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SYDENHAM REPLIES—My paper was compiled in late 1973, and represents events of that time. A vital word–'headed'—did not appear at the end of the title as intended.

By early 1974, experimental Earth-strain research had reached a stage at which mechanical instrumentation was no longer a significant unknown of any programme. The next stage in maturity was to know how to obtain geophysical contributions with the hardware. In 1974, the modifying effects of cavities and regional topography were universally recognised and serious studies were initiated (earlier work with linear arrays was simplistic and often non-productive). Important and unexpected results have emerged from this approach (ref. 1 and D. P. Blair, unpublished thesis) showing how and to what extent expected strain magnitude is altered. Blair (unpublished), extending Harrison's[1] finite-element work from two into three dimensions has found that topography selectively alters the amplitudes of the tidal spectral components. This result apparently explains that topography was the influencing parameter sought without success at the Queensbury site[2]; yet King questions my earlier predictions of the relevance of a tensor study.

Reference about strain-creep using linear arrays was indeed omitted as it is more usually studied using quite coarse instruments (though in my original manuscript it was suggested that creep theory, as studied by materials scientists, could be relevant to an understanding of creep mechanisms). This concept has since been invoked[3].

I suggested that theory might help reduce the number of components needed to understand how strains relate at different places. Available techniques now allow the prediction of site effects using only a geological map, a topographical map and computing ability. We can now eliminate most known causes of incompatibility between theory and practice, leaving the interesting differences to be investigated. Deep-sea cotidal charts, for instance, might soon be updated using corrected Earth-strain data (D. P. Blair, unpublished thesis). Resolution of the interval argument could be near: structural studies based on topographical studies should give quantitative guidelines.

My work on drift reduction in instruments led to the development of the mechanical instruments King's group now uses. A very real advantage of high stability is the ease it offers of transducer design and recording. Quartz chain catenary instruments need no autoranging recorder nor any adjustments over yearly periods. Long-period differential drift between two different quartz instruments is no greater than 5 parts in 10^{12} per hour[4]. Very long baseline interferometers excepted, no available geodetic technique can provide drift as low.

Frequency spectra could provide a means of assessing instrument performance but only if the power contributions of the Earth and the instrument can be distinguished separately. I have seen no point in publishing the tidal spectra for they contribute little. The work of Berger and Levine[5] referred to by King represents excellent spectral analysis, but does it provide useful knowledge? If a site topography alters the amplitude[1] and composition of signal (ref. 1 and D. P. Blair, unpublished thesis) it does likewise to the tensor fluctuation noise. Comparisons could be out by a factor of 0–3 if site coefficients differ greatly.

Is not the ultimate strainmeter one that provides adequate quality data, ease of recording, ability to be used anywhere in any direction, is the cheapest, the quickest to install, creates the smallest cavity error, automatically compensates for any local thermoelastic effect, and gives high reliability? A core-hole instrument (D. Curtis and P. H. S., unpublished) offers that at the economic ratio of 150 to one (compared with the first kilometre-length surface laser instrument). Until laser instruments reveal significant geophysical information not gained by well designed, mechanical equivalent extensometers, I cannot see how it can be argued that they are worthwhile geophysical tools. Surely, Levine does not justify laser expenditure on cost per unit length at this stage of uncertainty about the ideal interval?

Levine's comments on environmental effects on quartz can be misleading. Ambient control in subsurface installations is usually just within needs. Even if quartz is unstable with humidity change, it is easy to control relative humidity with a passive flexible enclosure.

The cost of advanced total environmental control[6] is no more than that of a supplying and running a fail-safe evacuated enclosure. I again stress the point, however, that this line of reasoning is not fundamentally correct. An instrument having the same thermal coefficient (made of stainless steel, not quartz) as the host rock in which it is buried, requires no ambient compensation.

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Extinction of taxa and Van Valen's law

VAN VALEN[1] has proposed an evolutionary law that has attracted considerable attention[2–6]. He showed that plots of survivorship of taxa as a function of age are commonly log–linear (see Fig. 1 of ref. 1). This he interpreted to mean that the probability of extinction of a taxon is independent of its age, and thus formulated the law: "The effective environment of the members of any homogeneous group of organisms deteriorates at a stochastically contant rate."[1] We show here that survivorship analysis of the log–linear curves constructed by Van Valen does not warrant the interpretation that he assigned to it; support for Van Valen's law must be sought elsewhere.

In life table analysis one of two methods are used to construct a survivorship curve[7–9]. Horizontal, dynamic, or cohort life tables require a set of optimisms of even age (usually taken at birth), whose survivorship curve is derived from a complete analysis of the timing of deaths. All that is required is to record the age of death of each member and plot the number of survivors for each age class. Vertical, static, or actuarial life tables differ in that a sample of living organisms (or of ageable remains of dead organisms) is collected over a series of ages[10]. The critical assumption of the vertical life table is that the proportion of organisms within each age class decreases as a function of the age-specific death rate.

Van Valen claimed that his data were compiled in cohort-type tables, but Raup[5] has argued that Van Valen used vertical life tables for extinct taxa. In fact, Van Valen combined the two types of life table analysis for both living and extinct taxa. The abscissa of his plots for extinct taxa is time of survival, arranged in such a way that all taxa are scaled as if they had originated simultaneously. For living taxa his abscissa scaling procedure is a straightforward reversal of the procedure for extinct taxa, since all living taxa are contemporary but evolved at various times in the past. In both cases the data seem to form a cohort, but they clearly do not. In the case of extinct taxa the scaling procedure is entirely responsible for the appearance of a cohort, but since the actual times of origin of all points are not the same, there can be no cohort analogous to an ecological life table. In reality the data constitute a vertical life table plotted to appear as a cohort, giving a survivorship curve without additional calculations. In the case of living taxa the data form a reversed cohort table, enabling survivorship to be estimated