The Occurrence of Planets

I wish to point out several errors and an inconsistency in Harrison Brown's report, "Planetary systems associated with main-sequence stars" (*1*). He begins by extrapolating the Salpeter luminosity function, which is nearly linear over an interval of 12 magnitudes, down to planetary masses. He then assumes that stars and planets are formed independently in groups of random size distribution. He concludes that great numbers of planets should exist, both as companions of stars and in invisible clusters of planetary objects. There is considerable evidence, however, that his basic assumptions are unjustified.

Brown's assumption that the Salpeter-van Rhijn luminosity function can be extrapolated down to low masses is extremely weak. The determination of the "observed" luminosity function is itself very difficult and uncertain because of the incompleteness and observational bias in the discovery of nearby stars. Recently, Wanner (*2*) has made a new determination of the luminosity function, using new and greatly extended observational data and an improved statistical technique. His luminosity function is practically constant from $M_B = +6$ to $+16$; in fact it peaks at $M_B = +9$. If Brown had used Wanner's luminosity function instead of the Salpeter-van Rhijn function, he would have predicted only half a planet (in the range of mass from Jupiter to one "Mars-equivalent") per star.

However, regardless of what luminosity function is adopted, the assumption that it represents a uniform mass-distribution function that can be linearly extrapolated over several orders of magnitude is certainly unsound. For the general luminosity function represents a mixture of stellar populations, with different ages, chemical compositions, and places of origin; and the proportions of the mixture vary with luminosity.

Even if the luminosity function itself were uniform and linear, the nonlinearity of the mass-luminosity relation would make the derived mass-frequency function nonlinear. Brown tacitly admits this by restricting his mass-frequency function to stars fainter than $M_v = 10$, thereby abandoning the upper main sequence and making irrelevant his previous discussion of the Salpeter function and evolutionary effects. This reduces the observational basis of his linear extrapolation to a range of only 3 magnitudes, or less than a factor of 4 in mass. To use such a short baseline to extrapolate over a factor of 1000 toward smaller masses—particularly when an extrapolation by even a factor of 10 in the other direction is clearly wrong—seems pointless.

Brown also assumes that stars and planets are formed "in discrete regions of space separated from each other by interstellar distances, and that within each region a cluster is formed containing an average of n bodies." He identifies each cluster as a multiple system in which typically only one or two objects are stellar. But the most convincing examples of recent star formation—the O-associations—indicate that such systems are formed much closer together than typical interstellar distances; for example, in the Trapezium cluster the star density is about 1000 times greater than that in the general field. When they were formed, these stars must have been even more crowded than at present, and interactions may have been important in determining the distribution of masses within each multiple-star system.

In fact, the assumption that the mass-distribution function in multiple systems is the same as that for single stars *is* contradicted by observational evidence. Blaauw (*3*) has reported that "For systems with primaries of a given mass . . . the frequency of the mass ratios between secondary and primary increases with decreasing value of this ratio, but not nearly as strongly as one would expect if the secondary masses were distributed according to the Initial Luminosity Function." In other words, companions of low mass are much less common than would be expected from Brown's assumption of randomness.

To sum up, Brown's argument seems too weak to support an expectation of either an abundance or a scarcity of planets in the universe.

ANDREW T. YOUNG
Harvard College Observatory, Cambridge, Massachusetts

References

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3. A. Blaauw, *Ann. Rev. Astron. Astrophys.* **2**, 231 (1964).

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Cytokinins

In 1955 we proposed the term *kinin* as a generic name for synthetic and naturally occurring substances that exhibit the same types of biological activity as does kinetin (6-furfurylaminopurine). We soon recognized the priority in the use of this term for a group of materials of animal origin and with physiological properties quite different from those of kinetin. However, we did not at that time expect serious conflict in the unfortunate duplicate usages of the term.

Recently it has become increasingly clear that much confusion and inconvenience may arise in indexes and the like, and therefore various names have been used to designate kinetin-like materials. On the urging of colleagues and on the basis of the expressed preference of several investigators for it, we now propose *cytokinins* as a generic term to replace kinins for designating all substances with kinetin-like biological activity. This term is based on the activity of these chemicals in promoting cytokinesis in cells of various plant origins and possibly also in some cells of animal origin. It is recognized that the activity of these chemicals may be expressed also in other ways, as in altered metabolic rates, enzyme activities, or nucleic acid contents, in cell enlargement, in induction of organ formation, in release of apical dominance, in mobilization of organic and inorganic nutrients, and in increased longevity of tissues and organs.

The term cytokinins will include kinetin (as a specific chemical) and 6-(γ,γ-dimethylallylamino)-purine, 6-benzyladenines, and other active synthetic purine derivatives, as well as Zeatin [6-(γ-methyl-γ-hydroxymethylallylamino)-purine], the active substance from corn endosperm, and active natural products of as yet unknown composition. Substances such as triacanthine [3-(γ,γ-dimethylallyl)-adenine], deoxyadenosine, or 1-substituted adenines, which acquire kinetin-like growth-promoting activity only as a consequence of chemical change, may act as precursors but are not considered to be bonafide cytokinins.

At present rigid proof of cytokinin activity is limited entirely to 6-substituted purine derivatives. Other types of substances may be found to qualify, but in our opinion those which have been reported so far to promote growth