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ᐃᖃᓗᐃᑦ, ᓄᓇᕗᑦ

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ᑖᓐᓇ ᐅᓂᒃᑳᖅᑕᐅᔪᖅ ᐅᖃᐅᓯᖃᖅᐳᖅ ᓄᓇᓂᒃ ᐊᕕᒃᑐᖅᓯᒪᔪᓂ ᖃᐅᔨᒪᔨᒋᐅᔭᐅᔪᑦ ᐱᓕᕆᐊᕆᕙᓪᓕᐊᓂᐊᖅᑕᖏᓐᓂᒃ ᑲᑎᒪᔾᔪᑕᐅᓚᐅᖅᑐᓂ ᑲᖏᖅᓱᐊᓗᑉ ᐃᒪᖓᓂ/ᓴᓂᕋᔭᐅᑉᓗ ᐃᒪᕐᑯᑖᓂ ᐊᕐᕕᖕᓂᒃ ᑲᑎᒪᔾᔪᑎᖃᖅᑎᓪᓗᒋᑦ ᐃᖃᓗᖕᓂ, ᓄᓇᕗᒥ, 17-18 ᔫᓐ 1999-ᒥ. ᑲᑎᒪᔪᑦ ᐅᖃᐅᓯᖃᓚᐅᖅᐳᑦ ᐆᒃᑑᑎᓕᐊᖑᔪᒥᒃ ᖃᓄᐃᓕᖓᓕᕆᐊᒃᓴᖓᓂᒃ ᐊᕐᕕᑦ ᐅᓄᕐᓂᖏᓐᓄᑦ, ᐅᓂᒃᑳᖅᑕᐅᔪᓄᑦ ᐃᑲᔫᑕᐅᔪᑦ ᐃᓚᐅᑎᑕᐅᕗᑦ ᐅᓂᒃᑳᖅᑕᐅᔪᓪᓗ ᐅᓂᒃᑳᖅᑐᓂᑦ ᐃᓚᐅᑎᑕᐅᖃᑕᐅᕗᑦ. ᖃᓄᐃᓕᖓᓕᕆᐊᒃᓴᖏᓐᓂᒃ ᐊᕐᕕᑦ ᐅᖃᐅᓯᖃᕐᕕᐅᓂᖏᑦ ᐆᒃᑑᑕᐅᔪᓂ ᓯᓚᑖᓂᑦ ᖃᐅᔨᓴᖅᑕᐅᓂᑯᐃᑦ ᐃᓚᐅᑎᑕᐅᓚᐅᕗᑦ. ᑲᑎᒪᖃᑕᐅᔪᑦ ᐃᑲᔪᖅᑐᐃᓚᐅᖅᐳᑦ ᒪᕐᕈᖕᓂᒃ ᑲᑎᒪᐅᖅᑐᖕᓂᒃ ᐅᑭᐅᖅᑕᖅᑐᑉ ᐊᕐᕕᖏᓐᓂᒃ. ᐅᐊᖕᓇᖅᐸᓯᖓ ᓴᓂᕋᔭᐅᑉ ᐃᒪᕐᑯᑎᖓᓂ ᓇᓗᓇᐃᖅᑕᐅᓚᐅᖅᐳᖅ ᒫᕋᓛᖅᑖᕈᑕᐅᖃᑦᑕᕐᓂᖓᓂᒃ. ᐃᓄᐃᑦ ᑲᑎᒪᖃᑕᐅᔪᑦ ᐅᖃᓚᐅᖅᐳᑦ ᐅᓄᕐᓂᖏᑦ ᐅᑎᖅᐸᓪᓕᐊᓕᕐᓂᖏᓐᓂᒃ. ᑲᑎᒪᖃᑕᐅᔪᑦ ᐊᖏᕈᑎᖃᓚᐅᖅᐳᑦ ᑕᒪᑐᒥᖓ, ᒪᓕᒡᓗᒋᑦ ᐆᒪᔪᐃᑦ ᑐᕐᑯᑕᐅᔪᑦ ᐱᔭᐅᖃᑦᑕᕐᓂᖏᑦ, ᐊᑕᐅᓯᕐᒥᒃ ᐊᕐᕙᖃᑦᑕᕋᔭᕐᓂᖏᓐᓂᒃ ᐅᑭᐅᒃ ᒪᕐᕈᒃ ᐊᓂᒍᖅᑎᓪᓗᒍ ᓈᒻᒪᒋᔭᐅᓚᐅᖅᐳᖅ. ᑲᑎᒪᓂᐅᔪᒥ ᑕᒡᕙᓂ ᐱᓕᕆᐊᖑᔪᑦ ᐊᑐᖅᑕᐅᓚᐅᖅᐳᑦ ᐋᖅᑭᒋᐊᖅᓯᔾᔪᑕᐅᑉᓗᑎᒃ ᖃᓄᐃᓕᖓᓕᕐᓂᐅᔪᑦ ᐅᓂᒃᑳᖏᓐᓂᒃ ᐊᕐᕕᑦ ᐅᓄᕐᓂᖏᓐᓄᑦ. ᓯᓚᑖᓂᓐᖔᖅᑐᑉ ᐅᓂᒃᑳᖏᑦ ᑲᑎᒪᔾᔪᑕᐅᔪᒥᑦ ᐊᑐᐃᓐᓇᐅᑎᑕᐅᕗᑦ ᓇᓗᓇᐃᒃᑯᑕᖃᖅᓱᑎᒃ ᑕᒡᕙᓂ ᐅᓂᒃᑳᖑᔪᒥ.

Abstract

This document reports on a Regional Advisory Process (RAP) meeting on the Hudson Bay/Foxe Basin Bowhead stock held in Iqaluit, Nunavut, 17-18 June 1999. Meeting participants discussed a draft Stock Status Report (SSR), supporting documents and presentations by participants. Comments on the draft SSR from an external reviewer are also appended. The participants favoured the two-stock hypothesis for eastern Arctic bowhead. Northern Foxe Basin was identified as a calf aggregation area. The Inuit participants offered information suggesting that the stock is recovering. The meeting participants agreed, based on the Potential Biological Removal approach, that a harvest level of one bowhead every two years is safe. The proceedings of this meeting were used to revise the SSR of the stock. The external reviewer's comments on the proceedings are provided in an appendix.

Résumé

Ce document rapporte les discussions tenues durant une réunion de Processus d'examen régional (PER) sur le stock de baleines boréales de la baie d'Hudson et du bassin Foxe, réunion du 17-18 Juin 1999 à Iqaluit, Nunavut. La discussion des participants à la réunion a porté sur le rapport d'état de ce stock (RES) et sur des documents de support et des présentations effectuées par des participants. Le rapport comprend aussi en annexe des commentaires fournis par un réviseur externe. Les participants ont fait part de leur support pour l'hypothèse voulant qu'il y a deux stocks dans l'est de l'Arctique. Le nord du bassin Foxe a été identifié comme un lieu d'aggrégation de veaux de baleines boréales. Les participants inuits ont fourni de l'information qui suggère que le stock est en voie de récupération. Les participants à la réunion se sont entendus pour conclure qu'un niveau de chasse de une baleine boréale aux deux ans est durable. Le compte-rendu de cette réunion a été utilisé pour réviser le RES de ce stock. Les commentaires du réviseur externe sur ce compte-rendu sont fournis en annexe.

ᐱᒋᐊᕐᓂᖓ

ᓄᓇᓂᑦ ᐊᕙᒃᑐᖅᓯᒪᔪᓂᑦ ᖃᐅᔨᒪᔨᒋᔭᐅᔪᑦ ᐱᓕᕆᐊᖑᕙᓪᓕᐊᓂᐊᖅᑐᓄᑦ ᑲᑎᒪᔾᔪᑎᖃᓚᐅᖅᑐᑦ ᐃᖃᓗᓐᓂ, ᓄᓇᕗᒻᒥᑦ ᔪᓐ 17-18-ᒧᑦ ᐊᕐᕌᑦ ᑲᑎᓯᒪᔪᑦ ᖃᓄᐃᓕᖓᓕᕐᓂᖏᓐᓂᒃ ᐅᓂᒃᑳᓕᐊᖑᔪᖅ (SSR) Hᐊᑦᓴᓐ ᐸᐃ/ᓴᓂᕋᔭᐅᓪᓗ ᐃᒪᖓᓃᑦᑐᑦ ᑲᑎᒪᔾᔪᑕᐅᓗᐊᓚᐅᖅᑐᖅ. ᑲᑎᒪᓚᐅᖅᑎᓐᓇᒥᑦ ᓱᓕ. ᑖᓐᓇ ᑎᑎᕋᕐᖓᐅᑕᐅᔪᖅ ᐊᕐᕌᑦ ᖃᓄᐃᓕᖓᓕᕐᓂᖏᓐᓂᒃ ᐅᓂᒃᑳᓕᐊᖑᔪᖅ ᐃᓄᒃᑎᑑᓕᖅᑎᑕᐅᓚᐅᖅᐳᖅ ᓄᓇᓕᖕᒥᐅᓄᓪᓗ ᑐᓂᐅᖅᑲᖅᑕᐅᓚᐅᖅᑐᖅ ᐊᖑᓇᓱᒃᑐᓕᕆᔨᒃᑯᑎᒧᑦ. ᑲᑎᒪᔾᔪᑕᐅᓚᐅᖅᑐᑦ ᑎᑎᕋᖅᑕᐅᓯᒪᕗᑦ ᑖᓴᓱᒪ ᐅᓂᒃᑳᓕᐊᖑᔫᑉ ᐅᐃᒍᖓᓂᑦ 1-ᒥᑦ. ᑲᑎᒪᖃᑕᐅᓚᐅᖅᑐᑦ ᐊᑎᖏᑦ ᑎᑎᕋᖅᑕᐅᓯᒪᕗᑦ ᐅᐃᒍᖓᓂᑦ 2-ᒥᑦ. ᐃᒻᒥᒃᑰᖅᑐᖅ ᖃᐅᔨᓴᖅᑎ ᑲᑎᒪᖃᑕᐅᔪᓐᓇᓚᐅᓐᖏᑦᑐᖅ ᐅᖃᐅᓯᕆᔭᖏᑦ ᐱᔾᔪᑎᖃᖅᑎᓪᓗᒍ ᕿᒥᕐᕈᓚᐅᖅᑕᒥᓂᒃ ᐊᕐᕌᑦ ᖃᓄᐃᑦᑑᓕᕐᓂᖏᓐᓂᒃ ᐅᓂᒃᑳᓕᐊᖑᔪᒥᑦ ᑎᑎᕋᖅᑕᐅᓯᒪᕗᑦ ᐅᐃᒍᖓᓂᑦ 3-ᒥᑦ.

ᑲᑎᒪᔾᔪᑎᒃᓴᑦ ᕿᒥᕐᕈᔭᐅᓚᐅᖅᐳᑦ ᐊᒻᒪᓗ ᑲᑎᒪᔾᔪᑕᐅᓪᓗᑎᒃ, ᓯᕗᓂᐊᒎᓪᓗ ᑲᑎᒪᓂᖅᑕᖅᑎᑦᑎᓪᓗᑎᒃ ᑖᒃᑯᓄᖓᑐᐊᖅ ᐊᖑᓇᓱᑦᑎᓄᑦ ᑲᑎᒪᔩᓐᓇᐅᔪᓄᑦ ᐊᒻᒪᓗ ᓄᓇᕗᒻᒥᑦ ᐆᒪᔪᓕᕆᕐᔪᐊᒃᑯᑦ ᐃᒃᓯᕙᐅᑕᖓᓐᓂᒃ. ᑲᑎᒪᖃᑕᐅᔪᑦ ᐊᖏᖃᑎᒌᓚᐅᖅᐳᑦ ᑲᑎᒪᔩᓐᓇᐃᑦ ᑲᑎᒪᒋᐊᖃᒃᑲᓐᓂᕈᑎᒃ ᑲᑎᒪᕐᑯᔭᐅᓪᓗᑎᒃ. ᑲᑎᒪᓂᕐᒧᑦ ᒪᑐᐃᕈᑕᐅᔪᒥᑦ ᑖᒃᑯᓄᖓ ᑲᑎᒪᔪᓄᑦ ᐃᒃᓯᕙᐅᑕᕆᔭᐅᔪᖅ ᐅᖃᓪᓚᓚᐅᖅᐳᖅ ᐅᖃᐅᓯᖅᖢᓂᓗ ᖃᓄᐃᓕᖓᔾᔪᑕᐅᔪᓂᒃ ᐊᒻᒪᓗ ᓇᓗᓇᐃᖅᓯᓪᓗᓂ ᐅᖃᐅᓯᐅᓂᐊᖅᑐᑦ ᖃᓄᐃᓕᖓᓂᕆᓇᔭᖅᑕᖏᓐᓂᒃ ᑲᑎᒪᑎᓪᓗᒥᑦ. ᑲᑎᒪᓂᕐᒧᑦ ᐃᒃᓯᕙᐅᑕᕆᔭᐅᔪᖅ ᐅᖃᐅᓯᖃᓚᐅᕆᕗᖅ ᖃᓄᐃᓕᖓᔾᔪᑎᓂᒃ ᖃᐅᔨᒪᔾᔪᑎᒧᑦ ᓄᓇᓂᒃ ᐊᕙᒃᑐᖅᓯᒪᔪᓂᑦ ᖃᐅᔨᒪᔨᒋᔭᐅᔪᑦ ᐱᓕᕆᐊᖑᕙᓪᓕᐊᓂᐊᖅᑐᓄᑦ ᓇᓗᓇᐃᔭᐃᓪᓗᓂ ᑖᒃᑯᓂᖓ ᓄᓇᓂᒃ ᐊᕙᒃᑐᖅᓯᒪᔪᓂᑦ ᖃᐅᔨᒪᔨᒋᔭᐅᔪᑦ ᐱᓕᕆᐊᖑᕙᓪᓕᐊᓂᐊᖅᑐᓄᑦ, ᐃᒪᕐᒥᐅᑕᓕᕆᔨᒃᑯᓪᓗ ᐱᓕᕆᐊᒃᓴᕆᔭᖏᓐᓂᒃ ᐊᒻᒪᓗ ᐊᓯᖏᑦ ᓄᓇᓂᒃ ᐊᕙᒃᑐᖅᓯᒪᔪᓂᑦ ᖃᐅᔨᒪᔨᒋᔭᐅᔪᑦ ᐱᓕᕆᐊᖑᕙᓪᓕᐊᓂᐊᖅᑐᓄᑦ ᑲᑎᒪᔨᐅᖃᑕᐅᔪᑦ ᐊᒻᒪᓗ ᓴᖅᑭᑕᐅᔪᕙᓃᑦ ᐱᓕᕆᐊᖑᓂᖅᑐᓄᑦ ᑖᒃᑯᓄᖓ ᓄᓇᓂᒃ ᐊᕙᒃᑐᖅᓯᒪᔪᓂᑦ ᖃᐅᔨᒪᔨᒋᔭᐅᔪᑦ ᐱᓕᕆᐊᖑᕙᓪᓕᐊᓂᐊᖅᑐᓄᑦ.

ᓄᓇᓕᖕᒥᐅᓂᑦ ᐊᒻᒪᓗ ᓄᓇᕗᒻᒥᑦ ᐆᒪᔪᓕᕆᕐᔪᐊᒃᑯᓐᓂᑦ ᑲᑎᒪᔭᖅᑐᖃᑕᐅᔪᑦ ᐃᓯᒫᓗᒥᔭᒥᓂᒃ ᐅᖃᐅᓯᖃᓚᐅᓇᕗᑦ ᐱᔾᔪᑎᖃᖅᖢᑎᒃ ᑲᑎᒪᓂᕐᒧᑦ ᐃᓚᐅᓇᔭᖅᑑᕙᓗᐊᓂᒃ, ᐱᔾᔪᑎᖃᓗᐊᓚᐅᖅᑐᑦ ᓇᐅᔮᒥᐅᑕᕐᒥᒃ ᑲᑎᒪᔭᖅᑐᓇᐊᖅᑰᖓᓗᐊᒥᒃ ᐃᓚᐅᔪᓐᓇᖅᑐᖃᓚᐅᓐᖏᒪᑦ ᐊᒻᒪᓗ ᑖᒃᑯᐊ ᒪᕿᒃ ᓄᓇᓕᖕᒥᓐᓂᑦ ᐆᒪᔪᓕᕆᔨᕐᑯᑎᒥᔭᖏᑦ ᑭᓯᐊᓂ ᐃᓇᖏᖅᑎᒥᔭᒥᔭᐅᐊᒥᒃ ᑲᑎᒪᖃᑕᐅᔪᖅᑕᖃᓚᐅᕐᒪᑦ. ᐊᖏᕈᑕᐅᓚᐅᖅᐳᖅ ᑲᑎᒪᔾᔪᑕᐅᔪᑦ ᑎᑎᖃᓕᐅᖑᔪᑦ ᓇᒧᑐᐃᓐᓇᖅ ᑐᓐᓂᖅᓴᖅᑕᐅᖁᔭᐅᓪᓗᓂ ᐅᖃᐅᓯᐅᔪᓐᓇᕐᓂᐊᕐᒪᑦ ᐊᒻᒪᓗ ᓇᒃᓯᐅᔾᔭᐅᓗᑎᒃ ᑖᒃᑯᓄᖓ ᐊᑐᓂ ᐆᒪᔪᓕᕆᔨᐅᔪᓄᑦ. ᐃᒪᕐᒥᐅᑕᓕᕆᔨᒃᑯᑦ ᐅᖃᓚᐅᖅᑐᑦ ᑭᖑᓪᓕᖅᐸᖑᔪᖅ ᑎᑎᕋᖅᑕᐅᔪᖅ ᐊᕐᕌᑦ ᖃᓄᐃᓕᖓᓕᕐᓂᖏᓐᓂᒃ ᐅᓂᒃᑳᓕᐊᖑᔪᑦ ᑎᑎᕋᖅᓯᒪᓂᖃᖅᑎᑕᐅᓇᓯᖕᓂᐊᕐᓂᖏᓐᓂᒃ ᐊᓯᔾᔨᕈᑕᐅᓯᒪᓕᖅᑐᓂᒃ ᐅᖃᐅᓯᐅᔪᓂᒃ ᐊᒻᒪᓗ ᐊᖏᕈᑕᐅᓯᒪᔪᓂᒃ ᑲᒪᑎᓪᓗᒥᑦ ᐊᒻᒪᓗᑦᑕᐅᖅ ᑕᒪᑐᒧᖓ ᑕᒪᐃᓐᓅᖓᓂᖃᕐᓂᖅᓴᐅᔪᒧᑦ ᕿᒥᕐᕈᔭᐅᓯᒪᓂᖓᓄᑦ. ᐃᒃᓯᕙᐅᑕᖅ ᓇᓗᓇᐃᖅᓯᓚᐅᕆᕗᖅ ᑖᓐᓇ ᐊᕐᕌᑦ ᖃᓄᐃᓕᖓᓕᕐᓂᖏᓐᓂᒃ ᐅᓂᒃᑳᓕᐊᖑᔪᖅ ᖃᖓᑐᐃᓐᓇᒃᑯᑦ ᐊᓯᔾᔨᖅᑕᐅᕙᓪᓕᐊᖃᑦᑕᕐᓂᐊᕐᓂᖓᓂᒃ, ᓲᕐᓗ ᓄᑖᒥᒃ ᖃᐅᔨᒪᔭᐅᒥᐊᖃᖅᑐᒥᒃ ᐱᑕᖃᖅᑐᖃᕐᓂᖅᐸᑦ ᑎᑎᕋᖅᑕᐅᕙᓪᓕᐊᕙᓪᓗᓂ.

ᐊᑖᓂ ᑎᑎᕋᖅᓯᒪᔪᑦ ᓇᐃᓪᓕᒋᐊᖅᓯᒪᔪᑦ ᐅᖃᐅᓯᐅᔪᕙᓂᕐᓂᒃ ᐊᒻᒪᓗ ᐃᓱᒪᒋᔭᐅᔪᑦ ᖃᓄᖅᑑᕈᑕᐅᖁᔭᐅᔪᑦ ᑎᑎᕋᖅᑕᐅᓯᒪᓂᖃᖅᑐᑎᒃ ᐊᕙᒃᑐᖅᓯᒪᔪᑎᒍᑦ ᑖᓐᓇ ᑎᑎᕋᕐᖓᐅᑕᐅᔪᖅ ᐊᕐᕌᑦ ᖃᓄᐃᓕᖓᓕᕐᓂᖏᓐᓂᒃ ᐅᓂᒃᑳᓕᐊᖑᔪᖅ ᕿᒥᕐᕈᔭᐅᕙᓪᓕᐊᑎᓪᓗᒍ ᑖᒃᑯᓄᖓ ᓄᓇᓂᒃ ᐊᕙᒃᑐᖅᓯᒪᔪᓂᑦ ᖃᐅᔨᒪᔨᒋᔭᐅᔪᑦ ᐱᓕᕆᐊᖑᕙᓪᓕᐊᓂᐊᖅᑐᓄᑦ ᑲᑎᒪᔨᒋᔭᐅᔪᓄᑦ.

ᑲᖏᖅᖢᐊᓗᑉ ᐃᒪᖓᓂ/ᓴᓂᕋᔭᐅᑉᓗ ᐃᒪᕐᑯᑖᓂ ᐊᕐᕌᑦ ᖃᓄᐃᓕᖓᓂᕆᔭᖏᓐᓄᑦ ᐅᓂᒃᑳᓕᐊᖑᔪᖅ

ᖃᓄᐃᓚᐅᕆᐊᒃᓴᖓᓂᒃ ᓯᕗᓂᐊᓂ

ᐆᒪᔪᑦ ᓇᓗᓇᐃᖅᑕᐅᓯᒪᓂᖏᑦ ᑎᑎᕋᖅᓯᒪᓂᖃᖅᑐᒥᒃ 'ᖃᓄᐃᓕᖓᔾᔪᑎᒋᔭᖏᑦ' ᐅᖃᐅᓯᐅᓚᐅᖅᑐᑦ. ᑲᑎᒪᖃᑕᐅᔪᑦ ᐊᖏᖃᑎᒌᓚᐅᖅᐳᑦ ᑎᑎᕋᖅᑕᐅᓯᒪᔪᖅ ᓇᓗᓇᐃᖅᓯᒪᓂᖃᕆᐊᖃᕐᓂᖓᓂᒃ ᐱᔾᔪᑎᖃᖅᑎᓪᓗᒥᑦ ᐊᑦᑕᓇᖅᑑᓕᕆᓂᖏᓐᓂᒃ ᓄᖑᑕᐅᑐᐃᓐᓇᕆᐊᖃᕐᓂᖏᓐᓂᒃ ᑎᑎᕋᖅᑕᐅᓯᒪᒋᐊᖃᕐᓂᖏᓐᓂᒃ ᓇᓗᓇᐃᖅᑕᐅᓯᒪᔪᑎᒍᑦ ᖃᓪᓗᓈᑎᑐᑦ ᑕᐃᔭᐅᓪᓗᓂ COSEWIC ᐃᓄᑦᑎᑐᑦ ᑐᑭᖃᖅᑐᖅ ᑲᑎᒪᔨᕋᓛᑦ ᓇᓗᓇᐃᖅᑕᐅᓯᒪᓕᖅᑐᓄᑦ ᐊᑦᑕᓇᖅᑐᒦᓐᓂᖏᓐᓄᑦ ᐆᒪᔪᐃᑦ ᑲᓇᑕᒥ. ᑐᙵᓂᖃᖅᖢᑎᒃ ᐅᖃᐅᓯᐅᓚᐅᖅᑐᒥᑦ ᑖᓴᓱᒧᖓ ᐃᒻᒥᒃᑰᖅᑑᑉ ᖃᐅᔨᓴᖅᑎᐅᑉ ᕿᒥᕐᕈᔾᔪᑎᒋᓚᐅᖅᑕᖓᓂᒃ (ᐅᐃᒍᖓᓂᑦ 3)

ᐊᒻᒪᓗ ᑖᒃᑯᐊ ᒥᑦᓯᓪ ᐊᒻᒪᓗ ᓅᕝᔅ (1982) ᑎᑎᕋᓚᐅᖅᑕᖏᓐᓂᒃ, ᐊᖏᖃᑎᒌᓚᐅᖅᐳᑦ ᐅᑭᐅᖅ 1951 ᓱᓕᑏᖑᓂᖓᓂᒃ ᓄᖅᑲᕈᑕᐅᓚᐅᕐᓂᖓᓄᑦ ᓂᐅᕐᕈᑎᒃᓴᓂᒃ ᐊᕐᕙᓕᓱᖅᑦᑕᖅᑎᓪᓗᒋᑦ (ᐅᐃᒎᓂᑦ 3). ᐃᒪᕐᒥᐅᑕᓕᕆᔨᒃᑯᒃ ᐱᕐᑯᔨᓚᐅᕆᕗᑦ ᐅᖃᐅᓯᖅ ᐊᕐᕕᑦ ᑕᒪᒃᑭᖅᑕᐅᓪᓗᑎᒃ ᐅᖃᐅᓯᐅᓂᖏᑦ ᐊᑐᖅᑕᐅᓂᖅᓴᐅᒥᐊᖃᕐᓂᖓᓂᒃ ᑕᒪᓐᓇᓕ ᐅᖃᐅᓯᖅ ᐊᕐᕕᑦ ᐊᕕᒃᑐᖅᓯᒪᓂᖏᑦ ᐱᔾᔪᑕᐅᑎᓪᓗᒋᑦ ᐊᑐᖅᑕᐅᙱᓪᓗᓂ. ᓯᕗᓪᓕᖅᓱᕈᑎᓂᒃ ᖃᐅᔨᓴᕈᑕᐅᕙᒃᑐᑦ ᓇᓗᓇᐃᖅᑕᐅᓯᒪᔪᑦ ᒪᐃᔅ ᐊᒻᒪᓗ ᖃᐅᔨᓴᖅᑎᓕᒫᑦ ᐅᓂᒃᑳᓕᐊᕆᓚᐅᖅᑕᖏᓐᓂᒃ (1999) ᖃᐅᔨᓴᕈᑕᐅᔪᑦ ᑎᑎᕋᖅᑕᐅᓯᒪᔪᑦ ᓇᓗᓇᐃᖅᓯᒪᓂᖃᕐᓂᖏᓐᓂᒃ ᑕᒃᑯᐊ ᕼᐊᒃᓴᓐ ᐸᐃ/ᓴᓂᕋᔭᐅᓪᓗ ᐃᒪᖓᑕ ᐊᕙᑖᓃᑉᐸᒃᑐᑦ ᐊᕐᕕᑦ ᐃᓛᒃᑰᖓᓪᓗᑎᒃ ᐃᕐᓂᐅᕐᕕᖃᕐᓂᖏᓐᓂᒃ ᑖᒃᑯᓇᙵᑦ ᑎᓄᔾᔨᕕᐅᑉ ᐃᒪᖓᓃᑉᐸᒃᑐᓂᒃ. ᐊᖏᕈᑕᐅᓚᐅᕆᕗᖅ ᐅᑭᐅᖅᑕᖅᑰᑉ ᑲᔾᔮᖓᓄᐊᖅᐸᒃᑐᑦ ᓇᓗᓇᐃᖅᑕᐅᓯᒪᒥᐊᖃᕐᓂᖏᓐᓂᒃ ᐃᓛᒃᑰᖓᓂᖏᓐᓂᒃ. ᓄᓇᕗᒻᒥᑦ ᐆᒪᔪᓕᕆᔨᕐᔪᐊᒃᑯᑦ ᑲᑎᒪᔨᖏᑦ ᐅᖃᓚᐅᖅᐳᑦ ᒪᒃᓚᕆᓐ ᐊᒻᒪ ᑎᕕᔅ (1982) ᖃᐅᔨᓴᖅᑕᕕᓂᖏᑦ ᐅᖃᖅᓯᒪᓂᕋᖅᓱᒋᑦ ᐅᑭᐅᒃᑯᑦ ᐃᓂᒥᕙᒃᑕᖏᑦ ᐃᓚᖃᕋᔭᕐᓂᖏᓐᓂᒃ ᑲᓇᖕᓇᖅᐸᓯᐊᓂᒃ ᑲᖏᖅᓗᐊᓗᑉ ᐃᒪᖓᑕ ᐊᒻᒪ ᑕᒪᓐᓇ ᑕᑯᒃᓴᐅᑎᑕᐅᒥᐊᖃᕐᓂᕋᖅᐸᖓ ᑕᕝᕙᓂ ᐊᕐᕕᑦ ᖃᓄᐃᓕᖓᓕᕐᓂᖏᓐᓂᒃ ᐅᓂᒃᑳᓕᐊᖑᔪᓂᑦ.

ᐆᒪᔪᐃᑦ ᐅᓂᒃᑳᖅᑕᐅᓂᖏᑦ

ᐅᖃᓪᓚᒃᑎᓪᓗᒋᑦ, ᑲᑎᒪᖃᑕᐅᔪᑦ ᐊᖏᖃᑎᒌᓚᐅᖅᐳᑦ ᑎᑎᕋᐅᓯᐅᓯᒪᔪᑦ ᐊᕐᕕᑦ 20 ᒦᑕ ᐅᖓᑖᓂᑦ ᑕᑭᓂᖃᖅᑐᑦ ᑎᑎᕋᐅᓯᐅᖃᑕᐅᒥᐊᖃᕐᓂᖏᓐᓂᒃ ᐊᒻᒪᓗ ᓇᓗᓇᐃᖅᑕᐅᓯᒪᓗᑎᒃ ᑕᒃᑯᐊ ᐆᒪᔪᐃᑦ ᐱᐅᕆᖕ/ᓱᒃᓯᒃ/ᑐᑐᔭᖅᑐᑉᓗ ᐃᒪᖓᓂ ᐊᕐᕕᐅᓂᖏᓐᓂᒃ ᑲᓇᖕᓇᒪᓂᕐᒥᐅᑕᐅᓐᖏᓪᓗᑎᒃ. ᐅᖃᐅᓯᐅᓚᐅᕆᕗᖅ ᖃᐅᔨᓴᕈᑎᓂᒃ ᐱᑕᖃᖅᑐᖃᙱᓐᓂᖓᓂᒃ ᓄᓇᕗᒻᒥᑦ ᑕᒃᑯᐊ ᐊᕐᕕᑦ ᐃᕐᓂᐅᕐᕕᒋᕙᒃᑕᖏᓐᓂᒃ. ᐅᖃᐅᓯᐅᓚᐅᕆᕗᖅ ᐆᒃᑐᕋᖅᑕᐅᔭᕕᓃᑦ ᓇᐃᓛᖑᔪᑦ ᐊᕐᓇᐃᑦ ᐱᐊᕋᖃᖅᑐᑦ ᑕᑭᓂᖏᑦ ᓴᓂᕋᔭᐅᑉ ᐃᒪᖓᑕ ᐊᕙᑖᓃᑉᐸᒃᑐᑦ ᑕᑭᓂᖏᑦ ᐊᔾᔨᒋᔭᐅᓯᒪᓂᖏᓐᓂᒃ ᐆᒃᑐᕋᖅᑕᐅᔪᓂ ᐱᐅᕆᖕ/ᓱᒃᓯᒃ/ᑐᑐᔭᖅᑐᑉᓗ ᐃᒪᖓᓂ ᐊᕐᕕᑦ ᐊᖏᓂᖏᓐᓄᑦ. ᐊᑕᐅᓯᖅ ᑲᑎᒪᖃᑕᐅᔪᖅ ᐅᖃᓚᐅᕆᕗᖅ ᓇᓗᓇᐃᖅᑕᐅᓯᒪᒥᐊᖃᕐᓂᖏᓐᓂᒃ ᖃᐅᔨᓴᕈᑕᐅᕙᒃᑐᑦ ᐊᖑᓇᓱᒃᑎᓄᑦ ᐱᔭᐅᔪᕕᓂᕐᓂᒃ ᐲᖅᓯᕕᐅᓯᒪᔪᑎᒍᑦ ᖃᐅᔨᓴᕈᑕᐅᕙᒃᑐᓂᒃ ᐊᒻᒪᓗ ᓇᓗᓇᐃᖅᑕᐅᓯᒪᒥᓗᑎᒃ ᖃᖓᑕᔫᒃᑯᑦ ᐊᔾᔨᓕᐅᖅᑕᐅᔪᕕᓂᕐᓂᒃ ᖃᐅᔨᓴᕈᑕᐅᔪᑎᒍᑦ. ᐅᖃᐅᓯᖃᓚᐅᕆᕗᑦ ᐊᕐᕕᑦ ᐅᑭᐅᖏᓐᓂᒃ ᖃᐅᔨᓇᓱᒃᑎᓪᓗᒋᑦ ᐊᑐᕈᑕᐅᕙᒃᑐᓂᒃ. ᐊᖏᕈᑕᐅᓚᐅᕆᕗᑦ ᑕᒪᕐᒥᒃ ᖃᐅᔨᔾᔪᑕᐅᔪᕕᓃᑦ ᓇᓗᓇᖏᔾᔪᑕᐅᓕᕐᓂᖓᓂᒃ ᐊᕐᕕᑦ ᐊᑯᓂᐊᓗᒃ ᐃᓅᔪᓐᓇᕐᓂᖏᓐᓂᒃ (ᐅᖓᑖᓄᑦ 100 ᐅᑭᐅᑦ).

ᐊᖑᓇᓱᖕᓂᖅ

ᐃᒻᒥᒃᑰᖅᑐᖅ ᖃᐅᔨᓴᖅᑎ ᓇᓗᓇᐃᖅᓯᓚᐅᕆᕗᖅ ‘ᐱᕐᑯᔭᐅᓂᒃᑯᑦ’ ᐊᖑᓇᓱᒍᓐᓇᕈᑕᐅᔪᑦ ᐊᓯᔾᔨᖅᑕᐅᒥᐊᖃᕐᓂᖓᓂᒃ ‘ᓚᐃᓴᖃᖅᑐᓄᑦ’ ᐊᖑᓇᓱᒍᓐᓇᕈᑕᐅᓂᖏᓐᓂᒃ. ᑮᔅ ᕼᐃ ᐅᖃᓚᐅᖅᑐᖅ ᑕᕝᕙᓂᒎᖅ ᐃᓄᐃᑦ ᖃᐅᔨᒪᔭᑐᖃᖏᑦᑎᑐᑦ ᐅᓂᒃᑳᓕᐊᖑᔪᓂᑦ ᐅᖃᐅᓯᖃᖅᓯᒪᔪᖅᑕᖃᖏᒻᒪᑦ ᐊᕐᕕᒃᑕᐅᔪᕕᓂᕐᓂᒃ ᑕᒫᓂ 1975 ᐊᒻᒪᓗ 1994 ᐊᑯᓐᓂᖓᓂᑦ ᑭᓯᐊᓂ ᑐᕐᑯᑕᐅᔪᕕᓂᕐᓂᒃ ᖃᐅᔨᖅᑦᑕᖅᓯᒪᕗᑦ ᐋᕐᓗᕐᓂᑦ. ᐊᑕᐅᓯᖅ ᓄᓇᓕᖕᒥᐅᑕᖅ ᑲᑎᒪᖃᑕᐅᔪᖅ ᐅᖃᓚᐅᖅᑐᖅ ᒪᕐᐴᖕᓂᒃ ᐊᕐᕕᒃᑐᖃᓚᐅᕐᓂᕋᖅᓱᓂᐅᒃ ᓴᓂᕋᔭᐅᑉ ᖃᓂᒌᔭᓂᑦ ᐅᑭᐅᑦ ᒪᕐᐴᒃ ᐃᓗᐊᓂ 1963 ᑑᖓᓂᑦ ᐊᒻᒪᓗ ᐃᖓᓱᓂᒃ ᐊᕐᕕᕕᓂᕐᓂᒃ ᓇᓂᔭᐅᔪᖅᑕᖃᓚᐅᕐᓂᖓᓂᒃ ᓯᑦᔭᒥᑦ.

ᐱᓕᕆᐊᖃᕐᕕᒡᔪᐊᒥ ᐃᓱᒪᒋᔭᐅᔪᖅ

ᑲᑎᒪᖃᑕᐅᔪᑦ ᐅᖃᐅᓯᖃᓚᐅᖅᐳᑦ ᑕᒪᑐᒥᖓ ᓴᓇᔭᒃᓴᐅᔾᔪᑕᐅᔪᓐᓇᕋᔭᕐᓂᖓᑕ ᒥᒃᓵᓄᑦ. ᐅᖃᐅᓯᐅᓚᐅᖅᐳᖅ ᐊᖑᓇᓱᖕᓂᖅ ᓴᓇᔭᒃᓴᓕᕆᓂᕐᒧᑦ ᓇᓗᓇᐃᖅᓯᒪᓂᖃᓇᐊᖃᙱᓐᓂᖓᓂᒃ ᑭᓯᐊᓂᓕ ᓇᓗᓇᐃᖅᓯᒪᓂᖅᑕᖃᓇᐊᖅᐳᖅ ᐊᑐᕈᑕᐅᔪᓐᓇᕐᓂᖓᓄᑦ ᑕᐅᑐᒃᑕᐅᓯᒪᓗᓂ. ᐊᑕᐅᓯᖅ ᑲᑎᒪᖃᑕᐅᔪᖅ ᐅᖃᓚᐅᕆᕗᖅ ᑕᒃᑯᐊ ᐳᓚᕋᖅᑎᐅᔪᑦ ᐱᔾᔪᑎᒋᓪᓗᒋᑦ ᐊᒻᒪᓗ ᐊᕐᕕᑦ ᖁᙱᐊᖅᑕᐅᖃᑦᑕᕐᓂᖏᑦ ᐱᔾᔪᑎᒋᓪᓗᒋᑦ ᑭᕝᒐᖅᑐᖅᑕᐅᒥᐊᖃᕐᓂᖏᓐᓂᒃ.

ᑲᑎᒪᐅᖅᑐᑦ ᐊᕐᕕᑦ ᓇᓃᓐᓂᖏᑦ

ᑎᑎᕋᖅᓚᐅᑕᐅᓯᒪᔪᖅ ‘ᓇᓂᕐᒥᐅᑕᐅᓂᖏᑦᑎᒍᑦ ᓇᓗᓇᐃᖅᑕᐅᓯᒪᓂᖏᑦ’ ᑎᑎᖅᓯᒪᓂᖓ ᐅᖃᐅᓯᐅᓚᐅᕐᒥᔪᖅ ᐊᒻᒪᓗ ᒪᐃᔅ ᐅᓂᒃᑳᓕᐊᕆᓚᐅᖅᑕᖏᑦᑎᒍᑦ ᓇᓗᓇᐃᖅᓯᒪᓂᖃᕐᓂᖏᓐᓂᒃ ᓯᕗᓪᓕᖅᓱᕈᑎᓂᒃ ᖃᐅᔨᔾᔪᑕᐅᔪᕕᓂᖅᑎᒍᑦ ᐃᑲᔪᕐᓂᖃᕈᑕᐅᕗᖅ ᑕᒪᑐᒧᖓ ᐱᔾᔪᑕᐅᔪᒧᑦ. ᑲᑎᒪᖃᑕᐅᔪᑦ

ᐅᖃᓚᐅᖅᐳᑦ ᓇᓗᓇᐃᖅᓯᒪᔾᔪᑎᒃᓴᓂᒃ ᐱᑕᖃᙱᓐᓂᖓᓂᒃ ᕿᑭᖅᑖᓘᑉ ᐅᐊᖕᓇᖓᓃᑉᐸᒃᑐᓂᒃ ᐊᒻᒪᓗ ᑕᐃᒪ ᑕᒪᒃᑯᐊ ᕿᑭᖅᑖᓘᑉ ᐅᐊᖕᓇᖓᓃᑉᐸᒃᑐᑦ ᐃᓚᒋᔭᐅᓇᓱᒋᔭᐅᓕᖅᓱᑎᒃ ᑕᒪᒃᑯᓄᖓ ᓴᓂᕋᔭᐅᑉ ᐃᒪᖓᑕ ᐅᐊᖕᓇᖓᓃᑉᐊᒃᑐᓄᑦ ᐊᕐᕖᖕᓄᑦ. ᐊᒻᒪᓗᑦᑕᐅᖅ, ᐊᒥᓲᓗᐊᖏᑦᑐᓂᒃ ᓇᓗᓇᐃᖅᓯᒪᔾᔪᑎᒃᓴᓂᒃ ᐱᑕᖃᕐᒥᔪᖅ ᕼᐊᒃᓴᓐ ᐸᐃᒥᑦ. ᑕᐃᒪᓕ, ᓯᕗᓪᓕᖅᓱᕈᑎᓂᒃ ᖃᐅᔨᓴᕈᑕᐅᕙᒃᑐᑎᒍᑦ ᖃᐅᔨᔭᐅᓯᒪᓕᖅᐳᖅ ᑕᒪᒃᑯᐊ ᐊᕕᒃᓯᒪᒻᒪᕆᖕᓂᖏᓐᓂᒃ ᓴᓂᕋᔭᐅᑉ ᐃᒪᖓᓃᑉᐸᒃᑐᑦ ᐊᒻᒪᓗ ᑕᒪᒃᑯᐊ ᑎᓄᔾᔨᕕᐅᑉ ᐃᒪᖓᓃᑉᐸᒃᑐᑦ ᐊᒻᒪᓗ ᐅᐊᓕᓂᕐᒥᐅᑦ ᐃᒪᖓᓃᑉᐸᒃᑐᑦ. ᑕᒪᓐᓇ ᖃᐅᔨᔭᐅᔾᔪᑕᐅᓂᖅ ᐃᑲᔪᕈᑕᐅᓄᖅ ᓇᓗᓇᐃᖅᓯᒪᔾᔪᑕᐅᓕᕐᓂᖓᓄᑦ ᑖᒃᑯᐊ ᒪᕐᕈᐃᖑᓪᓗᑎᒃ ᐊᕕᒃᓯᒪᓂᖃᕐᓂᖏᓐᓂᒃ ᐊᕐᕖᑦ ᓄᓇᕗᒻᒥᑦ ᑭᓯᐊᓂᓕ ᐃᓱᒪᒋᔭᐅᓯᒪᒋᕗᖅ ᑕᒪᒃᑯᐊ ᑎᓄᔾᔨᕕᐅᑉ ᐃᒪᖓᓃᑉᐸᒃᑐᑦ ᐃᓚᒋᔭᐅᓯᒪᒋᐊᒃᓴᖏᓐᓂᒃ ᕿᑭᖅᑖᓘᑉ ᐊᑯᑭᑦᑐᐃᓪᓗ ᓄᓇᖓᑦᑕ ᐃᑭᕋᓴᖓᓃᑉᐸᒃᑐᓄᑦ ᐊᕐᕖᖕᓄᑦ. ᑲᑎᒪᖅᑕᐅᔪᑦ ᐅᖃᓚᐅᕐᒥᔪᑦ ᑕᒪᒃᑯᐊ ᖁᑦᑎᒃᑐᒥᐅᑦ ᐃᒪᖓᑕ ᐊᕙᑖᓃᑉᐸᒃᑐᑦ ᐊᕐᕖᑦ ᓯᕗᓪᓕᖏᑦᑎᒍᑦ ᐊᔾᔨᒋᔭᐅᖏᓐᓂᖏᓐᓂᒃ ᐃᓚᒋᔭᐅᖏᓪᓗᑎᒃ ᑕᒫᓂᑉᐸᒃᑐᓄᑦ ᑎᓄᔾᔨᕕᐅᑉ ᐃᒪᖓᑕ ᐊᕙᑖᓃᑉᐸᒃᑐᓄᑦ ᑕᐃᒪᓕ ᖃᐅᔨᓴᖅᑕᐅᒃᑲᓐᓂᕆᐊᖃᕐᓂᖏᓐᓂᒃ ᓇᓂᕐᒥᐅᑕᐅᓂᖏᓂᒃ ᖃᐅᔨᓴᐅᓇᓱᒡᓗᑎᒃ ᐱᓕᕆᐊᖑᕆᐊᖅᖁᔭᐅᓪᓗᓂ. ᐃᒻᒥᒃᑰᖅᑐᖅ ᖃᐅᔨᓴᖅᑎ ᐅᖃᓚᐅᕆᕗᖅ, ᒪᕐᕈᐃᖑᒋᑦᑐᓂᒃ ᑭᓯᐊᓂᓕ ᐊᒥᓲᓂᖅᓴᓂᒃ ᐊᕕᒃᓯᒪᓂᖅᑕᖃᕋᓱᒋᓪᓗᓂᐅᒃ ᑲᓇᑕᐅᑉ ᐅᑭᐅᖅᑕᖅᑐᐊᑕ ᑲᓇᖕᓇᖅᐸᓯᐊᓃᑉᐸᒃᑐᓂᒃ. ᐊᖏᕈᑕᐅᓚᐅᖅᐳᖅ, ᑕᒪᓐᓇ ᐱᔾᔪᑕᐅᔪᖅ ᑕᕝᕙᓂ ᐊᕐᕖᑦ ᖃᓄᐃᓕᖓᓕᕐᓂᖏᓐᓂᒃ ᐅᓂᒃᑳᓕᐊᖑᔪᒥᑦ, ᓄᓇᐅᑉ ᐃᒪᖁᑖᓂᑦ ᓇᓃᑉᐸᓃᖏᑦ ᐊᕐᕖᑦ ᓇᓗᓇᐃᖅᑕᐅᓯᒪᔾᔪᑎᖃᖅᐸᓪᓗᑎᒃ ᖃᐅᔨᔭᐅᔾᔪᑕᐅᓂᖏᑦ ᓇᓗᓇᐃᔭᖅᑕᐅᓯᒪᓕᓵᖓᑦ.

ᖃᑉᓯᐅᓂᖏᑦ ᐊᕐᕖᑦ

ᑎᑎᕋᕐᖓᐅᑕᐅᓯᒪᔪᒥᑦ ‘ᑲᑎᓯᒪᕙᒃᑐᑦ ᖃᔅᓯᐅᓂᖏᑦ’ ᑎᑎᕋᖅᑕᐅᓯᒪᓂᖓ ᕿᒥᕐᕈᔭᐅᓚᐅᖅᑐᖅ. ᓲᓴᓐ ᑲᓯᓐᔅ ᓇᓗᓇᐃᔭᓚᐅᖅᑐᖅ ᐊᒥᓲᓂᕆᔭᖏᑦ 270 ᐊᒻᒪᓗ 75 ᑎᑎᕋᐅᓯᐅᓯᒪᔪᑦ ᓇᓚᐅᑦᑖᕈᑕᐅᑐᐃᓐᓇᖅᓯᒪᓂᖏᓐᓂᒃ ᐊᐅᔭᒥᑦ ᓈᓴᖅᑕᐅᓚᐅᖅᑐᓂᒃ ᑭᓯᐊᓂ ᐊᒻᒪᓗ ᑕᒪᒃᑯᐊᓕ ᑲᑎᓕᒫᖅᓗᒋᑦ ᑕᒪᒃᑭᖅᓯᒪᓂᖏᑦᑎᒍᑦ ᓈᓴᖅᑕᐅᓯᒪᙱᓪᓗᑎᒃ. ᐅᖃᓚᐅᕐᒥᔪᖅ ᓈᓴᖅᑕᐅᓯᒪᓂᖏᑦ ᖃᔅᓯᑦᓚᓇᐅᓂᖏᑦ ᓇᓗᓇᐃᖅᓯᒪᑦᓚᓇᒍᓐᓇᓚᐅᖏᓐᓂᖏᓐᓂᒃ ᐱᔾᔪᑎᒋᑦᑐᒍ ᐊᖅᑲᐅ ᒪᕙᒃᑐᑦ ᐊᒻᒪᓗ ᑕᒪᒃᑯᐊ ᓄᖅᑲᖓᕝᕕᒋᕙᒃᑕᖏᑦ ᐃᒪᐅᑉ ᖄᖓᓃᓐᓂᕆᕙᒃᑕᖏᓪᓗ ᑕᑯᔭᐅᔪᓐᓇᖏᓐᓂᖏᓐᓄᑦ ᓇᓗᓇᕈᑕᐅᓗᐊᓚᐅᕐᒪᑦ. ᑕᐃᒪᓕ ᑕᒪᒃᑯᐊ ᖃᔅᓯᐅᓂᖏᑦ ᓇᓚᐅᖅᑖᕈᑎᑐᐃᓐᓇᐅᓪᓗᑎᒃ ᓇᓗᓇᐃᖅᑕᐅᓯᒪᔾᔪᑎᒋᔭᑐᐊᖏᑦ. ᑲᑎᒪᖅᑕᐅᔪᑦ ᐅᖃᓚᐅᖅᐳᑦ ᑕᒪᒃᑯᐊ ᖃᔅᓯᐅᓂᖏᓐᓂᒃ ᓇᓚᐅᑦᑖᕈᑕᐅᔪᑦ ᐊᒥᓲᓂᖅᓴᐅᓂᖏᓐᓂᒃ ᖃᐅᔨᓴᕈᑕᐅᕙᓚᐅᖅᑐᓂᒃ ᐅᖃᐅᓯᕐᒥᑦ ᓇᓗᓇᐃᒃᑯᑕᖃᖅᑎᓪᓗᒋᑦ ‘ᖁᓕᐅᔪᑎᒍᑦ’ ᓈᓴᐃᔾᔪᑎᖃᖃᑦᑕᖅᑎᓪᓗᒋᑦ. ᐃᓚᖏᑦ ᑲᑎᒪᖅᑕᐅᔪᑦ ᐊᑲᐅᓂᖅᓴᐅᓇᓱᒋᓚᐅᖅᐳᑦ ᖃᐅᔨᓴᖃᑦᑕᕈᑎᒃ ᑕᖅᑭᖅ ᔪᓚᐃ ᕿᑎᐸᓗᐊᓂᑦ ᓄᓇᓕᖕᒥᐅᑦ ᑕᑯᖃᑦᑕᕐᓂᖅᓴᐅᓕᓵᖓᑦ ᐊᕐᕕᖕᓂᒃ ᓂᕆᔪᓂᒃ ᑕᒫᓂ ᓯᑰᑉ ᓯᓈᖓᓂᑦ. ᓲᓴᓐ ᑲᓯᓐᔅ ᓇᓗᓇᐃᔭᓚᐅᕐᒥᔪᖅ ᐊᐅᒍᔅᓯ ᖃᐅᔨᓴᖃᑦᑕᕈᑎᒃ ᐊᑲᐅᓂᖅᓴᐅᓂᕋᖅᓱᒍ ᐱᔾᔪᑎᒋᓪᓗᒍ ᔪᓚᐃ ᓱᓕ ᐊᑐᖅᑎᓪᓗᒍ ᑕᒪᒃᑭᖅᓯᒪᖃᑦᑕᖏᓐᓂᖏᓐᓂᒃ ᐊᕐᕖᑦ ᐅᑎᖅᑕᕝᕕᒋᕙᒃᑕᒥᓄᑦ ᓯᕗᒧᒃᓯᒪᔪᑦ. ᖃᐅᔨᓴᐅᔪᑎᒍᑦ ᓇᓗᓇᐃᖅᑕᐅᓯᒪᒋᕗᖅ ᑕᒫᓂ ᓴᓂᕋᔭᐅᑉ ᐃᒪᖓᑕ ᐅᐊᖕᓇᖅᐸᓯᐊᓂᑦ ᐃᓂᒋᔭᐅᖃᑦᑕᕐᓂᖓᓂᒃ ᐃᕐᓂᐅᕐᕕᐅᓂᖓᓄᑦ ᐊᕐᕖᖕᓄᑦ ᐊᕐᓇᓪᓗᕐᓄᑦ ᒫᕋᓛᖅᑐᓄᑦ ᐊᒻᒪᓗ ᐃᓐᓇᐅᖏᑦᑐᑦ ᓱᓕ ᑕᒫᓃᓐᓂᖅᓴᐅᖃᑦᑕᕐᓂᖏᓐᓂᒃ, ᑕᒫᓂ ᓄᓇᓕᖕᒥᐅᑦ ᐅᖃᖅᓯᒪᒋᕗᑦ ᑕᑯᖃᑦᑕᕐᓂᖏᓐᓂᒃ ᐱᐊᕋᓂᒃ ᑕᒫᓂ ᕼᐊᑦᓴᓐ ᐸᐃ ᐅᐊᖕᓇᖅᐸᓯᐊᓂᑦ (ᐊᒻᒪᓗ ᐃᓄᐃᑦ ᖃᐅᔨᒪᔭᑐᖃᖏᑦᑎᒍᑦ ᐊᕐᕖᖕᓂᒃ ᖃᐅᔨᓴᕈᑕᐅᒧᑦ).

ᖃᓄᐃᑉᐸᓪᓕᐊᓂᖏᑦ ᐊᕐᕖᑦ

ᐱᔾᔪᑎᒋᓪᓗᒍ ᑲᑎᓯᒪᔪᑦ ᐊᒥᓲᓂᖏᑦ ᐊᓯᔾᔨᖅᓯᒪᓂᖏᓐᓂᒃ, ᓄᓇᓕᖕᒥᐅᑦ ᑲᑎᒪᖅᑕᐅᔪᑦ ᐅᖃᓚᐅᖅᑐᑦ ᓴᓂᕋᔭᐅᑉ ᐃᒪᖓᑕ ᐊᕙᑖᓂᕐᒥᐅᑦ ᑕᑯᒐᔪᓗᐊᓚᐅᖅᓯᒪᖏᓐᓂᖏᓐᓂᒃ ᑕᐃᔅᓱᒪᓂ ᐅᑭᐅᑦ 40 ᐊᓂᒍᖅᓯᒪᓕᖅᑎᓪᓗᒋᑦ ᑭᓯᐊᓂᓕ ᒫᓐᓇ ᑕᑯᒐᔪᖕᓂᖅᓴᒻᒪᕆᐅᓕᖅᓱᑎᒃ. ᓄᓇᕗᒻᒥᑦ ᐆᒪᔪᓕᕆᔨᕐᔪᐊᒃᑯᖏᓐᓂᑦ ᑲᑎᒪᔨᖏᓐᓂᙵᖅᑐᑦ ᐅᖃᓚᐅᖅᑐᑦ ᐃᓄᐃᑦ ᖃᐅᔨᒪᔭᑐᖃᖏᑦᑎᒍᑦ ᓇᔪᒐᓇᔭᐅᕙᖕᓂᖏᑦᑎᒍᑦ ᖃᐅᔨᓴᕈᑕᐅᓚᐅᖅᑐᒥᑦ ᓇᓗᓇᐃᖅᓯᒪᓂᖃᕐᓂᖓᓂᒃ ᒫᓐᓇᐅᔪᖅ ᐊᕐᕖᑦ ᑕᑯᔭᐅᒐᔪᖕᓂᖅᓴᐅᓕᕐᓂᖏᓐᓂᒃ ᑕᐃᔅᓱᒪᓂᐅᓚᐅᖅᑐᒥᑦ 1960 ᐊᑐᖅᑎᓪᓗᒋᑦ ᑕᒫᓂ ᓴᓂᕋᔭᐅᑉ ᐃᒪᖓᑕ ᐊᕙᑖᓂ. ᕼᐊᑦᓴᓐ ᐸᐃ ᐅᐊᖕᓇᖅᐸᓯᐊᓂᑦ, ᑕᑯᔭᐅᖃᑦᑕᖅᓯᒪᓕᖅᑐᑦ ᐊᒥᓱᙳᖅᐸᓪᓕᖅᓯᒪᓕᕐᓂᖏᓐᓂᒃ ᑕᐃᔅᓱᒪᓂᐅᓚᐅᖅᑐᒥᑦ 1970 ᐊᑐᖅᑎᓪᓗᒋᑦ.

ᑲᑎᒪᖅᑕᐅᔪᑦ ᐊᑐᖃᑦᑕᖁᔨᓕᓚᐅᖅᐳᑦ ᖃᔅᓯᐅᓂᖏᓐᓂᒃ ᓇᓚᐅᖅᑖᕈᑕᐅᔪᑦ ᖃᐅᔨᓴᐅᓯᒪᓕᕐᓂᖏᑦ ᑲᑎᓯᒪᔪᑎᒍᑦ ᓈᓴᖅᑕᐅᔾᔪᑕᐅᖃᑦᑕᖁᔭᐅᓪᓗᑎᒃ ᒫᓐᓇᒍᑦ ᖃᔅᓯᓪᓗᐊᖑᓂᖏᓐᓂᒃ ᖃᐅᔨᔾᔪᑕᐅᓇᓯᓕᓵᖓᑦ. ᐊᑐᕈᑕᐅᖃᑦᑕᕐᓂᐅᖅᑐᑎᒍᑦ ᑲᑎᓯᒪᔪᑦ 575-ᓂᒃ ᐊᒥᓲᓂᖃᕐᓂᖏᓐᓂᒃ ᓇᓚᐅᖅᑖᕈᑕᐅᓯᒪᓂᖅᓚᖅᐳᑦ

(ᑕᑯᓗᒍ ᕗᑦᐱ ᐊᒻᒪᓗ ᐸᑦᑭᓐ ᐅᓂᒃᑳᓚᐊᖏᓐᓂᒃ, 1993, ᑕᐃᔭᐅᔪᒥᑦ ᐊᕐᕌᑦ, ᑲᑎᓯᒪᓂᕆᖃᑦᑕᖅᑕᖏᑦ ᐃᒪᕐᒥᐅᑕᑦ ᓂᕐᔪᑏᑦ, ᐅᖃᓕᒫᒐᒃᓴᓕᐊᖑᔪᖅ ᐊᐃᑉᐸᖓᓂᑦ 2). ᒫᓐᓇ ᖃᔅᓯᐅᓂᖓᓐᓂᒃ ᓇᓚᐅᖅᑖᕈᑕᐅᓯᒪᔪᑦ ᓈᓴᐃᓪᓗᑎᒃ ᐊᐅᔭᒃᑯᑦ ᐅᐸᒃᓯᒪᕙᒃᑕᖏᓂᑦ ᐊᒥᓲᓂᖅᓴᒻᒪᕆᐅᕗᑦ 50%-ᒥᑦ ᓇᓚᐅᖅᑖᕈᑕᐅᓯᒪᔪᑎᒍᑦ ᐊᑐᕈᑕᐅᖃᑦᑕᕐᓂᐊᖅᑐᑎᒍᑦ ᑲᑎᓯᒪᔪᑦ ᓈᓴᖅᑕᐅᓯᒪᓂᖏᓐᓂᒃ.

ᓄᓇᕗᒻᒥᑦ ᐆᒪᔪᓕᕆᔨᕐᔪᐊᒃᑯᑦ ᑲᑎᒪᔨᖏᓐᓂᖔᖅᑐᖅ ᑲᑎᒪᖃᑕᐅᔪᖅ ᐅᖃᓚᐅᖅᑐᖅ ᓈᓴᖅᑕᐅᓯᒪᓂᖏᑦ ᐊᑐᕈᑕᐅᖃᑦᑕᕐᓂᐊᖅᑐᑎᒍᑦ ᑲᑎᓯᒪᔪᑦ ᓇᓗᓇᖏᔾᔪᑕᐅᓂᖓᓂᒃ ᑮᐳᑎᖃᑦᑕᕐᓂᖏᑦ ᐊᕐᕌᑦ ᑕᒪᓃᑉᐸᒃᑐᑦ ᓴᓂᕋᔭᐅᑉ ᐃᒪᕐᑯᑖᓂᑦ ᐊᒻᒪᓗ Hᐊᑦᓴᓐ ᐸᐃ ᐅᐊᓕᓂᖅᐸᓯᐊᓃᑉᐸᒃᑐᑦ. ᑕᐃᒪᓕ ᑮᐳᑎᖃᑦᑕᙱᒃᑯᑎᒃ, ᓈᓴᖅᑕᐅᓯᒪᔪᑦ ᓇᓚᐅᑦᑖᕈᑕᐅᔪᑦ ᐊᑐᕈᑕᐅᖃᑦᑕᕐᓂᐊᖅᑐᑎᒍᑦ ᑲᑎᓯᒪᔪᑦ ᓈᓴᖅᑕᐅᓯᒪᓂᖏᑦ ᓱᓕᓇᔭᙱᑦᑐᑦ ᐊᒻᒪᓗ ᑐᕌᖓᓇᔭᖅᐳᖅ ᑲᖏᖅᓱᐊᓗᑉ ᐅᐊᓕᓂᖅᐸᓯᐊᓄᑐᐃᓐᓇᖅ. ᐊᓯᖏᑦ ᑲᑎᒪᖃᑕᐅᔪᑦ ᐅᖃᓚᐅᕐᒥᔪᑦ ᑕᑯᔭᐅᖃᑦᑕᖅᑐᑦ ᐊᒥᓲᔪᓐᓃᖅᐸᓪᓕᐊᓂᖏᑦ ᓴᓂᕋᔭᐅᑉ ᐃᒪᖓᑕ ᐊᕙᑖᓃᑦᑐᑦ (ᑎᑭᑦᑎᖅᖢᒍ 1960 ᐅᑭᐅᖑᔪᑦ), ᐊᕐᕙᒐᓱᒃᑎᓄᑦ ᐅᐸᒃᑕᐅᖃᑦᑕᓚᐅᖏᑦᑐᖅ, ᓇᓗᓇᖏᔾᔪᑕᐅᓂᖏᓐᓂᒃ ᑕᒪᒃᑯᐊ ᐊᕐᕌᑦ ᒪᕐᕈᐃᓂᒃ ᐅᐸᒃᓯᒪᕝᕕᖃᖃᑦᑕᖅᑐᑦ ᐊᑕᐅᓯᐅᓂᖏᓐᓂᒃ.

ᐊᑐᒃᑲᓐᓂᖃᑦᑕᖅᑐᑦ ᐊᖑᓇᓱᒃᑕᐅᓂᖏᑦᑕ ᐅᓄᕐᓂᖏᑦ

ᑎᑎᕋᕐᖓᐅᑕᐅᔪᖅ ᐱᔾᔪᑎᖃᖅᑐᖅ ‘ᓄᖑᕋᐃᕙᓪᓕᐊᖕᒥᓪᓗᑎᒃ ᐊᖑᓇᓱᒃᑕᐅᖃᑦᑕᕐᓂᖏᓐᓄᑦ’ ᕿᒥᕐᕈᔭᐅᓚᐅᖅᑐᖅ. ᑲᑎᒪᖃᑕᐅᔪᑦ ᑐᑭᓯᑎᑕᐅᔪᒪᓚᐅᖅᐳᑦ ᑎᑎᖃᑕᐅᓯᒪᔪᒥᒃ ᖃᓪᓗᓈᑎᑐᑦ ᑕᐃᔭᐅᔪᖅ Potential Biological Removal (PBR) ᐃᓄᒃᑎᑐᑦ ᑐᑭᖃᓪᓗᐊᖅᑐᖅ ᐊᐅᓚᑕᐅᓂᖃᑦᑎᐊᕐᓗᑎᒃ ᐊᖑᓇᓱᒃᑕᐅᔪᓐᓇᕐᓂᖏᑦ. ᓯ. ᐃᓐᓴ ᓇᓗᓇᐃᔭᓚᐅᖅᑐᖅ ᐱᔾᔪᑕᐅᓂᖏᓐᓂᒃ ᑕᒪᑐᒧᖓ ᐊᖑᓇᓱᒃᑕᐅᔪᓐᓇᕐᓂᖏᓐᓂᒃ. ᖃᓄᕐᓗ ᑕᒪᓐᓇ ᐊᐅᓚᑕᐅᓂᖃᑦᑎᐊᕐᓗᑎᒃ ᐊᖑᓇᓱᒃᑕᐅᔪᓐᓇᕐᓂᖏᑦ ᖃᔅᓯᐅᓇᔭᕐᓂᖏᓐᓂᒃ ᓇᓗᓇᐃᖅᑕᐅᓯᒪᓂᖏᑦ ᐊᒻᒪᓗ ᑐᒃᓯᕋᐅᑕᐅᔪᑦ ᑕᒫᓂᑉᐸᒃᑐᓄᑦ Hᐊᑦᓴᓐ ᐸᒥᑦ ᐊᕐᕌᑦ ᐊᒥᓲᓂᖏᑦ. ᐊᖏᕈᑎᖃᓕᓚᐅᖅᐳᑦ ᑕᐃᒪᓐᓇ ᐊᑐᕐᓂᖃᒻᒪᕆᖕᓂᖕᓴᓂᒃ ᖃᔅᓯᓂᒃ ᐱᔭᐅᔪᓐᓇᖅᑐᓂᒃ ᖃᐅᔨᒪᔾᔪᑕᐅᔪᓐᓇᕈᑎᒋᓂᖓᓄᑦ.

ᓯ. ᐃᓐᓴ ᐅᓂᒃᑳᓚᐅᕐᒥᔪᖅ ᖃᐅᔨᒪᔾᔪᑕᐅᑦᑎᐊᕈᓐᓇᕋᔭᖅᑐᓂᒃ ᖃᔅᓯᓂᒃ ᐊᐅᓚᑕᐅᓂᖃᑦᑎᐊᕐᓗᑎᒃ ᐊᖑᓇᓱᒃᑕᐅᔪᓐᓇᕋᔭᖅᑐᓂᒃ ᑕᒫᓂ Hᐊᒃᓴᓐ ᐸᐃ/ᓴᓂᕋᔭᐅᓪᓗ ᐃᒪᖓᓂᑦ. (ᓇᓗᓇᐃᒃᑯᑕᖅ 1). ᖃᔅᓯᐅᓇᓯᒋᔭᐅᓂᖏᑦᑎᒍᑦ ᒪᓕᖕᓂᖃᕋᔭᖅᐳᖅ ᑕᒪᒃᑯᐊ ᐊᕐᕌᑦ ᓴᓂᕋᔭᐅᑉ ᐃᒪᖓᑕ ᐅᐊᖕᓇᖅᐸᓯᐊᓃᑉᐸᒃᑐᑦ ᐊᒻᒪᓗ Hᐊᒃᓴᓐ ᐸᐃᒥᑦ ᑕᐅᑐᒃᑕᐅᓯᒪᔭᔪᑕᐅᕗᑦ ᐊᑕᐅᓯᕐᒥᐅᑕᐅᓂᖏᓐᓂᒃ ᐅᕝᕙᓘᓐᓃᑦ ᐊᓯᖓᓂᕐᒥᐅᑕᐅᓂᖏᓐᓂᒃ. ᖃᔅᓯᓂᒃ ᐱᔭᐅᔪᓐᓇᕋᔭᖅᑐᓂᒃ ᖃᐅᔨᒪᔾᔪᑕᐅᔪᓐᓇᕋᔭᖅᒥᔪᑦ ᓄᖑᑉᐸᓪᓕᐊᔾᔪᑕᐅᖕᒥᓪᓗᑎᒃ ᒪᓕᖕᓂᖅᓇᕗᖅ ᑖᒃᑯᐊ ᑲᑎᓯᒪᕙᒃᑐᑦ ᐊᒥᓲᓂᖅᓴᔭᖕᒥᖕᓂᒃ. ᑕᐃᒪᓐᓇ ᐋᖅᑭᐅᒪᓂᖅᑎᐊᖅᑐᒥᒃ ᐊᖑᓇᓱᒃᑎᑕᐅᖃᑦᑕᕐᓂᐊᕈᑎᒃ ᐊᒥᓱᙳᖅᐸᓪᓕᐊᓂᖅᑎᐊᕈᓐᓇᕋᔭᖅᑐᖅ ᑕᐃᒪᓐᓇ ᓯ. ᐃᓐᓴ ᖃᐅᔨᓴᕐᓂᕐᒥᒍᑦ ᐋᖅᑭᐅᒪᓂᖅᑎᐊᕈᑕᐅᔪᓐᓇᕋᔭᖅᑐᓂᒃ ᖃᐅᔨᓯᒪᓕᓚᐅᖅᑐᖅ, ᐊᔾᔨᒌᙱᑦᑐᓂᒃ ᒪᓕᖕᓂᖃᖅᑐᑎᒃ ᐱᖓᓱᖓᓂᒃ ᖃᓄᐃᓕᖓᓂᖃᕈᑎᒋᔭᖓᓂᒃ ᒪᓕᒍᑎᖃᖅᑐᑎᒃ (ᐊᑦᑕᓇᖅᑐᒦᑦᑐᑦ ᓄᖑᑉᐸᓪᓕᐊᑐᐃᓐᓇᕋᔭᕐᓂᖓᓄᑦ: 01; ᖃᐅᔨᒪᔭᐅᙱᑦᑐᑦ ᐊᑦᑕᓇᖅᑐᒦᑦᑐᓪᓘᓐᓃᑦ: 0.5; ᓇᓗᓇᙱᑦᑎᐊᖅᑐᑦ ᐊᒥᓲᓂᖏᑦ ᓱᕐᕌᖏᑦᑎᐊᕋᔭᕐᓂᖓᓄᑦ: 1.0).

ᓇᓗᓇᐃᒃᑯᑕᖅ 1. ᓇᐃᓪᓕᒋᐊᖅᓯᒪᔪᑦ ᐊᐅᓚᑕᐅᓂᖃᑦᑎᐊᕐᓗᑎᒃ ᐊᖑᓇᓱᒃᑕᐅᔪᓐᓇᕐᓂᖏᑦ ᖃᔅᓯᐅᔪᓐᓇᕋᔭᕐᓂᖏᑦ ᑕᒫᓂ Hᐊᑦᓴᓐ ᐸᐃ/ᓴᓂᕋᔭᐅᓪᓗ ᐃᒪᖓᓂᑦ ᐊᕐᕌᑦ.

ᑲᑎᓯᒪᔪᑦ ᓇᓗᓇᐃᖅᓯᒪᓂᖏᑦ	ᐊᑦᑕᓇᖅᑐᒦᑦᑐᑦ ᓄᖑᑉᐸᓪᓕᐊᓇᔭᕐᓂᖓᓄᑦ	ᖃᐅᔨᒪᔭᐅᙱᑦᑐᑦ ᐊᑦᑕᓇᖅᑐᒦᑦᑐᓪᓘᓐᓃᑦ	ᓇᓗᓇᙱᑦᑎᐊᖅᑐᑦ ᐊᒥᓲᓂᖏᑦ ᓱᕐᕌᖏᑦᑎᐊᕋᔭᕐᓂᖓᓄᑦ
ᓴᓂᕋᔭᐅᑉ ᐃᒪᖓᓂᑦ ᐃᓵᒃᑰᖓᔪᑦ	0.5/ᐊᕐᕌᒍᒧᑦ ᐅᕝᕙᓘᓐᓃᑦ 1-ᒥᑦ ᐅᑭᐅᑦ 2 ᐃᓗᐊᓂ	2.5/ᐊᕐᕌᒍᒧᑦ	4.9/ᐊᕐᕌᒍᒧᑦ
Hᐊᑦᓴᓐ ᐸᐃ ᐃᓵᒃᑰᖓᔪᑦ	0.1/ᐊᕐᕌᒍᒧᑦ ᐅᕝᕙᓘᓐᓃᑦ 1-ᒥᑦ ᐅᑭᐅᑦ 10 ᐃᓗᐊᓂ	5.6/ᐊᕐᕌᒍᒧᑦ	11.1/ᐊᕐᕌᒍᒧᑦ
ᓴᓂᕋᔭᐅᑉ ᐃᒪᖓ/ Hᐊᒃᓴᓐ ᐸᐃ	0.6 ᐊᕐᕌᒍᒧᑦ ᐅᕝᕙᓘᓐᓃᑦ 1-ᒥᑦ ᐅᑭᐅᑦ 2 ᐃᓗᐊᓂ	3.1 /ᐊᕐᕌᒍᒧᑦ	6.2/ᐊᕐᕌᒍᒧᑦ

ᓄᓇᕗᒻᒥᑦ ᐆᒪᔪᓕᕆᔨᕐᔪᐊᒃᑯᑦ ᑲᑎᒪᔨᖏᓐᓂᖔᖅᑐᑦ ᐊᒻᒪᓗ ᓄᓇᓕᖕᒥᐅᓂᑦ ᑲᑎᒪᖃᑕᐅᔪᑦ ᐱᔪᒪᓚᐅᖅᐳᑦ ᐊᑐᕈᑎᖃᕈᒪᓪᓗᑎᒃ ᓴᓂᕋᔭᐅᑉ ᐃᒪᖓᓂ/Hᐊᑦᓴᓐ ᐸᐃᒥᓗ ᐊᒻᒪᓗ ‘ᖃᐅᔨᒪᔭᐅᙱᑦᑐᑦ ᐊᑦᑕᓇᖅᑐᒦᑦᑐᓪᓘᓐᓃᑦ’ ᐊᑐᕈᑎᒋᓗᒍ. ᐱᔪᒪᓚᐅᕆᕗᑦ ᐱᖁᔭᐅᓯᒪᓂᖃᕆᐊᖃᕐᓂᖓᓂᒃ ᓄᖑᕋᐃᕙᓪᓕᐊᖏᓪᓗᑎᒃ ᐊᖑᓇᓱᖃᑦᑕᕈᑕᐅᓇᔭᖅᑐᒥᒃ ᐱᖓᓱᓂᒃ (3) ᐱᔭᓐᓇᖅᑎᑕᐅᓗᑎᒃ ᐊᖀᒍᒧᑦ. ᐃᒪᕐᒥᐅᑕᓕᕆᔨᒃᑯᑦ ᑕᒪᑐᒥᖓ ᐊᑲᖅᓴᓚᐅᙱᑦᑐᑦ ᐱᖁᔭᐅᔪᒥᒃ ᐱᔾᔪᑎᒋᓪᓗᒍ ᑮᐳᑎᖃᑦᑕᕐᓂᖏᑦ ᑕᒪᓂ Hᐊᑦᓴᓐ ᐸᐃᒥᑦ ᐊᒻᒪᓗ ᓴᓂᕋᔭᐅᑉ ᐃᒪᖓᓂᑦ ᐊᕐᕖᑦ ᓱᓕ ᓇᓗᓇᐃᖅᑕᐅᓯᒪᓪᓚᕆᖏᓐᓂᖏᓐᓄᑦ. ᐊᒻᒪᓗᒃᑲᓐᓂᖅ, ᐅᖃᐅᓯᐅᓚᐅᕆᕗᖅ ᐊᒥᓲᓂᖏᓐᓄᑦ ᐊᓯᔾᔨᕈᑕᐅᔪᑦ ᖃᐅᔨᔭᐅᔪᓐᓇᖃᔭᕐᓂᖏᓐᓂᒃ ᐅᑯᑎᒎᓇᖅ ᑲᑎᒪᔨᕋᓛᑦ ᓇᓗᓇᐃᖅᑕᐅᓯᒪᓕᖅᑐᓄᑦ ᐊᑦᑕᓇᖅᑐᒦᓐᓂᖏᓐᓄᑦ ᐆᒪᔪᐃᑦ ᑲᓇᑕᒥ (COSEWIC) ᐱᓕᕆᔾᔪᑎᒋᕙᒃᑕᖏᑦᑎᒍᑦ. ᑲᑎᒪᖃᑕᐅᔪᑦ ᐊᖏᖃᑎᒌᓚᐅᕆᕗᑦ ᓄᓇᕗᒻᒥᑦ ᐆᒪᔪᓕᕆᔨᕐᔪᐊᒃᑯᑦ ᑲᑎᒪᔨᕋᓛᑦ ᓇᓗᓇᐃᖅᑕᐅᓯᒪᓕᖅᑐᓄᑦ ᐊᑦᑕᓇᖅᑐᒦᓐᓂᖏᓐᓄᑦ ᐆᒪᔪᐃᑦ ᑲᓇᑕᒥ (COSEWIC) ᕿᒥᕐᕈᔭᐅᑎᑦᑎᒋᐊᖃᕐᓂᖏᓐᓂᒃ ᑕᒪᒃᑯᓂᖓ ᐊᑦᑕᓇᖅᑐᖅᑐᑦ ᖃᔅᓯᐅᓕᕐᓂᖏᓐᓂᒃ ᐊᕐᕖᑦ ᓄᓇᕗᒻᒥᑦ. ᐃᓱᒪᒋᔭᐅᔪᖅ ᑎᑎᖅᑕᐅᓯᒪᔪᓄᑦ ᐃᓚᓕᐅᔾᔭᐅᖁᔭᐅᔪᖅ ᐃᒪᐃᓕᖓᓚᐅᖅᑐᑦ:

> “ᐊᑐᕈᑎᖃᕐᓗᑎᒃ PBR ᐊᐅᓚᑕᐅᓂᖃᑦᑎᐊᕐᓗᑎᒃ ᐊᖑᓇᓱᒃᑕᐅᔪᓐᓇᕐᓂᖏᓐᓄᑦ ᐋᖅᑭᒃᓯᒪᓂᕐᒥᒃ (Wade 1998) ᑕᒪᒃᑯᓄᖓ ᐊᑦᑕᓇᖅᑐᒦᑦᑐᓄᑦ ᓄᖑᑉᐸᓪᓕᐊᑐᐃᓐᓇᖃᔭᕐᓂᖏᓐᓄᑦ ᑲᑎᓯᒪᔪᓄᑦ, ᓄᖑᕋᐃᕙᓪᓕᐊᓂᖃᖏᓪᓗᑎᒃ ᐊᖑᓇᓱᒃᑕᐅᑎᑦᑎᖃᑦᑕᕈᑎᒋᓂᐊᖅᑕᖏᑦ 0.6 ᐊᕐᕖᑦ/ᐅᑭᐅᒥ ᐊᑕᐅᓯᕐᒥ ᐅᕙᓘᓐᓃᑦ 1 ᐊᕐᕕᒃ ᐅᑭᐅᒃ ᒪᕐᕈᒃ ᐊᓂᒍᕐᓂᑕᒫᖅ ᐊᑐᕈᑕᐅᖁᔭᐅᓚᐅᖅᑐᖅ. ᑭᓯᐊᓂᓕ, ᒫᓐᓇᒥᑦ ᖃᐅᔨᔭᐅᓯᒪᓕᖅᑐᒃᑯᑦ ᑲᑎᓯᒪᔪᑦ ᐊᒥᓲᓂᖏᑦᑎᒍᑦ ᓇᓗᓇᖏᔾᔪᑕᐅᓕᖅᐳᖅ ᑕᒪᒃᑯᐊ ᐊᑦᑕᓇᖅᑐᒦᑦᑐᑦ ᓄᖑᑕᐅᑐᐃᓐᓇᕆᐊᖃᕐᓂᖏᓐᓄᑦ ᐊᒥᓲᓂᖏᑦ ᑖᒃᑯᓄᖓ ᐊᕐᕕᖕᓄᑦ ᑲᑎᓯᒪᔪᓄᑦ ᓈᒻᒪᖏᔾᔪᑕᐅᓕᕋᔭᖅᑐᖅ, ᐊᒻᒪᓗ ᑖᒃᑯᐊ ᑲᑎᓯᒪᔪᑦ ᐊᒥᓲᓂᖏᑦ ᖃᐅᔨᓴᖅᑕᐅᒃᑲᓐᓂᕆᐊᖃᕐᓗᑎᒃ”.

ᐊᖏᕈᑕᐅᓕᓚᐅᖅᐳᖅ ᐊᑐᕈᑎᖃᕈᒪᓪᓗᑎᒃ ᑖᔅᓱᒥᖓ ᐅᖃᐅᓯᐅᔪᒥᒃ. ᓄᓇᓕᖕᒥᐅᑕᑦ ᑲᑎᒪᖃᑕᐅᔪᑦ ᓇᓗᓇᐃᖅᓯᒪᓂᖃᑦᑎᐊᖁᔨᕗᑦ ᑖᓐᓇ ᓈᒻᒪᒋᙱᑦᑕᖓᑦ ᑎᑎᖅᓯᒪᔪᖅ ‘ᐊᑦᑕᓇᖅᑐᒦᑦᑐᓄᑦ ᓄᖑᑉᐸᓪᓕᐊᑐᐃᓐᓇᖃᔭᕐᓂᖏᓐᓄᑦ’ ᐊᒥᓲᓂᖏᑦ ᓇᓗᓇᐃᖅᑕᐅᓯᒪᔾᔪᑎᖏᓐᓂᒃ ᐊᒻᒪᓗ ᑕᐅᑐᖕᓂᕆᔭᖏᑦᑎᒍᑦ ᑖᒃᑯᐊ ᑲᑎᓯᒪᔪᑦ ᑎᑎᖃᑕᐅᓯᒪᔪᒥᑦ ᐲᖅᑕᐅᓯᒪᓂᖓ ᓇᓗᓇᐃᖅᑕᐅᓯᒪᑦᑎᐊᕆᐊᖃᕐᓂᖏᓐᓂᑦ ᑕᕝᕙᓂ ᐊᕐᕖᑦ ᑲᑎᓯᒪᔪᑦ ᖃᓄᐃᓕᖓᓕᕐᓂᖏᓐᓂᒃ ᐅᓂᒃᑳᓕᐊᖑᔪᖅ (SSR). ᐅᖃᓚᐅᕐᒥᔪᑦ ᑕᒫᓃᑉᐸᒃᑐᑦ Hᐊᑦᓴᓐ ᐸᐃ ᐅᐊᖕᓇᖅᐸᓯᐊᓂ ᐊᒻᒪᓗ ᓴᓂᕋᔭᐅᑉ ᐃᒪᖓᑕ ᐅᐊᑉᓇᖅᐸᓯᐊᓃᑉᐸᒃᑐᑦ ᓇᓗᓇᐃᖅᑕᐅᓯᒪᒋᐊᖃᕐᓂᖏᓐᓂᒃ ᐊᑕᐅᓯᕐᒥᐅᑕᐅᓂᖏᓐᓂᒃ ᑖᒃᑯᐊ ᐊᕐᕖᑦ. ᐊᑕᐅᓯᖅ ᑲᑎᒪᖃᑕᐅᔪᖅ ᐅᖃᓚᐅᖅᑐᖅ ᑕᒪᒃᑯᐊᒎᖅ ᐊᕐᕖᑦ ᐃᓐᓇᐃᑦ ᐊᖑᑎᑦ ᐊᒻᒪᓗ ᕿᑐᕐᖓᖃᙱᑦᑐᑦ ᐊᕐᓇᐃᑦ, ᑕᑯᔭᐅᖃᑦᑕᙱᑦᑐᑦ ᑕᒫᓂ ᓴᓂᕋᔭᐅᑉ ᐃᒪᖓᑕ ᐅᐊᖕᓇᖅᐸᓯᐊᓂᑦ ᐊᓯᐊᓃᖃᑦᑕᕋᔭᕐᓂᖏᓐᓂᒃ ᓇᓗᓇᖏᔾᔪᑕᐅᓕᖅᑐᖅ ᑕᐃᒪᓕ ᑕᒫᓂᓐᓇᓱᒋᔭᐅᖃᑦᑕᕋᔭᖅᐳᑦ Hᐊᑦᓴᓐ ᐸᐃᒥᑦ. ᓄᓇᕗᒻᒥᑦ ᐆᒪᔪᓕᕆᔨᕐᔪᐊᒃᑯᑦ ᑲᑎᒪᔨᖏᓐᓂᖔᖅᑐᑦ ᐅᖃᓚᐅᖅᑐᑦ ᑭᖑᓪᓕᖅᐸᖑᓚᐅᖅᑐᖅ ᐅᓂᒃᑳᓕᐊᖑᓚᐅᖅᑐᒥᑦ ᐃᓄᐃᑦ ᐊᕐᕕᖕᓂᒃ ᖃᐅᔨᒪᔭᑐᖃᖏᑦᑎᒍᑦ ᐅᓂᒃᑳᓕᐊᖑᓚᐅᖅᑐᒥᑦ, ᓄᓇᙳᐊᖅᑕᖃᓚᐅᕐᒪᑦ ᓇᓗᓇᐃᖅᓯᒪᓂᖃᖅᓗᓂᓗ ᐊᕐᕖᑦ ᐅᑎᖅᑕᕐᕕᒋᕙᒃᑕᖏᑦᑕ ᐊᖅᑯᑎᖏᓐᓂᒃ. ᑕᒪᒃᑯᐊ ᐅᑎᖅᑕᕐᕕᒋᔭᐅᓇᓱᒋᔭᐅᔪᑦ ᐊᖅᑯᑎᖏᑦ ᑲᑎᓯᒪᓂᖃᖅᐳᑦ Hᐊᒃᓴᓐ ᐸᐃᒥᑦ ᐊᒻᒪᓗ ᓴᓂᕋᔭᐅᑉ ᐃᒪᖓᑕ ᐅᐊᖕᓇᖅᐸᓯᐊᓂᑦ, ᐊᒻᒪᓗ ᖃᐅᔨᒪᔾᔪᑕᐅᒃᑲᓐᓂᓕᖅᓗᓂ ᑖᒃᑯᐊ ᐊᕐᕖᑦ ᐊᑕᐅᓯᕐᒥᐅᑕᑦ ᐃᓂᖅᖃᑦᑕᕐᓂᖏᓐᓂᒃ ᐊᐅᔭᒃᑯᑦ ᒪᕐᕈᐃᖑᔪᓂᒃ.

ᑕᒪᕐᒥᒃ ᑲᑎᒪᖃᑕᐅᔪᑦ ᐊᖏᖃᑎᒌᓚᐅᖅᐳᑦ ᑖᒃᑯᐊ ᓄᓇᕗᒻᒥᑦ ᐆᒪᔪᓕᕆᔨᕐᔪᐊᒃᑯᑦ ᑲᑎᒪᔨᖏᑦ ᐱᖁᔨᒋᐊᖃᕐᓂᖏᓐᓂᒃ ᕿᒥᕐᕈᖁᔨᓗᑎᒃ ᑖᒃᑯᓂᖓ COSEWIC, ᑲᑎᒪᔨᕋᓛᑦ ᓇᓗᓇᐃᖅᑕᐅᓯᒪᓕᖅᑐᓄᑦ ᐊᑦᑕᓇᖅᑐᒦᓐᓂᖏᓐᓄᑦ ᐆᒪᔪᐃᑦ ᑲᓇᑕᒥ. ᖃᐅᔨᓇᓱᒡᓗᑎᒃ ᖃᓄᐃᑦᑑᕐᓂᖏᓐᓂᒃ ᓄᓇᕗᒻᒥᑦ ᐊᕐᕖᑦ, ᐱᔾᔪᑎᒋᓪᓗᒍ ᓇᓗᓇᐃᖅᓯᒪᓂᖅᑕᖃᑦᑕᕐᓂᖅᓴᓂᒃ ᐊᒥᓲᓂᖏᓐᓂᒃ ᖁᐊᐊᖅᐸᓪᓕᓂᖅᑕᖃᑦᑕᐅᕐᓂᖏᓐᓂᒃ ᐅᑭᐅᑦ 30 ᐃᓗᐊᓂᑦ ᐊᒻᒪᓗ ᐃᒪᕐᒥᐅᑕᓕᕆᔨᒃᑯᑦ ᐅᓄᕐᓂᕆᔭᐅᑎᑕᖏᑦ 50 ᐳᓴᓐᒋᔭᐅᑎᓪᓗᒋᑦ ᐅᓄᕐᓂᕆᔭᐅᓇᓱᒋᔭᐅᓚᐅᖅᑐᓄᑦ. ᓂᕆᐅᒋᔭᐅᕗᖅ ᓯᕗᓂᒃᓴᒥᑦ ᑲᑎᓯᒪᕙᒃᑐᑦ ᓇᓗᓇᐃᖅᑕᐅᓯᒪᓂᖏᑦᑎᒍᑦ ᖃᐅᔨᓴᕈᑕᐅᓂᖏᑦ ᓇᓗᓇᐃᕈᑕᐅᔪᓐᓇᖃᔭᕐᓂᖓᓂᒃ ᐊᑕᐅᓯᕐᒥᐅᑕᐅᒋᐊᒃᓴᖏᓐᓂᒃ ᐅᕙᓘᓐᓃᑦ ᐅᖓᑖᓂᒥᐊᒃᓴᖏᓐᓂᑦ ᐊᕐᕖᑦ ᓄᓇᕗᑦ ᐃᓗᐊᓂᑦ ᐊᒻᒪᓗ ᑖᓐᓇ ᓄᑖᖅ ᖃᐅᔨᒪᔾᔪᑕᐅᓂᐊᖅᑐᖅ ᐃᓚᓕᐅᔾᔭᐅᔾᔪᑕᐅᒋᐊᒃᓴᖅᓗᓂ ᑖᔅᓱᒧᖓ ᐋᖅᑭᒋᐊᒃᑲᓐᓂᕈᑕᐅᓗᓂ ᐊᕐᕖᑦ ᑲᑎᓯᒪᔪᑦ ᖃᓄᐃᓕᖓᓕᕐᓂᖏᓐᓂᒃ ᐅᓂᒃᑳᓕᐊᖑᔪᓂᑦ (SSR).

ᓇᓗᒋᔭᐅᔪᓄᑦ ᑐᕌᖓᔪᑦ

ᑎᑎᕋᕐᖓᐅᑕᐅᓯᒪᔪᖅ ‘ᖃᐅᔨᓇᓱᒍᑕᐅᔪᓄᑦ ᓇᓗᓇᕈᑕᐅᓂᖏᑦ’ ᑎᑎᕋᖅᓯᒪᔪᖅ ᕿᒥᕐᕈᔭᐅᓚᐅᖅᑐᖅ. ᓲᓴᓐ ᑲᓯᓐᔅ ᓇᓗᓇᐃᔭᓚᐅᖅᑐᖅ ᖃᖓᑕᓲᒃᑯᑦ ᖃᐅᔨᓴᕈᑕᐅᕙᒃᑐᑦ ᖃᔅᓯᐅᓇᓱᒋᔭᐅᓂᖏᓐᓄᑦ ᖃᐅᔨᔾᔪᑕᐅᔪᓐᓇᑦᑎᐊᖏᓐᓂᖏᓐᓂᒃ ᑕᒪᒃᑯᐊ ᑲᑎᓯᒪᕙᒃᑐᑦ ᐱᔾᔪᑎᒋᓪᓗᒍ ᐃᓚᖏᑦ ᐳᐃᓯᒪᔫᒐᓗᐊᑦ ᑕᑯᔭᐅᔪᓐᓇᖃᑦᑕᖏᓐᓂᖏᓐᓄᑦ ᐊᒻᒪᓗ ᐊᓯᖏᑦ ᐊᕐᕖᑦ ᐊᖅᑲᐅᒪᖃᑦᑕᕐᓂᖏᓐᓄᑦ ᐊᒻᒪᓗ, ᑭᖑᓪᓕᕐᒥ, ᐃᓚᖏᑦ ᐊᕐᕖᑦ ᖃᐅᔨᓴᕐᕕᐅᔫᑉ ᐊᕙᑖᑕ ᓯᓚᑖᓃᓐᓂᖏᓐᓄᑦ ᑕᑯᔭᐅᐊᓐᓇᖃᑦᑕᖑᓗᑎᒃ. ᑲᑎᒪᖃᑕᐅᔪᑦ ᐅᖃᓚᐅᖅᑐᑦ 80%-ᓗᐊᖑᔪᑦ ᑕᑯᔭᐅᓯᒪᓕᖅᑐᑦ ᓴᓂᕋᔭᐅᑉ ᐃᒪᖓᓂᑦ ᒪᒃᑯᒃᑐᐃᓐᓇᐅᓂᖏᓐᓂᒃ ᐊᕐᕖᑦ ᑕᐃᒪᓕ ᐃᓐᓇᐅᓂᖏᑦ ᐊᓯᐊᓃᓐᓇᓱᒋᔭᐅᔪᑦ. ᑕᑯᔭᐅᓯᒪᔪᑦ 75 ᐳᓴᓐᑎᒥᑦ ᑖᒪᓂ Hᐊᑦᓴᓐ ᐸᐃ ᐅᐊᖕᓇᖅᐸᓯᐊᓂᑦ ᓈᓴᖅᑕᐅᓯᒪᓂᖃᑦᑎᐊᖏᑦᑐᒃᓴᐅᒋᕝᑦ ᐃᓐᓇᐅᓂᖏᑦ ᓇᓕᒧᒌᓕᕐᓂᖅᓴᐅᓂᖏᓐᓄᑦ ᖃᐅᔨᒪᔾᔪᑕᐅᓕᕈᑕᐅᓗᑎᒃ ᑕᒪᒃᑯᓄᖓ ᒪᒃᑯᒃᑐᑦ ᐊᒥᓲᓂᖏᓐᓄᑦ ᑖᒪᓂᑉᐸᒃᑐᓄᑦ ᓴᓂᕋᔭᐅᑉ ᐃᒪᖓᑕ ᐅᐊᖕᓇᖓᓂᑉᐸᒃᑐᑦ. ᓲᓴᓐ ᑲᓯᓐᔅ ᐅᖃᓚᐅᖅᑐᖅ ᑕᒪᒃᑯᐊ ᓇᓗᓈᕿᔾᔪᑕᐅᔪᑦ ᓈᓴᖅᑕᐅᓯᒪᓂᖃᑦᑎᐊᖏᓐᓂᖏᓐᓂᒃ ᑕᑯᒃᓴᐅᔾᔭᑦᑕᐅᓕᖅᐳᑦ ᐊᒥᓲᓂᖏᓐᓂᒃ. ᐅᖃᓚᐅᕐᒥᔪᖅ ᐊᐱᖅᑯᑕᐅᔪᑦ ᐱᔾᔪᑕᐅᑎᓪᓗᒋᑦ ᐆᒃᑐᕋᐅᑕᐅᓪᓗᑎᒃ ᖃᐅᔨᓴᕈᑕᐅᔪᑦ ᑕᑭᔪᑦ ᓯᓚᖕᓂᖏᑦᑎᒍᑦ ᓈᓴᕈᑕᐅᓚᐅᖅᑐᑦ ᓇᓗᓇᕐᒥᔭᑦᑕᐅᓂᖏᓐᓂᑦ ᐃᓚᖏᑦ ᐅᖓᑎᓗᐊᖓᓄᑦ ᓈᓴᕈᑕᐅᓯᒪᓕᕐᓯᒪᓂᖏᓐᓂᒃ. ᓇᓗᓇᐃᔭᖅᓯᒪᔪᑦ ᐃᓚᖏᑦ ᖃᐅᔨᓴᕈᑕᐅᓯᒪᔪᑦ ᐱᓕᕆᐊᖃᖅᑐᑦ ᐊᓯᖏᓐᓂᒃ ᐊᐅᓪᓚᖅᑐᓂ ᓄᓇᕗᑦ ᓯᓚᑖᓄᑦ.

ᑲᑎᒪᖅᑕᐅᔪᑦ ᐅᖃᓚᐅᖅᑐᑦ ᑕᒪᒃᑯᐊ ᐱᔪᓐᓇᖅᑐᑦ ᑲᑎᓯᒪᕙᒃᑐᑦ ᓇᓗᓇᐃᖅᑕᐅᓯᒪᓂᖏᓐᓂ (ᓲᕐᓗ ᐆᒃᑑᑎᒋᓗᒍ, ᓴᓂᕋᔭᐅᑉ ᐃᒪᖓᓂᑉᐸᒃᑐᑦ ᐊᒻᒪᓗ Hᐊᑦᓴᓐ ᐸᐃ ᐃᒪᖓᓂᑉᐸᒃᑐᑦ ᐊᕐᕖᑦ) ᓱᕋᒃᑕᐅᓯᒪᙱᑦᑐᖅ. ᐅᖃᒃᑲᓂᓚᐅᖅᑐᑦ ᖃᐅᔨᓴᖅᑕᐅᓂᖏᑦ ᐱᑕᖃᙱᓐᓂᖓᓂᒃ ᖁᑦᑎᒃᑑᑉ ᐃᒪᖓᓂᑉᐸᒃᑐᓂᒃ ᐊᕐᕕᖕᓂᒃ ᐊᒻᒪᓗ ᖃᓄᖅ ᐃᓚᒋᔭᐅᓂᖏᑦ ᑕᒪᒃᑯᓇᖓᑦ ᑲᑎᓯᒪᕙᒃᑐᓂᒃ ᐃᒪᖓᓂᑉᐸᒃᑐᓄᑦ ᐊᕐᕖᑦ ᓇᓗᓇᖅᑐᒥᒃ ᖃᐅᔨᓴᕈᑎᖃᕐᓂᒃᑲᓂᕐᓂᖅᓴᒥᒃ ᑖᒃᑯᓇᙵᑦ. ᐅᖃᐅᓯᖅᑲᓂᓪᓚᐅᖅᑐᑦ ᐃᓕᓐᓂᐊᕈᑕᐅᓂᖏᓐᓂᒃ ᑕᑯᔭᐅᔪᓐᓇᖅᑐᑦ ᐊᒻᒪᓗ Hᐊᑦᓴᓐ ᐸᐃ ᐅᐊᓕᓂᖕᒐᔭᐅᓪᓚᖅᑐᑦ ᐊᓯᐊᓂ ᑭᓯᐊᓂ ᓄᑖᓂᒃ ᖃᓄᑦᑎᓐᓂᖅᑎᓯᖃᕐᓂᐅᓇᔭᖅᑐᓂ ᐅᖅᑐᑦᑕᖃᐅᒻᒪᕆᒃᑐᑦ.

ᓄᓇᓕᖕᒥᐅᓂᒻᒪᕆᖅᑐᑦ ᑲᑎᒪᖅᑕᐅᔪᑦ ᓇᓗᓇᐃᓚᐅᖅᐳᑦ ᖃᓄᖅ ᐱᒪᕆᐅᓂᖓᓂᒃ ᐃᓕᖅᑯᓯᖅᑐᕐᕆᐊᖅᑐᐃᓐᓇᐅᓂᖓᓄᑦ ᐊᖑᓇᓱᖕᓂᐅᓯᐅᖅᑐᑦ ᐃᓄᖕᓄᑦ ᓄᓇᕗᒻᒥᑦ. ᑕᒪᒃᑯᐊ ᑕᐅᑐᒃᑕᐅᔪᑦ ᓇᐃᓪᓕᒋᐊᖅᓯᒪᔪᑎᒍᑦ ᑎᑎᕋᖅᑕᐅᓯᒪᔫᑉ ᑖᒃᑯᐊ ᐊᕙᓗᔅᓯᒪᓂᖏᓐᓂᒃ ᑕᐅᑐᒍᑎᓕᐅᑐᐃᓐᓇᖅᑐᓂᒃ ᐅᓂᒃᑲᓂᐊᖑᐊᒃᑲᓐᓂᕐᒥᒃ. ᓄᓇᓕᖕᒥᐅᓂᒻᒪᕆᖅᑐᑦ ᑲᑎᒪᖅᑕᐅᔪᑦ ᐅᖃᖅᑳᓐᓂᓚᐅᖅᐳᑦ ᑕᐅᑐᙱᓐᓂᖅᓴᒥᒃ ᑕᒪᒃᑯᐊ ᐊᑦᑕᓇᖅᑐᒥᒃ ᓄᖑᑕᐅᑐᐃᓐᓇᓕᕆᐊᖃᖅᓯᒪᔪᑦ ᑕᐃᒪ ᑲᑎᒪᔪᓂᒃ ᑎᑎᕋᖅᑕᐅᓯᒪᓂᖓ ᐲᖅᑕᐅᕆᐊᖃᖅᓯᒪᓂᖓᓂᒃ.

ᖃᓄᐃᓕᖓᓂᖓᓐᓄᑦ ᕿᒥᕐᕈᓂᖅ

ᑎᑎᕋᖅᑲᐅᑕᐅᔪᖅ ‘ᑕᐅᑐᒃᑕᐅᓂᖓ’ ᑎᑎᕋᖅᓯᒪᔪᖅ ᕿᒥᕐᕈᔭᐅᓚᐅᖅᐳᖅ. ᓲᕐᓗ ᑲᓯᐊᓐᔅ ᐅᖃᓚᐅᖅᑐᖅ ᐃᓄᐃᑦ ᖃᐅᔨᒪᔭᖏᑦ ᐱᖁᔭᖅᑎᒍᑦ ᐃᓪᓗᐊᓂᒃ ᖃᐅᔨᓴᖅᑕᐅᔪᒥᒃ ᓇᓗᓇᕐᕈᔾᔭᐅᓂᖓᓂᒃ ᑕᒪᒃᑯᐊ ᐊᕐᕖᑦ ᐊᒥᓱᓪᓗᐊᖅᓯᒪᓂᖏᓐᓂᒃ. ᑭ. Hᐃ ᐅᖃᓚᐅᖅᑐᖅ ᑎᑎᕋᖅᓯᒪᓂᖓ ᓇᓗᓇᐃᖅᓯᒪᓂᖅᓴᐅᓂᖓ ᖃᓄᐃᓕᖓᓂᖏᓐᓂᒃ ᓇᓗᓇᐃᔭᖅᓯᒪᓂᖏᓐᓂᒃ, ᐅᑦᑐᕆᓂᖓ ᐊᒻᒪᓗ ᖃᖓ ᐃᓚᐅᑦᑎᐊᕆᐊᖃᕐᓂᖏᓐᓂᒃ, ᐱᔭᐅᑉᐸᑎᒃ ᑐᓴᖅᑕᐅᓂᕈᓐᓇᕐᒥᑦ ᓄᓇᕗᒻᒥ ᐆᒪᔪᓕᕆᔨᒃᑯᑦ ᑲᑎᒪᔨᖏᓐᓄᑦ ᐃᓄᐃᑦ ᖃᐅᔨᒪᔭᑐᖃᖏᓐᓂᒃ ᐊᕐᕕᖕᓂᒃ ᖃᐅᔨᓴᖅᑕᐅᓚᐅᖅᑐᓂᒃ.

ᐃᒻᒥᒃᑰᖅᑐᖅ ᖃᐅᔨᓴᖅᑎ ᐅᖃᓚᐅᕐᒥᔪᖅ ᑕᒪᒃᑯᐊ ᐊᕐᕖᑦ ᐱᐊᕋᓐᓂᖏᑦ ᖃᐅᔨᓴᖅᑕᐅᒍᑎᒃ ᐊᑐᖅᑕᐅᒪᓂᕈᓐᓇᕐᓂᖏᓐᓂᒃ ᖃᐅᔨᓴᕐᓂᖃᑦᑕᕐᕕᐅᓗᑎᒃ ᐊᓯᔾᔨᕐᓂᐅᕙᑦᑕᐅᓂᖏᓐᓂᒃ ᖃᓄᐃᓕᖓᓂᖃᑦᑕᕐᓂᖏᓐᓂᒃ ᑖᒃᑯᓇᖓᑦ ᑲᑎᒪᔪᓄᑦ ᐊᕐᕖᕝᓄᑦ.

ᐊᓯᖏᑦ ᐃᓱᒪᒃᓴᖅᓯᐅᕈᑕᐅᔪᑦ

ᑲᑎᒪᖅᑕᐅᔪᑦ ᐅᖃᐅᓯᖃᕐᓂᐊᕐᒥᔪᑦ ᐊᓯᖏᓐᓂᒃ ᐱᔭᐅᔪᓐᓇᖅᑐᓂᒃ ᐊᒃᑐᐊᑕᐅᓗᑎᒃ ᐊᕙᓗᕐᓂᓗ ᖃᐅᔨᓴᖅᑕᐅᓂᖏᓐᓂᒃ. ᓄᓇᓕᖕᒥᐅᓂᑯᒻᒪᕆᖅᑐᑦ ᑲᑎᒪᖅᑕᐅᔪᑦ ᐅᖃᓚᐅᖅᑐᑦ ᑕᒪᒃᑯᐊ ᐋᓗᐃᑦ ᑕᑯᔭᐅᖃᑦᑕᓕᖅᑐᑦ ᑕᒪᓂ ᓴᓂᕋᔭᒥᑦ ᐊᒻᒪᓗ Hᐊᑦᓴᓐ ᐸᐃ ᐅᐊᓕᓂᖕᒐᓂᑦ. ᐋᓗᐃᑦ ᑕᑯᔭᖅᑕᕐᓂᖏᑦ ᐅᓚᑐᕐᓂᐅᓂᖅᓴᐅᓕᖅᑐᑦ Hᐊᑦᓴᓐ ᐸᐃ ᐅᐊᓕᓂᖕᒐᓂ ᑖᓐᓇᓂᐅᓂᖓᓂ ᓴᓂᕋᔭᒥᑦ ᐃᒪᖓᓂ. ᓄᓇᓕᖕᒥᐅᓂᑯᒻᒪᕆᖅᑐᑦ ᑲᑎᒪᖅᑕᐅᔪᑦ ᐅᖃᓕᕐᒥᔪᑦ ᑕᕝᕙᓐᓇᕈᑎᒋᔭᐅᓂᖅᑐᑦ ᐃᓗᓕᖓᓂᖏᑦ ᖃᓂᒋᔭᖏᑦ. ᓄᓇᕗᒻᒥ ᐆᒪᔪᓕᕆᔨᒃᑯᑦ ᑲᑎᒪᔨᖏᑦ ᐅᖃᓚᐅᖅᑐᑦ ᑖᓐᓇ ᐃᓄᐃᑦ

ᖃᐅᔨᒪᔭᑐᖃᖏᑦᑎᒍᑦ ᐊᕐᕕᖕᓂᒃ ᖃᐅᔨᓴᕈᑕᐅᔪᖅ ᐅᓂᒃᑳᖅᓯᒪᓂᖃᖅᐳᖅ ᑕᒪᒃᑯᐊ ᐋᕐᓗᐃᑦ ᐱᓇᓱᖃᑦᑕᕐᓂᖏᓐᓂᒃ ᐊᕐᕕᖕᓂᒃ. ᐅᖃᐅᓯᖃᓚᐅᕐᒥᔪᑦ ᖃᓄᖅ ᑕᒪᒃᑯᐊ ᐋᕐᓗᐃᑦ ᐊᕐᕕᖕᓂᒃ ᓯᒡᔭᒧᑦ ᐊᔪᖅᓯᑎᑦᑎᖃᑦᑕᕋᔭᕐᓂᖏᓐᓂᒃ. ᑐᖁᖓᓕᖅᑐᑦ ᐊᕐᕖᑦ ᓇᓂᔭᐅᔪᕕᓃᑦ ᖃᐅᔨᔭᐅᔪᓐᓇᖏᑦᑐᑦ ᑭᓱᒧᑦ ᑐᖁᔾᔪᑎᖃᕆᐊᒃᓴᖏᓐᓂᒃ ᑭᓯᐊᓂ ᐃᓄᐃᑦ ᐅᖃᕋᔪᒃᐳᑦ ᐋᕐᓗᓄᑦ ᑕᐃᒪᓐᓇᐃᑎᑕᐅᓂᕋᖅᓗᒍ. ᑲᑎᒪᖃᑕᐅᔪᑦ ᐃᓱᒪᓚᐅᖅᐳᑦ ᐋᕐᓗᐃᑦ ᐱᓇᓱᖃᑦᑕᕐᓂᖏᓐᓄᑦ ᐊᕐᕕᖕᓂᒃ ᑐᖁᕋᕈᑕᐅᖃᑦᑕᕋᓱᒋᓪᓗᓂᔾᔪᒃ. ᑲᑎᒪᖃᑕᐅᑦ ᐅᖃᓚᐅᕐᒥᔪᑦ ᓯᑯᓄᑦ ᐊᔪᖅᓯᓐᓂᖏᓐᓄᑦ ᑐᖁᕋᕈᑕᐅᓗᐊᖃᑦᑕᕋᔭᕐᓂᖏᓐᓂᒃᑕᐅᖅ.

ᐊᐅᓚᔾᔪᑕᐅᓇᔭᕐᓂᖏᓐᓄᑦ ᐃᓱᒪᓕᐅᕈᑕᐅᔪᑦ

ᑎᑎᕋᕐᖓᐅᑕᐅᔪᖅ ‘ᐊᐅᓚᔾᔪᑕᐅᓇᔭᕐᓂᖏᓐᓄᑦ ᐃᓱᒪᓕᐅᕈᑕᐅᔪᑦ’ ᑎᑎᕋᕐᓯᒪᓂᖓ ᕿᒥᕐᕈᔭᐅᓚᐅᖅᑐᖅ. ᑕᐅᑐᒍᑎᖃᕐᓂᖅᓴᐅᓚᐅᖅᑐᑦ ᐊᒃᑐᕈᑕᐅᔾᔪᑕᐅᓇᔭᖅᑐᓂᒃ ᐊᖑᓇᓱᖕᓂᕐᒧᑦ ᑕᒫᓂ ᓴᓂᕋᔭᐅᑉ ᐃᒪᖓᑕ ᐅᐊᖕᓇᖓᓂ ᐱᔾᔪᑎᒋᓪᓗᒍ ᐃᓐᓇᐃᑦ ᑕᒫᓃᑉᐸᒃᑐᑦ ᐊᕐᓇᐅᑐᐃᓐᓇᕆᐊᖃᕐᒪᑕ ᐱᐊᕋᖃᕐᓗᑎᒃ. ᓄᓇᓕᖕᓂᕐᒥᐅᓃᖓᖅᑐᑦ ᑲᑎᒪᖃᑕᐅᒧᑦ ᐅᖃᓚᐅᖅᑐᑦ ᑖᓐᓇ ᐊᕐᕖᑦ ᑲᑎᓯᒪᔪᑦ ᖃᓄᐃᓕᖓᓕᕐᓂᖏᓐᓂᒃ ᐅᓂᒃᑳᓕᐊᖑᔪᖅ (SSR). ᑎᑎᕋᖅᓯᒪᓂᖃᕆᐊᖃᕐᓂᖓᓂᒃ ᐃᓄᐃᑦ ᐊᖑᓇᓱᒍᒪᓂᕋᖅᑕᐅᓗᑎᒃ ᐊᕐᕕᖕᓂᑦ “ᐱᖃᑎᖃᖏᑦᑐᓂᒃ” ᐱᐊᕋᓂᒃ.

ᐅᒥᐊᕐᔪᐊᑦ ᐊᒃᑐᐃᓂᖃᖃᑦᑕᕋᔭᕐᓂᖏᓐᓄᑦᑕᐅᖅ ᐅᖃᐅᓯᐅᓚᐅᕐᒥᔪᖅ ᑭᓯᐊᓂᓕ ᐊᖏᕈᑕᐅᓚᐅᖅᐳᖅ ᒫᓐᓇᐅᔪᒥᑦ ᐊᒃᑐᐃᓂᖃᖏᓯᒪᑲᓚᐅᕐᓂᖏᓐᓂᒃ. ᒫᓐᓇᐅᔪᒥᑦ ᐊᕙᑎᓕᕆᓂᕐᒧᑦ ᖃᐅᔨᓴᕈᑕᐅᕙᓪᓕᐊᔪᑎᒍᑦ ᑕᑯᒃᓴᐅᓂᖃᖅᐳᖅ ᐊᒃᑐᕈᑕᐅᑐᐃᓐᓇᕆᐊᖃᖅᑐᓂᒃ ᐊᕐᕕᖕᓄᑦ ᐊᐅᔭᒃᑯᑦ ᐅᐸᒃᓯᒪᕙᒃᑕᖏᓐᓂᑦ.

ᐅᖃᐅᓯᑦ ᑭᖑᓪᓕᖅᐸᑦ

ᑲᑎᒪᖃᑕᐅᔪᑦ ᐊᖏᕈᑎᖃᓚᐅᖅᐳᑦ ᑎᑎᕋᖅᑕᐅᒃᑲᓐᓂᖅᓯᒪᔪᖅ ᐊᕐᕖᑦ ᑲᑎᓯᒪᔪᑦ ᖃᓄᐃᓕᖓᓕᕐᓂᖏᓐᓂᒃ ᐅᓂᒃᑳᓕᐊᖑᔪᖅ (SSR) ᐊᒻᒪᓗ ᑲᑎᒪᔾᔪᑕᐅᕙᓪᓕᐊᔪᑦ ᐃᓄᒃᑎᑑᓕᖅᑎᑕᐅᓂᐊᖅᑐᑦ ᐊᒻᒪᓗ ᐊᐅᓪᓚᖅᑎᑕᐅᓗᑎᒃ ᓄᓇᕗᒻᒥᑦ ᐆᒪᔪᓕᕆᔨᕐᔪᐊᒃᑯᑦ ᑲᑎᒪᔨᖏᓐᓄᑦ, ᐊᖑᓇᓱᒃᑐᓕᕆᔨᒃᑯᓐᓄᓪᓗ ᐊᒻᒪᓗ ᓄᓇᓂᒃ ᐊᕕᒃᑐᖅᓯᒪᔪᓂᑦ ᐆᒪᔪᓕᕆᔨᒃᑯᓐᓂᑦ ᑭᒡᓕᖅᑐᐃᔪᓂᒃ ᓴᓂᕋᔭᐅᑉ ᐃᒪᖓᓂᑦ ᐊᒻᒪᓗ ᐅᐊᖕᓇᖓ Hᐊᑦᓴᓐ ᐸᐃ ᐃᒪᖓᓂᑦ. ᐱᖁᔭᐅᔪᑦ ᑭᒡᓕᖅᑭᑕᐅᓇᔭᖅᑐᑦ ᐃᒪᐃᑦᑑᖁᔭᕗᑦ:

1) ᐊᐅᓪᓚᖅᑎᓪᓗᒋᑦ ᑎᑎᖅᑲᑦ ᕿᒥᕐᕈᔭᐅᓂᐊᕐᓗᑎᒃ ᐊᐅᒍᔅ ᕿᑎᖓᓂᑦ
2) ᕿᒥᕐᕈᔭᐅᕌᓂᒃᐸᑕ ᐃᓱᒪᒋᔭᐅᓯᒪᔪᑦ ᑎᑎᕋᖅᓯᒪᔪᑦ ᐅᑎᖅᑎᑕᐅᓗᑎᒃ ᐃᒪᕐᒥᐅᑕᓕᕆᔨᒃᑯᓐᓄᑦ ᓯᑎᐱᕆ ᕿᑎᐊᓂ.

ᑎᑎᖅᑲᑦ ᐃᓱᒪᒃᓴᖅᓯᐅᕈᑕᐅᓂᐊᕐᒥᔪᑦ ᓄᓇᕗᒻᒥᑦ ᐆᒪᔪᓕᕆᔨᕐᔪᐊᒃᑯᑦ ᑲᑎᒪᔨᖏᑦ ᑲᑎᒪᒃᑲᓐᓂᓕᖅᐸᑕ ᓇᐅᔮᓂᑦ ᐊᐅᒍᔅ 23-ᖑᓕᖅᐸᑦ.

ᑲᑎᒪᔪᓄᑦ ᐃᒃᓯᕙᐅᑕᕆᔭᐅᔪᖅ ᖁᔭᓐᓇᒦᖅᑐᖅ ᑲᑎᒪᖃᑕᐅᔪᓄᑦ ᐊᒃᓱᕈᖅᓗᑎᒃ ᐱᓕᕆᑦᑎᐊᓚᐅᕐᓂᖏᓐᓄᑦ. ᖁᔭᓐᓇᒦᖅᑕᐅᒻᒪᕆᓚᐅᕐᒥᔪᖅ ᐅᓖᐱᑲ ᒪᐃᒃ ᑐᓵᐅᔪᓐᓇᓚᐅᕐᓂᖓᓄᑦ ᑲᑎᒪᑎᓪᓗᒋᑦ ᑕᐃᒃᑯᐊ ᑲᓐᑐᓰᑎᒍᑦ ᑐᓵᐅᓂᐊᖅᑑᒐᓗᐊᑦ ᖃᐃᔪᓐᓇᖏᑎᓪᓗᒋᑦ. ᑲᑎᒪᓂᖅ ᓄᖅᑲᖅᑎᑕᐅᓚᐅᖅᑐᖅ.

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ᓇᓂᓯᔾᔪᑎᖓ 1
ᑲᑎᒪᔾᔪᑎᒃᓴᑦ Hᐊᑦᓴᓐᐸᐃᒥᑦ/ᓴᓂᕋᔭᐅᑉ ᐃᒪᖓᓂᑦ ᐊᕐᕖ ᖃᐅᔨᓴᖅᑕᐅᓂᖏᓐᓄᑦ
ᑲᑎᒪᔾᔪᑕᐅᓂᖏᓐᓄᑦ
ᐱᐊᕆ ᕆᑦᓴᑦ ᐃᒃᓯᕙᐅᑕᕆᔭᐅᔪᓂ

ᔫᓐ 17-18, 1999

ᐸᕐᓇᐃᕕᒃ ᑲᑎᒪᕝᕕᐊᓂᑦ, ᐃᖃᓗᐃᑦ, ᓄᓇᕗᑦ

9:00 ᑐᒃᓯᐊᕐᓂᖅ

9:05 ᑲᑎᒪᔾᔪᑎᒃᓴᑦ ᕿᒥᕐᕈᔭᐅᓂᖏᑦ ᐊᒻᒪᓗ ᐊᑐᓕᖅᑕᐅᓂᖏᑦ

9:15 ᖃᐅᔨᒋᐊᖅᑎᑦᑎᓂᖅ — ᐱᐊᕆ ᕆᑦᓴᑦ

1) ᖃᓄᐃᓕᖓᓂᖏᑦ ᐅᓄᕐᓂᖏᓐᓄᑦ ᖃᐅᔨᓴᕈᑕᐅᔪᑦ
2) ᐱᓕᕆᐊᒃᓴᕆᔭᐅᔪᑦ ᐊᒻᒪᓗ ᑲᒪᒋᔭᒃᓴᕆᔭᐅᔪᑦ (ᐃᒪᕐᒥᐅᑕᓕᕆᔩᑦ ᐊᒻᒪᓗ ᑲᑎᒪᖅᑕᐅᔪᑦ)
3) ᐊᖏᕈᑕᐅᓂᖏᑦ ᐱᓕᕆᐊᖑᕙᓪᓕᐊᔪᑦ ᐊᒻᒪᓗ ᖃᓄᐃᓕᖓᓕᕐᓂᖏᓐᓄᑦ ᓄᓇᐃᑦ ᐃᒪᕐᑯᑎᖏᑦᑕ ᐊᕙᑖᓃᑦᑐᑦ ᐆᒪᔪᐃᑦ ᖃᐅᔨᓴᖅᑕᐅᕙᓪᓕᐊᓂᖏᓐᓄᑦ
4) ᑭᒃᑯᑐᐃᑦ ᐃᓱᒪᒋᔭᒥᓂᒃ ᐅᖃᐅᓯᖃᖅᑎᓪᓗᒋᑦ ᐱᔾᔪᑎᖃᖅᑎᓪᓗᒋᑦ ᐊᖏᕈᑕᐅᓂᐅᔪᒍᑦ ᑕᒪᑐᒍᖓ ᕿᒥᕐᕈᔭᐅᓯᒪᔪᒍᑦ

10:15 ᑳᐱᑐᕐᓇᖅ

10:30 ᐅᖃᐅᓯᖅᑐᖅ ᐊᒻᒪᓗ ᐅᖃᐅᓯᕐᓂᖓ ᐆᒪᔪᐃᑦ ᖃᔅᓯᐅᓂᖏᓐᓄᑦ
ᓇᓗᓇᐃᖅᑕᐅᓯᒪᔪᑦ ᐅᓂᒃᑲᓯᐅᓂᖏᓐᓂᒃ — ᓲᓴᓐ ᑯᓯᓐᔅ,
(Susan Cosens)
ᑲᑎᒪᖅᑕᐅᔪᓕᒫᓪᓗ

ᖃᓄᐃᓕᖓᔾᔪᑕᐅᓂᖏᑦ
ᐆᒪᔪᑦ ᖃᐅᔨᓴᖅᑎᓄᑦ ᖃᐅᔨᓴᖅᑕᐅᓂᖏᑦ
ᐊᖑᓇᓱᒃᑕᐅᓂᖏᑦ

12:00 ᐅᓪᓗᕈᒻᒥᑕᕐᕕᒃ

1:00 ᐅᖃᐅᓯᐅᔪᑦ ᑲᔪᓯᑎᑕᐅᓂᖏᑦ ᐅᖃᐅᓯᕐᓂᖓ ᐆᒪᔪᐃᑦ ᖃᔅᓯᐅᓂᖏᓐᓄᑦ
ᓇᓗᓇᐃᖅᑕᐅᓯᒪᔪᑦ ᐅᓂᒃᑳᓯᐅᓂᖏᓐᓂᒃ — ᑲᑎᒪᖅᑕᐅᔪᓕᒫᑦ

ᖃᐅᔨᔭᐅᓯᒪᓕᖅᑐᑦ ᓇᓄᓇᐃᖅᑕᐅᓂᐅᖏᑦ - ᐅᓄᕐᓂᖏᑦ ᓇᓄᓇᐃᖅᑕᐅᓂᖏᑦ ᓂ. ᐅᓄᕐᓂᖏᑦ ᐊᓯᔾᔨᖅᐸᓪᓕᐊᓂᖏᑦ

3:00 ᑳᐱᑐᕐᓇᖅ

3:15 ᓇᓄᓇᕈᑕᐅᓂᖏᑦ **ᑲᑎᒪᖃᑕᐅᔪᓕᒫᑦ**

5:00 ᑲᑎᒪᓂᐅᑉ ᓄᖅᑲᕐᓂᖓ

ᐅᓪᓗᑐᐃᓐᓇᖅ, (ᑕᓪᓕᕐᒥ) ᔫᓐ 18

9:00 ᑐᒃᓯᐊᕐᓂᖅ

9:05 ᐊᓯᖏᑦ ᐃᓱᒪᒋᔭᐅᓯᒪᔪᑦ **ᑲᑎᒪᖃᑕᐅᔪᓕᒫᑦ**

9:30 ᐊᐅᓚᑕᐅᓂᖏᓐᓄᑦ ᐃᓱᒪᒋᔭᐅᓯᒪᔪᑦ **ᑲᑎᒪᖃᑕᐅᔪᓕᒫᑦ**

10:00 ᑳᐱᑐᕐᓇᖅ

10:15 ᐅᖃᐅᓯᐅᔪᑦ ᑲᔪᓯᑎᑕᐅᓂᖏᑦ ᐱᔾᔪᑎᖃᖅᑎᓪᓗᒋᑦ ᐊᐅᒪᑕᐅᓂᖏᓐᓄᑦ
ᐃᓱᒪᒋᔭᐅᓯᒪᔪᑦ **ᑲᑎᒪᖃᑕᐅᔪᓕᒫᑦ**

11:30 ᑭᖑᓪᓖᑦ ᐅᖃᐅᓯᐅᒃᓴᕋᔭᐅᔪᑦ ᐊᒻᒪᓗ ᐅᖃᐅᓯᕋᔭᐅᔪᑦ **ᑲᑎᒪᖃᑕᐅᔪᓕᒫᑦ**

12:00 ᑲᑎᒪᓂᖅ ᐃᓱᓕᑎᑕᐅᓂᖓ

ᓇᓂᓯᔾᔪᑎᖓ 2

ᑲᑎᒪᖅᑲᑕᐅᔪᑦ ᑲᖏᖅᖢᐊᓗᑉ ᐃᒪᖓᓂ/ᓴᓂᕋᔭᐅᑉᓗ ᐃᒪᖓᓂ ᐊᕐᕚᑦ ᐃᒪᓐᓈᕈᑕᐅᒋᐊᖅᑐᑦ ᖃᓄᐃᓕᖓᓕᕆᐊᒃᓴᖓᓂᒃ ᑲᑎᒪᓂᐅᔪᓄᑦ

ᐱ. ᕐᑯᕙᖅ	(ᓄᓇᕗᑦ ᐆᒪᔪᓕᕆᔨᕐᔪᐊᒃᑯᖏᓐᓄᑦ ᐊᐅᓚᑦᑎᔨᑦ ᑲᑎᒪᔨᖏᓐᓂ)
ᕆ. ᒪᐃᒃ	(ᓄᓇᕗᑦ ᐆᒪᔪᓕᕆᔨᕐᔪᐊᒃᑯᖏᓐᓄᑦ ᐊᐅᓚᑦᑎᔨᑦ ᑲᑎᒪᔨᖏᓐᓂ, ᑐᓵᔨᐅᔪᖅ ᑲᑎᒪᔪᓄᑦ)
W. ᓇᑯᓛᖅ	(ᓴᓪᓕᕐᒥ ᐆᒪᔪᓕᕆᔨᒃᑯᑦ ᑲᑎᒪᔨᖏᓐᓂᑦ)
J. ᐸᓪᓗᖅ	(ᐃᒡᓗᓕᐅᑉ ᐆᒪᔪᓕᕆᔨᒃᑯᑦ ᑲᑎᒪᔨᖏᓐᓂᑦ)
A. ᑭᑉᓯᒐᖅ	(ᓴᓂᕋᔭᐅᑉ ᐆᒪᔪᓕᕆᔨᒃᑯᑦ ᑲᑎᒪᔨᖏᓐᓂᑦ)
S. ᑰᓴᓐᔅ	(ᐃᒪᕐᒥᐅᑕᓕᕆᔨᒃᑯᓐᓂᑦ, ᕿᐃᓂᐲᖕᒥᑦ)
S. ᐃᓐᔅ	(ᐃᒪᕐᒥᐅᑕᓕᕆᔨᒃᑯᓐᓂᑦ, ᕿᐃᓂᐲᖕᒥᑦ)
L. ᒪᐃᔭᔅ	(ᐃᒪᕐᒥᐅᑕᓕᕆᔨᒃᑯᓐᓂᑦ, ᕿᐃᓂᐲᖕᒥᑦ)
P. ᕆᓵᑦ	(ᐃᒪᕐᒥᐅᑕᓕᕆᔨᒃᑯᓐᓂᑦ, ᕿᐃᓂᐲᖕᒥᑦ, ᐃᒃᓯᕙᐅᑕᐅᔪᖅ)
G. ᕙᓕᐊᒻᔅ	(ᓄᓇᕗᑦ ᑐᓐᖓᕙᒃᑯᓐᓂᑦ, ᐃᖅᑲᓗᐃᑦ)
K. ᕼᐃ	(ᓄᓇᕗᑦ ᐆᒪᔪᓕᕆᔨᕐᔪᐊᒃᑯᖏᓐᓄᑦ ᐊᐅᓚᑦᑎᔨᑦ ᑲᑎᒪᔨᖏᓐᓂ)
C. ᕙᓕᐊᓐ	(ᓄᓇᕗᑦ ᐆᒪᔪᓕᕆᔨᕐᔪᐊᒃᑯᖏᓐᓄᑦ ᐊᐅᓚᑦᑎᔨᑦ ᑲᑎᒪᔨᖏᓐᓂ)
R. ᕆᕝᔅ	(ᐅᑳᐱ ᐆᒪᔪᓕᕆᔨᒃᑯᑦ ᑲᑐᔾᔨᖅᑎᒌᖏᓂᑦ)

ᓇᓂᓯᔾᔪᑎᖕᒐ 3

Comments by external reviewer on the draft SSR prior to the workshop

Under background, 1st para. - "currently listed as endangered" - by whom, what authority? The IUCN currently lists the species as "Lower Risk: conservation dependent". Lower Risk means that the species "does not satisfy the criteria for any of the categories Critically Endangered, Endangered or Vulnerable." Conservation dependent means that the species is "the focus of a continuing taxon-specific or habitat-specific conservation program targeted towards the taxon in question, the cessation of which would result in the taxon qualifying for one of the threatened categories above [Critically Endangered, Endangered, Threatened] within a period of five years."

Of course, there is no reason at the moment to get into the details of the debate about the IUCN categories or criteria. My only point is that, in my view, it is important for you to specify here that the species is listed as endangered by the Committee on the ... (should spell out; don't assume that readers will know what COSEWIC stands for). I realize that your audience, from your point of view, is Canadian only. I always try to read something like this from an international (and IWC-influenced) perspective, so these things matter (to me).

2nd para.: Insert "mainly" after "summers" - obviously bowheads occur in many other parts of Hudson Bay, and even occasionally Hudson Strait, in "summer".

Also in the 2nd para., I strongly disagree that you should be looking for "the wintering ground" of this stock. There is little doubt that the whales overwinter in more than one "ground". In fact, you may never find a single area that can be described as "the wintering ground" of this stock

3rd para., Inuit were involved in commercial whaling in Hudson Bay into the early 20th C., as Comer's journal (W.G. Ross, ed., 1984) makes clear.

Note that the name was Hudson's Bay Company not Hudson Bay Company.

I am endlessly puzzled by the ways material that I have published gets interpreted. I guess it says a lot about my communication skills. Why did you pick the year 1930 to say that Inuit whaling in association with the HBC ended? In Mitchell/Reeves (1982, p. 63) reference is made to a hunt at Seahorse Point in 1934, and on p. 64 a kill at Lyon Inlet in 1940. As I keep trying to explain, the documentation needs to be interpreted for what it is - just the notes and jottings of people who happened to be places at particular times, and then happened to record what they saw and heard. The quote from Sutton & Hamilton on p. 66 of Mitchell/Reeves tells the story: "Other whales have doubtless been seen and killed since the establishment of the Post in 1924, but I do not happen to have data concerning them." We then offer our own judgment about the nature of our compilation: "We ... suspect that considerably more whaling activity has taken place than we can document; our data no doubt are heavily biased in favour of areas and periods for which literate informants were present."

I stand by those observations and therefore caution you (and Stu) that it is not responsible (or scientific) to use the data in Mitchell/Reeves as though they represent a complete record of bowhead whaling between 1915 and 1980. We did not ever imply that they did.

One reasonable interpretation of the data in Mitchell/Reeves (1982, Table 1) is that the apparently high level of activity in the 1920s and apparent decline in the 1930s are both artifacts of reporting. Specifically, note that Sutton and Hamilton's book was published in 1932, and it was a key source. Also, HBC journals just happened to be available for the 1920s in the areas where bowhead whaling was encouraged. Until one has really checked carefully, one should not conclude that whaling activity stopped in the 30's. Also, I think it is fair to assume that any whaling through at least the 1950s would have involved the sale of at least some of the products to the HBC.

Note that on p. 37 of Reeves and Mitchell (1990) we refer to two or three kills at Southampton Island in the 1940s, which were not included in the Mitchell/Reeves table. I tripped over the sources in the Public Archives while looking for something else. My search for data has been sporadic and miscellaneous, and the results should always be interpreted as a kind of lower end of the confidence interval - i.e. there was at least this much whaling, not there was this much whaling, period. Note that in the paper with Heide-Jorgensen published in 1996 on West Greenland, two more kills are noted for the Davis Strait stock that were not included in Mitchell/Reeves (Table 1). Again, please always bear in mind that the records of bowhead kills before the last few years (?) were not systematically recorded. Therefore, they don't mean what some people seem to think they do.

Final paragraph of the Background section:

1st sentence - why ignore the 1994 kill?

For two reasons I have a big problem with the 2nd to last sentence. First, the whole idea of managing a hunt for animals in any population, much less one this small, on the basis of past kill levels is extremely flimsy, esp. when later in the document reference is made to the PBR formula (a painstakingly developed, risk averse procedure) as though it is now serving as the basis for management of this bowhead hunt. Second, my diatribe above is intended to point out (once again) that you cannot use the catch history - incomplete as it is - along with qualitative judgments about recent population trends (traditional knowledge) to come up with a credible management formula. But more on that later.

Page 2 of SSR, under The Hunt:

The term "sanctioned" is ambiguous. Much (most?) of the hunting before 1979 was "unsanctioned". As we mentioned on pp. 69-70 of Mitchell and Reeves (1982), and see pp. 397-98 of Reeves and Mitchell (1985 - RIWC 35:387-404), there was a permit/licensing system in place from 1951 or so. I'm pretty sure that the hunting in N. Foxe Basin in 1964-71, Repulse Bay and Coral Harbour in the 1970s and Igloolik in 1976 was not "sanctioned". By the way you state it here, the reader is led to conclude that it was.

In the middle of this section, reference is made to there being a regular hunt. It was my understanding, based on Stu's PVA exercise, that the hunt in 1996 was authorized as a one-time "symbolic" event. Of course, I am not surprised that it is now being described as an ongoing and regular event.

Under "Stock Size" - your statement that the two estimates "appear to be additive". As I recall, there was a lot of uncertainty and confusion about the NW Hudson Bay survey's distance-from-trackline estimates. In fact, I remember being confused as to whether this was a strip or line transect survey (or some kind of hybrid?).Also, it seems presumptuous to assume that the distribution would be the same between years - esp. when you have hypothesized considerable interannual variation in calf production and thus, by inference, occupation by adult females of the "nursery" ground. This is a wobbly way to estimate numbers.

Although you admit the possibility under "Uncertainties" of positive bias (in the 1994 estimate) due to underestimation of distance from trackline, you ignore that possibility here under "Stock Size". As for the amount of availability bias, see my later comments re: possible differences in dive behavior between classes of bowheads.

Your parenthetical query about calves: I think it would be useful to have the historical data (e.g. Table 2 in Reeves and Mitchell 1990) re-examined for dates, localities, and size/sex of whales. I'm pretty sure that this exercise would demonstrate that adult females and young whales were more widely distributed than your photogrammetry work has indicated thus far.

At the end of this section, your observations about segregation sound remarkably similar to the situation in the Baffin Bay/Lancaster Sound region - perhaps it is worth noting that here and citing Finley (1990)?

Stock Trend -

Well, you wouldn't really expect traditional knowledge to supply an "estimate" in the sense you mean it here, would you? It would make more sense to just state that elders and hunters have reported seeing more ....

Sustainable Hunting Rate -

As discussed above, I do not accept the validity of Stu's approach. Also, I am bothered by the implicit assumption that all anyone is interested in is replacement yield in evaluating "sustainability". No one seems to have the slightest interest in population recovery. I trust that you intend, possibly during the meeting in Iqaluit, to discuss at length the suitability of using the PBR algorithm and that you will show clearly in the resultant draft of the SSR the various parameter values chosen (and why). I note with interest that PBR for the North Atlantic right whale, with a min. abundance estimate of 295, was calculated as 0.4 in 1997 (Waring et al. 1997).

Sources of Uncertainty:

Your reference to the possibility that animals beyond 600 m were counted made me go back to the paper and look at your methods. I agree with your conclusion that "a more rigorous survey is needed". In fact, rereading your descriptions of how you collected and analyzed the data made me wonder whether it would be more appropriately precautionary (and surely every scientist would agree that any assessment of a whale population thought to number only in the mid hundreds or so, at most, should be precautionary) to use your alternative central estimates of 150-170 rather than 250-280 as your "best" estimates of "surface" animals.

Outlook:

The first sentence is hopelessly vague, and thus misleading. Surely "the past" needs to be qualified somehow. Judging by the history of commercial whaling vs the present-day occurrence in NW Hudson Bay (Roes Welcome etc.), anyone would have to agree that there are fewer bowheads today than there were "in the past".

Your finding about calf production is extremely important. In fact, monitoring of calf production is probably the best way to assess this population through time.

Other Considerations:

Have you checked the review paper by Reeves and Mitchell (1988) on killer whales in the eastern Canadian Arctic? We found very little evidence that killer whales are present in Hudson Strait, Hudson Bay, and Foxe Basin. In fact, I find it interesting that you have some scarred and mutilated tails in the "Hudson Bay" bowhead population. If the incidence is more than a few individuals, I would become suspicious about what we think these bowheads are doing. Maybe they spend a part of their lives in more 'exposed' areas where killer whales are more common - e.g. Lanc. Sd, Davis Strait, Labrador Sea?

Management Considerations:

What would you want to protect the summering habitat from? Besides motor boat and ship traffic?

Looking at your Fig. 1 of the SSR I am troubled by the summer concentration marked in Cumberland Sound. I wonder if you should not be thinking about a much more complicated stock structure than just 2 stocks. The Maiers et al. paper, which I will comment on later below, is really using only the Cumb. Sd sample to represent the Davis Strait-Baffin Bay stock. But if there is a summer concentration in Cumb. Sd, perhaps those whales winter in the Labrador Sea while animals from Baffin Bay move down into Davis Strait for the winter. These summer concentrations may each be a stock unit of some kind - at least I think people should open their minds to the possibility ....

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Response of Reeves and Wade to the Proceedings

(commented by Richard P.R., S. Cosens
and S. Innes, DFO Winnipeg 12 Oct. 1999)

Stock Trend

There still appears to be a major misunderstanding as to what the estimates of ‘pre-exploitation stock size’ really mean. Meeting participants have already noted two of the problems in these estimates: the lack of good evidence of stock identity and the uncertainty about limits of distribution for different stocks. Other major problems are the incompleteness of the catch history and the absence of any quantitative data on the population size at the end of the era of intensive whaling. The utility of the estimates produced originally by Mitchell (1977) and later by Reeves and Mitchell (1990) and Woodby and Botkin (1993), all converging on values in the range of 450-575 for the aggregate Hudson Bay-Foxe Basin “stock,”was to show that there were *at least* a certain number of whales present in these areas when commercial whaling started. As Woodby and Botkin (1993) repeatedly emphasize, the estimates are *minima* and should not be used incautiously as though they were derived from good data on removals, biological parameters, and quantitatively derived benchmark abundance values. If the whales in this region are in fact a reproductively and demographically isolated population (a hypothesis that cannot be ruled out by the available genetic and other data), then an initial population size of only 450-600 would make them naturally rare and intrinsically vulnerable to extinction based on stochastic considerations alone. However, given the nature and amount of uncertainty in both the data and the underlying assumptions for the “pre-exploitation stock size” estimates, it is unreasonable and incautious to assess percent recovery using such estimates as though they were “best” estimates of initial stock size.

Comments: *It is agreed that estimates of initial population size are minima and the text of the SSR has been changed to reflect this. The estimates of present population size are also minima. The committee wanted the comparison to emphasize that the oft-quoted “low tens” of bowheads left in the stock does not reflect present knowledge and that the present population size, even if underestimated, is not a small fraction of the often-quoted minimum initial population size. The issue of vulnerability to extinction by stochastic events has been examined by DFO (natality, mortality, environment and killer whales) and concluded that small populations (~ 40 whales) could sustain a single removal without altering their probability of extinction due to demographic stochastic events (Innes, S. 1996. Population Viability Analysis for Bowhead Whales (Balaena mysticetus) in the Nunavut Settlement Area. Report to the National Marine Mammal Peer review Committee. 14 p.). In these simulations some small populations when extinct. However, populations of two hundred whales did not go extinct under even moderate catches (i.e., 3 adult females per year). A recent study presented at the IWC in 1999 also stated that, for populations on the order of 300 animals, ”demographic stochasticity had minor effects on population trajectories unless harvest rates approached the intrinsic rate of increase”. (Breiwick, J.M. and D.P. DeMaster. 1999 Exploratory Type 3 Fishery Simulations IWC SC/51/AWMP8 9 p.).*

Sustainable Hunting Rate

It is interesting, and in some ways gratifying, that the PBR approach has been judged a “useful tool for estimating a sustainable hunting rate.” As everyone should realize, the PBR concept was developed within the U.S. context where a Marine Mammal Protection Act (and the many studies associated with its interpretation, implementation, and amendment) provide a theoretical and legal framework for ensuring that all marine mammal populations are maintained at “optimal” levels. In Canada, no clear statement appears, anywhere, of the relevant management goals. Adoption of the PBR approach is consistent with the generally *ad hoc* nature of marine mammal conservation and hunt management in Canada. However, as Wade (1998:25) cautions, this approach was not designed for managing the direct exploitation of small populations. As he states: “... for populations of extremely low abundance, any human-caused mortality needs to be evaluated in the context of how much it might increase the risk of extinction for the population....” It is clear that although the authorization of 1 landed, 2 struck in Repulse Bay in 1996 was initially presented and justified as a one-time event, the intention is to move rapidly toward an authorization of 1, 2, or even 3 landed whales per year (meaning 4-6 strikes?) to be taken from this stock. Whereas in the U.S., PBR management takes place within an elaborate (and costly) overall national commitment to stock monitoring and assessment, no similar commitment seems to exist in Canada. Without it, use of PBR should be made in an ultra-precautionary manner, in our view.

Comments: *It is not clear to us that there is any difference is between human-induced mortality caused by accidental netting, boat collisions or hunting. A death is a death and the consequences to the population are the same. The history and rational behind the evolution of PBR was summarized to the committee. The value of the PBR approach is that it is precautionary. It uses conservative estimates of stock size, and modifiers to an expected maximum net productivity based on the status of the stock. It was agreed that the endangered status of this stock required the use of only the lower estimate of PBR. This estimate allows for only 1/10th of the expected maximum net productivity to be removed. That is only 0.2% of a stock size estimate that incorporates an adjustment for sampling uncertainty. Both the work by Innes (1996) and, Breiwick and DeMaster (1999) have been useful in defining what* “extremely low abundance” *means in bowhead whale numbers.*

Sources of Uncertainty

It is unclear why, given the relatively casual manner in which distances seem to have been estimated, the positive bias caused by inaccurate strip width estimation is dismissed as “small” compared with the negative biases caused by availability and detection concerns.

Comments: *To clarify, the workshop participants noted that the downward biases caused by a lack of correction for diving and a lack of estimate for animals present in parts of the stock range outside of the survey area was probably in excess of double the estimate compared to the upward bas caused by an inaccurate strip width which might cause an overestimate by less than 50%. While this potential bias was noted by reviewers and addressed by the authors of Cosens et al. (1996) the authors are not convinced that the strip width was incorrectly* *recorded. There were several other areas of uncertainty that were addressed during the RAP. The most important were the definition of stock (i.e., the huntable animals) and that the age structure of the surveyed population has too many immature animals for the number of adult whales seen.*

Management Considerations

The suggestion that hunters use only the presence or absence of an accompanying calf to decide whether to kill a whale should be reconsidered. In the case of the Bering-Chukchi-Beaufort population, the IWC Scientific Committee has consistently urged that non-adult whales be selected for. In other words, hunters are encouraged to hunt animals in the 30-35 foot range. There are several good reasons for this. From a biological point of view, it reduces the likelihood of taking individuals that are contributing to reproduction - e.g. pregnant females. Also, assuming that natural mortality rates of younger animals are higher than those of older animals (almost certainly true), hunting mortality of the former is more likely to replace, rather than add to, natural mortality. Finally, from a practical point of view, the killing, handling and processing of smaller whales is generally more efficient and less wasteful (see, for example, the papers by McCartney and Braham in "Hunting the Largest Animals," 1995).

In general, we believe that this bowhead population should be managed for recovery, not just maintenance of its present size and distribution. The sparsity of observations in Roes Welcome Sound and NW Hudson Bay, in spite of considerable survey coverage at the appropriate season (see not only Cosens and Innes 1999, but also Richard 1991 and Richard et al. 1990), is troubling. These were the commercial bowhead whaling grounds (see Ross 1974; Reeves and Mitchell 1990), and no evidence has been brought forward to show that the species has recovered there. Why is this?

Comments: *The suggestion that harvesting should preferably target immature animals has been incorporated into the Stock Status Report with a sentence paraphrasing the rationale given above. On the suggestion that bowheads should be managed for recovery, it is important to note that the PBR approach for both "endangered" and "unknown" status stocks allows recovery. A conservation plan with a 'recovery' objective is being prepared. With respect to the comments that few bowheads were seen by surveys of Roes Welcome Sound, an inspection of Ross (1974, in litt.) suggests that the lack of bowheads in Roes Welcome Sound is consistent with the seasonal distribution of whaling records. With respect to recovery, the Nunavut Wildlife Management Board's Bowhead Traditional Knowledge Study provides strong evidence that the numbers of bowhead whales seen by hunters has more than doubled in the last 20 to 30 years, depending on the community, within this stock's range (Hay pages). While this is support that the stock is recovering, it is not support that the stock has recovered to its pre-commercial-exploitation population size and distribution.*

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