The variation from the constant in these results is small enough to be accounted for by experimental error, and the authors themselves were well aware of frequent fluctuations in barometric height and they considered temperature to be one of its causes[12]. Boyle pointed out that their tubes were liable to be of non-uniform bore[13].

Examination of the evidence summarized here suggests that Richard Towneley and his collaborators played an important part in the postulation and experimental support of Boyle's law, and that Boyle was well justified in expressing his respect for the work of these minor scientific authors. It is overlooked that both Boyle and Hooke utilized the same apparatus, devised by Power and Towneley, in performing their own experiments on the gas law. They modified its use for their laboratory experiments, and obtained satisfactory results with it, although this original apparatus is less well known than the new apparatus which Boyle devised in which air was enclosed beneath mercury, in the short arm of a J tube. While Towneley's apparatus allowed the influence of reduced pressure on the volume of air to be measured, Boyle's J tube enabled the pressure to be increased.

Boyle's role in the establishment of the gas law remains important; his experimental results were accurate and extensive; the apparatus which he devised has become the standard one for illustrating the gas law; most significantly he gave the weight of his great reputation to the law, and published it in a highly influential work. He became the publicist of the gas law, which would have remained unnoticed in Power's work and Towneley's correspondence with Boyle.

However, the process of history has led to a loss of perspective in the examination of the achievements of such major figures as Boyle, leading to their excessive glorification, with the complementary neglect of such authors as Towneley and Power.

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COMETARY COLLISIONS AND TEKTITES

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In a recent communication to *Nature*, Adams and Huffaker[1] have presented arguments purporting to show that tektites cannot originate from the Earth on mechanical grounds. They argue that it is not possible to propel small objects through the atmosphere of the Earth to high altitudes because of the enormous accelerations that are required. They also discussed the question of whether very large objects could be propelled from the Earth's surface through the atmosphere. Such proposals are so obviously impossible that it has seemed to me unnecessary that they should even be discussed, and no proposals that they could occur have appeared in the literature. But since the *Scientific American* has recently accepted these arguments as valid, it may be well to discuss the subject briefly.

I proposed that a cometary collision produced the tektites[2]. It was pointed out that a collision of a comet head with the Earth might be an exceedingly energetic event. It was estimated that one such event could occur every 50 million years. Prof. Z. Kopal (personal communication) pointed out that this was much too low an estimate for the frequency of such events. The qualitative description of this process quite clearly envisaged that the cometary material and the atmosphere would be propelled to a very large distance with high velocities and would probably carry with it some material from the surface of the Earth. No suggestion that small objects would be propelled through the atmosphere was made. It was suggested that a comet head broke up and that the fragments landed at different points resulting in a wide distribution. This suggestion is difficult to reconcile with the very similar compositions of tektites from the widely separated tektite areas as Pinson[3] has shown and as pointed out by him.

Recently, Janet Bainbridge[4] has made numerical calculations in regard to this question and finds that the cometary material would behave approximately as has been assumed. Calculations were made for a cylindrical comet of density 0·01 g/cm³ approaching an olivine surface in vacuum at velocities appropriate for collisions with the Earth or Moon. Velocities of the colliding object after collisions both in the backward and horizontal directions were found to be nearly equal to the velocities of approach. In the presence of an atmosphere, the behaviour of small comet heads would be quite different but large objects would follow a pattern much like that for the objects in vacuum. The assumptions made above for the cometary object appear to be reasonable.

The proponents of an extraterrestrial origin of the tektites rest their arguments on the fact that the australites are often button-shaped and that these button-like structures have been produced by rapid flight through the atmosphere and on the difficulties encountered in matching the composition of the tektites exactly with that of common terrestrial materials.

Chapman[5] estimated the velocity of entry of the button-shaped australites as about the escape velocity of the Earth and hence reasoned that the tektites came from the Moon. Adams and Huffaker[1] estimate the entry velocity as possibly as low as 6·5 km/sec, and hence postulate a circum-terrestrial orbit which enters the atmosphere at glancing angles. No attempt is made to explain how large objects, that is, 1,700 tons, got from the Moon into such very specialized orbits. A criticism of this suggestion has been made recently[6]. No one has ever questioned that the button-like structures of the australites were made by the passage of these objects through gas at high velocity or at least that high velocity gases flowed over the objects in some way. But there seems to be a complete neglect of all the other evidence in regard to this problem by proponents of the lunar origin. The reader's attention is directed to a recent paper by Taylor[7] on the chemical composition of the australites in which it is concluded that their composition agrees closely with certain materials from the surface of the Earth, which were produced by all the complicated geological processes characteristic of a planet like the Earth. This is a conclusion entirely similar to one that came to my attention quite a number of years ago from the work of O. Joensuu at the University of Chicago in the early fifties. This work was not published but was referred to by me[8]. The large tektites of the Muong-Nong type have been discussed by Barnes and Pitakpaivan[9] recently. They are mixed with a laterite soil which the authors believe has evolved since the objects were produced by fusion of the surface of the Earth. Craters have been discussed by Cohen[10] which might well be the collision sites for cometary objects. The short time that the tektites have been in space has been pointed out by Viste and Anders[11]. The similarity of composition of tektites to certain soils has been discussed by Schwartz[12]. The impossibility of transferring material from the Moon to the Earth with the distributions that are observed was discussed by me in 1955 and again in 1962 [6,8]. The very uniform isotopic composition of the tektites as pointed out by Taylor and Epstein[13], and the very similar compositions of tektites from different areas as shown by Lowman[14] are difficult questions but perhaps not insurmountable for terrestrial origin. The very low nickel content is not easily reconciled with a lunar origin.

In connexion with the cometary collision idea, it is interesting to point out that the recent studies on the Tungussky catastrophe of 1908 are most informative[15]. The evidence is that an object having the energy of 4×10^{23} ergs (10 megatons of TNT equivalent) exploded five or more kilometres above the surface of the Earth. The radiated energy was about 1·1–2·8 times 10^{23} ergs. The estimated energy of the collision is seven times that estimated for the Canyon Diablio crater, yet no crater was observed. Interestingly enough, a forest remained standing directly under the point of explosion. There is considerable uncertainty in regard to the mass and velocity of the object. It apparently arrived at a low angle and Zolotov[15] believes at a low velocity, that is, 3–4 km sec^{-1}. Such a low velocity is not consistent with a high energy unless the energy was internal, that is, chemical energy. Krinov[16] mentions higher velocities. It scarcely could have stopped in the atmosphere if its mass per unit area had been much larger than the mass per unit area of the air through which it passed. But if the object were moving in an initial orbit about the Earth, its velocity would be minimal at 7·9 km sec^{-1}, and thus if its velocity of approach were only half this it must have lost three-quarters of its energy. Its path would have been from the south over some of the most populous regions of the Earth. It is difficult to believe that it would not have been seen by someone at seven o'clock in the morning. The velocity mentioned by Zolotov must be in error for some reason which is not evident from his paper. These uncertainties make it difficult to estimate its physical properties.

It is not possible to estimate the frequency of such collisions from one event. No event of such magnitude could have occurred in civilized regions of the world without some record having come down to us, but such areas have constituted only very small fractions of the Earth's surface. As noted previously[8], several astronomical objects have passed near the Earth during this century, and it is most likely that the inventory is not complete. A rough estimate of the probability of collision with the Earth indicates that this might occur within some millions of years or less. The estimated energy of collision is very much larger than 4×10^{23}, possibly some 10^{29} ergs. An object of this kind, if it had a high density, would pass through the atmosphere almost unobstructed and would produce a large crater. However, comet heads of low density would be stopped to a large degree by the atmosphere and no large deep craters would result. Since large craters are not numerous on the Earth's surface, these objects may well be comet heads of low density.

Suggestions have been made that the Tungussky object was contraterrene matter which is far too extreme a suggestion for serious consideration. This object was a small comet head according to Krinov[16]. If the object had had sufficient mass per unit area, it would have penetrated completely to the surface of the Earth, probably melted the surface of the Earth, and produced a very shallow crater. In other words, the Tungussky collision is an example of precisely the kind of collision that has been discussed in connexion with the origin of tektites, though smaller in mass than those suggested. Larger comet heads are discussed in the astronomical literature and such larger heads may well have produced the craters discussed by Cohen[10]. Both the probable cometary collision of an object only some 200 metres or so in dimension and these larger objects some kilometres in dimension moving near the Earth are consistent with the hypothesis that cometary collisions could be sufficiently frequent and of such character that tektites could be produced in this way. (Cohen at the Third International Space Science Symposium, May 1962, quoted a private communication from W. Gentner, H. J. Lippolt and O. A. Schaeffer to the effect that the potassium–argon age of a glass sample from the Ries crater and a moldavite tektite both have the age within experimental limits of error of 15 million years.)

Many features of tektites are accounted for by the cometary hypothesis. The chemical composition of the tektites is very like that of the more acid terrestrial materials. A mechanism for dispersal to great distances is provided by the cometary collision. High temperatures for rapid and incomplete melting are provided. The decreased estimate for the re-entry velocity, that is, 6·5 km sec^{-1}, makes it possible to account for the second melting of the australites. The types without flanges are those which did not re-enter at high velocities. The Muong-Nong types are residues of the melted pools produced at the collision sites and not scattered to great distances. A comet head collision has been observed in this century. Larger ones should melt the Earth's surface, produce craters and scatter material to great distances. All detailed features of tektites are not immediately explained by this theory, but its successes are so great that it would be well for authors to study possibilities for fitting their data into the general features of this theory. It is evident that a cometary collision is a very complicated event and that it is not possible to describe all its details with certainty.

It is evident that material of approximately tektitic composition is widespread on the Earth, though Taylor and Epstein's[13], Lowman's[14], Pinson's[3] and Taylor's[7] discussions show that only selected samples would account exactly for their composition. However, melting and distribution by a comet head collision would probably be highly selective. Thus, very fluid melts would scarcely remain as objects of the size of tektites in a blast of hot gases, and hence only very acid and viscous materials, that is, tektites and Libyan glass, would survive. Also basic melts would crystallize rapidly and would weather away in short periods of time. Even acidic materials melted at high temperatures would not survive. Only such material which had been barely melted, such as the tektites, would survive the blast of gases and would remain as uncrystallized and unweathered glassy objects after long periods of time. In this way the very selected compositions discussed by Lowman[14] can be understood possibly. Also, it should be noted that the present cometary collision hypothesis assumes that all the tektitic fields of south-east Asia and Australia originated from one site and hence their similarity is naturally explained. The general exclusion of Americanites and the Darwin and Libyan glasses from the classification of tektites make them a more uniform group than they are when these are included. Finally, melting by cometary collisions may be more selective than we are able to deduce from our general estimates in regard to such processes.

The postulate that tektites come from the Moon has never been discussed in an objective way. Varsavsky[17] tried to show that South Australia could be covered by objects from the Moon while North Australia did not receive such objects. He did not account for the other tektite areas nor for objects which missed the Earth on the first pass. O'Keefe has postulated objects moving in spiral orbits from the Moon and in no way considers the high probability, and in fact, certainty that many objects released from the lunar surface by some violent processes would move in very different orbits and arrive at all points of the Earth's surface. It is difficult indeed to recall any proposal discussed in the scientific literature in recent years which is so enmeshed in special pleading for nearly miraculous events as this proposal for the lunar-origin of tektites. Criticisms of these arguments have been made previously[18].

If comet heads collide with the Earth, they must also collide with the Moon, and some of the lunar craters must have been produced in this way. The central peaks with their apparent volcanic craters and the general "scooped out" shape are not inconsistent with qualitative ideas of the results of a gaseous collision such as that studied by Mrs. Bainbridge. These calculations indicate that a mild 'bounce' of the lunar surface at the collision point is possible. Such a rebound might explain the craters in the tops of many central peaks. The propellent and non-penetrating character of high-velocity gases and low-density solids rather than the more 'detonating' and penetrating character of high density silicate and metal objects might well account for some of the lunar craters. It is not evident that there are two classes of lunar craters. At present this suggestion is scarcely more than an intuitive estimate in regard to these phenomena.

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OLD MORTALITY

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During the early 'thirties, R. R. Kuczynski introduced then young research workers of Great Britain to the uses of the Actuarial Life Table (A.L.T.) as a substitute for standardized rates hitherto used for interpretation of vital statistics in the best sense of the term. Since the end of the Second World War there has been an agonizing reappraisal, largely due to the use of *cohort analysis* for the study of mortality and morbidity by R. A. M. Case[1] and for that of fertility by W. Taylor[2] and by P. K. Whelpton[3]. Thus the issue of a monograph by Dr. Case *et al.* on the Cohort Life Table (C.L.T.) from the Chester Beatty Research Institute on the eve of the Royal Society tercentenary celebrations of the publication of the *Observations . . . upon the Bills of Mortality* by John Graunt[4] is a timely event*. It is also greatly to the credit of the Institute. For like other inquiries by its senior author, his work in this field has the hall-mark of perceptive originality.

The A.L.T. is like the composite face which Galton obtained by photographic superposition in pursuit of Quetelet's Average Man. That is to say, it tells us what would be the number of survivors out of 1,000 births in each year of life, if age specific death-rates (mortality in each year of life related to the corresponding population at risk) were to remain constant as at the calendar year which dates the Table. No single C.L.T. is referable to one calendar year in this sense. Each such table pertains to the birth year of a particular cohort, that is, assemblage born in the same calendar year. Whereas each entry in the A.L.T. refers to a different cohort in that sense, successive entries of a C.L.T. thus record the life experience of one and the same assemblage, except in so far as migration distorts the picture. In short, the C.L.T. is as factual a record of a unique observable occurrence as is the photograph of an individual face.

* Chester Beatty Research Institute Serial Abridged Life Tables, England and Wales, 1841–1960. Part 1: Tables, Preface and Notes. Compiled by R. A. M. Case, Christine Coghill, Joyce L. Harley and Joan T. Pearson. Pp. xxvi+87. (London: The Chester Beatty Research Institute, Institute of Cancer Research, Royal Cancer Hospital, 1962.)

Needless to say, the C.L.T., and cohort analysis in general, has two limitations. At the time of writing, one can know nothing about mortality of 14-year-old children born in 1950, and standards of diagnosis may well have changed drastically during the life experience of the now-extinct 1860 cohort. None the less, the outcome of focusing attention on the life experience of the cohort as a unit of study is salutary from a medical point of view, if only because it compels us to take stock of hitherto insufficiently recognized shortcomings of the A.L.T. and of its applications.

Consider first a type of situation characteristic of Western Europe throughout the past half-century, during which mortality in all age-groups has diminished, or at worst remained static at the tail-end of life. Thus the younger age-groups now alive have suffered exposure to less health hazards than corresponding age-groups of the parental generation. In such a set-up, we may expect what this monograph clearly discloses: for nearly all (if not all) age-groups, the A.L.T. for the calendar year X will cite a lower proportion of survivors than the C.L.T. referable to birth-date X. In a uniformly improving situation, use of the A.L.T. to estimate the risk of death before 60 of persons thirty years old at the calendar date to which its figures refer therefore discloses an unduly pessimistic prospect. If, as some people idly imagine, the current A.L.T. actually dictated the premium policy of an Insurance Corporation, circumstances would be prejudicial to the candidate for a life policy and, *ceteris paribus* (that is, with no change of the cost of living), favourable to the purchaser of an annuity.

Alternatively, consider a situation in which risk of contracting a particular killing disease is increasing *pari passu* with net diminution of death risk from all so-called causes. This is indeed what some claim to be true of lung carcinoma with due allowance for changing diagnostic standards. In this *milieu*, cohort analysis exposes what should rightly go into the text-books as the "Robert Case Illusion". Briefly stated it is this: a plot of case incidence