The assertion that $a=2/3$ by Banavar et al. is at odds with their earlier theoretical argument for a 3/4 exponent for $a$. In any case, von Bertalanffy’s explanation (and that of Banavar et al.) for the origin of equation (1) cannot be correct, as both $B$ and $b$ scale as $m^{1/4}$, leading to $dm/dt \propto m^{1/4}$, or $m \propto t^4$ for all times.

To reveal the universality of growth that is implied by equation (1), we showed that, by plotting $r=(m/M)^{1/4}$ against $\tau=atM^{1/4}\ln \left(1-(m/M)^{1/4}\right)$, all organisms conform to a predicted universal curve, $1-e^{-\tau}$. Banavar et al. observe that a similar plot can be generated by using an unrealistic $a=2/3$, rather than $a=3/4$. Most data on ontogenetic growth are not of sufficient quality to distinguish between the two: we recognize this and made no claim that $a=3/4$ is a better fit than $a=2/3$. However, the statement by Banavar et al. that this curve is independent of $\alpha$ is misleading because $r$ and $\tau$ depend explicitly on $\alpha$, so the scaling curve cannot be constructed without knowing its value, as well as the values of $a$ and $b$. (Indeed, Banavar et al. use our values based on a 3/4 power.)

Banavar and colleagues’ comment misses our central point that, because equation (1) is derived from fundamental principles concerning how growth is fuelled by metabolic power at the cellular level, many important quantities can be understood quantitatively. For example, our model elegantly interprets $r$ as the proportion of total lifetime metabolic energy that is devoted to maintenance and other activities.

We further contend that the implication made by Banavar et al. that their equation $dm/dt = m^a f(M/M)$ is the most general form of the growth equation is also misleading. The function $f$ depends on several variables, including $m$, $M$, $b$, $E_b$, cell growth and lifetime, time to maturity, and so on. Without a specific mechanistic model, why should $f$ depend only on $M/M$, and what sets the fundamental timescale for growth? Our equation (1) answers these and other questions. It contains, derives and predicts many fundamental biological and physical variables that capture the essential features of ontogenetic growth, yet it yields an extraordinarily simple universal equation. **Geoffrey B. West*, Brian J. Enquist†, James H. Brown§**

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**Figure 1** Annual mean temperature for Nairobi airport (WMO 63741, 1.3° S, 36.9° E, 1,624 m) and Kericho (0.37° S, 35.35° E, 2,031 m) are plotted as bars to show deviations from the averages for 1961–90 (19.0°C) and 1988–97 (17.4°C), respectively. These station records are complemented with a large-area average from a 0.5°-gridded time-series defined by 4° S, 4° N, 28° E, 38° E (refs 2,3). The area-averaged annual mean temperature is plotted (top) as bars that display deviations from the 1961–90 average (22.2°C). Time series for Nairobi airport and area-averaged data are also plotted after smoothing with a 10-year gaussian filter to emphasize changes on decadal timescales. In addition to the observed temperature trends, note the marked altitudinal dependency in temperatures.
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dence at specific sites. But such climate and malaria data sets must be considered at comparable spatial and temporal scales. For example, in a comparison of monthly climate and malaria data in highland Kakamega, Kenya, we found a close association between malaria transmission and monthly maximum temperature anomalies from 1997 to 2000, using data from the same location and over the same period of time. Hay and colleagues simply compared point-incidence rates with downscaled gridded climate data, rather than coincident longitudinal malaria and climate data.

We conclude that a reliable assessment of long-term relationships between climate and malaria incidence requires increased local monitoring of appropriate climate and disease variables to establish data sets that can support long-term trend analysis. Interdisciplinary teams are needed to analyse processes as diverse as climate and human disease.

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6. Cox, J., Craig, M., Le Sueur, D. & Sharp, B. Mapping malaria risk in the highland areas of Africa (MARA, London School of Hygiene and Tropical Medicine, London, 1999).
8. www.giss.nasa.gov/data/update/gistemp

Hay et al. reply — In our study we did not address the impact of predicted climate change on malaria incidence because a complete global assessment is reported elsewhere. Regarding the climate surfaces we used, the data set has superior spatial resolution compared with other data sets of similar temporal extent1,2, and has been used to quantify climate change across Africa at 0.5° × 0.5° spatial resolution3 (although the resulting maps are smoothed to emphasize regional changes). It is inconsistent to assert that these same data are insufficient to demonstrate a lack of climate change.

Furthermore, these smoothed patterns are at odds with the marked and variable trends in temperature identified across east Africa using long-term temperature records from meteorological stations4. Events occurring at the 0.5° × 0.5° resolution will be less variable when averaged over wider areas, but we know of no evidence that climate surfaces interpolated from meteorological stations consistently fail to reveal trends in climate experienced at those locations.

Crucially, further work has confirmed a very high degree of correspondence between the climate surfaces2,3 and meteorological-station data from Kericho. Moreover, these station data show no significant trend in temperature or rainfall during the 1966–95 period over which complete longitudinal hospital records show malaria incidence to have increased significantly5. The malaria resurgences documented at these four sites are not “point-prevalence rates”, but estimates from longitudinal records of health facilities, whose catchment populations range over continuous highland areas that are similar in size to a pixel of the climate surface that we used2,3.

The sparse coverage of meteorological stations in the data sets, before 1910 in the east African region is problematic6, and these data were excluded from our analyses. The full 1901–95 data set was used by one of the correspondents, however, in their trend analyses of African climate7. Moreover, our conclusions remain unaltered in the light of tests repeated for the 1970–95 period, which is coincident with the malaria resurgence6. Finally, windowed Fourier analysis of the meteorological station data for Kericho also showed no change in annual temperature or rainfall variability since 1966 (ref. 11), a conclusion that is corroborated by global-scale analysis of climate variability during this century8.

 Rather than climate change, variations in environmental, social and epidemiological factors are more plausible explanations for the malaria resurgences at the four sites we examined4 and at three others in Ethiopia, Madagascar and Tanzania3. Evidence against the epidemiological significance of climate change in the recent malaria resurgences in Africa is mounting9,10 and remains unmatched by any contrary evidence.

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