



GREAT BARRIER REEF
MARINE PARK AUTHORITY

RESEARCH PUBLICATION No. 47

An Investigation of Optimum Methods and Unit Sizes for the Visual Estimation of Abundances of Some Coral Reef Organisms



**B D Mapstone and
A M Ayling**

577.880
9943
INV
1998

RESEARCH PUBLICATION No. 47

An Investigation of Optimum Methods and Unit Sizes for the Visual Estimation of Abundances of Some Coral Reef Organisms

B D Mapstone

Department of Marine Biology
James Cook University

A M Ayling

Sea Research

© Great Barrier Reef Marine Park Authority 1998

ISSN 1037-1508
ISBN 0 642 23043 9

Published March 1998
by the Great Barrier Reef Marine Park Authority

The opinions expressed in this document are not necessarily those of
the Great Barrier Reef Marine Park Authority.

Accuracy in calculations, figures, tables, names, quotations, references etc.
is the complete responsibility of the authors.

National Library of Australia Cataloguing-in-Publication data:

Mapstone, Bruce D. (Bruce David).

An investigation of optimum methods and unit sizes for the
visual estimation of abundances of some coral reef
organisms.

Bibliography.
ISBN 0 642 23043 9.

1. Coral reef biology - Research - Queensland - Great
Barrier Reef - Methodology. 2. Marine organisms - Research
- Queensland - Great Barrier Reef - Methodology. 3.
Environmental sampling - Queensland - Great Barrier Reef.
4. Environmental monitoring - Queensland - Great Barrier
Reef. I. Ayling, Tony, 1947- . II. Great Barrier Reef
Marine Park Authority (Australia). III. Title. (Series :
Research publication (Great Barrier Reef Marine Park
Authority (Australia)) ; no. 47).

577.8809943



GREAT BARRIER REEF

MARINE PARK AUTHORITY

PO Box 1379
Townsville Qld 4810
Telephone (07) 4750 0700



CONTENTS

EXECUTIVE SUMMARY	v
Acknowledgments	iii
INTRODUCTION	1
MATERIALS AND METHODS.....	5
Location and Time of Field Work	5
Organisms Counted	6
Sizes and Allocation of Sampling Units.....	6
<i>Transect Sizes</i>	<i>6</i>
<i>Allocation of Sampling Units.....</i>	<i>6</i>
Counting Procedures.....	7
Different Methods of Survey	8
Data Handling and Analysis	9
Characteristics of the Data	9
Biases in Estimates of Population Density	10
<i>Preliminary Multivariate Analyses.....</i>	<i>10</i>
<i>Univariate Analyses.....</i>	<i>12</i>
<i>Site and Pass Effects</i>	<i>13</i>
<i>Effects of Transect Length and Width.....</i>	<i>14</i>
Analyses of Auxiliary Variables	15
<i>Effects of Migration of Fish Across Transect Borders</i>	<i>15</i>
<i>Variations in Counting Times</i>	<i>15</i>
<i>Effects of Depth of Transects</i>	<i>16</i>
<i>Effects of Time of Day on Estimated Density</i>	<i>16</i>
Effects of Observer and Methods of Surveying Large Transects.....	17
Precision and Cost-Efficiency of Sampling.....	17
<i>Precision from Transects of Different Dimensions</i>	<i>17</i>
<i>Cost-efficiency of Transects of Different Dimensions</i>	<i>17</i>
RESULTS	19
Characteristics of the Data	19
Biases in Estimates of Population Density	20
<i>Preliminary Analyses - Multivariate Data</i>	<i>20</i>
<i>Large Transects - Univariate Analyses</i>	<i>22</i>
<i>Site Variation and Effects of Pass</i>	<i>22</i>
<i>Effects of Transect Length and Width.....</i>	<i>26</i>
<i>Small Transects - Univariate Analyses.....</i>	<i>30</i>
Analyses of Auxiliary Variables	34
<i>Migration of Fish Across Transect Margins</i>	<i>34</i>
<i>Estimated Densities After Correction for Migration</i>	<i>36</i>
<i>Variations in Counting Times</i>	<i>38</i>
<i>Effects of the Depth of Transects</i>	<i>39</i>
<i>Effects of the Time-of-Day of Sampling</i>	<i>42</i>
Effects of Observer and Methods of Surveying Large Transects.....	42
<i>Comparisons of Observers</i>	<i>42</i>
<i>Comparisons of Counting Methods</i>	<i>44</i>
Precision and Cost-Efficiency of Sampling.....	44
<i>Precision from Transects of Different Dimensions</i>	<i>44</i>
<i>Cost-efficiency of Transects of Different Dimensions</i>	<i>51</i>

DISCUSSION	55
Characteristics of the Data and Analytical Methods	55
Sources of Bias	57
<i>Transect Dimensions</i>	57
<i>Search Intensity</i>	58
<i>Depth and Time of Day</i>	59
<i>Observers and Counting Methods</i>	59
Precision and Cost Efficiency	60
Optimum Transect Dimensions	61
Suggested Procedure for Routine Survey	63
Concluding Remarks.....	64
REFERENCES	66

TABLES

Table 1: Site Descriptions	5
Table 2: (M)ANOVA Models	11
Table 3: Taxa Counted	19
Table 4: MANOVA of Estimated Densities	21
Table 5: Summary of ANOVA of Densities by Taxon- Large Transects	23
Table 6: Details of ANOVA of Densities by Taxon - Large Transects	24
Table 7: ANOVA of Average Densities - Lethrinids & Lutjanids	28
Table 8: Summary of ANOVA of Densities by Taxon - Small Transects	31
Table 9: Details of ANOVA of Densities by Taxon - Small Transects	32
Table 10: ANOVA of Average Densities - <i>C. talboti</i> & <i>L. dimidiatus</i>	33
Table 11: Migration Across Transect Boundaries - Chaetodons & Coral Trout	34
Table 12: ANOVA of Densities Adjusted for Migration	36
Table 13: Search Intensities by Transect Size	38
Table 14: Depths of Transects	41
Table 15: Density Differences Between Shallow & Deep Half-transects	41
Table 16: Rank Frequencies of Counts by Observers 1 & 2	42
Table 17: MANOVA of Precision - Small & Large Transects	44
Table 18: ANOVA of Precision by Taxon - Small & Large Transects	46
Table 19: MANOVA of Cost-efficiency - Small & Large Transects	52
Table 20: ANOVA of Cost-efficiency by Taxon	52

FIGURES

Figure 1:	<i>T. derasa</i> - Effects of Site*Transect Width on Density	22
Figure 2:	<i>A. planci</i> , <i>C. baronessa</i> - Effects of Site*Pass on Density	26
Figure 3:	Effects of Transect Width on Density, by Taxon	27
Figure 4:	Effects of Transect Length on Density, by Taxon	29
Figure 5:	<i>T. derasa</i> , <i>C. trifasciatus</i> - Effects of Transect Length*Width on Density	30
Figure 6:	<i>C. talboti</i> , <i>L. dimidiatus</i> - Effects of Transect Width on Density	33
Figure 7:	Effects of Migration to/from Transects on Density - Chaetodons & Coral Trout	35
Figure 8:	Effects of Transect Size on Density Adjusted for Migration	37
Figure 9:	Observed and Expected Times to Survey Transects	39
Figure 10:	Effects of Counting Method on Density, by Taxon	43
Figure 11:	Effects of Diver Activity on Density, by Taxon	45
Figure 12:	Effects of Transect Length on Precision, by Taxon	48
Figure 13:	Effects of Transect Width on Precision, by Taxon	50
Figure 14:	<i>T. derasa</i> - Effects of Transect Length*Width on Precision	51
Figure 15:	Effects of Transect Width on Cost-efficiency, by Taxon	53

Acknowledgments

We wish to thank the following people, all of whom contributed substantially to the project: Howard Choat, for useful discussions and provision of the facilities of the Department of Marine Biology, James Cook University; Ros Priest and Brian Cohen for assistance with both the organisation and execution of field work, and the tedious data entry; Greg Andrew, Linda Axe, Richard Birdsey, and Steve Neale for field assistance and data entry; Steve Hillman and Jamie Oliver for project management at the GBRMPA end; and (especially) Gina Mercer and Lucy Mercer Mapstone for their support and tolerance during the preparation of this report. We also thank the three reviewers of the draft final report - Peter Doherty, Helene Marsh, and Tony Underwood - whose comments were of great assistance. Any persistent deficiencies, however, are ours entirely. The work was funded by a consultancy agreement from the Great Barrier Reef Marine Park Authority (to JCU and Sea Research) and a National Research Fellowship (to BDM).

EXECUTIVE SUMMARY

Surveys of abundances of demersal biota provide the empirical bases for assessing the biological status of the Great Barrier Reef, testing the success or failure of management strategies, investigating anthropogenic impacts on the reef, and contextualising fundamental research. The accuracy and precision of the data that arise from surveys will be at least partly a function of the survey methods¹. It is important, therefore, to thoroughly document the sampling characteristics of those methods. We sought to examine the relative biases and characteristics of precision of sampling several species of benthic organisms and demersal fish by visual surveys of belt (or strip) transects of a range of sizes. We also examined the biases arising from several procedures for counting organisms within transects.

Estimates of population densities of most poritid corals, chaetodontid and lethrinid fishes, and coral trout were affected significantly by the width of transect surveyed. Estimated densities were greater for narrow (4m wide) transects than for wider transects (12m & 20m wide). Estimated densities of chaetodons and poritid corals were also affected by transect length (20m, 60m, 100m), with estimates declining as transect length increased. Counts of small fishes within small transects (1-4m x 10-30m) were generally unaffected by the length or width of transects. The results were consistent over six sites on two reefs, which represented a diversity of habitat and exposure conditions.

The effects of transect size on estimated density were not caused by observers failing to count or over-counting fishes which migrated into or out of the transects whilst they were being surveyed. Nor did the results arise because of trends in abundances with either the depth of substratum surveyed or the time of day when counts were taken. The most likely explanation for changes in bias with changing transect size was systematic variation in the intensity with which transects were searched. Larger transects were searched far less intensively (time taken per unit area) than small transects. It is likely, therefore, that organisms were more likely to be overlooked in large transects than in small transects.

The method by which transects were counted did not affect estimated densities, although estimates differed systematically between two observers. Thorough training and periodic re-calibration of observers will be essential for the utility of data stemming from ongoing monitoring programmes.

The precision of estimates from shorter and, for some taxa, narrower transects was consistently poorer than from longer (and wider) transects. Precision of estimates varies with the number of replicates surveyed, and so it is useful to consider the comparative costs (time taken) of sampling sufficient transects of each size to achieve a standard precision. We refer to this measure as cost-efficiency. Cost-efficiency varied greatly among sites for transects of all sizes, and there was no consistent relationship between cost-efficiency and transect size for most taxa.

We recommend the use of 50m x 5m transects for the survey of many large, mobile fishes and discrete benthic organisms. Transects of this size are likely to provide the least biased estimates of density with the optimum balance of cost-efficiency and logistic convenience. For survey of several small fishes, we recommend transects of 20m x 2.5m, although there is little empirical reason to favour one transect size over the others we considered. It is important to note, however, that application of the methods we recommend to taxa other than those we considered should be preceded by thorough verification that the methods are appropriate for those other taxa.

We describe a set of procedures by which these transects can be surveyed efficiently by two-three divers and suggest that these methods provide a sound, well documented methodological basis for the further development of quantitative reef-wide monitoring.

¹ Provided that the sampling design is appropriate for the objectives being pursued.

INTRODUCTION

Increasing concern about the ethical and economic trade-offs between exploitation and conservation of natural biological resources in recent years has placed greater pressure on environmental managers. The Great Barrier Reef is a particularly topical case, both within Australia and world-wide. The gazetting of the Great Barrier Reef as a multi-use marine park explicitly demanded the conservation of the biological characteristics of the Great Barrier Reef in the context of ongoing recreational use and commercial development (GBR Marine Park Act, 1975). As managers of the GBRMP, therefore, the Great Barrier Reef Marine Park Authority (GBRMPA) is faced with balancing the interests of an array of commercial activities (*e.g.* tourist industries; scale, crustacean and sessile invertebrate fisheries; commercial shipping), recreational users, Aboriginal users, and research groups, whilst endeavouring to ensure that the bio-physical system is conserved. Management in this context effectively entails the regulation of human use, *eg.* by zoning areas for different allowable uses, rather than intervention in natural bio-physical processes *per se* (Hendee *et al*, 1990). The success of such regulatory management practices should be assessed with reference to two variables: i) status of the ecosystem that is to be conserved; and ii) the degree to which human use is facilitated to the satisfaction of users. Monitoring the status and dynamics of the natural system, and of human use of the system, therefore, provides the feedback necessary to assess the success or failure of management strategies. The quality and quantity of information derived from such monitoring is crucial to the evolution of prudent and justifiable management (Hendee *et al*, 1990).

Management responsibilities in the GBR region are manifest at a variety of scales of space and time. Impact assessment and issues of reef use are typically addressed at relatively local scales (within reefs) and over short times (one to five years). Zoning of the GBR and general management strategies, however, extend to very large spatial scales (reefs, regions) and are operative over long times (5 years - decades). To adequately assess the responses of reef biota to these multiple activities requires sound empirical knowledge of spatial and temporal patterns in the distribution and abundance of organisms on the GBR under 'normal' conditions, and of the resilience of populations to perturbation. This information is most efficiently provided by carefully planned quantitative descriptive studies over a range of spatial and temporal scales - *i.e.*, *via* a sound monitoring programme - supplemented with the manipulative experimental studies that are the province of fundamental research.

The information derived from monitoring is, in part, likely to be of a general, descriptive nature intended to provide an empirical context within which to make decisions about the management of specific issues or perturbations, and within which the results of local fundamental research can be better interpreted. It is important therefore, that field sampling is designed carefully to provide context-information for a variety of issues, as well as providing empirical tests of the effects of management strategies on biota (Alcala 1988, Craik 1981, Crimp 1986, GBRMPA 1978, 1979, Hendee *et al* 1990, Russ 1984, 1989). In most cases, management decisions will be made on the basis of existing information, such as that delivered by general monitoring, or after relatively short term studies. Typically it will be desirable to integrate information from different sources in a coherent way. The strongest and most general basis for coherent integration among studies is sound empirical knowledge about the sampling and analytical procedures from which each piece of information was derived (Andrew & Mapstone 1987, Green 1979). The choice of sampling methods for general monitoring, therefore, should be related to methods used widely in other studies, and the documentation of their characteristics will be an important component in the development of monitoring programmes.

The implementation of a general monitoring programme over such a range of scales will be expensive and logistically constrained. It is critical, therefore, that sampling methods are cost-effective. That is, methods should be chosen that i) provide reliable data ii) will maximise the potential to identify changes or patterns in the abundance of biota; iii) are logistically feasible to

repeatedly implement in a wide range of situations; and iv) are cheap, so that maximum flexibility for the design of monitoring can be retained, within what will always be limited budgets.

In addition to the above, mainly logistic considerations, the final design of a general monitoring programme must be based on considerations of the biology of the organisms of interest, the ways in which we perceive and count them, and the scales at which natural fluctuations in abundances occur and management actions are likely (Andrew & Mapstone 1987, Green 1979, Resh 1979).

Particularly important biological considerations include:

- the spatial and temporal characteristics of dispersion of the organisms;
- the rates and ranges of movement of the organisms, particularly with respect to the size of sampling units and the time taken to count them;
- the spatial and temporal scales at which abundances vary most.

Important methodological considerations are:

- the size and shape of unit within which organisms are counted;
- the rates and ways in which the units are surveyed by an observer;

Critical aspects of the design of a monitoring programme are:

- the numbers of units used;
- the arrangement of sampling units at spatial and temporal scales greater than those of the sampling units.

Interactions among the above biological, methodological, and design factors will determine the degree to which real patterns in abundances are reflected in our data, the precision of estimates obtained from a sampling programme, and the resolving power of statistical tests based on those estimates. The most effective allocation of available resources rests on a thorough understanding of these factors.

Thus, an early step in the development of a monitoring programme should be the examination of the relationships between methodology and small-scale biological features, resulting in the choice of sampling unit and method of survey that provides an optimum balance between reducing the costs of local sampling and maximising the precision of estimates for each subject species or group of organism. It should be verified, as far as possible, that the chosen sampling unit has adequate sampling characteristics over the range of environmental conditions (*e.g.* habitat, population density) within which it will be used. Non-destructive methods are generally preferred to methods that alter the environment being monitored. Subsequently, the optimum sampling method is used to estimate the variation in abundances at a range of scales, and these estimates are then employed to decide where best to allocate limited resources to answer a specific question.

Assuming that a range of sampling methods are feasible within an envelope of cost and logistic considerations, two main empirical features have been considered to distinguish which sampling units and/or methods are most desirable: i) accuracy or relative bias; and ii) precision. The accuracy of an estimate refers to its degree of departure from the true population value (Andrew & Mapstone 1987, Lincoln *et al* 1982, Sokal & Rohlf 1981, Underwood 1981). Bias refers to the consistency of inaccuracy - *ie* the extent to which repeated estimates tend to differ from the 'truth' in the same direction. Both accuracy and bias will be extremely difficult to assess in absolute terms for field studies because we generally always have only estimates of the parameter of interest and, therefore, can never be sure of its true value. The relative bias of a number of estimates can be inferred, however, if it can be assumed that all are biased in the same direction - *i.e.*, if it is probable that all are either over estimates or underestimates. In general, sampling units that provide data with the smallest bias will be preferred. The precision of an estimate refers to the expected variation in repeated estimates of the same population (Andrew & Mapstone 1987, Cochran 1963, Cochran & Cox 1957, Elliot 1977, Lincoln *et al* 1982, Sokal & Rohlf 1981), and is often expressed as the (unit-less) ratio of standard error to mean. The precision of an estimate is independent of its bias, and is by definition a relative property of a estimate unrelated to any absolute 'truth'. Hence, comparisons of precision are straightforward. Methods which provide better precision for the same sampling effort are preferred.

Several authors have found that either or both of these properties vary greatly with sampling method (e.g. Fowler 1987, Gray and Bell 1985, Lincoln Smith 1988, 1989, Samoilys & Carlos 1992), and/or the size of sampling units (see review by Andrew & Mapstone 1987, Downing & Anderson 1985, Downing & Cyr 1985), and may vary among habitats, times, sites etc. for a given sampling unit (Short & Bayliss 1985). These and other authors have raised the spectre of spurious inferences arising from inappropriate choice of sampling methods, and have emphasised the need to quantify the characteristics of precision and bias associated with chosen methods (Bros & Cowell 1987, Green 1979, Downing 1979, Pringle 1985).

The sampling characteristics of several methods of visual survey have been examined previously (e.g. hectare counts of *Plectropomus* spp. (Ayling 1983a, Ayling & Ayling 1984a, GBRMPA 1979); timed visual surveys and/or belt transects for estimating abundances of fish (Bohnsack & Banerot 1983, DeMartini & Roberts 1982, GBRMPA 1978, 1979, Kimmel 1985, Samoilys & Carlos 1992, Sanderson & Salonsky 1980); manta-tow, and strip-transect counts of holothurians over sand (Harriott 1984); manta-tow surveys of hard corals and *Acanthaster planci* (Fernandez, 1990, Fernandez *et al* 1990, Kenchington 1978), line-intercept methods, point, and quadrat methods (Weinberg 1981), and video methods (AIMS, Mapstone in progress) for estimating abundance of sessile fauna. Whilst the manta tow technique has received considerable scrutiny, there continues to be some uncertainty over the strengths and weaknesses of methods for the survey of benthic biota and demersal fishes on the GBR (Ayling 1983a, Ayling & Ayling 1984a, Crimp 1986, 1987, GBRMPA 1978, 1979, 1986),). In most cases, the relative bias of survey methods have been the primary characteristic of interest, in many cases based on a desire to adequately characterise the assemblage structure of the sampled areas (e.g. GBRMPA 1978, Russell *et al* 1978, Sale & Douglas 1981). Considerations of the precision of estimates of abundances or the utility of methods in the context of the likely design characteristics of monitoring programmes, however, have received less attention.

In this study we investigated the methodological aspects of estimating the abundances of a number of reef organisms. We were concerned primarily with the utility of belt (or strip) transects of various sizes as sampling units for the estimation of population densities. The characteristics of relative bias and precision of sampling by belt transects have not been considered in the context of a multi-species monitoring programme, although their application to counting particular species have been examined previously (Ayling 1983a, Ayling & Ayling 1984a, Fowler 1987, Mapstone 1988, Sale & Sharp 1983). The organisms with which we were concerned were relatively large, discrete organisms amenable to rapid counting in the field by divers, which were of interest to the GBRMPA as measures of management success (GBRMPA 1978), and which had been surveyed by related methods in the past (e.g., Ayling 1983b,c, Ayling & Ayling 1984b,c, 1985, 1986a,b, Doherty 1987, Doherty & Williams, 1988, Fowler 1987, Mapstone 1988). Our interest was in the selection of optimum methods for the estimation of population densities in the framework of future monitoring programmes for the GBR, and we were not considering the utility of visual survey as a means of estimating population structure (see Ayling 1983b,c, Ayling & Ayling 1984b,c, 1985, 1986a,b, 1991, 1992a,b, Ayling *et al* 1991, Crimp 1987, GBRMPA 1978, 1979).

Thus, the project had two primary objectives.

1. Estimation of relative biases caused by sampling unit dimensions, diver activity, different observers, and searching procedures.
2. Estimation of the characteristics of precision of estimates from units of different size and the relative cost-efficiency of sampling with those units. Relative cost-efficiency here means the expenditure required to obtain estimates with uniform precision.

MATERIALS AND METHODS

Location and Time of Field Work

Field work was done at Davies Reef (18°50’S 147°38’E) and Bowden Reef (19°01’S 147°56’E) in the Central Section of the Great Barrier Reef Marine Park between August 6 and September 30 1989. Both reefs are mid-shelf reefs, approximately 75km from the coast. The work was done during two trips to the reef(s). On the first trip (August 6-14), two sites were sampled on the front reef slope of Davies Reef, whilst on the second trip (September 17-30), a further two sites were sampled on Davies Reef and two sites were sampled on Bowden Reef.

The definition of a site was necessarily arbitrary *a priori*, but we sought to minimise habitat variation within sites whilst allowing the random allocation of sampling units with little overlap. Each site comprised, therefore, a band of superficially homogeneous consolidated reef 800-1000m long, 25m wide from the 2m depth contour, and with a maximum depth of 20m. In choosing different sites, we sought to maximise variation among sites to ensure generality of the results of the work. Hence, the six sites were chosen to represent a variety of conditions in which shallow reef organisms might be surveyed routinely on mid-outer shelf reefs. Site details are given in Table 1.

The allocation of transects in the field, however, was constrained such that transects did not span major and conspicuous discontinuities of habitat, such as sand patches, deep passes in the reef front, etc. Thus, some reef slope sites were lengthened by moving some transects along-shore to avoid such discontinuities, and transects were distributed among 10 and 14 bommies at sites 1 and 3 respectively. From one to five transects were counted on each bommie. This fragmentation of transect allocation was appropriate because: i) bommie fields are, by definition, fragmented habitat; ii) it would be likely (and desirable) that in routine sampling several bommies would have to be sampled to realise even moderate replication of transects within that habitat; and iii) given the above constraints, it was impossible and undesirable to fit all transects of all sizes on a single bommie.

Table 1: Locations, depth characteristics, and times of sampling of sites surveyed with transects of each dimension. Depths are the minimum and maximum depths of the margins of transects sampled, times are the earliest - latest times at which surveys were begun/finished at each site. Site # gives the number used in the text to refer to each site. 'Bommies' refer to large (>50mΦ) patch reefs isolated from other reef by expanses of sand. Sites 1 and 3 included 10 and 14 bommies respectively.

Reef	Location/Habitat	Depth (m)	Dates	Times	Site #
Davies	Windward reef slope	2 - 20	August 6-10	08:43 - 17:03	5
	Windward reef slope	2 - 18	August 10-14 ²	08:38 - 16:40	6
	Leeward large bommies	3 - 18	September 17-20	08:12 - 16:52	3
	Leeward reef slope	1 - 18	September 20-23	08:17 - 16:53	4
Bowden	Leeward large bommies	2 - 10	September 24-27	08:13 - 16:36	1
	Leeward reef slope	2 - 12	September 27-30	08:19 - 16:43	2

² As a result of injury, small fish were sampled at only one front reef site on Davies reef in the first field trip, and were sampled at the second front reef site on the second trip.

Organisms Counted

Organisms were surveyed in three groups, with individuals identified to species wherever possible. The rationale for taxon inclusion was in part arbitrary and in part reflected the priorities of the GBRMPA. Hence we set out to count:

- All chaetodontids, coral trout, lethrinids, and lutjanids;
- Crown of thorns starfish (COTS), large clam species, massive/sub-massive poritid corals;
- Selected small site-attached fishes.

Poritid corals were not distinguished taxonomically but were counted into five arbitrary size-classes: < 30cm Φ ; 31-60cm Φ ; 61-100cm Φ ; 101-200cm Φ ; >200cm Φ . Selected other taxa that could be sampled conveniently without additional cost were also counted.

It was intended also that *Drupella* spp and small (<100mm Φ), cryptic *Acanthaster planci* would also be counted, by destructively sampling within quadrats of 0.25-4.0m², but searches of quadrats of each size at each of two sites failed to find any juvenile *A. planci* and only 2 drupellids. This work was subsequently abandoned.

We chose to cover as many organisms as logistically possible because i) a general monitoring programme should take into account the status of several species, ii) the range of sizes of sampling units expected to be appropriate was the same for several organisms, iii) many of the organisms could be efficiently counted concurrently, and iv) much of the cost of such a study was incurred in vessel and support costs whilst in the field, so it was desirable to maximise the return from such costs.

Sizes and Allocation of Sampling Units

Transect Sizes

Several criteria were used to select the range of transect sizes surveyed:

- Prior experience of the likely local distribution, abundances, and behaviours of the major target organisms - transects were to be large enough for there to be reasonable expectation of encountering at least one individual of a species, but small enough to avoid having to count more than about 100 individuals;
- Logistic and safety considerations - the methods should be at least potentially useable in a variety of situations without demanding excessively deep diving or moving great distances from a dive tender;
- Methods used previously - future surveys had to be able to be related to prior surveys and so some overlap of sampling units was required to assess the relative performance of past and future methods;
- Minimum scale of sampling - units should be small enough to be able to sample within habitats or sites of moderate-small area.

On these bases, large, relatively mobile fish and benthic organisms expected to be of relatively low abundance were counted in transects of 20, 60, 100m lengths and 4, 12, 20m widths (hereafter 'large transects'). Small, abundant, and/or site-attached fish were counted along transects of 10, 20, & 30m lengths by 1, 2.5, and 4m widths (hereafter 'small transects'). Lengths and widths of transects were varied orthogonally for each group of organisms.

Allocation of Sampling Units

In allocating transects within each site, we sought to minimise variation in densities among transects without introducing systematic dependence among transects. Our intention was to ensure, as far as practicable, that transects of all sizes were providing estimates of the same population of organisms.

Prior to field work, a notional site map was drawn as a 800 x 25 unit rectangle. Four replicate transects of each size were allocated randomly within the 800 x 25 (metre) grid by choosing from random number tables the coordinates of the beginning of the shallow margin of each transect. The order in which transects were allocated was randomised across lengths and widths, with the constraints that: i) transects of the same size never overlapped, and ii) transects of the same width (but different lengths) or of the same length (but different widths) did not overlap. Since it would be impossible to survey all large or small transects at a site in a single day, transects were divided into three groups, with each group to be surveyed in a single **pass** over the site. In allocating transects to each pass, we chose transects such that i) at least one transect of each size was surveyed in each pass, and ii) transects would be surveyed in a uni-directional swim with minimum back-tracking along-shore from the end of one transect to the beginning of the next. The first condition ensured that effects of transect length and/or width would be orthogonal to any pass effects, which would include temporal variation in abundances, effects of increasing observer familiarity with each site, and effects of prior diver activity on the behaviours of target organisms. Minimising back-tracking was intended to reduce the potential for counts to be affected by recent activity of divers nearby. The three passes spanned 3 days for the large transects, and 1.5-2 days for the small transects.

Before counting began at a site, the shallow margin of the site was marked with numbered sub-surface buoys placed every 100m along the 2m isobath. All data were recorded in the field directly onto prepared waterproof data-sheets on which the transects were listed in order of survey, and which included a description of the starting position for survey of each transect. Starting positions were specified as both the absolute coordinates (x=1-780, y=1-21) and relative coordinates (x-y distances from the shallow end-point of the previously surveyed transect) of the beginning of the shallow margins of the transects. Hence, each transect was easily located with reference to either the coloured buoys or the end of the previous transect, which was marked by a small buoy dropped when that transect was surveyed (see below).

All four replicates of each transect size were surveyed at the first two sites visited (sites 5 & 6, Table 1). Given the time constraints of the work, and following preliminary analyses of those data, only three replicates of each size were surveyed at subsequent sites. Large transects were still surveyed in three passes, however, and because of the allocation of one transect of each size to each pass, this meant that the sampling design remained balanced across the orthogonal factors of transect length, transect width, and pass at each of these sites. For logistic reasons, surveys of the small transects were treated slightly differently from large transects. At sites 5&6, the fourth replicate of each transect size was allocated to either pass one or two only, with a total of 15, 12, and 9 transects being surveyed on the first, second and third passes respectively. At sites 1-4, the third pass was dropped, meaning that sampling remained balanced over transect length and width at these sites, but was unbalanced with respect to the main effect of pass, and interactions involving pass.

Counting Procedures

Organisms within each group (large fish, small fish, benthos) were counted concurrently during surveys. Large transects were surveyed independently of small transects, but large fish and benthos were counted within the same sets of large transects.

Having located the starting co-ordinates for a large transect, two divers swam isobathically along shore, laying a tape along each long edge of the transect. The two tape layers were connected by a length of buoyed twine equal in length to the width of the transect. Hence, by keeping the twine taught between them, the divers remained 4, 12, or 20m apart for large transects and 1, 2.5, and 4m apart for small transects. Because of the drag of the line, a third diver swam up the centre-line of each transect carrying the weight of the rope. The principal observer (Observer 1, A. Ayling) swam slightly ahead of this entourage, his rate of progress determining the rate at which the transect was layed, and the two tape layers providing his reference points for transect boundaries.

The following data were collected as each transect was layed:

- Observer 1 counted large fish (by species) by thoroughly searching over the width of the transect in a zig-zag pattern;
- Observer 2, swimming along the mid-line of each transect, also counted coral trout, chaetodontids, and lutjanids (as families) separately to their right and left out to the boundaries of the transect;
- The tape layers recorded the numbers of coral trout, chaetodontids, and lutjanids which entered or left the transect for up to 5m ahead of them, the forward boundaries being defined by projection of their swim path;
- The tape layers recorded their depths at the beginning and end of the transect, and observers 1 & 2 recorded the time at the beginning and end of each count.

A small buoy was dropped at the end of the shallow margin of the transect, from which the beginning of the next transect could be located (as described above).

As the tape layers wound up the tapes after completion of the outward swim over the transect, observer 1 returned along the transect counting *A. planci*, *Linckia*, *T. derasa*, and *T. gigas*. Observer 2 followed him counting heads of poritid corals into the above five size classes. Both observers searched the transects in a zig-zag pattern.

When counting small fish, transects were layed prior to surveys. Concurrent laying and counting were not feasible because two thirds of the transects were too narrow to allow three or four divers to work in the available area. Tape layers moved well ahead of the observers, laying transects up to 15 minutes ahead of the time they were surveyed. Observer 1 (Ayling) counted several species of labrid and pomacentrid within the transects. At sites 1-4, the second observer counted the selected species in the same way as did Ayling. Depths and times for these transects were recorded as for the larger transects.

Different Methods of Survey

We also considered four aspects of visual survey that might be expected to affect the estimation of abundances, as follows.

- i. Effects of disturbance of fishes attributable to previous diver activity were estimated by comparing counts made concurrent with laying transect tapes, shortly after laying the tapes, and 15-20 minutes later. These effects were assessed for only large (non-site attached) fish. Counts came from four 50m x 12m transects which were searched on each visit by two observers simultaneously swimming in zig-zag patterns.
- ii. Effects of diver activity on counts were also assessed by comparing counts obtained by swimming first along one half of the transect and then along the other (hereafter 'sequential counts'), as done by Ayling and Ayling in previous surveys (Ayling & Ayling 1983a,b, 1984a,b, 1985, 1986), with counts from a) a single pass over each transect by one observer swimming in a zig-zag pattern, and b) a single pass over each transect by two observers swimming in parallel, each counting fish in only one half of each transect (hereafter 'parallel' counts). These comparisons were derived from the data for each observer from the first count over the above transects (a), data from another four 50m x 12m transects surveyed by both observers in parallel (b), and a further 8 transects (4 per observer, 50m x 12m) surveyed in sequential halves.
- iii. Counts of fish from the midline of a transect were compared with those resulting from a zig-zag search of the same transects. These comparisons were derived from transects of all lengths and widths surveyed for the main part of the study (above).
- iv. Inter-observer variation was assessed by comparing counts between observers when those counts had been obtained by the same method, either on the same or different replicate transects. Thus, unconfounded comparisons of observers were possible for zig-zag counts from i), and parallel and sequential counts from ii). Comparison of observer specific counts of small fish were derived from the data for both observers from all small transects at sites 1-4 (Table 1).

Comparisons of methods in ii) (part b) and iii) were based on the assumption that the two observers counted fish equally well. Comparisons i-iii were done for counts of large fish only.

Data Handling and Analysis

Data were processed using dBase® software on a IBM-compatible personal computer. Statistical analyses were done using SAS® software, RT (Manly 1991, 1992) and software written by the senior author.

Data were entered into dBase tables from the original waterproof data-sheets and two readers cross-checked the print-outs of the databases with the raw data sheets. Discrepancies were corrected to conform to the raw data sheets. This process was repeated until no further errors were detected, which generally involved three or four iterations. Custom software was written to perform several range, consistency, and logical checks on the data and any apparent errors were checked against the raw data sheets. Finally, 100 records from the multiple database tables were selected at random and re-checked against the original data sheets. Analyses commenced only after no inconsistencies in the data were detected by these methods.

Characteristics of the Data

All count data were converted to estimates of population density before analysis. Density estimates from large transects were in units of individuals/100m², whilst those for small transects were expressed as individuals/10m². Only those taxa or groups of taxa for which less than one-third of the data were zero counts were analysed by raw densities. This cut-off was arbitrary. For these taxa, the data from all sites were pooled and the distributional characteristics of the data examined for transects of each length and width. Next, a linear model comprising the factors pass, transect length, transect width, and their pairwise interactions was fitted to the data from each site. Plots of the residuals after fitting the model against the predicted population densities were then examined for evidence of heteroscedasticity, non-linearity, and the existence of additional sources of systematic variation.

Because of the close proximity of the transects within each site, we were concerned about the potential for dependence among the data from nearby transects. The degree of dependence related to spatial proximity was assessed by the following method:

- Identify all unique pairwise combinations of transects at each site, excluding pairings of transects with themselves;
- Calculate the difference between the residuals (above) for transects in each pair;
- Calculate the distance between the centres of the transects in each pair;
- Plot the differences in residuals against the distances between transects and examine plots for evidence of trends in residual differences with inter-transect distance.

Plots of differences between residuals ($r_s = r_i - r_j$) against distance between the transects from which the residuals came (d_{ij}) could fall into three classes, depending on the degree and type of dependence between closely spaced transects:

1. If there was no proximity-related dependence among transects, residuals from closely spaced transects would be expected to be no more similar than residuals from distant transects. Hence, the plot of $r_s : d_{ij}$ would show a fairly consistent scatter over all values of d_{ij} around a horizontal line with zero intercept (the 'zero-line').
2. If counts from close transects were positively correlated, but the strength of correlation decreased as transects were placed further apart, the plot of $r_s : d_{ij}$ would show points clustered closely around the zero-line at the left of the plot (where d_{ij} was small) but an increasing spread of points around the line from left to right, as distance between transects (d_{ij}) increased.

3. Conversely, if counts from near transects were negatively correlated, there would be an exaggerated scatter of values of r_s at the left of the plot, with a convergence of points toward the right (as d_{ij} increased)

Distances between transect centres were calculated from the expected coordinates of the corners of each transect in the 800 x 25 grid to which they were randomly allocated. We assumed here that each site was 800m long and 25m wide. Clearly this would not have been so for the leeward bommie sites, since transects were scattered among 10 or 14 bommies. Distances between transects within bommies would often be overestimated because of the roughly circular shape of bommies, but their distances from transects on other bommies would be under-estimated. Thus greater emphasis was placed on the interpretation of plots from contiguous reef sites than from the bommie sites.

Biases in Estimates of Population Density

Four factors were considered in analyses of estimated population densities: variations among sites, the effects of transect length (3 treatments), transect width (3 treatments), and the pass over a site from which data arose (3 treatments for large transects; 2 or 3 treatments for small transects). Transect length and width were clearly fixed effects, and site variation was considered a random variable. Pass, however, was less clearly defined. Each pass over a site represented a) a different amount of observer familiarity with that site, b) a different amount of recent exposure of the organism at that site to divers, and c) a different day or half-day at each site for large and small transects respectively. 'Pass' effects, therefore, entailed these three inseparably confounded effects which might have been expected to influence estimates of population density. Both a) and b) would be expected to be systematic effects applied to all sites, but temporal variation might be expected to be a random variable nested within sites. We considered pass a fixed effect applied to all sites, however, because the passes occurred on successive days or half-days and so were not allocated randomly in time, and because the other two effects, if present, were expected to be non-random. Thus, the four factors were treated as orthogonal.

Preliminary Multivariate Analyses

Data from large or small transects were first analysed by multivariate analyses of variance, incorporating the above four factors. Data from all taxa counted sufficiently frequently for analysis (see above) were included in the respective multivariate dataset, after first considering the matrix of correlations among variables.

Two MANOVA were done for large and small transects. Firstly, the data from only sites 5 and 6 were analysed by the fully orthogonal model Length*Width*Pass*site, including all interaction terms (Table 2). This model was unbalanced because four replicates of each transect size were distributed among three passes at each site. Thus, each size of transect would be replicated in only one pass, there being a single sample of that size in each other pass. The replication of sizes was, however, randomised among passes. These were the only analyses for large transects, that provided tests of the four-way interaction in the full model and this was the primary reason for analysing the data from sites 5 and 6 alone. The imbalance in the data would be more likely to precipitate spurious rejection of the null hypothesis of no interaction than spurious failure to reject H_0 , so this test was considered conservative with respect to Type II errors. Where the 4-way interaction was non-trivial ($F > 1$) in these analyses, separate univariate analyses of the same model were done for each of the taxa in the multivariate data-set to identify those taxa for which the effect could be ignored and those for which it could not. The criterion for ignoring the effect after univariate analyses was $\alpha_{L*W*P*s} > 0.25$ (Winer 1971, Winer *et al* 1992). Since the univariate analyses represented multiple and dependent tests on data from common sampling units, these inferences would be extremely prone to experimentwise Type I error and so conservative with respect to Type II error.

Secondly, data from all sites were analysed by similar four-factor MANOVA, but analyses differed slightly between small and large transects. For large transects, the fourth replicate transects of each size from sites 5 and 6 were dropped for these analyses, so the data were balanced over all factors but there was only one datum per cell. The four-way interaction $L*W*P*s$ acted as a surrogate residual in these analyses, a strategy which rested on the assumption that the interaction was, in fact, trivial (Winer 1971, Winer et al 1992). Thus, only those taxa for which analyses of data from sites 5 and 6 indicated a trivial effect of this interaction were included in these analyses. For small transects, the number of passes was reduced to two, with 15 transects in the first and 12 in the second. Hence, data remained unbalanced with respect to pass effects, but tests of the $L*W*P*s$ interaction were possible, and the four-way interaction was retained in the analyses.

Table 2: Structures of four-factor analyses of variance to tests for the effects of pass, transect length, transect width, and variations among sites on estimates of population densities of several demersal taxa. Degrees of freedom are tabulated separately for analyses of data from sites 5 & 6 only and from all six sites. Degrees of freedom differed between analyses for large transects (L) and small transects (S) for the latter analyses. No test was possible for the $L*W*P*s$ interaction in analyses of large transect data from all sites, and these data were analysed only when the four-way interaction was trivial in data from sites 5 & 6.

SOURCE	DEGREES OF FREEDOM			F DENOM.	
	Analysis Transects	sites 5 & 6	sites 1- 6		Both
		L,S	L	S	L,S
Length	2	2	2	L*s	
Width	2	2	2	W*s	
Pass	2	2	1	P*s	
site	1	5	5	Res	
L*W	4	4	4	L*W*s	
L*P	4	4	2	L*P*s	
L*s	2	10	10	Res	
W*P	4	4	2	W*P*s	
W*s	2	10	10	Res	
P*s	2	10	5	Res	
L*W*P	8	8	4	L*W*P*s	
L*W*s	4	20	20	Res	
L*P*s	4	20	10	Res	
W*P*s	4	20	10	Res	
L*W*P*s	8	-	20	Res	
Residual	18	40	54	-	

Because we would be making equally important inferences from either the rejection or the non-rejection of at least some of the null hypotheses in these analyses (see below), we adopted the following relatively liberal interpretations of the tests. Effects for which the observed F-ratio had a chance of arising under H_0 of 10% or less ($\alpha \leq 0.1$) were considered significant, *ie* H_0 was rejected. Effects in the MANOVA with $\alpha \leq 0.25$ were considered worthy of further examination by univariate analyses. Effects for which $\alpha > 0.25$ were dropped from the above model when univariate analyses were done. We adopted Pillai's trace as the test statistic for MANOVA, but if any two of the remaining three test statistics commonly reported for MANOVA (Wilke's Lambda, Hotelling-Lawley Trace, Greatest Characteristic Root) precipitated $\alpha \leq 0.25$ when Pillai's Trace did

not, then we retained that term for further consideration. These steps were taken to reduce the potential for Type II error when dismissing 'non-significant' effects.

Univariate Analyses

The above multivariate results were followed by univariate ANOVA to resolve which taxa were precipitating significant effects in the MANOVA. The ANOVA models included only main effects and those interaction terms in the MANOVA for which $\alpha \leq 0.25$. Full model univariate analyses, analogous to the above MANOVA, were also done for the two inclusive groups, all chaetodons and all poritids, to examine whether transect dimensions were likely to be important for surveys in which only such inclusive groups were counted, without distinguishing species or size classes.

Clearly, the univariate ANOVA were for multiple variables sampled from the same sampling units, and therefore were non-independent. In order to contain the overall experimentwise Type I error rate to a (liberal) maximum of about 0.25 (the upper limit for 'potentially interesting' effects in MANOVA), the critical Type I error rate for conventional hypothesis testing with univariate ANOVA was set at 0.02 for the 11 taxa analysed for large transects, and 0.04 for the 6 taxa analysed for small transects. This corresponds to a Sidak adjustment of the significance level, rounded to two decimals for each data set (SAS Technical Report P229 1992, Sokal & Rohlf 1981, Winer *et al* 1992).

In addition to the above conventional procedures, we adopted the 'scalable' or 'flexible' decision criteria suggested by Mapstone (Mapstone 1992, 1995, 1996) for hypothesis testing. This procedure involves the rejection or non-rejection of H_0 against a variable critical Type I error rate which is set in relation to the desired maximum potential for Type II error. The procedure places emphasis on the choice of a nominated lower limit to what would be considered an important effect size (the differences among means under an alternative hypothesis, H_a), and the *a priori* evaluation of the relative importance of committing a Type I or Type II error. The procedure is as follows:

- a. Set the minimum magnitude and pattern of differences among means that is considered important - *ie* The minimum critical effect size (ES_c) that it is considered desirable to have a high probability of detecting if it exists;
- b. Evaluate the 'cost' of i) failing to detect that effect, or a greater one, if it really occurs (committing a Type II error), and ii) inferring such an effect if it does not occur (a Type I error);
- c. Label these costs C_{II} and C_I respectively, or if they can't be evaluated in absolute terms, specify their relative importance, $k = C_{II}/C_I$;
- d. Given ES_c , set the desired maximum risk of Type II error (β') and the critical type I error rate for rejection/non-rejection of H_0 (α_c) such that $\alpha C_{II} = \beta' C_I$, or alternatively, $\alpha_c/\beta' = k$, where $k = C_{II}/C_I$.

The final value of α_c employed is set by iteration, as follows:

- i. Set an initial (desirable) value for α_c ;
- ii. Using ES_c and the relevant error variance for the term in question, calculate the estimated value of β (β_0) on the assumption that H_0 was not rejected at that critical value of α ;
- iii. If $\beta_0 > \alpha_c/k$ or $\beta_0 < \alpha_c/k$, then reset α_c up or down respectively;
- iv. Iterate steps ii and iii until $\beta_0 = \alpha_c/k$.
- v. Calculate α for the observed F-ratio of the effect of interest (α_0) and reject H_0 if $\alpha_0 < \alpha_c$, where the comparison is with the final value of α_c resulting from the above iteration.

In practice, then, α_c will be a variable for evaluation rather than a standard. It will depend on the values of ES_c and k chosen by the investigator, and will vary both within and between analyses in relation to the degrees of freedom and, in the case of ANOVA, the denominator variance of the F-test.

We have used Mapstone's procedure for setting 'significance' criteria throughout our univariate analyses, but we present the results of conventional hypothesis tests for information.

In order to maximise the power of tests in univariate ANOVA, we also pooled the Sums of Squares (SS) and degrees of freedom (df) of trivial effects in the model with their respective F-ratio denominators. Tests for any remaining effects would thus be more powerful than with the unpooled SS and df (Winer 1971, Winer *et al* 1992). The criteria for pooling effects were either i) $\alpha_o > 0.25$; or ii) $\alpha_o > 0.1$ and $\beta_o < 0.05$, where β_o was calculated for ES_c . The first criterion has been widely recommended (Sokal and Rohlf 1981, Underwood 1981, Winer 1971, Winer *et al* 1992) whilst the second stems from Mapstone's flexible decision criteria and an arbitrary weighting of our willingness to risk pooling erroneously against our desire to maximise the df of our tests.

Because it was expected that our data would depart from the assumptions of the parametric analyses we wished to use, we took the following steps to verify that any statistically significant effects arose because of real effects and were not artefacts of violating the assumptions of the analyses.

- Data from each transect size at each site were averaged over passes and the averages treated as replicates for two-factor ANOVA comparing the effects of transect lengths and widths
- The above two factor analyses were repeated using the median values from transects of each size at each site
- The raw data were analysed by ANOVA in which the probability of the observed arrangements of the estimates were calculated by randomisation procedures (Manly 1991, 1992).

The distributional properties of the averaged data were also examined to verify whether averaging had improved conformity with the assumptions of parametric statistics. If discrepancies arose between the outcomes of the analyses of raw data and the above analyses of means/medians or randomisation tests, then the interpretation of 'significance' for the relevant effect was considered uncertain.

Finally, differences among means in significant terms in the univariate ANOVA were resolved by one-way ANOVA (for breaking down interaction effects) and/or Ryan-Einot-Gabriel-Welsch procedures (= 'Ryan's Test', Day & Quinn, 1989), for which the critical Type I error was the same as used for the original ANOVA.

Site and Pass Effects

Our principal interest in this project was to assess the effects of transect length and width on estimates of population density and sampling efficiency (see below), and to make recommendations about the use of these visual survey methods for general application on the GBR. Hence, any effects of pass or site on the effects of transect length and/or width would be seen as 'nuisance' effects, potentially invalidating the generality of statements that might be made about such length or width effects. Note that it was only interaction effects that posed a problem in this context. Significant effects of pass, site, or pass*site, in the absence of interactions with transect length or width, would not be considered important for conclusions about the effects of transect length and/or width. Note also that we sampled a wide range of situations ONLY in order to examine the consistency of effects of transect length and width on estimates of abundances over variable conditions. We were not interested in comparisons of abundances among, for example, habitats *per se*, and our data do not facilitate such comparisons since four of the six sites surveyed were irreconcilably confounded with habitat and/or reef.

The non-significance of interactions between pass and/or site and transect length and/or width would be taken as licence to make general recommendations about the relative merits of transect dimensions. Alternatively, the presence of such effects might mean that we concluded that there was no evidence for general effects of transect size, which might be taken as endorsement of 'anything goes'. Either conclusion would have important implications for the design of future sampling programmes based on our results. Hence, we considered Type I and Type II errors of equal importance for tests of pass and site effects and set $k=1$. For consistency, we set the same critical effect size for pass effects as we set for the effects of transect length and width (see below). For the random effects of site or interactions between site and other factors, we assumed that site effects would be important if they caused the effects of transect length and/or width to vary as much among sites as those effects precipitated differences in density within sites (see below).

Effects of Transect Length and Width

Failure to reject an hypothesis of no effects of transect size on mean estimates of density would be taken as evidence that sampling biases associated with visual surveys were independent of transect size. This conclusion would mean that relative cost-efficiency, precision, or power became the principal basis for choice of transect size. Issues of relative bias would be cast aside.

Alternatively, rejection of that null hypothesis would lead to the inference that bias varied with transect size. In this case, the choice of transect size would be strongly influenced by consideration of the relative biases of using transects of different sizes. We considered an erroneous inference of either conclusion equally important (and consequential), and here also set $k=1$.

The critical effect size for the main effects of transect length and width was based on the following premises.

- The random allocation of transects within sites would result in replicates of each transect size sampling the same population with the same true density for transects of all sizes. Thus differences in estimates among transects of different length and/or width would reflect only sampling bias associated with transect dimensions.
- We would not be able to assess the absolute accuracy (Andrew & Mapstone 1987) of our counts or the absolute bias of our estimates, so comparisons among transects of different size would reflect only their relative biases.
- We were less likely to have over-estimated density by counting organisms that we should not have, than to have under-estimated density because of failure to count organisms. Hence, larger estimates were likely to be less biased than smaller estimates.
- The power to detect relative changes in population density in future visual surveys would be likely to increase with increasing mean (estimated) density.

Thus, it was desirable to identify those transect sizes that resulted in highest estimates of population density.

In this context, we examined by simple simulation the relationship between reduction in expected rates of Type II error (=increase in resolving power) and increasing magnitude of estimates, given the presence of a standard *relative* ES. We plotted the reduction in Type II error against proportional change in the mean for a wide range of degrees of freedom. Critical Type I error rate was standardised at 0.05 (though any constant value would have done), and it was assumed that error variance varied in proportion to the square of the mean. Such plots clearly would vary with the df of the test and the relationship between the variance and the mean, but they will generally be monotonically decreasing and concave-up. For the majority of cases we examined, Type II error was reduced by less than 0.2 for proportional increases in mean of less than 1.5, and the curves began to flatten toward an asymptote at about a two-fold change in mean. On this evidence, we set the critical ES for the calculations of power such that the differences between two adjacent means would be 50% of the overall mean density, with the three means in a comparison of transect lengths or widths all differing.

Thus, the H_a we specified for the main effects of both transect dimensions (m_1, m_2, m_3) was:

$$m_1 \neq m_2 \neq m_3,$$

where

$$m_2 - m_1 = m_3 - m_2 = G/2,$$

and

$$G = \text{the grand mean of all data.}$$

Hence,

$$\frac{m_2}{m_1} = 2.0 \quad \text{and} \quad \frac{m_3}{m_2} = 1.5,$$

which covered the range of increments in means that were likely to result in reduction of Type II error by at least 0.2, but were likely to be in the region of the above curves where expected rates of Type II error were still declining rapidly with increasing magnitudes of estimates. The same effect sizes were applied to interactions among fixed effects on the assumption that the effects of one factor on another (or on an interaction) was orthogonal to existing effects.

Analyses of Auxiliary Variables

Effects of Migration of Fish Across Transect Borders

Estimates of immigration and emigration of fish to/from transects were derived from the counts by the tape layers of fish crossing projected borders. Rates of migration were standardised to fish/10m of transect length ($10 \times \text{Entries/Length}$, $10 \times \text{Exits/Length}$), with immigration being given a positive sign and emigration being negatively signed. Net rates at which fish entered or left the transects ($\# / 10\text{m}$) were calculated as the sum of immigration (positive) and emigration (negative). The means of each measure of movement were calculated for transects of each length and width over all sites. The expected effects of such behaviour on true population densities (individuals/100m²) were calculated by dividing the numbers of immigrants, emigrants, and net migration by transect area and multiplying by 100.

Where migration varied with transect size, the potential existed for apparent effects of transect size to arise because of differential treatment of migrants by observers. For example, observers might more readily include or exclude migrants when counting narrow transects than when searching wider transects because the full width of the narrowest transects would be in view at all times, whereas much of the observers time spent searching wider transects would not be within view of one or other side of those transects. The true population density of fish in a transect would be observed if all fish inside the transect margins when the transect was defined were counted. On the premise that all those fish that exited transects during counting were within the transect boundaries immediately prior to counting, and that all immigrants were outside transects prior to counting, we considered the best estimate of true density to be that which included in counts those fish that were observed leaving transects, but excluded from counts those fish that entered the transects during counting. On this basis, raw counts for transects were adjusted for potential effects of migration such that any apparent differences in estimated densities were minimised. If systematic differences among transects persisted in spite of such corrections, the inference of transect size effects could be said to be robust to effects of fish migration during counting.

Variations in Counting Times

When estimated densities varied with transect dimensions, we compared mean search intensity among transects of different sizes. Search intensity was defined as the time per unit area spent surveying a transect. Variations in search intensity with transect size would potentially introduce transect size dependent biases in counts. If search intensity remained relatively constant over all transects, it would be expected that total search time would vary in direct proportion to transect area. We assessed the degree to which this happened by graphically comparing the mean times taken to survey transects of each length and width with the times expected to be taken if all

transects were searched with equal intensity. Where clear discrepancies appeared between observed and expected search times, and patterns in these discrepancies were consistent with differences in estimated density related to transect dimensions, we multiplied our counts by the ratio {expected search time} / {observed search time}, and analysed these standard search time corrected data in the same ways as we analysed the raw estimated densities. Differences between these analyses and analogous analyses of raw estimates provided insights to the extent to which variation in search efficiency might have accounted for observed differences in bias among transects of different size.

Effects of Depth of Transects

Because densities of several taxa were expected to be correlated with the depth of transects within which they were counted, and because transects of increasing width would be expected to encompass increasing ranges of depths, there existed the potential for depth-related trends in estimated density to precipitate apparent effects of transect width. For example, given a sloping substratum and a negative correlation between depth and density, a 20m wide transect starting at a depth of 3m would be expected to return a lower estimated density than a 4m wide transects starting at 3m simply because the wider transect included more deep substratum than the narrower transect. Thus, it was considered important to examine as far as possible the potential for confounding of effects of depth and transect width.

This involved four strategies.

- i) Calculations of correlations between estimated densities and depths of transects and correlations between the residuals of the P*L*W models (see *page 9*) and transect depth at each site. Removal of significant correlations between density and depth by fitting the P*L*W model would indicate potential confounding of depth related trends in density with effects of interest in analyses.
- ii) Comparison of the average depths of the centres, and shallow and deep margins of transects of different widths to establish whether width-dependent variations in transect depth were consistent with density-depth correlations and observed width effects on estimated density.
- iii) Calculation of differences between counts from the shallow and deep halves of transects by observer 2, swimming in the transect mid-line.
- iv) Analyses of data from observer 2 in which the estimates of densities from the shallow halves of 12m and 20m wide transects were compared to the estimates from the whole of the 4m wide transects.

The fourth set of analyses was based on the assumption that a) the shallow halves of 12m and 20m wide transects were of similar depth to 4m wide transects, and b) counting from the mid-line of the transects was equally efficient over distances of 2m, 6m, and 10m. Any reduction in counting efficiency with increasing distance from the observer would precipitate effects of transect width and so undermine the basis for these tests because of confounding between depth effects and width effects. Thus, the results from these analyses would be noteworthy only if any effects of transect width that were apparent in data from observer 1 were absent in these tests.

Effects of Time of Day on Estimated Density

The time of day when each transect was surveyed was calculated as the average of the starting and finishing times expressed as the number of minutes after sunrise. Correlations between these times and estimated densities and residuals after fitting the P*L*W model (*page 9*) were calculated for each abundant species. As with depth effects, substantial reductions in correlations after fitting the P*L*W model would suggest confounding of effects in the model and variation in density with time of day.

Effects of Observer and Methods of Surveying Large Transects

Estimates of densities by the two observers counting by the same methods were compared in two ways. Where observers counted fish by the same method but on different replicate transects, differences between observers were assessed by single factor ANOVA. This was the case for the sequential counts. Where both observers counted fish on the same replicate transects, by the same or different methods, the difference between the counts was calculated for each transect and these differences analysed. The average difference between observers in this case was compared with zero by one sample t-test for each taxon. This was done for the paired zig-zag and parallel counts. The binomial probabilities of the observed frequencies of positive and negative differences were also calculated for each set of tests.

Different counting methods were compared as follows:

- Counts at different intervals after laying transect tapes were compared only graphically.
- Comparisons of the zig-zag, sequential, and parallel counts were done by single factor ANOVA. Data for each observer from zig-zag and sequential counts were compared to the combined data of both observers from the parallel counts in two sets of analyses.
- Differences between zig-zag and midline counts on the same transects (which were confounded with observers) were compared among sites and transects of different lengths and widths by three factor ANOVA. These analyses were done only if it was unlikely that there were consistent differences between observers, in which case the comparison of methods would be unambiguous.

In all of the above cases, multiple species were counted by each observer, and data were first analysed by multivariate methods when possible. In such cases, univariate analyses were only done when significant effects ($\alpha \leq 0.1$) were apparent for the multivariate analyses.

Precision and Cost-Efficiency of Sampling

Precision from Transects of Different Dimensions

The precision (Standard Error/Mean) of estimates from transects of each size was calculated for each site. Average precision was compared among transect lengths and widths by treating these 6 (site) estimates for each transect size as replicates in two factor analyses of variance, the factors being transect length and transect width. Hence, we compared the relative performance of units of different dimensions in a range of habitat/site/reef situations. Transects with lower values for average precision would be preferred.

As before, Type I and Type II errors were considered equally important in these tests, and so $k=1$ was used for the calculation of α_c for hypothesis tests by the proposed flexible decision rules. The critical effect size for these analyses was set at 0.1. That is, we wished to detect with power $(1-\beta)$ equal to $1-\alpha_c$ changes in precision of 0.1 or greater. This was an arbitrary criterion, and was an absolute rather than relative measure of effect.

Cost-efficiency of Transects of Different Dimensions

We defined cost efficiency as the estimated time required to obtain an estimate of population density with a specified desired precision. The expected time required to realise that precision (t_p) was calculated from

$$t_p = n_p t_u$$

where

t_u = average time taken to survey one sampling unit (of a given length and width at a given site); and

n_p = estimated number of sampling units needed for precision p
 $= s^2 / (\bar{x} p)^2$,

\bar{x} , s^2 being the mean and variance of data from transects of a given size at a given site.

We set desired precision arbitrarily at 0.2 for the purpose of these analyses, and calculated estimates of $t_{0.2}$ for transects of each size at each site. These estimates of cost for desired precision were then compared among units of different size by two factor analyses of variance, as for comparisons of estimated precision (above). More efficient units would require less time to realise the given precision.

The critical effect size here was set at 60 minutes, on the basis that it would be possible to allow an additional hour in a day of sampling for use of a less efficient sampling unit. If use of that unit required more than an additional hour per day, however, it would be likely that fieldwork would extend to a second day. This would represent a substantial increase in the cost of a field programme.

RESULTS

Characteristics of the Data

A total of 42 taxa were counted along large transects and 7 taxa along small transects (Table 3).

Table 3: Taxa and/or size classes of organisms counted on at least one transect. Those post-scripted with * are the taxa that were recorded on more than 67% of transects.

LARGE		TRANSECTS	SMALL TRANSECTS
Chaetodontidae	Serranidae	Tridacnidae	Labridae
<i>Chaetodon auriga</i>	<i>Plectropomus laevis</i>	<i>Tridacna gigas</i>	<i>Labroides dimidiatus*</i>
<i>C. aureofasciatus*</i>	<i>P. leopardus*</i>	<i>T. derasa*</i>	<i>Thalassoma lunare*</i>
<i>C. baronessa*</i>			
<i>C. bennetti</i>			
<i>C. flavirostris</i>	Lutjanidae	Acanthasteridae	Pomacanthidae
<i>C. kleinii</i>	<i>Lutjanus bohar</i>	<i>Acanthaster planci*</i>	<i>Centropyge bicolor</i>
<i>C. lineolatus</i>	<i>L. carponotatus</i>		
<i>C. melannotus</i>	<i>L. fulviflamma</i>		
<i>C. plebeius</i>	<i>L. russelli</i>	Ophidiasteridae	Pomacentridae
<i>C. rafflesi</i>	<i>L. sebae</i>	<i>Linckia laevigata</i>	<i>Amblyglyphidodon curacao*</i>
			<i>Chrysiptera talboti*</i>
<i>C. rainfordi*</i>	<i>L. quinquelineatus</i>		<i>Paraglyphidodon nigroris*</i>
<i>C. speculum</i>			<i>Pomacentrus moluccensis*</i>
<i>C. trifascialis</i>			
<i>C. trifasciatus*</i>	Lethrinidae	Poritidae	
<i>C. ulietensis</i>	<i>Lethrinus obsoletus</i>	(massive / sub-massive)	
<i>C. unimaculatus</i>	<i>L. miniatus</i>	<i>Poritids 1-30cm*</i>	
<i>C. vagabundus</i>	<i>L. atkinsoni</i>	<i>Poritids 31-60cm*</i>	
<i>Chelmon rostratus*</i>	<i>L. nebulosus</i>	<i>Poritids 61-100cm*</i>	
<i>Coradion chrysozonus</i>	<i>L. erythracanthus</i>	<i>Poritids 101-200cm</i>	
	<i>Monotaxis grandoculis</i>	<i>Poritids > 200cm</i>	

Only 17 of these taxa, however, were recorded with sufficient frequency at two or more sites for analysis of estimated population densities (Table 3). Data from the low abundance chaetodontid species were pooled to form another group ('Other *C. spp*'), and analyses were also done for the inclusive groups 'all chaetodons' and 'all poritids'. Since the blue-spot coral trout *P. laevis* was relatively uncommon on the reefs sampled, and secondary observers did not reliably distinguish *P. leopardus* and *P. laevis*, we analysed only counts of total coral trout (hereafter *Plectropomus spp.*). Data for an additional two agglomerated groups (total lutjanids, total lethrinids) were averaged over transects of each length and width within sites and these average densities were then analysed, with site means representing replicate estimates from sampling units of different sizes. This strategy was also adopted for *Labroides dimidiatus* and *Chrysiptera talboti*, although these species were common enough also for analyses of raw densities at two of the six sites.

Only the three smallest size classes of poritids (hereafter 'small' 'medium' 'large') were common, and data from the largest size classes were analysed only by their inclusion in the group 'all poritids'. Small poritids were counted only at the first two sites sampled (sites 5 & 6). They were not sampled subsequently because counting them within the large transects took too long and jeopardised completion of the other work. Consideration should be given to counting these corals

in smaller transects (such as those by which we counted small fish) in future surveys, though we did not examine such options. Thus, analyses for small poritids apply to only two sites.

Density data were significantly right skewed ($g_1 > 1.04$, $\alpha < 0.05$) and leptokurtic ($g_2 > 0.435$, $\alpha < 0.05$) for most sampling units and most taxa, but the degree of skewness was not consistently related to transect length and/or width. Residuals of fitted Pass*Length*Width linear models for each site were mostly uniformly distributed over the major range ($\geq 90\%$) of predicted densities, with only infrequent increase of residuals with density. For no taxon was heteroscedasticity indicated at more than one site. Residual plots suggested some non-linearity or the effect(s) of some other variable(s) for several taxa at one site, but again these results were not present at more than one site for any taxon, and nor were they consistently associated with one site for all taxa. When means and medians of data from transects of each size at each site were examined, they were found to be approximately normally distributed (Shapiro-Wilke statistic, $\alpha > 0.05$; $-1.66 < g_1 < 1.66$, $\alpha > 0.05$) and homoscedastic (Cochran's test, $\alpha < 0.05$) for all taxa from most transect sizes. Departures from normality were not consistently related to particular sizes of transect. With the exception of *T. derasa*, results of analyses of these means and medians, and of randomisation tests on raw data, consistently agreed with the results of analyses of raw data with respect to inferences about effects of transect length and width on estimated densities. All analyses presented hereafter are of raw data.

There was no evidence of strong dependencies among observations from transects in close proximity for any taxon. In all cases, both the signs and magnitudes of differences between pairs of residuals showed no clear relationship simply to the distances between the transects from which they were derived. For several taxa, there was weak evidence of systematic variation in residuals with position along-shore at one or two sites. Such patterns were never evident at more than two sites and were not consistent among sites or taxa. Further, in few cases were residuals from transects at similar positions along shore more similar than transects separated by over half of the length of each site. Where such similarities, apparently related to proximity, were apparent, they were local phenomena within a site and at other places in the same site residuals of adjacent transects were greatly disparate. Finally, these plots clearly indicated that the randomisation of transects of different lengths and widths across the sites had effectively ensured that whatever systematic long-shore effects were present were shared among replicates of each length or width. We inferred, therefore, that the data from even closely juxtaposed transects were effectively independent for the purposes of the analyses of interest here, though we cannot rule out unequivocally weak serial correlations (among transects of different sizes) at one of the six sites for some taxa.

Biases in Estimates of Population Density

Preliminary Analyses - Multivariate Data

MANOVA of estimated population densities of all non-inclusive taxa (*i.e.* excluding groupings of taxa such as all chaetodons or all poritids) at the first two sites sampled indicated that the 4-way interaction Length*Width*Pass*site was a trivial effect for large transects ($F_{96,112}=0.862$, $\alpha = 0.772$) but was a statistically significant effect for the small transects ($F_{48,108}=1.409$, $\alpha = 0.073$). Univariate analyses for each of the six taxa sampled on small transects at sites 5 and 6 indicated that this effect was non-trivial only for *L. dimidiatus* ($F_{8,18}=2.683$, $\alpha = 0.039$, $F < 1$ for all other species). Repeating the MANOVA without *L. dimidiatus* produced a result consistent with this conclusion ($L*W*P$'s: $F_{40,90}=1.119$, $\alpha = 0.325$). Since the four-way interaction was clearly trivial at sites 5 and 6 for all taxa sampled on large transects, MANOVA for all six sites were done for data from large transects on the assumption that the interaction $L*W*P$'s was trivial at all sites. The same conclusion arose for the composite groups of all chaetodons and all poritids after univariate analyses of data from the first two sites sampled ($F_{L*W*P*s} < 1$ in both cases).

Multivariate analyses of data from the six sites showed significant effects of interactions between transect length and width, pass and transect width, and pass and site for the large transects. The interaction between pass and site was significant for the small transects (Table 4). All other terms in the analyses were non-significant even against a liberal criterion of $\alpha_c=0.1$. For both sets of transects, most interaction terms involving transect dimension(s) and site were trivial, having F-ratios of less than one. Indeed, the only such term which would be inferred to explain any variance in the model ($F>1.0$) was the interaction between site and transect width for both assemblages (Table 4).

Table 4: Results of four-factor MANOVA of estimated densities from transects of different lengths and widths surveyed in two passes (small transects) or three passes (large transects) over each of six sites. Data from large transects included those from *Plectropomus* spp, *Chaetodon aureofasciatus*, *C. baronessa*, *C. rainfordi*, *C. trifasciatus*, *Chelmon rostratus*, other chaetodontids, *Acanthaster planci*, *Tridacna derasa*, medium and large poritids. Estimated densities of *Amblyglyphidodon curacao*, *Paraglyphidodon nigroris*, *Pomacentrus moluccensis*, and *Thalassoma lunare* were analysed for the small transects. Effects with $\alpha\leq0.1$ (*) were considered statistically significant, whilst effects with $\alpha\leq0.25$ (?) were considered of potential interest and retained in the model for univariate analyses. ‘-’ indicates those effects that were not considered further, most of which accounted for little or no variation in the data ($F \leq 1$). Lightly shaded terms are those which would potentially restrict conclusions about effects of transect dimensions to data from the first pass; heavily shaded terms are those that would potentially undermine any generalisations about effects of transect dimensions.

SOURCE	LARGE TRANSECTS				SMALL TRANSECTS			
	F _{PT}	df	α	Infer	F _{PT}	df	α	Infer
Length	N/A ³				2.033	8,16	0.108	?
Width	N/A ³				1.291	8,16	0.315	-
L*W	1.980	44,52	0.009	*	1.493	16,80	0.123	?
L*P	1.074	44,52	0.400	-	0.596	8,16	0.768	-
W*P	1.468	44,52	0.092	*	1.267	8,16	0.326	-
L*W*P	1.080	88,296	0.314	-	0.748	16,80	0.737	-
L*s	0.853	110,390	0.840	-	0.963	40,216	0.540	-
W*s	1.155	110,390	0.163	?	1.263	40,216	0.150	?
L*W*s	0.770	220,440	0.986	-	0.874	80,216	0.756	-
L*P*s	0.993	220,440	0.521	-	0.418	40,216	0.999	-
W*P*s	0.931	220,440	0.725	-	0.708	40,216	0.904	-
L*W*P*s	N/A				1.071	80,216	0.345	-
Pass	N/A ³				3.290	4,2.	0.247	?
site	3.917	55,170	0.0001	*	4.001	20,216	0.0001	*
P*s	1.491	110,390	0.003	*	1.618	20,216	0.050	*

³ Note that there were insufficient error degrees of freedom to test the main effects of transect Length, transect Width, and Pass for the large transects, but all main effects were included in *a posteriori* univariate ANOVA.

Large Transects - Univariate Analyses

Results of univariate ANOVA for data from large transects are summarised in Table 5, with detailed results presented in Table 6.

Site Variation and Effects of Pass

Site effects were large and significant by both conventional and flexible decision criteria for most taxa counted on large transects (Tables 5, 6). As discussed earlier (page 13-14), site effects *per se* were not of special interest here, and were not examined in detail. Of greater interest was the consistent non-significance of interactions between site and transect width (Table 5). The only exception to this generalisation was for *T. derasa*, for which effects of transect width were site dependent (by both decision criteria, Table 5, 6). This interaction arose because estimates from 4m wide transects at site 1 (Davies Reef, front reef slope) were significantly greater than estimates from 12m and 20m wide transects at the same site ($\alpha=0.009$), whereas transects of all widths returned fairly similar estimates at all other sites (Fig. 1; sites 2-5: $f<1$; site 6: $\alpha=0.053$). Estimates from transects of 12m and 20m width were not statistically distinguishable at any site. Estimates from 4m wide transects were ranked above those from 12m and 20m wide transects at 3 of the six sites ($\alpha=0.32$). From these results, it seems unlikely that transect width had important effects on estimates of density of *T. derasa*.

Figure 1: Effects of transect width on estimated densities of *T. derasa* at each of the six sites sampled in this study. Density is in clams/100m². Error bars are standard errors.

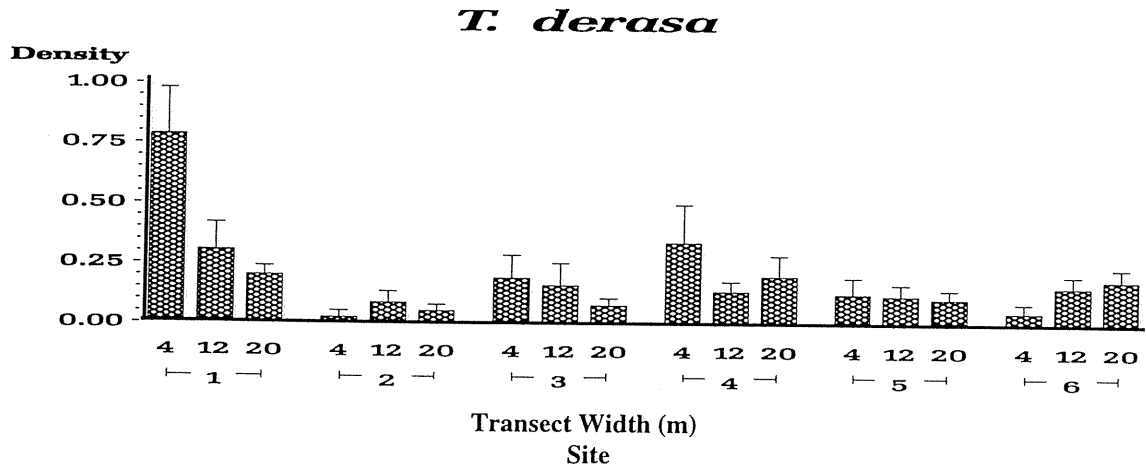


Table 5: Summary of results for univariate ANOVA of data from large transects. Table 5A summarises results from ANOVA done to resolve which of the potentially important effects identified in MANOVA were significant for which species. Table 5B summarises results for those taxa not included in MANOVA and for which univariate analyses were of the full L*W*P*s model. Effects considered to be statistically significant are indicated by # for the variable α_C decision rule, and by * for a conventional criterion of $\alpha_C=0.02$. Non-significant effects are indicted by '-'. Lightly shaded terms are those which would potentially restrict conclusions about effects of transect dimensions to data from the first pass; heavily shaded terms are those that would potentially undermine any generalisations about effects of transect dimensions.

A.

SPECIES	SOURCE OF VARIATION								
	Length	Width	L*W	W*P	W*s	Pass	site	P*s	
<i>Plectropomus</i>	- -	# *	- -	- -	- -	- *	# *	- -	
<i>C. aureofasciatus</i>	- -	# *	- -	- -	- -	- -	# *	- -	
<i>C. baronessa</i>	- -	# *	- -	- -	- -	- -	# *	# *	
<i>C. rainfordi</i>	# *	# *	- -	- *	- -	- -	# *	- *	
<i>C. rostratus</i>	- -	# *	- -	- -	- -	- -	- -	- -	
<i>C. trifasciatus</i>	- -	- -	# *	- -	- *	- -	- -	- -	
Other <i>C. spp</i>	- -	# *	- -	- -	- -	- -	# *	- -	
<i>A. planci</i>	- -	- -	- -	- -	- -	- -	# *	# *	
<i>T. derasa</i>	- -	- -	# *	- -	# *	- -	# *	- -	
Poritids 31-60cm	# *	# *	- -	- *	- -	- -	# *	- -	
Poritids 61-100cm	- -	- -	- -	- -	- -	- -	# *	- -	

B.

SOURCE OF VARIATION	SPECIES GROUP		
	All Chaetodons	All Poritids	Poritids 1-30cm ϕ
Length	# *	- *	# *
Width	# *	# *	# *
L*W	- -	- *	- -
L*P	- -	- -	- -
W*P	- -	- *	- -
L*W*P	- -	- -	- -
L*s	- -	- *	- -
W*s	- -	- -	- -
L*W*s	- -	- -	- -
L*P*s	- -	- -	- -
W*P*s	- -	- -	- -
L*W*P*s	- -	- -	- -
Pass	- -	- -	- *
site	# *	# *	- -
P*s	- *	- -	- -

Table 6: Results of univariate ANOVA of observed density of separate taxa or size classes counted within large transects. Tabulated statistics are those derived after pooling trivial higher order effects. Columns headed $\alpha_{c=\beta}$ give the critical Type I error rate for testing H_0 by the proposed flexible decision rule. Thus, this value is also the estimated probability of Type II error when H_0 is not rejected. Effects considered to be statistically significant are indicated by # for the variable α_c decision rule, and by * for a conventional criterion of $\alpha_c=0.02$. Non-significant effects are indicted by '-'. Columns headed β_{02} give the estimated probability of Type II error when H_0 was not rejected against critical Type I error rates of $\alpha_c=0.02$. See pages 14-15 for discussion of effect sizes used in estimation of β . Lightly shaded terms are those which would potentially restrict conclusions about effects of transect dimensions to data from the first pass; heavily shaded terms are those that would potentially undermine any generalisations about effects of transect dimensions.

A: Fish

<i>SPECIES</i>	<i>SOURCE</i>	<i>df</i>	<i>F</i>	α	$\alpha_{c=\beta}$	<i>Infer</i>	β_{02}
<i>Plectropomus</i>	Width	2,150	13.986	0.000	0.004	# *	
	Pass	2,150	5.043	0.008	0.004	- *	
	site	5,142	2.949	0.015	0.020	# *	
All Chaetodons	Length	2,126	11.235	0.000	0.000	# *	
	Width	2,10	26.188	0.000	0.001	# *	
	L*W	4,126	2.140	0.080	0.000	- -	0.000
	W*s	10,100	1.687	0.094	0.000	- -	0.000
	site	5,110	12.356	0.000	0.001	# *	
	P*s	10,100	2.819	0.004	0.000	- *	
<i>C. aureofasciatus</i>	Width	2,152	9.377	0.000	0.015	# *	
	site	5,142	3.061	0.012	0.035	# *	
<i>C. baronessa</i>	Width	2,136	5.526	0.005	0.021	# *	
	L*W	4,136	2.066	0.089	0.082	- -	0.215
	site	5,132	4.658	0.001	0.037	# *	
	P*s	10,132	2.434	0.011	0.014	# *	
<i>C. rainfordi</i>	Length	2,122	12.523	0.000	0.000	# *	
	Width	2,10	14.730	0.001	0.012	# *	
	L*W	4,122	2.042	0.093	0.001	- -	0.000
	W*P	4,122	3.395	0.011	0.001	- *	
	W*s	10,122	1.983	0.041	0.000	- -	0.000
	site	5,122	11.744	0.000	0.005	# *	
	P*s	10,122	3.246	0.001	0.000	- *	
<i>C. rostratus</i>	Length	2,157	2.049	0.132	0.089	- -	0.239
	Width	2,157	4.230	0.016	0.089	# *	
<i>C. trifasciatus</i>	L*W	4,136	8.106	0.000	0.022	# *	
	W*s	10,122	2.690	0.005	0.004	- *	
	site	5,132	2.674	0.025	0.017	- -	0.016
Other <i>C. spp</i>	Length	2,150	3.170	0.045	0.008	- -	0.003
	Width	2,150	13.024	0.000	0.008	# *	
	Pass	2,150	2.514	0.084	0.008	- -	0.003
	site	5,142	3.108	0.011	0.026	# *	

Table 6: (continued)

B: Benthos

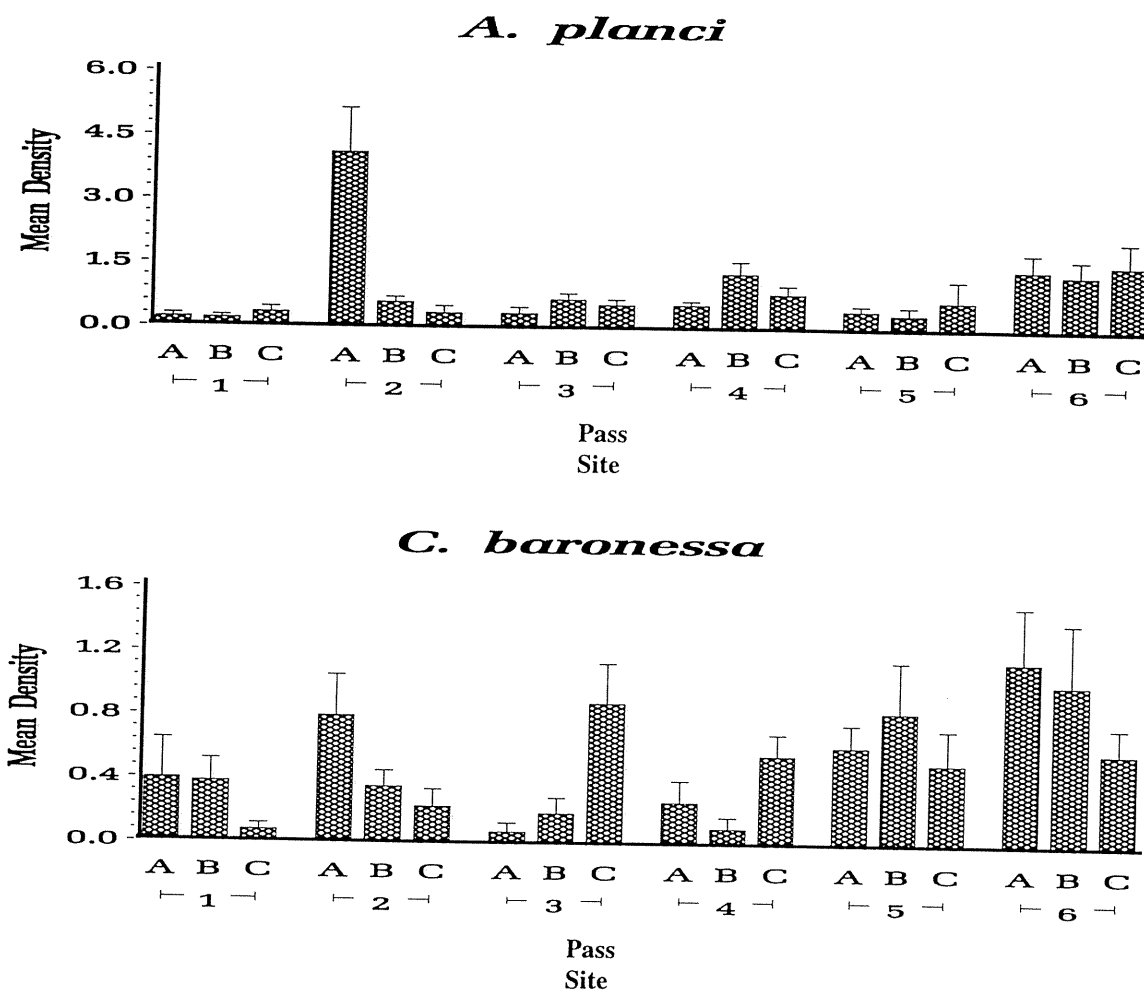
SPECIES	SOURCE	df	F	α	$\alpha_{c=\beta}$	Infer	β_{02}
<i>A. planci</i>	W*P	4,132	1.552	0.191	0.103	- -	0.291
	site	5,132	8.086	0.000	0.045	# *	
	P*s	10,132	6.972	0.000	0.019	# *	
All Poritids	Length	2,10	6.459	0.016	0.008	- *	
	Width	2,10	13.147	0.002	0.006	# *	
	L*W	4,88	3.155	0.018	0.000	- *	
	W*P	4,88	4.872	0.001	0.000	- *	
	L*s	10,80	2.391	0.015	0.000	- *	
	W*s	10,80	2.086	0.035	0.000	- -	0.000
	L*P*s	20,60	1.947	0.025	0.000	- -	0.000
	site	5,80	72.962	0.000	0.003	# *	
	P*s	10,80	2.149	0.030	0.000	- -	0.000
Poritids 1-30cm ⁴	Length	2,57	11.596	0.000	0.000	# *	
	Width	2,57	37.923	0.000	0.000	# *	
	L*W	4,57	2.863	0.031	0.001	- -	0.000
	W*P	4,53	2.854	0.032	0.001	- -	0.000
	Pass	2,57	7.396	0.001	0.000	- *	
Poritids 31-60cm	Length	2,132	11.918	0.000	0.001	# *	
	Width	2,132	17.748	0.000	0.001	# *	
	L*W	4,132	3.006	0.021	0.009	- -	0.004
	W*P	4,132	3.278	0.013	0.009	- *	
	site	5,132	6.642	0.000	0.011	# *	
	P*s	10,132	1.831	0.061	0.002	- -	0.000
Poritids 61-100cm	Width	2,152	2.447	0.090	0.011	- -	0.006
	site	5,142	6.095	0.000	0.028	# *	
<i>T. derasa</i>	L*W	4,132	2.546	0.042	0.127	# -	0.376
	W*P	4,132	1.599	0.178	0.127	- -	0.376
	W*s	10,122	3.041	0.002	0.027	# *	
	site	5,132	8.418	0.000	0.055	# *	

Effects of Pass on estimates of population density were also small and non-significant for most taxa, though interactions between pass and site effects were statistically significant for *C. baronessa* and *A. planci* (Table 5, 6). The Pass*site interaction for *A. planci* arose because of one exceptionally high estimate, derived from the first pass over one of the front reef slope sites at Davies Reef. Pass effects were trivial at all other sites (Fig. 2). For *C. baronessa*, pass effects were not consistently related to the order of survey (Fig. 2), with the greatest density estimates at sites 1,2, and 6 coming from the first pass, that at site 5 arising from the second pass, and the greatest estimates at sites 3 and 4 resulting from the last visit to those sites. Differences between the greatest and the other two estimates were statistically significant only at sites 2, 3, and 4.

⁴ Data from sites 5 and 6 (Table 1) only.

Given this variation in pass effects, we inferred that these results probably reflected daily variation in the local abundance of *C. baronessa* and that diver activity was unlikely to have affected counts in a consistent way.

Figure 2: Effects of pass on estimates of density of *A. planci* and *C. baronessa* at each of six sites. Density is individuals/100m². Error bars are standard errors.

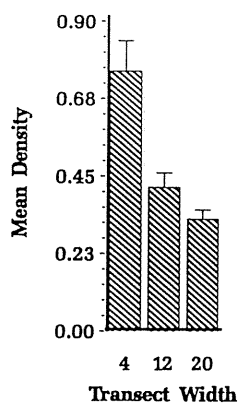


Effects of Transect Length and Width

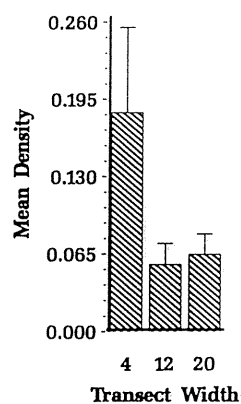
Effects of transect length and/or width on estimates of density from large transects were statistically significant for 12 of the 14 taxa or groups analysed (Table 5, 6). The main effect of transect width was significant and independent of interactions with other factors for 10 of the taxa (Table 5). In all cases, estimates of population density decreased with increasing transect width (Fig. 3), and estimated density from 4m wide transects was substantially and statistically significantly greater than estimates from 20m wide transects. Estimates from 4m wide transects were also significantly greater than those from 12m wide transects for all taxa except *C. rostratus* (Fig. 3). Estimates from 12m wide transects were significantly greater than those from 20m wide transects for *C. rainfordi*, *C. rostratus*, and total chaetodons. Maximum differences between mean estimates of density from transects of different widths were about 70%-90% of the total sample population means (Fig. 3).

Figure 3: Effects of transect width (m) on estimates of population densities (individuals/100m²) of taxa counted within large transects. Error bars are standard errors.

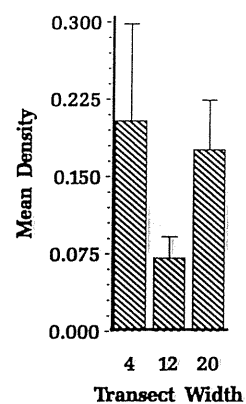
***Plectropomus* spp.**



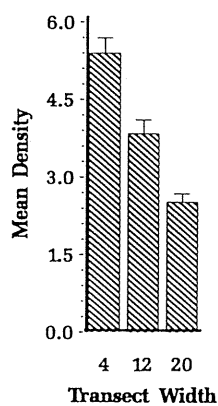
Lethrinids



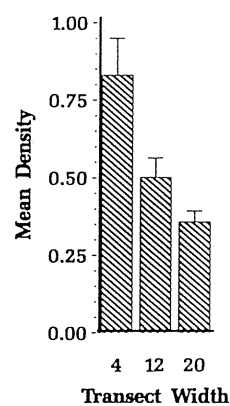
Lutjanids



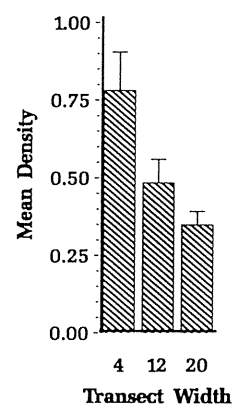
All Chaetodonts



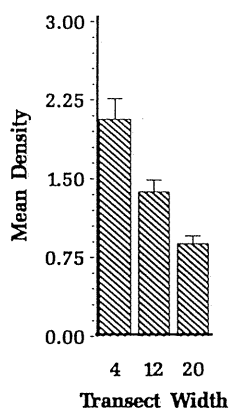
C. aureofasciatus



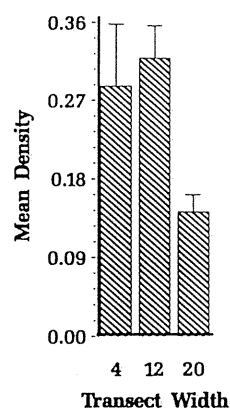
C. baronessa



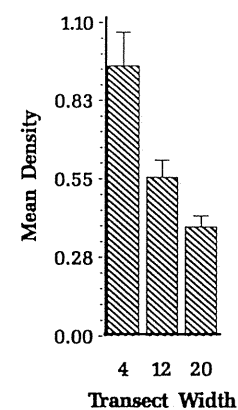
C. rainfordi



C. rostratus

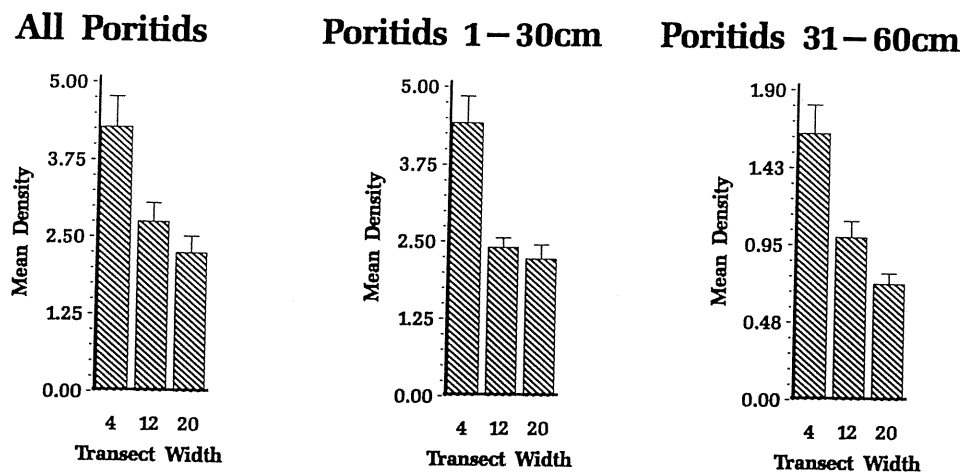


Other *C.* spp



(... Continued)

Figure 3 (Continued).



Effects of transect width on estimated densities of total lutjanids and total lethrinids were also considered significant when site means for transects of each size were analysed by the simplified L*W model (Table 7). As with other species, 4m wide transects produced higher average estimates of densities than either 12m or 20m wide transects for both groups (Fig. 3). The effects were weak relative to variation among sites, however, and given the higher value for α for total lutjanids ($\alpha=0.306$), we regard the importance of this result with caution. Note, however, that the confidence we would have in inferring no effect of transect width would be much less by both conventional and scalable decision criteria ($\beta_{05}=0.878$, $\alpha_{c=\beta}=0.427$). Effects of transect length and the interaction between transect length and width were trivial ($F<1$) for both groups.

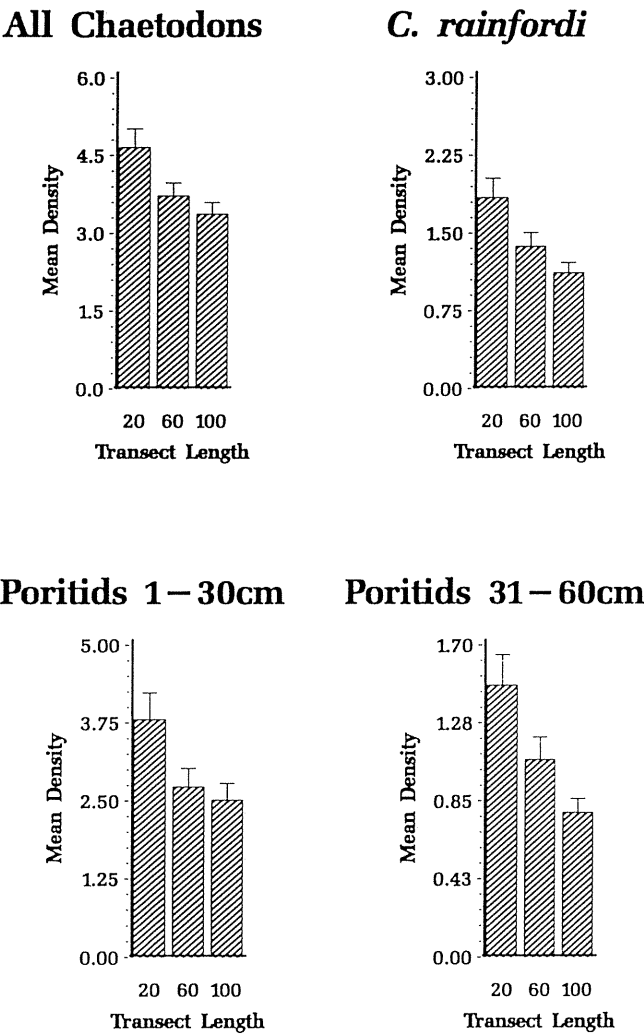
Table 7: Results of ANOVA comparing effects of transect length and width on mean estimated densities of all lutjanids and all lethrinids. Raw data were the averages of estimated densities from all transects of each size at each site. Columns headed $\alpha_{c=\beta}$ give the critical Type I error rate for testing H_0 by the proposed flexible decision rule. This value is also the estimated probability of Type II error when H_0 is not rejected. Effects considered to be statistically significant are indicated by # for the variable α_c decision rule, and by * for a conventional criterion of $\alpha_c=0.02$. Columns headed β_{02} give the estimated probability of Type II error when H_0 was not rejected against critical Type I error rates of $\alpha_c=0.02$. See pages 14-15 for discussion of effect sizes used in estimation of β .

SPECIES	SOURCE	df	F	α	$\alpha_{c=\beta}$	Infer	β_{05}
Lethrinids	Width	2,45	2.386	0.104	0.436	# -	0.889
Lutjanids	Width	2,45	1.216	0.306	0.427	# -	0.878

The main effect of transect length was independent of other effects and statistically significant (scalable α_c) for only four taxa: *C. rainfordi*, all chaetodons, small poritids, and medium poritids (Tables 3,4). Again, estimates of density decreased with increasing transect size, in this case transect length (Fig. 4). For all groups, estimates from 20m long transects were significantly greater than those from 100m long transects, but differed significantly from 60m long transects for

only the small poritids (Fig. 4). Sixty metre and 100m long transects could not be distinguished for any group. Mean estimates of density from transects of different lengths differed at most by about 40%-60% of the total sample population means (Fig. 4).

Figure 4: Effects of transect length (m) on estimated population densities (individuals/100m²) of those taxa from large transects for which density varied significantly with transect length. Error bars are standard errors.

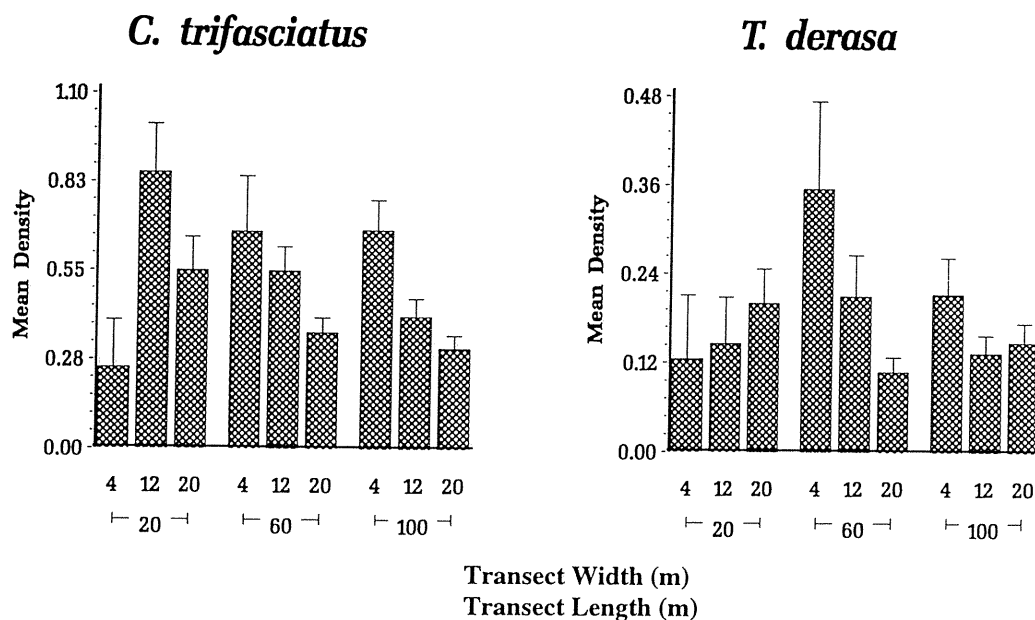


Interactions between transect length and width were considered significant against a scalable α_c for only *C. trifasciatus* and *T. derasa* (Tables 5, 6), whilst by conventional procedures $L*W$ would be considered statistically significant also for total poritids ($\alpha=0.018$; Table 6). The result for *T. derasa* must be considered with caution, however, since it was not reproduced in analyses of site means or medians. We cannot infer whether this discrepancy arose because of violations of analytical assumptions or because in analysing means we failed to partition out variance associated with the interaction between sites and transect widths.

For *C. trifasciatus*, estimated density decreased with increasing transect width for 60m and 100m long transects, but estimates differed significantly ($\alpha<\alpha_c=0.022$) only for 100m long transects

(4m>12m≈20m; Fig. 5). For 20m long transects, the estimate from the narrowest transects (4m) was considerably less than those from the 12m and 20m wide transects, though only the difference between 4m and 20m widths was statistically significant (Fig. 5). Estimates from transects 12m wide decreased with increasing transect length, whilst those from the 60mx4m and 100mx4m transects were similar, but both were considerably greater than estimates from the 20mx4m transects (Fig. 5). In summary, if the 20mx4m transect was disregarded, the use of narrower and shorter transects resulted in higher estimates of density of *C. trifasciatus* than did longer and wider transects.

Figure 5: Effects of transect width on estimated population densities (individuals/100m²) of *C. trifasciatus* and *T. derasa* at each transect length. Error bars are standard errors.



The L*W interaction for *T. derasa* may have arisen because the estimate from 60mx4m transects was considerably greater than estimates from all other transects (Fig. 5). Estimates did not differ significantly or consistently with transect width for either 20m long or 100m long transects, nor with transect length for 12m or 20m wide transects (Fig., 7). Again, it seems unlikely that either transect length or width had general effects on our counts of *T. derasa*.

Small Transects - Univariate Analyses

Results of univariate ANOVA for data from small transects are summarised in Table 8, with details being given in Table 9. The main effect of site was large and significant (by both criteria) for all species except *L. dimidiatus*, but *L. dimidiatus* was the only species for which interactions between site and transect length and width (and pass) were significant (Tables 8, 9). Indeed, for no other species were any effects of transect length or width considered significant by the scalable decision criterion we adopted when raw density data were analysed (Tables 8, 9). Pass effects were significant for *C. talboti* only, whilst the Pass*site interaction was significant for *P. moluccensis*.

Table 8: Summary of results for univariate ANOVA of data from small transects. Table 8A summarises results from ANOVA done to resolve which of the potentially important effects identified in MANOVA were significant for which species. Table 8B summarises results for those taxa not included in MANOVA and for which univariate analyses were of the full L*W*P*s model. Effects considered to be statistically significant are indicated by # for the variable α_c decision rule, and by * for a conventional criterion of $\alpha_c=0.04$. Non-significant effects are indicted by '-'. Lightly shaded terms are those which would potentially restrict conclusions about effects of transect dimensions to data from the first pass; heavily shaded terms are those that would potentially undermine any generalisations about effects of transect dimensions.

A.

SPECIES	SOURCE OF VARIATION								
	Length	Width	L*W	W*P	W*s	Pass	site	P*s	
<i>A. curacao</i>	- *	- *	- -	- -	- -	- -	# *	- -	
<i>P. moluccensis</i>	- -	- -	- -	- -	- -	- -	# *	# *	
<i>P. nigroris</i>	- -	- -	- -	- -	- -	- -	# *	- -	
<i>T. lunare</i>	- -	- -	- -	- -	- -	- -	# *	- -	

B.

SOURCE OF VARIATION	SPECIES	
	<i>Chrysiptera talboti</i>	<i>Labroides dimidiatus</i>
Length	- -	- -
Width	- -	# *
L*W	- -	- -
L*P	- -	# -
W*P	- -	- -
L*W*P	- -	- -
L*s	- -	# -
W*s	- -	- -
L*W*s	- -	# -
L*P*s	- -	- -
W*P*s	- -	- -
L*W*P*s	- -	# *
Pass	# *	- -
site	# *	- -
P*s	- -	# -

The significant main effect of Pass for *C. talboti*, ($\alpha<\alpha_c=0.047$) arose because estimated density from all transects on the first pass over the sites (2.4 fish/10m²) was nearly twice that estimated on the following passes (1.3 & 1.04 fish/m²). The only significant pass effects for *P. moluccensis* occurred at sites 1 and 4, and in both cases estimates from the second pass were greater than those from the first. This and the very site-attached character of *P. moluccensis* (Mapstone 1988) render it unlikely that such effects represented responses to diver activity.

The four-way interaction for *L. dimidiatus* arose because: (i) considerably greater estimates arose from 10mx1m transects than from all other transects for the first and third passes at site 5; and (ii) zero estimates resulted from transects of different dimensions in different passes at each site. There was no clear, consistent change in density across transects between successive passes. In summary, there was no evidence of pass effects or transect length or width effects that were consistent over the two sites for which raw data were considered for *L. dimidiatus*.

Table 9: Results of univariate ANOVA of observed density of separate species counted within small transects. Only those terms which were not pooled are shown, and tabulated statistics are those derived after pooling trivial higher order effects. Columns headed $\alpha_{c=\beta}$ give the critical Type I error rate for testing H_0 by the proposed flexible decision rule. This value is also the estimated probability of Type II error when H_0 is not rejected. Effects considered to be statistically significant are indicated by # for the variable α_c decision rule, and by * for a conventional criterion of $\alpha_c=0.04$. Non-significant effects are indicted by '-'. Columns headed β_{04} give the estimated probability of Type II error when H_0 was not rejected against critical Type I error rates of $\alpha_c=0.04$. See pages 14-15 for discussion of effect sizes used in estimation of β . Lightly shaded terms are those which would potentially restrict conclusions about effects of transect dimensions to data from the first pass; heavily shaded terms are those that would potentially undermine any generalisations about effects of transect dimensions.

SPECIES	SOURCE	df	F	α	$\alpha_{c=\beta}$	Infer	β_{04}
<i>A. curacao</i>	Length	2,144	4.387	0.014	0.003	- *	
	Width	2,144	4.351	0.015	0.003	- *	
	W*P	2,144	2.732	0.068	0.014	- -	0.003
	site	5,144	6.410	0.000	0.017	# *	
	P*s	5,144	2.333	0.045	0.031	- -	
<i>P. moluccensis</i>	site	5,144	5.431	0.000	0.008	# *	
	P*s	5,144	4.724	0.001	0.016	# *	
<i>P. nigroris</i>	site	5,144	6.796	0.000	0.027	# *	
	P*s	5,144	2.258	0.052	0.046	- -	0.044
<i>T. lunare</i>	site	5,149	3.425	0.006	0.035	# *	
<i>C. talboti</i> ⁵	W*P	4,50	1.462	0.228	0.147	- -	0.347
	W*s	2,38	1.989	0.151	0.130	- -	0.202
	Pass	2,58	5.828	0.005	0.047	# *	
	site	1,42	13.964	0.001	0.208	# *	
<i>L. dimidiatus</i> ⁵	Width	2,25	5.119	0.014	0.055	# *	
	L*P	4,25	2.442	0.073	0.148	# -	0.366
	L*s	2,24	3.064	0.065	0.124	# -	0.191
	L*W*s	4,22	2.885	0.046	0.073	# -	0.100
	W*P*s	4,18	2.181	0.112	0.078	- -	0.113
	L*W*P*s	8,18	2.683	0.039	0.051	# *	
	P*s	2,24	3.321	0.053	0.131	# -	0.209

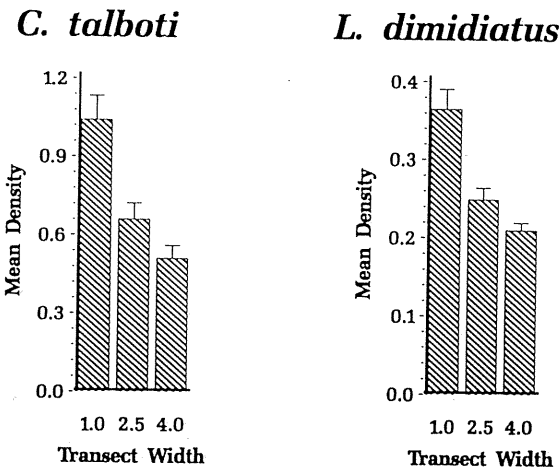
⁵ Data from sites 5 & 6 only (Table 1).

When data for *C. talboti* and *L. dimidiatus* from transects of each size were averaged at each site and compared across sites, effects of transect width were the only non-trivial effects on estimated densities (Table 30). Estimated density declined with increasing transect width, with the greatest change being between transects of 1.0m and 2.5m width (Fig. 6). As with the lutjanids, however, these results should be accepted with caution because of the relatively high likelihood of them arising under H_0 ($\alpha=0.294$, $\alpha=0.185$ for *C. talboti* and *L. dimidiatus* respectively).

Table 10: Results of ANOVA comparing effects of transect length and width on mean estimated densities (fish/10m²) of *C. talboti* and *L. dimidiatus*. Raw data were the averages of estimated densities from all transects of each size at each site. Columns headed $\alpha_{c=\beta}$ give the critical Type I error rate for testing H_0 by the flexible decision rules. This value is also the estimated probability of Type II error when H_0 is not rejected. Effects considered to be statistically significant are indicated by # for the variable α_c decision rule, and by * for a conventional criterion of $\alpha_c=0.05$. Columns headed β_{05} give the estimated probability of Type II error when H_0 was not rejected against critical Type I error rates of $\alpha_c=0.05$. See pages 14-15 for discussion of effect sizes used in estimation of β .

SPECIES	SOURCE	df	F	α	$\alpha_c=\beta$	Infer	β_{05}
<i>C. talboti</i>	Width	2,45	1.257	0.294	0.379	# -	0.817
<i>L. dimidiatus</i>	Width	2,45	1.753	0.185	0.390	# -	0.689

Figure 6: Mean estimates of densities (fish/10m²) of *C. talboti* and *L. dimidiatus* from transects of each width (m), averaged over all sites, lengths and passes. Error bars are standard errors.



Analyses of Auxiliary Variables

Migration of Fish Across Transect Margins

Comparison of mean net migration (fish/10m of transect margin) of all chaetodons to or from transects of different sizes indicated that emigration of fish almost always exceeded immigration, though not significantly so for 12m and 20m wide transects (Table 11). Net migration (emigration) from 4m wide transects was consistently greater than from the wider transects ($F_{2,171}=6.92, \alpha=0.001$).

When immigration and emigration were analysed separately, it was found that the rate of **immigration** of chaetodons to 4m wide transects was **less** than to 12m and 20m wide transects, and that their rate of **emigration** from the narrow transects was **greater** than that from the wider transects (Table 11). Immigration and emigration across the boundaries of 12m and 20m wide transects roughly balanced and were independent of transect width (Table 11). The expected effect of these migration rates on the true densities of fish in transects is shown in Figure 7.

Table 11: Net migration of chaetodons and *Plectropomus* spp (fish/10m of transect margin) across the margins of transects of each width (chaetodons) and length and width (*Plectropomus* spp). Estimates of immigration and emigration of fish to or from transects are presented as positive and negative values respectively, to indicate their effect on true density. Means preceded by * were unlikely to have arisen when true migration was zero ($\alpha<0.02$ in all cases). Data were averaged over all sites. SE: Standard Error.

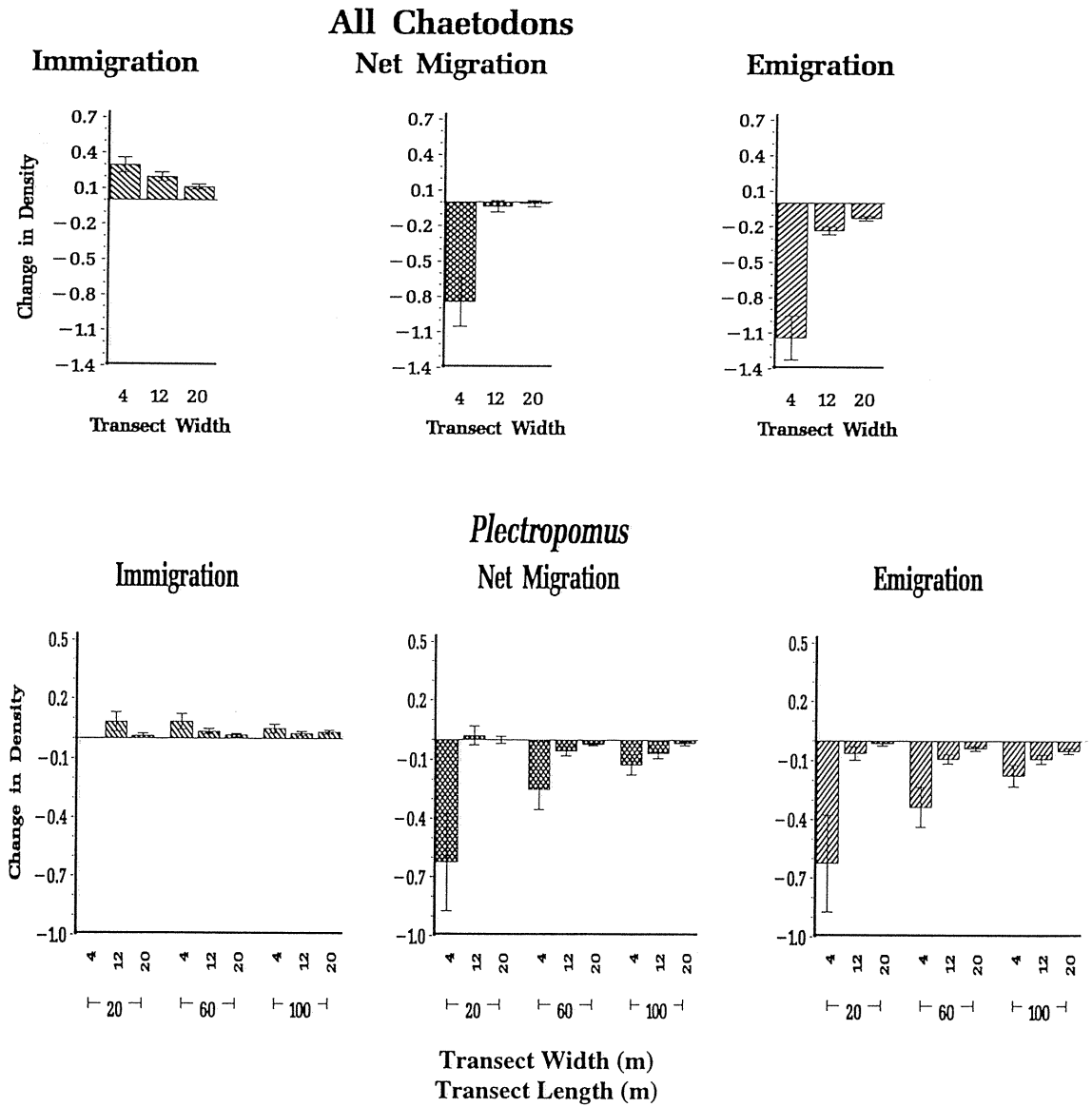
Chaetodons							
	TRANSECT WIDTH (m)	Immigration		Emigration		Net Migration	
		Mean	SE	Mean	SE	Mean	SE
	4.0	*0.12	0.03	*-0.47	0.07	*-0.34	0.08
	12.0	*0.24	0.04	*-0.28	0.04	-0.04	0.06
	20.0	*0.23	0.04	*-0.25	0.04	-0.03	0.05

<i>Plectropomus</i>		TRANSECT LENGTH (m)					
	TRANSECT WIDTH (m)	20		60		100	
		Mean	SE	Mean	SE	Mean	SE
Immigration	4.0	0.00	0.00	*0.03	0.02	*0.02	0.01
	12.0	0.10	0.06	*0.04	0.02	*0.03	0.01
	20.0	0.03	0.03	0.03	0.02	*0.07	0.02
Emigration	4.0	*-0.25	0.10	*-0.13	0.04	*-0.07	0.02
	12.0	-0.08	0.04	*-0.11	0.03	*-0.11	0.03
	20.0	-0.03	0.03	*-0.08	0.02	*-0.10	0.03
Net Migration	4.0	*-0.25	0.10	*-0.10	0.04	*-0.05	0.02
	12.0	0.03	0.06	*-0.07	0.03	*-0.08	0.03
	20.0	0.00	0.04	*-0.04	0.02	-0.04	0.02

For coral trout, differences in emigration and net migration with transect width varied across transect lengths (emigration: $F_{4,171}=2.57$, $\alpha=0.040$; migration: $F_{4,171}=3.38$, $\alpha=0.011$). Both emigration and net migration from the 4m wide transects exceeded those from wider transects on 20m long transects, but not on 60m or 100m long transects (Table 11). Immigration did not vary significantly with either transect length or width ($\alpha>0.15$ for all terms), but emigration at least slightly exceeded immigration for most transect sizes (Table 11). The expected effects of these movements of coral trout on estimates of density from transects are shown in Figure 7.

Clearly, the apparent effects of transect width on estimates of population density could not have arisen simply because the observer failed to count fish that exited the transects during counting, since that source of bias would tend to favour lower estimates from narrow transects than from wider transects - the reverse of what was observed.

Figure 7: Immigration, emigration, and net migration of all chaetodons and *Plectropomus* spp adjusted for transect area. Thus, entries and exits of fish to or from transects are presented in terms of the expected change in density they would precipitate. Error bars are standard errors.



Estimated Densities After Correction for Migration

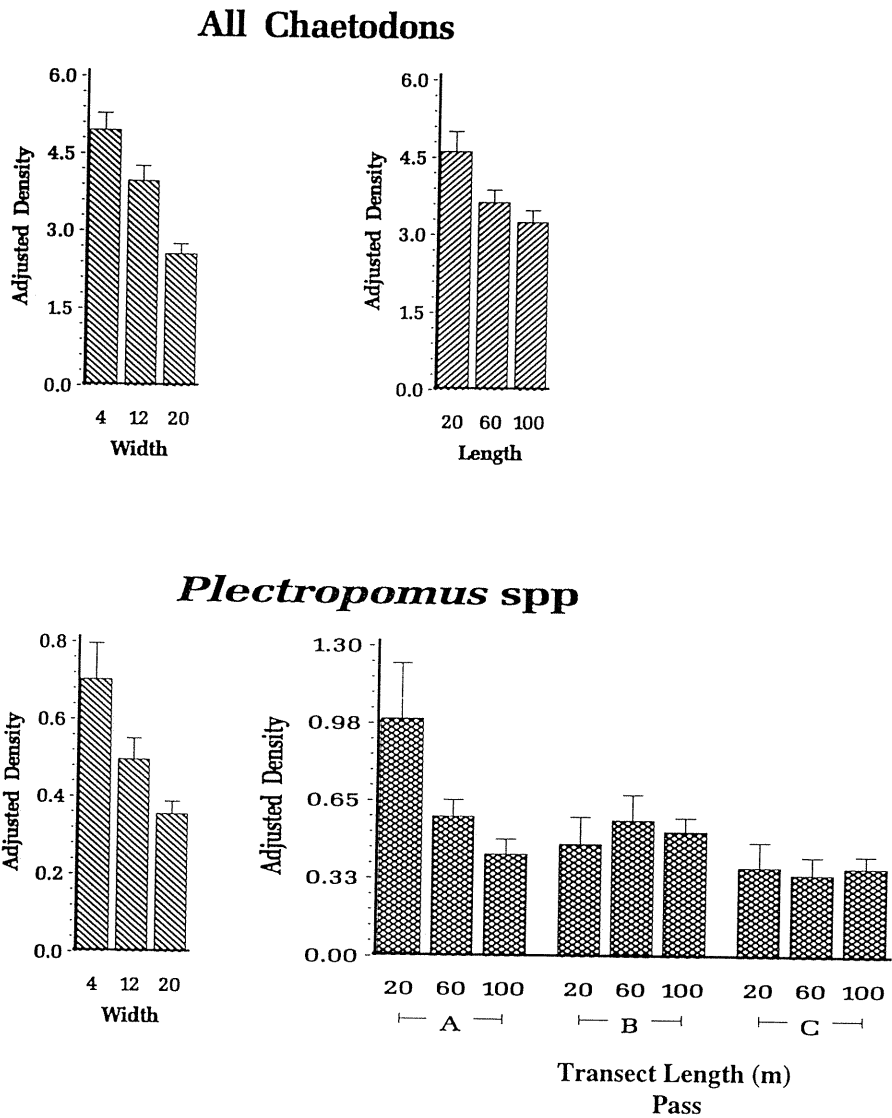
Bias in estimates consistent with what we observed may have arisen, however, if both emigrants were counted before they left and immigrants were counted after they entered narrow transects, but neither were counted along wider transects. To examine the effect of this potential source of bias, counts from 4m wide transects were adjusted downward by subtracting counts of fish that entered transects during counting, and counts from the wider transects were adjusted upward by adding to them fish that were observed leaving transects during counting. The adjusted counts were then transformed to estimates of density (fish/100m² as before) and these data re-analysed.

Results of analyses of migration-corrected estimated densities of *Plectropomus* spp and all chaetodons are given in Table 12 and Figure 8. For both groups, the results generally parallel those for unadjusted densities, indicating that whatever biases might have arisen because of migration of fish across transect boundaries, they did not precipitate the pattern in bias of estimates associated with transect dimensions. Shorter and narrower transects still produced significantly greater estimates of density than longer and wider transects, though for *Plectropomus* the effect of transect length was marked only on the first pass over a site (Fig. 8).

Table 12: Results of analyses of estimated densities after adjusting for observed migration to/from transects and potential transect width-related biases in counting those migrants. Columns headed $\alpha_{c=\beta}$ give the critical Type I error rate for testing H_0 by the proposed flexible decision rule. This value is also the estimated probability of Type II error when H_0 is not rejected. Effects considered to be statistically significant are indicated by # for the variable α_c decision rule, and by * for a conventional criterion of $\alpha_c=0.05$. Columns headed β_{05} give the estimated probability of Type II error when H_0 was not rejected against critical Type I error rates of $\alpha_c=0.05$. See pages 14-15 for discussion of effect sizes used in estimation of β .

SPECIES	SOURCE	df	F	α	$\alpha_{c=\beta}$	Infer	β_{05}
<i>Plectropomus</i>	Width	2,138	9.094	0.000	0.003	# *	
	L*P	4,134	3.273	0.014	0.026	# *	
	L*W*P	8,130	1.431	0.190	0.103	- -	0.180
	Pass	2,138	6.869	0.001	0.003	# *	
	site	5,130	3.426	0.006	0.018	# *	
All Chaetodons	Length	2,106	12.096	0.000	0.000	# *	
	Width	2,10	13.347	0.002	0.003	# *	
	W*s	10,80	2.482	0.012	0.000	- *	
	L*W*s	20,80	1.539	0.091	0.000	- -	0.000
	site	5,90	12.220	0.000	0.001	# *	
	P*s	10,80	3.005	0.003	0.000	- *	

Figure 8: Estimated mean population densities (fish/100m²) of all chaetodons and *Plectropomus* spp after adjusting for potential width-related biases in treatment of fish migrating across transect boundaries. For chaetodons, density estimates varied significantly with transects width and length independently of any other effects. For *Plectropomus* spp, the effect of transect width on estimated density was independent of all other effects, but that of length depended on the pass when data were collected. Error bars are standard errors.



Variations in Counting Times

Mean search intensity (mins/100m²) decreased with increasing transect length and width (Table 13). Search intensity of large transects declined by about one-third with the maximum five-fold increase in transect length, but declined by about two-thirds over a five-fold increase in transect width. This pattern was common to counts of both fish and benthos, and a similar pattern was apparent for counts of fish on small transects (Table 13).

Table 13: Mean search intensities (minutes/100m²) derived from observed times to survey transects of different lengths and widths. Calculations were based on times taken by observer 1 unless he had not recorded his time for a transect, in which case the time taken by observer 2 was substituted. Times taken to survey transects generally differed little between observer 1 and 2. Data were averaged over all transects of each size (n=20).

A: Large Transects

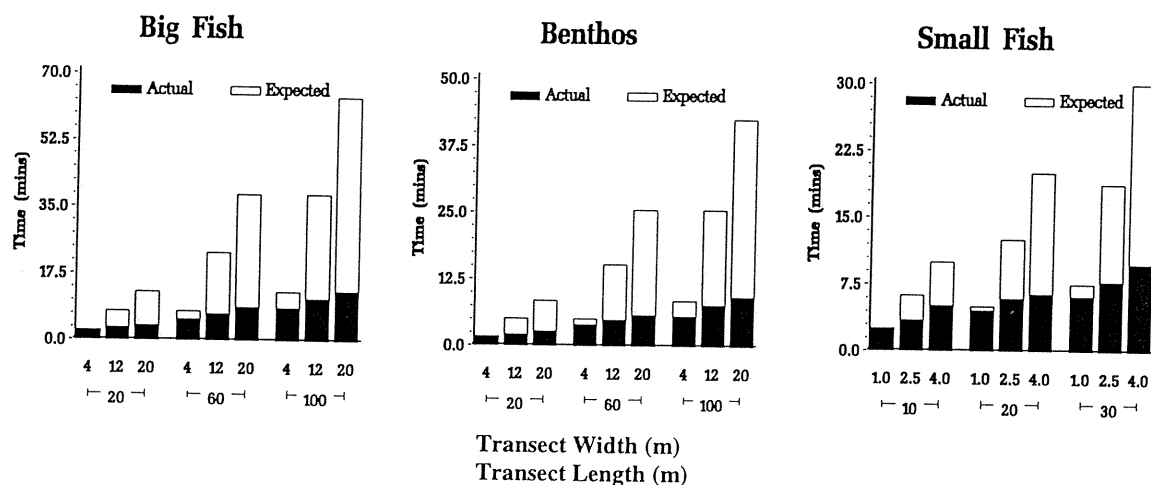
TAXA	WIDTH (m)	LENGTH (m)					
		20		60		100	
		Mean	SE	Mean	SE	Mean	SE
Big Fish	4.0	3.19	0.18	2.31	0.10	2.14	0.06
	12.0	1.35	0.06	0.96	0.04	0.92	0.04
	20.0	0.96	0.07	0.73	0.03	0.65	0.03
Benthos	4.0	2.13	0.16	1.67	0.10	1.40	0.06
	12.0	0.88	0.06	0.68	0.04	0.64	0.04
	20.0	0.69	0.06	0.48	0.04	0.47	0.05

B: Small Transects

TAXA	WIDTH (m)	LENGTH (m)					
		10		20		30	
		Mean	SE	Mean	SE	Mean	SE
Small Fish	1.0	25.00	2.40	22.75	1.45	22.50	1.60
	2.5	13.80	1.20	11.80	0.90	10.40	0.61
	4.0	12.75	2.20	8.00	0.66	8.17	0.66

Comparing the expected times required to search transects with uniform intensity (constant time/area) with the observed times emphasised the degree to which larger transects were searched relatively more rapidly and less intensively than smaller transects (Fig. 9). The discrepancy between actual and expected search times increased dramatically with transect width, but the width-related discrepancies were exacerbated by increasing transect length.

Figure 9: Average observed times (minutes) to survey transects of each size, and differences between those and the times expected to be taken if all transects were surveyed at the same rate (min/100m²) as the smallest transect (see Table 13). Note that the white portions of each bar represent the additional time expected to be required to ensure that all transects were surveyed with equal intensity. Hence, the total height of each bar is the total time that would be expected to be taken.



Hence, the potential existed for both length and width effects on estimated density to arise simply because of reduction in search intensity with increasing transect lengths and widths. This was likely only for data from the large transects, however, since estimates generally did not vary with the sizes of small transect. Multivariate ANOVA of estimated densities scaled to those expected from searches of standard intensity indicated that only the effects of transect length, width, and their interaction, and the effects of pass, site and their interaction were non-trivial ($\alpha < 0.25$; all other $F < 1$). Subsequent univariate ANOVA of the reduced model including only those terms, indicated only that time-corrected densities were strongly affected by transect width for all taxa except *C. baronessa* and *C. aureofasciatus*. The effects of transect width were the reverse of those discussed above, however: estimates **increased** with increasing transect width. In all cases, estimates from 4m wide transects were smaller than those from 12m and 20m wide transects, which generally did not differ significantly. Thus, transect size-related bias in search times clearly had the potential to account for the observed effects of transect dimensions on estimated population densities.

Effects of the Depth of Transects

Correlations between estimated densities and transect depths were relatively common for the pomacentrids and labrids surveyed on small transects. Five of the six species analysed showed significant density-depth correlations. Moreover, such correlations were considered significant at three or more sites for three of these taxa (*P. nigrororus*, 3 sites; *P. moluccensis* & *A. curacao*, 4 sites). Density-depth correlations were calculated for *L. dimidiatus* and *C. talboti* at only two sites, and each species showed a significant correlation at one of those sites. The density-depth correlations were not consistently positive or negative at all sites for most species. The single exception was *A. curacao*, for which density was always positively correlated with transect depth. Residuals after fitting the Pass*Length*Width model at each site were also significantly correlated with depth for most instances of significant density-depth correlation, suggesting that depth-related trends in density were unlikely to be confounded with terms of interest in the model. This was not

the case, however, at one site for *P. nigroris*, *P. moluccensis*, and *L. dimidiatus*, and at 2 sites for *A. curacao*. Thus, at one site for most species there existed the potential for spurious inferences of transect width effects because of confounding with depth-related changes in density. Given the absence of strong effects of transect dimensions for counts of these fishes, however, this potential clearly was not realised.

Estimated densities of most taxa surveyed in large transects were correlated significantly ($\alpha < 0.1$) with the depths of transects at one site, but only *C. rainfordi*, *C. baronessa*, total chaetodons, and *Plectropomus* spp showed significant correlations at two sites ($\alpha < 0.05$ in all cases). For no taxa were density-depth correlations significant or larger than ± 0.3 at more than two of the six sites. Correlations of estimated density with depth were negative at both sites for *Plectropomus* spp ($r = -0.329$, $r = -0.366$, $n = 36$), total chaetodons ($r = -0.622$, $n = 27$; $r = -0.354$, $n = 36$), and *C. baronessa* ($r = -0.666$, $r = -0.637$, $n = 36$), but were of opposite signs at the two sites for *C. rainfordi* ($r = 0.624$, $r = -0.480$, $n = 27$). The frequency of significant density-depth correlations apparently was also site dependent. No significant density-depth correlations occurred at site 2 (Bowden Reef, front-reef slope) and only one occurred at site 3 (Davies Reef, back-reef bommies), whereas three or four density-depth correlations were significant at each of the other sites.

Ignoring the magnitude of correlation coefficients, their signs varied among sites for all taxa, although negative coefficients were more common than positive coefficients. In several cases where estimated density was correlated significantly with transect depth, residuals after fitting the Pass*Length*Width model were not correlated significantly with depth, suggesting that effects of one or more factors of interest (Length/Width of transects) might have been confounded with depth related gradients in density. This was the cases for *T. derasa*, medium poritids, all poritids, *C. trifasciatus*, *Plectropomus* spp (each at one site only), and *C. rainfordi* (at both sites).

Average depths of the centres of transects varied by only one metre with transect width, but the 4m and 12m wide transects had a greater range in average depth than the 20m wide transects (Table 14). The depth distribution of the narrow and deep margins of transects suggested that wider transects extended into deeper water from similar starting depths (as expected). The average depth of the shallow margin of narrow transects was greater than the depths of the shallow edges of the wider transects, and the depths of the deep margins of the transects increased with transect width on average (Table 14). From these data, it seems unlikely that the observed effects of transect width on estimates could have arisen because of the depth characteristics of the (randomly placed) transects, unless very strong negative correlations of density with depth were common. Few such strong effects were observed, however, and those that existed were not consistent over sites, whereas effects of transect width were consistent over sites.

Estimated densities of coral trout, *C. rainfordi*, *C. trifasciatus*, other chaetodon spp, and all chaetodons were consistently greater for the shallower halves of transects than for the deeper halves of transects at most sites. These differences, however, were not consistently related to transect width (Table 15). If the effects of transect width were products of depth-related trends in density, the differences between shallow and deep half-transects would have to be substantially greater for the wider transects than for the narrowest transects. Clearly, this was not the case (Table 15).

Finally, when densities estimated from only the narrower halves of the wider transects were compared with those from the full width of the 4m wide transects, the effect of transect width was again present, and was consistent with the results of the earlier analyses. Thus, these results do not suggest that the effects of transect width might have been a product of differences in the depths of transects, but nor do they categorically refute that hypothesis.

Table 14: Mean depths of transects of each width. Data for **TRANSECT CENTRES** were derived by averaging depth data from the four corners of each transect. Equipment failure meant that this was possible from transects from three sites only. Data for **SHALLOW MARGINS** and **DEEP MARGINS** were derived by averaging depth data for the beginning and end of each margin. Data for each margin from transects of all sizes were available at all sites, but data from both margins of any given transect were not available at sites 1-3 (because of equipment failure). Data from site 1 were not included because transcription errors rendered it uncertain as to which margin the data from each transect referred.

WIDTH (m)	TRANSECT DEPTH (m)								
	TRANSECT CENTRE			SHALLOW MARGIN			DEEP MARGIN		
	Mean	Min.	Max	Mean	Min	Max	Mean	Min	Max
4.0	8.4	2.5	15.8	7.1	2.0	15.0	9.1	2.0	16.5
12.0	8.6	2.0	14.8	5.3	2.0	12.0	11.1	1.0	17.5
20.0	9.4	3.8	13.8	5.6	3.0	15.0	12.5	4.0	19.5

Table 15: Mean differences between the densities of some fish in shallower and deeper halves of transects of each length and width. Densities were estimated after counting from the mid-line of transects by observer 2 (see methods). Data were from all sites.

SPECIES	TRANSECT WIDTH (m)	TRANSECT LENGTH (m)					
		20		60		100	
		Mean	SE	Mean	SE	Mean	SE
<i>All Chaetodons</i>	4.0	1.25	0.97	2.29	0.63	0.63	0.54
	12.0	1.46	0.47	0.20	0.38	0.41	0.30
	20.0	1.43	0.44	0.82	0.25	0.48	0.18
<i>C. rainfordi</i>	4.0	0.83	0.47	0.56	0.30	0.08	0.31
	12.0	0.28	0.31	-0.12	0.20	0.07	0.24
	20.0	0.29	0.17	-0.00	0.12	0.07	0.09
<i>C. trifasciatus</i>	4.0	0.00	0.31	0.07	0.07	0.21	0.29
	12.0	0.14	0.17	0.07	0.19	-0.03	0.09
	20.0	0.21	0.24	0.00	0.05	0.03	0.03
Misc. <i>C. spp</i>	4.0	0.00	1.15	0.70	0.29	0.25	0.45
	12.0	0.21	0.29	-0.26	0.23	0.04	0.18
	20.0	0.29	0.37	0.25	0.14	0.24	0.18
<i>Plectropomus</i>	4.0	0.38	0.33	-0.04	0.21	0.05	0.17
	12.0	0.33	0.19	0.08	0.06	0.02	0.07
	20.0	0.03	0.12	0.07	0.04	0.02	0.03

Effects of the Time-of-Day of Sampling

Estimated density was correlated significantly with the time of day at which transects were surveyed at one site only for *T. derasa*, small poritids, total poritids, *C. baronessa*, *C. aureofasciatus*, *T. lunare*, and *P. moluccensis*, and at two out of six sites for medium poritids, *A. planci*, *P. nigroris*, *C. talboti*, and *A. curacao*. The signs of the correlations were not consistent either within species (where significant at 2 sites), or among species for which density-time correlations were significant at the same site. In all cases the correlation was reduced to near zero after removal of Pass, Length and Width effects. These characteristics suggested that the correlations of raw densities with time of survey were unlikely to be simply time-of-day effects, and may have represented effects potentially confounded with the interaction of pass, length and width, though at only one or two sites. Given that transects of each length and each width were surveyed over the entire range of times of the day, and that a pass over a site took approximately half a day for the small transects and a whole day for the large transects, it seems unlikely that time-of-day or related effects would be confounded with the main effects of pass, length, or width.

Effects of Observer and Methods of Surveying Large Transects

Comparisons of Observers

In all but two comparisons of observers, observer 1 (AA) counted more fish on average than observer 2 (RB) (Table 16, Figs. 10, 11). The likelihood of such bias arising by chance was low in each trial (Table 16). Despite this consistent direction of differences, species specific comparisons of estimates from the two observers for zig-zag counts, sequential counts, and parallel counts generally indicated that differences between observers were not significantly different from zero. These tests were typically of low power, however, because of low replication of transects and/or the stringent critical Type I error rates required for each comparison in order to keep experimentwise error rates to 0.1 or less.

When observers were compared on the basis of their counts of small fishes, observer 1 was again found to produce higher estimates on most transects. Differences in estimates between the observers varied significantly with site and most interactions between site and pass, transect length, and transect width (MANOVA, $\alpha < 0.1$). There was no discernible consistency in these differences, however, other than that means from observer 1 were always the greater. Thus, in view of the consistent direction of observer bias for counts of both small and large fish, we inferred that the observers did not count fish equally well.

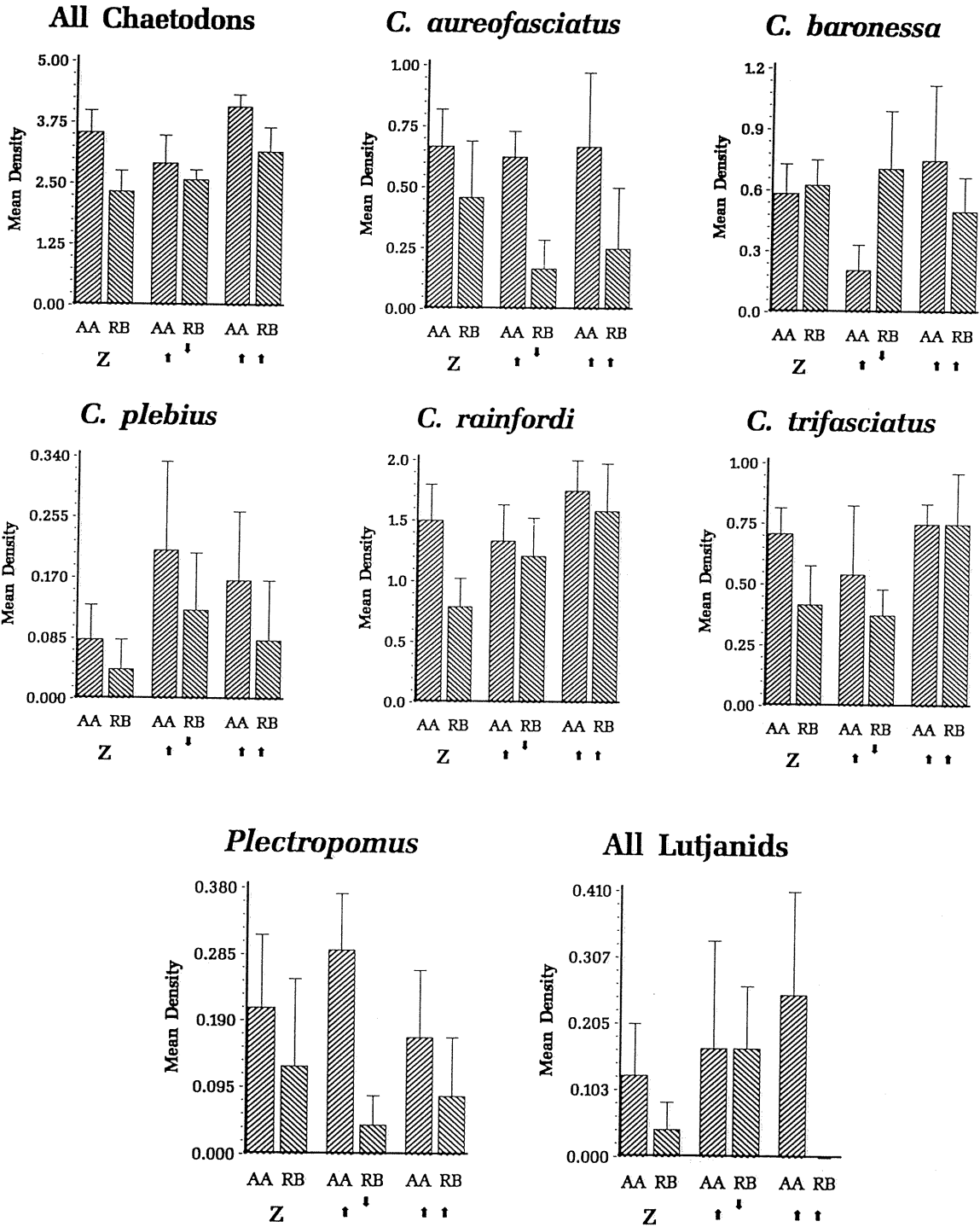
Table 16: Results of pairwise comparisons between counts from observer 1 (AA) and observer 2 (RB). All comparisons were based only on data collected by the same methods by each observer. The total number of comparisons was # counts * # species in each case. α = the probability of observed outcome or one more extremely biased if the probability that either observer had the greater count in a single trial was 0.5.

METHOD	# SPECIES	# COUNTS	AA>RB	RB≥AA	α
Zig-zag					
Small transects	4	3 ⁶	11	1	0.0002
Large transects	8	3 ⁷	23	1	0.000002
Sequential	8	1	6	2	0.145
Parallel	8	1	7	10	0.035

⁶ Counts made at each of three sites, with independent replication.

⁷ Counts made at three intervals after laying tapes, being repeated measures of same replicates.

Figure 10: Estimated densities (individuals/100m²) of abundant fish surveyed on large transects by three counting methods (Z, $\uparrow\downarrow$, $\uparrow\uparrow$) by each of two observers (AA, RB). Counting methods: Z - search transect in zig-zag pattern over entire width; $\uparrow\downarrow$ - search first one half-width of transect, and then the other half-width; $\uparrow\uparrow$ - two observers simultaneously search half of the total width of the transect in one pass. Hence, density estimates from method three for each observer were derived from counts over only half of the 12m width of the transects. Error bars are standard errors.



Comparisons of Counting Methods

Different methods of surveying transects generally did not significantly effect estimated density. Estimates from transects surveyed by swimming a single zig-zag pass along the transects (as for most of this study) did not differ significantly from those derived from sequential counts or parallel counts. This result was obtained irrespective of which observer's data for zig-zag and sequential counts were used (MANOVA: Observer 1 - $F_{14,16}=0.44$, $\alpha=0.937$; Observer 2 - $F_{14,16}=1.452$, $\alpha=0.304$; Fig. 10). The consistent ranking of observers by data from the transects where both observers surveyed by the same method suggested that comparisons between zig-zag counts (by observer 1) and counts from the midline of transects (by observer 2) were likely to be affected at least partially by observer bias. Since we are unable to separate observer bias from methodological bias with respect to these comparisons, counts from the midlines of transects were not analysed.

Diver activity along transects prior to counting large, wide ranging fish also seemed to have little effect for most species (Fig. 11). Prior activities of divers seemed to introduce positive bias in counts of *C. plebeius*, and a short-term, temporary effect may have occurred for *Plectropomus* spp (Fig. 11). There was no evidence of effects on other species, a result consistent with the absence of differences between sequential counts and other methods for most species (Fig. 10).

Precision and Cost Efficiency of Sampling

Precision from Transects of Different Dimensions

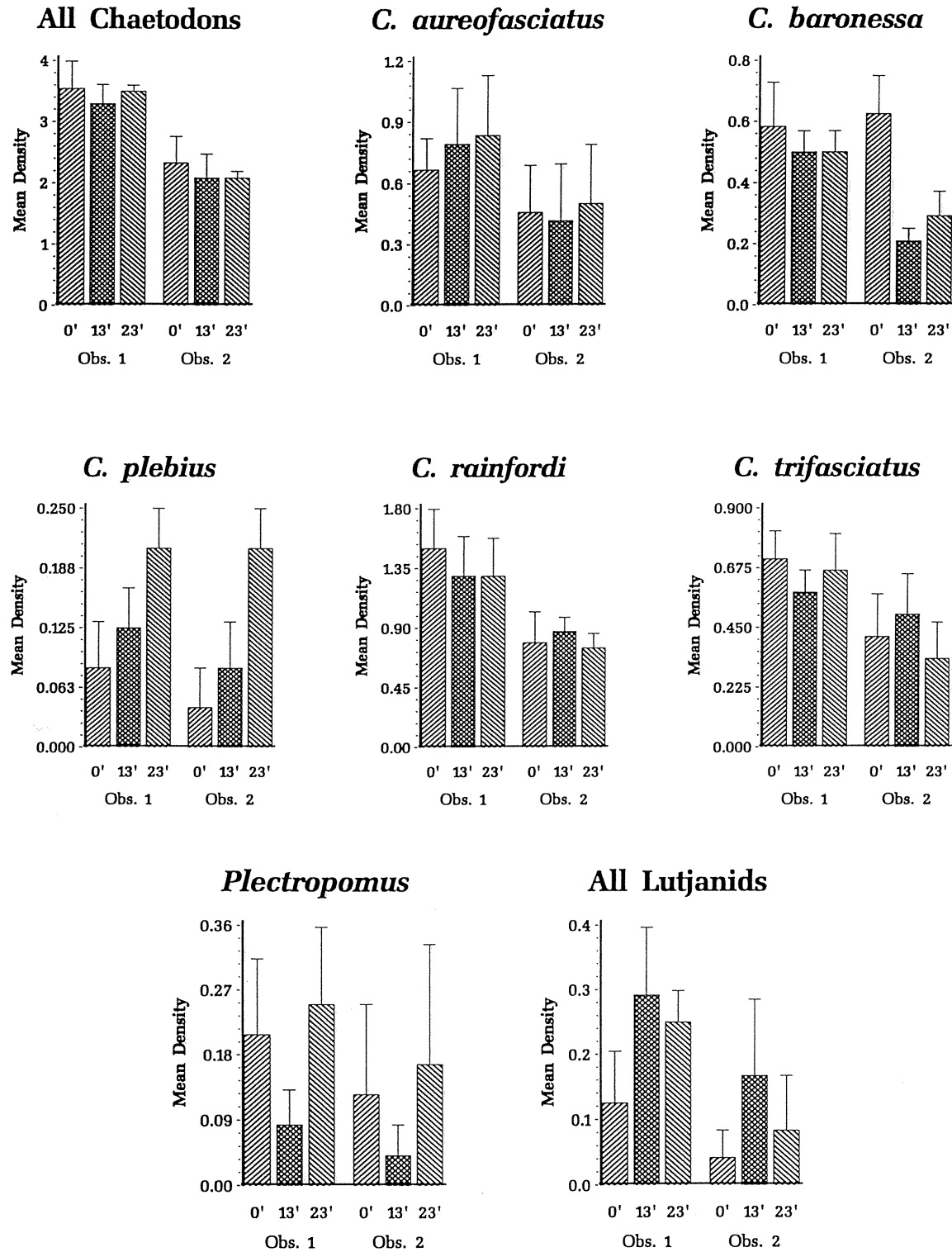
Multivariate ANOVA of the precision of estimates of mean densities indicated significant effects of transect length for large transects, and potentially interesting ($\alpha<0.25$) effects of transect width for large transects and transect length for small transects (Table 17).

Note that in the analysis of data from large transects, *C. trifasciatus* and *T. derasa* were excluded from the MANOVA because together they resulted in reduction of the cell-size for 20mx4m transects to only 1 replicate site. This arose because all counts of those species in the smallest transects were zero at several sites and neither precision nor cost-efficiency could be calculated. SAS, when calculating MANOVA, ignores any record in which any one of the multiple dependent variables has a missing value, with the result that cell size is reduced accordingly for all dependent variables. Thus, data for these two species were analysed by univariate ANOVA, as were data for total chaetodons, total poritids, and small poritids, with the Sidak-adjusted 'significance' criterion being 0.02 for all these tests.

Table 17: Results of MANOVA to test for effects of transect length and width on estimated precision from samples of three replicate transects of each size at each of six sites. F_{PT} refers to the estimated F-value for Pillai's trace.

UNITS	SOURCE	F_{PT}	df	α	Infer
Large Transects	Length	2.403	18,58	0.006	*
	Width	1.404	18,58	0.165	?
	L*W	0.828	36,124	0.739	-
Small Transects	Length	1.483	8,70	0.191	?
	Width	1.294	8,70	0.261	-
	L*W	0.754	16,148	0.754	-

Figure 11: Estimates of population density (fish/100m²) of fish on large transects by each of two observers (Obs1, Obs2) taken concurrent with and at two intervals after laying the tapes to measure out the transects. Average times between laying the tapes and counting were 0, 13, and 23 minutes ('). Error bars are standard errors.



The length of large transects significantly affected the precision of estimates for all taxa except small poritids and *T. derasa* (Univariate ANOVA, Table 18). For all taxa, the precision of estimates improved with increasing transect length (Fig. 12). Where precision varied significantly with transect length, average precision of estimates from 20m long transects was significantly poorer for a given sample size (in this case 3) than that from longer transects for all taxa except *C. rainfordi* and total poritids (Fig. 12). For the latter two taxa, precision of estimates from 20m and 60m transects did not differ significantly, but both produced estimates with worse precision than did 100m long transects (Fig. 12). Precision of estimates from 60m long transects was significantly poorer than that from 100m long transects for *C. aureofasciatus*, *C. rostratus*, *C. trifasciatus*, and large poritids (Fig. 12). For none of the taxa sampled with small transects were effects of transect length or width significant in univariate analyses, as assessed by either a conventional decision criterion or against an adjusted α (Table 18). Nor were any effects of transect length or width non-trivial for *L. dimidiatus* or *A. curacao* when assessed by univariate ANOVA of data from sites 5 and 6 only (all $F < 1$).

Table 18: Univariate ANOVA of the effects of transect length and width on estimates of precision. Results for fish and benthos counted within large transects, and fish counted within small transects are given in tables A., B., and C. respectively. Only terms that were non-trivial in the preliminary MANOVA were included for ANOVA, except for those groups not included in the multivariate analyses. For the latter, only those effects for which $F > 1$ are tabulated. Columns headed $\alpha_{c=\beta}$ give the critical Type I error rate for testing H_0 by the flexible decision rule. This value is also the estimated probability of Type II error when H_0 is not rejected. Effects considered to be statistically significant are indicated by # for the variable α_c decision rule, and by * for a conventional criterion of $\alpha_c = 0.02$. Non-significant effects are indicated by '-'. Columns headed β_{02} give the estimated probability of Type II error when H_0 was not rejected against critical Type I error rates of $\alpha_c = 0.02$.

A: Fish - Large Transects

SPECIES	SOURCE	df	F	α	$\alpha_{c=\beta}$	Infer	β_{02}
<i>Plectropomus</i> spp	Length	2,48	9.855	0.000	0.051	# *	0.110
	Width	2,48	1.453	0.244	0.051	- -	
All Chaetodonts	Length	2,45	3.353	0.044	0.092	# -	0.043
	Width	2,45	2.728	0.076	0.092	# -	
<i>C. aureofasciatus</i>	Length	2,46	6.348	0.004	0.212	# *	0.638
	Width	2,46	3.667	0.033	0.212	# -	
<i>C. baronessa</i>	Length	2,48	6.848	0.002	0.276	# *	0.767
	Width	2,48	2.022	0.143	0.276	# -	
<i>C. rainfordi</i>	Length	2,49	2.074	0.137	0.222	# -	0.659
	Width	2,49	1.653	0.202	0.222	# -	
<i>C. rostratus</i>	Length	2,43	9.872	0.000	0.270	# *	
	Width	2,43	10.779	0.000	0.270	# *	
<i>C. trifasciatus</i>	Length	2,39	2.260	0.118	0.233	# -	0.688
Other <i>C.</i> spp	Length	2,48	4.835	0.012	0.283	# *	

(... continued)

Table 18 (Continued)

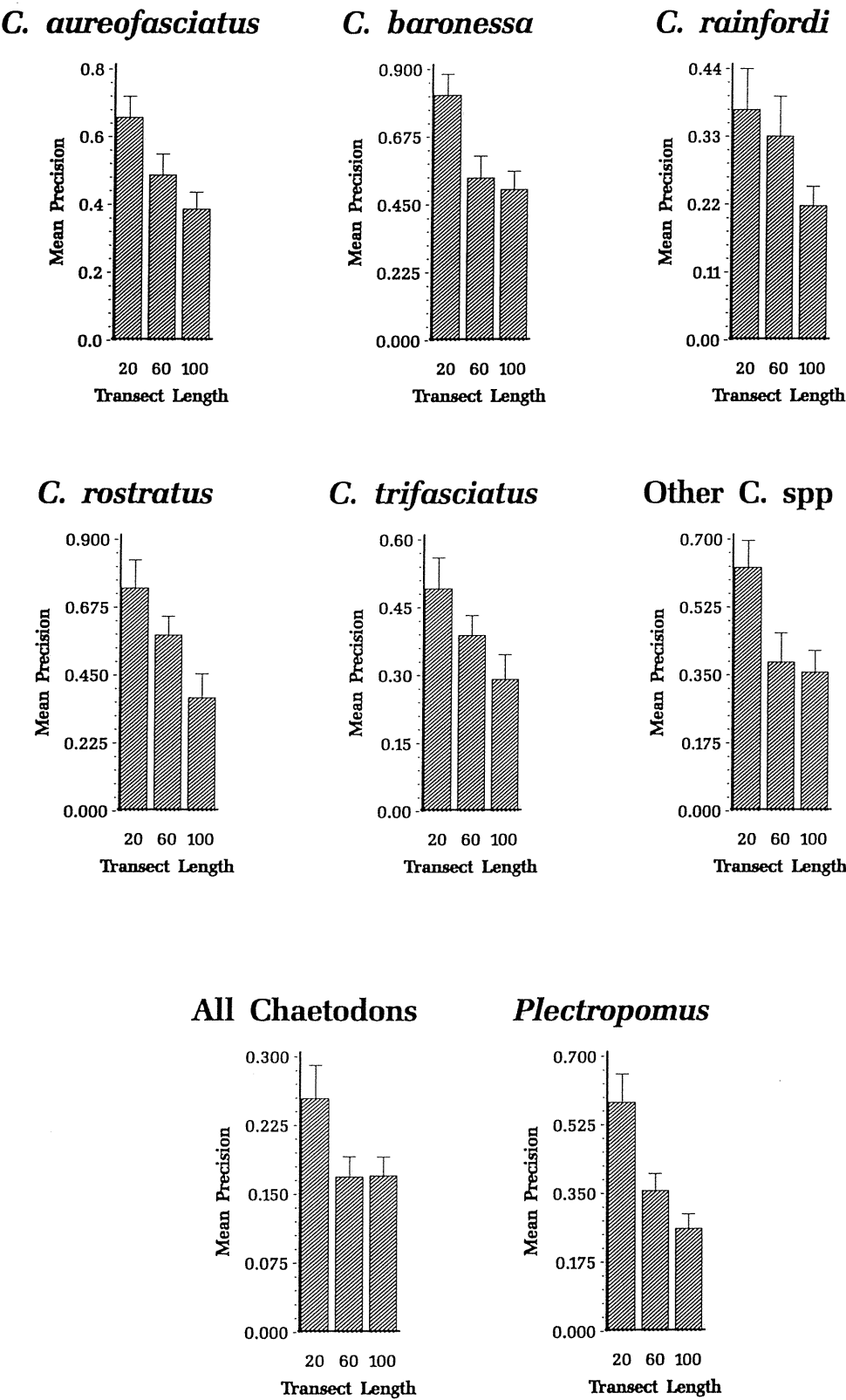
B: Benthos - Large Transects

SPECIES	SOURCE	df	F	α	$\alpha_c=\beta$	Infer	β_{02}
<i>A. planci</i>	Length	2,47	4.282	0.020	0.183	# *	0.561
	Width	2,47	3.180	0.051	0.183	# -	
All Poritids	Length	2,45	6.022	0.005	0.164	# *	0.506
	Width	2,45	2.637	0.083	0.164	# -	
Poritids 1-30cm	Length	2,9	3.404	0.079	0.069	- -	0.233
	Width	2,9	1.241	0.334	0.069	- -	0.233
	L*W	4,9	2.253	0.143	0.040	- -	0.100
Poritids 31-60cm	Length	2,49	4.736	0.013	0.172	# *	0.530
	Width	2,49	1.601	0.212	0.172	- -	
Poritids 61-100cm	Length	2,47	5.882	0.005	0.244	# *	0.707
	Width	2,47	2.005	0.146	0.244	- -	
<i>T. derasa</i>	Length	2,35	1.195	0.315	0.308	- -	0.822
	L*W	4,35	2.192	0.090	0.247	# -	0.732

C: Fish - Small Transects

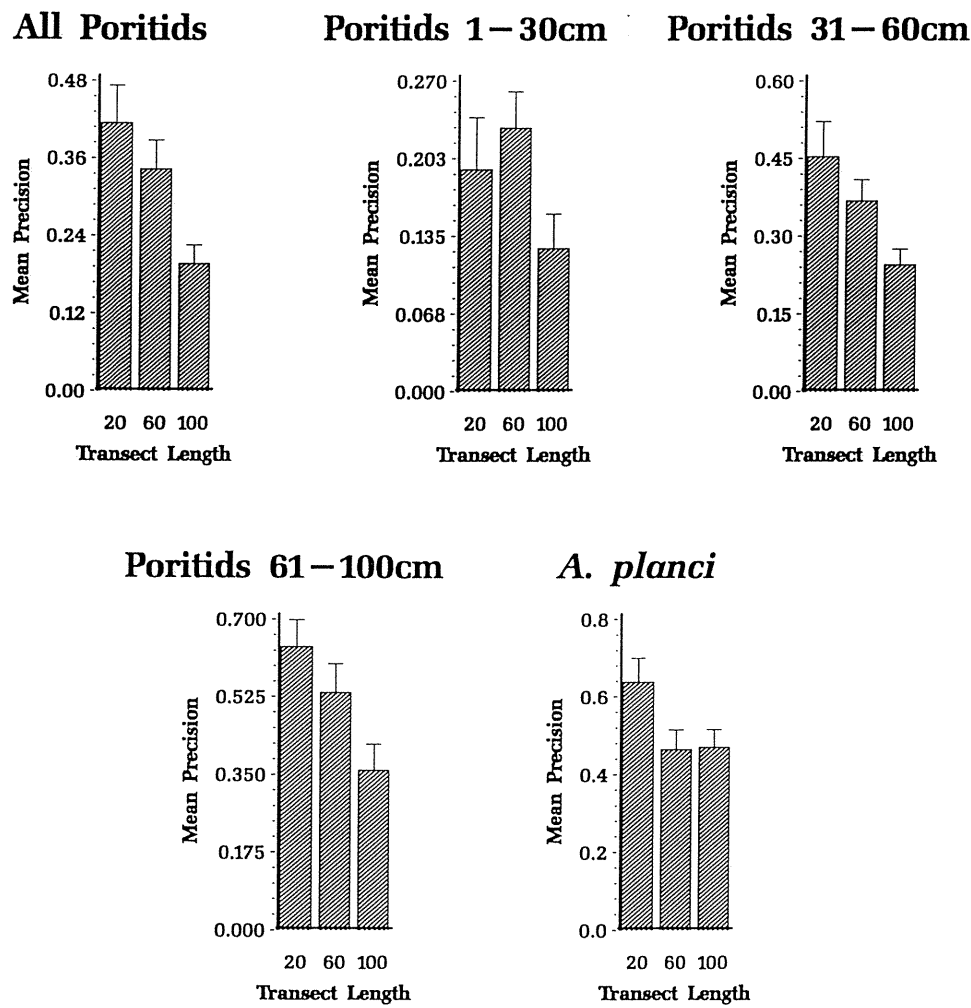
SPECIES	SOURCE	df	F	α	$\alpha_c=\beta$	Infer	β_{05}
<i>P. moluccensis</i>	Length	2,51	1.907	0.159	0.015	- -	0.004
<i>P. nigroris</i>	Length	2,46	2.171	0.126	0.121	- -	0.232

Figure 12: Effects of transect length (m) on mean precision of estimated population densities of taxa counted within large transects. Error bars are standard errors.



(... continued)

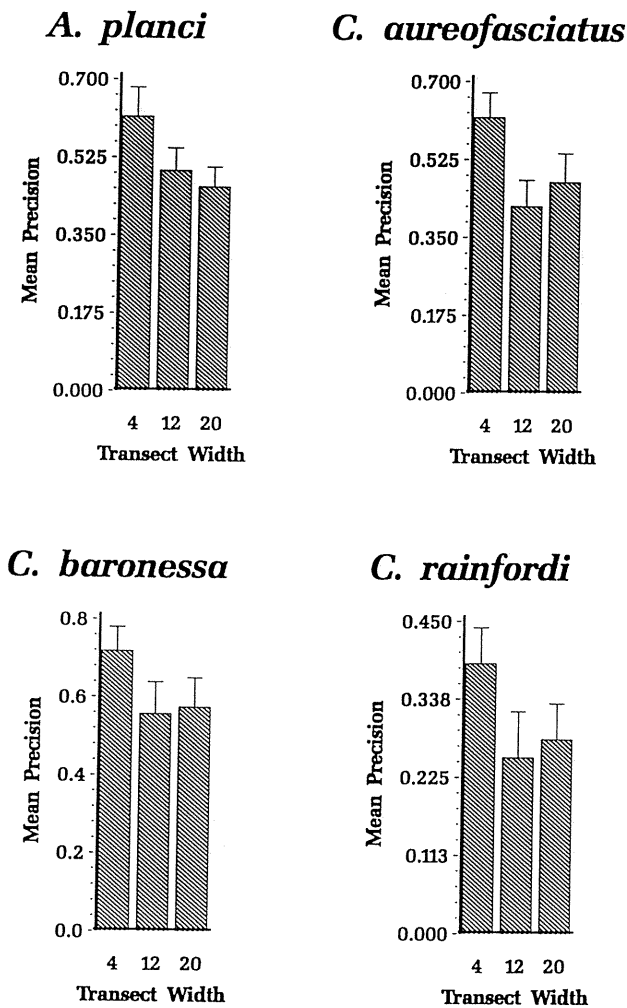
Figure 12 (Continued).



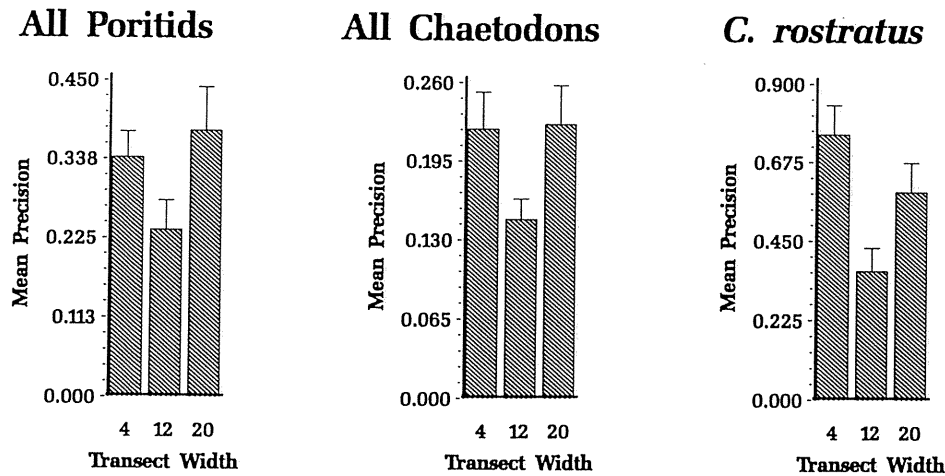
Transect width significantly affected the precision of estimates for *A. planci*, total poritids, and all but two of the chaetodontids (*C. trifasciatus*, other *C. spp*; Table 18). Ryan’s tests and plots for these groups, however, indicated two taxon-specific effects of transect width on precision. For *C. aureofasciatus*, *C baronessa*, *C. rainfordi*, and *A planci*, precision from 4m wide transects was significantly worse than that from wider transects, which provided estimates with similar precision (Fig. 13). For *C. rostratus*, total chaetodonts, and total poritids, transects of 4m and 20m width produced estimates with similar precision, but estimates from 12m wide transects had significantly better precision than both (Fig. 13).

Figure 13: Effects of transect width on mean estimated precision for taxa sampled within large transects. Mean precision was calculated from six estimates from each unit size, one from each site. **A** - Taxa for which precision from 12m and 20m wide transects was significantly better than that from 4m wide transects; **B** - Taxa for which precision was significantly better from 12m wide transects than from 4m or 20m wide transects. Error bars are standard errors.

A.

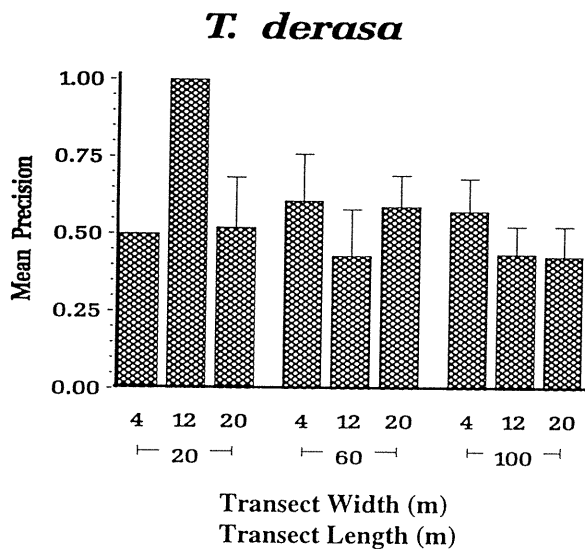


B.



The significant interaction between transect length and width with respect to precision of estimates for *T. derasa* arose because precision was significantly poorer for 20mx12m transects than for other 20m long transects and other 12m wide transects. Transects of all other dimensions provided estimates with roughly similar precision (Fig. 14).

Figure 14: Effects of transect width at each transect length on mean precision of estimated densities of *T. derasa*. Mean precision was calculated from six estimates from each unit size, one from each site. Error bars are standard errors.



In summary, on the basis of precision of estimates alone, transects of 60-100m long and 12m wide would be those most likely to produce estimates with best precision over a wide range of sites for several taxa amenable to sampling with relatively large transects. We found little evidence, however, of consistent effects of transect length or width on the precision of estimates of densities of smaller fish from smaller transects.

Cost-efficiency of Transects of Different Dimensions

MANOVA tests of the effects of transect length and width on the time required to sample sufficient transects to provide estimates with precision of 0.2 indicated that large transects of all sizes were about equally cost-effective for sampling the taxa included in the analysis (Table 19). As for analyses of precision, data for *C. trifasciatus* and *T. derasa* were excluded from MANOVA and analysed only by univariate ANOVA, along with data for total chaetodons, total poritids, and small poritids.

Cost-efficiency varied significantly with transect width for sampling total chaetodons and total poritids, but no other effects were considered significant (Table 20). Indeed, for *T. derasa* and *C. trifasciatus* all F-ratios were less than one, indicating that neither transect length, width nor their interaction explained any variance in those data. This result was consistent with the results of the MANOVA for the remaining species sampled on large transects (Table 19).

Table 19: Results of MANOVA to test for effects of transect length and width on the estimated costs (minutes) required to sample sufficient replicate transects to achieve a precision of 0.2. F_{PT} refers to the estimated F-value for Pillai's trace.

UNITS	SOURCE	F_{PT}	df	α	Infer
Large Transects	Length	0.980	18,56	0.527	-
	Width	0.838	18,56	0.650	-
	L*W	0.689	36,124	0.901	-
Small Transects	Length	1.294	8,70	0.261	-
	Width	1.579	8,70	0.147	?
	L*W	0.626	16,148	0.859	-

Table 20: Univariate ANOVA of the effects of transect length and width on the estimated cost of realising a nominal precision of 0.2. Results for fish and benthos counted within large transects, and fish counted within small transects are given in tables A., and B. respectively. Terms that were trivial in the preliminary MANOVA were not included for ANOVA, except for those species or groups not included in the multivariate analyses. For the latter, only effects for which $F>1$ are tabulated. Columns headed $\alpha_{c=\beta}$ give the critical Type I error rate for testing H_0 by the flexible decision rule. This value is also the estimated probability of Type II error when H_0 is not rejected. Effects considered to be statistically significant are indicated by # for the variable α_c decision rule, and by * for a conventional criterion of $\alpha_c=0.05$. Non-significant effects are indicted by '-'. Columns headed β_{05} give the estimated probability of Type II error when H_0 was not rejected against critical Type I error rates of $\alpha_c=0.05$.

A: Fish & Benthos - Large Transects

SPECIES	SOURCE	df	F	α	$\alpha_{c=\beta}$	Infer	β_{05}
All Chaetodons	Length	2,45	1.254	0.245	0.071	- -	0.097
	Width	2,45	4.387	0.018	0.071	# *	
All Poritids	Length	2,45	1.738	0.187	0.168	- -	0.368
	Width	2,45	4.463	0.017	0.168	# *	
Poritids 1-30cm	Length	2,9	3.717	0.067	0.091	- -	0.007
	Width	2,9	3.163	0.091	0.091	- -	0.007

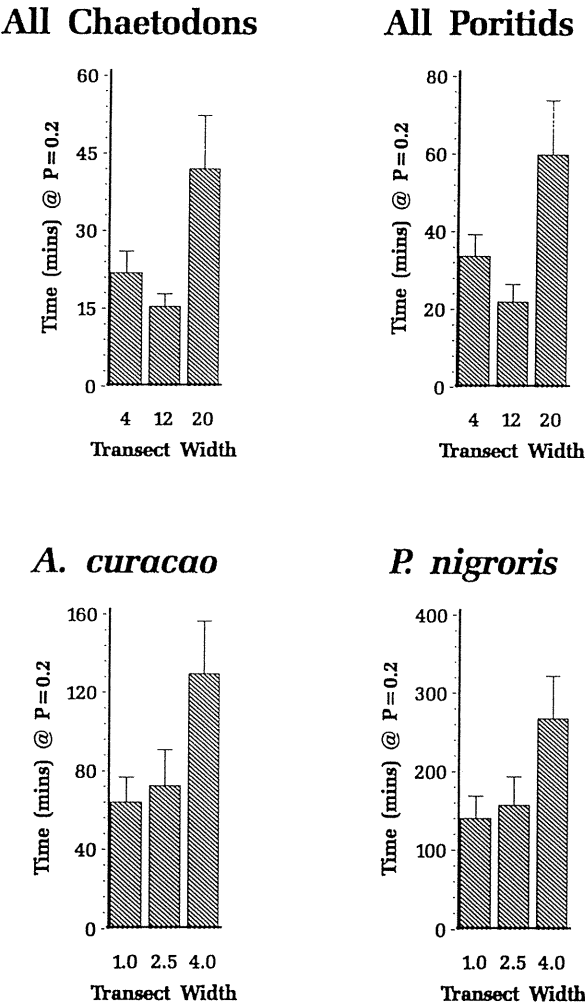
B: Fish - Small Transects

SPECIES	SOURCE	df	F	α	$\alpha_{c=\beta}$	Infer	β_{05}
<i>A. curacao</i>	Width	2,50	3.095	0.054	0.249	# -	0.575
<i>P. nigroris</i>	Width	2,46	2.845	0.068	0.416	# -	0.866

MANOVA of data from the small transects suggested potentially important effects of transect width on cost-efficiency (Table 19). Univariate ANOVA for the 4 included species and *L. dimidiatus* and *C. talboti*, however, indicated that such effects were non-trivial only for *A. curacao* and *P. nigroris* (Table 20).

The statistically significant effects of transect width on cost-efficiency were similar for all four taxa (chaetodons, poritids, *A. curacao*, *P. nigroris*) (Fig. 15). Sampling with the two narrower transect widths was likely to be more cost-effective than sampling with the widest transects in all cases, with estimated times to realise a precision of 0.2 being about half those required for the widest transects (Fig. 15).

Figure 15: Effects of transect width (m) on estimated cost-efficiency of sampling with transects of different sizes. Cost-efficiency is here defined as the cost of achieving a desired (estimated) precision (0.2) for estimates of mean population densities. Only taxa for which effects were significant by the flexible decision criteria are shown. Error bars are standard errors.



DISCUSSION

In 1978 the GBRMPA considered "... that the single most important impact in the Great Barrier Reef Region [was] the effect of fishing (both recreational and commercial) and that one of the most important recreational activities for visitors [was] fish watching" (GBRMPA 1978). Presumably, the aesthetic worth of the GBR also extends to the hard corals and conspicuous macro-fauna on reefs, such as giant clams. Accordingly, monitoring the status of organisms in these two major categories - fished species and aesthetically important species - has been emphasised as a means of assessing the "health" status of the GBR and the effectiveness of the Authority's management of it. The recurrence of outbreaks of *A. planci* and fears of declining water quality in recent years have emphasised the need to obtain reliable quantitative estimates of reef biota in order to assess the impacts of both natural and anthropogenic disturbances on the GBR at an early stage. Since 1978, there have been several reports to the GBRMPA and Queensland Department of Environment and Heritage (formerly QNPWS) concerning methods of surveying fish on the GBR, with particular attention being given to estimating abundances and/or length frequency distributions of coral trout (Ayling 1983, Ayling & Ayling 1983, 1984a,b, Crimp 1986, 1987, GBRMPA 1978, 1979, 1986). Additional work on survey techniques for benthic organisms, especially hard corals and *A. planci*, have also been supported by the GBRMPA, either directly or through the Crown of Thorns Starfish Research Committee (Fernandez 1990, Fernandez *et al* 1990, Kenchington 1978). This report builds on this previous work, with a focus specifically on belt transect methods for surveying both demersal fish species and selected benthic biota.

Characteristics of the Data and Analytical Methods

The data we obtained for most taxa from transects of all sizes were clearly not consistent with the assumptions underpinning most parametric statistical procedures. Even for those species where individuals were counted in most transects, at least one transect of each size contained no individuals, and the data from transects of at least one size (most often the smallest) were generally positively skewed and leptokurtic. Ayling & Ayling (1984a) attributed skewness in their data to the clumped distribution of *Plectropomus* spp, and noted that clumping occurred even at the scale of hectare counts. Several authors have pointed out that this is to be expected from count data (Elliot 1977, Green 1979, Sokal & Rohlf 1981, Burnham *et al* 1980), and it is to be expected that skewness will be a fact of life for monitoring data for most species.

Crimp (1986) found substantial skewness in large samples of counts of coral trout in 50m x 20m transects, and argued that it undermined the utility of normal-theory parametric statistics. Others have demonstrated, however, that many of the commonly used parametric procedures are robust to "moderate" violations of assumptions of normality, and homoscedasticity (see review by Glass *et al* 1972, Kendall & Stewart 1976, Scheffè 1959, Underwood 1981, Winer *et al* 1992). The textbook solution to heteroscedasticity, variance-mean correlation, and non-normality is to transform the raw data before analysis (Winer 1971, Sokal & Rohlf 1981), although McArdle *et al* (1990) warned that transformations such as $\log(x+1)$, involving the addition of a constant to the raw data to compensate for zeros, introduce their own biases in variance estimation and may fail to correct the above problems when sample sizes are small (<50!). Crimp (1986) examined the advantage of three transformations applied to simulated coral trout survey data, but the results seemed inconclusive: each of the transformations performed better than the others in some cases, and in yet others none of them improved the reliability of the parametric analyses over analyses of the raw data.

We chose not to transform our density data before analysis by (M)ANOVA on several grounds. Firstly, our data contained a relatively high frequency of zero counts and so we were concerned that the standard transformations (*e.g.* $\log(x+1)$) would introduce unknown biases in the results of

analyses (McArdle *et al* 1990). Secondly, we expected *a priori* the effects of the main factors of interest (transect length and/or width) on estimated densities to be linear, rather than quadratic or log-linear, which would be implied by square root or logarithmic transformations. Thirdly, we were particularly interested in the interactions among a number of factors, and the form(s) and meaning of those interactions in transformed space would be difficult to interpret, and possibly misleading (McArdle pers. com.). Finally, no single transformation was appropriate for all taxa.

The most likely consequence of heteroscedasticity and/or skewness and/or variance-mean correlation in ANOVA is inflated rates of Type I error - *i.e.* the false inference of (non)effects (Glass *et al* 1972, Kendall & Stewart 1976, Underwood 1981, Winer *et al* 1992). Thus, we were confident that inferences of trivial effects were conservative, especially given our liberal criteria for the rejection of null hypotheses. Accordingly, it is likely that conclusions that effects of transect length and width were consistent over sites and passes were robust. Further, we found little evidence that the inferences of non-trivial effects (particularly effects of transect length and width) were artefacts of violations of the assumptions of the analyses. Analysing the means and medians of data from transects of each size at each site (which were approximately normally distributed, even though means were comprised of only three data) produced the same inferences of effects of transect length and width as analyses of the raw data, as did analyses of data by randomisation techniques (Manly 1992). We suggest, therefore, that major difficulties do not arise when applying parametric analyses to the sort of count data we obtained, although further (simulation) work has been commenced (Mapstone, in progress) to examine this recommendation in greater detail.

In most cases the inferences we made from scalable decision criteria were consistent with those that would have been made from conventional procedures, although the 'critical significance levels' were often different. We suggest that the alternative procedures have considerable epistemological advantages, however, since they focus attention *a priori* on the consequences of both Type I and Type II errors that can arise from dichotomous inferential procedures. This is particularly important where strong inferences are likely to stem from failure to reject null hypotheses, as was the case here. Inferences arising from the scalable decision criteria essentially amount to decisions about whether data were most likely to have arisen from either of two competing sets of hypotheses, where both sets are specified. This forces evaluation of what will be considered an important alternative hypothesis, which Mapstone (in press, in review) has argued is appropriate if hypothesis testing procedures are to be used. In cases where hypothesis tests have low statistical power to detect a nominated effect, the scalable decision procedures will result in large critical values of α , and hence highlight as standard procedure the caution that should be attached to decisions based on such tests.

It is important to note here that it is likely that most routine monitoring programmes will comprise sampling over a hierarchy of scales, which will manifest as several nested sources of random variation in ANOVA models. Thus, if data are analysed by ANOVA or related procedures, fixed effects of interest will be tested against non-residual terms in the analyses. These higher level error terms will represent variances among the means of several sampling units. For example, where sampling occurred at several random sites within each of several reefs (ignoring stratification within reefs), tests of reef effects would be against the sites mean square, which represents the variation among site means. Our data suggest that those site means are likely to be approximately normally distributed and homoscedastic, even when only a small number of transects are sampled within each site. This is somewhat surprising since it is generally expected that the central limit theorem, which explains the normal distribution of means of samples even when raw data are non-normal, is not effective with small sample sizes (SAS 1990, Sokal & Rohlf 1981, Johnson & Kotz 1977). We suggest, therefore, contrary to Crimp (1986), that count data from transects such as those we collected will be amenable to straightforward parametric analyses, especially in the situations most likely to arise in routine monitoring. In the interests of minimising the risks of spurious inferences, however, we suggest that the first nested level of

variation above inter-replicate (residual) variation (*e.g.* site variation) always be retained in analyses, even when pooling that variation with the residual might appear justified (Underwood, 1981, Winer 1971, Winer *et al* 1992). In this way, the main tests of interest will not be subject to the biases arising from 'badly' distributed raw count data. Such a strategy should also be considered for non-ANOVA analyses, such as most multivariate procedures.

Sources of Bias

Transect Dimensions

Within the subset of species frequently observed in belt transects, it was clear that bias in estimates varied markedly with transect dimensions, particularly with the width of large transects. The consistency among species of the relative biases was remarkable, as was the fact that those relative biases were unaffected by the combination of reef, habitat, exposure conditions etc. represented by different sites. Fowler (1987) and Mapstone (1988) argued that variations in estimates of population density with changes in transect width might arise from two sources: i) observer biases in the treatment of fishes that crossed the boundaries of transects during counting; and ii) the potential for missing areas or double-counting areas within transects as transect width changes. The latter source of error also might be expected to vary with environmental conditions, such as substratum complexity and water visibility. Ayling (1983) suggested that smaller transects might have caused over-estimation of densities because of double counting of some fish, but provided no test of that speculation. Mapstone (1988) found that counts of *P. moluccensis* from very narrow transects (0.5m) over-estimated abundances, which he attributed to biases in the treatment of fish moving across the boundaries of the transects. We found, however, that the width-dependent bias in counts of *Plectropomus* spp and (collectively) chaetodontids were not altered substantially by correction for possible errors in the treatment of fish that migrated across transect boundaries during counting.

Effects of sampling unit dimensions on estimates of abundance have been noted previously by several researchers (Ayling 1983, Ayling & Ayling 1984a, Harriott 1984, McCormick & Choat 1987, Mapstone 1988, Morin 1984, Sale & Sharp 1983, Weibe & Holland 1968, Weibe 1971, Downing 1979). Sale and Sharp (1983) also found that estimated densities of reef fish from several taxa increased with decreasing transect width (from 3m to 1m), as did Mapstone (1988) in a study of *P. moluccensis* (widths from 0.5m - 3m). Fowler (1987), however, found estimates of *C. rainfordi* and *C. rostratus* to be constant over several sizes of sampling units, though he considered transect widths of only 1-3m. Fowler's result for *C. rainfordi* was not consistent with that of Sale & Sharp (1983), who found effects of transect width on estimated densities of *C. rainfordi* over the same range of sampling widths as Fowler (1987). Some of these earlier results clearly differ from ours, in that we failed to see any effects of transect width for some of the same species (*e.g.* *P. moluccensis*) but did find changes in bias with transect width for others (*e.g.* *C. rainfordi*, *C. rostratus* - cf Fowler 1987). We cannot infer why these discrepancies arose, but note that our data cover a greater range of habitat characteristics than the earlier studies and might, therefore, be considered more generally applicable.

Clearly, we have no definitive estimate of 'true' population densities of these organisms at the sites we sampled. Such estimates would be very difficult to obtain, a fact which underpins the extreme difficulty in assessing accuracy of counts and biases of counting procedures (Andrew & Mapstone 1987, Fowler 1987, Sale & Douglas 1981, Sale & Sharp 1983). Because the differences in estimated density were robust to sources of potential over-estimation of fish, and because the same patterns were present for estimates of sessile biota, we infer that the decline in estimates with increasing transect size reflected increasing under-estimation, rather than decreasing over-estimation. That inference is consistent with results of others who have attempted to quantify bias in visual surveys of reef organisms (Sale & Douglas 1981, Sale & Sharp 1983, Lincoln Smith

1988, 1989, Samoilys & Carlos 1992, Thresher & Gunn 1986). Most authors have argued that overestimation is less likely than under-estimation in transect methods because observers are far more likely to miss individuals than count individuals twice or count those that were not there (Burnham *et al* 1979, 1980, Eberhardt 1978, Caughley, 1977, Fowler 1987, Harmelin-Vivien *et al* 1985, Keast & Harker 1977, Sale & Sharp 1983, Sale & Douglas 1981, Short & Bayliss 1985), a contention that has been supported where visual surveys have been compared with putatively more accurate methods such as destructive sampling (Brock 1982, Mapstone 1988, Russell *et al* 1978, St. John *et al* 1990, Sale & Douglas, 1981, Samoilys & Carlos 1992, but see McCormick & Choat 1987). If this is accepted, our greatest estimates were the least biased, and therefore narrower, and in some cases shorter, transects would be preferred for estimating the densities of most of the organisms we considered.

Search Intensity

It was clear from our data that larger transects were being searched more rapidly than smaller transects. Ayling & Ayling (1992b) reported similarly disproportionate changes in search times with increased transect width (from 5m to 10m), though their surveys of 5m and 10m wide transects (average 4.2 and 6.1 min/transect respectively) were done in different years (1991 & 1992 respectively). Though the comparison is clearly confounded with inter-annual variations, they also found that estimated densities of all species analysed were generally lower by about 20-50% for the wider transects sampled on the same reefs as the narrower transects the previous year. Mapstone (1988) found that counts of *P. moluccensis* decreased slightly, though statistically significantly, as swimming speed of the observer increased, and recommended slower rather than faster rates of survey. Samoilys & Carlos (1992) reported similar results for several families of reef fish, though they noted that none of the apparent effects they observed were statistically significant. On the basis of experimental studies, Lincoln Smith (1989) reported that bias in counts of temperate reef fish over constant areas varied with swimming speeds, but that the effects of swimming speed differed between cryptic and conspicuous species of fish. Estimates of drab and/or cryptic species declined as swimming speed increased (and hence search intensity decreased), whereas highly mobile species were often double-counted at slower swimming speeds. Since several of the species we counted fall into the latter category, Lincoln Smith's result might suggest that we were over-estimating those species on the smaller transects. Two factors undermine such an inference, however: i) Differences in estimates the observed patterns in bias persisted after corrections for potential differential biases in the treatment of migrants; and ii) the same patterns of transect size-related bias existed for benthic biota. It seems unlikely that benthic biota would have been double-counted, unless the primary observer back-tracked over substratum he had already searched. This seems highly unlikely, given that the four divers moved steadily in only one direction along each transect.

Simple adjustment of counts to standardise for search intensity demonstrated that change in search intensity could have precipitated more than the observed differences in bias among transects of different size. No other auxiliary variables we measured had the potential to so convincingly explain the observed patterns in bias. It is likely, however, that the relationship between bias in counts and the time taken to search transects is something more complex than the simple linear model we used. For example, the efficiency of a search would be expected to decline little as transect width increased from very narrow widths to the maximum width that could be covered continually within the field of vision of the observer (Mapstone 1988). This might explain why we found no convincing evidence for changes in bias with increasing width of small transects, despite declining search intensity. As the width of transects increased beyond the observer's field of vision, however, the observer would be required to count organisms in a sequence of blocks, roughly equal in dimensions to the observers field of vision. The reference points for what had already been counted and what was yet to be counted would be constantly re-appraised. The potential for double counting or missing small areas would be expected to increase as the observer

had to move over increasing distances to survey the entire transect. It seems likely, therefore, that the relationship between increasing widths of transects and increased potential for errors in counts would be non-linear (Mapstone 1988), or at least have a slope of less than one. The adjustment of counts based on a simple linear model of a one-to-one relationship between area and search time reversed apparent width-related bias in counts. This result suggested that less dramatic effects of differential search intensity could still have precipitated the changes in bias we observed.

Depth and Time of Day

It was unlikely that depth or time of day of sampling precipitated these effects because: i) trends in density related to depth or time of day were not consistent among sites or taxa; ii) the allocation of transects was randomised across time and (effectively) depth within each site, but placement of transects relative to each other was the same at all sites; and iii) the combination of these two features would tend to obscure consistent differences among transect sizes rather than precipitate them. GBRMPA (1978) noted that coral trout tended to be seen more frequently in deeper water off Heron Island (9-14m vs 3-9m), and in a subsequent workshop (GBRMPA 1979) found no consistent trends in counts with either tidal phase or time of day (08:00-17:00). Patterns in the depth-distribution of fishes have received some attention elsewhere (Bouchon-Navaro 1980, Clarke 1977, Galzin 1987, Harmelin-Vivien 1977, Jones 1968, Jones & Chase 1978), but have received little attention in the GBR region (but see Russ 1984, Williams & Hatcher 1983). Done (1983) discussed depth stratification of coral assemblages. Even if evidence of depth stratification of most species is sparse or absent, parsimony would recommend choosing survey methods that allow sampling in a depth-stratified manner. Thus, narrower transects would be preferred over wider transects.

Observers and Counting Methods

We also noted a consistent difference between the two observers counting fish in this study. This is perhaps to be expected, but it is important to note that both observers were considered experienced in counting fish on coral reefs. Variation among the counting abilities of observers has been recorded in prior studies (Bell *et al* 1985, Short & Bayliss 1985, St. John *et al* 1990, GBRMPA 1978, 1979, but see Samoilys & Carlos 1992), and these authors have stressed the importance of frequent observer training and cross-calibration (see also Ayling & Ayling 1984a, Crimp 1986, 1987). It will be important, therefore, in the implementation of routine monitoring studies to emphasise observer training to maximise the similarity in counts by different observers. We also suggest that periodic 're-calibration' of observers will be necessary to ensure that observer-related biases remain consistent. This will be imperative where different observers are responsible for counting organisms at different places and/or times. Legitimate comparison of such counts will rest on the assumption that all observers were counting with equal bias.

The observer related bias in this study rendered assessment of the relative biases of counts from transect mid-lines impossible. There is an ample literature on the biases in such counts from the terrestrial faunal surveys, however, (see review by Burnham *et al* 1980) and it seems likely that similar characteristics would apply to counts of fish from transect mid-lines (Hope 1989). The relationship between the probability of seeing an organism and distance from the observer (termed the sightability curve) has been found to be highly depended on target species and habitat characteristics, as well as observer experience. Whilst it might be argued that sightability curves for conspicuous species like chaetodontids would be essentially flat over several metres, this is less likely for cryptic fishes and benthic biota, especially in topographically complex habitats. In order to compare counts among sites with very different habitat and water quality characteristics, considerable time would have to be spent characterising sightability curves for those disparate conditions (Hope 1989). For these reasons alone, there seems good reason to ensure that even narrow transects are searched thoroughly rather than surveyed from the mid-line.

If the observed counting biases related to transect dimensions arose only because of changes in search intensity, it might be expected that alternative methods of survey, in which less zig-zagging was required, would produce greater estimates than the single pass zig-zag counts for wider transects. This proved not to be the case. Though the tests for effects of counting method were weak, there was clearly no consistent ranking of the counts from different methods, and no convincing hint that we might have simply failed to recognise statistically an effect that was really there (Fig. 10). This is surprising, given that comparisons between parallel counts and other methods were confounded with differences in the effective transect width over which each observer counted (6m for parallel counts, 12 m for sequential and zig-zag counts). Further, each observer took slightly longer to complete sequential counts than single pass zig-zag counts (8.25min vs 7.4min, 9.5min vs 8.2min for observers 1 & 2 respectively) and both observers took about 70% as long for parallel counts, despite searching only half the area. Thus, it might have been expected that parallel counts would be greater than others at least partly because of the above mentioned differences in bias related to transect width and/or search intensity. This did not occur for either observer, however. The mean estimated densities of all species in the 60m x 12m transects surveyed by the different methods were fairly similar to those estimated from transects of the same size in the major part of the study, and all were less than estimates from the 4m wide transects in the main study. This provides circumstantial evidence that the difference in bias between 4m and 12m wide transects would not be corrected simply by employing a different method of counting that reduced the degree of zig-zag swimming.

Nor did we find any significant or apparent effects of prior diver activity on counts, except perhaps for *Plectropomus* and *C. plebeius*. Fowler (1987), in contrast to our results, found no effect of prior disturbance on counts of *C. plebeius*, but did find significant effects of prior diver activity on *C. rainfordi* and *C. rostratus*. Mapstone (1988) recorded disturbance of *P. moluccensis* attributable to laying transect tapes prior to counting, but that disturbance lasted less than three minutes. Ayling & Ayling (1984a) found no effects of diver activity on their counts of coral trout, and routinely counted *Plectropomus* spp and other fishes by either the parallel or sequential methods we examined. Fowler (1987) made a useful distinction between 'simultaneous' counting procedure where the transect is measured and surveyed in a single pass over the substratum, and 'sequential' procedures in which the tape measuring the transect to be surveyed is layed prior to counting. He recommended simultaneous methods over sequential methods to minimise the effects of disturbance on counts. We adopted similar procedures in this survey, and although we have no strong evidence that simultaneous and sequential surveys would produce different results in general application, caution would recommend continued use of the simultaneous methods.

Precision and Cost Efficiency

Generally, considerations of precision for large transects would favour longer transects than shorter transects, contrary to considerations of bias. Whilst the distinction between 60m and 100m long transects often was negligible, 20m long transects always provided estimates with the poorest precision. Although precision was relatively constant among transects of different width for most species, where it did differ with transect width, the safest generalisation would be that 12m wide transect provided more precise estimates than 4m wide transects, and precision at least as good as 20m wide transects. There were no striking variations in mean precision with the changes in the dimensions of small transects.

Variation with precision with the dimensions of sampling units is to be expected for most field populations for two reasons: i) populations are generally over-dispersed - *ie* have a clumped distribution; and ii) as sampling unit size increases, the probability of sampling at least part of a clump of individuals will increase, with the result that variation in counts will tend to decrease (see review by Andrew & Mapstone 1987, Elliott 1977, Downing 1979, Green 1979, Greig-Smith 1983, Peilou 1977, Resh 1979, Southwood 1969). It is important to note, however, that aggregation can

occur at several scales, with 'clumps of clumps' being a phenomenon at larger scales. The characteristics of aggregation will also vary with species and their relationships to habitat characteristics, and so the relationship between the spatial distribution of organisms and the results of sampling with units of different sizes will also be species-specific. Ayling and Ayling (1984a) for example, noted that *Plectropomus* spp appeared aggregated at the scale sampled by 50 x 20m transects, but also at the scales sampled by hectare counts. Our results suggested that the relationship between dispersion of organisms and sampling unit size is not a simple function of area. For example, transects of 20m x 12m sampled the same areas as 60m x 4m transects (240m^2), and 20m x 2m transects sampled the same area as 10m x 4m transects, but 20m long transects gave estimates of precision more similar to each other than to the precision derived from longer transects that sampled the same area. We cannot infer why this pattern arose, though it may indicate that dispersion of most organisms is more variable along the reef slope than over similar distances down the slope. This might be expected because many major topographic structures of reefs tend to be radially oriented: for example spurs and grooves radiate down slope from the reef crests and alternate along shore.

When the costs (time) of surveying transects were considered, the increased time involved in surveying longer transects clearly balanced any gains in precision with increasing transect length. Even when only the actual times we took to survey transects were considered, transects of all lengths would be considered about equally cost-efficient on average. The only statistically significant variations in cost-effectiveness were with transect width for four taxa, despite the liberal selection criteria precipitated by the combination of high variation in precision among sites and the scalable decision rules. In these cases, the (observed) cost of surveying the widest transects outweighed considerably their advantages with respect to precision, and the 4m and/or 12m wide transects appeared more cost-effective sampling units. The general lack of discrimination among sampling units on the grounds of cost-efficiency is unusual in the context of relevant published studies, most authors having found it more cost-effective to sample many small units than a few large ones (review by Andrew & Mapstone 1987, McCormick & Choat 1987). In most published studies, however, the precision and cost-effectiveness of sampling units were compared at only one site, and without reference to the variation in precision and/or cost-efficiency among multiple samples taken with units of the same size.

It is difficult to speculate how standardisation of search intensity might affect such considerations because i) the functional relationship between search intensity and area is not certain; ii) if other factors were at least partly responsible for the differences in bias with transect dimensions, it would be uncertain how increased search intensity would affect estimated density and the variances of those estimates (and hence precision). As discussed above, it is generally accepted that increasing size of sampling units will tend to reduce variance:mean ratios for constant mean density because larger transects are less likely to be influenced by over-dispersion of organisms in the field, but it is also likely that variances will increase as mean estimates increase (Sokal & Rohlf 1981). Hence, it is not clear that transects of different sizes searched with uniform intensity would have equal characteristics of precision, even if they produced uniform estimates of density. If the precision of counts from larger transects remained relatively constant as estimated density increased with increasing search intensity, however, any increase in search time for the larger transects would tend to render them less cost effective than the smaller transects.

Optimum Transect Dimensions

We have argued that for surveys of large, highly mobile fishes and relatively sparsely distributed and/or uncommon benthos narrow transects are to be preferred over wider transects because of differential bias in estimates. For two grouped taxa (all chaetodons and all poritids), this strategy would result in a slightly sub-optimal sampling strategy with respect to the cost of achieving a desired precision because transects of 12m width proved more cost effective than 4m wide

transects. The trade-off is slight, however, the use of 4m wide transects taking on average only an additional 6min for chaetodons and 12min for poritids to sample a site with a precision of 0.2. We believe that this inefficiency is outweighed by the considerable and conspicuous improvement in accuracy offered for most taxa by sampling with the narrower transects. It is likely also that equalisation of search intensity would diminish or reverse the discrepancy in cost-efficiency.

The choice of optimum transect length is less clear cut. For four groups (all chaetodons, *C. rainfordi*, small and medium poritids), bias was less for 20m transects than for 100m transects, though the effect was considerably smaller than the effects of transect width. For these groups, considerations of bias would favour the choice of 20m or 60m long transects, which were statistically not distinguishable. Considerations of cost-efficiency would not alter that choice. Given our choice of 4m wide transects, this would result in sampling by 20m x 4m or 60m x 4m transects. We considered the 60m x 4m transects preferable because for all taxa the frequency of zero counts was greatest in the smallest transects, suggesting that those transects were sampling at a scale less likely to span aggregations of most organisms. Long, narrow transects are also logistically advantageous. Surveying over narrow widths is likely to be easier in difficult counting conditions, such as rough weather and poor visibility, and because they cover relatively small ranges in depth, it will be easier to maintain safe diving routines for prolonged field trips. Since 'off-the-shelf' 50m long fibreglass tapes are far easier to use than the 100m tapes, we suggest that 50m long transects would be logistically easier than 60m transects, but if 50m transects are adopted we recommend surveying over a width of 5m to maintain the transect area at a minimum of 240m². Clearly, we are speculating here that there is little loss in relative accuracy by increasing transect width from 4m to 5m.

The absence of clear patterns in either bias or cost-efficiency for most fish counted within small transects allows great flexibility in the choice of sampling unit sizes for survey of them, although the cost efficiency of surveying 4m wide transects was less than for the narrow transects for two of the most abundant species (*A. curacao*, *P. nigroris*). We recommend, therefore, that the units of 20m length and 2-2.5m width (half the width of the large transects) would facilitate easy combination of counts over both large and small transects together.

Since the patterns we observed in bias, precision, and cost efficiency were remarkably similar across so many organisms, we recommend that one transect size will suffice for sampling the range of organisms we considered, although clearly several taxa would not be adequately sampled by any of the units we used (see above). There are also strong logistic arguments for using transects of the same size for counting all these organisms, and to lay out and survey separate transects for each organisms would be prohibitively expensive.

Given the continued emphasis by the GBRMPA on surveys of coral trout, some comment is appropriate on the use of small transects (relative to earlier surveys: Ayling 1983b,c, Ayling & Ayling 1984b,c, 1985, 1986a,b) for recent estimation of density of *Plectropomus* in areas of low abundance. In surveys of reefs in the Cairns Section of the GBR Marine Park since these data were collected, we have found estimated densities of *Plectropomus* spp from 50m x 5m transects to be considerably lower than at Davies and Bowden Reefs, and that the proportion of transects in which no *Plectropomus* spp were recorded has been very high (up to 45%). Ayling & Ayling (1991, 1992a,b) have argued that these results indicate that the use of 5m wide transects should be reviewed, and suggested that 50m x 10m transects were likely to reduce the frequency of zero counts and produce similar estimates of density without increases in survey time. Since they were estimating the length of each coral trout observed, they also argued that it was important to maximise the number of *Plectropomus* spp recorded in order to construct useful length frequency distributions for each reef. Crimp (1987), however, has demonstrated in simulation studies that very large numbers of coral trout would be needed to confidently infer temporal changes or differences among reefs in size-frequency distributions, and it seems unlikely that sufficient

numbers will be recorded even from the larger transects when densities of the fish are low. Crimp (1987) has recommended that visual surveys not be used for such analyses.

Comparisons of data from reefs surveyed with transects of 50m x 20m, 50m x 10m, and 50m x 5m indicate that mean densities estimated from 5m transects have been generally greater than those from the wider transects, though the precision of estimates within each site was, as expected, poorer. These comparisons are difficult to interpret, however, since in most cases the different methods were used in different years. Comparisons of analyses based on data from 5m and 10m wide transects, with variation among site or reef means comprising the error terms for F-tests, indicated that data from 5m wide transects provided estimates of power equal to or better than data from 10m wide transects (Mapstone & Ayling, unpublished). For both data sets, means were approximately normally distributed and homoscedastic. Since considerable fractions of the counts even from 10m wide transects were zero (up to 35%), it would be unwise to test effects of interest against the variation among transects in such tests irrespective of whether 5m or 10m wide transects are used (but see Kuno 1986 for methods of estimating density from presence absence records for individual sampling units). In view of the nested sampling designs now being used and likely to continue to be used to survey these and other fishes, we suggest that the use of 10m wide transects will not be demonstrably better than the 5m wide transects, and may be less efficient (more time-consuming) to survey sufficiently thoroughly to ensure that bias approaches that of the narrower transects.

In summary, we suggest that the narrower transects have several advantages over their wider counterparts. In addition to providing less biased estimates, they will allow examination of finer scale variations in density (*e.g.* depth effects) than wider transects, and they are likely to be easier to use in difficult conditions and within the limits of diving safety on long field trips. Further, improvements in precision of estimates for sites, reefs, habitats, etc. are likely to be more rapid through increased replication of the 50-60m x 4-5m transects than through increased size of transects. Finally, even if more thorough surveys of larger transects reduced their apparent biases, the added costs would severely constrain the flexibility in future surveys to increase replication and improve power.

Suggested Procedure for Routine Survey

Although we carefully controlled the width of transects in this study, it is likely to be difficult to do so in routine application. Two strategies might be employed to account for variation and/or bias in counts because of inexact estimation of transect width. Samoilys & Carlos (1992) used a system similar to ours, in which the diver laying the tape and the observer were linked with a buoyant chord equal in length to the transect width. The observer worked on only one side of the tape, and was constrained by the cord to the desired transect width. Alternatively, we have estimated the widths of transects and kept records of the accuracy of those estimates by the observer. At the beginning of each transect, a length of tape equivalent to the transect width is laid perpendicular to the direction in which the transect will be laid. The observer uses this as his/her reference for estimation of transect width during counts. At the end of the transect, the observer nominates a feature on the substratum that s/he estimates to be at the distant edge of the transect from the tape layer. This distance is then measured by the tape layer and recorded. This allows for ongoing adjustment of estimation by the observer, and correction of counts if observers are found to be consistently biased. We have not examined the tolerance of estimates to errors in measurement of transect dimensions (Mapstone 1988).

We suggest the following procedures for survey of organisms of the types we have considered. Transects of 50m x 5m are surveyed for the large, mobile fishes and larger, sparser discrete benthic organisms. Surveys require at least two divers, although a third may be required if numerous benthic organisms are to be counted. Each transect begins with attaching the loose end

of the tape to the substratum, recording the depth at that point, measuring out the 5m reference for estimation of transect width (if this Method is being used), and recording the time at which the count begins. One diver begins laying out the tape to measure transect length, and the principal observer counts large fishes as the transect is being demarcated, with the observer swimming roughly parallel to the tape layer and counting over the entire transect width to one side of the tape. Transects should be searched thoroughly in a zig-zag manner. At the end of the count, the principle observer records the time and indicates a point estimated to be 5m from the tape layer, perpendicular to the tape. The tape layer records the depth at the end of the tape, measures the distance to the point indicated by the principal observer, and then records the measurement and shows it to the observer. As the tape is being re-wound, the observer(s) count benthic biota over the same area as the fish counts. If small transects are to be surveyed, we suggest that the rewinding of the tape stop at 20m from the tape-end and the observer counts small fishes over the remaining 20m and a width of 2.5m. The beginning and ending times of these counts should also be recorded. The tape-layer remains at the 20m point until the observer returns, having completed the counts of benthic biota between 0 and 20m following the counts of small fish. The time of completing the counts is recorded, the tape is taken up and the divers move to the next transect, which should be located either a random distance from the end of that just completed, or at least a full transect-length away.

Additional information can also be recorded by the tape layer if desired. For example, they can sum the total length of tape overlaying (for example) live hard coral, soft coral, and recently killed standing coral as the tape is rewound. We recommend that no more than four variables be recorded in this way since the tape layer will need to keep running mental tallies of these figures as s/he rewinds the tape.

Concluding Remarks

Although belt transects proved to be a useful method from which to estimate population densities of several species counted in this study, including *Plectropomus* spp, they could not be recommended as a primary method of survey for many others. Several species of chaetodontids, all the lutjanids and lethrinids, very large poritid corals, and the giant clam *T. gigas* were at such low abundances, distributed so patchily, or sufficiently wary of divers that there would not be reasonable expectation of encountering individuals in most transects of even 100m x 20m. Ayling (1983a) also found that most of the species encountered in transect and hectare counts occurred only infrequently, and long lists of uncommon species were also evident in the data from the earlier workshop on fish survey methods (GBRMPA 1978, 1979). Counting these uncommon species routinely would be desirable only if it was considered important to estimate the richness or diversity of some groups or if estimates of population density were sought at some higher, agglomerative level (such as total chaetodontids). By virtue of their infrequent occurrence in transects, however, these species are unlikely to increase substantially the time required to survey transects. Where sampling will occur at a hierarchy of spatial scales, the means or medians of counts of these rare species at higher levels in the hierarchy (such as average density at each site) may provide useful and analysable measures of the relative abundance of some groups of species, but several species will have to be treated simply as 'present or absent' even at this larger spatial scales (but see Kuno 1986). We recommend that surveys of abundances utilising belt transects of the sizes we considered should be designed principally with reference to the more abundant species expected, but that selected rarer species should be counted to the extent that doing so does not increase the cost of surveys.

In conclusion, we support the continued use of visual survey techniques for estimation of abundances of demersal biota on the GBR. We have demonstrated that estimates of abundances are likely to depend in part on the sizes of sampling units used to count them, and have provided a thorough assessment of the sampling characteristics of several sizes of belt transects for several

taxa. It will at times be desirable to review the use of methods (including those we have recommended) for particular purposes. For example, where the objectives of specific surveys do not require precise estimates of abundance, alternative methods that are cheaper and easier to apply might be sufficient for the purpose. Similarly, the slavish use of transects of the dimensions we have considered for taxa other than those we counted should be discouraged, unless such use both suits the objectives of a particular study and has been verified to be appropriate for those taxa. We recommend that such review, extension, or changes to the methods we evaluated should be documented thoroughly, including quantitative description of the sampling characteristics of the new or revised methods. Without such documentation, the integration of data from different sources will be fraught with difficulty and prone to spurious inferences simply as a result of confounding between the sampling characteristics of disparate methods with the circumstances of their application. This issue is perhaps most important in use of field data to assess or guide management strategies, since it is in this context that data from several sources are likely to be interpreted with one purpose in mind.

REFERENCES

- Alcala A.C. 1988. Effects of protective management of marine reserves on fish abundances and fish yields in the Philippines. *Ambio*. **17**: 194-199.
- Andrew N. L., and B. D. Mapstone. 1987. Sampling and the description of spatial pattern in Marine Ecology. *Oceanogr. Mar. Biol. Ann. Rev.* **25**: 39-90.
- Ayling A.M. 1983a. *Coral trout survey methods with special reference to estimating juvenile numbers*. **Unpublished Report to GBRMPA**. 7pp.
- Ayling A.M. 1983b. *Distribution and abundance of coral trout species (Plectropomus spp) in the Cairns section of the Great Barrier Reef Marine Park*. **Unpublished Report to GBRMPA**.
- Ayling A.M. 1983c. *Distribution and abundance of coral trout species (Plectropomus spp) in the Townsville and Whitsunday areas of the Great Barrier Reef*. **Unpublished Report to GBRMPA**.
- Ayling A.M., and A.L. Ayling. 1984a. *Determination of the most accurate survey size and method for visual counting of coral trout (Plectropomus spp.)*. **Unpublished Report to the GBRMPA**. 9pp.
- Ayling A.M., and A.L. Ayling. 1984b. *Distribution and abundance of coral trout species (Plectropomus spp) in the Swain Group of : Southern Section of the Great Barrier Reef*. **Unpublished Report to GBRMPA**.
- Ayling A.M., and A.L. Ayling. 1984c. *A biological survey of selected reefs in the Far-North Section of the Great Barrier Reef Marine Park*. **Unpublished Report to GBRMPA**.
- Ayling A.M., and A.L. Ayling. 1985. *A biological survey of selected reefs in the Central Section of the Great Barrier Reef Marine Park*. **Unpublished Report to GBRMPA**.
- Ayling A.M., and A.L. Ayling. 1986a. *A biological survey of selected reefs Capricorn Section of the Great Barrier Reef Marine Park*. **Unpublished Report to GBRMPA**. 61pp.
- Ayling A.M., and A.L. Ayling. 1986b. *Coral Trout Survey Data*. **Unpublished Report to GBRMPA**. 212pp
- Ayling A.M., and A.L. Ayling. 1991. *Effects of fishing pilot study: visual surveys on Cairns cluster reefs and closed reefs that will be opened under the new zoning plan*. **Unpublished Proposal for funding to GBRMPA**.
- Ayling A.M, and A.L. Ayling. 1992a. *Effects of fishing pilot study: visual surveys on Cairns cluster reefs and closed reefs that will be opened under the new zoning plan*. **Unpublished Report to GBRMPA**. 48pp.
- Ayling A.M., and A.L. Ayling. 1992b. *Bramble reef replenishment area - baseline survey*. **Report to GBRMPA**. 23pp.
- Ayling A.M., and A.L. Ayling. 1993. *Bramble reef replenishment area - first post-closure survey*. **Unpublished Report to GBRMPA**. 45pp.
- Ayling A.M., A.L. Ayling, and B.D. Mapstone. 1991. Possible effects of protection from fishing pressure on recruitment rates of the coral trout (*Plectropomus leopardus*: Serranidae). *Proceedings of Workshop of Recruitment Processes*. **Australian Society for Fish Biology**. Hobart.

- Bell J.D., G.J.S Craik, D.A. Pollard, & B.C. Russell. 1985. Estimating length frequency distributions of large reef fish underwater. *Coral Reefs*, **4**:41-44.
- Bohnsack J.A and S.P. Banerot. 1983. *A random point census technique for visually assessing coral reef fishes*. **Unpublished report to South Carolina Sea Grant Consortium**.
- Bouchon-Navaro Y. 1980. Quantitative distribution of the Chaetodontidae on a fringing reef of the Jordanian coast (Gulf of Aqaba, Red Sea). *Tethys*. **9**:247-251.
- Brock R.E. 1982. A critique of the visual census method for assessing coral reef fish populations. *Bull. Mar. Sci.*, **32**: 269-276.
- Bross W.E. and B.C. Cowell. 1987. A technique for optimizing sample size (replication). *J. Exp. Mar. Biol. Ecol.*, **114**: 63-71.
- Burnham K.P., D.R. Anderson, and J.L. Laake. 1979. Robust estimation from line transect data. *J. Wildl. Manage.*, **43**: 992-996.
- Burnham K.P., D.R. Anderson, and J.L. Laake. 1980. Estimation of density from line transect sampling of biological populations. *Wildl. Monogr.*, **No 72**.
- Caughley G. 1977 *Analysis of Vertebrate Populations* John Wiley & Sons 234 pp.
- Clarke R.D. 1977. Habitat distribution and species diversity of chaetodontid and pomacentrid fishes near Bimini, Bahamas. *Mar. Biol.* **40**:277-289.
- Cochran, W.G. 1963. *Sampling Techniques*, 2nd ed. John Wiley & Sons, New York.
- Cochran W.G. and G.M. Cox. 1957. *Experimental Designs*. 2nd Ed. Wiley, New York. 611pp.
- Craik G.J.S. 1981. Underwater survey of coral trout *Plectropomus leopardus* (Serranidae) populations in the Capricornia section of the Great Barrier Reef Marine Park. *Proc. 4th Int. Coral Reef Symp.*, **Vol. 1**: 53-58.
- Crimp O. 1986. *The applicability of some statistical tests in analysing coral trout (Plectropomus leopardus) survey data*. **Internal Report to QNPWS**. 45pp.
- Crimp O. 1987. *The applicability of the Kolmogorov-Smirnov tests in analysing coral trout (Plectropomus leopardus) size frequency data*. **Internal Report to QNPWS**. 38pp.
- Day R.W., and G.P. Quinn. 1989. Comparisons of treatments after an analysis of variance in ecology. *Ecol. Monogr.*, **59**: 433-463
- DeMartini E.E., and D. Roberts. 1982. An empirical test of biases in the rapid visual technique for species-time censuses of reef fish assemblages. *Mar. Biol.* **70**:129-134.
- Doherty P.J. 1987. The replenishment of populations of coral reef fishes, recruitment surveys, and the problems of variability manifest on multiple scales. *Bull. Mar. Sci.*, **41**:411-422.
- Doherty P.J., and D. McB. Williams. 1988. The replenishment of coral reef fish populations. *Oceanogr. Mar. Biol. Ann. Rev.*, **26**: 487-551.
- Done T.J. 1983. Coral zonation: Its nature and significance, pp107-147 in *Perspectives on coral reefs*, D.J. Barnes Ed. Australian Inst. Mar. Sci.
- Downing J.A. 1979. Aggregation, transformation, and the design of benthos sampling programs. *J. Fish. Res. Bd. Can.*, **36**:1454-1463.
- Downing J.A., and M.R. Anderson. 1985. Estimating the standing biomass of aquatic macrophytes. *Can. J. Fish. Aquat. Sci.*, **42**:1860-1869.
- Downing J.A., and H. Cyr. 1985. Quantitative estimation of epiphytic invertebrate populations. *Can. J. Fish. Aquat. Sci.*, **42**:1570-1579.
- Eberhardt L.L. 1978. Transect methods for population studies. *J. Wildl. Manage.*, **42**:1-31.

- Elliott J.M. 1977. *Some methods for the statistical analysis of samples of benthic invertebrates*. **Freshwater Biological Assoc., Scientific Publication No. 25**, Ferry House, U.K..
- Fernandez L. 1990. Effect of the distribution and density of target organisms on manta tow estimates of their abundance. *Coral Reefs*, **9**:161-165.
- Fernandez L., H. Marsh, P.J. Moran, and D.F. Sinclair. 1990. Bias in manta tow surveys of *Acanthaster planci*. *Coral Reefs*, **9**:155-160.
- Fowler A.J. 1987. The development of sampling strategies for population studies of coral reef fishes. A case study. *Coral Reefs*, **6**:49-58.
- Galzin R. 1987. Structure of fish communities of French Polynesian coral reefs. I. Spatial scales. *Mar. Ecol. Prog. Ser.*, **41**:129-136.
- GBRMPA. 1978. *Workshop on reef fish assessment and monitoring*. **GBRMPA Workshop Series no. 2.**, Townsville, Australia. 64pp
- GBRMPA. 1979. *Coral trout assessment techniques*. **GBRMPA Workshop Series no. 3.**, Townsville, Australia. 86pp.
- GBRMPA. 1986. *Coral trout survey techniques*. **GBRMPA Workshop Series no. 10 (draft).**, Townsville, Australia.
- Glass G.V., P.D. Peckham, and J.R. Sanders. 1972. Consequences of failure to meet assumptions underlying the fixed effects analysis of variance and covariance. *Rev. Educat. Res.*, **42**:237-293.
- Gray C.A., and J.D. Bell. 1986. Consequences of two common techniques for sampling vagile macrofauna associated with the seagrass *Zostera capricorni* *Mar. Ecol. Prog. Ser.*, **28**: 43-48.
- Green, R. 1979. *Sampling Design and Statistical Methods for Environmental Scientists*. John Wiley & Sons, New York.
- Greig-Smith, P. 1983. *Quantitative Plant Ecology*, 3rd ed. Blackwell Scientific, Oxford.
- Harmelin-Vivien M.L. 1977. Ecological distributions of fishes on the outer slope of Tulear Reef (Madagascar). *Proceedings of the 3rd Int. Coral Reef Symp.*, **1**:289-296.
- Harmelin-Vivien M.L., J.G. Harmelin, C. Chauvet, C. Duval, R. Galzin, P. Lejeune, G. Barnabé, F. Blanc, R. Chevalier, J. Duclerc, & G. Lasserre. 1985. Evaluation visuelle des peuplements et populations de poissons: Methodes et problemes. *Rev. Ecol. (Terre Vie)*, **40**: 467-539.
- Harriott V.J. 1984. *Census techniques, distribution, abundance and processing of the large sea-cucumber species (Echinodermata: Holothuroidea) on the Great Barrier Reef*. **Report to GBRMPA**. 39pp.
- Hendee J.C., G.H. Stanky, and R.C. Lucas. 1990. *Wilderness Management*. North American Press. 546pp.
- Hope M.L. 1989. *Computer modelling of reef fish distributions to investigate the line transect sampling method*. Honours Thesis, James Cook University. 58pp.
- Johnson N.L., and S. Kotz. 1977. *URN Models and Their Application: An Approach to Modern Discrete Probability Theory*. John Wiley & Sons, New York. 352pp.
- Jones R.S. 1968. Ecological relationships in Hawaiian and Johnston Island Acanthuridae (Surgeonfishes). *Micronesica*. **4**:309-361.
- Jones R.S., and J.A. Chase 1975. Community structure and distribution of fishes in an enclosed high island lagoon in Guam. *Micronesica*, **11**:127-148.

- Keast, A., and J. Harker. 1977. Strip counts as a means of determining densities and habitat utilization patterns in lake fishes. *Env. Biol. Fish.*, **1**:181-188.
- Kenchington R.A. 1978. Visual surveys of large areas of coral reefs, pp149-161 in D.R. Stoddart and R.E. Johannes (eds.). *Coral Reefs: Research Methods. Monographs on Oceanographic Methodology*, Vol 5. UNESCO, Norwich, UK.
- Kendall M.G., and A. Stuart 1976. *The Advanced Theory of Statistics. Vol 3: Design and Analysis, and Time Series*. Griffin. 585pp.
- Kimmel J.J. 1985. A new species-time method for visual assessment of fishes and its comparison with established methods. *Env. Biol. Fish.*, **12**:23-32.
- Kuno E. 1986. Evaluation of statistical precision and design of efficient sampling for the population estimation based on frequency of occurrence. *Res. Popul. Ecol.*, **28**:305-319.
- Lincoln R.J., G.A. Boxhall, and P.F. Clark. 1982. *A Dictionary of Ecology, Evolution, and Systematics*. Cambridge University Press, Cambridge. 298pp.
- Lincoln Smith M.P. 1988. Effects of observer swimming speed on sample counts of temperate rocky reef fish assemblages. *Mar. Ecol. Prog. Ser.* **43**:223-231.
- Lincoln Smith M.P. 1989. Improving multispecies rocky reef fish censuses by counting different groups of species using different procedures. *Env. Biol. Fish.* **23**
- Manly B.F.J 1991. *Randomization and Monte Carlo Methods in Biology*. Chapman & Hall, London.
- Manly B.F.J. 1992. *RT: A Program for Randomisation Testing*. Centre for Application of Statistics and Mathematics, University of Otago. 16pp.
- Mapstone B.D. 1988. *The determination of patterns in the abundance of Pomacentrus moluccensis Bleeker on the southern Great Barrier Reef*. PhD Thesis, University of Sydney. 240pp.
- Mapstone B. D. 1992. *Flexible Decision Rules for Hypothesis Testing in Environmental Science. Abstracts of International Environmental Biometrics Conference*, Vol. 2. Sydney. December 1992.
- Mapstone B.D. 1995. *Scalable decision rules for environmental impact studies: effect size, Type I, and Type II errors*. *Ecological Applications* **5**(2): 401-410.
- Mapstone B.D. 1996. *Scalable decision criteria in environmental impact assessment: effect size, Type I, and Type II errors*, in Schmitt R. J. and C. W. Osenberg (eds). **Detecting Ecological Impacts: Concepts and Applications in Coastal Habitats**. Academic Press.
- McArdle B.H., K.J. Gaston, & J.H. Lawton. 1990. Variation in the size of animal populations: Patterns, problems and artefacts. *J. Anim. Ecol.*, **59**,439-454.
- McCormick M.I., and J.H. Choat. 1987. Estimating total abundance of a large temperate-reef fish using visual strip transects. *Mar. Biol.*, **96**:469-478.
- Morin A. 1985. Variability of density estimates and the optimization of sampling programs for stream benthos. *Can. J. Fish. Aquatic. Sci.*, **42**:1530-1534.
- Pielou, E.C. 1977. *Mathematical Ecology*. John Wiley and Sons, New York. 385pp.
- Pringle J.D. 1984. Efficiency estimates for various quadrat sizes used in benthic sampling. *Can. J. Fish. Aquat. Sci.*, **41**:1485-1489.
- Resh V.H. 1979. Sampling variability and life history features: basic considerations in the design of aquatic insect studies. *J. Fish. Res. Board Can.*, **36**:290-311.

- Russ G.R. 1984. *Effects of fishing and protective management on coral reefs at four locations in the Visayas, Philippines (Phase III)*. UNEP-NRMC Coral Reef Monitoring Project. Silliman University. Philippines. 54pp.
- Russ G.R. 1989. Distribution and abundance of coral reef fishes in the Sumilon Island reserve, central Philippines, after nine years of protection from fishing. *Asian Mar. Biol.*, **6**:59-71.
- Russell B.C., F.H. Talbot, G.R.V. Anderson, and B. Goldman. 1978. Collection and sampling of reef fishes, pp.329-345. in D.R. Stoddart and R.E. Johannes (eds.). *Coral Reefs: Research Methods. Monographs on Oceanographic Methodology, Vol 5*. UNESCO, Norwich, UK.
- Sale P.F., and W.A. Douglas. 1981. Precision and accuracy of visual census techniques for fish assemblages on coral patch reefs. *Env. Biol. Fish.*, **6**:333-339.
- Sale P.F., and B.J. Sharp. 1983. Correction for bias in visual transect censuses of coral reef fish. *Coral Reefs*, **2**:37-42.
- Samoilys M. & G. Carlos. 1992. *Development of an Underwater Visual Census Method for Assessing Shallow Water Reef Fish Stocks in the South West Pacific. Report to AIDAB*. 100pp.
- Sanderson S.L., and A.C. Solonsky. 1980. A comparison of two visual survey techniques for fish populations. *Pac. Sci.* **34**:237.
- SAS. 1990. *SAS Procedures Guide*. SAS Institute. 705pp.
- SAS. 1992 *SAS Technical Report P229 - SAS/STAT Software: Changes and Enhancements*. SAS Institute. 620pp.
- Scheffè, H.A. 1959. *The Analysis of Variance*. Wiley, New York 477 pp.
- Short j. & P. Bayliss. 1985. Bias in aerial survey estimates of kangaroo density. *J. App. Ecol.*, **22**:415-422
- Sokal R.R., and F.J. Rohlf. 1981. *Biometry*, 2nd ed. Freeman Co., New York. 859pp.
- Southwood T.R.E. 1966. *Ecological Methods*. Chapman and Hall, London.
- St. John J., G.R. Russ, and W. Gladstone. 1990. Accuracy and bias of visual estimates of numbers, size structure and biomass of a coral reef fish. *Mar. Ecol. Prog. Ser.*, **64**:253-262.
- Thresher, R.E., and J.S. Gunn. 1986. Comparative analysis of visual census techniques for highly mobile, reef-associated piscivores (Centrarchidae). *Environ. Biol. Fish.*, **17**:93-116.
- Underwood A.J. 1981. Techniques of analysis of variance in experimental marine biology and ecology. *Oceanogr. Mar. Biol. Ann. Rev.*, **19**:513-605.
- Weibe, P. H. 1971 A computer model study of zooplankton patchiness and its effect on sampling error. *Limnol. Oceanogr.* **16**, 29-38.
- Weibe, P. H., & Holland, W. R. 1968. Plankton patchiness: effects on repeated net tows. *Limnol. Oceanogr.* **13**, 315-321.
- Weinberg, S. 1981 *Bijdragen tot de Dierkunde*
- Williams D.McB., and A.I. Hatcher. 1983. Structure of fish communities on outer slopes of inshore, mid-shelf and outer shelf reefs of the Great Barrier Reef. *Mar. Ecol. Prog. Ser.*, **10**:239-250.
- Winer B.J. 1971. *Statistical Principles in Experimental Design*. McGraw-Hill Kogakusha, Tokyo.
- Winer B.J., D.R. Brown, and K.M. Michels. 1992. *Statistical Principles in Experimental Design*. 3rd Ed. McGraw-Hill Kogakusha, Tokyo. 1057pp.

GBRMPA LIBRARY



R005251

PA LIBRARY

No.

Cost

Accession

No.

Call No.

577.880

9943

INV

1998

COPY 2

ABN