

DESIGN OF A LARGE SCALE EXPERIMENT FOR MEASURING EFFECTS OF FISHING ON THE GREAT BARRIER REEF

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SUMMARY RECOMMENDATIONS

1. A partnership of institutions, under the general coordination of GBRMPA, should conduct a long term (10+yr) experimental program to evaluate the effects of line and interreef trawl fishing on reef and interreef communities. The annual cost of this program may be as high as \$2.1 million/yr or as low as \$0.71 million/yr depending on how varied and reliable a field program is considered worthwhile.
2. The experimental program should involve a minimum of 8 "clusters" of reefs distributed through the Cairns, Central, and Mackay/Capricorn Sections of the GBR, with each cluster containing at least 5 reefs.
3. The 8 clusters of reefs should be chosen so that 4 clusters have nearby or interreef trawling, while the other 4 clusters should be in areas without such trawling; in the event that at least three trawled clusters cannot be found or created by opening closed areas, then the experiment would have to be reduced to consider only line fishing effects, on 4-6 clusters.

4. As necessary, cooperation should be sought with trawlers to trawl in interreef areas within the clusters open to trawling. Hopefully such clusters will be attractive to fishing so that a reasonable fishing effort will be applied; in the event that they are not, charter arrangements should be made to insure that a reasonable level of fishing effort is exerted in designated open clusters. If possible, interreef areas within the experimental clusters should be opened to trawling using permits rather than rezoning, and should be opened for a relatively short time each year (weeks or months) and at a time when they will be most attractive to trawl fishermen. This will allow better monitoring of the experimental trawl effort and catch.
5. Five reefs within each cluster should be "treated" with a contrasting set of line fishing regimes, with the fishing effects supplemented as necessary by deliberate experimental fishing so as to maintain annual exploitation rates of at least 60% on line fishing target populations of the reefs open to fishing.
6. The five-reef treatment pattern for each reef cluster should include (1) one reef that has been closed to fishing as long as possible and remains closed for the duration of the experiment; (2) two reefs that are fished intensively for the first five years of the experiment; (3) two reefs that are closed at the start of the experiment. After evaluating results from the first 5 years, it will likely be decided to close one reef that has fished intensively for the first 5 years of the experiment, and to open one or both of the closed reefs to line fishing.
7. Where practical, clusters should contain additional replicate reefs for the line treatment regimes, to provide better measures of within-cluster variation in response to treatment
8. Every experimental reef, and selected interreef sites, should be monitored annually using a relatively simple (and easily repeated given expected changes in personnel and available funding) sampling protocol with (1) visual surveys for recruitment of index fish species; (2) visual surveys for abundance of larger fish and ecosystem indicators such as crown of thorns starfish and coral cover; (3) fish trapping for larger species; and (4) trawling and/or trapping for interreef fish.
9. The on-reef monitoring program should be accompanied by increased aerial surveillance to enforce closures and monitor line fishing effort, and by a port-based catch survey to estimate line fishing catch rates.
10. Recreational, commercial line, and trawl fishermen should be involved in field aspects of the program whenever possible, including "fish-in" tagging and removal experiments, diving surveys, collection of by-catch samples, and intensive monitoring of changes in fish abundance at times when closed reefs are opened to fishing.

11. Researchers should be encouraged to utilize the contrasting situations and logistical support opportunities created by the experimental program to conduct focussed studies on ecological processes and hypotheses about fishing impact that are not addressed with the basic monitoring program.
12. The experimental program should be preceded by a two-year pilot study. On reef clusters already designated in the Cairns Section, the pilot study should aim to (1) evaluate and compare field survey procedures (eg, traps versus visual surveys), (2) work out logistical arrangements for minimizing the field cost associated with monitoring each cluster, (3) test procedures (cooperative line fishing, trapping) for insuring high exploitation rates on those experimental reefs open to fishing as well as adequate sample sizes for tagging studies of interreef movement, and (4) estimate dispersal rates of larger fish among reefs by means of tagging (discovery that dispersal rates are high would mean that the overall program needs to be redesigned, with line fishing experimental regimes applied at larger spatial scales than single reefs). In the Central Section, pilot studies should focus on background information needed to identify the best possible trawl/untrawl cluster locations, and should include (1) descriptive surveys of interreef habitat structure (2) general patterns of use of interreef habitat by fish, and (3) interreef fish tagging. The total cost of this pilot study will range from \$0.31 million to \$0.938 million depending on what field programs are considered worthwhile for the long term study.
13. Field costs and logistical difficulties will be minimized if the pilot and annual experimental monitoring programs are concentrated in a single season, with as much of the work as possible being done from a single large platform (ship or barge) that is based for about 10 days at each cluster.
14. A key administrative component of the program should be an inter-institutional scientific council, charged with allocation of specific research projects among participating institutions/scientists, reviewing and recommending any changes in the program that are necessary to meet changed circumstances, and arbitration of disputes arising over precedence and authorship for publication of scientific results that involve synthesis of data across projects.
15. The proposed program shares a number of elements with research proposed for the Far Northern Section by CSIRO (comparison of trawled and untrawled interreef areas, creation of experimental trawl impacts through opening areas, assessment of interreef fish communities) but does not contain a research element concerned with the fate of trawl bycatch. Every effort should be made to integrate the two programs so as to make more efficient use of funds for experimental trawling and trawl effects measurement.

INTRODUCTION

There is considerable concern about the effects of fishing on the GBR. Two types of fishing may have substantial effects over large areas: (1) "line" fishing (recreational and commercial) for larger species such as coral trout, and (2) commercial "trawl" fishing for prawns and scallops in interreef areas and in the GBR lagoon inshore of the midshelf reef complex. Besides directly affecting the abundance of target species, line fishing may have a variety of indirect effects by altering the trophic structure (predator-prey interactions, competition) of reef communities. Trawling may affect benthic communities used by reef species for functions such as feeding, dispersal, and juvenile rearing. There may be important "interaction effects" between line and trawl fishing, particularly if trawling affects dispersal of fish among reefs and hence the immigration component of recruitment to reef populations subject to line fishing (ie, line fishing effects may be larger in areas where trawling is present, due to reduced replenishment of heavily fished reef populations by dispersal from areas where less fishing occurs). Consequently, management agencies should be concerned with equal emphasis on both line and trawl fisheries and their interactions, on both reef and interreef areas and their interdependencies, and on both direct and indirect effects of fishing.

We believe that the best strategy for estimating effects of fishing on the GBR will be to conduct a large scale field experiment, involving direct comparison of reefs subject to different fishing regimes. Alternatives to such an experiment are (1) correlative studies of reef communities where historical patterns of fishing distribution have already produced differences in impact; or (2) exhaustive "process" research on the myriad of fishing and ecological processes through which effects might develop. The second of these alternative approaches can be rejected out of hand for the GBR; the system is simply too complex and there are too many ways that any hypothesized effect measured through localized process studies could be counteracted or exaggerated through other processes operating in the field. The first alternative is unlikely to work either, since the distribution of fishing impacts among reefs is anything but "accidental" or random: there are strong gradients (north-south, onshore-offshore) in fishing intensity, and there are almost certain to be major differences among reefs due to other processes (besides fishing) that vary along these same gradients and are thus confounded with the fishing effects.

This report evaluates alternative experimental designs and monitoring strategies for a large scale effects-of-fishing experiment on the GBR, and recommends what we consider to be the best design options given some known ecological, institutional and financial constraints. We begin with the following broad assumptions about the scope and conduct of the experiment: (1) the experiment will be conducted by a partnership of research and management institutions, each contributing specialized skills; (2) the experiment will continue for at least 10 yrs, with each member of a predefined set of about 30 experimental reefs being monitored for key response indices in every year; (3) line fishing experimental "treatments" will involve closing and/or opening individual reefs

(distinguishable map units at 2-10 km spatial scales) to line fishing and supplementing "natural" fishing effort levels by deliberate depletion fishing where necessary, whereas trawl fishing treatments will involve closing and/or opening "clusters" of reefs with associated interreef areas; (4) the experimental results should be broadly applicable to the GBR at least from the Cairns section southward, ie not to just a local region such as the Cairns or Southern Section; and (5) the experiment should produce not only estimates of average long term differences between fished and protected reefs, but also estimates of how rapidly reefs change when protection is provided or removed (ie, there is a concern with the dynamics of response to fishing changes).

The difference in spatial scale between line and trawl treatment opportunities implies that the most efficient experimental design will be of the general type called "split-plot" designs, where the "plots" are clusters of reefs subject to the same trawling regime and these plots are split into individual reefs within clusters. We take it as a given that there would be insurmountable political problems in any case with trying to close and open experimental units larger than individual reefs to line fishing, since closing larger units would create substantial disparities in recreational fishing opportunities among Queensland coastal communities. We restrict the analysis to designs of the split-plot type where no more than one or two reefs might need to be closed to fishing in the proximity of any community.

In designing and evaluating the experiment we considered that the specific aims of the experiment would be:

- (1) To determine the effect of line fishing on the reefs of the GBR, of trawl fishing in the GBR interreef areas, and the interaction effects of line and trawl fishing on the abundance of index species (such as coral trout) that are directly impacted by fishing (including both fish and invertebrates).
- (2) To describe the effect of line fishing, trawl fishing, and the interaction of line and trawl fishing on the abundances of index species that are not directly impacted by fishing but may be affected indirectly through ecological processes such as predation and competition.
- (3) To determine the dynamics of recovery of index reef populations when reefs are closed to fishing, and to determine the effects of interreef trawling on the time dependence of these dynamics.

We recognize that these objectives are complementary to, but also overlap, the aims of an effects of fishing experiment that has been proposed by CSIRO. That proposal deals only with trawl fishing effects, in the Far Northern Section of the GBR. It also has the objective of comparing trawled and untrawled areas in terms of fish and benthic communities, and would utilize deliberate manipulations of trawl fishing to measure direct effects of trawling. However, it aims to deal with a broader range of concerns about trawling effects, including effects of lagoonal trawling inshore of the main reef complex and fate of trawl bycatch. This report does not deal with the issue of how the two

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programs might be integrated to provide a more cost-effective assessment of trawl fishing effects throughout the GBR.

To assist in evaluation of design alternatives, we have developed a PC based program called REEF. We describe this program in Appendix A, in hopes that it will be of continuing use in the analysis of design options as further information becomes available through pilot field studies to assess sampling variability and other aspects of the experiment. REEF can simulate reef population responses to altered harvest regimes, carry out some of the statistical analyses that might be applied to the experimental results, and generate simulated data files for analysis by standard statistical packages such as SYSTAT.

POSSIBLE EFFECTS TO BE MEASURED BY THE EXPERIMENT

Many responses are possible, throughout the Barrier Reef ecosystem, over several temporal and spatial scales. We roughly classify these effects into three groups:

(1) Direct and immediate impacts of fishing on abundance of target and incidental (bycatch) species, and on habitat features required by these species. These impacts are expected to become evident on time scales ranging from days (for depletion impact following openings to fishing) to a few years (for rebuilding of population age structures following closures).

(2) Secondary effects of abundance reduction on regulatory processes directly related to or occurring as a consequence of abundance: changes in survival rate and abundance of prey and competitor species, and changes in recruitment rates of the target species (recruitment overfishing). Most of these effects are expected to become evident on time scales of 3-5 yrs, though some highly nonlinear responses such as release of crown of thorns outbreaks due to reduced predation may require as much as 10 yr to first become evident. Clearly the experiment will not be capable of detecting responses with such long delays.

(3) Tertiary responses in community trophic structure: changed abundances of prey species as a result of changes in the abundance of predators, effects of COT release on coral community structure, release of other species such as urchins, etc. We can envision an almost endless variety of such responses. A key decision in the field monitoring design will be whether or how to conduct "synoptic" monitoring to detect the broad effects of various possibilities.

CRITERIA FOR COMPARING DESIGN OPTIONS

We consider that an acceptable design should meet at least two basic criteria: (1) when time-aggregated experimental results are analyzed by classical statistical procedures (repeated measures ANOVA, MANOVA), the power of standard tests for direct effects (line, trawl, line x trawl) should be at least 0.9 given that these effects involve abundance changes of at least 50% relative to unfished situations and that a 5% significance level is used in tests for presence of effects; and (2) when the temporal data are fitted to realistic nonlinear dynamical models of population responses, model parameter estimates representing direct fishing effects should have no more than a 10% chance of falling inside the 95% confidence limits for zero effect, when the parameter values are such as to produce at least 50% reductions in abundance due to fishing.

These minimum standards eliminate some design alternatives. We then compare surviving alternatives in terms of the power of ANOVA tests and the variances of parameter estimates for nonlinear models. For alternatives that would be good for estimating one type of effect (eg line fishing) but poor at the other (eg trawling), we simply note the tradeoff without judging which type is more important for GBR policy development.

It should be noted here that simple statistical tests for presence of effects (power of test to distinguish from null hypothesis of no effect, etc) really should not be used at all in comparing design alternatives. We should be using decision-theoretic criteria that measure the risks or costs of management decisions that might be made on the basis of the experimental results. However, we do not know how to specify a utility function for GBR management, and specification of some arbitrary function would be more difficult to defend than use of simple scientific criteria for design comparison.

A variety of statistical models might be used to analyze the results of the experiment. We have examined estimation performance and power of hypothesis tests for three models of increasing complexity and progressively less defensible assumptions about the use of the time series data from each experimental reef. First, the safest analysis is assumed to be by using a simple MANOVA model for nested or split plot designs, where the average responses from the first and second halves of the time series are taken to be two (correlated or structurally related) multivariate observations on each reef.

Second, a riskier approach is to assume a general linear model (Appendix B) of the form $y(i, j, t) = C(j) + F(i, t) + C(j, t) + w(i, t)$ where $y(i, j, t)$ is an abundance measurement for reef i in year t , $C(j)$ is an "intrinsic" average response for reefs in the j th cluster containing reef i , $F(i, t)$ is a time varying fishing effect on reef i in year t that depends on how reef i is treated, $C(j, t)$ is a time varying cluster effect, and $w(i, t)$ is an autocorrelated random effect that we assume can be adequately modelled as $w(i, t) = v(t) + rw(i, t-1)$ where the $v(t)$ are independent random effects and r is the first-order autocorrelation between $w(i, t)$ and $w(i, t-1)$; under this model, a set of "independent" linear model observations given an assumed r are the differences $y(i, t) - ry(i, t-1)$. The advantage of this model over the simple ANOVA is to

produce estimates of time-varying fishing effects $F(i,t)$ for at least some times t , but at the risk of requiring stronger assumptions about the error structure of the time series data.

Third, we assume that the data may be analyzed by fitting them to general population dynamics models of the form $N(t)=f(B,N(t-1), F(t))$, $y(t)=h(B,N(t))$, where $f(\cdot)$ is a nonlinear population model with parameters B and fishing policies $F(t)$, $N(t)$ is a vector of experimental reef population sizes, and $y(t)$ is a set of observations assumed to be related to $N(t)$ through the function $h(\cdot)$. In this third approach the covariance matrix of the parameters B is assumed to be approximately equal to $(J'V^*J)^{-1}$ where J is the matrix of partial derivatives of the predictions $y(t)$ with respect to B and V^* is the inverse of the covariance matrix of the observations $y(t)$; we assume that nonmodelled processes will result in significant lag-1 autocorrelations and cross correlations among reefs, implying that V^* is tridiagonal with elements that are functions of assumed sampling variation and auto- and cross-correlation coefficients of unmodelled "process" errors. The advantages of this approach are to (1) make joint use of different types of observations (larval, juvenile, adult abundances, catches, etc) in the estimation; and (2) provide estimates of parameters that are directly meaningful in terms of population processes (recruitment, survival, dispersal, etc) and that are directly usable in predicting the outcome of applying a management policy to a system following such processes.

FACTORS THAT COULD CAUSE A DESIGN TO FAIL

There are at least eight basic reasons why a large-scale experiment on the GBR might fail to show significant effects of fishing, given that such effects are actually present, or poorly estimate the magnitude of the effects:

(1) Confounding of treatment effects with other causes of variation, because of inadequate replication in the design. We expect strong geographic gradients in fish abundance and response to fishing at various spatial scales, especially north-south and onshore-offshore. If only one reef cluster were used for each trawl treatment, it would be impossible to say whether this cluster differed from others due to trawling or to its location; at smaller scales, the same problem applies to individual reefs treated with different line fishing regimes.

(2) High variance among replicate clusters or confounding of cluster and treatment effects due to the strategy for choosing cluster locations along the north-south axis of the GBR. Since it will be impractical to use a large number of reef clusters in the experiment, the question arises whether to (1) choose the few replicate clusters for each trawl treatment at random from all possible clusters in the GBR, (2) systematically spread (intersperse) these replicates along the main north-south axis of the GBR, (3) try to "pair" the clusters into trawled/untrawled pairs spread along the north-south axis of the GBR; or (4) "cluster" the clusters near the north and south extremes of the

system, so as to improve replication within each of these extreme geographic contexts. Any systematic choice aimed at assuring representative results along the whole north-south gradient will increase the risk of confounding gradient and trawl effects.

(3) High movement rates of fish among reefs. If dispersal rates of fish among reefs (both within and among clusters) are high enough, differences in abundance among treatments will be dampened or masked. As an extreme possibility, under very high movement rates it is possible to have overfishing over a large area, with movement causing nearly the same abundance decline for each reef in the area, even if the fishing mortality is actually occurring on only a few of these reefs. In simpler terms, high movement rates would mean that an individual reef is not the appropriate experimental unit for measuring the effects of any type of fishing. It should be noted that high enough movement rates to cause the experiment to fail would also imply that the current zoning strategy (reefs as basic unit for most closures) is ineffective in creating and protecting preservation areas.

(4) Extreme intrinsic (time-independent) variation among reefs in population dynamics and/or response of key fish species to fishing treatments. Recruitment rates and relationships to spawning abundance may vary greatly among reefs due to differences in larval retention rates and proximity to external larval sources. Availability of juvenile rearing areas (lagoons, interreef areas, etc) may also vary relative to total reef size. The combination of larval and juvenile survival variation could easily produce order of magnitude variation in average adult fish abundance among reefs subject to the same fishing treatment, thus swamping any differences due to treatment. To hedge against this eventuality, it is important to replicate line treatments within clusters; if extreme variability arises at scales larger than the clusters, then replicated comparisons within clusters will at least give some information about the best policy to use on a cluster-specific basis.

(5) Extreme autocorrelation in deviations of each reef from its long term average abundance, due to biological mechanisms that cause unusual disturbances to have a persistent effect. For example, an unusually high larval settlement can result in a perturbation to abundance that can persist for as long as the species' lifespan. For relatively long-lived species such as coral trout, unusual recruitment events can cause population changes that persist for at least 5-10 yrs, while creating abundance trends over this period that are difficult to interpret or separate from transient effects of treatment.

(6) Highly correlated variation in recruitment rates among reefs within clusters. There is evidence (Williams, Doherty) that "pulses" of recruitment can occur over spatial scales as large or larger than reef clusters; furthermore these pulses may have a complex interannual "structure" (runs of good years followed by runs of bad years). The resulting cross- and auto-correlation in deviations from average abundance could make it impossible to distinguish transient effects of fishing, or even the average effect of fishing over time.

(7) Inadequate survey design. If funding for field monitoring is spread over too many distinctive types of measurements or reefs, sampling variation for each type of response measurement may be so high as to mask all other effects.

(8) Confounding of effects due to intrinsic differences between study reefs and/or reef clusters. In particular, there may be intrinsic differences between trawled and untrawled interreef areas, and between reefs open to line fishing versus reefs that have been closed for long periods. Interreef areas now closed to trawling may be fundamentally different from areas now trawled or that could be opened to trawling, in terms of bottom habitat types and utilization by reef fishes for functions including dispersal and juvenile rearing. In this event the experiment can show only that trawled and untrawled areas are different in terms of reef and interreef fish communities, but not that the difference is due solely to trawling. Basic differences are possible between reefs available for use as the long-term closed areas, versus other nearby reefs that form each treatment cluster; closed reefs tend to be further offshore than reefs that are heavily fished, and may have lower abundance of some species due to offshore position (hence making effects of fishing appear smaller than would be seen if comparisons were based solely on fished versus unfished reefs near the center of the midshelf area).

In the following sections we suggest ways to deal with these concerns. For items (1), (2), (4), (5), (6), and (7), the basic scientific answer is to provide adequate replication at the various spatial scales of measurement and treatment, and we use the REEF program to estimate just how extensive that replication needs to be. Item (3) is a basic unknown that should be a central focus of the pilot study that should precede the program; as a working target in the design development, we have sought experimental designs that can cope with among-reef dispersal rates of up to 25% of adult fish per year. The concerns about trawl confounding in item (8) can be partly dealt with through habitat mapping in the pilot study. Concerns about closed reef positioning can be partly addressed through careful choice of reefs for closure at the start of the experiment.

DESIGN OPTIONS AND TRADEOFFS

We have examined a variety of general options in terms of the duration of the experiment, choice of treatment regimes, variables to be monitored, auxiliary experiments to be conducted, and tradeoffs between number of clusters and reefs per cluster. This section summarizes our general conclusions about the various options.

Short Term Versus Long Term Experiment

It is not practical for institutions participating in the program to make financial and manpower commitments to an experiment that will detect all possible effects, on all time scales. Some things will happen too quickly to measure, and other things will happen too slowly. Population dynamics responses for key index species such as coral trout and COT are likely to require at least 5 years to become evident. An experiment of shorter than 5 yrs duration might be adequate to measure "average" differences between fished and unfished areas, but would not provide information on rates of decline/recovery under zoning changes.

It is doubtful that an experiment of less than 5 yrs duration, involving no measurement of transient responses under policy change, would provide much information that is not already available (or easily obtained) from comparisons of areas presently subject to different zoning. Such comparisons might be included in the pilot study phase of the experiment, particularly as a way of helping to decide whether a longer-term program should involve deliberate exaggeration of fishing effects by controlled fishing in the "open" treatment reefs or open interreef areas. But they cannot be viewed as an adequate substitute for longer term response measurements.

It makes good sense in terms of planning periods for zoning and research funding to think of the experiment as consisting of an adaptive sequence of 5-year treatment periods or rotation blocks. Near the end of each period, a major synthesis conference or workshop would provide a key milestone for (1) making sure that research results to date are fully analyzed and reported; (2) obtaining external reviews and critiques of the program; (3) deciding whether or not to continue the program based on results to date; and (4) revising experimental treatment schedules, field programs, and sharing arrangements for research funding.

One attractive option is to view the experiment as consisting of three broad phases in terms of research funding and scientific commitment: (1) a 2-yr pilot study mainly aimed at testing and refining sampling methods; (2) a 5-yr main study with intensive monitoring, that will likely be extended for an additional 5 yrs; and (3) a 10+ yr long term monitoring program, where only a few key variables are monitored on a subset of reefs and interreef areas that have remained open or closed during the study.

Choice of Line Treatment Regimes in the First 5-yr Period

There should be changes in line treatment (line fishing policy) for at least some reefs at the start of each 5-yr experimental period, including the first, so at least some reefs will be undergoing transient recoveries (or declines) during the period. After examining a variety of options, we conclude that the most informative combination of reef (line fishing) treatments during the first 5-yr period will be to have (1) reefs that have been closed for as long as possible before the experiment and remain closed for the period; (2) reefs that have been open and remain open for the period; and (3) reefs that have been open and are closed to fishing at the start of the period.

The fourth possibility would be to include reefs that have been closed prior to the experiment and are opened at the start of the experiment. We do not believe that the closed-to-open treatment regime should be included in the first period because (1) few such reefs are available in the GBR, and these reefs are most valuable as places to measure the long term effect of closure to fishing; and (2) the initial transient responses to opening a reef to fishing (high fishing pressure, rapid depletion of larger fishes) are obvious and are already fairly well understood.

Wherever possible, it would be wise to provide nearby (within-cluster) replicates of the three initial treatment regimes. Dr. A. Underwood has pointed out to us that these replicates would serve two quite distinct but very important purposes: (1) to measure variation among reefs within clusters, ie to measure whether responses to management are predictable within small regions but different on larger geographic scales (such that management measures might need to be highly localized); and (2) to provide flexibility in the choice of treatment regimes to be followed in the second 5-yr period of the experiment. The second of these arguments is particularly compelling; for example, having several reefs that have been fished heavily (and monitored closely) for five years would create the opportunity to close some (and replicate the recovery response at a different starting time) while leaving others open, and having several closed reefs would create the opportunity to measure depletion responses on some while leaving others closed.

Choice of Trawl Treatment Regimes

The trawl component of the experiment could focus on either lagoonal trawling inshore of the main reef complex, or on interreef trawling. Most of the midshelf area is now closed to interreef trawling, so there is the opportunity to manipulate at least this type of trawling through openings. Examination of the present distribution of trawl effort showed that lagoonal trawling now occurs inshore of most reefs, excepting two cross-shelf closures (Far North Section and Whitsundays). Thus manipulation of lagoonal trawling would have to involve further cross-shelf closures, which would create significant economic costs and hardships for the trawl industry.

We initially hoped that it would be possible to find a balanced set of clusters with and without interreef trawling. However, most of the lagoon just inside the mid-shelf reefs is now trawled, and there are only a few areas where interreef trawling has apparently been extensive. A comparison of clusters differing only in whether or not there has been trawling inshore in the adjacent lagoon area is likely to provide a much weaker test of the effects of trawling than the comparison of clusters which differ as well in the extent of interreef trawling. Also, manipulation of lagoonal trawling would not provide information useful for future management responses to requests from trawlers to extend their activities further into interreef areas. Therefore, we feel that it is important to establish and compare areas where interreef trawling has

been extensive, even if that means opening some closed areas and even chartering trawlers to work in them.

Considering the long time scales likely required for recovery of benthic community composition and physical structure following cessation of trawling, we do not feel that additional trawl closures in the lagoon inshore of the midshelf reefs would provide useful comparisons during the planned duration (5-10yrs) of the experiment. It would, however, be useful to establish at least two relatively small permanent closures in the lagoon area, to be used for very long term studies on benthos recovery. Hopefully such closures could be established in "fair trade" for access to interreef areas used in the experiment.

Extensive Versus Intensive Sampling

There is a basic choice in field sampling between doing a complex set of measurements on a few reefs versus doing a simpler set of measurements on a larger number of reefs. Ecological field researchers often opt for the first of these choices, on the grounds that otherwise "something important may be missed". The trouble with this approach is that ecological systems are complex enough to insure that something important will be missed, no matter how elaborate the sampling program. Therefore it is not even an issue whether the experiment will measure all effects of fishing; it certainly will not.

Thus the basic case for an extensive (large spatial scale, few measurements per location) design is: (1) even very intensive study cannot insure that key interactions and effects will not be missed; (2) managers of the GBR cannot trust that results from any few sites or reefs are representative of the whole geographic region, due to heterogeneity and geographic trends among reefs; (3) the long term nature of the experiment will require careful administration of commitment by key researchers (long term studies must not take a large percentage of any individual's research time--otherwise the program will be too risky for careers); and (4) over the long term, it is important to avoid complex sampling procedures that require highly experienced people, ie the program should not be vulnerable to problems and biases associated with turnover in scientific staff.

Auxiliary Sampling and Experimentation

While the experiment should have a core, long term monitoring program that is relatively simple and easy to repeat across many reefs, there are some key needs for short term studies to evaluate sampling procedures and check for processes and interactions that might invalidate the experiment. There will also be opportunities for "opportunistic" research projects that make use of the contrasting situations created by the experimental treatment regimes.

One set of important auxiliary studies should involve calibration of "indexing" methods (visual surveys, catch rates, trap indices) and evaluation of sample sizes. This calibration will require obtaining independent and relatively reliable estimates of absolute abundance for at least some important species like coral trout, over locations that have a range of absolute abundances and where index measurements are also taken. The absolute abundance estimates can be obtained by a combination of depletion (removal) experiments, intensive sampling of changes in abundance indices at times of experimental regime change, and mark-recapture (or sequential tagging) experiments.

Another key need is for estimation of linkage among reefs, and of off-reef residence patterns, created by dispersal of line fished species. This need can be met by short term tagging experiments. We cannot overstate the importance of these experiments. As noted above, if dispersal rates among reefs are high, it will be difficult or impossible to maintain strong contrasts in abundance among reefs. Also, recovery/depletion dynamics on individual reefs will be dominated on time scales of a few years by movement/dispersal processes, so that dynamic responses on single reefs will not be representative of larger scale responses associated with mechanisms such as recruitment overfishing (eg, depletion of larval sources over whole sets of reefs within heavily fished regions such as the Cairns Section).

Tagging studies must be conducted on both trawled and untrawled clusters. In clusters that are to be opened to trawling, the studies must be repeated before and after the trawl opening; this will provide the only measurement in the whole program of the direct and immediate impact of trawling on fish behavior and dispersal.

There should be regular sampling for the age composition of at least a few index fish species, on all the experimental reefs. Age composition data can be used to reconstruct histories of recruitment variation (complementing direct larval-juvenile surveys) and to measure changes in survival rate associated with fishing. But large-scale age composition sampling creates two needs for auxiliary experimentation: (1) validation of aging methods (tetracycline marking, tagging); and (2) tests of preparation procedures and routine laboratory handling for large numbers of samples (otoliths, etc).

The trawled/untrawled clusters should create opportunities for short term studies that need not be repeated annually. For example, it would be useful to compare of water quality measures (turbidity, nutrients) among the clusters, and to assess how seasonal variation in these measures is affected by trawling (this comparison would be a useful component of the proposed study on nutrients and runoff to be coordinated by GBRMPA).

Another short term need is for source-sink modelling of larval transport among the experimental reefs and clusters. Specificity of source-sink linkages has been indicated by crown of thorns larval transport modelling. If some of the experimental reefs with different line treatments are tightly linked, then the treatment effects may be transmitted in ways that mask or exaggerate effects in the sink reefs.

Also, the experiment will provide useful data in the long term for testing source-sink modelling predictions, and it would be best to have these predictions "up front" at the start of the program.

Tradeoff between Number of Clusters (plots) and Reefs per Cluster

Figure 1 shows some basic constraints and tradeoffs involved in the choice of how many reef clusters to use in the experiment, and how many reefs to include in each cluster. In an ideal scientific experiment, it would of course be best to have many clusters (many replicates of each trawl treatment regime) and many reefs in each cluster (many replicates of each line treatment regime, within each cluster); but since the number of reefs to be monitored is the product of the number of clusters times the number of reefs per cluster, increasing either clusters or reefs per cluster causes the total number of reefs needed to increase explosively. A minimum cluster size of two reefs is needed to make any comparison between reefs subject to different line fishing regimes. On the other hand, the minimum number of clusters must be at least two times the number of distinctive trawl fishing regimes to be evaluated, so that there are at least two (replicate) clusters for each trawl regime. If there were to be three trawl regimes (none, trawled over experiment, trawled before experiment but closed during it), then there would be at least 6 reef clusters. This implies a maximum cluster size of 5 reefs/cluster under the assumption that the experiment will deal with no more than 32 reefs in total. Even without considering the recovery-from-trawling treatment type, there must be at least four clusters each containing at least four reefs, for a minimum experiment size of 16 reefs.

There are only a few options for trading off cluster number and size assuming a balanced design is to be maintained and that the total number of reefs is to be around 30-32 (excluding some replicate reefs where there are opportunities to use them):

- 16 clusters of 2 reefs
- 10 clusters of 3 reefs
- 8 clusters of 4 reefs
- 6 clusters of 5 reefs
- 4 clusters of 8 reefs.

The option "4 clusters of 8 reefs" can be included only if there are to be just two trawl treatments (none, continue historical trawling). If fewer clusters are used, then the increased number of reefs per cluster can be used to either (1) provide replication of line treatments within clusters; or (2) increase the number of distinctive line treatment regimes applied within every cluster, eg by adding one or more "crossover" treatments where reefs are closed at first, then opened later to line fishing.

RECOMMENDED REEF CLUSTERS FOR THE EXPERIMENT

There are relatively few reef clusters in the GBR that provide the opportunity for trawl/untrawl comparisons, and also contain even the one unfished reef needed for unreplicated, paired comparisons between reefs that have been closed and open to line fishing for long times. We could identify a total of only twelve such clusters in the Cairns, Central, and Mackay/Capricorn Sections. Further, there are no clusters that have extensive interreef trawling in the Cairns Section, and it is too late in the zoning review process for that Section to create a trawled cluster in the one area (near Whartle Reef) where interreef trawling has occurred historically. Most existing and potential sites for interreef trawling are in the Central Section.

Figure 2 shows the location of a basic 8 cluster design proposal developed by scientists participating in a planning workshop (Townsville July 31-August 2 1990). This proposal has three types of reef clusters (Figure 2): (1) four clusters where there is not and has not been interreef trawling; (2) two clusters that now appear to have extensive interreef trawling; and (3) two clusters where trawlers have indicated interest in working (on maps of trawl patterns submitted to GBRMPA as part of the last Central Section zoning review process). One of the untrawled clusters (Beaver) has also been identified as trawlable.

On a cluster-by-cluster basis, the trawl history and opportunities for trawling are as follows:

Agincourt #3: this cluster was described in zoning submissions in the early 1980s as being trawlable but not commercially trawled.

Beaver: this cluster was also described in the early 1980s as being trawlable but not trawled.

Duncan: the central part of this cluster was described in zoning submissions in the mid-1980s as being trawlable but not commercially trawled, and the interreef area on the lagoonal side (between Britomart, Bramble, and Trunk Reefs) was trawled prior to GBRMPA zoning.

Bowl: there is no information on the trawlability of this cluster, but charts indicate narrow strips of trawlable area between most reefs.

Kangaroo: the interreef on the lagoonal side of the cluster is extensively trawled, and prior to GBRMPA zoning trawling extended well into the cluster toward Tiger Reef. Zoning submissions in the mid-1980s identified a large area of trawlable but untrawled bottom in the eastern part of the cluster adjacent to Tiger and Kangaroo Reefs.

Jacquelin: the central part of this cluster (between Jack, Elizabeth, Kennedy, and Cobham Reefs) was identified as being trawlable but untrawled in the mid-1980s zoning submissions. The remainder of the cluster appears untrawlable except for narrow strips between some of the reefs.

Hardy: No trawling or trawlable areas were identified in the zoning submissions, but advice from QDPI (Mike Dredge) indicates that this cluster is probably trawlable.

20-137: Prior to GBRMPA zoning, trawling extended into this cluster between Cannon, Nixon, and Packer Reefs, and also to an extent between Hunt and Box Reefs. Conditions in the remainder of the cluster are unknown.

These 8 clusters allow reasonably confident allocation of trawl treatments into three trawled and three untrawled treatments, and result in good interspersion of treatments. Allocation of the remaining two clusters (Hardy, 20-137) requires further information on their interreef areas. If neither of these two areas are trawlable, then an additional trawled cluster should be sought. A possible additional trawled cluster is around Rip Cay in the Mackay/Capricorn Section; however, use of this cluster may be undesirable for operational reasons and because use of a cluster from this Section may considerably increase variability among clusters.

The proposed "green reef" (now closed to line fishing, and to remain closed throughout experiment) for each cluster in Figure 2 is shown by name with an arrow indicating its offshore position. Note that for the untrawled cluster set, three of the four closed reefs (Agincourt #3, Bowl, and Jacquelin) are offshore reefs in terms of likely cross shelf gradients in fish abundance and species composition, while one (Beaver) is on the inner edge of the midshelf complex. For currently trawled clusters, Duncan is probably of offshore type while Kangaroo is nearer the centre of the midshelf complex. For clusters where trawling could likely be introduced, there is one closed reef on the inner edge of the midshelf (Hardy) and another near the centre of the midshelf complex (20-137). Thus there is not an ideal interspersion of unfished reefs with respect to cross shelf gradients; it would be better to have a closed reef closer to shore in at least one more trawled and one more currently trawled clusters, and to avoid using Jacquelin which is far offshore. However, we could identify no such options.

Specific reefs to receive each line treatment regime have been made public for the Cairns Section clusters, but have not yet been proposed for the Central Section clusters. Our recommendation is to concentrate the reefs to be closed at the start of the experiment near the inner edge of the midshelf complex, for at least the clusters containing Duncan and Jacquelin reefs. At the midterm (5 year) decision point, that choice would provide at least some comparison of unfished abundances for inner versus outer midshelf reefs in clusters subject to trawling, and will provide an even stronger comparison at the end of the experiment if it is decided to keep at least one of the reefs closed for the full 10 years. A risk associated with the choice would be to inflate the variance among unfished reefs relative to what would likely be seen under a strategy of randomly assigning reefs within clusters to line treatments, hence weakening ANOVA comparisons.

The selection of reef clusters in Figure 2 will almost certainly create long-shore gradients or differences among clusters in line fishing

effects, unless such effects are carefully controlled through experimental fishing. Without experimental exaggeration of line fishing, the Cairns Section clusters are likely to have larger line fishing effects than the Central Section clusters, since the more southerly clusters are less accessible to recreational fishermen. It might be necessary to add as much as 100 recreational man-days of fishing per cluster in the Central Section, beyond what has been planned in the basic budget for the program (see section below); this would involve one additional expedition per cluster beyond what has been planned, at a cost per expedition of roughly \$20,000.

RECOMMENDED COMPONENTS OF THE ANNUAL MONITORING PROGRAM ON ALL EXPERIMENTAL REEFS

The core monitoring program for each experimental reef should be capable of providing information on interannual changes in recruitment and abundance of line fishing species, reef species that may be influenced by changes in abundance of the line species, and species that are key structural features of the reef ecosystem (corals, algae, etc.). The program should also provide information on fish movements and utilization of interreef habitats, and on the macro-structure of such interreef habitats (bottom types, coral cover, etc.). Finally, fishing activities (fishing effort, catch rates) should be monitored on a routine basis, and in detail following changes in zoning (especially openings).

Considering manpower and ship time costs for access to the reefs, it appears that the best monitoring strategy will be to do most of the annual sampling for each reef cluster during one annual visit by a single large research vessel or barge. With this arrangement, several teams of field people would work in parallel to carry out various sampling programs, and could cooperate in terms of both sampling and use of smaller vessels for access to reefs within the cluster. An ideal research platform would be the barge that is being planned as a National Facility by Peter Moran and others at AIMS.

The core monitoring program for each reef/cluster should have at least the following basic elements:

- (1) Visual surveys: 20-30 transects for coral trout abundance/size distribution, COT abundance (50m scale), and Chaetodontid abundance; 5-10 larger (500m scale) transects for large aggregated fishes (Lethrinids, Lutjanids); 20-30 transects for smaller and/or juvenile fishes; 20-30 transects for benthic community structure (coral types, algae, cover, invertebrate grazers); synoptic large-area surveys (transect, grid) of interreef benthic substrate type and community structure.

- (2) Fish trapping using traps with video cameras--minimum of 20 3-hr trap sets on reef sites, and same number on off-reef (interreef) sites. All fish released live from the traps should be tagged, and a sample killed for otolith based ageing; if necessary there should be additional sets to provide removal samples for otoliths, so as to provide an age composition sample of at least 100 coral trout/reef.
- (3) Line fishing by cooperating anglers--at least 1 rod hr/ha of reef, for tagging/tag recovery/age sampling and to maintain target exploitation rates of at least 50%/yr. Lower efforts (1 rod hr/10-20ha) could safely be permitted even in closed reefs, as an inducement for cooperation.
- (4) Trawl and/or trap sampling for interreef areas, using trawls equipped with TV to provide information on habitat use by the fish sampled. Sample sizes for this project yet to be determined.
- (5) Aerial surveillance to provide fishing effort estimates--at least 3 overflights per week, using sampling program currently in place, plus 10-20 additional "random" visits per year; night visits if practical, on at least 30 nights/yr.
- (6) Boat ramp and fishing club surveys to provide catch per effort and catch species/size composition information. Clubs should be requested to record all catches, and boat ramp surveys should sample at least 20 days at a major ramp nearest each reef cluster (to provide comparison/calibration with club data).
- (7) Fish counts on artificial interreef substrates. A set of artificial reef substrates should be put in place around each reef, arranged in 3 or more transects of 5-10 substrates running at least one km off the reef. Index fish counts around these reefs might be done by TV for deeper waters, or by divers where depth permits.

For all of the survey methods outlined above, there is a need to evaluate optimum numbers of transects versus number of reefs surveyed within each cluster, using standard formulations for optimum sample sizes in nested experimental designs (eg Sokal and Rohlf, p 294). That assessment might reveal that it is better to use more replicate reefs per cluster than we have assumed in the design planning, and fewer transects/trap sets, etc. per reef. The sample sizes suggested above are based on preliminary calculations assuming (1) variance/mean ratios for transect counts or trapping of 2.0, and among-reef variance/mean ratios of 1.0; and (2) relative costs to access and work each cluster:reef:transect vary in the ratios 3:1:0.1. Under these assumptions, the optimum number of reefs per cluster is around 6 rather than the 4 that we have assumed in Monte Carlo tests of design performance (see next section).

MONTE CARLO TESTS OF PERFORMANCE FOR ALTERNATIVE EXPERIMENTAL DESIGNS: HOW LARGE MUST THE EXPERIMENT BE?

We conducted a large number of simulation trials, using the REEF experimental reefs model to generate realistic fake data for analysis by MANOVA and General Linear Model (Appendix B) statistical procedures. It quickly became apparent from these trials that intrinsic, time-independent differences among reefs (in average larval loadings, juvenile carrying capacities) would make the results of any small experiment (<16 reefs) highly suspect even if there were no sampling variation and little or no stochastic variation in recruitment rates. Gross line fishing effects should be clearly evident even with smaller experiments (6-8 reefs would give good estimates of average differences in abundance of targeted species like coral trout, between fished and unfished reefs, and transient depletion responses after line openings). But the key losses with small experiments are in ability to estimate (1) trawling and line x trawl interaction effects, and (2) transient recovery responses after closure to line fishing.

Design Screening Procedure

For larger designs involving 4-8 clusters of 4 reefs/cluster (16-32 reefs), we did a suite of Monte Carlo trials to measure robustness of the designs to uncertainty about magnitudes of effects and patterns of stochastic variation (Table 1). We also did extra tests for an 8 cluster, 40 reef design to check the effect of improved replication of line treatments. The basic procedure for each design was to set up a hypothesis about recruitment variation and fishing effects, then do 20 simulation trials (approximately 20 min. microcomputer time) and tally the number of significantly nonzero (5% alpha level) General Linear Model (Appendix B) effects detected in the "data". The tally of significant outcomes over trials is a crude measure of the power of the design. For some cases that did not give an extreme tally (0 or 20 significant outcomes), we did an additional 20 trials to confirm that the power was neither very high or very low; these tests showed that the 20-trial screening procedure does give a good first indication of power. Results from a few simulation trials were passed to the MANOVA routines in Systat, for general tests of line, trawl, and line x trawl effects; here the multivariate observation for each reef was taken to be the two mean abundances seen during the first and second 5 year phases of the experiment. In most cases we found that the Systat results were very similar to the General Linear Model results, even though the GLM uses the annual data and estimates many more time-related parameters. Here we mostly discuss the GLM results.

For each design evaluated with this screening procedure, we examined 8 hypotheses about effect sizes and variability. These hypotheses were roughly ordered from very pessimistic (low effects, high variability) to about as optimistic as we feel is justified based on available data. For the most pessimistic scenario (labelled E,L,H,H in Table 1), we assume (1) high (5-fold) variability in average larval loading among experimental reefs, with all of this variability "transmitted" into changes in population age structure (density dependence occurs prior to larval settlement); (2) low fishing mortality-33%/yr line fishing and 10%/yr juvenile mortality due directly to

trawling; (3) high off-reef residence proportions (50% of fish) and dispersal rates among reefs (25%/yr) in the absence of trawling (but no residence/dispersal in the presence of trawling); and (4) high stochastic variation in recruitment rates, such that $\log(\text{recruitment})$ has a normally distributed stochastic component with standard deviation 0.4 due to unique effects on each reef and an additional 0.4 due to effects shared among reefs within each experimental cluster. In all cases, the standard error due to survey variation was assumed to be 20% of the mean abundance. The high stochastic variation case corresponds to the worst variation seen in the Williams-Doherty recruitment surveys for Chaetodontids; the Ayling coral trout data suggest much lower recruitment variation. For the most optimistic case, we assumed that juvenile density dependence causes damping in larval variation effects so that there is only 2-fold variation among reefs in average recruitment, 60%/20% fishing mortality effects, no off reef residence or migration among reefs, and $\log(\text{recruitment})$ standard deviations of 0.2 unique to reef, 0.2 shared across cluster.

As noted in Appendix A, the REEF program simulates other sources of variation that are not represented as explicit effects in MANOVA or the General Linear Model. These include (1) 2-fold geographic "cline" in larval and juvenile carrying capacities (lower in north); (2) persistence of stochastic recruitment effects through population age structure and survival; and (3) interannual "coupling" of stochastic recruitment effects through the effect of older juveniles on post-settlement density dependence in early juvenile survival.

Reef clusters used in the Table 1 screening were identified by GBRMPA staff. In terms of general geographic locations, these were as follows:

8-cluster design: Mossman (Agincourt 3 etc), not trawled
 Innisfail (Beaver, etc), not trawled
 Hinchinbrook (Duncan, et), trawled
 Palm (Bowl, etc), not trawled
 Bowling Green (Kangaroo, etc), trawled
 Upstart (Jacquelin, etc.), not trawled
 Whitsunday (Hardy, etc), not trawled
 Mackay (20-137, Bax, etc), trawled

6-cluster design: as above but omit Hinchinbrook, Palm

4-cluster design: as 6-cluster case but also omit
 Bowling Green and Upstart clusters

Note here that we could find only two clusters that appear to already have extensive interreef trawling (Duncan, Kangaroo). Note also that there are no clusters designated for the Mackay/Capricorn section. We did a few checks with other cluster locations and trawl treatment designations, and could see no indication that the details of such choices have much effect on general predictions about power of tests (proportion of significant outcomes, etc.).

How Big Should the Experiment Be?

The results in Table 1 indicate that even the largest of the locally unreplicated designs tested (32 reefs) would not have adequate power to confidently detect the direct effect of trawling (on juvenile survival), or various line fishing effects in the absence of trawling, for the worst case scenario where fishing effects are small in the first place (33% line exploitation, 10% trawl mortality) and off-reef dispersal/migration is high enough to dampen effects of fishing (see Table lines for E,L,L,H and J,L,L,H scenarios). However, these exploitation rates are far lower than we had set as basic standards for the experiment to detect (see Section above); the apparently discouraging results in the Table simply indicate that it will be important to deliberately maintain or exaggerate at least the line fishing effects to make sure that higher exploitation rates occur on the fished reefs.

Some effects may not be measured accurately by the 32 reef design, even if exploitation rates are high (60% line, 20% trawl--see Table 1 lines E,H,H,H and J,H,H,H). There will be only about a 50%-75% chance of obtaining significantly nonzero estimates for the direct trawl effect, and for the effect of trawling on the average effect of line fishing. Also, only about 10-20% of the annual transient effects estimates for line fishing recovery/depletion response in the absence of trawling will differ significantly from zero. However, it should be noted that these transient response effects would only be observed on a few reefs (only 3 untrawled clusters, transients seen on only two reefs in each of these clusters).

Note also for the 32 reef case in Table 1 that transient recovery responses after line closures would not be significantly different from zero for most years, even if stochastic recruitment variation is low and if there is no off-reef residence/dispersal (see Table columns marked RNT, RTT). There are two reasons for this apparently discouraging result: (1) by arbitrary convention, the recovery responses are measured as differences from unfished abundance, so lack of significance can mean that abundance has recovered to not differ significantly from unfished levels; (2) there is a basic difficulty in measuring transients in the presence of high stochastic variation in recruitment, which can cause transients in abundance that are regularly as large as the response transients. This masking of effects would of course be even more severe if dispersal among reefs tends to dampen the transients. To be fair, we have used a very harsh standard in the General Linear Model estimation, by insisting that each annual effect be treated as an independent response that might be of any magnitude and sign; much higher power would be obtained for tests of patterned effects over years (linear trends, quadratic trends, etc.).

Tests of a 40 reef design, with replication of the closed-for-first-five-years-then-opened treatment, showed some improvement in the probability of detecting differences from unfished abundance during recovery after closure. This difference was minor for untrawled clusters in the worst case scenario (E,L,L,H) (where weak line fishing effects are masked, and recoveries speeded, by interreef dispersal), but was dramatic for trawled clusters and for untrawled clusters in the more optimistic scenarios.

Table 1 indicates that there is not a simple monotonic relationship between power of all tests and number of reefs included in the design. For some major effects there is an obvious decline in power from the 32 to 16 reef options, but even for these effects the 32 reef option is not consistently superior to the 24 reef option. The lack of a clear monotonic relationship occurs because additional sources of variation are added when more clusters are added, and the clusters are not selected from a large random universe of choices in the first place.

The 16 reef design is clearly not acceptable, since it cannot be relied upon to provide nonzero estimates of even the main effects. MANOVA tests using Systat reinforce this conclusion; in a 10-simulation test using optimistic assumptions (60%/20% harvest), we failed to get significant trawl effects in 8 cases and significant line x trawl interaction effects in all cases.

We did some simulation trials pretending that one additional cluster has no nearby trawling. These trials indicated that if we had been able to identify four untrawled reef clusters (instead of the 3 untrawled/5 untrawled set used in the Table 1 simulations), the Monte Carlo trials would have shown much clearer differences between the 32 and 24 reef options. This difference was particularly apparent for MANOVA tests of trawl and line x trawl effects using Systat.

The 16, 24, and 32 reef designs differ considerably in power to detect a significant direct effect of trawling, as a function of the juvenile mortality rate due to trawling. Over 20-40 Monte Carlo trials, the following proportions of trials resulted in significant (5%) direct trawl effects under the most pessimistic biological hypothesis, for the juvenile mortality rates shown:

Juvenile mortality rate	16 reef case	24 reef case	32 reef case
0.1	0.00	0.05	0.10
0.2	0.10	0.10	0.75
0.3	0.10	0.90	1.00
0.4	0.65	1.00	1.00
0.5	1.00	1.00	1.00

What these results show is that the 32 reef design should be reliable for detecting a trawl mortality rate exceeding 0.2, while the 24 reef design will reliably detect only rates exceeding 0.3 and the 16 reef design is likely to demonstrate the rate to be statistically significant only if it exceeds 0.4. Essentially identical results were obtained for the 40 reef design as for the 32 reef design. It is very unlikely that the rate is so high as 0.4 for any common reef fish species. Based on the REEF population dynamics model, a juvenile mortality rate of 0.3 per year should result in average juvenile and adult abundances about half as high on reefs in trawled areas as on reefs in untrawled areas.

On the basis of reef clusters that have been identified to date, the 8 cluster/32-40 reef designs do not appear to be substantially better than a 6 cluster/24 reef design at measuring some of the simplest line fishing effects; they differ primarily in power to find trawling effects.

Unless additional untrawled clusters can be identified, serious consideration should be given to using a 6 cluster design with an increased number of reefs in each cluster. This would allow better replication of line fishing treatments, and/or more choices about line treatment changes to use later in the experiment.

We did some Monte Carlo trials with a 6 cluster/5 reefs per cluster (30 reef) design suggested by participants in the Townsville planning workshop (July 31-August 2, 1990). These trials indicated that the design should perform about the same as a 6 cluster/24 reef design at estimating trawl effects. Its main advantage would be to provide estimates of cluster-specific differences in response to line fishing treatments.

INSTITUTIONAL ARRANGEMENTS

Various scientists from QDPI, JCU, AIMS, and CSIRO are interested in participating in the experiment. Their interests and experience are far-reaching and largely complementary. However, there are several institutional problems that will plague the program: (1) there will doubtless be competition for favored studies / projects / synthesis efforts, due to overlapping interests of some key scientists; (2) there will be considerable difficulties in holding scientific teams and expertise together over the long life of the program; (3) intellectual and administrative leadership of the program will be seen as a real public relations asset for any agency that assumes this role; (4) there will be a continuing need for cooperation with fishermen, even during times when there are no obvious benefits to them, such as opening reefs to fishing. Below we suggest some ways of dealing with these institutional problems.

An Equal Partnership of Participating Agencies

There have been various proposals about where to provide an administrative and scientific "home" for the program. To the extent that an administrative and financial home is required, the obvious agency to handle this is GBRMPA--any of the other institutions could too easily be accused of using such a position to provide favored projects to its own staff. Considering such potential conflicts of interest, we believe that it is important that there not be any designated lead institution for scientific project planning and synthesis; instead the scientific planning should involve an equal partnership of agencies.

There should of course be an inter-agency coordinating council or committee. This council should be required to establish formal internal and external review and arbitration procedures, to resolve conflicts and make selections among competing proposals as necessary.

Roles and Responsibilities of the Participating Agencies

Based on discussions with scientists from the various agencies, the following initial division of responsibilities and roles would fit the interests of existing scientific staff:

GBRMPA--Administrative and financial coordination of projects, overall data base management, aerial surveys and surveillance.

JCU--Experimental and survey design, statistical analysis advisory work, coral trout-COT surveys, fish trapping, development and operation of ageing laboratory for batch processing large numbers of samples, artificial reef (interreef) transect studies.

AIMS--Recruitment and small fish surveys, benthic community structure surveys, -COT surveys, interreef habitat and fish surveys.

QDPI--Coral trout surveys, fish trapping and tagging, line fishing cooperative projects, trawl by-catch monitoring, recreational catch per effort surveys.

CSIRO--Fish trapping methods, effects of trawling on demersal systems, evaluation of adaptive management regimes.

This list of interests contains some key overlaps that could potentially lead to difficulties, especially when it comes time to publish results of the program. The overlaps that particularly need attention early in the program are: (1) JCU/QDPI interests in coral trout surveys; (2) JCU/QDPI interests in fish trapping and tagging; (3) GBRMPA/QDPI interests in line fishing monitoring; and (4) CSIRO/QDPI interests in direct effects of trawling.

We do not recommend that problems with overlapping interests be sorted out on a geographic basis (eg, QDPI doing surveys/trapping in north, JCU in south). While this is the simplest approach from a logistic viewpoint, it would create serious difficulties in the longer term. There will be enough trouble with standardization of methods and with variability among clusters in the measurements, without compounding matters with possible observer/handling biases. Worse, who would have priority or responsibility for scientific reporting of the results? It is silly to think about publishing two papers with the same title, differing only in the geographic region where the data were gathered.

Cooperation with Recreational and Commercial Fishermen

Our statistical analyses indicate that it will likely be necessary to deliberately exaggerate and standardize effects of line fishing for at least some reefs, and this will necessitate cooperative fishing activities on an annual basis throughout the program. Likewise, there will be continuing need for assistance of fishermen in obtaining fish for tagging, recovery of tags, and sampling of trawl bycatch.

We do not envision major problems in obtaining cooperation from line fishermen for short term and pilot studies. However, it will likely be very difficult to maintain cooperative arrangements for more than a few years unless these arrangements are (1) strongly institutionalized (organized as regular and predictable annual "events", planned far in advance); and (2) accompanied with strong incentives such as opportunities to fish in closed areas, coverage of costs (food, drink, fuel, bait, etc.), prizes for fishing performance, and lottery arrangements or other direct financial incentives.

An alternative to relatively complicated organization of fishing clubs and other public participants would be to handle line fishing operations entirely through charter arrangements with commercial line fishermen and recreational charter operators. While this approach is appealing from an administrative viewpoint, it might be far more costly in the long run. We do not recommend using it in at least the initial few years of the program, except perhaps in a few of the less accessible (southern) clusters.

If it is necessary to charter trawlers as well to work in interreef areas, then the overall cost of the program will be increased substantially. A prawn trawler can sweep about 1.5 km² per night, and the full charter cost per night would be around \$3500. For most reef cluster choices, around 100-200 km² might need to be trawled at least once every 2-3 yrs; without cooperation from trawlers, this would represent an annual cost per cluster of at least \$100,000.

Interim Review and Reporting (Program Benchmarks)

All scientific data from the program should be published in report and microcomputer machine-readable format on a regular annual basis. Every year or two there should be a workshop involving all scientists in the program, plus external reviewers from universities and government agencies.

Every 3-5 years there should be a major synthesis conference, where results are reported and synthesis/critique papers are solicited from external scientific reviewers.

We cannot overemphasize the importance of making use of external reviewers, especially from the Australian academic community, for both criticism and broad synthesis of program results. Regular involvement of such people will not only strengthen the program scientifically, but will also help to avoid conflicts among project scientists over who is to have priority for general synthesis work and publication.

Documentation of Field Procedures

Over even the 5-yr time blocks between treatment rotations, there is likely to be substantial turnover of field staff. This means that it

will be necessary to make all field procedures as simple as possible, and to precisely document them. All sample locations (permanent plots, transect patterns) should be indicated on maps and aerial photos, and should be marked in the field where possible. Visual surveys should involve extra (volunteer?) personnel whenever practical, to help establish a group of experienced people such that loss of the whole group between samplings becomes very unlikely.

Documentation and training will obviously be a substantial burden for program scientists for the first few years. We see this as a necessary price to be paid for the chance to become involved in the program.

Data Base Management and Exchange

Again because of the long term nature of the program, and because of the complexity of the field monitoring program, we feel that it is essential to be ruthless about requiring regular (annual or faster) reporting of all numerical results in microcomputer machine-readable form. GBRMPA should undertake a data management "set-up" service to all program scientists at the start of the program, providing (where needed) spreadsheet interfaces for data entry, standard file reporting formats, and instruction (where needed) in data entry and file management procedures.

Research Synthesis and Publication

The most interesting and important results of the program will likely be a set of serendipitous findings that are not anticipated at all by the scientists who have planned it. Particularly for situations where such findings involve looking at data across monitoring projects (eg, comparing trapping and visual survey data), it will be important to define a set of protocols before the program begins for deciding who should author any publications that result from the findings.

COSTS OF THE PROGRAM

Here we present annual cost assessments for an 8 cluster, 40 reef design, and for several less expensive options involving reduction in design size and in the complexity of the field monitoring program. We also present cost estimates for a 2-yr pilot study focussed on the Cairns section clusters already designated for the experiment, but also including preliminary studies on trawling and interreef biota in the Central Section where the main trawl comparisons are proposed. The estimates were developed by scientists from the cooperating agencies, and our assessment is that they are as realistic as can be expected at present (no major costs omitted, no obvious frills added). In developing

the estimates, scientists worked from the following basic assumptions: (1) in so far as possible, the on-reef sampling should be done from the fewest possible chartered research vessels, during a single concentrated field season where research teams remain at each cluster for approximately 10 working days; (2) field work will be mainly carried out by a professional crew hired specifically for the program, with limited assistance by scientific staff already employed by (salaries already covered by participating agencies); (3) project specific administrative costs (administrative overheads, conference travel, etc.) will be borne by the agencies; and (4) some initial equipment development costs (traps, etc.) will be covered through a separate pilot study budget (see next section).

Annual Costs of the Full Field Program

Table 2 presents annual costs estimated for the full recommended program. Note that nearly half of the annual budget consists of vessel charter costs, and that labor costs are about evenly divided between field and laboratory (sample processing, data management, etc) work.

Table 2. Estimated annual cost for the full experimental program with 8 clusters, 40 reefs. Costs in \$1000s.

PROJECT AND COST COMPONENT	ANNUAL COST
1. Visual surveys for coral trout, COT, etc.	
Senior observers, annual pay (2x40)	80
Seasonal field crew (2x0.4x25.2)	20
Data entry clerk (1x11)	11
Equipment/supplies (dive gear, etc.)	19
Ship time (100@1.2)	120
2. Visual surveys for juvenile recruitment	
Senior observers, annual (2x40)	80
Seasonal field crew (6x100x0.12)	80
Equipment/supplies	10
Ship time (different season from 1.)	100
3. Benthic community visual surveys	
Senior observers, annual (2x35)	70
Seasonal assistant (1x100x0.12)	12
Data entry, analysis (1x100x0.36)	35
Equipment/supplies	19
4. TV-based trapping, tagging for large fish	
Field assistants (5x0.33x30)	50
Video analysis clerks (3x30)	90
Equipment (Trap construction, etc)	96
Ship time (100x1.2)	120
5. Interreef habitat and fish use surveys	
Field assistants, annual (1x40,1x30)	70
Seasonal assistant (1x10)	10
Equipment/supplies (nets, etc)	31
.....Ship time (large trawler, 52x4)	220
6. Public involvement (fishing, tagging, census)	
Angler club organization fees (8x2)	16
Census and data management clerks (2x32)	64
Equipment/supply (small boats, bait, etc)	108
Ship time (8 x 8 days x 2.5)	160
7. Fishdown assessments (tagging, census)	
Investigator, seasonal assistants	49
Equipment/supplies	18
Ship time (70x1.2)	89
8. Fish ageing facility (sample laboratory)	
.....Laboratory technicians (2x40)	80
9. Increased surveillance (fishing effort)	
Air time (100 hr @1)	100
10. Program coordination and operations	
Coordinator and assistant (2x40)	80
Travel, organization, data mgmt.	20
TOTAL ANNUAL COST	2127

Costs for the Two-Year Pilot Study

We have developed cost estimates for a 2-year pilot study with four basic objectives: (1) evaluate sampling procedures that have not been used extensively in the GBR context (trapping, long transect visual surveys, etc.); (2) provide basic information about interreef areas proposed for use in the Central Section (operability for trawling, habitat patterns, fish sampling methods); (3) provide calibration information for survey methods through opportunity for depletion experiment provided by rezoning of Wardle and Escape reefs in the Cairns Section in 1991; and (4) initiate basic monitoring programs on experimental clusters that will be created by rezoning in the Cairns Section. We emphasize that the opportunities created by Cairns Section rezoning should be utilized to fullest degree possible, even if the pilot study indicates that the overall experiment is unlikely to be successful. The pilot budget also includes one "trouble shooting" study concerning the overall experimental design, involving source-sink modelling of larval transport patterns between the experimental reefs and reef clusters; this study will help to anticipate variability in abundance among reefs and also whether there are likely to be any specific reef-reef larval linkages that might cause misleading changes related to how the source reef(s) are treated.

Table 3 provides cost estimates for research activities only. It does not include surveillance and program administration costs; we assume that these costs will be borne by GBRMPA as part of its investment in the development of the program.

Table 3. Cost assessment for a full two-year pilot study with both methods evaluation and routine monitoring objectives. Costs in \$1000s.

1. Visual surveys (trout + large transect eval.)	
Manpower (2 yr @ 15/yr)	30
Equipment (2 yr @ 3)	6
Ship time (2 yr @20)	40
2. Visual recruitment surveys	
Manpower (2 yr @ 70)	140
Equipment (2 yr @ 5)	10
Ship time (2 yr @ 30)	60
3. TV-trap tests, tagging (reef and interreef)	
Manpower (aggregate pilot and routine)	29
Equipment (trap development, etc)	17
Ship time	28
4. Public involvement tagging program	
Manpower (2 yr @ 20)	40
Equipment (small boat use, bait, etc)	48
Ship time (2 x 2yr @40)	80
5. Reef benthos survey, interreef stereoscopic photographic survey test	
Manpower	8
Equipment (cameras, etc)	16
Ship time	11
6. Wardle/Escape Reefs depletion study	
Manpower (2x49)	98
Equipment and field expenses	36
Ship time	160
7. Interreef prawn trawl test, photo survey	
Manpower	10
Equipment (nets, etc)	10
Ship time (trawler charter)	21
8. Source/sink modelling for proposed reefs	
Manpower (1 full time, 1 yr)	40
TOTAL PILOT STUDY COST, 2YR	938

Options for Reducing Costs of the Experiment

The cost assessments in Tables 2-3 are for a comprehensive program that is scientifically conservative. It calls for relatively high expenditures to insure adequate experimental replication, cross-comparison of the different methods used for measuring abundances of affected species, and broad surveys of the aquatic organisms that might be affected by fishing. Here we suggest a series of less costly options, and indicate some of the risks associated with each. There are basically two strategic directions for reducing costs: (1) reduce the number of clusters/reefs included in the experiment; and (2) reduce the variety and/or intensity of field monitoring programs. Options of the first strategic type are labelled 1a, 1b, etc. below, while options of the second type are labelled 2a, 2b, etc. Options of the first type do not reduce pilot study costs.

Option 1a: 8 cluster, 32 reef design: \$2.037 m/yr

The number of reefs to be monitored can be reduced by 8 by omitting within-cluster replication of the recovery after closure treatment. With all monitoring projects in place, the savings would amount to about \$90,000/yr, mainly in seasonal labor costs and ship time. The number of long-term employed people would not be reduced, nor would the substantial costs of public involvement and interreef sampling. There would be no savings in pilot study costs.

This option would sacrifice the flexibility and cluster level response discrimination afforded by the basic design, and would provide only modest savings. It, and similar minor reductions in design size, are not wise alternatives to the full program.

Option 1b: 6 cluster, 24 reef design: \$1.637 m/yr

Under this much reduced design, there would be substantial savings in ship time, manpower costs, and in interreef sampling programs, totalling nearly \$0.5 m/yr.

The key loss under this option would be in ability to detect effects of trawling. According to our assessments of statistical power (see above), the smaller design would be incapable of reliably detecting direct mortalities due to trawling of less than 50%/yr (far higher than expected).

Option 2a: full design, no redundant monitoring: \$1.533 m/yr

This and the other options below would involve omitting components of the sampling program, while insisting on a well-replicated design. Under option 2a, we would omit the visual survey project for fish recruitment (0.27 m/yr), combine depletion studies and the public involvement project for a savings of 0.156 m/yr, do benthic surveys only

every other year to save 0.068 m/yr, and omit extra financing for the GBRMPA aerial surveillance program (0.1 m/yr). Also, under this option the pilot study would be reduced in cost by 0.266m, to a total cost of \$672,000. The JCU fish ageing facility would assume critical importance, since routine age composition sampling would become the main procedure for measuring variability and changes in fish recruitment rates.

Along with a loss in ability to study fish recruitment variation and in accuracy of calibration of survey methods to independent estimates of absolute abundance, this option would reduce the variety of fish species monitored and hence the ability of the design to detect indirect effects of fishing on reef communities. It would also involve higher risks of having treatment effects masked through illegal fishing activities.

Option 2b: full design, omit even more monitoring: \$1.202m/yr

An extension of Option 2a would be to also omit either the interreef trawling or fish trapping projects, depending on the outcome of pilot studies to evaluate these methods, for an additional savings of about 0.33 m/yr. If TV-trapping does not work, then only trawling would be used to sample interreef areas; if trapping does work, then it would be used instead of trawling as the main means of interreef sampling.

This option involves the same losses as option 2a, along with a risk of either inadequate sampling of interreef fish populations or of inadequate abundance indexing and tagging (for interreef movement studies) of both reef and interreef fish populations. Its viability cannot be assessed until the pilot study is completed.

Option 2c: full design, minimum viable program: \$0.73 m/yr

Under this option the on-reef studies would be reduced to only visual surveys (large fish, benthos) and a small trapping program to obtain fish for ageing, and the interreef studies would involve only habitat mapping and modest fish sampling through either TV-trap/photographic surveys or TV/trawl surveys. There would be no financial support for the public involvement project, no increased surveillance, no extensive tagging for fish movement, no calibration of sampling gear by depletion experiments, and no funded program coordination. The JCU fish ageing facility would be provided for studies of recruitment variation based on age composition. In preparation for option 2c, it would still be necessary to conduct the 2-yr, multi-objective pilot study, but this study could be reduced to cost a total of \$320,000.

Option 2c would measure mainly the most crude and direct effects of fishing on abundances of targeted species, and provide some information on interreef habitat differences between trawled and untrawled areas. It would be fully viable in terms of the statistical measurement of the direct effects. It would give little information on dynamic processes (eg movement, recruitment) that might be affected by

trawling, and would provide no cross-validation of abundance indexing methods.

APPENDIX A--SIMULATION TO ASSESS DESIGN PERFORMANCE: THE REEF PROGRAM

We consider that the best a priori way to evaluate a design alternative is to generate realistic simulated data, then see how well the statistical methods that will eventually be used to analyze the real experimental results can do at recovering the (known) treatment effects present in the simulated data. If the simulated data are generated with a dynamic model that represents the main perversities that may occur in terms of variance sources among reefs and over time (and how these are propagated through the population dynamics), along with realistically high sampling errors, the result should be a quite conservative assessment (ie, the actual experimental result should be cleaner than the simulation result). Furthermore, such tests should be made not with a single dynamic model, but instead with a suite of alternative models that capture extreme (but credible) hypotheses about key population dynamics mechanisms; for example, the stock-recruitment component of the simulated population dynamics should be represented at one extreme by larval-driven recruitment (with associated high variation and linkage among reefs due to larval transport), and at another by juvenile-space-limited recruitment (with most larval variation damped out by the juvenile density dependence). We should at least be able to determine before the experiment begins whether its success will depend on which (if any) extreme hypothesis is correct, ie whether the design is robust to basic uncertainties about the population dynamics of key index species.

The REEF program, written in QuickBasic for IBM PCs (with minimum 640K memory and VGA graphics adaptor), was developed to do many of the chores associated with generation and analysis of simulated experimental responses. It contains routines to (1) simulate spatial patterns of larval dispersal over the whole GBR in order to provide (a) rough assessments of linkages among experimental reefs through dispersal and (b) estimates of how background levels of larval input from other reefs might be expected to vary among candidate reefs for the experiment; (2) allow rapid (spreadsheet type) selection of candidate reefs and treatment regimes for "gaming" situations where many interactive simulations are to be tried in order to quickly check design alternatives; (3) an experimental reefs simulation routine that can simulate a variety of hypotheses about dispersal, recruitment, survival, and harvest effects on reef populations over time, while accounting for linkages among the reefs generated by larval dispersal; (4) a routine that generates SYSTAT files from the reefs simulation results, for ANOVA/MANOVA tests of design performance; (5) a General Linear Model (GLM) routine for estimating reef, fishing treatment, and cluster-time interaction effects from experimental or simulated data; (6) a nonlinear estimation algorithm to predict variances of parameter estimates to be expected if the reefs simulation were fitted to results of the experiment; and (7) a whole-reef simulation routine to examine changes over time in an index fish population (eg coral trout) in relation to the space-time dynamics of larval dispersal, adult movement, and dynamic changes in the levels and distribution of line fishing effort through regional population growth and selection of fishing sites by anglers.

Key assumptions used in the REEF dynamic models are described in the following subsections. An important point is that we do not pretend that these models and the alternative hypotheses that they represent are "correct" or optimal descriptions of reef dynamics; they are intended only to provide general assessments of scale effects (distances of larval dispersal, linkages, etc), and realistically perverse behavior of simulated abundances.

Larval Dispersal and Linkage Among Reefs

The REEF system sets up a 10x10km grid of spatial cells over the reef from Cape Flattery to Gladstone, and from shore to the outermost reefs. This grid is oriented along shore, so that dominant water transport will be between rows of the grid (from NW to SE). The program user can define any arbitrary transport "rosette" specifying probabilities per day of a larva in any cell being transported to each of the four adjoining cells (NW, NE, SE, SW). Using short time steps, such that the total probability of movement per time step is equal to 0.5, the program then uses the rosette values to predict probabilities of movement among all cells reachable from the source cell over time. In conjunction with a user-defined larval competence period (first and last days competent), the program then accumulates total larval-days (of exposure to settlement) for cells reached from the source cell, per larva successfully dispersing from the source cell. This exposure pattern is stored as a "dispersal table" or grid centered on the source cell, and this table can then be applied to other source cells to predict cumulative larval exposure throughout the system.

Three types of outputs can be generated from this part of the REEF system: (1) a "linkage table" for a predefined experimental reef set, specifying larval days exposure generated on each experimental reef per larvae successfully dispersing from each other experimental reef; (2) a "background loading" estimate for each experimental reef, measuring the total larval days exposure at that reef per larvae successfully dispersing from all reefs that the dispersal table predicts could reach the reef; and (3) a "linkage list" for the entire GBR, specifying larvae days exposure per larvae successfully dispersing for every source-sink combination of reef pairs (or more precisely, of 10x10km grid cells containing reefs) such that the sink member of the pair is predicted to have greater than 0.01 larvae days exposure per larvae dispersing from the source member.

The experimental reefs linkage table and the background loading estimates for experimental reefs are used in the REEF simulations of dynamic changes in experimental reefs over time in response to experimental fishing regimes. The whole reef linkage list is used in simulations of population dynamics and responses of fishing effort over the GBR.

REEF contains a data filing and retrieval system that encourages users to construct and simulate a variety of dispersal hypotheses. The results of these simulations can then be saved and retrieved later for

use in the other REEF simulations, thus permitting a systematic exploration of how alternative dispersal patterns may interact with other uncertain processes to influence performance of alternative experimental designs.

The larval dispersal calculations are not integrated directly into the other REEF simulations for the simple reason that these calculations involve a large amount of computation on very short time scales. To redo them over many years for many reefs in the other simulations would be impractical (as well as unnecessary).

Components of Recruitment: Larval sources, Larval Retention, Pre- and Post-Settlement Density Dependence in Survival

The "experimental reefs" and "whole GBR" simulations in REEF use the same basic logical structure to simulate recruitment mechanisms on individual reefs. This structure involves three basic steps for each simulated year for each reef: (1) predict total larval loading onto the reef from spawning on the reef and from outside sources; (2) apply early mortality (which may be density dependent) to the larvae, to predict recruitment to the juvenile population on the reef; and (3) predict survival of juveniles over ages, and add juveniles reaching the age at maturity to the adult population of the reef as new recruits to that population.

For the experimental reefs simulation, larval loading S onto each reef i is assumed to consist of four components LL , BL , EL , and PL :

$$S(i) = rLL(i) + (1-r)[BL(i) + EL(i)] + PL(i) \quad (A1)$$

Here $LL(i)$ is a retention proportion r of the total settling larvae produced by spawning on reef i itself, $BL(i)$ is a background larval loading that is calculated from an assumed average larval production per reef over the whole GBR and from the background loading rate for reef i estimated in the REEF larval dispersal model (see above section), $EL(i)$ is a sum of larval productions from other experimental reefs multiplied by linkages to reef i calculated in the REEF larval dispersal model, and $PL(i)$ is addition of larvae due to random "pulses" or swaths of larvae of unknown source settling over blocks of cells including and surrounding reef i .

The larval production components LL , BL , and EL are calculated from adult spawning abundances using Beverton-Holt recruitment equations of the form

$$L = fN / (1 + fN/k) \quad (A2)$$

where L is net number of larvae produced and surviving to settlement, f is the product of average adult fecundity times maximum survival rate to settlement in the absence of larval competition, N is the number of spawners involved in producing L (reef i adults for LL , average per reef adult abundance for BL , and experimental reef adult abundances for terms

in the EL sum), and k is a larval "carrying capacity" representing possible density dependence of larval survival prior to settlement. Setting k large results in the assumption (or hypothesis) that larval settlement is globally (over the whole GBR) proportional to adult abundance (ie no limiting factors on recruitment at the larval stage), while setting k small results in the assumption that there are limiting factors operating somewhere in the larval stage such that the number of settling larvae is independent of the number of eggs released except when total egg production is very low.

Given total larval loading $S(i)$ onto each reef, the simulations then allow for the possibility of post-settlement density dependence in survival through a second Beverton-Holt recruitment relationship:

$$J(i,1) = sS(i) / [1 + (sS(i)+JT(i))/k'] \quad (A3)$$

where $J(i,1)$ is the number of juveniles reaching age 1 on reef i , s is a base (or maximum) survival rate from settlement to age 1, $JT(i)$ is total juveniles present on the reef, and k' is juvenile "carrying capacity". Setting k' to large values results in juvenile (and later adult) abundance being proportional to larval settlement (ie, larval variability transmitted into population structure and abundance), while setting k' to small values results in the hypothesis suggested by P.F. Sale that abundance is limited by post-settlement competition except when (or where) larval abundance is very low.

Equations A1-A3 permit simulation of a rich variety of hypotheses about mechanisms of population regulation in reef fishes. Varying the larval retention (r) parameter in A1 allows simulation of whether or not reefs are "self-seeding" (and hence have a local stock-recruitment relationship). Varying the fecundity \times survival parameter f allows simulation of varying risks of recruitment overfishing (risk high when f is low). Varying the k and k' parameters results in different scenarios for how variability in larval abundance influences later juvenile and adult abundance.

The juvenile age structure on each reef is simulated by passing the juveniles through an age-structured survival process:

$$J(j+1,i) = s(1-T(i)) J(j,i) \quad (A4)$$

where $J(j,i)$ is the number of juveniles of age j present on the reef, $J(j+1,i)$ is resulting juveniles of age $j+1$ a year later, s is annual survival rate in the absence of trawling effects, and $T(i)$ is juvenile mortality rate per year due to direct effects of trawling (capture of juveniles while they are foraging off the reef, etc.). Equation A4 is applied up to an assumed age at maturity j^* , at which the juveniles are entered into the adult population $N(i)$. To speed up calculations in the "whole GBR" simulation, the juvenile age structure is not stored explicitly for each reef, and recruitment to the adult stock is calculated instead as a proportion $1/j^*$ of the total juveniles present each year.

Natural and Fishing Mortality, and Dispersal of Larger Fish among Reefs

Year-to-year dynamics of adult abundance $N(i)$ on each reef are simulated with the balance relationship

$$\begin{aligned} N(i) &= s[1-H(i)V(i)]N(i) + m(i)[N(i)-N^*] + J(j^*,i) \\ \text{(year } t+1) & \qquad \qquad \qquad \text{(year } t) \end{aligned} \quad (A5)$$

Here the term $s[1-H(i)V(i)]N(i)$ represents surviving adults from the previous year, $m(i)[N(i)-N^*]$ represents dispersal among reefs, and $J(j^*,i)$ represents recruitment to the adult stock of juveniles reaching the age at maturity. s is annual natural survival rate, $H(i)$ is the line fishing harvest rate applied to reef i in year t , $V(i)$ is the proportion of reef i adults that are resident on the reef and hence vulnerable to line fishing in year t (see below), $m(i)$ is the proportion of fish assumed to disperse from each reef per year, and N^* is a background average abundance of adults over the GBR (source abundance for dispersal of new adults into the reef i population).

The experimental reefs simulation treats fishing mortality $H(i)$ as a fixed (planned, experimentally maintained) annual exploitation rate set as a policy choice by the REEF user. The whole-GBR simulation predicts fishing mortality from line-fishing effort on each reef, using the usual fisheries catch equation: $H(i) = 1 - \exp\{-qE(i)\}$ where $E(i)$ is fishing effort on reef i and q is catchability.

A possible mechanism that could produce line-trawl fishing interactions is represented through the $V(i)$ and $m(i)$ parameters. If trawling damages interreef habitat "patches" that act as foraging-resting sites and dispersal "stepping stones" for larger fish, then trawled areas should have (1) lower proportions of large fish utilizing interreef patches as foraging-resting sites, and hence a larger proportion V that are vulnerable to line fishing directed at reef habitats; and (2) lower dispersal rates m among reefs. In trawled areas with lower m values, local reef populations should be more variable and should recover more slowly under experimental reduction in line fishing (due to lack of immigrant fish to both dampen effects of local recruitment variation and replace fishing losses more rapidly).

Dynamic Responses of Line Fishing Effort to Spatial Distribution of Fish Abundance

The whole GBR simulation predicts fishing effort over time (for a 20 year development period) on each of the REEF 10x10km grid cells containing at least one reef, and population dynamics responses in each grid cell to this fishing effort. Effort in each cell in any simulation year, $E(i)$, is assumed to depend on three factors: (1) the total fisherman population along the GBR coastline; (2) the position of the cell (longshore, offshore) relative to sources of fishermen; and (3) relative abundance of fish on the reef.

The GBR fisherman population is represented in the simulation by relative population sizes for 8 population centres (Cooktown, Cairns, Innisfail, ..., Gladstone). These relative population sizes are increased

geometrically over time at different rates, to represent differences in regional economic development (Cairns is increased at 10%/yr, other centres at 8%/yr). Each population centre "c" is assumed to generate an annual total fishing effort ET(c) proportional to its relative size, and the program then distributes these total efforts among reefs.

Each total effort ET(c) is distributed among reefs in each simulation year by calculating an "attractiveness index" A(c,i) for each reef i, then assigning the proportion A(c,i)/[sum over i of A(c,i)] of ET(c) to reef i. This "gravity model" for the effort distribution can generate a variety of realistic effort distributions, and changes over time, depending on how the attractiveness indices are calculated. We assume that attractiveness is (1) inversely proportional to the longshore distance from center c to reef i; (2) inversely proportional to the square of the offshore distance of reef i from the coast; and (2) proportional to the square of the adult fish population N(i). That is, A(i) is calculated as

$$A(c,i) = N^2/[DS.DO] \quad (A6)$$

where N² is N(i) squared, DS is longshore distance from c to i, and DO is the square of the distance offshore of reef i. This attractiveness index will result in disproportionately high fishing effort on reefs that are inshore and that have higher than average fish abundance.

The total fishing effort predicted to occur on reef i in any simulation year is then just the sum of the population centre contributions A(c,i)ET(c)/[sum of A(c,i)]. Note here that there is a hidden or implied assumption that fishermen from every population centre have at least some information about fish abundances throughout the GBR, and that they base choices about where to fish partly on the basis of this information. However, we found that the calculated fishing effort for each centre according to the model was generally concentrated within a relatively narrow radius (200-300km) of the centre, so that the assumption of global knowledge and choice does not cause an unrealistic spreading of effort. Long distance shifts in fishing effort (and long range movement of fishermen) is generated only in model scenarios where fish abundances are grossly reduced in all coastal and northern areas, so that the only remaining good fishing opportunities are in the Swain group (farthest from population centres in both longshore and offshore directions).

Responses of Prey and Competitor Species to Reduction in Predatory Fish Density: Simple Mortality Changes versus Release Effects under Nonlinear Functional Responses

The experimental reefs simulation in REEF has a "submodel" for crown of thorns (COT) dynamics, as an index or example of ecosystem changes that might accompany changes in abundance of line fishing species. COT dynamics are the best known example of possible "pathological" effects of altering coral reef trophic structure through fishing: the basic notion represented in the simulation is that there may

exist an unstable or cyclic predator-prey association between COT and coral when COT juvenile survival is high, but fish predation on juveniles may dampen or prevent outbreak cycles when fish abundance is high.

COT is represented as an abundance index scaled in terms of relative coral damage, ie $COT(i,t)=1$ means enough starfish on reef i in year t to consume 100% of the coral cover on a reef in one year, $COT(i,t)=0.1$ means enough to consume 10% of the coral cover, etc. Coral abundance is represented as a cover index $C(i,t)$, ranging from 0.0 (no live coral on reef i in year t) to 1.0 ("healthy" coral covering 100% of the available reef habitat).

Interannual changes in the coral cover index for each experimental reef is modelled as

$$C(i,t+1) = C(i,t) + gC(i,t)[1-C(i,t)] + K - COT(i,t) \quad (A7)$$

where g is a logistic growth-spreading parameter (the term $gC[1-C]$ represents growth of corals already present at the start of year t), and K is a coral colonization rate (new coral cover added per year from formation of new colonies). Note here that there are no additional "carrying capacity", or COT predation parameters, due to the units of measurement defined above for COT and C . There is an implied assumption that the COT have a very high rate of effective search for coral, so that their consumption is simply proportional to their abundance. This assumption must be wrong for cases where COT as defined above is greater than C , so the program has a check to make sure that the consumption loss term does not exceed 90% of C per year, ie so that the negative term is actually the lesser of $0.9 \cdot C(i,t)$ and $COT(i,t)$.

Interannual changes in COT are modelled with the balance relationship

$$COT(i,t+1) = sCOT(i,t) + SP(i,t)L(i,t)C(i,t) / [1+L(i,t)] \quad (A8)$$

where $sCOT(i,t)$ represents surviving COT from year t to $t+1$, $SP(i,t)$ is predation-related juvenile survival rate for the COT larvae $L(i,t)$ settling in year t (the delay from COT settlement to appearance is modelled as 12 months rather than a more realistic 18+ months), and the multiplier term $C(i,t) / [1+L(i,t)]$ represents effects of coral cover and intraspecific competition among COT juveniles on COT juvenile survival.

COT larval settlement $L(i,t)$ is modelled with the same basic structure as for fish larvae (see section above, eq A1), ie as retention of larvae produced by $COT(i,t)$ plus addition of "background" larvae from other reefs plus addition of non-retained larvae from other experimental reefs. However, the background larval addition of COT is assumed to vary in a periodic pattern, with each reef receiving a "pulse" of larval settlement lasting 4-5 yrs every 15-20 yrs; the timing of this pulse is shifted with latitude to form a space-time "wave", so that reefs in the southern part of the Central Section receive pulses about 10 yrs later than reefs in the center of the Cairns section. The pulse size for each reef is assumed proportional to the background larval addition rate for fish on the reef, since this background rate (see larval dispersal

section above) is assumed to reflect general position of the reef in terms of upstream sources of larvae of all types.

Alternative hypotheses concerning effects of fish predation on COT outbreaks are represented in the juvenile survival term $SP(i,t)$ in eq A8. It is assumed that fish predators have a Type II functional response to COT juvenile density, and that combined functional response and exploitation effects can be approximated by the exponential survival relationship

$$SP(i,t) = \exp\{-eN(i,t)/[1+ehL(i,t)]\} \quad (A9)$$

where $N(i,t)$ is the abundance of adult fish on reef i in year t (see fish dynamics sections above), $L(i,t)$ is COT larval density at the start of year t , e is the rate of effective search by fish for COT larvae (slope of relationship between COT juveniles eaten per fish per year versus number of juveniles available, when juvenile density is low), and h is the "handling time" per juvenile eaten (so that $1/h$ is the maximum annual consumption of COT juveniles per $N(i,t)$). The key uncertain parameters here are e and h . Low e values imply that fish take few COT juveniles even when these juveniles are abundant. High h values imply that the maximum number of COT juveniles eaten per fish is low, even when COT juvenile abundance is high. Low e values imply that fish predation is never important, while high h values imply that predation becomes progressively less important as COT abundance is increased even if it is important when COT abundance is low.

For some combinations of the predation e and h parameters, equations A8-9 imply a multiple equilibrium behavior for COT on any reef. If e and h are both quite large (efficient predators but with limited feeding rates), then COT juvenile density will tend to decrease to a low level set by larval immigration rates provided it is low enough initially. But if COT juvenile density is initially high and/or there is a sufficiently large input of larvae from outside sources, the predation mortality rate will decrease (same number eaten but from a larger initial abundance) so as to "release" or permit further increase. Another way to obtain similar multiple-equilibrium predictions is to assume that the fish have Type III (sigmoid) functional responses to changes in COT density; then local COT populations may be maintained by having the fish "switch off" (reduce predation rate) when COT densities are very low, rather than having low populations maintained (prevented from extinction) by immigration.

Thus the hypothesized predation functional response parameters (and type) are major determinants of the qualitative pattern of response predicted by the model to changing fish abundance. If e and/or h are small, then predicted effects of reduced fish predation may include a quantitative increase in prey (eg COT) abundance, but no qualitative changes in prey population dynamics. If e and h are large, the predicted effects may include abrupt "outbreaks" or qualitative changes in prey abundance associated with "release" from predation effects.

We do not mean to imply here that fish predation on COT is of Type II or type III or is even significant in the first place. By providing

the REEF user with the option to vary predation parameters, so as to generate different qualitative behaviors (quantitative increases versus outbreak releases), we seek only to provide a reasonable representation of how rapidly and strongly such behaviors should become evident at different locations on the GBR under different regimes of experimental fish reduction/recovery.

Sources of Statistical Variability: Stochastic and Spatial Structure Effects

For simulations of experimental design performance, the experimental reef model will include three sources of stochastic and structural variation among reefs: (1) random and persistent variations among reefs in recruitment rates; (2) interannual variation in fishing mortality rate; and (3) sampling variation in monitoring indices. Parameter values for these sources of variation are set on entry to the REEF program to "worst case" values based on analysis of available data from the GBR.

Five types of variation in recruitment rates are represented. First, "background" larval loading levels will vary among reefs due to reef position (see larval transport section above); these loadings are not varied from year to year in the calculations, but will produce up to 10 fold variation among reefs in average number of larvae settling per year. Second, there is assumed to be variation among reefs in juvenile carrying capacities due to variation in habitat structure; again this variation is assumed to be persistent over time, and for default parameter values will produce 5 fold variation among reefs in juvenile abundance. Third, there is assumed to be a north-south geographic cline in both larval and juvenile carrying capacities, so as to produce a 2x variation in capacities from Cape Flattery to the Capricorn-Bunker Group (capacities are varied linearly with latitude). Fourth, there is assumed to be log-normally distributed variation in early post-settlement (first year) survival rate, with each reef receiving an independent disturbance each year (ie, no autocorrelation or crosscorrelation among reefs). Fifth, the GBR is assumed to be "hit" each year by a set of randomly distributed larval "pulses", where each pulse is shaped like a bivariate normal distribution with longshore and offshore scale parameters such as to produce correlated recruitment variation among the reefs within one cluster but not (usually) among clusters.

Analysis of among-reef and interannual variation in recruitment rates from available survey data (off Townsville by Williams, over the whole GBR by Williams and Doherty, and by Doherty in the Capricorn-Bunker Group, all focussing on Pomacentrids) indicated that the five sources of variation listed above account for most of the possible "perversity" to be expected in recruitment dynamics. The only indication of strong autocorrelation in recruitment variation was in Doherty's 1981-88 data from the Capricorn-Bunker Group, where there was indication of a sudden and persistent decrease in recruitment rates in 1983. For most species analyzed by Williams and Doherty, the log-normal (reef-year specific) component of recruitment variation had a variance of around 0.2-0.4 and

accounted for about half of the total interannual variation around the mean for each reef; the other half of this variation was associated with "shared" effects among reefs (ie, cluster-scale larval pulses).

Interannual variations in line fishing harvest rate are represented by assuming that the rate is a uniformly distributed random variable for each reef, over a range of 0.5 to 1.0 times the "target" rate (set as a REEF policy variable) for the experiment. No autocorrelation or crosscorrelation among reefs is represented, nor are any clinal variations along or across the GBR. Since such variations certainly do exist, the model implicitly assumes that harvest rates on the experimental reefs will be deliberately controlled or exaggerated to target levels, though with randomly varying success.

Sampling variation is represented as having an independent, log-normally distributed effect on each measured abundance index (reef-year combination). The variance of this effect is set so as to give a relative sampling precision ($100 \times \text{standard error}/\text{mean}$) of about 20% on entry to REEF, though the user can change this precision to reflect increased or decreased survey effort. For coral trout (Ayling-Mapstone) and Pomacentrid (Doherty-Williams) surveys, the among-transect variance/mean ratio appears to generally be between 1.0 and 3.0 for trout, and 2.0-5.0 for Pomacentrids. At these levels of variation, the 20% baseline relative precision corresponds to sample sizes of 8-10 transects/reef/yr for trout, and 10-20 transects/reef/yr for the more variable Pomacentrids. To halve the relative error (to 10%) would require substantially larger numbers of transects (eg, 40/reef/yr for trout, 80-100/reef/yr for Pomacentrids). Thus the 20% base value for relative error is set on entry to REEF since it is doubtful that substantially more precise surveys will be worthwhile.

APPENDIX B. A GENERAL LINEAR MODEL (GLM) FOR STATISTICAL ANALYSIS OF RESULTS FROM THE EXPERIMENT

Here we provide additional details about the GLM used in Monte Carlo experiments to assess power of tests for alternative experimental designs. As noted in the text, the GLM model involves statistical assumptions that are more difficult to justify than would simple ANOVA/MANOVA models, and we review those assumptions here. For further information about GLM in general, we recommend the introductory texts by Searle and Graybill.

Suppose that $y(i, j, t)$ is the measured response for some variable (eg, coral trout adult density or COT index density) on a reef subjected to the i th line treatment regime in cluster j in year t . We assume that this response can be written as an additive sum of cluster, fishing, and local (reef) effects as

$$y(i, j, t) = C(j) + C(j, t) + F(i, t) + w(i, t) \quad (B1)$$

where $C(j)$ is a base mean response over time in the absence of line fishing for reefs in cluster j , $C(j, t)$ is a time dependent departure from this mean that is shared by all reefs in cluster j , $F(i, t)$ is a time dependent departure from $C(j)$ due to the i th fishing treatment regime as expressed in year t , and $w(i, t)$ is reef-specific variation due to location and time effects not explained by fishing or shared with other reefs in the same cluster.

In model (B1), the "residuals" $w(i, t)$ cannot be assumed to be independent and identically distributed random effects or forced to be so through any randomization process used in the selection of experimental reefs. In particular, the $w(i, t)$ are certain to be autocorrelated (expected value of $w(i, t)w(i, t+k)$ not equal to 0.0 for k nonzero) due to (1) intrinsic differences among reefs not explained by the cluster mean and cluster-time parameters; and (2) biological mechanisms that cause "random" disturbances to have persistent effects on abundances (eg, survival over many years of unusually strong year classes). A simple and reasonably conservative way to deal with this nonindependence is to essentially throw out some of the time series data for each reef, by assuming $w(i, t) = rw(i, t-1)$ where r is a first-order autocorrelation coefficient; this assumption leads to independent errors for the transformed observations $Y(i, j, t) = y(i, j, t) - ry(i, j, t-1)$. Note that there is one less $Y(i, j, t)$ than $y(i, j, t)$ for each reef. The GLM for $Y(i, j, t)$ has parameters $C(j)(1-r)$, $C(j, t) - rC(j, t-1)$, and $F(i, t) - rF(i, t-1)$. Note that these parameters will generally be smaller (and hence more difficult to detect as being significantly different from zero) than for eq. B1. In Monte Carlo trials, we did the estimation in terms of the Y variables (generated by the REEF system simulations), but we assumed $r=0$; thus we threw out part of the data but did not assume strong autocorrelation

(available data from Ayling's surveys and the Williams-Doherty surveys suggest $r < 0.3$).

The experimental line treatments and line-trawl interaction effects will create a variety of line fishing effects $F(i,t)$ which have to be "coded" as individual present/absent effects for the GLM estimation. Suppose these coded effects are called $f(1), f(2), \dots, f(n)$ where n is the number of fishing effects parameters to be estimated. We use the following coding convention in the REEF system (the user can change this coding through one of the program menus): $f(1)$ is the average (time-independent) effect of continued line fishing on (abundance on) a reef in an untrawled cluster; $f(2)$ is the average (time-independent) effect of continued line fishing on a reef in a trawled cluster; $f(3-6)$ are transient differences (year 2-year 1, year 3 - year 2, etc) between fished and unfished abundance for the 2nd-5th years following closure to line fishing for a reef in an untrawled cluster; $f(7-11)$ are transient departures from unfished abundance for reefs in untrawled clusters that are opened to fishing at the end of the 5th year of study; and $f(12-16)$ are transient departures during the second five years of the experiment for reefs that were open for the first five years and are then closed, in untrawled clusters. Fishing effects parameters $f(17-20)$ are the same as $f(3-6)$, except that they apply to reefs in trawled clusters; $f(21-25)$ are the same as $f(7-11)$ except that they are for reefs in trawled clusters; and $f(26-30)$ are the same as $f(12-16)$ except that they are for reefs in trawled clusters. For designs involving replication of line treatments within clusters, it would be possible to further articulate the f effects list to represent differences among clusters in the fishing responses; due to time constraints in the model development and Monte Carlo testing phases of our work, we did not investigate this possibility, though we suspect that it would lead to "overparameterization" (trying to estimate too many parameters) of the model.

An estimate of the variance of each of the coded GLM parameters ($C(j), C(j,t), f(i)$) is provided by a diagonal element of the GLM covariance matrix $C = s^2(X'X)^{-1}$, and variance estimates for individual linear contrasts of the parameters are calculated as quadratic forms $c'Cc$ where c is a vector with the weighting for each parameter included in the contrast (eg, the variance of the contrast $f(1)-f(2)$ is obtained by setting the c element for $f(1)$ equal to 1.0, the c element for $f(2)$ to -1.0, and all other c values equal to 0.0, then calculating $c'Cc$). Given the variance for any parameter or contrast, it is a simple matter to test whether the parameter or contrast differs significantly from zero, just by seeing whether its confidence interval (T statistic times square root of variance) includes zero. For each such (planned) test, the T statistic has the error degrees of freedom for the whole linear model estimation, which for all the designs we considered is large enough to assume $T=2.0$ for 95% confidence limit calculations.

Trawl and line-trawl interaction effects are not represented explicitly in the basic encoding of fishing effects described above. Instead, we have examined specific (and policy-relevant) components of these effects as contrasts among the model parameters. We have represented the "basic effect of trawling" as the mean difference between the $C(j)$ effects for trawled and untrawled clusters, ie the mean effect

of trawling on abundance in the absence of line fishing. We have represented the simplest component of the line-trawl interaction as the difference $f(1)-f(2)$, ie the difference between trawled and untrawled clusters in the basic effect of continued line fishing. Line-trawl interactions involving influences of trawling on recovery/depletion responses to line fishing could be examined by constructing contrasts among appropriate $f(.)$ effects; we have not examined estimation performance for such contrasts in the design evaluations to date.

General tests for the presence of overall trawl and line x trawl interaction effects would be simpler to do in the context of ANOVA models. The limited tests that we did by passing time-aggregated REEF simulation results to SYSTAT indicated that the GLM above has about the same power to detect the "basic trawl effect" as ANOVA has for detecting overall trawl effects. However, the ANOVA is less likely to detect line x trawl interaction effects. We are unclear about the reason for this difference between methods in power to detect interaction effects; most likely the problem is that all transient observations (and effects) are lumped for the ANOVA, hence masking obvious differences between trawled and untrawled clusters in transient responses to line fishing.

Table 1. Power of test indices for tests of differences in adult abundance under three experimental design options, for a range of hypotheses about response dynamics, effect sizes, and variability in recruitment. Tabled indices are percentage of general linear model (including temporal and cluster parameters) effects that differed significantly from zero over 10 Monte Carlo trials, where "data" for the linear model were generated with the REEF experimental reefs simulation. Each table row represents one combination of hypotheses about recruitment limitation (E=larval stage; J=juvenile stage), Fishing mortality rates (L=33%/yr line, 10%/yr juvenile trawl mortality; H=60%/yr line, 20%/yr juvenile trawl mortality), proportions of fish resident/migrating off reefs (L=none; H=50% off reef resident and 25% migrating/yr), and standard deviation of interannual variation in log recruitment (L=0.2; H=0.4). For example, hypothesis combination labelled E,L,L,H represents larval stage limitation, low fishing mortality, low off reef residence and migration, and high interannual variation.

Effects defined as follows:

TNL-base effect of trawling in absence of line fishing
 TBL-effect of trawling on base effect of line fishing
 LNE-effect of line fishing in absence of trawling
 LTE-effect of line fishing in presence of trawling
 RTE-recovery effects after line closure, no trawling
 DNT-decline effects after line opening, no trawling
 RTT-recovery effects after line closure, trawling open
 DTT-decline effects after line opening, trawling open

Design 1: 32 reefs in 8 clusters (5 trawled, 3 closed)

Hypo.	TNL	TBL	LNE	LTE	RTE	DNT	RTT	DTT	
E,L,H,H		10	90	10	100	20	0	30	70
J,L,H,H		50	70	60	100	20	20	20	55
E,H,H,H		50	50	80	100	20	0	40	90
J,H,H,H		90	20	90	100	10	20	40	70
E,H,L,H		80	50	100	100	20	60	20	80
J,H,L,H		100	80	100	100	40	90	20	70
E,H,L,L		100	40	100	100	10	70	10	100
J,H,L,L		100	90	100	100	40	100	40	100

Design 2: 24 reefs in 6 clusters (3 trawled, 3 closed)

Hypo.	TNL	TBL	LNE	LTE	RTE	DNT	RTT	DTT	
E,L,H,H		0	90	10	100	15	0	20	60
J,L,H,H		40	60	60	100	10	20	15	50
E,H,H,H		80	50	60	100	30	0	20	90
J,H,H,H		90	60	100	100	30	30	60	70
E,H,L,H		100	20	100	100	15	40	25	80
J,H,L,H		100	20	100	100	40	80	15	80
E,H,L,L		100	50	100	100	15	80	10	90
J,H,L,L		100	100	100	100	35	95	35	95

Design 3: 16 reefs in 4 clusters (2 trawled, 2 closed)

Hypo.	TNL	TBL	LNE	LTE	RTE	DNT	RTT	DTT	
E,L,H,H		0	100	0	100	0	0	0	60
J,L,H,H		0	40	20	100	20	20	25	50
E,H,H,H		0	100	10	100	30	5	30	90
J,H,H,H		60	30	80	100	10	20	15	50
E,H,L,H		10	40	80	100	25	10	20	70
J,H,L,H		0	50	100	100	15	50	15	60
E,H,L,L		0	90	100	100	75	5	30	90
J,H,L,L		90	70	100	100	50	90	30	70

Figure 1. Tradeoffs and constraints that define experimental design choices. Power of tests for trawl effects increases as number of clusters is increased; fewer clusters than shown by vertical line would represent an unreplicated experiment with regard to at least one trawl treatment. Possible variety of line fishing treatments and opportunities for local replication improve with increase in number of reefs per cluster. Curve shows design combinations (clusters, reefs/cluster) resulting in total experiment size of around 40 reefs; region above and to right of curve represents experiments that are considered too large in terms of costs, and region below and to left of curve represents weaker (lower power) experimental choices.

REEFS PER CLUSTER

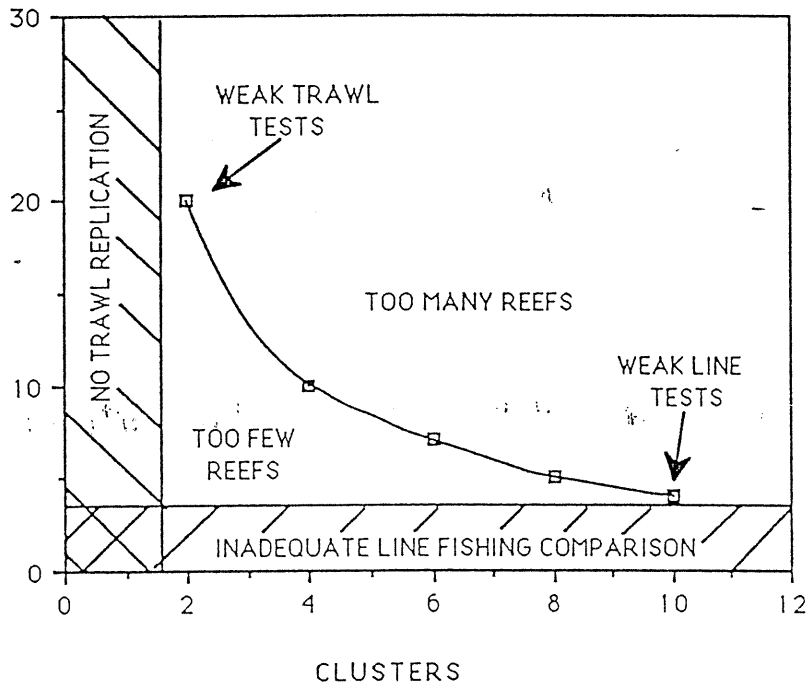
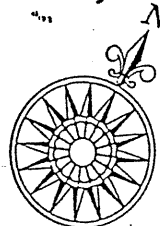


Figure 2. Recommended reef clusters for the effects of fishing experiment. Named reef for each cluster is the unfished line treatment reef, that has been closed to fishing prior to the experiment and would remain closed throughout. Arrows show cross-shelf positions of the closed reefs.

Coral Sea

Untrawled (Aqincourt#3)



Trawled (Beaver)

Trawled (Duncan)

Untrawled (Bowl)

Trawled (Kangaroo)

Untrawled (Jacquelin)

Trawled/Untrawled? (Hardy)

Trawled Untrawled (20-137)

