INFLUENCE OF BRIDGE OFFICER EXPERIENCE ON ICE MANAGEMENT EFFECTIVENESS

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ABSTRACT

The research presented in this thesis investigates the influence of human expertise on the effectiveness of ice management operations. This was accomplished in an experiment using a marine simulator in which 36 participants with a range of seafaring experience levels were tasked with individually completing ice management exercises. Effectiveness was assessed in terms of the operator’s ability to lower pack ice concentration around an offshore structure and to keep a defined area free of ice for a lifeboat launch. These responses were compared to two independent variables: i) experience level of the participant, and ii) ice concentration. The results showed a significant difference in ice management effectiveness between experience categories. Characterizations of effective ice management techniques were presented based on the results. This result has implications for training in the nautical sciences and can help inform best practices in ice management.

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LIST OF ABREVIATIONS AND SYMBOLS

AHTS	Anchor Handling Tug Supply
ANOVA	Analysis of Variance
CRD	Completely Random Design
DP	Dynamic Positioning
FPSO	Floating, Production, Storage and Offloading
GPS	Global Positioning System
IMO	International Maritime Organization
LOESS	Locally Weighted Scatterplot Smoothing
LSD	Least Significant Difference
OSV	Offshore Supple Vessel
REML	Restricted Maximum Likelihood
STCW	Standards of Training, Certification and Watchkeeping for Seafarers
TEMPSC	Totally Enclosed Motor Propelled Survival Craft
A_Z	Area of ice management zone
C_i	Incremental concentration drop
Δt	Sampling interval

INTRODUCTION

Expertise of human operators plays an important role in ice management operations, which rely heavily on the knowledge and proficiency of individuals orchestrating operations from the bridge of support vessels. It follows that expertise of bridge officers may distinguish one individual from another in terms of ice management effectiveness. Despite its importance, though, expertise is rarely included in engineering assessments, mainly due to the difficulty it poses in terms of assessment. In this research, the influence of expertise of bridge officers in ice management is quantified. Systematic investigation was made possible with the use of a full-mission marine simulator.

The influence of two factors on the effectiveness of ice management operations was studied: i) bridge officer experience and ii) ice severity. It was hypothesized that ice management would be more effective (that is, more ice would be cleared in a given amount of time) with more experienced bridge officers compared to novice ones. Furthermore, it was hypothesized that this experience effect would be stronger in more severe ice conditions (higher ice concentrations).

The context of the research question is important for marine industry operators in areas where sea ice and glacial ice must be managed to enable operations to proceed safely [1], [2]. This research investigates the case of drifting broken sea ice when it enters an area in which a moored, floating installation is present. An experimental campaign was designed to test the relationship between the human factor of bridge crew experience and the environmental factor of ice concentration on the effectiveness of a defined ice management operation.

1 LITERATURE REVIEW

A marine simulator was used to create a virtual scenario in which drifting broken sea ice entered an area in which a moored, floating installation was present. Such operations have been documented for many regions, including the Arctic Ocean [3], the Okhotsk Sea [4], and the Beaufort Sea [5]. The simulation scenario was used in an experimental campaign to investigate the influence of bridge officer experience on the effectiveness of ice management. Ice management has no formal definition, although one definition has been offered that fits well within the scope of the research discussed herein [1]: "[ice management is] a systematic operation enabling a main activity that could not be safely conducted without additional actions due to potential existence of sea ice." In the simulated scenario, the main activities are oil production and offloading on a Floating, Production, Storage & Offloading vessel (FPSO). The stand-by vessel is tasked with enabling this main activity to proceed safely in the presence of sea ice by, among other things, keeping its port and starboard lanes clear should lifeboats have to be deployed during a major event.

Several studies have reported how ice loads on structures might be reduced through ice management measures [6], [7], [8], [9], and [10]. Barker et al. [6] describes a moored station-keeping vessel in moving pack ice and suggests that reduction of ice severity through ice management can drastically reduce loads on the mooring lines. Spencer & Molyneux [7] go one step further, showing that ice concentration, measured in fraction of coverage, plays a vital role in predicting loads acting on the hull of a moored, floating ship-shaped structure. Such loads increase exponentially in proportion to ice coverage, with the notable exception being ice concentrations under 4-tenths coverage, for which insignificant loading is experienced. Considering that most floating production systems located in arctic and sub-arctic waters will likely encounter small floes drifting freely under environment forces, the focus on ice concentration as a key predictor of operational success is an important one and one that will be considered in this research during simulated ice management scenarios. Level ice has been shown to produce very high loads on moored structures [10], but remains outside the scope of this research. In terms of operational procedures in ice management, Hamilton et al. [8] showed that ice management in drifting pack ice for a station-keeping vessel may take several different forms. Variation in operational techniques are described as "fleet deployment patterns" and come in a variety of different techniques ranging from "circular" to "linear." This suggests that the way in which ice management is carried out on the bridge of a support vessel plays an important role in ice management effectiveness, not just according to which technique is employed, but also to the extent that a given fleet deployment pattern is executed effectively according to the expertise of the operator in control. Iceberg drift, investigated in [9], also plays a deciding role in ice management operations in areas where icebergs may be seasonally present, however in this research ice management simulations do not include icebergs. The ice that is encountered in the simulations used in this study is based on pack ice typical of that which may be found seasonally on the Grand Banks of Newfoundland. This pack ice is first year ice with a thickness of 30 to 70 cm and is composed of small floes in the range of 20 to 100 m in diameter, drifting at a rate of 0.5 knots [11]. The simulated ship-interaction is based on PhysX rigid body mechanics software [12], which was initially developed for computer gaming. This research is not the first time that such a numerical technique has been used for ship-ice interaction modeling; Lubbad & Loset [13] showed that complex and computationally taxing real-time ship-ice interaction simulations done in PhysX exhibited satisfactory agreement to full-scale tests. The authors even suggested that the real-time criterion of such simulations may have applications in training of offshore personnel in arctic operations, which is the predominant theme of the current research.

The use of marine simulators for training in maritime operations is not a novel invention. Simulator training is mandated and regulated internationally by the *Standards of Training, Certification and Watchkeeping for Seafarers* (STCW). In 2017, the STCW was amended for training requirements for vessels operating in polar waters, which includes ice management activities. These requirements reflect regulations described by the International Maritime Organization's *International Code for Ships Operating in Polar Waters* – known as the "Polar Code" – which came into effect on January 1, 2017. Model courses were approved by the IMO in the Spring of 2017 to help ensure knowledge transfer of Polar Code certification requirements to students and to help set the expectations for training institutions. These model courses highlight marine simulator technology as an effective means of transferring skills to an individual – should simulators be available. In academic studies, marine simulators have been shown to be effective tools for training for operations where the same skills would be costly, resource-intensive, and risky to practice onboard real vessels [14], [15]. Still, some studies show that simulators are not to be treated as panacea for training. Some studies have suggested that there is a limit to what can be experienced in a simulator and that simulator training should be approached with measured skepticism. For instance, one study suggested that marine operations are so complex on a spatial and temporal scale and so intricate in terms of socio-technical interactions that recreating them in a simulator may be of limited use in training [16]. The same author also argues that lack of photorealism can affect seafarers' learning objectives negatively, since it may cause trainees to idly navigate the marine simulator environment without meaningful engagement required for learning [17].

Despite known values and limitations, many questions related to the use of simulators in maritime training and assessment remain unanswered. A literature review conducted in 2017 showed that few empirical studies have investigated the pedagogical aspects of bridge officer training in marine simulators [18]. This was impetus for a 2018 study by the same author, who aimed to investigate the role of instructions and assessments for developing trainees' competencies in a simulator-based maritime education program [19]. This was accomplished with the use of ethnographic fieldwork and analysis of video data. Among other things, the study showed that good seamanship practice is hard to teach, and that while instructional support in the forms of monitoring, assessment, and feedback should form the core of the training program, there is still a lack of understanding of simulator training and assessment. In other words, the obvious questions of how to design an effective training program in a marine simulator remains, for the large part, unknown. The work in this thesis may help to inform this question, specifically in aspects of simulation scenario design and participants' assessment in ice management training.

2 METHODS

2.1 Simulator

The ice management simulator used in the experiment was designed and built for research. It uses PhysX software rigid body mechanics computation and simulation software [12]. The simulator consists of a bridge console positioned in the middle of a 360-degree panoramic projection screen. The bridge console is a (2 m x 2 m) platform mounted on a Moog motion bed. For this experiment, the motion bed was turned off. A schematic of the simulator is shown in Figure 1.

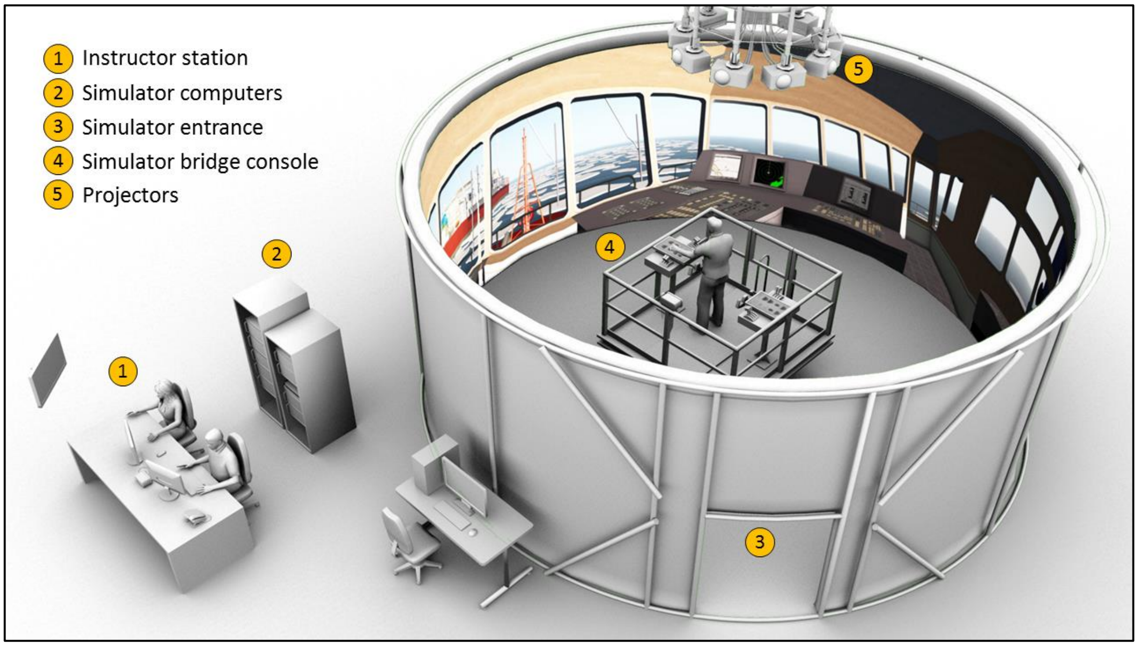


Figure 1: Schematic of the simulator set-up

The simulated vessel used in the ice management scenarios was based on Anchor Handling Tug Supply (AHTS) vessels typical of those used in the Newfoundland offshore area. It has a length overall of 75 m and is powered by twin 5369 kW diesel engines. For propulsion, it has two controllable pitch (CP) propellers and rudders, and forward and aft tunnel thrusters, each with 895 kW of power. The simulator bridge consisted of a simplified forward console and aft console. To switch between consoles, the bridge officer had to turn to the opposing console and transfer controls using "Transfer" toggle switches. Both consoles had basic controls: main propellers (port and starboard), steering, and tunnel thrusters (fore and aft). A schematic of the forward console is shown in Figure 2.

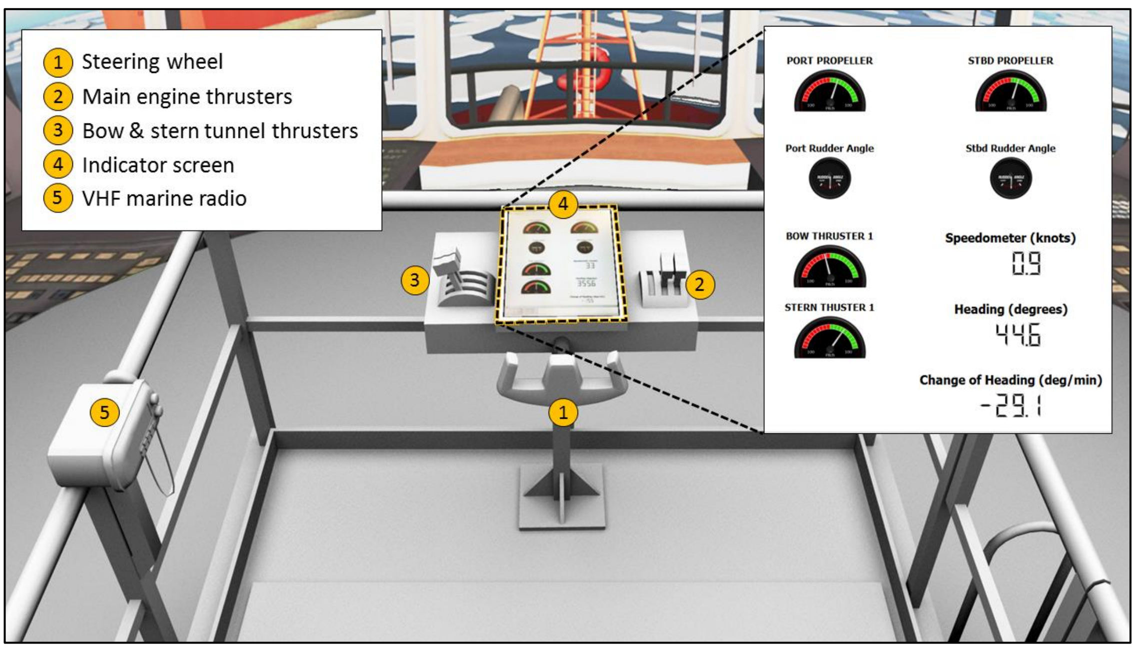


Figure 2: Schematic of the simulator bridge console

The bridge console was highly simplified and did not have navigational components such as radar, GPS, or chart systems. Moreover, the simulated version of the AHTS was not exact in hydrodynamic likeness, particularly with regard to its seakeeping and maneuvering characteristics. Notwithstanding these limitations in similitude, the simulator was good enough for this experiment whose purpose was to detect differences between bridge officer experience groups and characterize general principles of good ice management practices. Face validation of the simulator, conducted prior to the execution of this study, was completed using feedback from masters and mates operating similar AHTS vessels to that being simulated.

All participants in the experiment were given 60 minutes to familiarize themselves with the controls and maneuvering characteristics prior to completing the ice management scenarios. This was accomplished in three basic 20-minute-long scenarios designed to habituate participants to the simulation environment. None of the participants had used the simulator before. Signs and symptoms of simulator-induced sickness were monitored before and after each exposure period to the simulator using a self-reported questionnaire [20]. No participants noted simulator sickness symptoms severe enough to justify stopping a simulation trial.

The Instructor Station was located several meters outside the periphery of the projection screen, out of view from inside the simulator. This is where the experimenters started and stopped scenarios and provided scripted instructions to the bridge officer inside the simulator. Instructions were communicated with a two-way VHF radio. Distances from the “own-ship” (the vessel being operated in the simulator) to specified targets could also be communicated in this manner, whenever they were requested. A screenshot taken from the Instructor Station monitor is shown in Figure 3.

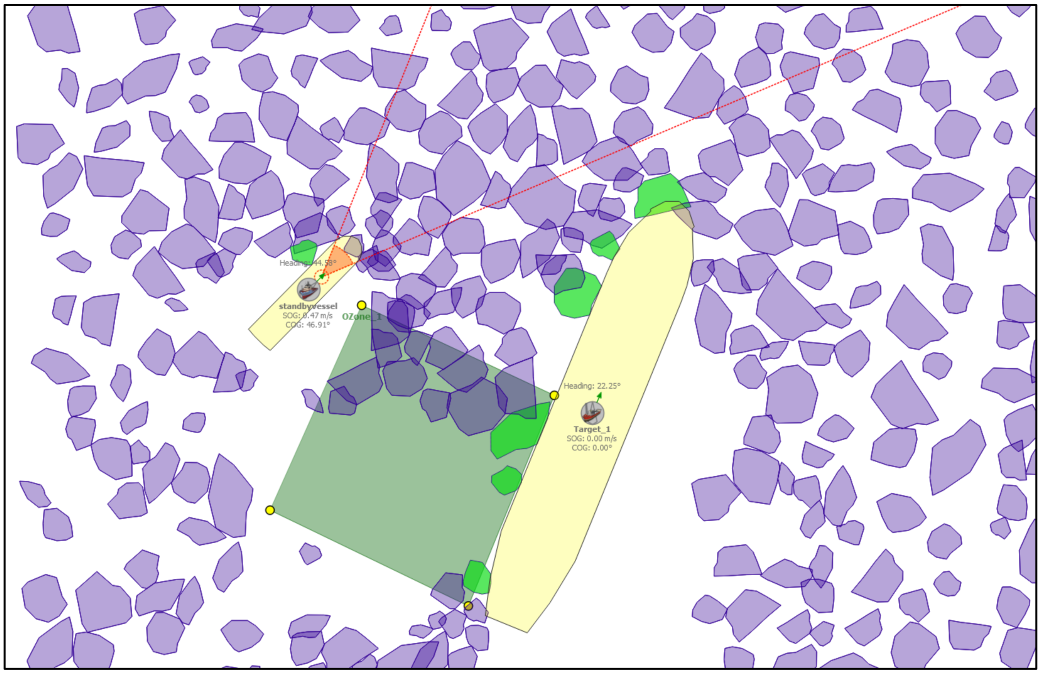


Figure 3: Screenshot from the Instructor Station monitor during simulation. Graphics are identical to those that appear in Replay files.

Data acquisition was handled by five dedicated processing computers. Zonal concentrations, time, latitude and longitude position, speed, and heading were recorded. A video "Replay" file was saved upon completion, which upon playback showed the 30-minute simulation from start to finish. The Replay file imagery appeared as shown in Figure 3.

2.2 Design of Experiments

The approach adopted a formal design of experiments [21]: a 2^k full factorial was completed with nine replicates and k = 2 factors, totaling 36 runs. The first factor, ice severity, was represented by ice concentration and could be changed in the parameter settings of the simulator. The low-level treatment was set to 4-tenths ice concentration; the high-level treatment was set to 7-tenths ice concentration. The second factor, experience level of bridge officers, was represented by a high- and low-level categorical variable. The low-level experience category consisted of eighteen cadets enrolled in a local seafaring program (average years spent at sea from 0-3 years). This group included six students from each of the 1st, 2nd, and 4th year classes. The high-level experience category consisted of eighteen seafarers (masters and mates) employed in the marine industry (average years spent at sea = 20 ± 10 years). This group included operators of coastal ferries, bulk carriers, cargo tankers, offshore supply vessels (OSVs), and anchor-handling tug supply vessels (AHTSs), with the latter two subset groups contributing the highest number of participants. The number of participants was based on a power analysis, whereby an estimate of effect size and variance in a given response variable was used to estimate the required sample size at a given statistical power and Type 1 error rate [22]. For the interested reader, more information about the power analysis procedure used in sizing this experiment is described in the Appendices (Section 7.1). Participants were recruited on a volunteer basis. Following the research protocol approved by Memorial University's interdisciplinary committee on ethics in human research, all volunteers provided their informed consent before participating in the experiment.

Due to the logistical challenges of scheduling thirty-six voluntary research participants, it was impractical to run a Completely Random Design (CRD). In standard experiment designs, the order of individual trials is determined randomly – a method that usually ensures that observations are independent, thereby complying with statistical methods. A way to circumvent the problem of restricted randomization is the split-plot design [23], [24]. In this design, the hard-to-change *experience* variable stayed constant in pairs of consecutive runs (the whole-plot) while the easy-to-change *concentration* variable was randomly selected for each group (the sub-plot). The experimental error of effects estimates could thereafter be estimated separately, thereby balancing the biasing effect of uncontrollable experimental conditions that may have otherwise been undetectable.

Having just two independent variables in the experiment (ice concentration and experience level) and one dependent variable (concentration reduction), the aim was to use straightforward scenarios in order to avoid introducing confounding factors. Still, the scenario had to be a realistic representation of an ice management exercise. Each of the thirty-six participants completed two different ice management scenarios to reflect different ice management tasks. The first scenario was called "Precautionary" ice management, in which the participant was tasked with keeping the area around a moored FPSO clear of ice. The second scenario was called "Emergency" ice management, in which the participant was tasked with clearing away ice from an area underneath one of the FPSO's lifeboat launch zones in preparation for evacuation. The areas the participants were tasked with clearing are outlined in Figures 4 and 5. Other than FPSO heading (0 degrees for the "Precautionary" and 23 degrees for the "Emergency" scenario) and ice concentration (low level 4-tenths and high level 7-tenths), all other conditions in the scenarios were equivalent. As there were two ice concentration levels and two different ice management scenarios, there were four different scenarios used in total in this experiment (the two 7-tenths concentration cases are shown in Figures 4 and 5). Each participant completed both scenarios at the same ice concentration level. All scenarios were 30 minutes long. The floe sizes were randomly sampled from a lognormal distribution whose parameters were based upon input from a subject matter expert during face validation of the simulator. The floe thickness was uniformly set to 40 cm.

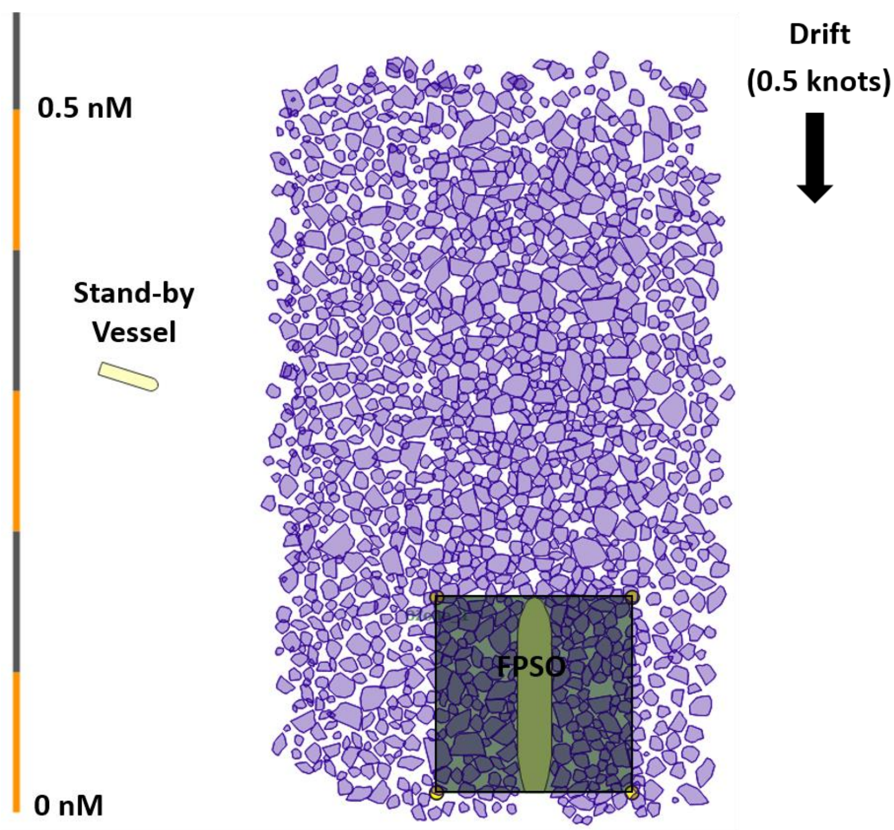


Figure 4: "Precautionary" ice management scenario (7-tenths)

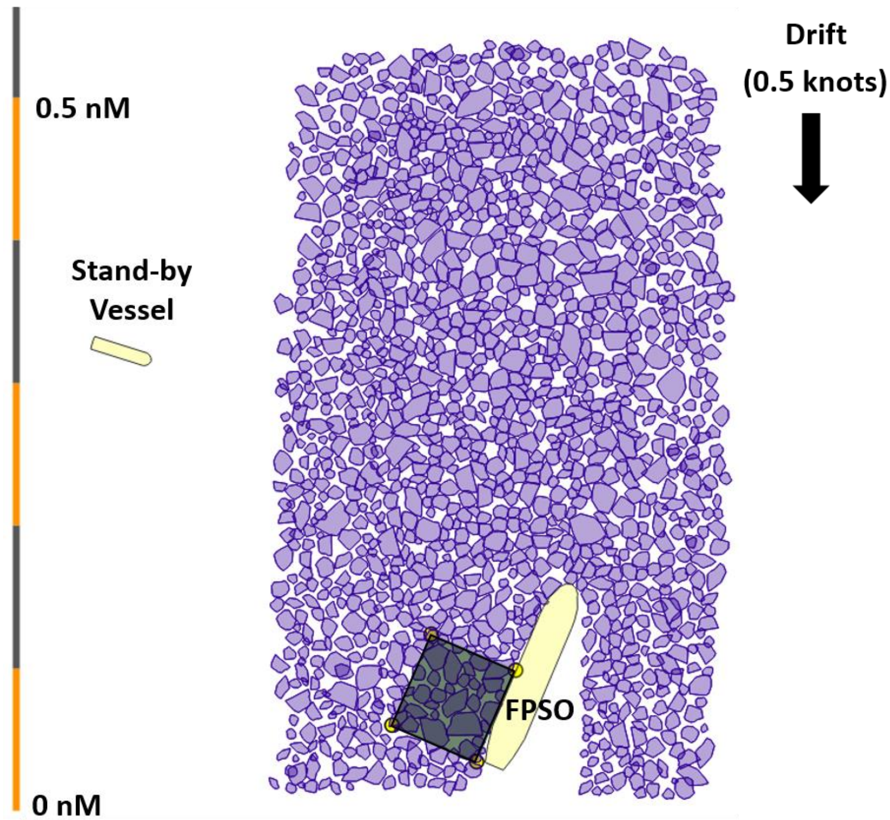


Figure 5: "Emergency" ice management scenario (7-tenths)

2.3 Analysis

Five performance metrics were used to assess ice management effectiveness: i) *average ice clearing* (tenths concentration), ii) *peak ice clearing* (tenths concentration), iii) *total ice clearing* in a defined area (km^2), iv) *clearing-to-distance ratio* (km^2/km) (all measured in a defined area), and v) *cumulative ice-free lifeboat launch time* (minutes). The latter of the five metrics applied to the "Emergency" scenario, only.

Analysis of each of the five performance metrics was performed in a similar way. First, data were explored in a descriptive and visual sense. This was then followed by a more rigorous approach whose aim was to determine the extent to which ice concentration and bridge officer experience influenced the effectiveness of the simulated ice management scenarios. The main effects of each factor and their interaction effect are determined in the same way as in a regular factorial design. The key difference is that a half-normal plot of effects was used to screen for significant effects for whole-plot and sub-plot groups separately [25]. Then ANOVA is used to check for significant effects and accounted for the group terms separately [26]. Significant effects from both groups are then combined to give the final model.

Residual plots were used to check modelling assumptions [27]. Diagnostic checks of modeling adequacy showed that assumptions of normally distributed residuals, heteroscedasticity, and independence of residuals with run order were. This check was performed for all analyses in this work because it showed that modelling assumptions were valid, thereby supporting any inferences on which they were based.

Additional analysis helped to answer the question of what characterized good ice management practices for particularly strong performers. For a chosen scenario and for a chosen performance metric, we looked at three data sources: i) plots of position during simulation, ii) screenshots captured from Replay files, and iii) exit interviews. This combination of qualitative and quantitate data enabled us to present a general description of the most effective ice management strategy for the “Emergency” scenario.

3 RESULTS

An example is given of a single test to illustrate what was measured. The example is followed by a more detailed discussion about general results. Note that all data collected in this experimental campaign is freely available for dissemination [28].

Results from a single test are shown in a plot in Figure 6. It shows the concentration measurement taken at a rate of once every 30 seconds during a 30-minute "Emergency" ice management scenario. The values reflect instantaneous zonal concentration measurements in 30 second intervals. The concentration measurements were recorded from the box area under the port lifeboat launch zone shown in Figure 5. This is the area that participants in the experiment were tasked with clearing. Also shown in the plot in Figure 6 is the baseline un-managed ice concentration within this zone; that is, the ice concentration that occurs within the box area when no ice management is performed.

This example case was selected randomly from the 72 tests that were done. The driver in this case was from the seafarer group (high-level experience), with 16 years of experience at sea. The participant had performed ice management operations within the past three years and had experienced between 3 to 10 seasons in ice over his or her career.

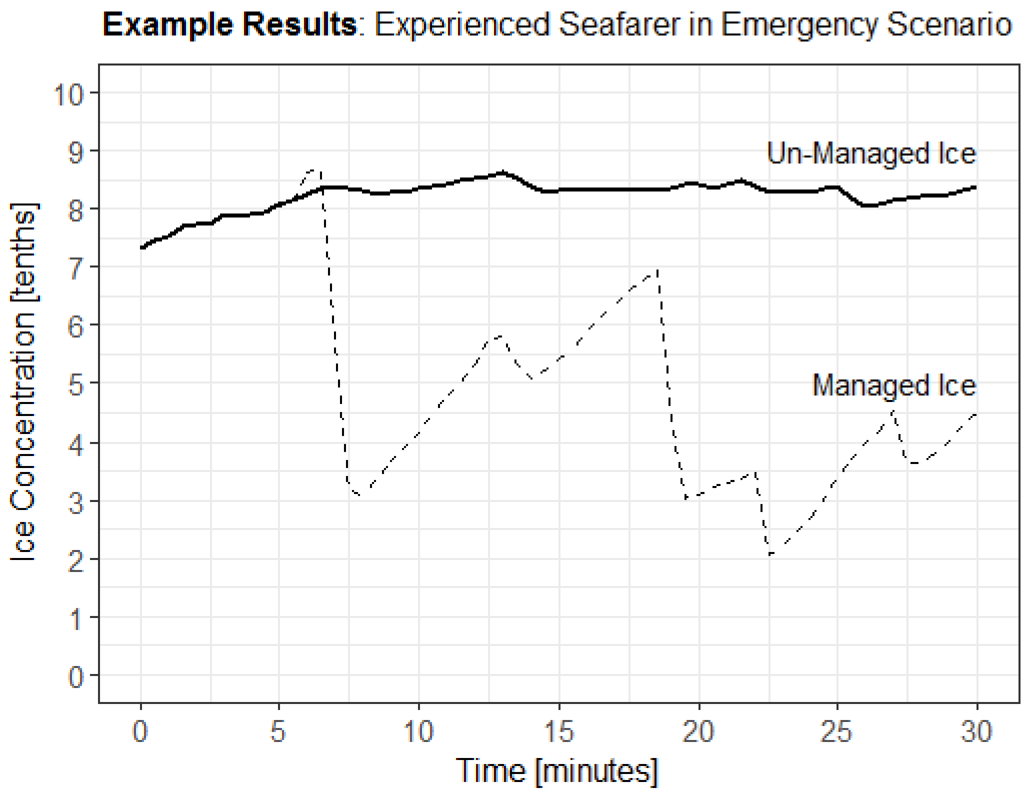


Figure 6: Example measurements from a single simulation trial

From Figure 6, it is clear that the baseline un-managed ice concentration within the measurement zone is not steady. In fact, for the "Emergency" ice management scenario, the ice accumulated along the side of the FPSO as it drifted past such that the ice concentration started at 7-tenths and rose to almost 9-tenths. Therefore, it is more helpful to analyze the relative drop in ice concentration compared to this baseline measure, rather than the recorded concentration in the box area at a given instant. From the computed concentration drop relative to the baseline, various metrics of ice management performance can be derived. This includes absolute peak clearing (in terms of ice concentration), average clearing (in terms of ice concentration) and in total clearing (in km^2 of ice cleared). The reduction in ice concentration and the corresponding metrics are shown in Figure 7 for the example case.

The ice concentration in the specified zones (Figures 4 and 5) was measured using image post-processing of the Replay files recorded during simulation. Screen captures of the Replay files were recorded at a rate of once every 30 seconds and the images were subsequently processed using Matlab (Version 9.1.0 R2016b), where pixel counts of the sequence of raster images were distinguished into three areas: open water, ice, and ship. The concentration calculation (Equation 2.1) was computed as the total area of ice divided by the area of the zone, corrected for the presence of the own-ship and FPSO, if and when they entered the zone, by subtracting the ships' water-plane areas from the calculation.

$$Ice\ Concentration = \frac{\sum Area\ of\ ice}{Area\ of\ clearing\ zone} \qquad \textbf{Equation 3.1}$$

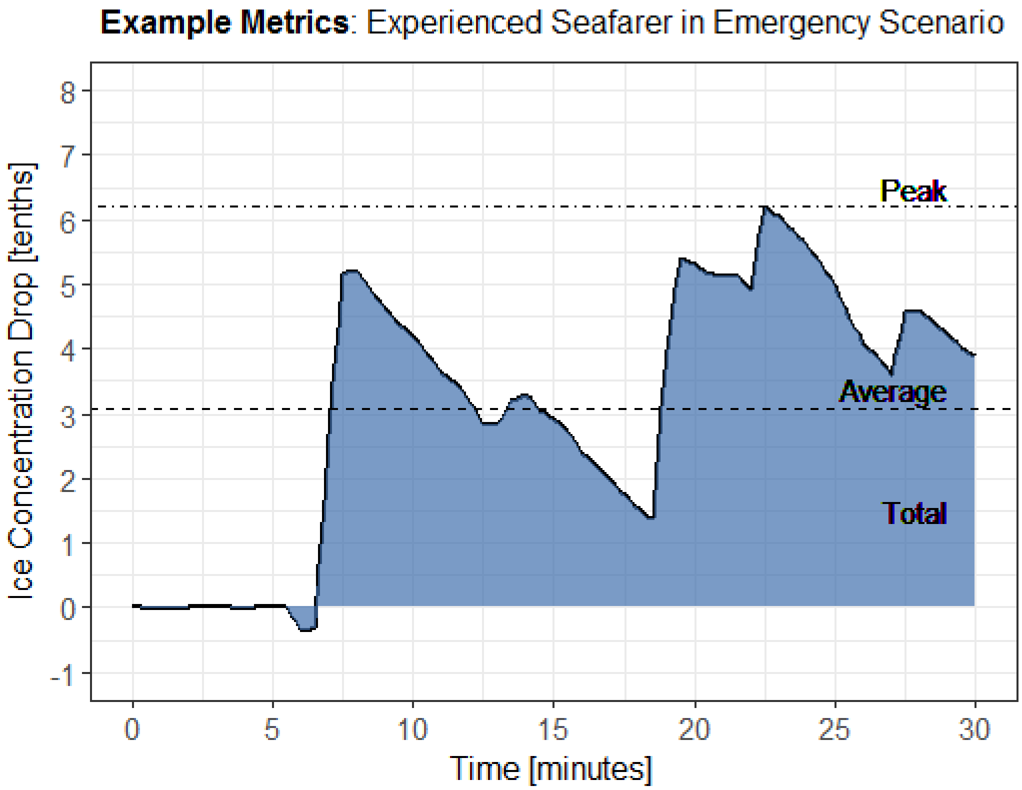


Figure 7: The example case with three performance metrics illustrated

3.1 Average Clearing

Having examined a single case in Figures 6 and 7, we now look at the entire sample of 36 participants tasked with two 30-minute scenarios each (72 total simulator trials and 36 hours of total simulator time) to characterize the data in terms of the chosen performance metrics. Table 1 shows descriptive statistics for the *average clearing* metric for the cadet (low-level experience) group for “Precautionary” and “Emergency” ice management scenarios. Similarly, Table 2 shows descriptive statistics for the *average clearing* metric for the seafarer (high-level experience) group for “Precautionary” and “Emergency” ice management scenarios.

Table 1: Descriptive statistics for cadets’ average clearing (tenths concentration)

Scenario	Concentration	*Mean*	*Standard Deviation*	*Minimum*	*Median*	*Maximum*
Precautionary IM	**4**	*0.4*	*0.3*	*0.0*	*0.4*	*1.0*
	7	*1.0*	*0.6*	*0.4*	*0.9*	*2.1*
Emergency IM	**4**	*1.0*	*0.6*	*0.1*	*1.0*	*2.1*
	7	*1.7*	*0.8*	*0.3*	*1.8*	*2.9*

Table 2: Descriptive statistics for seafarers’ average clearing (tenths concentration)

Scenario	Concentration	*Mean*	*Standard Deviation*	*Minimum*	*Median*	*Maximum*
Precautionary IM	**4**	*0.3*	*0.4*	*-0.1*	*0.2*	*1.0*
	7	*1.2*	*0.7*	*0.2*	*1.0*	*2.5*
Emergency IM	**4**	*1.6*	*0.5*	*0.8*	*1.7*	*2.1*
	7	*2.6*	*0.7*	*0.9*	*2.9*	*3.4*

From Tables 1 and 2, some characterizations can be made about the relative performance of the two groups across like scenarios and concentration levels. For instance, there is little difference between groups when comparing across the "Precautionary" ice management scenario. The "Emergency" ice management scenario, on the other hand, appears more suited to detect differences between groups for the *average clearing* metric. Another important trend to note is that clearing is consistently higher at the higher concentration level.

These trends are visually represented in the boxplots in Figure 8, which are grouped by *concentration* and *experience* for all trials of the "Emergency" scenario.

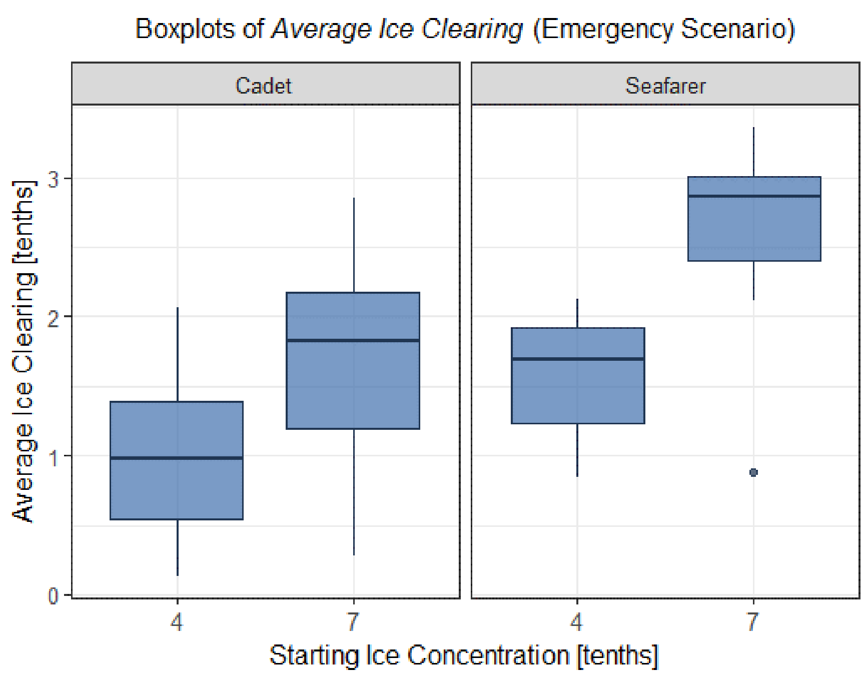
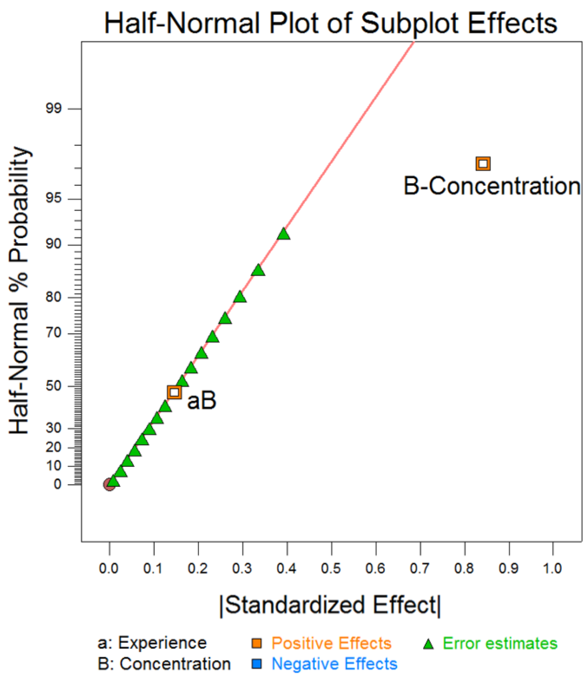


Figure 8: Boxplots of average ice clearing for "Emergency" scenario

Half-normal plots of sub-plot effects and whole-plot effects are shown in Figures 9 and 10, respectively. Half-normal plots can be used to select significant factors for the analysis. Here *concentration* (easy-to-change whole-plot factor) appears significant in the sub-plot effects and *experience* (hard-to-change sub-plot factor) appears significant in the whole-plot effects. The interaction effect is plotted on the whole-plot half-normal plot and it does not appear significant, so it is dropped from the analysis. Restricted Maximum Likelihood (REML) ANOVA (Table 3) is computed to formally test for significant effects. It confirms that the *experience* and *concentration* factors are significant effects, with *p*-values less than that prescribed by the acceptable Type 1 error rate of $\alpha = 5\%$.

The finding that *concentration* is significant is not surprising because the factor is at least partly collinear with the response, *average ice clearing*. What is of interest is the interaction effect occurring at different treatment combinations. Since the *experience-concentration* interaction effect is not statistically significant we can reject the hypothesis that ice management effectiveness increases with increasing experience *and* ice concentration.



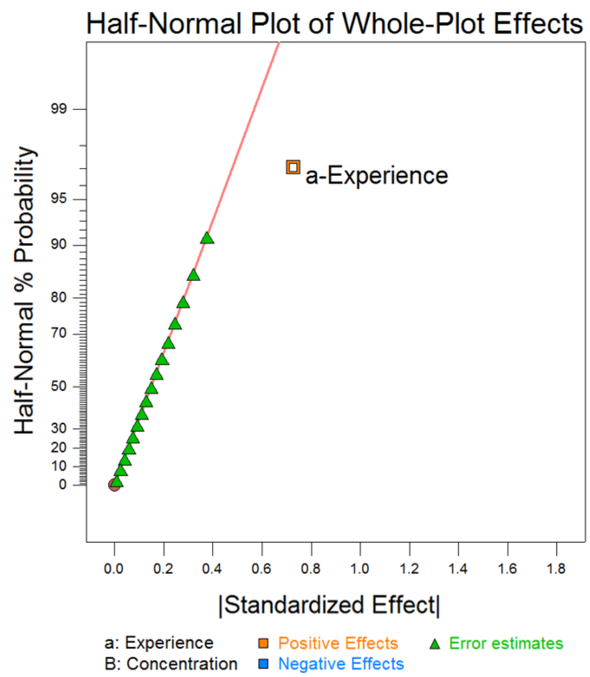


Figure 9: Half-normal plot of subplot effects

Figure 10: Half-normal plot of whole-plot effects

Squares indicate positive effects; triangles indicate error estimates.

Note that when analyzing the results as if they came from a CRD using standard ANOVA, the results yield the same conclusions. This is explained by the REML variance component estimates (Table 3), which show a group variance of zero, indicating that the whole-plot model is explaining all the variation between groups. The analysis is, in other words, equivalent to a randomized design. Despite this, experimenters running similarly designed experiments should take the same precaution and should not analyze results as if they came from a CRD. This finding applies to all other metrics analyzed in this work described in Section 3.2 to 3.5.

Table 3: REML ANOVA (average clearing)

REML (REstricted Maximum Likelihood) analysis for selected model

Kenward-Roger p-values

Fixed Effects [Type III]

Source	Term df	Error df	F	p-value Prob > F
Whole-plot	1	32.00	10.64	0.0026
a-Experience	*1*	*32.00*	*10.64*	*0.0026*
Subplot	2	32.00	7.34	0.0024
B-Concentration	*1*	*32.00*	*14.25*	*0.0007*
aB	*1*	*32.00*	*0.43*	*0.5165*

Table 4: REML variance components table

Variance Components

Source	Variance	StdErr	95% CI Low	95% CI High
Group	*0.000*	*0.000*	*0.000*	*0.000*
Residual	*0.44*	*0.11*	*0.29*	*0.76*
Total	0.44			

Key results of the model are shown in the effects plot in Figure 11. The plot shows the *average clearing* metric for all 36 participant trials of the “Emergency” ice management scenario. Data are summarized with Fisher’s Least Significant Difference (LSD) I-bars around predictions at each treatment combination. This is an approximate way to check whether predicted means at displayed factor combinations are significantly different. As the slopes of the lines are almost parallel, it is clear there is no interaction effect between the two factors on the average drop in managed ice concentration.

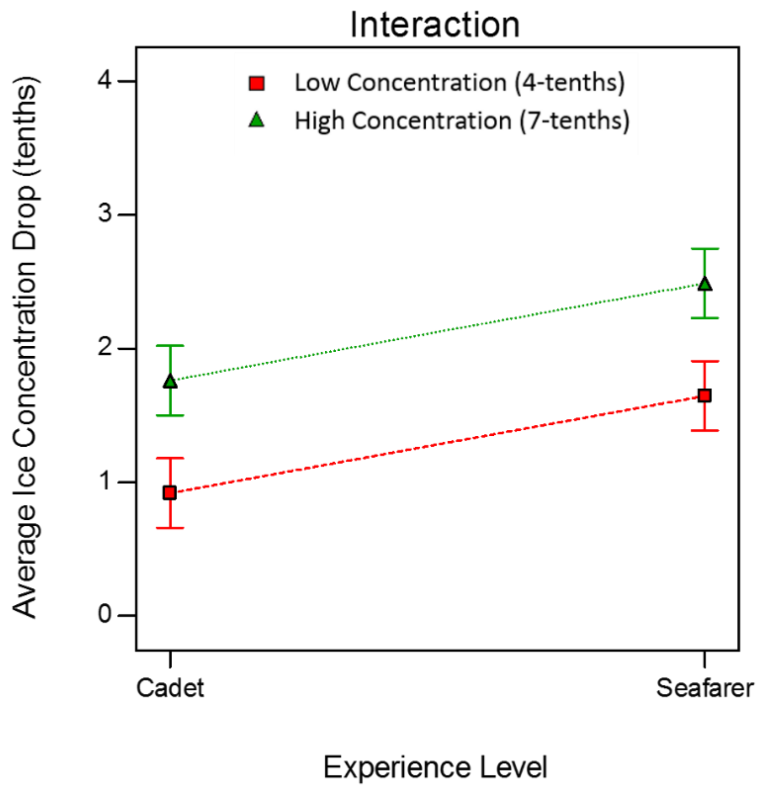


Figure 11: Interaction plot of concentration and experience on average clearing.

Residual plots are presented here to show that the experimenter has checked underlying assumptions required for the data analysis. The normal probability plot (Figure 12) checks the assumption required in ANOVA that residuals are normally distributed. From Figure 12, it is clear that residuals follow approximately a straight line on the plot with no definite patterns; as such, the assumption that residuals are normally distributed holds. The residuals versus predicted plot (Figure 13) is another diagnostic tool for visually checking modelling assumptions. It plots residuals versus predicted response values. When the plot shows random scatter, it indicates that variance is not related to the size of the response. This constant variance is called heteroscedasticity and is another assumption required by ANOVA. It follows from Figure 13 that the assumption that residuals are heteroscedastic is acceptable.

Figure 14 shows a plot of residuals versus run order. The random scatter indicates that no time-related variables that went unaccounted for in the analysis are influencing results. Finally, the Box-Cox plot in Figure 15 shows that based on a curve generated by the natural logarithm of the sum of squares of the residuals, no power law transformation is needed to stabilize variance or induce normality (transformation parameter lambda = 1). The long vertical line in the Box-Cox plot indicates the best power transformation parameter; the one currently used is represented by the short vertical line nearest to this long line.

These important diagnostic checks are performed for all remaining analyses of performance metrics in this work. Note that in the interest of abridging this work, diagnostics plots are not shown for the remaining metrics in the text and are instead listed in the Appendices (Section 7.2). The exception is Box-Cox plots, which are presented in the text for models for which a power transformation was applied.

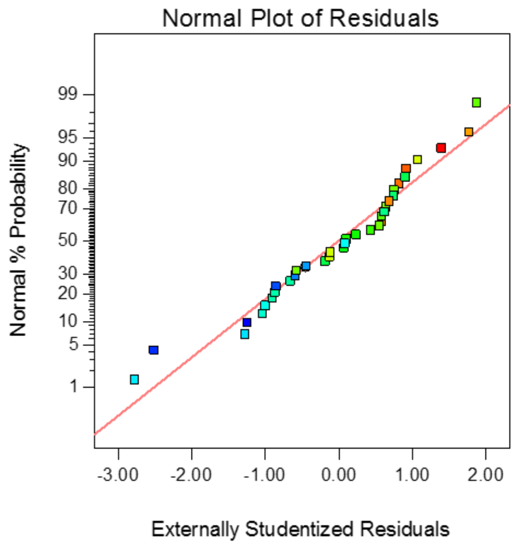


Figure 12: Normal plot of residuals

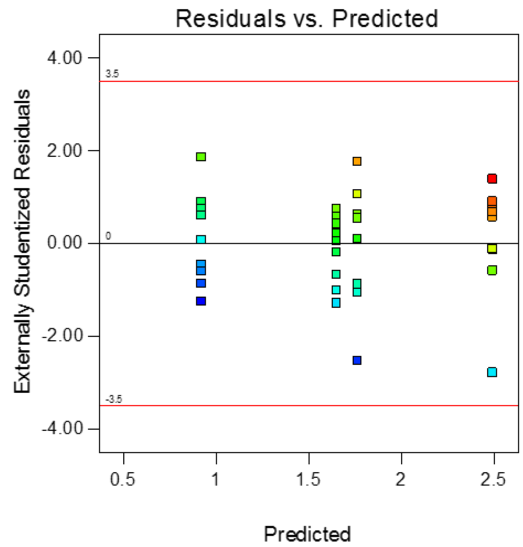


Figure 13: Residuals versus predicted values

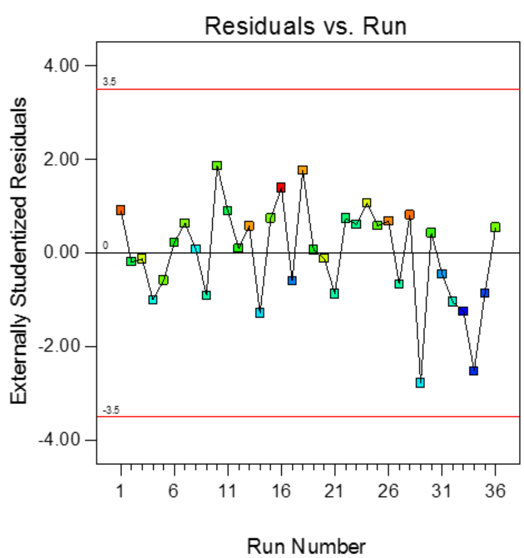


Figure 14: Residuals versus run order

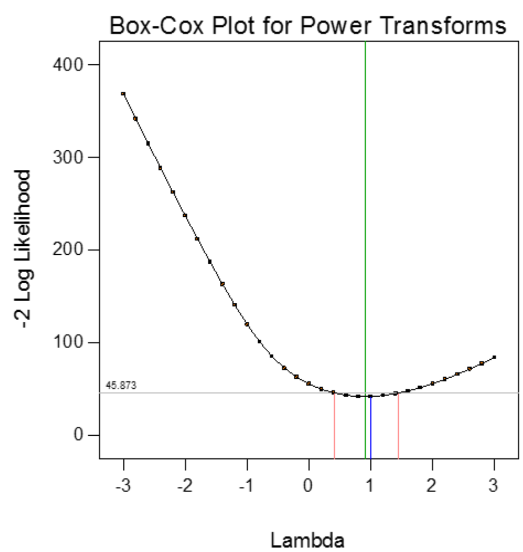


Figure 15: Box-Cox plot for power transform

3.2 Peak Clearing

Here we examine the entire sample of 36 participants tasked with 2 scenarios each (72 total simulator trials) to characterize the data in terms of the *peak ice clearing* response, measured in tenths concentration during 30-minute simulations. This metric is illustrated in the example case in Figure 7. Table 5 shows descriptive statistics for the *peak clearing* metric for the cadet (low-experience) group for "Precautionary" and "Emergency" ice management scenarios. Table 6 shows the same statistics for the seafarer (high-experience) group.

Table 5: Descriptive statistics for cadets' peak clearing (tenths concentration)

Scenario	Concentration	*Mean*	*Standard Deviation*	*Minimum*	*Median*	*Maximum*
Precautionary IM	**4**	*1.7*	*0.9*	*0.2*	*1.7*	*2.9*
	7	*2.7*	*1.1*	*1.2*	*2.5*	*4.0*
Emergency IM	**4**	*4.0*	*1.1*	*2.1*	*3.8*	*5.8*
	7	*5.1*	*1.1*	*3.4*	*5.2*	*7.0*

Table 6: Descriptive statistics for seafarers' peak clearing (tenths concentration)

Scenario	Concentration	*Mean*	*Standard Deviation*	*Minimum*	*Median*	*Maximum*
Precautionary IM	**4**	*1.3*	*0.9*	*0.2*	*1.0*	*3.1*
	7	*2.9*	*1.3*	*1.2*	*3.2*	*5.0*
Emergency IM	**4**	*4.2*	*1.0*	*2.8*	*4.0*	*5.8*
	7	*5.6*	*1.1*	*2.9*	*5.9*	*7.0*

From Tables 5 and 6, we may begin to characterize the relative performance of the two groups across scenarios and across concentration levels. For instance, just as with the *average clearing* metric, there is little difference between groups when comparing across the “Precautionary” ice management scenarios. For the “Emergency” ice management scenario there does appear to be some difference in performance, but not much. To illustrate this point, the mean *peak clearing* in the “Emergency” scenario is 4.0 ± 1.1 for the cadets; for the seafarers, it is 4.2 ± 1.0, just incrementally higher and less variable.

The boxplots in Figures 16 and 17 help to visualize the data. They are grouped by *concentration* and *experience* for all trials of the “Precautionary” and “Emergency” ice management scenarios, respectively.

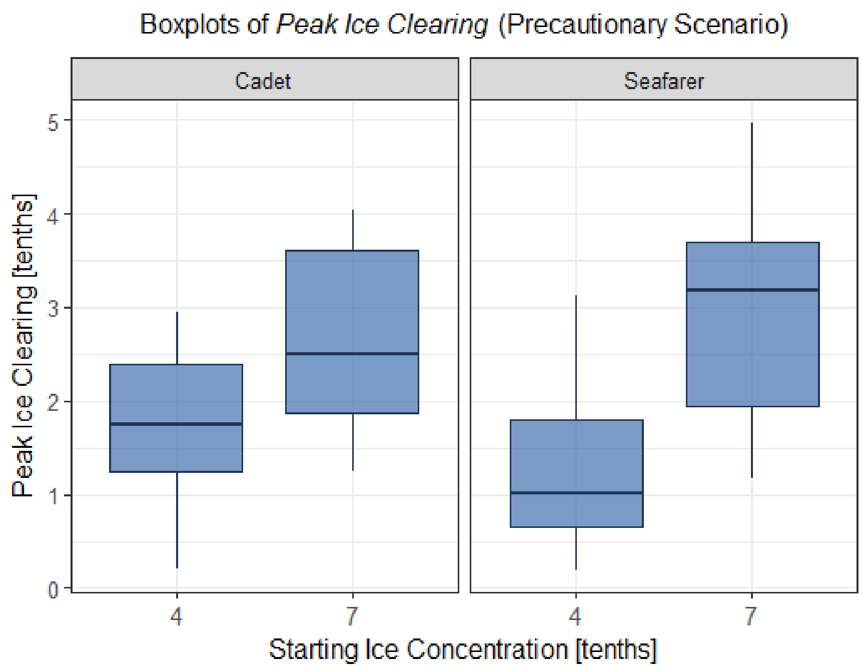


Figure 16: Boxplots of peak ice clearing for "Precautionary" scenario

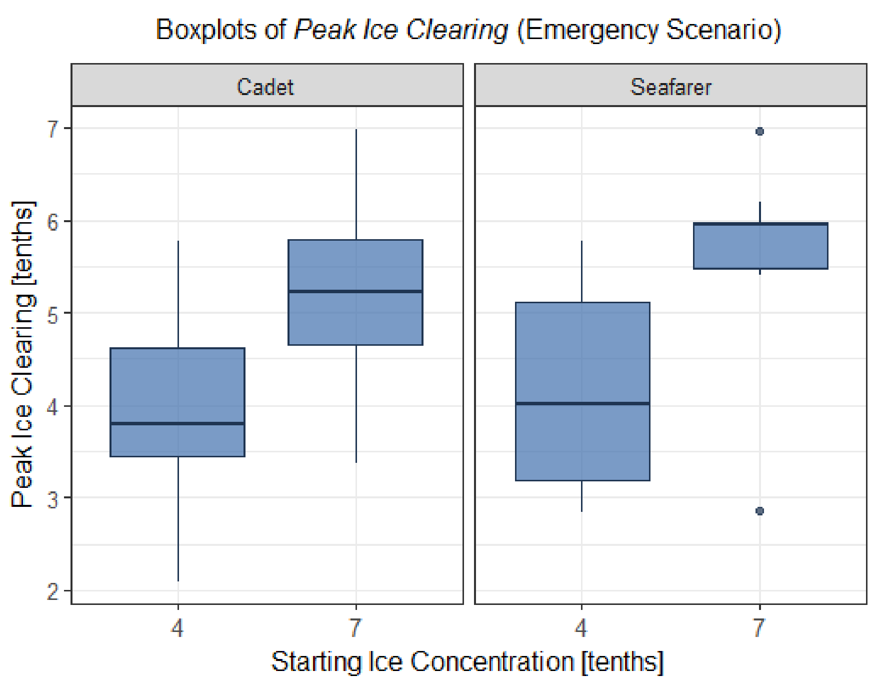


Figure 17: Boxplots of peak ice clearing for "Emergency" scenario

Figures 16 and 17 show that *experience* appears not to have an appreciable effect on *peak clearing* for either scenario. Comparing between cadets and seafarers across like ice concentration treatments, the *peak clearing* response may be higher for seafarers on average, but variability within groups overshadows this difference. The relatively high spread in performance within the seafarer group is also contrary to our hypothesis that experienced bridge officers perform ice management more effectively than their novice counterparts. For more details, see the Discussion (Section 3).

ANOVA shows unequivocally that when using *peak clearing* as a measure of ice management effectiveness, experience of bridge officers has no significant effect, with a *p*-value higher than the acceptable Type 1 error rate of $\alpha = 5\%$ (Table 7). Diagnostic checks of modeling adequacy show that assumptions of normally distributed residuals, heteroscedasticity, and independence of residuals with run order are all valid (see Appendices Section 7.2.1).

Table 7: REML ANOVA (peak clearing in "Emergency" scenario)

REML (REstricted Maximum Likelihood) analysis for selected model

Kenward-Roger p-values

Fixed Effects [Type III]

Source	Term df	Error df	F	p-value Prob > F
Whole-plot	1	32.00	0.94	0.3402
a-Experience	*1*	*32.00*	*0.94*	*0.3402*
Subplot	2	32.00	6.31	0.0049
B-Concentratior	*1*	*32.00*	*12.45*	*0.0013*
aB	*1*	*32.00*	*0.16*	*0.6880*

3.3 Total Clearing

The third performance metric in this study is *total clearing*, measured in square kilometers of ice cleared from a defined area in 30 minutes. It is derived by summing the incremental concentration drops (sampled at a rate of once every 30 seconds during the simulation) over the 30-minute scenario duration, and then multiplying this total concentration drop value by the area of the clearing zone. The derivation is expressed in Equation 3.2. The clearing zone is outlined in Figure 5.

$$Total\ Clearing = \frac{1}{2} A_z \sum_{i=1}^{n} (C_i + C_{i+1}) \Delta t \qquad \textbf{Equation 3.2}$$

where $A_z =$ Zonal area

$C_i =$ Incremental concentration drop

$\Delta t =$ Sampling interval

Equation 3.2 may be generalized by a "swept" area as a function of ice drift speed, as shown in Equations 3.3. This may be useful should experiments be repeated at a different drift rate; however, the analysis presented will adopt the form expressed in Equation 3.2.

$$A_z = W_z V t_{tot} \qquad \textbf{Equation 3.3}$$

where $W_z =$ Zonal width

$V =$ Ice drift rate

$t_{tot} =$ Total length of simulation time

Once again, we examine results of all 36 participants tasked with 2 scenarios each (72 total simulator trials) to characterize the data in terms of the chosen performance metric. Table 8 shows descriptive statistics for the *total clearing* metric for the cadet (low-experience) group for “Precautionary” and “Emergency” ice management scenarios separately. Table 9 shows the same statistics for the seafarer (high-experience) group.

Table 8: Descriptive statistics for cadets’ total clearing (km^2)

Scenario	Concentration	*Mean*	*Standard Deviation*	*Minimum*	*Median*	*Maximum*
Precautionary IM	**4**	*0.7*	*0.6*	*-0.1*	*0.6*	*1.6*
	7	*1.6*	*0.9*	*0.7*	*1.4*	*3.4*
Emergency IM	**4**	*0.4*	*0.3*	*0.05*	*0.4*	*0.9*
	7	*0.8*	*0.4*	*0.1*	*0.8*	*1.3*

Table 9: Descriptive statistics for seafarers’ total clearing (km^2)

Scenario	Concentration	*Mean*	*Standard Deviation*	*Minimum*	*Median*	*Maximum*
Precautionary IM	**4**	*0.6*	*0.7*	*-0.1*	*0.4*	*1.7*
	7	*1.9*	*1.2*	*0.3*	*1.7*	*4.1*
Emergency IM	**4**	*0.7*	*0.2*	*0.4*	*0.8*	*1.0*
	7	*1.2*	*0.3*	*0.4*	*1.3*	*1.6*

From Tables 8 and 9, just as with the *average clearing* and *peak clearing* metrics, it is once again evident that little difference exists between groups when comparing across the “Precautionary” ice management scenario. For the “Emergency” ice management scenario, on the other hand, there does appear to be a notable performance difference.

The boxplots in Figure 18 help to visualize this apparent trend. Data are grouped by *concentration* and *experience* for all trials of the “Emergency” scenario.

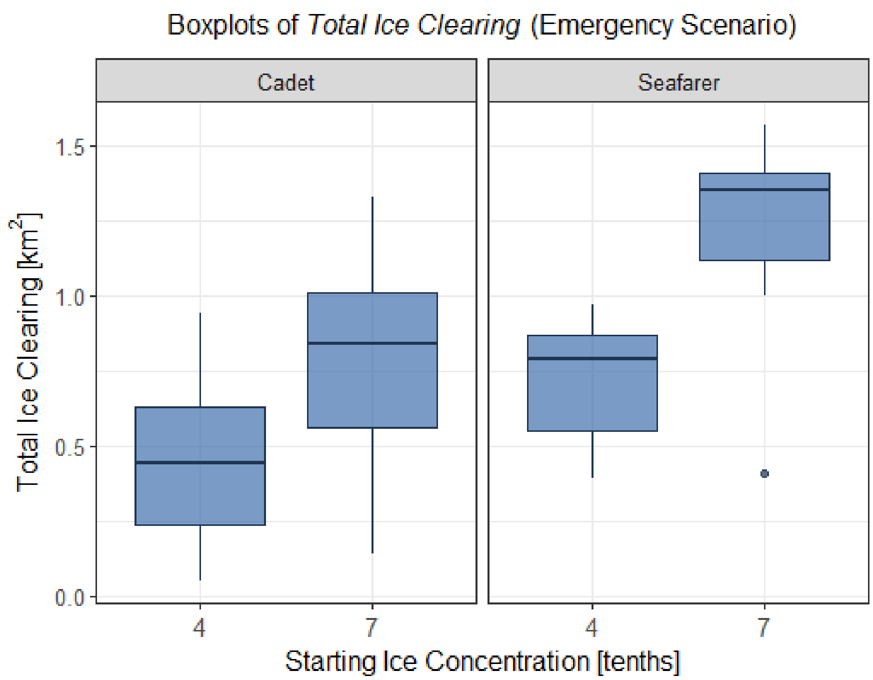


Figure 18: Boxplots of total ice clearing for "Emergency" scenario

From Figure 18, it is clear that seafarers are clearing more total ice from the defined area than the cadets at a given starting ice concentration. In fact, the general trends are remarkably similar to those of the *average clearing* performance metric (Figure 8). Just as in that case, the variability in performance appears to be higher in the cadet group, and the skew appears slightly positive in the seafarer group.

ANOVA confirms that experience is a significant factor effect when using *total ice clearing* as a measure of ice management effectiveness, with a *p*-value lower than the acceptable Type 1 error rate of $\alpha = 5\%$. (Table 10). The *experience-concentration* interaction effect is not significant. Diagnostic checks of modeling adequacy show that assumptions of normally distributed residuals, heteroscedasticity, and independence of residuals with run order are all valid (see Appendices Section 7.2.2).

Table 10: REML ANOVA (total clearing for "Emergency" scenario)

REML (REstricted Maximum Likelihood) analysis for selected model

Kenward-Roger p-values

Fixed Effects [Type III]

Source	**Term df**	**Error df**	**F**	**p-value Prob > F**
Whole-plot	1	32.00	10.92	0.0024
a-Experience	*1*	*32.00*	*10.92*	*0.0024*
Subplot	2	32.00	8.13	0.0014
B-Concentratior	*1*	*32.00*	*15.78*	*0.0004*
aB	*1*	*32.00*	*0.49*	*0.4893*

Key results of the model are shown in the effects plot in Figure 19. The plot shows the *total ice clearing* metric for all 36 participant trials of the "Emergency" ice management scenario. Data are summarized with Fisher's LSD I-bars around predictions at each treatment combination. This is an approximate way to check whether predicted means at displayed factor combinations are significantly different. As the slopes of the lines are almost parallel, it is visually clear there is no interaction effect between the two factors on the average drop in managed ice concentration.

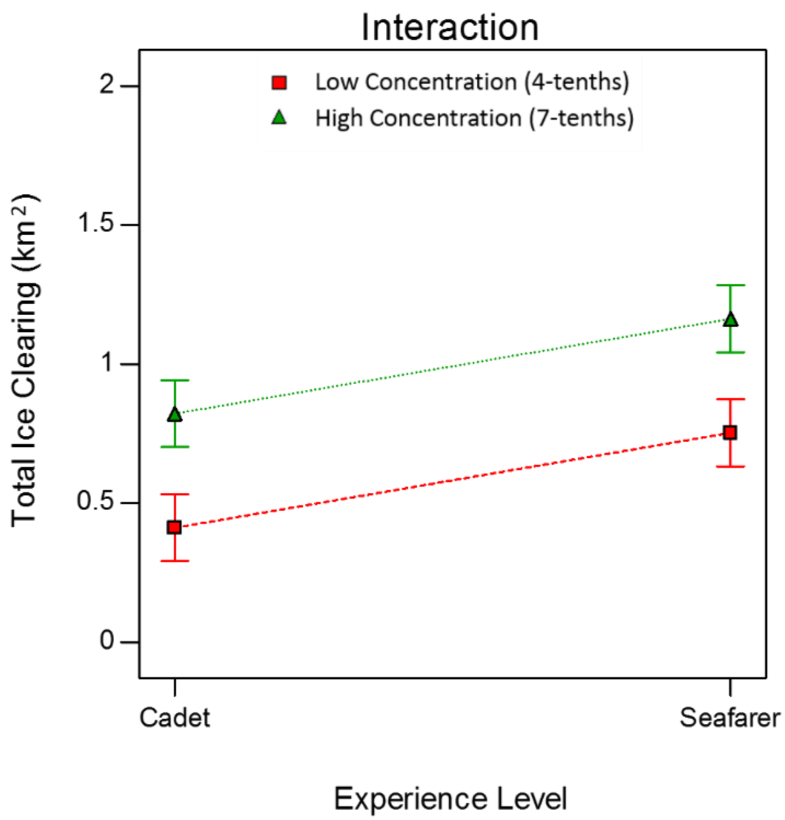


Figure 19: Interaction plot of concentration and experience on total ice clearing

3.4 Clearing-to-Distance Ratio

It was observed during simulation trials that seafarers accumulated a shorter trip distance than cadets during the thirty minutes in which they were tasked with ice management (Table 11). From the recorded observations, it should be noted that three samples were lost from the seafarer group because of data logging errors during acquisition. Because seafarers also cleared more ice on average and in total during the simulated scenarios (Sections 2.1 and 2.3), it followed that effective ice management might have been correlated with shorter trip distance. To explore this, we divided *total ice clearing* by total trip distance to get a *clearing-to-distance ratio* for each 30-minute scenario, in units of km^2 of ice cleared per km travelled. The results of this new performance metric are tabulated in a descriptive sense in Table 12 for cadets and Table 13 for seafarers.

Table 11: Total distance travelled (km)

Scenario	Concentration	*n*	*Mean*	*Standard Deviation*
Precautionary IM	**4**	*18*	*2.1*	*0.6*
	7	*18*	*1.7*	*0.4*
Emergency IM	**4**	*18*	*2.0*	*0.6*
	7	*15*	*1.8*	*1.1*

Table 12: Descriptive statistics for cadets' clearing-to-distance ratio (km^2/km)

Scenario	Concentration	*Mean*	*Standard Deviation*	*Minimum*	*Median*	*Maximum*
Precautionary IM	**4**	*0.3*	*0.3*	*0.0*	*0.3*	*0.7*
	7	*0.9*	*0.5*	*0.4*	*0.8*	*1.8*
Emergency IM	**4**	*0.2*	*0.1*	*0.0*	*0.2*	*0.55*
	7	*0.4*	*0.2*	*0.1*	*0.5*	*0.75*

Table 13: Descriptive statistics for seafarers' clearing-to-distance ratio (km^2/km)

Scenario	**Concentration**	*Mean*	*Standard Deviation*	*Minimum*	*Median*	*Maximum*
Precautionary IM	**4**	*0.4*	*0.5*	*-0.1*	*0.2*	*1.3*
	7	*1.0*	*0.6*	*0.3*	*1.15*	*1.9*
Emergency IM	**4**	*0.5*	*0.2*	*0.2*	*0.5*	*0.6*
	7	*0.8*	*0.4*	*0.3*	*0.85*	*1.6*

From Table 12 and Table 13, just as with all previous performance metrics, it is once again evident that little appreciable difference exists between groups when comparing across the "Precautionary" ice management scenario. For the "Emergency" ice management scenario, on the other hand, there does appear to be a notable performance difference.

From Tables 12 and 13, just as with all previous performance metrics, there is little appreciable difference between groups when comparing across the "Precautionary" ice management scenario. For the "Emergency" ice management scenario, on the other hand, there does appear to be a notable performance difference.

The boxplots in Figure 20 help to visualize the data. They are grouped by *concentration* and *experience* for all trials of the "Emergency" ice management scenario.

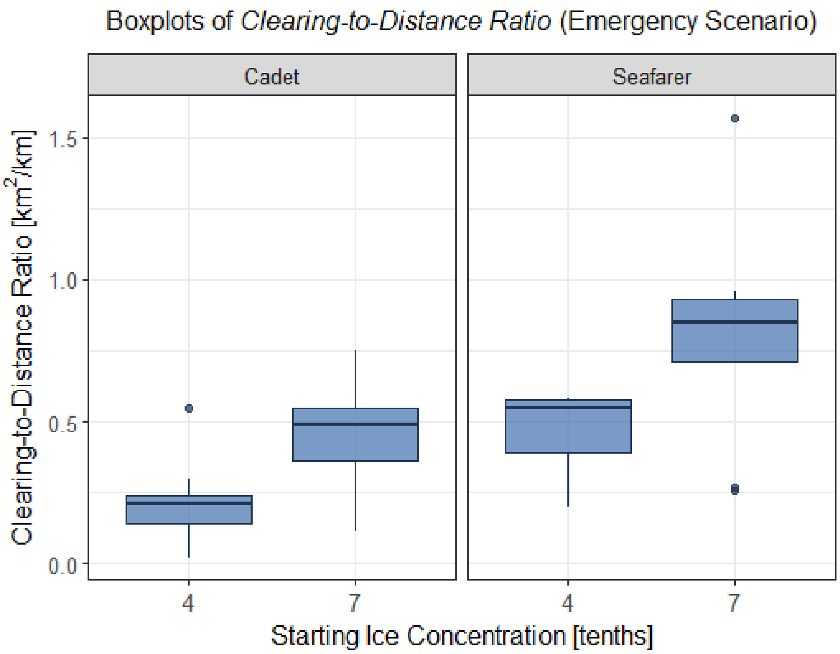


Figure 20: Boxplots of clearing-to-distance ratio for "Emergency" scenario

From Figure 20, it is clear that seafarers are clearing more total ice per distance travelled from within the defined area compared to cadets at a given starting ice concentration. The variability tends to be slightly higher in the seafarer group. This may be an indication that a relatively large spread of expertise levels may exist within the experienced seafarers, despite being better performers than the cadet group overall. More on this subject is presented in the Discussion (Section 4).

ANOVA confirms that experience is a significant factor effect when using the *clearing-to-distance ratio* as a metric of ice management effectiveness, with a *p*-value lower than the acceptable Type 1 error rate of $\alpha = 5\%$. (Table 14). Also, the *experience-concentration* interaction effect is not significant. Diagnostic checks of modeling adequacy show that assumptions of normally distributed residuals, heteroscedasticity, and independence of residuals with run order are all valid (see Appendices Section 7.2.3).

Table 14: REML ANOVA (clearing-to-distance ratio for "Emergency" scenario)

REML (REstricted Maximum Likelihood) analysis for selected model

Kenward-Roger p-values

Fixed Effects [Type III]

Source	Term df	Error df	F	p-value Prob > F
Whole-plot	1	29.00	12.39	0.0014
a-Experience	*1*	*29.00*	*12.39*	*0.0014*
Subplot	2	29.00	0.67	0.5176
B-Concentratior	*1*	*29.00*	*1.31*	*0.2624*
aB	*1*	*29.00*	*0.095*	*0.7607*

Key results of the model are shown in the effects plot in Figure 21. The plot shows the *clearing-to-distance ratio* metric for all 36 participant trials of the "Emergency" ice management scenario. Data are summarized with Fisher's LSD I-bars around predictions at each treatment combination.

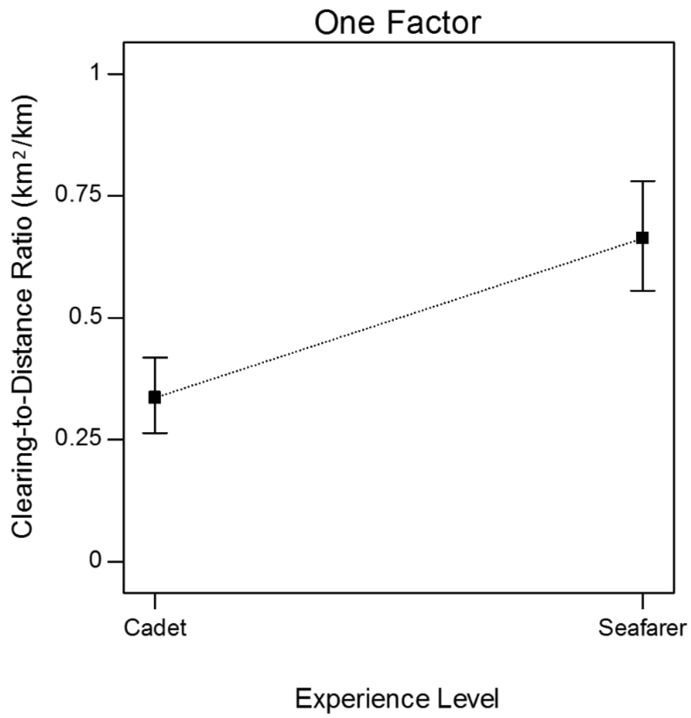


Figure 21: Main effect plot of clearing-to-distance ratio versus experience

A square root power transformation was applied to stabilize variance and thereby validate modelling assumptions (lambda = 0.5 in Box-Cox plot in Figure 22).

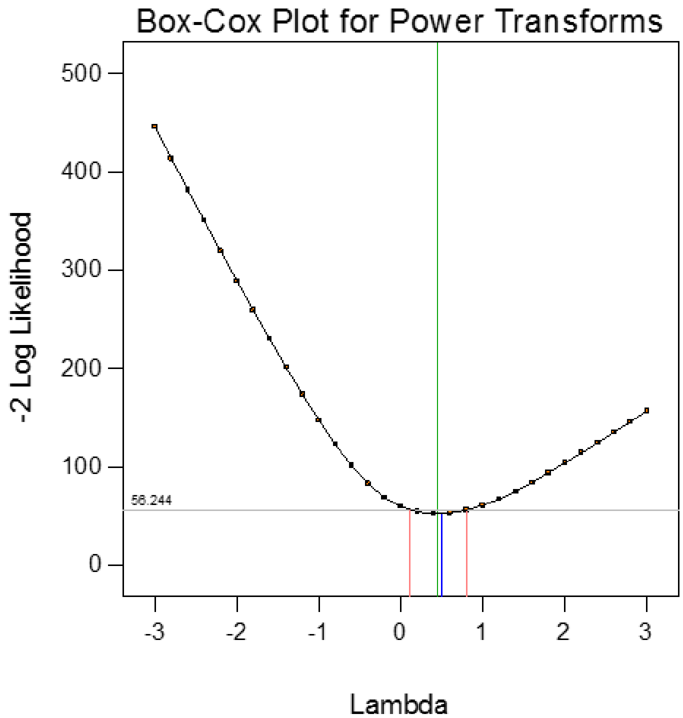


Figure 22: Box-Cox plot for power transform (clearing-to-distance ratio model)

3.5 Ice-Free Lifeboat Launch Time

During the "Emergency" scenario, each participant was told "to clear ice from underneath the port lifeboat launch zone." The lifeboat was visible near the port quarter of the FPSO, and participants had remarked in exit interviews that this visual aid had helped guide them to the location in which clearing was required. Although it was not the original intention, it followed that the *cumulative ice-free lifeboat launch time*, measured in minutes, would be a good metric of performance for this scenario.

To set up an appropriate analysis, the size and location of the lifeboat launch zone had to be specified. The lifeboat drop zone radius was set at 8 m, based on the size required to accommodate an 80-person capacity totally enclosed motor propelled survival craft (TEMPSC) typical of those found on FPSOs. These lifeboats have dimensions of approximately 10 m in length and 3.7 m in breadth. Results of experiments by Simões Ré et al. (2002) found that a target drop point radius of approximately 1.5 m accommodated launches for a TEMPSC from an offshore installation [29]. The 8 m "splash-zone" radius which was set to circumscribe this target area would conservatively encompass offsets. These offset distances might occur due to missed target points and setbacks by first wave encounters. The origin of the zone was set at 8 m off the side of the port quarter of the FPSO, so that the zone was tangent to the side of the FPSO hull. A schematic showing the lifeboat launch zone is presented in Figure 23.

The cumulative time that the lifeboat splash zone was ice-free was computed using Matlab image processing software. Successive replay files, captured at 30-second intervals during simulation, were cropped to the shape and size of the lifeboat splash zone. From here a pixel count of the raster image was computed; if the image had more colored pixels than that of a cropped blank image, it meant that ice (or the own-ship itself in rare instances) was in the lifeboat zone. For each successive ice-free image, a 30-second time increment was added to the total time the zone was ice-free. The resulting total cumulative time was therefore an approximation. Given that at a rate of current drift of 0.5 knots ice would drift no more than 8 m in 30 seconds, coinciding with the radius of the lifeboat splash zone, this approximate cumulative ice-free time estimate was considered appropriate for this study.

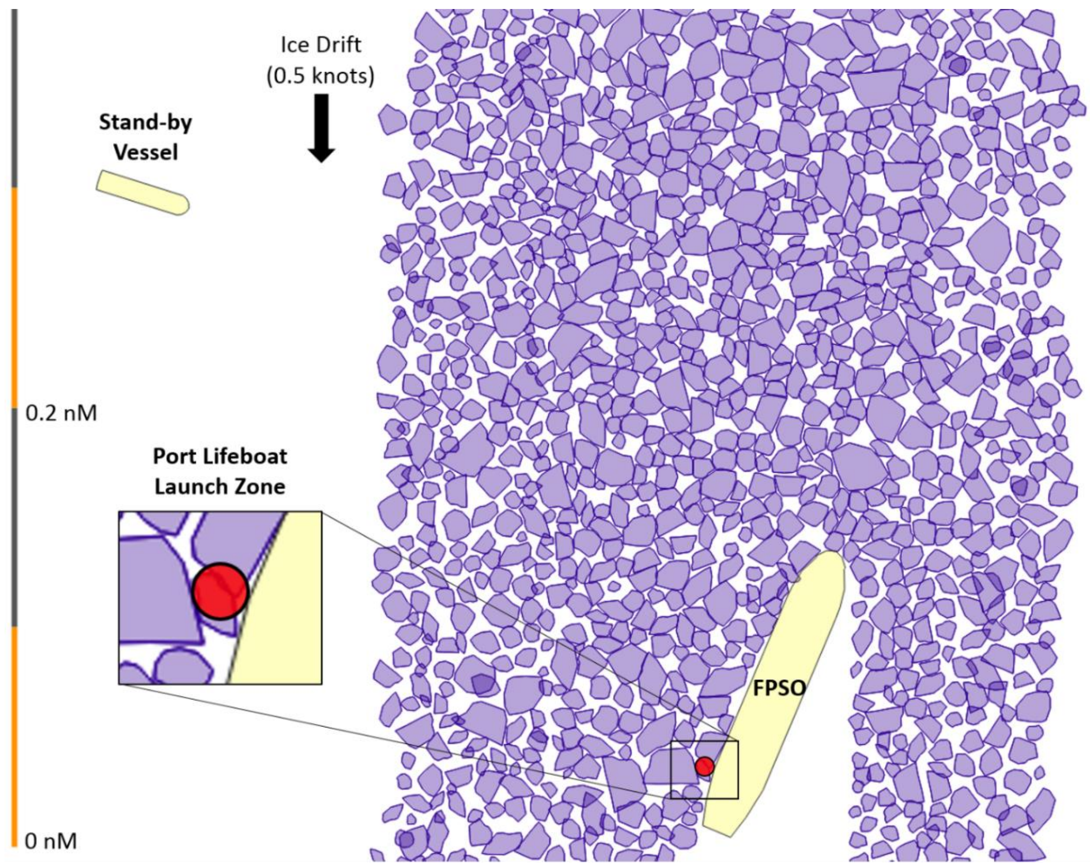


Figure 23: Port lifeboat launch zone (not to scale)

We can look at the sample of 36 participants tasked with the "Emergency" scenario to characterize the data in terms the ice-free lifeboat launch zone performance metric. This metric is measured in minutes of cumulative time that no ice is present in the 8 m radius lifeboat launch zone. Table 15 shows descriptive statistics for the *cumulative ice-free time* metric for the cadet (low-experience) group for the "Emergency" ice management scenario. Table 16 shows the same statistics for the seafarer (high-experience) group.

Table 15: Descriptive statistics for cadets' cumulative ice-free lifeboat launch times (minutes)

Scenario	**Concentration**	*Mean*	*Standard Deviation*	*Minimum*	*Median*	*Maximum*
Emergency IM	**4**	*7.3*	*4.4*	*0.0*	*6.5*	*16.5*
	7	*4.5*	*4.1*	*0.0*	*4.0*	*11.5*

Table 16: Descriptive statistics for seafarers' cumulative ice-free lifeboat launch times (minutes)

Scenario	**Concentration**	*Mean*	*Standard Deviation*	*Minimum*	*Median*	*Maximum*
Emergency IM	**4**	*10.0*	*3.2*	*7.0*	*8.5*	*16.5*
	7	*11.6*	*7.0*	*0.0*	*13.0*	*19.0*

From Tables 15 and 16 there is a clear difference between experience groups. The mean cumulative time the lifeboat zone was ice free is consistently higher for the seafarers. This trend is particularly striking at the high concentration treatment (7-tenths), in which seafarers kept the lifeboat zone ice-free more than twice as long, on average, than cadets.

The boxplots in Figure 24 help to visualize the data. They are grouped by *concentration* and *experience* for all trials of the "Emergency" ice management scenario.

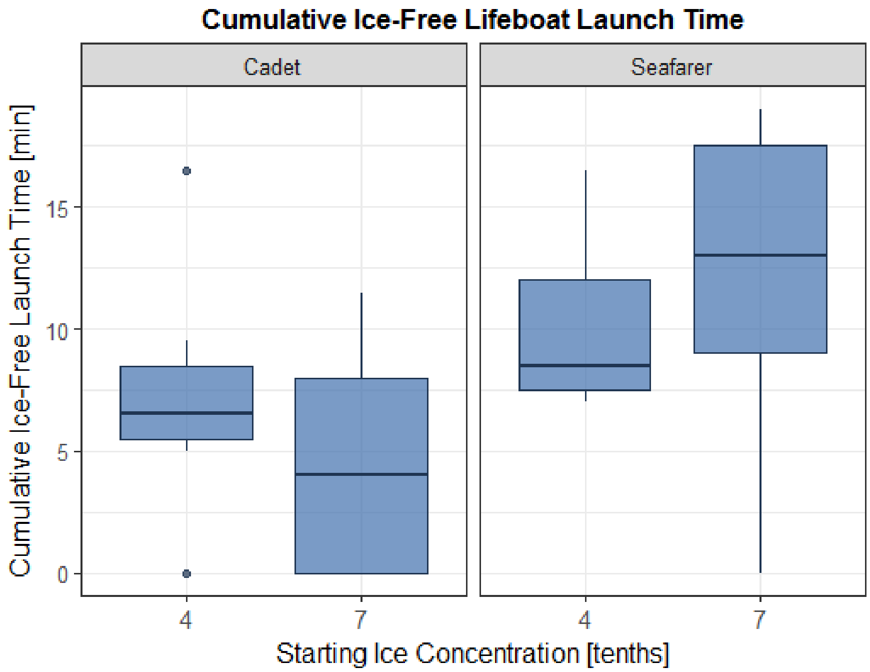


Figure 24: Boxplots of cumulative ice-free lifeboat launch times during "Emergency" scenario

From Figure 24 some additional characterizations can be made about the data. The overall difference between groups in terms of *cumulative ice-free lifeboat launch times* is significant. For example, the median values for the seafarer group at both concentration treatments are higher than even the respective third quartiles recorded for the cadet group. The spread is lower for the seafarers in the low concentration treatment (4-tenth), but higher in the high-concentration treatment (7-tenths). Despite the better performance overall, the high spread may indicate a relative higher degree of expertise variability within the seafarer group. This is a similar trend observed as when using *total ice clearing* to measure performance. Also, it is remarkable that responses are higher for the 7-tenths treatment in the seafarer group. The response in this case is independent of starting ice concentration and the high-level treatment (7-tenths) was expected to represent a significant challenge compared to the low treatment (level 4-tenths). This surprising result cannot be explained by experience difference within the seafarer group, either, as experience level for both treatments were similar (4-tenths treatment: seafarers' average years at sea = 20 ± 9; 7-tenths treatment: seafarers' average years at sea = 20 ± 10).

ANOVA shows that experience is a significant factor on the *cumulative ice-free lifeboat launch time*, with a *p*-value higher than that prescribed by the acceptable Type 1 error rate of $\alpha = 5\%$. (Table 17).

Table 17: REML ANOVA (cumulative ice-free lifeboat launch time)

REML (REstricted Maximum Likelihood) analysis for selected model

Kenward-Roger p-values

Fixed Effects [Type III]

Source	Term df	Error df	F	p-value Prob > F
Whole-plot	1	32.00	6.84	0.0135
a-Experience	*1*	*32.00*	*6.84*	*0.0135*
Subplot	2	32.00	1.03	0.3683
B-Concentratior	*1*	*32.00*	*1.10*	*0.3025*
aB	*1*	*32.00*	*0.96*	*0.3338*

Key results of the model are shown in the effects plot in Figure 25. The plot shows the *cumulative ice-free time* metric for all 36 participant trials of the "Emergency" ice management scenario. Data are summarized with Fisher's LSD I-bars around predictions at each treatment combination.

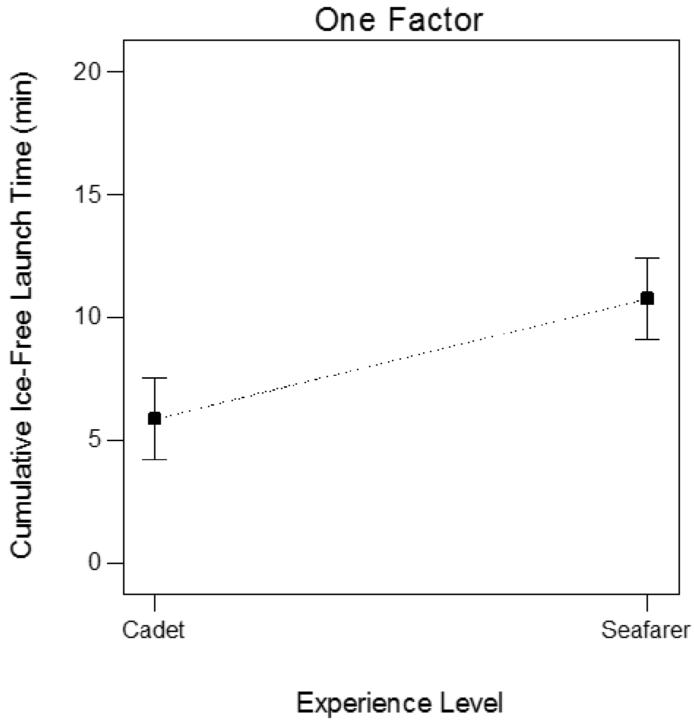


Figure 25: Main effect plot of cumulative ice-free lifeboat launch time versus experience

Diagnostic checks of modeling adequacy show that assumptions of normally distributed residuals, heteroscedasticity, and independence of residuals with run order are all valid. A square root power transformation was applied to stabilize variance and thereby validate modeling assumptions (lambda = 0.5 in Box-Cox plot in Figure 26).

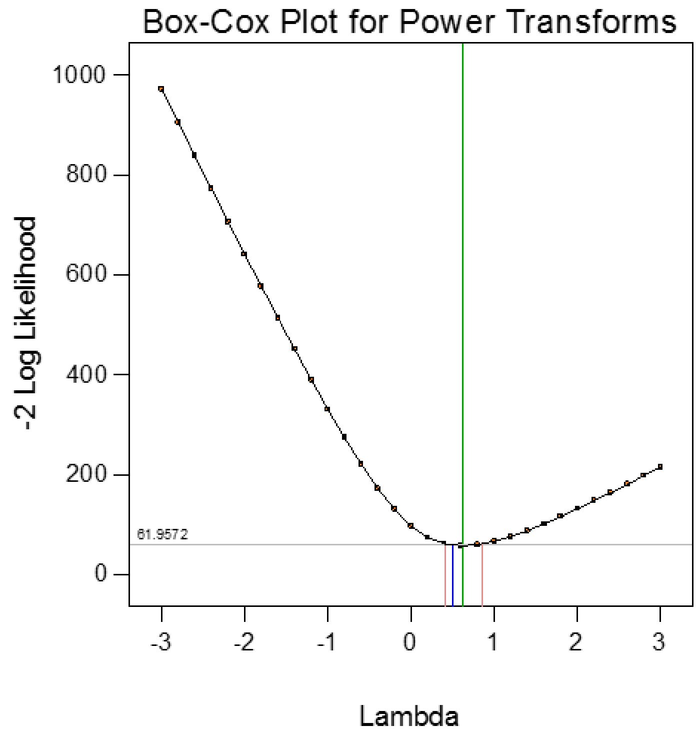


Figure 26: Box-Cox plot for power transform (cumulative ice-free lifeboat launch time model)

3.6 Ice Management Tactics

So far, considerable effort has gone into showing that a statistically significant difference exists between experienced seafarers and inexperienced cadets when it comes to performance in ice management. To measure performance in ice management, we used five different metrics of overall effectiveness: *average clearing*, *peak clearing*, *total clearing*, *distance-to-clearing ratio*, and *cumulative ice-free lifeboat launch time*. Differences were detected between groups only when making assessments with the "Emergency" scenario. The "Precautionary" scenario (Figure 4) failed to detect any differences between experience groups. Suggestions as to why this may be the case are presented in the Discussion (Section 3). For the "Emergency" scenario, with the exception of peak clearing, each metric showed that experience had a significant influence on ice management effectiveness. These findings underscore an important question: what is the seafarer group doing that makes them more effective than the cadets? We examine this question in this section.

As a starting point, we may begin to understand what effective ice management looks like by plotting the tracks taken by seafarers during the 30-minute simulation. Additionally, if we trace all the tracks taken by seafarers in one plot, and then repeat this for cadets' tracks, we should begin to see spatial differences in maneuvers that may characterize underlying differences in tactics. The problem is, if we do this we end up with messy plots from which it is difficult to ascertain meaningful results. One solution is to present "heatmaps" of the respective groups' tracks (Figures 27 and 28). The heatmaps are constructed by dividing the simulation area into bins and counting instances in which the own-ship passes through a given bin during simulations. The aggregate counts for a given scenario are assigned colors: the higher the number, the brighter the corresponding color for that bin. This way we have a clearer way of visualizing the two groups' aggregate tracks. Figures 27 and 28 display such tracks for the high-level (7-tenths) concentration cases for the "Emergency" scenario for cadets and seafarers, respectively. Similar plots can be constructed for the low-level (4-tenths) concentration cases, although they are not included here because the differences between experience levels is most pronounced in the 7-tenths concentration level.

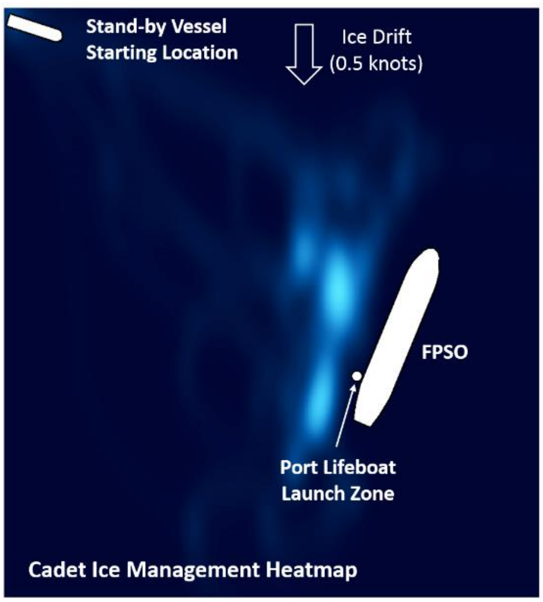


Figure 27: Heatmap for cadets' tracks during "Emergency" scenario

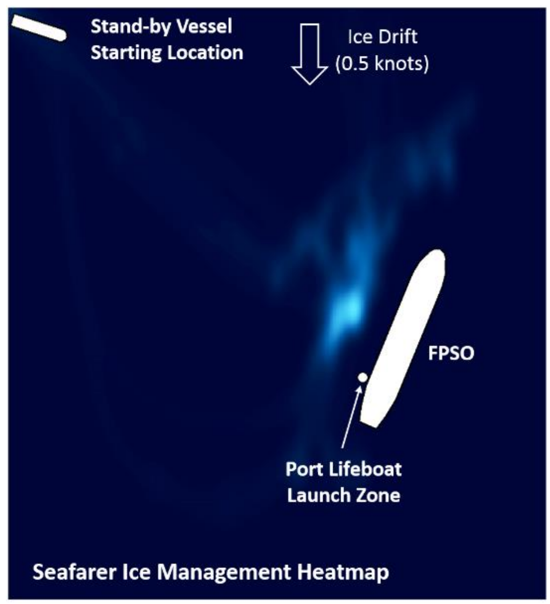


Figure 28: Heatmap for seafarers' tracks during "Emergency" scenario

From Figures 27 and 28 it is obvious that spatial differences exist between the cadet group and the seafarer group. For one, seafarers appear to focus their position on a single area (visible as the only bright patch in the heatmap in Figure 28), whereas cadets are divided into two or three relatively large areas (Figure 27). From this insight, the seafarers' chosen maneuvering tactics may be characterized as more uniformly executed. Because participants were actively discouraged from discussing the experiment with others, each trial was independently orchestrated. And yet, there appears to be a high degree of similarity among seafarers' chosen tactics. Specifically, they appear to focus just upstream of the port lifeboat launch zone. The position tactic proved to be effective, as evidenced by results of the *clearing-to-distance ratio* metric (Section 2.4), which detected a distinct difference between experience groups in how much ice was cleared per kilometer travelled.

From boxplots of *cumulative ice-free lifeboat launch time* (Figure 24) there was considerable variability in seafarer performance. This same trend is visible for other metrics (Figures 8, 18, and 20). So, despite seafarers performing more effectively than cadets on average, it merits a closer examination within the seafarer group to determine what may distinguish an individual's effective trial compared to another individual's ineffective one. Figure 29 plots two tracks based on the midships position of the own-ship during the 30-minute simulations scenario. The two tracks represent the best and the worst of all seafarers, where best and worst are measured by corresponding highest and lowest amounts *cumulative ice-free lifeboat launch time*, respectively. The resulting difference represents the largest single gap in performance between any two individuals for this scenario, including cadets. Clearly, positioning makes a difference, and the "best" track in this case demonstrates a highly effective tactic. The track shows a straightforward line heading toward the lifeboat launch zone, where it stops upstream, swings about to create a lee down-drift of its port side and holds position for the duration of the simulated scenario. The "worst" track, on the other hand, plots a track farther upstream of the FPSO and lifeboat launch zone, covering almost twice as much as distance as the "best" track.

An inspection of the Replay files provides further clues as to what may distinguish successful tactics. Figures 30 and 31 show that heading with respect to the ice also plays a major role. For instance, the best trial (Figure 30) shows that a "wedging" maneuver (so-called by the seafarer who produced it), whereby the vessel's quarter is positioned close to the FPSO and its heading is approximately 30 degrees off the FPSO heading, effectively traps the ice between the two vessels. Ice accumulated and eventually drifted around the wedge created by the stand-by vessel, effectively clearing the area downstream that required attention. The worst trial (Figure 31) appears to show an attempt to clear ice sideways, using the side thrusters to clear ice while maneuvering upstream. The issue with this appears to be that ice drifting into the bow of the FPSO was deflected along the length of the port side by the current. From here the ice subsequently drifted into the lifeboat launch zone, unimpeded by the vessel's presence. Moreover, during the best trial (Figure 30), use of the aft console allowed good visibility of the deck, which would have been an advantage while working close to the FPSO. For the worst trial, on the other hand, the stand-by vessel was oriented bow-on to the FPSO, and visibility over the bow would have been limited. Much of the ice between the bow and the FPSO would have been completely hidden from view. Inspection of the Replay files can therefore provide valuable insights into good practices in ice management to complement plots of tracks, which show a more general picture of overall tactics.

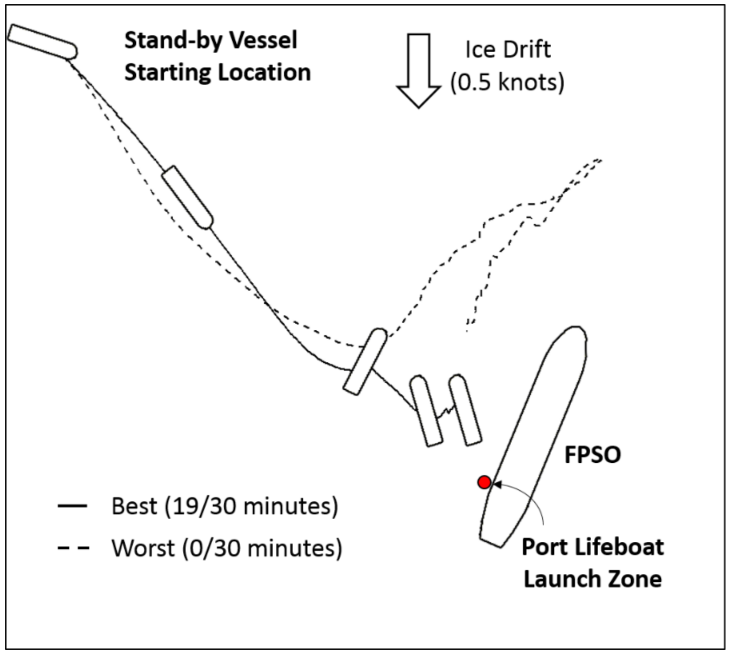


Figure 29: Plot of best and worst tracks for "Emergency" scenario.

Criterion is cumulative ice-free lifeboat launch time.

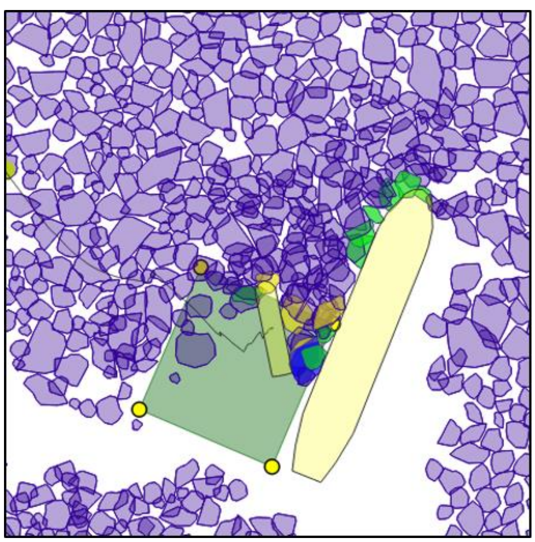


Figure 30: Midway mark (15 min) during "Emergency" scenario for best trial

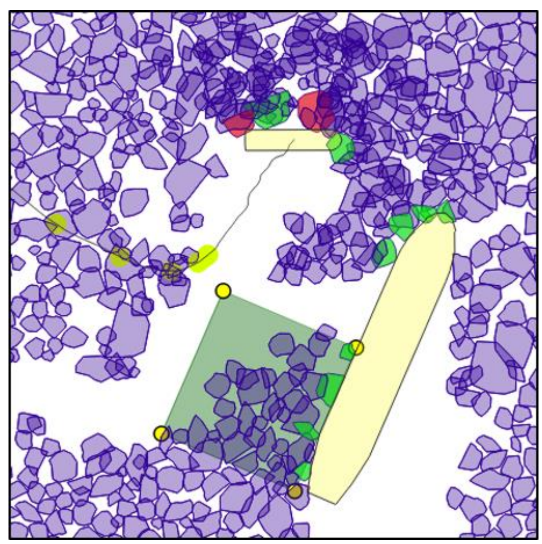


Figure 31: Midway mark (15 min) during "Emergency" scenario for worst trial

Exit interviews were conducted for all participants and these may also offer important clues about the tactics. For example, the “best” track from Figure 29 (depicted in the Replay images in Figure 30) was performed by a seafarer code-named C79, who was asked during the after-action interview to reflect on the tactics undertaken during the simulation scenario. C79 was a master with approximately 30 years of experience at sea, which included more than 10 seasons of operations in sea ice (including ice management), most recently within the past 3 years. Note than identifying details are not provided to respect participants’ anonymity. Asked about the strategy employed in the simulation scenario, C79 stated:

> *I caught a large floe, which is advantageous to push against. My stern thrusters were at 100% [power allocation], pushing against trapped ice. I used side thrusters to maintain position. I tried to maintain a 30-degree heading using my thrusters. Ice travelling down would drift down and around [my bow]. I moved fast at the start to take advantage of clear water. Then I slowed in ice to less than 3 knots.*

Compared to the Replay file imagery and the plots of tracks before that, exit interview transcripts such as this provide valuable qualitative information about ice management tactics. For example, we now know that a large ice floe was used strategically to block others, and that the ice trapped in the “wedge” was almost overpowering the own-ship. Additionally, when asked about what factors might be important for success in such a scenario, C79 replied, “[One should] get set up instead of moving too much.” The track plot (Figure 29), which showed a direct path to a location upstream of the lifeboat launch zone and minimal movement thereafter, corroborates this assertion, as does the quantitative metric *clearing-to-distance ratio* (Section 2.4), which showed that ice clearing and distance travelled were inversely correlated (and for which C79 was the peak performer).

In comparison, the “worst” track was performed by a master, code-named S41, who had accumulated 10 years of experience. This included 3 to 10 seasons of operations in sea ice (including ice management), most recently within the past 3 years.

When asked about tactics employed for the “Emergency” scenario, S41 stated that he or she had been attempting to implement an industry procedure. Details about which procedure this referred to were not provided. When asked about what changes might be done in a hypothetical repeat trial of the simulated scenario, S41 stated:

> *[In a repeat I would] come up closer to the bow of the FPSO. I would’ve cleaned out ice closer [to the FPSO], stern-first.*

Interestingly, S41’s remarks about positioning closer to the FPSO and maneuvering stern-first are both characteristics observed during C79’s successful maneuver. This suggests that had S41 had the chance to repeat the trial, he or she would have applied a tactic similar to that of C79. Although learning effects were not directly measured in this experiment, this is a strong qualitative indicator that learning effects may exist for ice management simulator training.

4 DISCUSSION AND FUTURE WORK

The experimental campaign undertaken highlights the importance of appropriate simulation scenario design when assessing ice management effectiveness in a marine simulator. The "Precautionary" scenario (Figure 4) failed to detect any significant difference between the two experience groups, whereas the "Emergency" scenario did. In the former, individuals were tasked with keeping the port and starboard lanes of the FPSO clear of ice as a precautionary measure so that lifeboats would be able to launch safely in the event of a major event. Why did this scenario fail to detect differences between experience groups? The question may be best answered by the experienced seafarers who performed it. Transcripts of exit interviews taken shortly after simulation showed that out of 18 seafarers, 7 said that "clearing of both sides was not possible with a single vessel", with 5 of these 7 specifying that "two vessels are required for such a scenario." Furthermore, 2 individuals stated that they had performed a similar precautionary ice management exercise "in real life," and in those cases at least two stand-by vessels had been on-site to complete the job. It follows that the scenario would have been better suited for two or more stand-by vessels working together, rather than just the one own-ship. In other words, the "Precautionary" scenario was challenging by virtue of having a single vessel attempting a multi-vessel mission. This challenge overshadowed differences in performance that could be explained by differences in officer experience. This serves to illustrate an important design element for experimenters and simulator course developers: care and attention must go into developing an appropriate scenario if one hopes to measure an effect.

Care must also be taken when applying performance metrics to simulation scenarios. In this study, when using a specified region in which to measure pack ice modification, global measures of average and total ice clearing indicated significant differences between experience groups, whereas the event-specific peak ice clearing indicated no differences at all. This finding may help model course developers and experimenters select appropriate performance metrics for similar applications. Moreover, for the *clearing-to-distance ratio* and *cumulative ice-free lifeboat launch time* performance metrics, only experience was found to be a significant factor on effectiveness response. This meant that effectiveness was independent of starting ice concentration for these two metrics, making them ideal candidates for measuring performance in simulation scenarios.

There were some important limitations with the simulator that must be noted. Most importantly, this included the inherent difficulties associated with judging spatial distance from a two-dimensional screen. With no radar present, either, the officers had to rely on the VHF radio for information about distances to specified targets. The officers' watch-keeper (role-played by an experimenter at the Instructor Station) would report back distances measured from a built-in software tool, when asked. Another limitation of the simulator stemmed from the omission of transverse and astern velocities on the display screen in the bridge console. Only total speed and heading were displayed. During ice management close to the FPSO, subtle shifts in speed in any direction were important for the officer to know about as he or she adjusted controls to maintain good position. Finally, visibility from the simulator bridge console was an issue. In a real ship, officers can walk to the wing and look out along the side of the vessel to see ice. This probably made ice management in the simulator more challenging since ice was effectively invisible off the bow and sides of the own-ship. Improvements could perhaps be made to the simulator technology and the resulting data collected would likely be more reliable. Regardless, the simulator used in the experiment met the intended scope of the research: it was sufficiently representative to detect differences between bridge officer experience groups and it was able to characterize general principles of good ice management practices.

So far, it has not been mentioned whether the seafarers had had formal training in ice prior to completing the test trial. This information was collected in experience questionnaires and it was found that out of the 18 experienced seafarers tested, only 6 reported to have had formal training in ice management, with 4 of these describing their training as "Basic" and the remaining 2 describing it as "Advanced." Despite this, 17 out of 18 reported to have done ice management in the field. The lack of formal training in ice may explain the high degree of variability generally observed in performance measured within the seafarer group (Figures 8, 18, 20, and 24).

It was also observed during exit interviews that 5 out of 12 seafarers operating OSVs and AHTS vessels stated that they were accustomed to dynamic positioning (DP) for station keeping and maneuvering, which is the industry norm for vessels of these types. However, from the exit interview it was clear that few, if any, officers would rely on DP for station keeping in drifting pack ice. Apparently, this was due to the nature of loading that sea ice imposes on the hull. With this in mind, the experiments may be viewed as scenarios presenting conditions that required manual take-over of positioning controls. Bainbridge (1983) argues that the skills required for manual take-over from an autonomous control system like DP need special attention and training if they are to be accomplished with success [30]. In our experiments, only a third of operators had formal training in this area despite all but one having completed it in the field, potentially highlighting a gap in training for manual take-over of vessel position and station keeping in drifting pack ice.

Learning effects, which are observed when a scenario is repeated with improved results, are often of interest in simulator experiments. Learning effects were not directly measured in this experiment because repeat trials were not conducted. Still, they were indirectly detected from the interviews. Specifically, we compared the individuals' performance as-measured in the "Emergency" scenario to corresponding performance scores as-reported in a self-assessment during exit interviews. The self-reported score was on a subjective scale of 1 to 5, where 1 was poor and 5 was excellent. For the measured performance, the *cumulative ice-free lifeboat launch times* for each trial were scaled linearly from 1 to 5 so that 1 equated to 0 minutes and 5 equated to approximately 18 minutes (95^{th} percentile of recorded cumulative times). Note that because the 95^{th} percentile was used, some "actual" scores were above 5. The results were plotted and a locally weighted scatterplot smoothing (LOESS) fit (weight parameter = 0.75) was used to explore trends (Figure 32). The LOESS fit matched very closely to the unity line, indicating that on average, participants were strikingly accurate when perceiving the effectiveness of their own performance. A closer look, though, reveals this trend is only slightly linear (Pearson's $\rho = 0.56$, $p = 0.0003$), indicating that this perception is only accurate on average for a relatively large group size and that variation is quite high. Still, this finding indicates that on average, individuals may accurately recognize their own needs for training after having completed a simulation scenario.

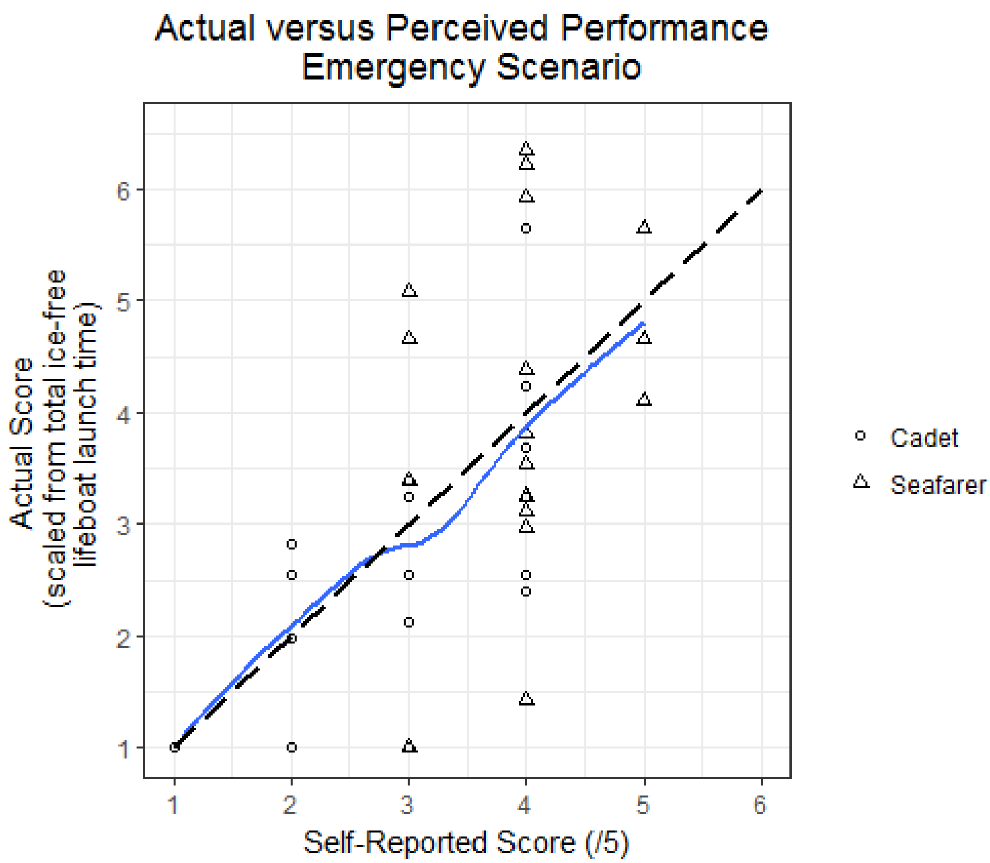


Figure 32: Plot of actual versus self-reported performance scores with LOESS line

5 CONCLUSIONS

Experienced crew have been shown to perform ice management more effectively than inexperienced crew in a simulator experiment. Based on the results, it can be concluded that experience level of bridge officers onboard a vessel tasked with an ice management operation will significantly influence the resulting effectiveness. Effectiveness was measured in four ways: i) *average ice clearing* in a defined area (tenths concentration), ii) *total ice clearing* in a defined area (km^2), iii) *clearing-to-distance ratio* (km^2/km), and iv) *cumulative ice-free lifeboat launch time* (in minutes). The latter of the four metrics applied to the "Emergency" scenario, only.

The hypothesis that the human factor of expertise influences effectiveness of ice management operations has been formally tested and accepted in this experimental campaign. Also, the hypothesis that the human factor of experience level has a larger effect when combined with higher concentration (positive interaction effect) has been formally tested and rejected.

The question about what made experienced seafarers more effective than inexperienced cadets was addressed using a combination of quantitative and qualitative data. The data came from three sources: i) plots of position during simulation, ii) screenshots captured from Replay files, and iii) exit interviews. This information allowed us to characterize good ice management practices in terms of a chosen performance metric for our simulated scenario.

Overall, the results provide a basis for assessing what the experienced crew does that the inexperienced crew does not in terms of operating tactics. This could offer insights for informing good practices in ice management as they apply to offshore operations. The gap between the two groups, as well as the variability within the respective groups, also provides a quantitative basis for the design of a training curriculum that could close the performance gap and reduce its variability.

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7 APPENDICES

7.1 Power Analysis and Experiment Sizing

A common question that arises in experimental design is *how many samples is enough?* The question suggests that there is an optimal number of samples: one that meets the needs of the experiment by detecting the effect under investigation with sufficient accuracy, but one that does not exceed that number and that does not waste valuable time and resources in doing so [22], [31]. An estimate for the minimum sample size required for this experiment was produced using a prospective power analysis.

A prospective power analysis means that the experiment is sized based on a defined statistical power level and based on an estimate of the spread, or standard deviation, of the response being analyzed. As a first step, it is important that the objective of the experiment be defined. For illustrative purposes, we may define the objective as follows: the goal of the experiment is to compare *cumulative ice-free lifeboat launch times* during a 30-minute-long ice management simulation between low- and high-experience groups. Such a comparison suggests a comparison test between means, like the basic 2-sample *t*-test. As such, we can produce an estimate of the minimum sample size required for our experiment based on a power analysis of 2-sample *t*-test using this response variable. Note that this will produce an estimate only (effect estimates of the actual results were computed more precisely using REML ANOVA techniques in Section 3). The results of prospective power analyses such as the one shown here will therefore not be exact; however, given that it is a tool meant to inform sizing given the best available information prior to running the experiment, such a methodology is suitable for this application. The procedure also relies upon the assumption that response data will be normally distributed and that each group will have the same distribution. These are strong assumptions and are especially so given the lack of prior data; as such, it is highlighted once again that power analyses should be used as a guide only and not as an explicit method for determining minimum sample size.

The method and results shown here were produced in Minitab (Version 18.1). Curves of statistical power for the experiment were computed given the standard deviation of the response and any two of the three following items: i) sample size, ii) difference in means (effect size), or iii) power values. Because we wished to obtain curves for sample size, the latter two of the three will be defined over a range of possible differences in means that we might expect. This is computed at an acceptable Type 1 error rate of $\alpha = 0.05$, as shown in the summary of inputs in Table 18.

Table 18: Inputs for prospective power analysis based on 2-sample *t*-test

Range of expected differences in means:	*2, 3, 4, 5, 6, 7, and 8 minutes*
Power value:	*0.8 or 80%*
Estimated standard deviation of the response:	*4 minutes*
Acceptable alpha risk (Type 1 error rate)	$\alpha = 0.05$ *or 5%*

Ideally, an estimate of the standard deviation for the response variable would be obtained either from the literature, from prior experiments, or from a meta-analysis. The unique nature of the experiment in this study, though, precludes such a strategy because no similar work has been done before. A pilot study could have been done, however for this experiment it was not considered feasible given the difficulty in obtaining human participants. A reasonable best estimate, therefore, based on good judgment and expert advice was considered an appropriate substitute. Correspondingly, an effect size estimate (for the response *cumulative ice-free lifeboat launch time* measured across experience groups) of 4 minutes was proposed, with an expected mean difference between groups centered on approximately 5 minutes. The null hypothesis assumed equivalency between groups and the alternative hypothesis was that the low-experience group had a lower response. A power of 80% was considered acceptable for this design and the alpha risk, or Type 1 error rate, was set to 0.05 or 5%. The results of the power analysis with these inputs are summarized in Table 19.

Table 19: Results of prospective power analysis for 2-sample *t*-test

Difference	Sample Size	Target Power	Actual Power
2	51	0.8	0.8059
3	23	0.8	0.8049
4	14	0.8	0.8241
5	9	0.8	0.8138
6	7	0.8	0.8409
7	5	0.8	0.8100
8	4	0.8	0.8015

From Table 19 a sample size between $n = 14$ and $n = 23$ would be appropriate for detecting differences in means of *cumulative ice-free lifeboat launch times* between groups of 4 and 3 minutes. This was based on the inputs listed in Table 18. Note that the sample sizes indicated in Table 19 are for each group.

The actual sample size *n* used in the experiment for each experience group was $n = 18$ (there were two experience groups – a low and a high – so there were $n = 36$ samples in total). Having selected this sample size, the power analysis method could be repeated, this time to determine the minimum detectable difference in means, or effect size, between groups given the sample size and power value used. A power curve is produced in Figure 33 that shows an intersection (represented with a dot) at 80% power corresponding to a minimum detectable effect size of approximately 3.38.

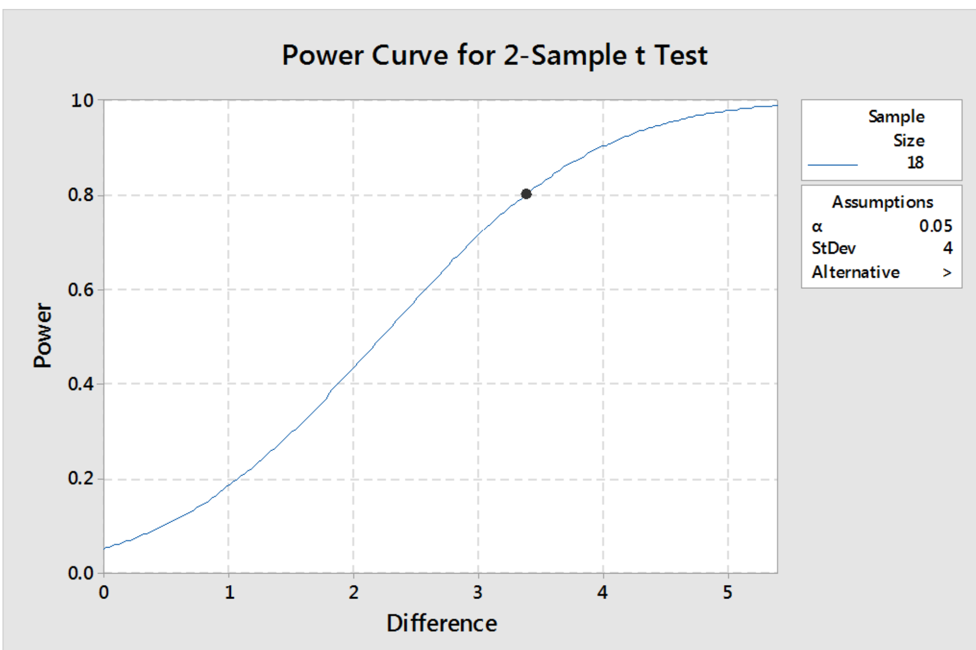

Figure 33: Prospective power curve for 2-sample *t*-test

A retrospective, or post-hoc, power analysis was computed after the experiment to check whether the estimates of standard deviation in the response and the estimate of differences between means (i.e. the signal-to-noise ratio of the response) used in the prospective power analysis were appropriate, and, by extension, whether the sizing of the experiment sufficed to obtain the desired statistical power of 80%. This time, we define the sample size (n = 18) and computed the power. The power, as shown in the power curve in Figure 34 is approximately 0.85 or 85%, which is higher than the required power of 80% and therefore assures the experimenter that enough samples were used to detect the effect size under investigation – and that resources were not wasted by using *too many* samples.

Table 20: Inputs for retrospective / post-hoc power analysis based on 2-sample *t*-test

Actual difference in means:	*4.9 minutes*
Sample size	*18*
Actual standard deviation of the response:	*5.4 minutes*
Acceptable alpha risk (Type 1 error rate)	$\alpha = 0.05$ *or 5%*

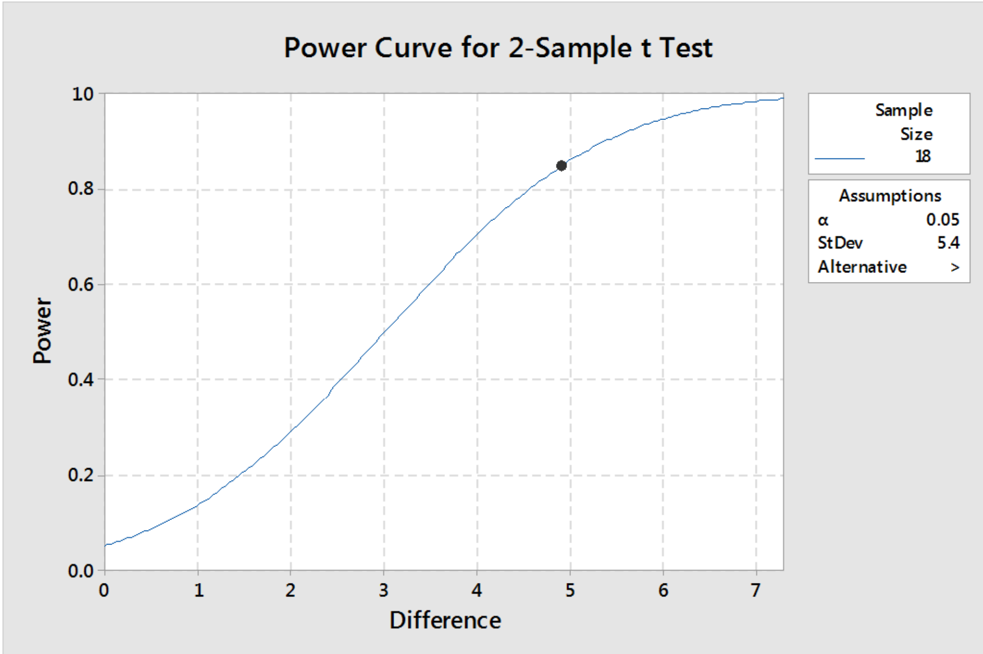

Figure 34: Retrospective / post-hoc power curve for 2-sample *t*-test

7.2 Diagnostics Plots

The following section present diagnostics plots of residual error and Box-Cox plots for power transformations for each of the separate analyses described in Sections 3.2 to 3.5, respectively (*peak clearing*, *total clearing*, *clearing-to-distance ratio*, and *ice-free lifeboat launch time* metrics). All modelling diagnostics apply to the “Emergency” ice management scenarios results analyzed and presented in the text.

7.2.1 Diagnostics Plots for Peak Clearing Metric

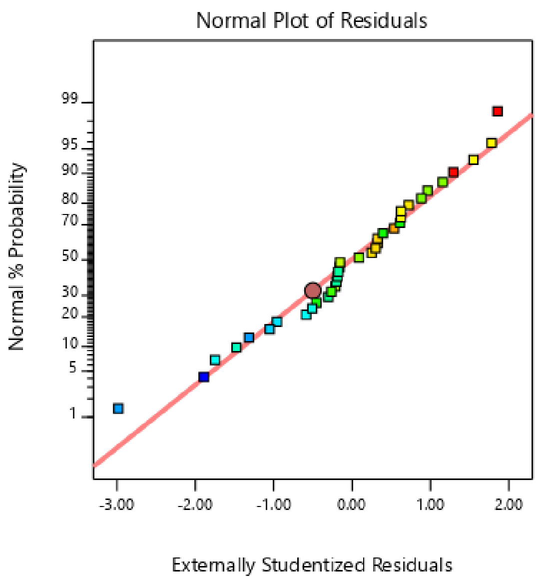

Figure 35: Normal plot of residuals (peak clearing)

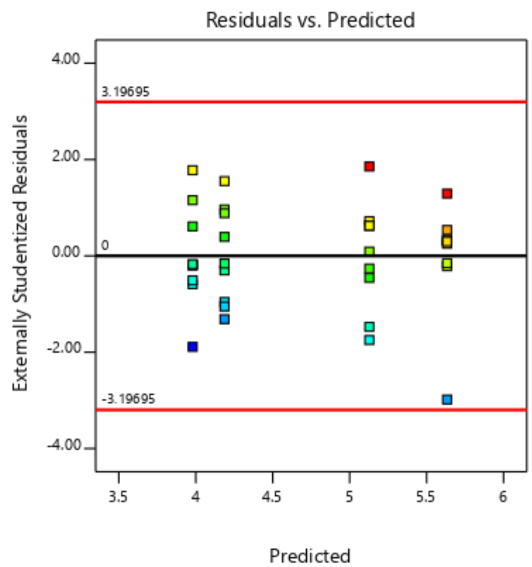

Figure 36: Residuals versus predicted values (peak clearing)

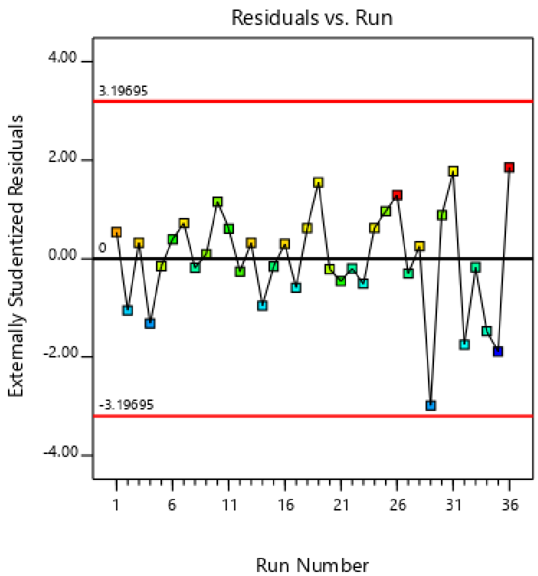

Figure 37: Residuals versus run order (peak clearing)

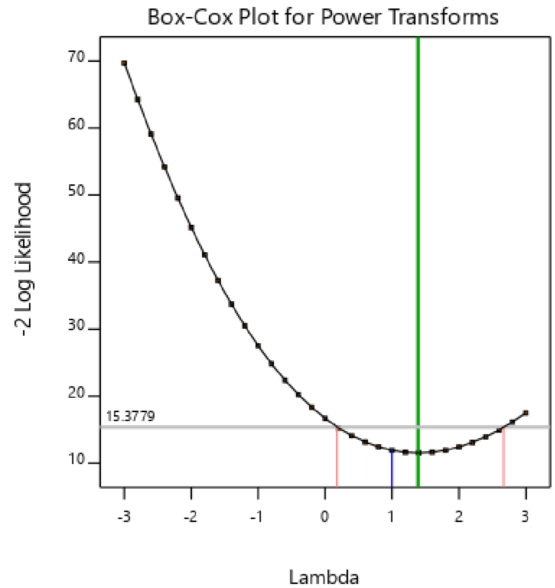

Figure 38: Box-Cox plot for power transform (peak clearing)

7.2.2 Diagnostics Plots for Total Clearing Metric

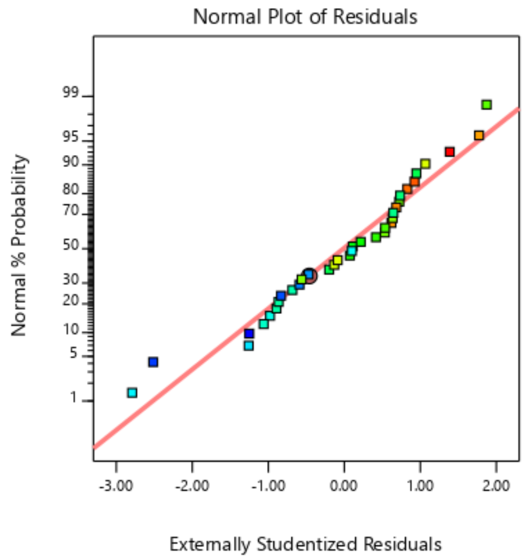

Figure 39: Normal plot of residuals (total clearing)

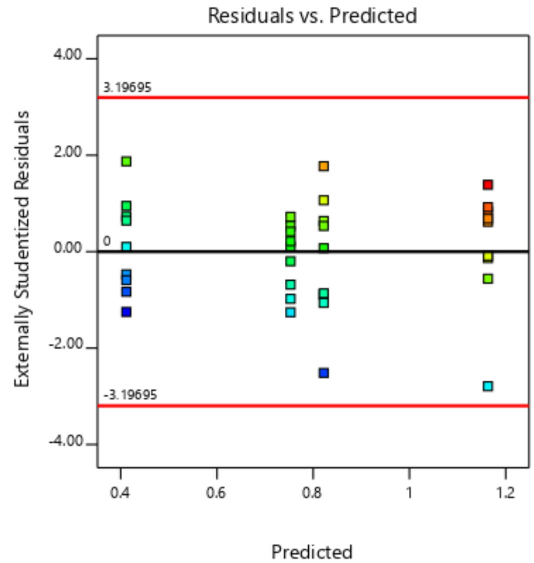

Figure 40: Residuals versus predicted values (total clearing)

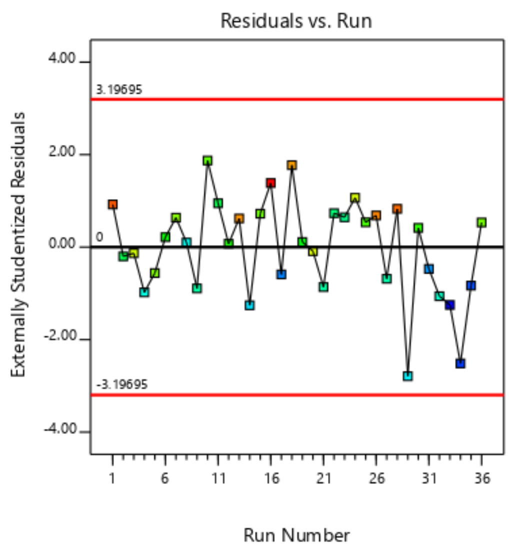

Figure 41: Residuals versus run order (total clearing)

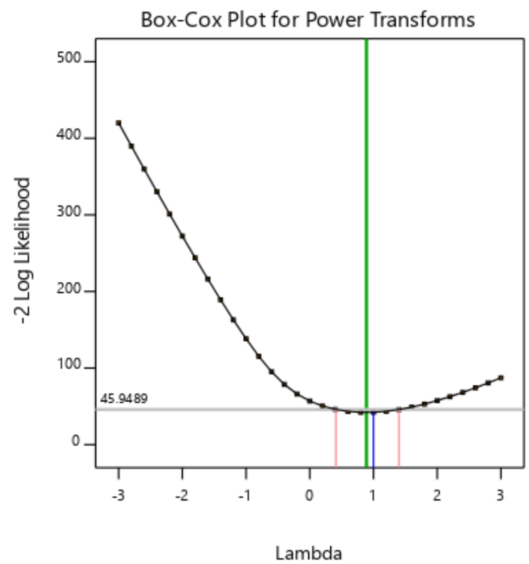

Figure 42: Box-Cox plot for power transform (total clearing)

7.2.3 Diagnostic Plots for Clearing-to-Distance Ratio Metric

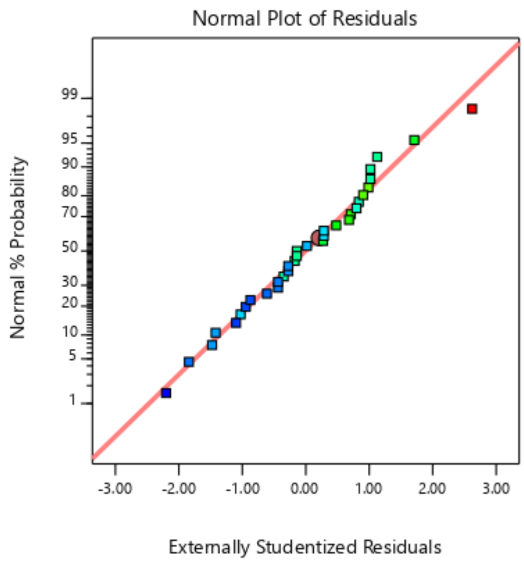

Figure 43: Normal plot of residuals (clearing-to-distance ratio)

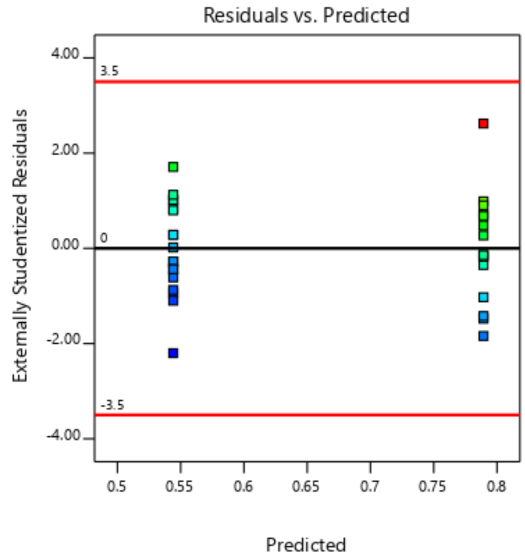

Figure 44: Residuals versus predicted values (clearing-to-distance ratio)

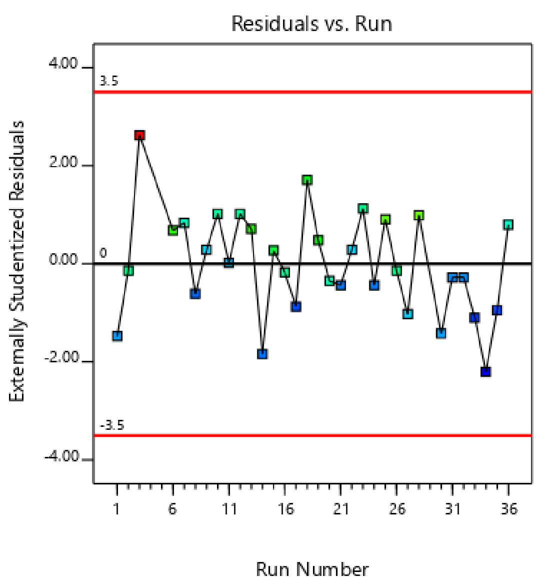

Figure 45: Residuals versus run order (clearing-to-distance ratio)

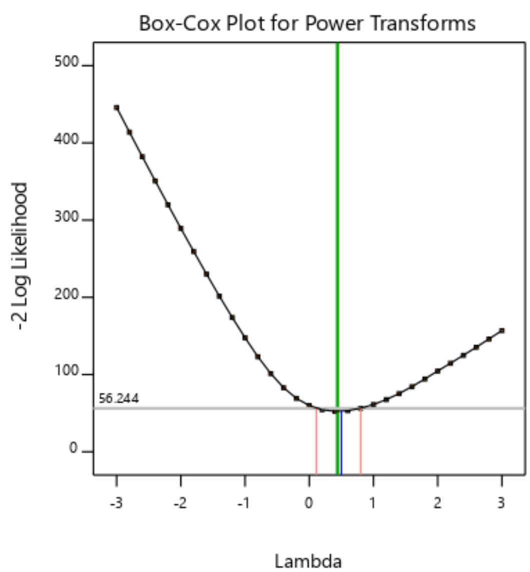

Figure 46: Box-Cox plot for power transform (clearing-to-distance ratio)

7.2.4 Diagnostic Plots for Ice-Free Lifeboat Launch Time Metric

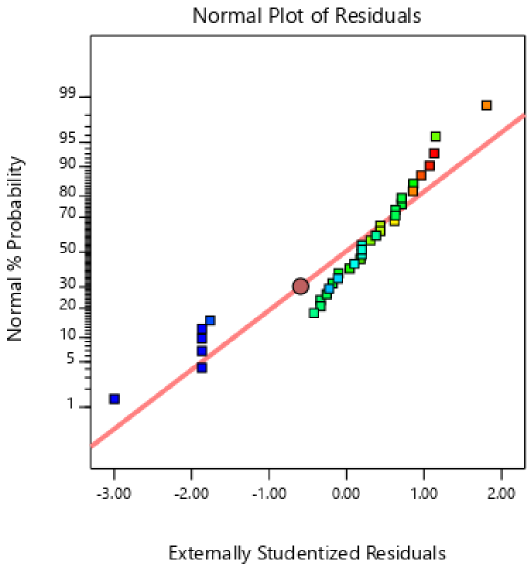

Figure 47: Normal plot of residuals (ice-free lifeboat launch time)

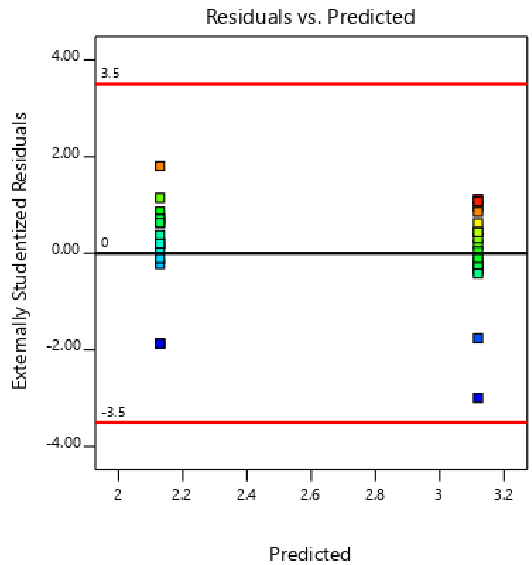

Figure 48: Residuals versus predicted values (ice-free lifeboat launch time)

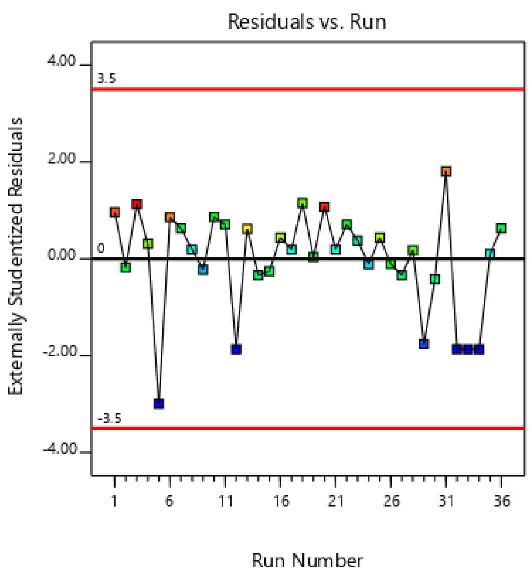

Figure 49: Residuals versus run order (ice-free lifeboat launch time)

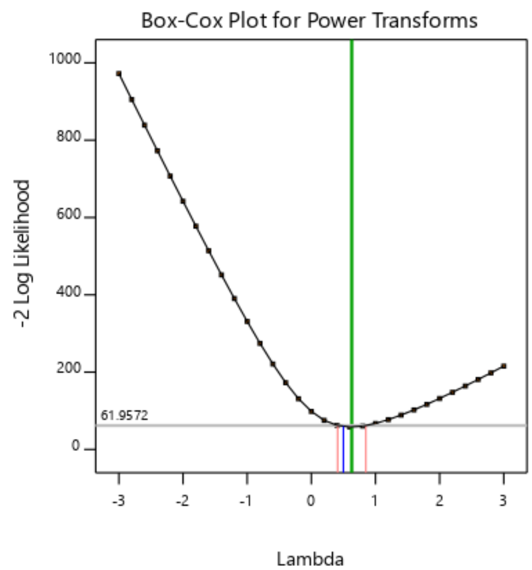

Figure 50: Box-Cox plot for power transform (ice-free lifeboat launch time)