Racemization and Epimerization

In recent reports (*1*) the term *racemization* is used inappropriately.

It is stated in both reports that L-isoleucine racemizes to D-alloisoleucine. This reaction is epimerization, not racemization. Racemization of L-isoleucine results in the formation of DL-isoleucine. As stated in Eliel (*2*): "racemization is the process of producing a racemic modification starting with one of the pure enantiomers."

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In the strictest sense Smith is correct in pointing out that the reaction is epimerization, not racemization. In a practical sense, however, the use of the term racemization may be justified or at least rationalized.

In geochemical situations configuration changes of amino acids involve asymmetric centers attached to amino and carboxyl functions. Centers not attached to these functions remain fixed. Although isoleucine, for example, has two centers it is only at the carbon next to the carboxyl that the configuration changes. Therefore, so far as the molecule is concerned, isoleucine reacts with regard to changes in configuration in the same way as any amino acid with a single asymmetric center.

Most amino acids have single asymmetric centers and can undergo racemization. Exactly the same reaction can take place with isoleucine, but because of definitions, a different name is given—epimerization. Use of the term racemization does not seem to create any confusion even when applied to amino acids like isoleucine which have two centers only one of which is involved in configuration changes. Certainly when a collection of L-amino acids undergoes reactions yielding DL-amino acids the term racemization seems adequate even though a few of the amino acids in this collection may have two asymmetric centers.

In earlier geochemical literature (*1*) the term racemization has been employed in a general, practical sense and can be justified in the light of the arguments just presented. When future work is done in organic geochemistry, however, all authors probably should consider using the term epimerization where applicable.

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Was the Moon Originally Cold?

Recently several vigorous attacks have been made on the idea of a cold or cool origin of the moon. Since this idea was first definitely stated by me (*1*) and since there is considerable misunderstanding of what is involved, I would like to explain the origin and purpose of the suggestion and my present ideas in regard to the problem.

Brown (*2*) pointed out that the ratio of the polar flattening of Mars to the ratio of centrifugal force at its equator to its gravitational force was sufficiently large to suggest that it had no core. Reviewing the evidence led me to conclude that this was the case. The planet accumulated at low temperature, and it had not become sufficiently hot, owing to radioactive heating, to form a core. Today, it still seems that this is the case. Mars has no magnetic dipole field, and the constants still seem to be in accord with this conclusion.

This led to the question as to whether the earth was formed as a mixture of metal and silicate rocks without a metal core and had formed such a core during its history. It is generally believed that, if this is the case, the core was formed early in terrestrial history. The changing moment of inertia has provided a way of separating the moon from the earth by those who favor this origin. Also, single cell convection in the earth, which is possible only in an earth without a core, has been a favorite explanation in the theory that all the continental masses formed on one side of the earth and were subsequently scattered over the earth by several convection cells which developed later, after a core had formed. The timing of these events proved to be very difficult, and this theory is generally not thought to be true.

Was the moon also formed in a low-temperature condition? Its moments of inertia deviate markedly from those expected for a body under the forces of rotation and terrestrial tidal effects, and, if the moon is a body whose density does not vary with angular position, a stress of 19 bars should exist at the center of the moon, implying considerable strength somewhere in the body of the moon. Also, high mountains exist on the moon, and, since mountains on the earth are in fairly good isostatic adjustment, it appeared that the moon's mountains, if they had no low density roots, were now supported and had been supported throughout lunar history by more rigid rocks than now exist below the earth's surface. Thermal calculations made in the early 1950's indicated that this was difficult to explain unless the moon began its history at a fairly low temperature.

No one was very happy about this conclusion. My co-workers and I (*3*) suggested the formation of the moon from a limited number of objects of variable density, which made possible a statistical variation of density along the three axes, and thus permitted isostatic conditions in the moon as a whole but also permitted the observed variation in moments of inertia. Levin (*4*) pointed out that lower temperatures at the poles were another way of securing density variations at the polar and equatorial regions. Runcorn (*5*) has argued for deep convection currents that produce similar effects. Others (*6*) have pointed out that a cold outer shell of considerable thickness would support the variations required and yet permit a hot interior. The past history of the moon has always been a problem in some theories, for the various differences in moments of inertia that have been produced imply the ability of the surfaces to move at one time and then to freeze later.

There was no change in this situation until Muller and Sjogren (*7*) discovered positive gravitational anomalies and, thus, the mascons in the circular maria and a few other places. Although I had suggested the presence of metal masses in these maria (*8*), this discovery was a surprise to me and to everyone. The excess masses required in the larger maria are very large, and, since the maria are not elevated above the surrounding area—in fact, they are depressed areas—the total masses must be several times the excess masses. Thus, if the mass in Mare Imbrium has a density that is 10 percent larger than the mean density of the surrounding material, it would cover the inner area of the mare (680 km in diameter) to a depth of 12 km. (This area is slightly less than the area of California.) The excess pressure beneath the mass, if the object covers this entire area, is 70 bars. If the object is metallic iron, the mass is less but the excess pressure remains the same. The support of these masses requires great strength in the moon, and this strength has been required since their formation, presumably since the very early history of the moon. One must conclude that the radioactive heating of the moon has not been sufficient to weaken an outer rigid shell that supports the mascons. If the potassium, uranium, and thorium concentrations in the moon are equal to those of the chondritic meteorites, this is very difficult to understand, even if the moon's interior began its history at 0°C. After Wasserburg *et al.* (*9*) pointed out that the concentration of potassium in the earth is probably much less than this concentration in chondritic meteorites, I made calculations on the thermal history of the moon (*10*), which indicated that lowered concentrations of potassium would indeed meet this difficulty. MacDonald and I (*11*) have therefore adopted the potassium abundances of the type III carbonaceous chondrites.

The demonstration by Turkevich *et al.* (*12*) that the surface regions of the moon were highly differentiated, and the subsequent confirmations of these results by the analyses of samples from the Apollo 11 and Apollo 12 missions, showed that both titaniferous basalt and anorthosite were present on the lunar surface; thus, a modification of previous ideas in regard to lunar structure was required. A model of a melted surface layer and a cool interior for the early structure of the moon was adopted (*13*). It was assumed, and is now assumed, that the mascons are supported and were supported throughout the moon's history on the originally cool interior, which has not become so hot that the maria surfaces and their mascons could sink about 1 km and remove the anomaly. It appears that a general surface layer of anorthositic material must cover the entire moon and that a basaltic type of material has flowed out over the anorthositic material in the area of the maria. This follows from Turkevich's results and from the work of others [see Wood *et al.* (*14*)]. The dating of the rocks shows that an early melting occurred some 4.4 to 4.6 eons ago and was followed by other melting processes up to about 3.25 eons ago to produce the basaltic rocks and some more acidic ones. This model for the moon is under active discussion at the present time, and Baldwin (*15*) has proposed a very similar model recently. He suggests collisional heating as a method of heating the outer regions and leaving the interior cold, but I find it difficult to believe that a very substantial fractionation process could exist while temperatures could simultaneously be maintained by a collisional process that would mix materials badly. I have suggested in the past that collisions produced melting. Although I still believe that some melting may have occurred in collisions, I doubt that this was the principal source of energy to produce any large melting processes on the moon. In particular, anorthosite melts at fairly high temperatures and would hardly be, as Baldwin suggests, the first fraction of approximately meteoritic material to melt below the surface and to flow out like terrestrial lava. A very puzzling part of lunar history is the necessity for a surface melting to have occurred some 4.5 eons ago, followed by a cooling, to produce rigid rocks at a time when radioactive heating within the moon was at a maximum. The intense collisional processes that produced the many craters on the highlands of the moon, including the craters Albategnius and Ptolemaeus with their negative gravitational anomalies and the great collisional maria and their positive gravitational anomalies, followed this cooling process. Then, some 10^9 years later, melted rocks were produced, presumably by radioactive heating processes, when the concentrations of radioactive nucleids had decreased substantially! Some physical explanation of this difficulty is required, and such will be attempted in the near future (*16*).

Recent physical observations bear on these problems. The present interpretations of the seismic evidence indicate that no solid sheets of silicate rocks exist below the surface of Oceanus Procellarum (*17*). These areas may be ash flows containing liquid masses that have solidified. Also, the studies of the flow of magnetic fields over the moon appear to indicate moderate temperatures at the present time, which is quite consistent with a lower temperature history in the past (*18*).

The idea that the moon may have been formed at low temperatures, with melting produced by collisions, was developed about 20 years ago, and I disagree with many things that I advanced at that time, when little detailed information was available. If one waits for detailed information before attempting interpretations of the data available at an early stage, one is less likely to make mistakes. However, in the case of an initially cool moon, it seems that I was more right than wrong, as will be recognized by those who attempt to understand the arguments. At no time have I thought that the interior of the moon is cold at present. To me, the present moon has always been on the verge of melting and assuming an equilibrium shape as a whole. Since the discovery of mascons, I have wondered whether, after some 4.5 billion years, these objects might not settle about 1 km and remove the anomalies. But also, I find it difficult to understand how these objects could have been supported by the moon if it were at the melting point throughout at the time of origin or if it had produced massive lava flows from the interior at some time in its history. If the far side of the moon had faced the earth instead of the near side, there would have been little contrary argument until analyses become available. But the additional data present problems to those of us who try to account for all the lines of evidence.

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16 February 1971

Characterization of Virulent Bacteriophage Infections of Escherichia coli in Continuous Culture

There are several points in the article by Horne (*1*) on bacteriophage infections in continuous culture which deserve comment. He reported only measurements of plaque titers and of viable bacteria but was able to note changes in both the bacteria and phage with time. We have studied a very similar system of *Escherichia coli* B with T2 phage in which a more detailed population analysis was performed (*2, 3*). It was shown that resistant strains increased dramatically, but sensitive organisms persisted as a significant proportion of the total cells. Although Horne states that no regular pattern could be discerned in the first 20 to 40 hours, we observed a definite relationship in which bacterial peaks and declines were followed by corresponding fluctuations in phage titer.

Although there are probably differences in the behavior between T2 phage and the T3 and T4 phages studied by Horne, he described only one new type of phage appearing, whereas we noted several distinct phage mutants based on plaque morphology (*3*). In contrast to the parent organism, our T2 resistant bacterial cells formed mucoid colonies on nutrient agar and could not be infected by high concentrations of stock T2 phage. Resistant cells had the common property of having thick capsules, but the sensitive cells showed none. However, capsulation of a culture prior to infection was found to offer only partial protection against phage attack (*4*).

Although Horne successfully maintained his systems for very long periods of time, we think that his dilution rate (0.04 hr^{-1}) was relatively low so that his observations were complicated by the presence of too many cells not in the active growth phase, a condition necessary for phage production. Furthermore, many significant details of population behavior were missed by not distinguishing between the various types of cells present. Viable cells, total cells, infected cells, sensitive uninfected cells, and resistant cells can all be measured, and such data do elucidate the evolution of cell types. The systems are much more complex than Horne's limited data would suggest.

Many investigators use chemostat to refer to a continuous culture system, but to avoid confusion, we are of the opinion that this bacterium-phage culture was not in chemostasis as implied by the title of the article.

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My report (*1*) gave some preliminary findings of a study which investigated the usefulness of this system as a model of host-parasite, prey-predator situations, to which it has many analogies and where mathematical formulation is simplified by the absence of a search component. Emphasis was placed on the effects on the components of the population of selection over very extended periods of time (*2*). The published data were of populations growing at a low rate chosen as typical of some natural and seminatural habitats of these organisms where many bacterial cells may be growing extremely slowly (*3*), a condition which does not preclude phage growth. However, populations of *Escherichia coli* B and phages T3 and T4r have been grown at higher dilution rates (0.16 hr^{-1}). These similarly come to an equilibrium, the stability of which increases with time; they differ from slower growing populations by maintaining relatively higher titers of the phage component during each stage of the interaction. Inverse correlations of phage and bacterial numbers are common in the early stages of such interactions [figure 1 in (*1*)], but *regularity* of the amplitude and frequency of the cycles, even after an extended period of coevolution, is not found and is probably not to be expected where the potential rates of increase of the two components are so very different.

That coevolution of the components of these populations involves several successive changes in the relationship of bacterial resistance and phage host-range is generally accepted (*4*). Many hundreds of phage-resistant mutants (*5*) and of phage host-range mutants (*6*) have been isolated. A frequent characteristic of the former is a "watery" or "mucoid" appearance of the colony (*5*).

Evidence of the heterogeneity of the phage component in continuous culture populations is reflected in the diverse plaque morphology which is a normal feature of the early stages of the interaction. However, here the usefulness of such mutants is limited since mutations expressed as differences in plaque morphology do not necessarily correspond to differing abilities of the phage to reproduce in the selected populations. Also the extreme minuteness of plaques isolated from later stages of the interaction—reported not as a new mutant of the phage T3 but as a characteristic of all T-series phages grown in such populations and probably resulting from an accumulation of many mutations affecting the reproductive capacity of the phage—makes categorization of the phage mutants by plaque morphology unsatisfactory.

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