was chaired by R. O'Boyle and M. Lundy acted as the rapporteur.

After introductions, it was pointed out that the last assessment of this resource was in November 1999, and was the first time that a population model had been constructed. However, a number of problems were identified in the model and thus it was not used as the basis of management advice. The current analysis attempts to rectify these issues and provide an analytical basis for harvest advice. Specifically, the data base has been edited, standardization of the survey tows conducted, new meat weight information used, and a new model constructed.

Before the review of the analysis, the Chair pointed out that this meeting was a scientific review of an assessment and would not be dealing with allocation issues. Also, it was noted that the discussions of the meeting would be confidential until its results were made public approximately two weeks after the meeting.

The meeting was then turned over to S. Smith, who presented the analysis in the working paper. During the talk, only questions of clarification were taken. Following the presentation, the external reviewers were asked to present their comments, and afterwards a general discussion ensued. The meeting ended with a review of the draft Stock Status Report which had been prepared by S. Smith and M. Lundy.

## STOCK ASSESSMENT REVIEW

Working Paper: Smith, S.J. 2000. Update on the Status of Scallops in SPA 4 for 2000. DFO Maritimes Region Working Paper 2000/42.

Referees: R. Claytor, E. Kenchington, R. Mohn, and D. Swain

Rapporteur: M. Lundy

The reviewers' comments given in Appendix 3 proved useful in focusing the meeting on the main issues of the analysis. The comments below highlight additional issues raised.

A number of questions were raised on the impact of changes to the data. It was asked what was the relative impact in change of data when comparing the 1999 model and the 2000 model. The table on Page 7 of the working paper gives these impacts. It was pointed out that the impact of using the new model, as compared to the 1999 model, should be shown, i.e 1999 approach redone on the 2000 data. This should be included in the research document.

A description of the changes to the tow length calculation was requested. While this was described, it should also be included in the research document.

It was suggested that another graph accompany Figure 14, showing the straight survey versus CPUE and not the model.

Figure 33 provides the frequency of clappers. What is  $M(0.1)$ ? Is this consistent to 0.1? This could not be answered but seemed to be generally consistent.

Could a possible total mortality ( $Z$ ) be inferred from figures 11 and 12, which could be compared to the  $F$  &  $M$ . They could be in future assessments.

It was suggested that the retention curves for unlined and lined gear be shown as well as growth curves for shallow and deep water areas for information.

Figure 15 showed a peak in recruits but not in prerecruits. This could be a function of the knife edge size for greater than and less than 80 mm. It was noted that the CPUE is not always coming from the same spatial fishing area, i.e. 1995-96 for 6- 8 mile area, 1997 eastern area of SPA. It should be clarified what data spatially is used for CPUE calculation. It was also stated the catchability for each model should be shown. The implications of using the same estimate for the lined and unlined gear was queried.

It was suggested that the actual weights instead of regression values be used. It was noted that we only have actual weights for 1996-2000.

Regarding the model formulation, it was suggested that partitioning for half-year mortality be attempted, given the additional information available.

It was queried whether or not  $F_{0.1}$  was appropriate for this fishery. What are the definitions of overfishing, targets, thresholds, etc.? These are the topic of future RAPs.

It was stated that we should show the effect, if any, on using different size definitions for fully recruited individuals: 80+mm for 1991-95 and 95+mm for 1996-99. Does  $F_{0.1}$  relate the same for both definitions? We should define the size ranges for  $F_{0.1}$  calculations. It was suggested that two separate analysis for the deep and shallow areas be done and possibly change the stratification for depth. This is for future analyses.

It was suggested that a breakdown of effort for early years be shown, however, it was stated that a comparison of current effort with the early 1990s cannot really be done due to the source methods being different (early years had a very low fishing log compliance).

It was noted that the 1998 landings are questionable and are probably an underestimate due to misreporting of catch to adjacent SPA 1.

There was a general consensus that the model used may be a useful tool and is summarized well in figures 34-36. In conclusion, a fishing mortality of 0.19 with some variation coupled with recent effort (9000 hours) suggests a possible catch of 88 t for the area. Any partial closures of the area would need to be coupled with a decrease in the suggested TAC. Dealing with the significant recruitment seen during the 2000 survey was felt to be a management, not science, issue. Discussion of the level of fishing mortality on this large year-class followed. There is presently no data on the level of incidental fishing mortality. Common sense suggests that modifications to the gear presently used would lessen impact, i.e. change to steel washers, removal of tail pole, use of lighter tow bar, seasonal closures (Jan/Feb). This discussion was not pursued further and felt better discussed at the Inshore Scallop Advisory Committee meeting.

Overall, there was a consensus that the changes to the data and the model were appropriate and provided the basis for the fall 2000 harvest advice.



## Appendix 1. List of Participants

<u>Participant</u>	<u>Affiliation/Address</u>	<u>Telephone</u>	<u>Fax</u>	<u>E-mail</u>
Maureen Butler	DFO, Invertebrates, BIO	(902) 426-5342	(902) 426-1862	Butterm@mar.dfo-mpo.gc.ca
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Stephen J. Smith	DFO, Invertebrates, BIO	(902) 426-3317	(902) 426-1862	Smithsj@mar.dfo-mpo.gc.ca
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**Appendix 2. Meeting Remit****Resource Status**

- Provide historical trends in abundance, biomass and exploitation, and the uncertainty in these trends. Specifically review the Collie/Sissenwine model application to the data for the resource as a basis of assessment.
- Provide fall 2000 status stock indicators, along with uncertainty, in relation to commonly used limit and target reference points, e.g.  $F_{0.1}$  and  $SSB_{min}$ .

**Management Considerations**

- Consider the development of indicators and associated reference points that can be used to monitor stock performance.

**Outlook**

- Provide harvest advice for the 2000 fishery.

### Appendix 3. Reviewers written comments

#### *Comments by E. Kenchington*

The document is largely focussed on a population model first used by the authors in the 1999 assessment and further developed here. My principal concern over this model is that mortality (or technically survival) is not applied until after the fishery and recruitment has been factored in. This would be a reasonable assumption if the fishery was very short and took place immediately after the survey. However, the fishery occurs over a couple of months three to four months after the survey. Mortality should be calculated on the numbers at intervals (four?) throughout the year. Further, the use of  $M=0.1$  is not consistent with the most recent evaluation of  $M$  for this area (Kenchington et al. 1995), and it is not reasonable to assume that the value is the same for prerecruit and recruited scallops, given the high predation rates on the smaller sizes.

Further, knife-edge recruitment is a questionable assumption when recruitment to the gear is the only factor (cf. Kenchington et al. 1995). However when you consider that there is no enforcement of shell height, that there is a blended meat count allowing for small scallops to be harvested, and the minimum shell height is at least 20 mm greater than it was previously requiring small scallops to be thrown back, this assumption is undoubtedly simplistic. The authors should discuss the effect this will have on their model predictions.

*New Survey Series:* p.4. More details on how the tows were converted from two to three decimal places are needed including a description of the time series (when were three decimals collected on the GPS vs. when were they inferred through regression or other means?). Were GPS data available for the whole time series of the survey (1990 onwards)? If not how were the tow distances collected by Loran converted? The “thorough” analysis and production of conversion factors must be documented.

*Update for the 2000 Survey:* General: there is some confusion between the graphs and tables supporting this and the next section. The distribution maps for the 55-79mm and > 95 mm should be shown as these categories are referred to later. When is a recruit and recruit? It seems to change throughout the paper. p.4. Is there statistical support for the statement that the mean number per tow for the two size classes represent a slight increase over the previous year? (see also p. 5, para 2). p.5 . A comparison of Figure 4 and Figure 5 does not support the statement that the recruits and the pre-recruits are in the same area. They appear to be in different areas in most strata.

*Evaluation of Population Model:* p.5. How is  $R_{t-1}$  determined? Do you assume that all scallops between 45 and 79 mm will move into the recruited class? Is this a reasonable assumption? The size frequency data from the survey does not indicate a clear mode at this interval suggesting rapid growth (needed to move this broad size class forward). Certainly this would not hold for the scallops in the >90 mm stratum. p.6. What are the values of  $q$  and do they change from year to year? Are the changes reasonable both inter-annually and relative to one another? Why are the error terms not tested for normality? Smith et al. (2001) should read Smith et al. (in press). p.

7. I would put more emphasis on the lamda for the survey data then for the model (reverse of what was presented). The survey seems very good whereas the model has a number of flaws. The table presented in the text is not clearly labelled to show whether biomass is population biomass or survey biomass. The statement that the biomass trends and the commercial CPUE track each other through time (Fig. 14) is not one I would draw from that figure. p.8. the first brackets are unintelligible. p.9. First brackets doubled. p.11. The February survey covers more ground than the previous seasonal study but only provides a second temporal data point. Define the model that you refer to and explain more clearly how you think these data can be useful.

*Discussion:* p.12. On what basis are the results in this paper a more realistic view than the results of the previous year? Explain. The second paragraph introduces new results and should be in the previous section. What is the survey CPUE and how does it compare to the commercial value? It used to be very similar in this area. 3<sup>rd</sup> para: Replace average mean weight with average meat weight.

*Research Idea:* The authors believe that the weakness in the data lies in the port sampling of the catch. As the survey and catch data are available as biomass it would be useful to calculate the population biomass using only weight data. This would eliminate the error introduced in converting the catch in weight to catch in numbers. This biomass could then be converted to numbers using the survey conversion data (vs. average weight used now) and M applied. How disparate are the two values of Nt?

Tables: Consistent use of SPA, Scallop production area, etc.

Table 1: Explain the second column in the title. Provide 1999 data for comparison.

Table 4 and Table 6 should be combined with a separator.

Table 5: Define Recruits and Fully Recruited and correct column headers for each depth

Table 7: No model is presented either in the table or in the text. This is very confusing to me and I don't understand how you have used this information or plan to use it in the future.

Figures: Consistent use of SPA, Scallop production area, scallop production area etc.

Figure 3: Include error bars

Figure 5, 6: Change colour for 500 and 1000 bins as they can't be distinguished. Fig. 5,6, Centreville is masked. Need figures for >95mm, 55-94 mm, 75-94mm?

Figure 9-12. Put line to indicate recruits

Figure 15: Why do the recruit and prerecruit peaks in the >90 m coincide? The prerecruits should be seen the year before. Is this an artifact of the survey?

Figure 30: font is different from others and there is a typo in the size class legend

*Reference:*

Kenchington, E., D. Roddick & M.J. Lundy, 1995. DFO Atl. Fish Res. Doc. 95/10, 70pp.

*Comments by D. Swain*

This document presents a careful and thorough analysis of the survey and fishery data available for scallop in SPA 4. The weak link in the analysis, identified by the authors, is the mean meat weight data for the fishery landings. These weight data are not extensive and appear to be biased in some years, particularly 1998. This results in considerable uncertainty regarding the numbers landed and fishing mortality in some recent years. The authors have explored several alternative analyses and I can offer no suggestions for substantial improvements to their approach. The alternative to using the fishery meat weight samples appears to be to use meat weight/shell height data available from the survey since 1996. As noted by the authors, it may be possible to estimate mean weight in the landings by using these data to map predicted mean weights over the whole area. However, there are many difficulties, and further analyses are needed to determine whether this approach has merit.

Minor points:

1. P. 5, 1<sup>st</sup> Par: This is a bit confusing. In Fig. 5, density of small scallops (<80 mm shell height) is greatest in relatively deep areas of SPA 4 (>40 to over 50 fm), not in the shallower areas, i.e. <40 fm (as indicated in the text: “In SPA 4 these areas correspond to the shallower areas...”). (Though there are few small scallops in the deepest areas of the survey, in the Centreville stratum – maybe that is what is referred to here?)
2. P. 7: I wouldn’t say that survey biomass estimates and fishery CPUE tracked each other well through time (Fig. 14). Survey biomass increased steadily from 1994 to 1998 (to the 2<sup>nd</sup> highest value in the time series) but there is no evidence of this in the CPUE time series, which has remained low since 1994. I gather that part of this divergence can be attributed to problems with the mean meat weight data, which are used to convert population numbers into population biomass. Do changes in the spatial distribution of the scallops, resulting in changes in catchability to the fishery, also contribute to the divergence?
3. Table 5: Column 5 should be titled ‘Fully recruited’, not ‘Recruits’.
4. P. 12, par 2: I don’t follow this. The correspondence between the observed survey index and that predicted by the model in 1998 is not perceptibly different between Fig. 16 and 34 (though the catch numbers are – perhaps this is the difference that the authors are referring to?).