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Part III

Assessment of the effectiveness of marine protected areas

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6 · *ECOLOGY – Assessing effects of marine protected areas: confounding in space and possible solutions*

Craig W. Osenberg, Jeffrey S. Shima, Sonja L. Miller, and Adrian C. Stier

6.1 Introduction

Marine protected areas (MPAs) are increasingly used as tools to conserve biodiversity, manage fishing effort, and facilitate recovery of degraded ecosystems (Roberts *et al.*, 2001; Sale, 2002; Hastings and Botsford, 2003; Cinner *et al.*, 2006). Marine protected areas are expected to produce long-lasting local increases in the density, size, diversity, and/or productivity of marine organisms within MPA boundaries (Russ and Alcala 1996; Claudet *et al.*, 2006; Chapter 2), as well as regional increases outside of the MPA via spillover from the MPA to sites that continue to be fished (Chapter 3). Assessment of the *actual* effects of MPAs relative to these goals is essential for adaptive management and decision-making. Despite a large number of assessments, however, there remains considerable uncertainty about the actual effects of MPAs (e.g., Osenberg *et al.*, 2006).

The central question (and challenge) that underlies the assessment of any MPA is easy to express but difficult to measure: "How does the state of the system (e.g., density or size of a target organism) within the MPA compare to the state that would have existed had the MPA never been established?" The former can be directly observed; however, the latter cannot and must therefore be estimated through indirect means (Stewart-Oaten *et al.*, 1986; Osenberg and Schmitt, 1996; Stewart-Oaten, 1996a,

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1996b; Stewart-Oaten and Bence, 2001; Osenberg *et al.*, 2006). Several different approaches have been proposed to estimate this baseline (Box 6.1; see also Osenberg *et al.*, 2006), yet, considerable controversy still exists (Box 6.2).

Box 6.1 *Types of assessment designs*

ASSESSMENTS, in contrast to experiments, aim to quantify site-specific effects (e.g., due to a particular MPA) rather than average effects (e.g., across many MPAs). General goals and approaches are described by Stewart-Oaten *et al.* (1986), Stewart-Oaten (1996a, 1996b), Stewart-Oaten and Bence (2001); issues specific to MPAs are addressed by Osenberg *et al.* (2006). The basic designs are summarized below. We assume there are two sites: an "Impact" site (e.g., the region within an MPA boundary) and a "Control" site (a region outside of the MPA). Each site may consist of multiple subsites.

CONTROL–IMPACT (or After-only): The Control and MPA site are assumed to be identical in the absence of an effect of the MPA. Under this assumption, the difference between the Control and MPA site after the establishment of the MPA provides an estimate of the effect of the MPA. In reality this estimated effect confounds the effect of the MPA with other sources of spatial variation (e.g., due to pre-existing differences in habitat). This is the most common assessment design.

BEFORE–AFTER (Box and Tiao, 1975): An MPA site is sampled Before and After enforcement. In the simplest approach, the difference from Before to After is a measure of the effect of the MPA. If the MPA site is sampled only once Before and once After, then the effect of the MPA is confounded with other sources of temporal variation (e.g., larval supply). If a time-series is obtained, and serial correlation is accounted for, then better estimates can be obtained; however, because ecological time-series are noisy, this approach is often problematic.

BEFORE–AFTER–CONTROL–IMPACT (BACI): the Control and Impact sites are sampled both Before and After establishment of the MPA. The change in the MPA site from Before to After, relative to the change at the Control site, provides a measure of the effect. In general, BACI designs (especially BACIPS; see below) provide more reliable measures of effects than either Control–Impact or Before–After designs. There are several variants of this approach.

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- BACI (WITHOUT TIME SERIES) (Green, 1979): This design uses only a single sampling time Before and After. Thus, it assumes that the Impact and Control site will respond identically through time in the absence of an MPA effect, and therefore confounds the effect of the MPA with other sources of spatiotemporal variation. The other BACI designs use time-series Before and After to avoid this problem.
- BACI PAIRED SERIES (BACIPS) (Stewart-Oaten *et al.*, 1986): The Control and MPA sites are sampled at the same times (or nearly so) so that shared temporal effects can be removed by differencing (e.g., Equation 6.1). A change in the differences from Before to After provides one possible measure of the effect.
- BEYOND BACI (Underwood, 1992, 1994): Multiple Control and MPA sites are sampled multiple times Before and After, but the sites are not sampled at the same points in time. Although championed for introducing spatial replication, this design-based approach assumes random sampling (see Box 6.2).
- PREDICTIVE BACIPS (Bence *et al.*, 1996): Similar to BACIPS, this design provides estimates of effects that can vary with the ambient environmental conditions (as indexed by the state of the Control).

Box 6.2 *Design-based versus model-based assessment frameworks*

Two general frameworks have been discussed in assessments of ecological impacts and/or restorations: *design-based* and *model-based* approaches (see Edwards, 1998; Stewart-Oaten and Bence 2001; Benedetii-Cecchi and Osio, 2007; Stewart-Oaten, 2008).

Before–After and BACIPS designs are typically model-based. Box and Tiao (1975) provide the classic application of the Before–After approach, in which they modeled the dynamics of ozone in downtown Los Angeles and quantified the effects of two interventions (e.g., the opening of the 405 Freeway which diverted traffic around the city). Because data were plentiful and ozone dynamics were well behaved, the Before–After approach worked well. In contrast, ecological time-series are notoriously noisy and sparse, and ecological parameters are poorly behaved. In BACIPS, a Control site is therefore used to improve the modeled dynamics of the Impact site; the Control plays the role of a covariate (Bence *et al.*, 1996; Stewart-Oaten and Bence, 2001;

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Osenberg *et al.* 2006). This approach is flexible and allows a variety of functional forms of impacts to be modeled: e.g., step changes vs. gradual temporal responses (Box and Tiao, 1975) or effects that vary with environmental conditions (Bence *et al.*, 1996).

These model-based analyses have been criticized because they do not explicitly incorporate spatial replication (Hurlbert, 1984; Underwood, 1992, 1994; Bulleri *et al.*, 2008). Before–After and BACIPS analyses *can* use data from multiple Control sites, but they do not require it (Stewart-Oaten 1996b). Design-based approaches are appealing because they fit into classic experimental design contexts (e.g., using analysis of variance) with which most field ecologists are familiar. Most Control–Impact and Beyond-BACI designs, which rely upon spatial replication, are examples of this school of thought. However, such design-based approaches assume random assignment. In a classic experiment, sites are assigned at random to the two treatments. In our context, that is not the case: the MPA site is not selected at random from candidate sites that define the pool of all possible sites. Instead, the MPA site is usually selected for very specific reasons, many of which cannot be clearly articulated. Control sites are often then selected a posteriori by the investigator. Put differently, the MPA site and Control site(s) are not expected (in the statistical sense) to be the same in the absence of an "effect," unlike the situation in standard experiments with random assignment.

These philosophical issues are not simple, and we doubt the underlying issues will be resolved in the short-term (e.g., see the debate between Stewart-Oaten [2008] and Bulleri *et al.* [2008]). Benedetti-Cecchi and Osio (2007) have attempted to integrate the design- and model-based approaches for impact assessments. We applaud their effort, but note that their approach remains design-based with random assignment assumed (albeit with covariates, not unlike our habitat-based adjustments). Furthermore, measures of effect sizes are difficult to extract from their ANOVA results. One notable advantage of the model-based approach is that it provides direct estimates of effect sizes, usually as specific parameters in the fitted model (e.g., Box and Tiao [1975]; see also Osenberg *et al.* [2006] for a simple Before–After example using MPA data). Future resolution of these debates will likely come as ecologists embrace model-based approaches and estimation more generally (Clark, 2007; Bolker, 2008).

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Most assessments of MPAs rely upon a Control–Impact design (Cole *et al.*, 1990; Roberts, 1995; McClanahan *et al.*, 1999; Halpern, 2003; Westera *et al.*, 2003; Osenberg *et al.*, 2006), in which the MPA (i.e., "Impact" or unfished) site is compared to a nearby Control (i.e., fished) site(s). A difference in response (e.g., in fish density) is taken as evidence of an effect of the MPA. In a well-known and influential meta-analysis of MPA effects, Halpern (2003) reported a 91% increase in the density and a 192% increase in biomass of target species inside MPAs relative to Controls. This finding has been heralded as strong evidence for the effectiveness of MPAs. However, Control–Impact designs (which comprised over 70% of the studies summarized by Halpern: Osenberg *et al.*, 2006) implicitly assume that, in the absence of the MPA, measured variables will be identical in the MPA and Control sites. Yet for many reasons, the organisms present at any two sites are certain to vary in traits such as size, density, or diversity. Furthermore, siting of MPAs is a complex and challenging task, involving political, socio-economic, and ecological considerations: MPAs are not located at random among candidate sites and as a result, MPAs will differ in many ways from nearby areas. For example, MPAs may be established in places deemed to be ecologically valuable because the greater habitat complexity at the locale may provide important habitat to fishes. Assessments that do not take this potential variation into account (e.g., Control–Impact designs) may give misleading estimates of the effect of protection (Osenberg *et al.*, 2006).

Thus, despite considerable empirical effort, we are left with an unresolved question: are observed differences inside vs. outside of MPAs indicative of a positive effect of the MPA on local fish densities (or other variables), or do these differences reflect natural spatial variation that potentially existed prior to MPA establishment? Adding to this controversy is the observation that MPA effects appear to arise quickly, often being present at the very earliest sampling date after establishment of the MPA (Halpern and Warner, 2002). This result is exactly what would be expected by pre-existing site differences if particular types of sites (e.g., with greater initial fish density or greater habitat complexity) were selected for protection. Despite these obvious limitations, most of the available data to assess the efficacy of MPAs come from Control–Impact designs (Osenberg *et al.*, 2006). Unfortunately, there has been inadequate acknowledgement of these limitations.

In contrast to Control–Impact designs, BACIPS (Before–After–Control–Impact Paired Series) designs can provide less biased estimates of effects because they deal with many extraneous sources of spatial and temporal variability that limit other assessment designs (Stewart-Oaten *et al.*, 1986; Osenberg *et al.*, 2006). However, BACIPS studies have rarely been implemented (but see Castilla and Bustamante, 1989; Lincoln-Smith *et al.*, 2006; Shears *et al.*, 2006), due in part to the need for prior planning and the collection of a time-series of data from Before the establishment of the MPA (Osenberg *et al.*, 2006). Even studies with Before data tend to have, at most, only one sample taken prior to establishment of the MPA (e.g., Roberts *et al.*, 2001; McCook *et al.*, 2010). Better approaches are needed (Osenberg *et al.*, 2006).

Given the preponderance of Control–Impact studies in the literature, the continued opportunity for new Control–Impact studies (i.e., based on new studies of existing MPAs), and the obvious conceptual limitations of Control–Impact designs, it would be valuable to determine if existing conclusions about the beneficial effects of MPAs have been in error. The natural variability that contaminates Control–Impact studies not only has the potential to be confounded with MPA effects, but it may also add noise to more appropriate designs (e.g., BACIPS), thus limiting their ability to detect effects. Therefore, it also would be useful to ask if other approaches can be taken that could achieve more reliable results, even in the absence of Before data. Here, we have two goals: first, we determine whether past inferences of MPA effectiveness (e.g., Halpern, 2003) arose from the use of poor designs (Control–Impact assessments) that misattributed siting effects (i.e., spatial differences) to MPA effects (see also Halpern *et al.*, 2004). Second, we examine how the use of habitat covariates might improve assessment designs, for example, by removing the spatial variation that plagues studies lacking Before data.

6.2 "Effects": due to siting bias or protection?

6.2.1 Goals

To determine if results from Control–Impact designs have biased our conclusions about the effectiveness of MPAs, we compiled data from published assessments that included Before data (see Table 6.1). From these BACIPS studies, we estimated effect sizes using either the full dataset (using Before and After data) or only the After data (i.e., ignoring information from the Before period and pretending the data came from a Control–Impact design). We also extracted data from Halpern's meta-analysis, which was based mostly on Control–Impact (CI) studies

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Table 6.1 *Studies of MPA effects that included Before data and were used in our analyses*

Original paper	Marine reserve system(s)	Response: target group(s) (data source from original paper)	No. effect sizes (i.e., studies)	No. sampling dates (Before, After)	Other notes
Roberts *et al.* (2001)	St. Lucia	Biomass: commercially important fishes (Fig. 2)	1	1, 3	
Nardi *et al.* (2004)	Easter; Wallabi	Density: *Choerodon rubescens*; *Plectropomus leopardus* (Figs. 2d, 3d, 4d, 5d)	4	1, 4	
Francini-Filho and Moura (2008)	Marine Extractive Reserve of Morumbau	Biomass: *Mycteropterca bonaci; Carangoides crysos; Ocyurus chrysurus; Anisotremus surinamensis; A. virginicus; Scarus trispinosus; Sparisoma amplum; S. axillare* (Fig. 2); non-target fishes (Fig. 3)	9	1, 4	Due to zeros, analyses based on biomass + 0.07. Control data were means of unprotected sites at 400–800 m and 800–1200 m
Claudet *et al.* (2006)	Cote Bleu Marine Park (Couronne MPA)	Density: *Coris julis*; *Serranus cabrilla*; *Symphodus doderleini* (Fig. 4a, c, d and authors)	3	1, 2	Based on medians; due to zeros for *S. doderleini*, analyses based on density + 1
Lincoln-Smith *et al.* (2006)	Arnavon Islands	Density: *Holothuria atra; H. fuscogilva; H. fuscopunctata; Stichopus chloronotus; Tectus pyramis; Thelanota anax; Tridacna maxima; Trochus niloticus* (authors)	8	3, 3	

(*cont.*)

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Table 6.1 (*cont.*)

Original paper	Marine reserve system(s)	Response: target group(s) (data source from original paper)	No. effect sizes (i.e., studies)	No. sampling dates (Before, After)	Other notes
Russ and Alcala (2004)	Sumilon Island	Biomass: large Serranidae (Epinephelinae), Lutjanidae, Lethrinidae, and Carangidae (Fig. 1a)	1	3, 7	
Shears *et al.* (2006)	Tawharanui Marine Park	Density: *Jasus edwardsii* (Fig. 2 and authors)	1	7, 6	Due to zeros, analyses based on density + 0.2
McClanahan and Kaunda-Arara (1996)	Mombasa Marine National Park	Biomass: Acanthuridae; Balistidae; Chaetontidae; Labridae; Lethrinidae; Pomacanthidae; Pomacentridae; Scaridae; others (Fig. 4)	9	1, 3	Due to zeros, analyses based on biomass + 0.07
Castilla and Bustamante (1989)	Punta El Lacho	Biomass and density: *Durvillaea antarctica* (Fig. 3a, b)	2	4, 12	
Castilla and Durán (1985)	Punta El Lacho	Density: *Concholepas concholepas* (Figs. 1 and 3)	1	1, 4	Combined density in center and lower sites
Samoilys *et al.* (2007)	Asinan; Batasan	Density: Chaetodontidae; Labridae; Pomacentridae; Scaridae; top trophic-level fishes (Figs. 3, 4, 5, 6, 7)	5	1, 4	Analyses contrasted "Inside" with the "Control" sites (not "Outside")

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(i.e., that lacked Before data). Thus we had three different effect sizes: (1) CI comparisons from CI designs; (2) CI comparisons from BACIPS designs; and (3) BACIPS comparisons based on BACIPS designs. Halpern *et al.* (2004) previously noted the possible confounding of siting and MPA effects. They addressed this possibility by using the few existing BACIPS studies to determine if Control and Impact sites were similar *prior* to establishment of the MPA. Our approach differed from theirs in two important ways: we calculated effect sizes, and we recognized that by taking Before data, investigators might have been more likely to choose similar Control and Impact sites than in situations when Before data were not obtained. Thus, we hypothesized that:

(i) if siting is biased and MPAs are often located in the best locations, then the effect sizes estimated from Control–Impact studies will be larger than the effect sizes from BACIPS studies;
(ii) if sampling Before enables investigators to pick more "similar" sites (and thus reduce the pre-existing differences), then effect sizes using a Control–Impact comparison will be smaller for BACIPS studies than for Control–Impact studies (i.e., the BACIPS studies will have reduced siting bias relative to CI studies).
(iii) If siting bias exists even in BACIPS studies (a possibility that does not compromise the BACIPS analyses), then calculated effects will be greater using Control–Impact comparisons (relative to BACIPS comparisons) using the same data set.

6.2.2 Methods

We found 11 papers that sampled organismal or biomass density Before and After establishment of an MPA at sites both inside and outside of the MPA (Table 6.1). From the data in these papers, we estimated 44 effect sizes for different MPAs and/or species (herein called a "study"); 23 of these effects were based on density (e.g., number of individuals/m^2) and 21 were based on biomass (e.g., kg/m^2). Given the paucity of studies with Before data, we used multiple estimates for different target species from the same MPA investigation. We separated the studies using different response variables (biomass vs. density) because biomass responses were expected to be greater owing to the combined responses of density and organismal size (Halpern 2003).

For the *i*th study, sampled in the *P*th Period (Before or After), during the *t*th sampling date, we calculated the difference in log-transformed

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densities between the MPA and Control site (if multiple subsites were available, we first averaged those):

$$Di,\ P,\ t = \ln(F\text{MPA},\ i,\ P,\ t) - \ln(F\text{Control},\ i,\ P,\ t)$$
$$= \ln(F\text{MPA},\ i,\ P,\ t/F\text{Control},\ i,\ P,\ t), \tag{6.1}$$

where F is density or biomass of the monitored group (e.g., a species of fish). In a few cases (Table 6.1), $F = 0$, so we added a constant to each estimate of F in that study (generally corresponding to one individual per sampling unit, or the smallest observed biomass in the data set). We then averaged those differences within each period for each study and response variable (i.e., density or biomass) and calculated effects and variances using either the After data only (i.e., mimicking a CI design) or using the After and Before data:

$$E_{i,\text{CI}} = \frac{\sum_{t=1}^{n_A} D_{i,A,t}}{n_A}\ E_{i,\text{CI}} = \frac{\sum_{t=1}^{n_A} D_{i,A,t}}{n_A} - \frac{\sum_{t=1}^{n_B} D_{i,B,t}}{n_B} \tag{6.2}$$

$$V(E_{i,\text{CI}}) = \sum_{t=1}^{n_A} \left(D_{i,A,t} - D_{i,A}\right)^2 \Big/ n_A\,(n_A - 1)\ V(E_{i,\text{CI}})$$
$$= \sum_{t=1}^{n_A} \left(D_{i,A,t} - \bar{D}_{i,A}\right)^2 \Big/ n_A - 1 \tag{6.3}$$

$$E_{i,\text{BACIPS}} = \frac{\sum_{t=1}^{n_A} D_{i,A,t}}{n_A} - \frac{\sum_{t=1}^{n_B} D_{i,B,t}}{n_B}\ E_{i,\text{BACI}} +$$
$$= \frac{\sum_{t=1}^{n_A} D_{i,A,t}}{n_A} - \frac{\sum_{t=1}^{n_B} D_{i,B,t}}{n_B} \tag{6.4}$$

$$V\left(E_{i,\text{BACIPS}}\right) = \sum_{t=1}^{n_A} \left(D_{i,A,t} - D_{i,A}\right)^2 \Big/ n_A\,(n_A - 1)$$
$$+ \sum_{t=1}^{n_B} \left(D_{i,B,t} - D_{i,B}\right)^2 \Big/ n_B\,(n_B - 1)$$
$$V\left(E_{i,\text{CI}}\right) = \sum_{t=1}^{n_A} \left(D_{i,A,t} - \underset{i,A}{\bar{D}}\right)^2 \Big/ n_A - 1$$
$$+ \sum_{t=1}^{n_B} \left(D_{i,B,t} - D_{i,B}\right)^2 \Big/ n_B - 1 \tag{6.5}$$

where n_P is the number of sampling dates in period P. If $n_B = 1$, Equation 6.5 was modified to reflect the lack of multiple observations in

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the Before period:

$$V\left(E_{i,\mathrm{BACIPS}}\right) = \left[\frac{\sum_{t=1}^{n_A}\left(D_{i,A,t} - D_{i,A}\right)^2}{n_A - 1}\right]\left[1 + \frac{1}{n_A}\right]$$

$$V\left(E_{i,\mathrm{CI}}\right) = \sum_{t=1}^{n_A}\left(D_{i,A,t} - D_{i,A}\right)^2 \Big/ n_A - 1 + \sum_{t=1}^{n_B}\left(D_{i,B,t} - D_{i,B}\right)^2 \Big/ n_B - 1 \qquad (6.6)$$

We also summarized among studies by calculating weighted means across studies, using a random effects model in which the weights were the inverse of the within-study variance and the among study variance. All means and confidence intervals were back-transformed with exponentiation to yield final relative effect sizes: e.g., an effect of 1 indicates no effect, and an effect of 2 indicates a doubling in density. Densities that required the addition of a constant were exponentiated, but the constant was retained (the influence of the constant was small relative to the overall effect). Equations (6.2) and (6.3) differ slightly from standard Control–Impact designs because the BACIPS studies we used all had >1 After sampling date. We therefore averaged the differences through time; the resulting confidence intervals will generally be larger than those reported in the original study because we used dates as replicates rather than spatial subsamples.

Finally, we also extracted effect sizes from Halpern's (2003) meta-analysis of effects on organismal density, which was based on MPA studies derived almost entirely from Control–Impact studies. We used this as a sample of Control–Impact effect sizes from studies that did not have any Before sampling. An overall mean and confidence interval was obtained based on unweighted analyses (because variances from individual studies were not provided). Use of unweighted effects from different studies should increase the variance of the average effects size (and the associated confidence interval), but not the expected effect size itself.

6.2.3 Results

Effects of MPAs were variable both within and among studies, giving rise to considerable uncertainly in patterns (Figures. 6.1 and 6.2). All meta-analyses (CI effect size from CI studies, CI effect size from BACIPS studies, and BACIPS effect size from BACIPS studies; for both density and biomass) indicated demonstrable beneficial effects of MPAs

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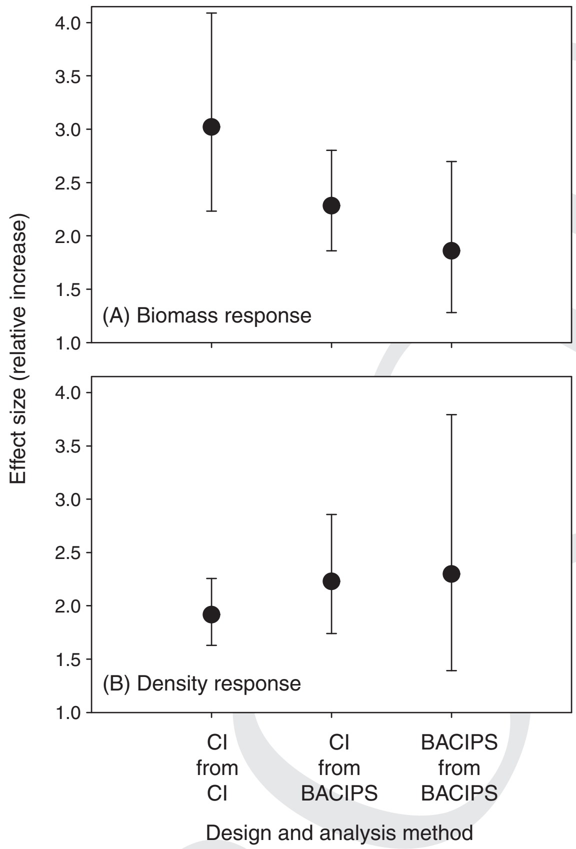

Figure 6.1 Effect sizes (ratio of reserve to non-reserve sites) based on (A) biomass and (B) density of study species from Control–Impact (CI) and Before–After–Control–Impact Paired Series (BACIPS) designs. The data for CI designs were digitized from Halpern (2003) and include a small number of Before–After studies. The data from BACIPS studies were used to calculate effect sizes based on both CI calculations (using only the After data) and BACIPS calculations (using data Before and After). Sample sizes are: (A) 32 (CI) and 21 (BACIPS), and (B) 65 (CI) and 23 (BACIPS). Plotted are means and 95% confidence intervals. For BACIPS studies, means were weighted based on the inverse of the variance of the individual effect sizes.

(i.e., confidence intervals that excluded the null hypothesis of equality inside and outside the MPA: Figure 6.1); however, individual studies varied in their effects and in the uncertainty of those effects (Figure 6.2). The high variability made it difficult to clearly assess our three core hypotheses.

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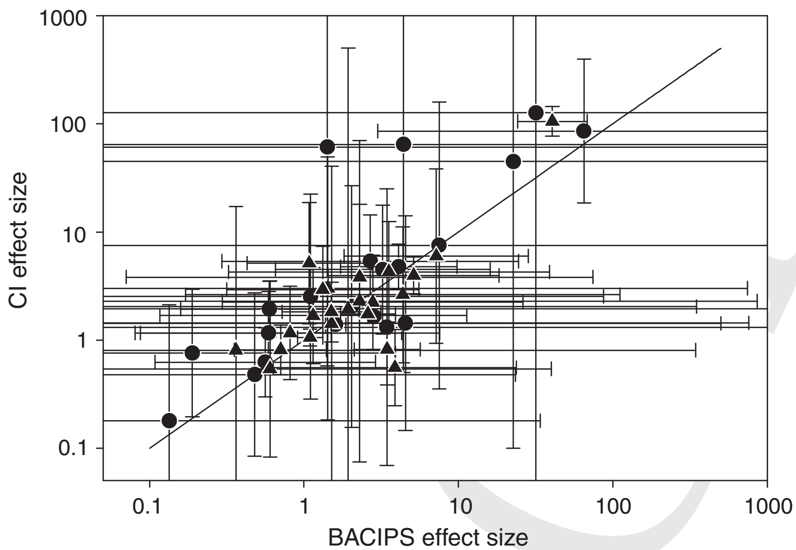

Figure 6.2 Effect sizes (ratio of MPA response to Control response) from studies of MPAs ($n = 44$) that included Before data, based on biomass (circles) and density (triangles) responses. Effect sizes for each species were estimated using data from the After period only (CI effect size) and using the full BACIPS design (BACIPS effect size). All BACIPS studies had >1 sampling date in the After period (range: 2–7), but only 12 studies had >1 sampling date in the Before period (range: 1–7). Plotted are the means and 95% confidence intervals, and the 1 : 1 line along which the two effect sizes are equal; excessively large error bars were truncated for presentation.

Analysis of BACIPS effect sizes from BACIPS studies revealed an approximately 86% (95% confidence interval: 28–170%) increase in biomass within the MPAs relative to the Controls (Figure 6.1a). The effect size based on CI comparisons using the same BACIPS studies was larger but not demonstrably so (average effect: 128% increase in biomass). Both effect sizes calculated from BACIPS studies exclude the threefold increase that is often referred to in citations of Halpern's (2003) meta-analysis (i.e., primarily CI studies).

Based on the results for biomass responses, we conclude that BACIPS and CI studies likely yield different effect size estimates, although we caution that the high uncertainty precludes a definitive evaluation. The similarity in effects using CI and BACIPS comparisons from BACIPS studies suggests that when investigators conduct Before sampling they tend to select sites that are relatively similar in initial conditions, presumably because they are able to see the sites during their planning phases and

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a priori select sites more similar in their biological and physical attributes. The apparent disparity in effects from BACIPS studies (∼100% increase using both BACIPS and CI comparisons) vs. CI studies (∼200% increase) suggests that either: (1) when investigators did not have the opportunity to sample Before MPA establishment (i.e., when they conducted CI studies), they tended to select sites that were more disparate in initial (but unknown) conditions; and/or (2) investigators are more likely to publish negative results in more recent years, and because BACIPS studies have generally been published since the publication of Halpern's review, the BACIPS studies may be more likely to include negative results. Both explanations likely play some role. If the first explanation is correct, then some of the results from previously published CI studies using biomass responses arose from a combination of MPA effects and pre-existing site differences. We suspect that previous CI studies may have overestimated the effectiveness of MPAs by about 100% – i.e., rather than generating an approximately 200% increase in biomass MPAs appear to lead to only a 100% increase. The effectiveness of MPAs remains well established – it just may not be as strong as previous analyses suggested.

The results based on density (instead of biomass) are even more ambiguous (Figure 6.1B). These data provide no support for siting effects, although the large uncertainty (e.g., the confidence interval for BACIPS effects from BACIPS studies ranges from a 39% to 279% increase) makes any inference problematic. No doubt, we could resolve some of this variation by considering attributes of the species that were studied (e.g., whether they are heavily fished, sedentary vs. mobile, herbivores vs. piscivores: Micheli *et al.*, 2004; Claudet *et al.*, 2010), but the limited number of studies preclude such an analysis.

The effect sizes based on CI and BACIPS comparisons from the same BACIPS study were positively correlated (Figure 6.2), and followed a 1:1 relationship. Further, BACIPS studies yielding larger effect sizes based on BACIPS comparisons tended also to yield larger effects using only the After data (i.e., based on a CI comparison). There was no indication that effects were smaller or absent in the BACIPS comparison relative to a CI comparison from these BACIPS studies. This suggests that siting biases were small or non-existent in the BACIPS studies. That said, there were few studies with Before data, so these results must be interpreted with caution.

Perhaps more important to the design of assessment studies is the substantial uncertainty in the effect estimated from any particular BACIPS

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study (Figure 6.2): only 9/44 effect sizes based on BACIPS had confidence intervals that excluded 1.0 (i.e., that led to the rejection of the null hypothesis of no effect), and many had confidence intervals that spanned several orders of magnitude in possible effects! This uncertainty reflects not only the tremendous spatiotemporal variation (i.e., variation in the difference: Equation (6.1)) but also the limited number of sampling dates in the BACIPS studies (Table 6.1). This large uncertainty was rarely discussed in the original BACIPS papers because effect sizes were seldom determined (although some investigators carried out careful and thorough null hypothesis tests within a BACIPS framework: e.g., Lincoln-Smith *et al.*, 2006).

6.3 Habitat as a covariate: helpful or misleading?

Fishes and invertebrates often respond to the availability of habitat (e.g., Carr 1994; Holbrook *et al.*, 2000; Munday, 2002; Galst and Anderson, 2008). Thus, one source of pre-existing differences in the density of organisms (when measured per unit of transect without regard to substrate) could be the availability of their habitat. If variation in habitat explains variation in organism density then habitat could be used to adjust density,[1] possibly giving rise to more accurate assessments, even in the absence of Before data. Indeed, if siting effects bias CI assessments, and if these siting effects are manifest via differences in habitat availability, then the use of habitat-adjusted densities could eliminate much of the concern about CI designs.

The efficacy of using habitat-adjusted responses will ultimately depend upon the pathways that couple the MPA, organism, and habitat. We envision five scenarios (Figure 6.3), although others also are possible. These five scenarios all assume that the MPA has an effect, but they differ in whether the direct effect is on the organismal density, habitat, or both. They also differ in whether habitat affects organismal density (e.g., compare scenarios C and E).

Given these scenarios (Figure 6.3), we compared the effect sizes derived from CI or BACIPS studies using either raw densities (per unit area), or densities adjusted by habitat availability (Table 6.2). In all cases,

[1] We assume that habitat adjustment is achieved by dividing organismal density by the areal extent of habitat (i.e., habitat availability), although more general, non-linear corrections might be more appropriate: e.g., Packard and Boardman (1988).

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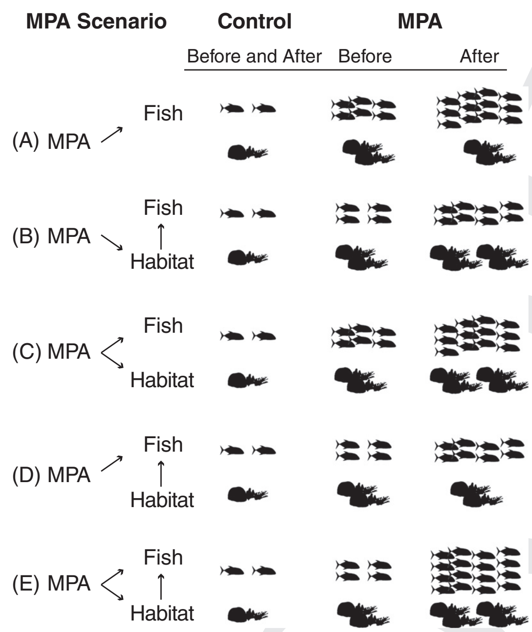

Figure 6.3 Models of fish and habitat responses to MPAs. (A) MPA increases fish abundance independent of habitat availability; (B) MPA increases fish abundance indirectly by increasing habitat availability; (C) MPA increases fish and habitat availability independently; (D) fish increase in response to MPA but also are habitat-dependent; (E) fish and habitat are positively affected by the MPA and fish are habitat-dependent. We assume a 2 : 1 fish : habitat relationship in scenarios where fish are habitat-dependent, that fish and habitat are more common at the MPA prior to MPA establishment (i.e., there are pre-existing site differences), that the Control site does not change from Before to After, and that the MPA has a twofold direct effect on fish (and/or habitat).

we assumed that there were pre-existing differences among sites with the MPA starting out with higher densities of target organisms and more habitat.

In these scenarios, a CI design using unadjusted densities always overestimated the actual effect of the MPA because the pre-existing differences were incorrectly attributed to effects of the MPA. Habitat adjustment sometimes reversed and sometimes eliminated this bias. However, adjustment only worked if habitat affected organismal density because the adjustment corrected for site differences in habitat, thus putting all sites on equal footing. If habitat was unaffected by the MPA, then the effect

Table 6.2 *Illustration of the effects of assessment designs on effect sizes under different assumptions about the relationships between MPAs* (M)*, fish* (F)*, and habitat* (H)*. We contrast Control–Impact and BACIPS designs, with two different response variables: unadjusted fish density* (F) *and density adjusted by habitat availability* (H)*. We assume that MPAs are placed in sites with greater habitat and fish density (which creates the pre-existing differences in* F *and* H*), and that the MPA increases* F *and/or* H *(arrows indicate positive effects). Figure 6.3 provides the expected patterns of response at the MPA and Control sites*

Scenario	Control–Impact		BACIPS		
Response variable	Fish	Fish/Habitat	Fish	Fish/Habitat	
Effect Formula	$\frac{F_{A,MPA}}{F_{A,C}}$	$\frac{F_{A,MPA}/H_{A,MPA}}{F_{A,C}/H_{A,C}}$	$\frac{F_{A,MPA}/F_{A,C}}{F_{B,MPA}/F_{B,C}}$	$\frac{F_{A,MPA}/H_{A,MPA}}{F_{A,C}/H_{A,C}} \Big/ \frac{F_{B,MPA}/H_{A,MPA}}{F_{B,C}/H_{B,C}}$	Reality
(A) $M \rightarrow F$	$\frac{12}{2} = 6$ Biased High	$\frac{12/2}{2/1} = 3$ Biased High	$\frac{12/2}{6/2} = 2$ **Correct**	$\frac{12/2}{2/1} \Big/ \frac{6/2}{2/1} = 2$ **Correct**[a]	2× increase
(B) $M \rightarrow H \rightarrow F$	$\frac{8}{2} = 4$ Biased High	$\frac{8/4}{2/1} = 1$ Biased Low	$\frac{8/2}{4/2} = 2$ **Correct**	$\frac{8/4}{2/1} \Big/ \frac{4/2}{2/1} = 1$ Biased Low	2× increase
(C) $M \rightarrow F$, $M \rightarrow H$	$\frac{12}{2} = 6$ Biased High	$\frac{12/4}{2/1} = 1.5$ Biased Low	$\frac{12/2}{6/2} = 2$ **Correct**	$\frac{12/4}{2/1} \Big/ \frac{6/2}{2/1} = 1$ Biased Low	2× increase

(cont.)

Table 6.2 (*cont.*)

Scenario	Control–Impact		BACIPS		Reality
Response variable	Fish	Fish/Habitat	Fish	Fish/Habitat	
Effect Formula	$\frac{F_{A,MPA}}{F_{A,C}}$	$\frac{F_{A,MPA}/H_{A,MPA}}{F_{A,C}/H_{A,C}}$	$\frac{F_{A,MPA}/F_{A,C}}{F_{B,MPA}/F_{B,C}}$	$\frac{F_{A,MPA}/H_{A,MPA}}{F_{A,C}/H_{A,C}} \Big/ \frac{F_{B,MPA}/H_{A,MPA}}{F_{B,C}/H_{B,C}}$	
(D) M F H	$\frac{8}{2} = 4$ Biased High	$\frac{8/2}{2/1} = 2$ **Correct**	$\frac{8/2}{4/2} = 2$ **Correct**[b]	$\frac{8/2}{2/1} \Big/ \frac{4/2}{2/1} = 2$ **Correct**[b]	$2\times$ increase
(E) M F H	$\frac{16}{2} = 8$ Biased High	$\frac{16/4}{2/1} = 2$ Biased Low	$\frac{16/2}{4/2} = 4$ **Correct**	$\frac{16/4}{2/1} \Big/ \frac{4/2}{2/1} = 2$ Biased Low	$4\times$ increase

[a] Habitat-adjusted and unadjusted BACIPS approaches give correct answers, although the adjusted analysis will likely have lower power (by introducing noise produced by variation in habitat).

[b] Habitat-adjusted and unadjusted BACIPS approaches give correct answers, although the adjusted analysis will likely have higher power (by reducing noise produced by variation in habitat-driven fluctuations in fish abundance).

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on adjusted density correctly measured the effect of the MPA on the target population. However, if the MPA also increased habitat availability, then adjustment of density by habitat eliminated some of the effect of the MPA (mediated via habitat) and therefore underestimated the MPA effect (scenario E).

There were different results from BACIPS using raw or adjusted densities. Raw densities always gave the correct expected effect size (Table 6.2), because the habitat adjustment was unnecessary with Before data (the effect of habitat differences was already included in the analysis): the Before period was used to estimate the existing difference between the Control and Impact sites and BACIPS therefore already incorporated the average spatial variation between the two sites (Stewart-Oaten *et al.*, 1986; Osenberg *et al.*, 1996). Adjustment actually led to misleading effect sizes in some cases (Table 6.2: scenarios B, C, and E). If habitat did not affect density, but the MPA increased habitat availability, then adjustment led to a decrease in the response variable, leading to an underestimation of the MPA effect (scenario C). If, on the other hand, the MPA increased habitat and this change also increased organismal density, then habitat adjustment underestimated the MPA effect. The problem here was that the portion of the MPA effect on the target population that was mediated through habitat was "removed" via the habitat adjustment and therefore was not included in the final effect size.

Even when adjusted and unadjusted BACIPS analyses gave the correct effect sizes (scenarios A and D), they potentially differed in their statistical power (Table 6.2). If MPAs increased density directly, but had no effect on habitat, *and* if habitat varied (among subsites within a site or through time) but had no effect on the target population (scenario A), then habitat adjustment would increase variance in adjusted densities and reduce statistical power. In contrast if both the MPA and habitat affected the target population (but not one another: scenario D), then adjustment by habitat would reduce nuisance variation in density, leading to a more powerful analysis.

In general, this discussion suggests that in the absence of more precise information about the dynamics of the system and the likely linkages, BACIPS analyses with unadjusted response variables, may be the most effective. Although CI and BACIPS designs using adjusted density can work (and be more powerful), this is only likely when MPAs do *not* affect habitat, when habitat plays a strong role in determining organismal abundance, and when habitat varies spatiotemporally.

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6.4 Discussion and conclusions

Marine protected areas are a potentially powerful management tool. However, their effectiveness may have been overestimated due to the reliance on assessment designs that confound pre-existing differences with intended effects of protection. Unfortunately, spatial variation always exists (any two sites can be shown to be different if sampled sufficiently), and therefore CI designs will always be problematic. This is true in assessments as well as experiments, where replication and random assignment overcome problems posed by spatial variation. For example, Halpern (2003) saw consistent effects among CI studies. However, these "treatments" were not randomly assigned. Is the observed consistency in effects a reflection of consistent bias in siting or a consistent effect of MPAs? Our analysis (albeit with limited and noisy data) suggests the answer to both questions is an equivocal yes.

One approach to help with future CI assessments is to adjust densities (e.g., counts per transect area) by habitat variables (e.g., percent cover by preferred substrates) to remove sources of spatial variation unrelated to the MPA. This can work, but only in a limited number of situations (Table 6.2). In other situations, it may lead to new sources of bias. For example, fish are known to respond to the amount of biogenic habitat, such as live coral (e.g., Holbrook *et al.*, 2000; Munday, 2002; Graham *et al.*, 2008), seagrass (Galst and Anderson, 2008), and large kelps (Carr 1994), suggesting that adjusting for the amount of this habitat might be advantageous. However, if MPAs alter dynamics of these biogenic habitats (Selig and Bruno, 2010) then habitat adjustment may obscure the beneficial effects of the MPAs on the target organisms (Table 6.2). In cases where the habitat is unaffected by MPA protection, or where habitat recovers but on a very slow timescale, CI analyses using habitat adjustment *might* work (see Miller [2008] for an example of a CI assessment of MPAs where fish and invertebrate densities were adjusted by an index of habitat selectivity). The problem of course is that we cannot know *a priori* which situation we are in (i.e., does the MPA affect the habitat or not?). So, instead of "fixing" the CI approach, why not just use a more appropriate design to start with?

The obvious answer is that we cannot always obtain Before data. Doing so requires extensive planning and funding. But rather than continue to rely on flawed designs, it is vital that we collect such data when possible (e.g., as in Lincoln-Smith *et al.*, 2006 and de Loma *et al.*, 2008). Interestingly, even in cases where there has been extensive planning of

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MPAs (e.g., California, Florida Keys, Great Barrier Reef), assessments still have only a few Before samples, typically from a single date (e.g., Babcock *et al.*, 2010; McCook *et al.*, 2010). A single Before sample is insufficient for model development in BACIPS designs, which is a critical step that can avoid confounding of true effects with other sources of spatiotemporal variability (Stewart-Oaten *et al.*, 1986, 1995). The limited number of Before dates also limits the power of any subsequent analysis and therefore increases the uncertainty of any effect size estimate (Osenberg *et al.*, 1996b).

Finally, we note that our analyses are predicated upon the detection of local effects: comparisons of inside and outside MPAs. These local effects, while important, are perhaps the least important effect of MPAs. Regional effects (Chapter 3) arising from spillover provide the mechanism that underlies promised improvements in fisheries and regional biodiversity. Regional assessments remain rare, because they require a sampling scale that is hard to achieve: e.g., comparing a region with an MPA to a region lacking MPAs (Osenberg *et al.*, 2006). Designs that rely on comparisons at even larger spatial scales may lead to even greater pre-existing differences, further necessitating the need for Before data.

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