Response

The models on indirect reciprocity (*1*) predict that generosity increases the donor's image score and thereby his or her chances of being treated generously by others in the future. These are the proposed long-term benefits of generosity, but there are obvious short-term costs, because by being generous one gives something away. Therefore, as Kazantzis and Sutton point out, in a relatively short game like we set up as described in our report, a player's image score is likely to be confounded with his or her account, that is, with the relative need of a donation.

Indeed, there seemed to be a negative correlation between the players' accounts and their image scores, but this correlation was statistically not significant. We reanalyzed our data in the light of Kazantzis and Sutton's hypothesis using the receivers' accounts instead of their image scores as the dependent variable in our main analysis, the repeated measures analysis of variance (ANOVA) that was explained in Fig. 2 of our report. The statistics results were analogous but apparently of weaker significances. Giving or not giving would correspond to the receiver's account ($F = 4.08$, $P = 0.05$) as it did to the receiver's image score ($F = 8.20$, $P = 0.006$), whereas the effect of the group ($F = 0.46$, $P = 0.86$) and the interaction ($F = 1.68$, $P = 0.13$) were again both not significant. However, in half of the groups (groups 2, 3, 6, and 7), we had not only displayed the receivers' previous decisions as donors, but also their current accounts. The effect of this experimental treatment (that is, of displaying the account or not) was not significant when included in a nested repeated measures ANOVA, with the receiver's image score as the dependent variable and with groups nested in treatment (effect of displaying the account: $F = 0.07$, $P = 0.79$). We conclude that our data are in agreement with the predictions from indirect reciprocity models, and our experimental treatment provides no support for Kazantzis and Sutton's alternative hypothesis.

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What Story Is Told by Oceanic Tracer Concentrations?

In their report "Temporal trends in deep ocean Redfield ratios" (4 Feb., p. 831), Pahlow and Riebesell suggest that the marine biota has changed in the last few decades in response to human activities. These findings challenge the steady-state paradigm of ocean biogeochemistry and might have important implications for the global carbon cycle. However, the signals that Pahlow and Riebesell analyzed are subject to numerous methodological uncertainties [discussed elsewhere (*1*)], and their interpretation of the signals hinges critically on the exclusion of alternative explanations. Here we propose alternative explanations for the reported trends consistent with existing data and knowledge.

For North Atlantic deep waters, Pahlow and Riebesell report an increase in nitrate to phosphate (N:P) ratios and suggest increased nitrogen deposition as a cause. However, this mechanism would decrease the ratio of apparent oxygen utilization (AOU) to nitrate, because this mechanism should lead to an increase in nitrate without changing oxygen. This effect is not seen in Pahlow and Riebesell's analysis. A small change in preformed nutrients [resulting in a cumulative decrease in deep-water phosphate of about 0.02 micromoles per kilogram (μmol/kg) over 20 years and a cumulative increase in deep-water nitrate of 0.14 μmol/kg over the same period (*2*)] could also explain the reported N:P trends. Such a change in preformed nutrients, caused for example by a slight shift in the deep-water source regions rather than in biology, would be undetectable with the presently available data because of large seasonal variability and inadequate sampling.

For North Pacific deep waters, Pahlow and Riebesell report a small increase in AOU and suggest enhanced export production as a cause. However, increasing AOU could also be caused by decreasing water ventilation rates—a change predicted as a response to global warming (*3*) and perhaps manifested in recent chlorofluorocarbon budgets (*4*). Testing this alternative ventilation mechanism poses a formidable (and so far open) challenge.

There are many observations that demonstrate that the marine biota is variable, especially on local scales (*5*). However, the conclusions by Pahlow and Riebesell that the marine biota has changed over large spatial and temporal scales are unsubstantiated because alternative hypotheses cannot be firmly excluded.

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2. Computed by using the rates of changes reported by Pahlow and Riebesell and their equation 3, solved for the change in nutrient concentration ($n_{t1} - n_{t0}$) and adopting a value of 120 μmol/kg for AOU_{t0}.
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Response

Gruber, Keller, and Key suggest alternative explanations for the trends in deep-ocean Redfield ratios obtained in our analysis. They suggest that a small decrease in preformed nutrients could explain the trends observed in the North Atlantic and would be undetectable with presently available data. As we outlined in our report, however, the decrease in preformed phosphate concentrations necessary to generate the observed increase in the N:P ratio would have to be at least 12% or about 0.2 micromolar over 20 years. A change in preformed phosphate concentrations of this magnitude, should it exist, could be easily detected in the data available for the time period covered in our analysis.

Gruber *et al.* say that increased aeolian nitrogen (N) deposition "would decrease the ratio of apparent oxygen utilization (AOU) to nitrate" in the North Atlantic. This would be true only if primary production in the North Atlantic was not limited by nitrogen. Although a transition from nitrogen to phosphorus limitation has been suggested for some areas, nitrogen is considered to be the dominant limiting nutrient in the North Atlantic (*1*). A recent study by Wu *et al.* (*2*) reports phosphate depletion in parts of the western North Atlantic. The authors conclude, however, that the concentration of available nitrogen is decreased to limiting levels in these regions because of the formation of refractory dissolved organic nitrogen. Although increasing aeolian nitrogen deposition may have in fact reduced the AOU:N ratio locally in phosphorus-limited regions, the effect would not be detected by our large-scale analysis.

For the North Pacific, Gruber *et al.* suggest that decreasing deep-water ventilation rates as a result of global warming may explain the observed increase in AOU. North Pacific deep waters originate almost entirely from the South Pacific (*3*). If decreasing deep-water ventilation had caused the trends indicated for the North Pacific, similar trends should also have occurred in the South Pacific. Because this is not observed, changes in deep-water ventilation are not likely to be the main cause for the changes in AOU and oxidative ratios observed in the North Pacific.

In essence, although the nonbiologically mediated processes suggested by Gruber *et al.* may have contributed to the observed temporal trends in deep-ocean Redfield ratios, they cannot account for their magnitude. The changes in marine biota that we proposed in our report thus remain the most plausible explanation for the observed trends.

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CORRECTIONS AND CLARIFICATIONS

News Focus: "Louisiana's vanishing wetlands: going, going,..." by J. Bourne (15 Sept., p.1860). The word "levy" and its derivatives were used in the article instead of "levee."