

Letters to the Editor

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Extinction Rates and Butterflies

RATES OF POPULATION EXTINCTIONS IN BRITISH invertebrates have now been measured in several different ways [(1), "Comparative losses of British butterflies, birds, and plants and the global extinction crisis," J. A. Thomas *et al.*, Reports, 19 March, p. 1879] and may have relevance to estimating global extinction rates (2). We (1) have used the rate of extirpation of species from the whole of the British Isles, measured over the 20th century from the British Red Data Book (RDB), whereas Thomas *et al.* use distribution changes in the last 20 to 40 years measured in about 3000 map grid cells by 20,000 volunteers. There are notable consistencies and differences in the conclusions of these two approaches.

The extinction of species from the whole of the British Isles is likely to be relatively accurately recorded: Rare species are actively sought, and only one of the 43 species recorded as likely extinct in the RDB has since been rediscovered (with a low and local population). The national extinction rate per century ranges from 0.4% overall for the 14,000 insect species covered in the RDB to over 5% for the 60 species of butterflies and 7% for the 40 species of Odonata (the two best-recorded taxa).

Both studies find the rate of loss of selected invertebrate taxa to be roughly the same order of magnitude as the rate of loss of plants and birds. In both studies, butterflies have a notably higher rate of loss than plants or birds, which is not a recording artifact, because these three taxa are well studied.

Given the relatively high local extinction rates of butterflies recorded by these and other studies, we disagree with the conclusion of Thomas *et al.* that butterflies represent good indicators for losses of other taxa. Rather, Thomas *et al.*'s study supports our suggestion (2, 3) that butterflies (being mostly warmth-loving and herbivorous) are atypical invertebrates that are relatively sensitive to climatic fluctuations and thus give a potentially misleading

guide to extinction rates and human impacts.

CLIVE HAMBLER AND MARTIN R. SPEIGHT

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Response

HAMBLER AND SPEIGHT SUGGEST THAT butterflies have experienced amplified extinction rates in Britain, and thus their widespread use as indicators of change in insects (1, 2) is inappropriate. We consider this argument to be flawed, because of an artifact of recording.

It is widely accepted that comparisons of the proportion of species believed to have become extinct in different taxonomic groups will be biased if the groups being compared experienced different levels of past recording (1, 3). This occurs because the early species lists for undersampled groups contain a disproportionately high representation of common widespread species (4), and it is the rare and local species in a taxon, which tended not to have been recorded in the first

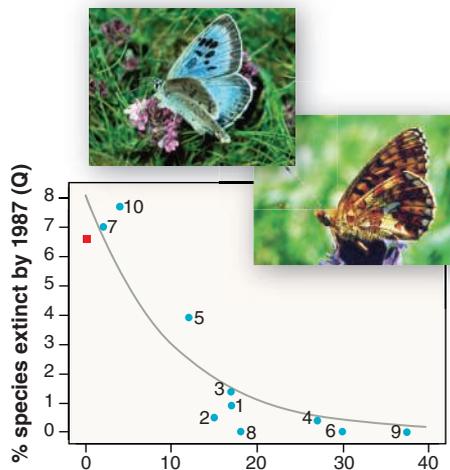
place, that are especially prone to extinction (1, 3). McKinney (5) quantified this artifact in six groups (mammals, birds, molluscs, crustaceans, insects, and marine invertebrates) and obtained a strong correlation between the proportion of species recorded as being globally extinct against the proportion of species that was estimated to have been discovered ($r^2 = 0.82$). We can extend this analysis to different groups of British insects using, like Hambler and Speight, the British RDBs as the main data source (see figure).

The figure, which represents change in 9.2% of all known British insect species plus spiders, shows a similar relationship to McKinney's, indicating that for groups in which "only" 90% of species had been listed a century ago, recorded national extinction rates were less than half those of groups in which 100% of species had been known. Given the rigor of early butterfly recording, their documented declines were not unusual.

Nor are British butterflies atypically thermophilous, as Hambler and Speight claim. The immature, not adult, stages define climatic constraints on insects (6), and distribution maps show that higher proportions of aculeate Hymenoptera and Orthoptera species than butterflies are restricted to the warmest regions of Britain; moths and dragonflies are similar to butterflies, while staphilinid beetles and woodlice are less confined to warm spots (7). Furthermore, because of climate warming, those butterfly species that are thermophilous experienced population increases in Britain that frequently mitigated the effect of habitat degradation (8). Only four of the ten most rapidly declining butterfly species could be classed as thermophilous: The majority include alpine species.

We are also surprised that Hambler and Speight consider phytophagous insects to be unduly sensitive to environmental change. This contradicts their earlier statements (9), with which we agree (6), that specialists, such as taxa inhabiting rotting trees, are more threatened; moreover, the (well-recorded) taxa with the highest reported extinction rates in Britain have different lifestyles: carnivorous aquatic (dragonflies) and social terrestrial (bumbees). In theory, parasitic species are the most vulnerable of all to change (10). Parasitoids are too poorly described to assess critically, but social parasites of ants have a disproportionately high representation in RDBs (6).

In conclusion, we do not claim that butterflies are ideal indicators of other insect changes, but they appear to be suffi-



Percentage (Q) of insect (+ spiders) groups considered to have become extinct in circa 1900–87 in relation to the percentage (U) of native species in current British lists that were unknown in circa 1900. Least squares fitted line: $Q = 8.13e^{-0.0996U}$, $r^2 = 0.92$, $P < 0.001$. Squares, butterflies; circles, other groups: 1, other Macrolepidoptera ($n = 900$); 2, spider ($n = 622$); 3, weevil ($n = 612$); 4, hoverfly ($n = 266$); 5, macro-Brachyura ($n = 154$); 6, ant ($n = 47$); 7, dragonfly ($n = 43$); 8, grasshopper-cricket ($n = 38$); 9, mosquito ($n = 32$); 10, bumblebee species ($n = 26$).

ciently representative to be employed usefully, due to their comprehensive recording levels, as the only invertebrate taxon for which it is possible to estimate rates of decline in many parts of the world (1, 2).

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Noguchi's Contributions to Science

THE RANDOM SAMPLES ITEM "ON THE money" (4 June, p. 1443) states that Hideyo Noguchi discovered the syphilis-causing microbe *Treponema pallidum* and that he was trying to develop a vaccine for yellow fever. Both of these statements are incorrect.

Noguchi proved that the neurological disease called tabes dorsalis was due to late stage syphilis infection. He demonstrated the presence of *Treponema pallidum* in some sections from the spinal cord of a patient with tabes dorsalis.

He became interested in yellow fever, and because of his experience with *Treponema*, he thought that this disease was also caused by some spirocheta-like organisms. He went to Merida, Mexico, to study yellow fever. A local physician introduced him to a patient who had Weil disease, which also produced jaundice but was caused by *Leptospira icterohemorrhagiae*, a spirocheta-like organism. Noguchi discovered this organism and published it as the cause of yellow fever. Many competent microbiologists failed to repeat his findings, and his statement was considered a mistake. He went to Ghana to study yellow fever once more, and he died there

from the disease. He never realized that this disease was caused by a virus, which was eventually discovered by Walter Reed.

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Networks by Design: A Revolution in Ecology

ENVIRONMENTAL CHANGE AND ANTHROPOGENIC activities threaten biodiversity and compromise essential ecosystem services at local to global scales (1, 2). Despite this, current ecological understanding derives mainly from site-specific research and measurements at scales of ≤ 10 m² and at durations of ≤ 5 years (2–4). Several new [e.g., Conservation International's TEAM, the National Park Service Vital Signs, and SAEON (South African Environmental Observatory Network) (5–7)] or proposed [e.g., the U.S. National Science Foundation's NEON and ORION) (8, 9)] initiatives for continental and global-scale research and monitoring networks represent unprecedented new funding in support of ecological research. These programs promise to expand scales of ecological understanding and transform ecology into a more mechanistic and predictive science.

Some assume that such networks should be assembled by locating a single site in each of a number of ecoregions, biomes, or biodiversity hotspots [e.g., (10)], or that by developing large networks employing standard methods, many questions will be answered by brute force. However, the high degree of variability inherent in large-scale systems makes it difficult to disentangle exogenous and endogenous sources of change and may compromise the efficacy of network designs. Designing an effective, large-scale ecological network is remarkably complex. In particular, ensuring appropriate levels of integrated sampling to achieve adequate statistical power at multiple spatial and temporal scales is extremely demanding. Previous efforts, often involving substantial expense (11–15), have been limited by several recurring problems: absence of clear questions underlying the design, sampling inadequacy and bias, inadequate statistical power, heterogeneity of measurement, incomplete and unstructured metadata, lack of tools for integration and analysis of heterogeneous data, and cultural or institutional impediments to data sharing.

By tradition, ecology has been a grassroots discipline in which individual investigators drive the scientific enterprise in an

uncoordinated and serendipitous fashion. Transforming this paradigm to one that will advance large-scale, mechanistic understanding across multiple spatial and temporal scales that reflect critical environmental gradients will require a revolutionary change in approach and in the culture of the discipline.

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CORRECTIONS AND CLARIFICATIONS

Reports: "Indian Ocean climate and an absolute chronology over Dansgaard/Oeschger events 9 to 13" by S. J. Burns *et al.* (5 Sept. 2003, p. 1365). The chronology for the climate time series presented in this Report has been found to be ~2.3 ky too old, due primarily to a systematic standardization error in measurement of the thorium isotopes. A new age model for stalagmite M1-2 based on 19 new Th/U analyses measured by thermal ionization mass spectrometry (TIMS) at the Heidelberg Academy of Sciences and 6 new measurements by induction-coupled plasma mass spectrometry at the University of Bern is shown in fig. S1 (see Supplementary Online Material available at www.sciencemag.org/cgi/content/full/305/5690/1567a/DC1). A simple linear fit through the data was used to recalculate ages for individual data points in the stable isotope time series. The slope of this line (7.59 year/mm) is nearly identical to the slope of a linear fit through the original age model (7.60 year/mm). Thus, the pattern of climate change observed in the oxygen isotopic time series does not change with the new age model. The climate record, however, is moved forward by 2290 years. On the revised time scale, the ages of climate events found in the record, specifically the Dansgaard/Oeschger cycles, match well with two other independently dated records (fig. S2): Hulu Cave stalagmites [Y. J. Wang *et al.*, *Science* **294**, 2345 (2001)] and the most recent chronology for the GRIP Greenland ice core [S. J. Johnsen *et al.*, *J. Quat. Sci.* **16**, 299 (2001)].

TECHNICAL COMMENT ABSTRACTS

COMMENT ON "Managing Soil Carbon" (I)

K. Van Oost, G. Govers, T. A. Quine, G. Heckrath

The assessment of the potential carbon sequestration benefits of no-till agriculture presented by Lal *et al.* (Policy Forum, 16 April 2004, p. 393) is overly optimistic, because the carbon dynamics of water erosion remain poorly understood and because Lal *et al.* have not accounted for carbon storage as a result of tillage-induced soil redistribution.

Full text at www.sciencemag.org/cgi/content/full/305/5690/1567b

COMMENT ON "Managing Soil Carbon" (II)

W. H. Renwick, S. V. Smith, R. O. Slezzer, Robert W. Buddemeier

Based on erosion and sediment budgets for the United States, we contend that the estimates of oxidation for eroded soil carbon by Lal *et al.* are too high. Such overestimates have important implications for estimates of fluxes involved in the atmospheric carbon dioxide budget in the context of the missing carbon sink.

Full text at www.sciencemag.org/cgi/content/full/305/5690/1567c

Response to Comments on "Managing Soil Carbon"

R. Lal, M. Griffin, J. Apt, L. Lave, M. G. Morgan

Although eroded soil carbon is a major contributor to atmospheric carbon dioxide, Renwick *et al.* and Van Oost *et al.* are correct that the emission from eroded soil carbon is uncertain, with a range of 0 to 100% and with some values at about 20%. Erosion and tillage destroy structure, alter temperature and moisture, and expose soil carbon to microbial action that increases carbon dioxide emission (estimated at 1 gigaton of carbon per year). Deep burial may stabilize carbon, but the labile fraction in the surface layer is mineralized following tillage. Despite uncertainties, no-till farming and retaining crop residues and cover cropping are certain to increase soil carbon storage and enhance productivity.

Full text at www.sciencemag.org/cgi/content/full/305/5690/1567d