

## Correspondence

### Nutrient enrichment and coral reproduction: between truth and repose (a critique of Loya et al.)

God offers to every mind its choice between truth and repose—Take which you please—you can never have both. R.W. Emerson, *Essays: Intellect*

For the past four decades, the coral reef at Eilat (Red Sea) has been declining (Fishelson, 1995; Epstein et al., 1999; Rinkevich et al., 2003) due to a wide range of anthropogenic impacts. In the last few years, however, a public campaign (Rinkevich et al., 2003) has claimed that the continuous deterioration of Eilat's reef is a direct result of effluents released from fish farms, located 8 km to the north. This claim, which was not scientifically documented, has been challenged by recent publications (Bongiorni et al., 2003a,b; Golani and Lerner, 2003). The subsequent intense public debate has stimulated research into the causes of Eilat reef decline. It is therefore of great importance to evaluate any new study adding results to our meager information on this subject.

In a short (15 months) field experiment, Loya et al. (2004) [LEA], proponents of the hypothesis of fish farming harming the reef, tried “to ascertain the long term effect” of effluents released from the fish farms on the reproduction of *Stylophora pistillata*, one of the most abundant coral species in the Gulf. They claimed that their study “shows unequivocally that reproduction effort of *S. pistillata* was detrimentally affected” by the effluents. However, their study proved incapable to provide support for such a proposition due to poor design, methodological mistakes and inaccurate presentation.

### 1. Erroneous experimental design

LEA attempted to examine “in situ effects of nitrification caused by net-pen fish farms on the reproductive processes of experimentally transplanted coral colonies”.

However, four major flaws in experimental design invalidate conclusions that, otherwise, could have been drawn from the results:

#### 1.1. The choice of site

LEA were wrong in claiming that “the reference [IUI, adjacent to the Coral nature Reserve] site has a similar sandy bottom ...” as the fish cages (FC) site. The fine, unconsolidated terrigenous substrate at the FC, where no natural reef existed formerly due to the alluvium of the Arava valley, differs from the coarser sandy substrate at IUI (Rinkevich et al., 2003). Moreover, since the corals used in their experiments were placed a few centimeters above the sediment, it is very likely that the easily re-suspended fine sediment at the FC site have impaired various physiological and life history traits of these coral colonies. In a previous experiment, Loya and Kramarsky-Winter (2003) found that *Favia fava* colonies transplanted to the FC site died within seven months, while all control colonies transplanted to the IUI site survived. However, we (Shafir et al., submitted for publication) reared 18 *F. fava* colonies in a mid-water nursery (14 m above sea bottom), only 10 m from the fish cages. Now, nearly a year later, all the colonies have survived, enjoying significant growth rates. These results indicate that the probable cause of mortality of the *Favia* colonies (Loya and Kramarsky-Winter, 2003) was “suffocation” from re-suspension of soft sediments.

#### 1.2. Choice of coral placement

LEA claim that at the FC site corals were “exposed to chronic eutrophication caused by in situ fish cages”. This, too, is inaccurate. LEA placed their corals 150 m west of the Ardag fish cage facility (unjustly claimed, by the same authors, to be 200 m from the fish cages [Loya and Kramarsky-Winter, 2003]). Since levels of nutrients at a distance of less than 150–200 m from the cages drop to open sea background levels (Angel et al., 1998; David, 2002; IET program in <http://www.sviva.gov.il>), ergo, none of the results of LEA's study can be attributed to “chronic” fish cage effluents. In contrast, our own studies (Bongiorni et al., 2003a,b) tested direct exposure to fish cage effluents at a distance of only 10 m from the cages.

### 1.3. Choice of coral source and proper controls

LEA collected the colonies from a depth of 10–15 m at the commercial port of Eilat, the most polluted and disturbed site along the Israeli Red Sea coast, without taking into consideration that these colonies were already stressed. Moreover, they did not have two required controls to eliminate the “stress” possibility: (1) reproduction rates in resident colonies at the commercial port; (2) reproduction rates in resident colonies at IUI, the reference site. Even without considering the pre-experimental impacts on these colonies, repositioning some colonies from depth of 10 m at the port to the highly turbid depth of 19 m at the FC could seriously have affected reproduction (i.e. Gleason et al., 2001).

### 1.4. Choice of procedures for histological sections and lipid extraction

LEA assumed they could obtain representative data for colony reproduction by sampling a single branch per colony, because of the synchronization in breeding between branches within a healthy colony (Rinkevich and Loya, 1979a,b). However, in stressed *S. pistillata* colonies, this synchronized reproduction breaks down, a possibility that had not been scrutinized by LEA. Moreover, LEA sampled “the upper part of the branch” for lipid extraction, assuming that the lipid content in the branch tips is “an additional indication of the reproductive state of corals during the reproductive season”. This was a curious choice as LEA must have undoubtedly been aware that in *S. pistillata* the upper parts of branches lack gonads (Rinkevich and Loya, 1979b). Hence, LEA’s conclusion that lipid content “is indicative of greater reproductive effort in IUI colonies” is invalid.

## 2. Incongruity between the text and figures

In their abstract, LEA stated that “histological sections” were performed “on 20 *S. pistillata* colonies transplanted to each of the two study sites”. However, their results (in Table 1) were based on only 5–9 colonies at each site. LEA further stated that they examined in histological sections “50–60 polyps” per colony, per sampling date. Strangely, at the FC site, five colonies were studied during March and May 2001, and March 2000, but only 218, 209 and 210 polyps were examined (Table 1), instead of the expected 250–300 polyps. Similarly, in the May 2001 sample at IUI site, only 230 polyps were examined instead of 300–360, (Table 1). No reason was offered for eliminating so many polyps from the analysis.

According to the text, the standard deviations of average oocyte size at the FC and IUI sites during March 2001 were 55.8  $\mu\text{m}$  and 43.8  $\mu\text{m}$ , respectively, but Fig.

3a depicts values of just 19  $\mu\text{m}$  and 8  $\mu\text{m}$ . The same discrepancy occurred in the results of March 2002. What, then, are the real figures? LEA further stated that 4% of the oocytes at the FC site and 13% of the oocytes at the IUI site reached “mature size” (>200  $\mu\text{m}$ ). Judging from their own data (Fig. 3a; for example, IUI values in March 2001: 135  $\pm$  8  $\mu\text{m}$ , and in May 2001: 60  $\pm$  15  $\mu\text{m}$ ), it is implausible that any of the oocytes in the histological sections could actually reach the “mature size”.

The results (“annual reproductive efforts”) presented in Fig. 4 are also problematic. If the values in Fig. 4 for polyps containing planulae were based on an average of two sampling periods each year (the text fails to supply this information), then they should not have been presented as “annual reproductive efforts”, a legend that may lead readers to a fallacious conclusion. LEA claimed in the legend to Table 2 to have examined 22 colonies, but in reality, only 10–14 colonies were examined (their Table 1).

## 3. What do the results portray?

Cumulatively, the results reported by LEA were based on a single experiment, prejudiced by the use of stressed colonies. Regrettably, the authors omitted basic reproductive parameters of corals (such as, number of oocytes per polyp) that would have clarified or refuted stress. During the peak of the reproductive season (March–May) gravid *S. pistillata* colonies usually contains 2–5 female gonads per polyp and most, if not all polyps contain male gonads (Rinkevich and Loya, 1979a,b, 1987). In LEA’s study, however, it was reported that only a limited number of the polyps contained gonads during both reproductive seasons (1–27% male gonads, 5–21% female gonads) even in their “reference site”. It seems that even their control group colonies had severely depressed reproduction. Nevertheless, they used these faulty control data for comparison with the experimental group (Figs. 2 and 3; Table 1).

Six colonies were studied during May 2001 at IUI of which five contained male gonads (Table 1). However, only  $\approx$ 1% (Fig. 2) of the total 230 polyps studied in these colonies had testes, i.e., only 2–3 polyps in five male coral colonies. Thus, paradoxically, some of the “male colonies” (Table 1) were simultaneously also sterile! Additionally, during May 2001 at IUI only about 3.5% of the polyps contained female gonads (Fig. 3). This is not a reproductive value to be considered in this species during the peak of the reproduction season (Rinkevich and Loya, 1987). Similarly, in the next reproduction season (March 2002), 8 out of 9 colonies possessed male gonads but only  $\approx$ 20% of the polyps had testes. Either the entire data set is faulty, or the entire group of colonies transplanted to the reference site, underwent stress during the 15 months of observations.

In June 2002, all five IUI colonies studied were sterile (Figs 2 and 3a and b; this observation is emphasized in the text, p. 348: “in June 2002, no polyps containing oocytes were found at the IUI site”). Astonishingly, four of these five sterile colonies were characterized in Table 1 as being hermaphrodites! Stressed corals or not, even the results reported appeared to be self-contradictory.

#### 4. Between truth and repose

It seems that LEA were so certain of the perceived negative impact of fish cages effluents on coral reproduction that they neglected to carefully plan the experiment and to genuinely analyze their results. The design of the experiment failed to institute proper controls. In addition, the use of coral colonies originating from the Port of Eilat is very questionable and the presentation of the results is riddled with omissions and inaccuracies. Moreover, the entire ecological situation in the northern Gulf of Eilat, as depicted by LEA, is biased, inaccurate and unsupported by any published data (i.e., in the Introduction: “... resulting in an increase in nitrates and phosphates in the Gulf as well as an increase in particulate matter in the water column”). LEA therefore, failed to prove that nutrient enrichment from fish farms in Eilat is detrimental to coral reproduction.

Undeniably, various anthropogenic impacts (effluents from the adjacent commercial and naval ports, intermittent municipal sewage outflow, phosphate dust, heedless development of the city of Eilat, and others) have hastened the demise of Eilat reef. A more comprehensive look at these activities, instead of adopting the “repose approach”, i.e., identifying the impacts of nutrients released from the fish farms as the major cause for reef decline, would provide a better understanding of the situation. This repose approach, though perhaps trendy, deserves therefore no scientific support from LEA. Only robust and carefully controlled experiments would unveil the relative contribution of each type of stressor to the decline of Eilat reef.

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B. Rinkevich

Israel Oceanographic and Limnological Research

The National Institute of Oceanography

Tel Shikmona

P.O.Box 8010

Haifa 31080

Israel

Tel.: +972 4 8565275; fax: +972 4 8511911

E-mail address: buki@ocean.org.il