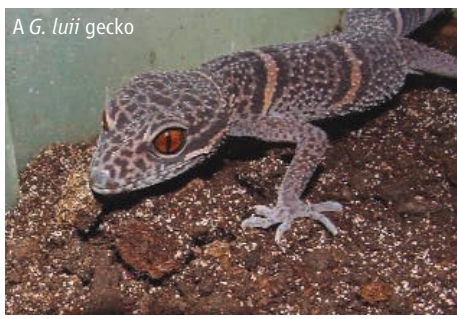
LETTERS

edited by Etta Kavanagh



A *G. luii* gecko

Captive Breeding and a Threatened Gecko

IN THEIR LETTER "SCIENTIFIC DESCRIPTION CAN IMPERIL SPECIES" (26 MAY, p. 1137), B. L. Stuart *et al*. warn that scientific description can draw attention to newly described species attractive for hobbyists, which could lead to their overexploitation or even extinction. Although this scenario sounds plausible, and taxonomists should keep in mind the conservation impacts of their work, at least one of the three examples given is incorrect. The gecko *Goniurosaurus luii* from southeastern China was heavily threatened by hunting for pet trade and local medicine purposes and was probably extirpated from its type locality before it was scientifically described. The specimens of *G. luii* obtained from pet dealers and listed as *Goniurosaurus* sp. were studied by Japanese molecular phylogenetics before the official description (*1*). Lui, the collector of the holotype of *G. luii*, himself "became aware of the existence of *Goniurosaurus luii* and *G. araneus*" from "individuals who specialise in gecko collecting for commercial purposes" (*2*).

Stuart *et al.* also claim that immediately after being described in 1999, *G. luii* reached a breathtaking price of $1500 to $2000 per individual in importing countries. During the last few years, hobbyists perfectly mastered the keeping and breeding of *G. luii* and closely related *G. araneus* and established numerous breeding colonies of both species. Recently, hundreds of captive-born juveniles have been available on the world pet market every year for about $40 each, which has two important conservation consequences. First, there is no further demand on the imported, wild-caught animals. Second, as *G. luii* is a species with limited range still hunted for local medicine trade (*3*) and endangered by habitat damage (*2*), the captive population will soon outnumber the wild one and can serve as a guarantee that this species will survive at least in captivity with a potential chance for re-introduction.

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Response

OUR LETTER USED THREE ASIAN HERPETOlogical examples to illustrate the point that publishing scientific descriptions of new species may inadvertently facilitate their overexploitation by advertising "novelties" to hobbyists and providing detailed locality information to commercial collectors. Kratochvíl correctly notes that one of our examples, the gecko *Goniurosaurus luii*, was already being heavily harvested in China for sale in the international pet trade (*1*, *2*) prior to its description as a new species (*1*). However, immediately after being described, its value in the U.S. pet trade jumped from approximately $500 under an older name to approximately $1500 under its new name as a result of increased demand from hobbyists seeking a unique addition to their collections (the $2000 quote in our Letter referred to a second reptile example, *Chelodina mccordi*, provided in the same sentence). Thus, we feel that *G. luii* remains an appropriate example of how scientifically describing a new species can unintentionally fuel its commercial exploitation (*3*). It is fortunate for *G. luii* that demand for wild-caught individuals has now diminished, owing to the availability of inexpensive, captive-born individuals produced by hobbyists. The conservation merits of unregulated, private, captive breeding programs are beyond the scope of our Letter, but it does seem that *G. luii* paid a high cost for the end result of inexpensive, captive-born substitutes in the pet trade.

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3. For additional plant and animal examples, see L. Guterman, *Chron. Higher Educ.* **52**, A12 (21 July 2006).

Roles of CITES in Protecting New Species

IN THEIR LETTER "SCIENTIFIC DESCRIPTION can imperil species" (26 May, p. 1137), B. L. Stuart *et al.* warn of a dilemma faced by scientists who publish the first scientific description of a new species. Revealing geographical locations in the publication can guide unscrupulous collectors from the international pet trade to the species, which could lead to a rapid decline in population size and even extinction.

To prevent this, Stuart *et. al.* suggest that taxonomists should work closely with relevant governmental agencies. The problem

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with governmental agencies, however, is that the protection is local, not global. Once the species is illegally exported from the country of origin, it can be legally imported into most other countries. For example, the snake species *Bothrops insularis* occurs solely on Queimada Grande, a small island (of 43 ha) off the Brazilian coast, where it could potentially be collected in large numbers. This species is protected by Brazilian law and listed as "Critically Endangered" in the IUCN Red List. However, once illegally exported from South America, the species is completely legal in Europe. No law or convention protects this species from the trade there.

If newly described species are to be protected from international trade, it must be at a global level through CITES (Convention on International Trade in Endangered Species of wild fauna and flora) registration. Before scientists publish their descriptions of new species, population sizes and potential vulnerability to trade should be carefully assessed against the relevant criteria for amendments on the CITES list (with the CITES secretariat in Geneva probably being the best contact point), and the process of listing the species initiated in conjunction with the preparation of its formal scientific description.

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A Problem in Archaeology Too

THE LETTER "SCIENTIFIC DESCRIPTION CAN imperil species" (B. L. Stuart *et al.*, 26 May, p. 1137) notes that formal publications of new species "advertise 'novelties' for hobbyists and drive new markets." The authors document tragically increased commercial exploitation of reptiles and amphibians following publication in the literature. Ironically, this same "dual-use dilemma," as they term it, has also followed formal publication in another discipline: archaeology. Site location data have stimulated pothunters and collectors who use the reports as veritable guidebooks to further their illegal activities. This has been particularly the case in Americanist studies, and I have little doubt of its foreign analogs.

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Photosynthesis in Balance with Respiration?

AS AN ORDINARY BIOLOGIST, I ASSUMED THAT living organisms' impacts on atmospheric CO_2 and O_2 levels were more or less "in balance," with plant photosynthesis being equalled by the summed respiration of plants, animals, and soil and aquatic microbes.

Thus, I find puzzling the attempt by A. W. King *et al.* ("Plant respiration in a warmer world," Perspectives, 28 Apr., p. 536) to use an adaptation of plant respiration to higher temperatures as compensation for increased CO_2 production owing to temperature-stimulated increases in photosynthesis. Surely, temperature also affects rates of respiration in almost all organisms that utilize photosynthates for their energy source? Thus, only the small handful of animals capable of thermal control of body temperature could effectively offset rises in body temperature to lower respiration rates—and even those capacities can add to respiration-derived energy demands. Why is plant adaptation by lowering temperature-induced increases in respiration a necessary hypothesis to offset higher photosynthetic rates?

Surely, if we are to estimate the production of CO_2 as a function of ambient temperatures, we must also consider the impacts of such temperatures on photosynthesis, as well as on the rates of respiration not only of plants, but also of all other lifeforms—from microbes to humans. How well do they adapt their metabolic needs to persistent temperature increases? On balance, over eons of time, the photosynthate has more or less been "in balance" (once the great quantities of reduced carbon were sequestered in fossil fuels, creating an oxygen-rich atmosphere)—through periods of warming and cooling—to provide relatively stable CO_2 to O_2 ratios in the atmosphere. Shouldn't the temperature-dependent responses of all these metabolic regimes be part of any meaningful analysis? If all reactions are more or less equally affected by temperature, how can there be a net "problem" from increased plant respiration?

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Response

CLARK'S ASSUMPTION THAT GLOBAL PHOTOsynthesis is more or less "in balance" with total plant and animal respiration holds as an approximation only when those processes are not being forced from their quasi-equilibrium by disturbance. The ongoing anthropogenic perturbation of the atmosphere by fossil-fuel burning is a major disturbance of Earth's carbon cycle (*1*). Rising atmospheric CO_2 increases photosynthesis. The concurrent increases in temperature alter photosynthesis and respiration, but with different sensitivities. These perturbations, combined with deforestation accompanying large-scale agriculture, are large enough that the world's terrestrial ecosystems are not in equilibrium with respect to CO_2 and O_2 fluxes.

We did not investigate "adaptation" of plant respiration, as suggested by Clark, but rather acclimation to higher temperatures. Acclimation commonly refers to physiological and metabolic adjustments to environmental change, distinguishing these responses from genetic adaptation. Nor did we examine "photosynthesis," but rather temperature-stimulated respiration. Clark asks, "Surely, temperature also affects rates of respiration in almost all organisms…?" Yes, it does, and all rates of metabolic respiration in our model are functions of temperature (*2*, *3*). Furthermore, organisms can indeed lower respiration rates in the facing of rising temperatures. The concept of thermal acclimation applies to respiratory rates (and rates of other enzymatic-based processes) in any poikilothermic organism (*4*, *5*), which includes plants. Acclimation of plant respiration to warmer temperatures is not included in global models of carbon cycle response and feedback to climate change. It is important to understand how including or not including it influences the simulation and interpretation of positive feedback between Earth's carbon cycle and future climate change.

The argument can be made that if one's purpose is to estimate the production of CO_2 as a function of changes in temperature, then one must consider the impacts of temperature (and temperature acclimation) not only on plant respiration, but also photosynthesis and respiration of all lifeforms. The respiration of all lifeforms in the simulations was modeled as a function of temperature, but we purposefully designed the simulation experiments to isolate the contribution of acclimation of plant (leaf) respiration to temperature. Nonetheless, the differential effects of increases in atmospheric CO_2 and changes in climate on cellular reactions could result in plants making an additional net contribution to the imbalance in atmospheric CO_2.

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TECHNICAL COMMENT ABSTRACTS

Comment on "Computational Improvements Reveal Great Bacterial Diversity and High Metal Toxicity in Soil"

Igor Volkov, Jayanth R. Banavar, Amos Maritan

Based on analysis of the reassociation kinetics of bacterial DNA in soil, Gans *et al*. (Reports, 26 August 2005, p. 1387) claimed that millions of microbe species existed in 10 grams of pristine soil and that 99.9% of the diversity was lost as a result of toxic metals. We show that the data do not support these startling conclusions unambiguously.

Full text at www.sciencemag.org/cgi/content/full/313/5789/918a

Response to Comment by Volkov *et al.* on "Computational Improvements Reveal Great Bacterial Diversity and High Metal Toxicity in Soil"

Jason Gans, Murray Wolinsky, John Dunbar

Volkov *et al*. claim that significant conclusions about the total number of species (*S*) cannot be made because different abundance models cannot be distinguished and the sensitivity of the chi-square measure to changes in estimates of *S* is low. We point out that currently available data do not support these claims.

Full text at www.sciencemag.org/cgi/content/full/313/5789/918b

Comment on "Computational Improvements Reveal Great Bacterial Diversity and High Metal Toxicity in Soil"

John Bunge, Slava S. Epstein, Daniel G. Peterson

Gans *et al*. (Reports, 26 August 2005, p. 1387) provided an estimate of soil bacterial species richness two orders of magnitude greater than previously reported values. Using a rederived mathematical model, we reanalyzed the data and found that the statistical error exceeds the estimate by a factor of 26. We also note two potential sources of error in the experimental data collection and measurement procedures.

Full text at www.sciencemag.org/cgi/content/full/313/5789/918c

Response to Comment by Bunge *et al.* on "Computational Improvements Reveal Great Bacterial Diversity and High Metal Toxicity in Soil"

Jason Gans, Murray Wolinsky, John Dunbar

Bunge *et al*. claim that we underestimated the error in our analysis of bacterial diversity in noncontaminated soil. However, they used an unsatisfactory model that exhibited pathological behavior and consequently led to an exceptionally high calculated error. In contrast, the zipf distribution yielded an error estimate only 0.7 times the estimate of the total number of species (*S*), and it is more biologically relevant.

Full text at www.sciencemag.org/cgi/content/full/313/5789/918d

Letters to the Editor

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