LETTERS

Edited by **Jennifer Sills**

Overlooked local biodiversity loss

IN THEIR REPORT "Assemblage time series reveal biodiversity change but not systematic loss" (18 April, p. 296), M. Dornelas *et al.* summarized 100 time series of biodiversity monitoring programs and concluded that the world's biomes are "undergoing biodiversity change but not systematic biodiversity loss." This conclusion is misleading, given that Dornelas *et al.* did not account for several of the most pervasive processes known to drive biodiversity loss on the planet.

Jungle burned for agriculture in Southern Mexico.

Nowhere in their data set did Dornelas *et al.* consider the local losses of biodiversity that occurred as 13 million hectares of tropical rainforest were cleared each year from 1990 to 2010 (*1*). Nowhere in their data set did they account for local losses of biodiversity that have occurred as more than 90% of wetlands have been drained, more than 50% of grasslands destroyed, and 70% of Mediterranean and temperate woodlands cut down to make way for 4.9 billion hectares of cropland and pastures (*2*). Nowhere in their data set did the authors quantify losses of biodiversity that have occurred as 3.5 million km^2 of the world's land surface has been converted into urban environments (*3*). Their summary did not account for any direct impacts of habitat loss or land conversion, despite the fact that these are the most pervasive drivers of local diversity loss on the planet (*4*–*6*). Nor did Dornelas *et al.* account for other factors known to cause local diversity loss, such as extinctions caused by overexploitation of resources through hunting, poaching, and overfishing (*7*, *8*).

Instead, the authors focused on biodiversity trends in habitats that are mostly intact and yet to be fully exploited by humans. Because they did not make this important caveat clear, their paper could well be misinterpreted as evidence that human activities have not caused local biodiversity loss. Any accurate assessment of biodiversity change must not only include data from Earth's intact habitats; it must also account for diversity change in habitats that have already been lost, converted, and directly exploited by humanity.

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Response

THE GOAL OF our study was to quantify biodiversity change on the planet in the recent past. Our approach was to include as many data sets as possible that met the following criteria: entire assemblages rather than just populations, standardized sampling repeated through time, and abundance of each species reported. The decision to include a data set was based exclusively on these criteria; we did not consider hypothesized drivers of change, whether habitats were pristine or modified, which taxa were sampled, or the location of samples. Strongly supporting the limited role of sampling bias in our results, another study (*1*) included mostly sites known to be heavily impacted by humans and also found, on average, little temporal change in alpha diversity of 168 terrestrial plant assemblages.

The data sets we used include both pristine and heavily exploited areas. Examples of the latter include data from a nuclear power station (*2*), an oil terminal (*3*), and fisheries data from the North East Atlantic (*4*). In our search for data, we did not find a data set that corresponded to a tropical area of active deforestation that matched our inclusion criteria. However, even a loss rate of 13 million hectares per year of tropical forest corresponds to less than 0.02% of the planet surface, making it unlikely that a before-after deforestation plot in tropical forest would be included in a random sample of the planet's surface. On the other hand, we did include a previously logged large plot of secondary tropical forest (*5*) and a 30-year data set that covers human-disturbed grassland, wetlands, and even suburban areas (*6*). Finally, as shown in Figure 2A of the original paper, our compilation included several data sets that show drastic declines in alpha diversity (the number of species at a given site), but these data sets are not the majority and are counterbalanced by other studies with increases in alpha diversity. In his Letter, Cardinale is selectively focusing on one end of the observed distribution of outcomes, and hence missing the bigger picture. Collectively, the data sets represented in our Report and in Vellend *et al.* (*1*) constitute the most rigorous bio-monitoring studies available on the planet, and their results merit careful consideration.

Cardinale implies that our paper's message is that we do not need to be concerned for the future of biodiversity. This is a misread of our results and discussion. First, we explicitly stated that our results do not contradict the fact that there is a rapid, recent decline in global biodiversity. Our analysis of alpha diversity addresses a different question at a different spatial scale. Second, our analysis of temporal beta diversity (the difference in species at one site over time) revealed that the species composition of communities is changing on average 10% each decade. Cardinale is silent about this alarming and hitherto unappreciated result, which deserves much additional attention and study. Transport of nonnative species, shifts in species geographic ranges, and the establishment of novel ecosystems are important drivers of the current biodiversity crisis, but they are acting mostly through changing which species are present, not decreasing the total number of species in any one location. This is a useful finding for conservation practice and in no way indicates that the status quo is acceptable.

The perfect data set to quantify biodiversity change would span several centuries, encompass multiple taxa, and contain a random sample of points over Earth's surface. We agree that such a data set is desirable, but it currently does not exist (*7*). These new analyses of biodiversity monitoring data [our Report and (*1*)] underline the urgent need for expanding investment and

harmonization in the collection of quality biodiversity data.

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Talk therapy results speak for themselves

THE NEWS FOCUS story "Talking back to madness" by M. Balter (14 March, p. 1190) highlights the importance of talk therapies as part of mainstream treatment of schizophrenia. Balter focuses largely on one form of psychotherapy, cognitive behavioral therapy (CBT), which helps positive, or psychotic, symptoms, but does not benefit other types of symptoms (*1*).

Other core features of schizophrenia, such as negative symptoms (social withdrawal, lack of motivation, and flat affect) and cognitive deficits (impairments in attention, memory, and problem-solving abilities) are more common, persist longer, and contribute more to the lifelong disability of schizophrenia. Fortunately, these symptoms also respond to psychotherapies. One meta-analysis showed that negative symptoms respond to social skills training (*1*). Another meta-analysis showed that cognitive remediation therapy improves cognitive functioning (though not other symptoms), especially when combined with psychiatric rehabilitation (*2*).

The field may have relied too much, and for too long, on antipsychotic medications as the mainstay in treating schizophrenia. Medications work, but mainly for psychosis, and are limited by side effects. It is encouraging that psychotherapies are receiving more attention. Treatment of this chronic, complex illness must involve multipronged interventions, including medications and effective talking treatments, optimally tailored to the individual patient and phase of illness.

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TECHNICAL COMMENT ABSTRACTS

Comment on "Engineering coherence among excited states in synthetic heterodimer systems"

Alexei Halpin, Philip J. M. Johnson, R. J. Dwayne Miller

Hayes *et al.* (Reports, 21 June 2013, p. 1431) used two-dimensional (2D) electronic spectroscopy to study molecular heterodimers and reported a general mechanism for the prolongation of electronic coherences, consistent with previous interpretations of 2D spectra for light-harvesting systems. We argue that the dynamics attributed to electronic coherences are inconclusive based on experimental inconsistencies arising from limited sample characterization and insufficient control measurements.

Full text at http://dx.doi.org/10.1126/science.1250926

Response to Comment on "Engineering coherence among excited states in synthetic heterodimer systems"

Dugan Hayes, Graham B. Griffin, Gregory S. Engel

Halpin, Johnson, and Miller contest our assignment of quantum beating signals observed in the two-dimensional electronic spectra of a series of fluorescein heterodimers to electronic coherences. Here, we present resonance Raman spectra, statistical analysis on multiple data sets, and an explanation of differences between the family of molecules described in our Report and the homodimer examined by the commenters. We contend that these results all support our assignment of the beating signals to electronic coherences.

Full text at http://dx.doi.org/10.1126/science.1251717