Haemophilus, 38 percent; the thermophilic actinomycetes, 44 to 77 percent; *Proteus vulgaris*, 39 percent; *P. morganii*, 50 percent; mycoplasmas, 23 to 39 percent; *Neisseria catarrhalis*, 41 percent; *N. meningitidis*, 51 percent; and variations within genera *Bacillus*, 33 to 53 percent; *Clostridium*, 27 to 48 percent; *Halobacterium*, 55 to 68 percent; *Lactobacillus*, 34 to 53 percent; and marine *Micrococcus*, 57 to 73 percent.

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References and Notes

1. C. E. Singer and B. N. Ames, *Science* **170**, 822 (1970).
2. A complete list of references on G + C contents in microorganisms is available from the authors on request.
3. G. Darland, T. D. Brock, W. Samsonoff, S. F. Conti, *Science* **170**, 1416 (1970); J. Stenesh, B. A. Roe, T. L. Snyder, *Biochim. Biophys. Acta* **161**, 442 (1968); T. D. Brock and H. Freeze, *J. Bacteriol.* **98**, 289 (1969); R. Craveri, L. R. Hill, P. L. Manachini, L. G. Silvestri, *J. Gen. Microbiol.* **41**, 335 (1965); H. Prauser, *Spisy Prirodoved. Fak. Univ. J. E. Purkyne Brne Ser. K* **38**, 268 (1966).
4. M. R. Zelle and A. Hollaender, *Radiat. Biol.* **11**, 365 (1955); R. H. Haynes, in *Physical Processes in Radiation Biology. International Symposium on Physical Processes in Radiation Biology, 1963* (Academic Press, New York, 1966), p. 51.
5. H. P. Treffers, V. Spinelli, N. O. Belser, *Proc. Nat. Acad. Sci. U.S.* **40**, 1064 (1954).
6. T. C. Gibson, M. L. Scheppe, E. C. Cox, *Science* **169**, 686 (1970).
7. E. C. Cox and C. Yanofsky, *Proc. Nat. Acad. Sci. U.S.* **58**, 1895 (1967).
8. E. B. Freese and E. Freese, *ibid.* **57**, 650 (1967).
9. J. De Ley, *Antonie van Leeuwenhoek J. Microbiol. Serol.* **34**, 109 (1968); M. F. Bacon, W. G. Overend, P. A. Lloyd, A. R. Peacocke, *Arch. Biochem. Biophys.* **118**, 352 (1967).
10. B. H. Hoyer and N. B. McCullough, *J. Bacteriol.* **95**, 444 (1968).
11. B. W. Catlin and L. S. Cunningham, *J. Gen. Microbiol.* **26**, 303 (1961).
12. A. N. Belozersky and A. S. Spirin, in *The Nucleic Acids*, E. Chargaff and J. N. Davidson, Eds. (Academic Press, New York, 1960), vol. 3, p. 147.
13. L. Bouisset, J. Breuilland, G. Michel, G. Larrouy, *Ann. Inst. Pasteur* **115**, 1063 (1968).
14. M. Sebald, F. Gasser, H. Werner, *ibid.* **109**, 251 (1965).
15. F. Gasser and M. Mandel, *J. Bacteriol.* **96**, 580 (1968).
16. L. Bouisset, J. Breuilland, G. Michel, *Ann. Inst. Pasteur* **104**, 756 (1963).
17. M. Mandel and R. Rownd, in *Taxonomic Biochemistry and Serology*, C. A. Leone, Ed. (Ronald, New York, 1964), p. 585; M. P. Starr and M. Mandel, *J. Gen. Microbiol.* **56**, 113 (1969); J. N. Baptist, C. R. Shaw, M. Mandel, *J. Bacteriol.* **99**, 180 (1969).
18. C. A. Ouelette, R. H. Burris, P. W. Wilson, *Antonie van Leeuwenhoek J. Microbiol. Serol.* **35**, 275 (1969).
19. M. Rogul, J. J. Brendle, D. K. Haapala, A. D. Alexander, *J. Bacteriol.* **101**, 827 (1970).
20. M. A. Hogan and R. R. Colwell, *J. Appl. Bacteriol.* **32**, 103 (1969).
21. G. F. Saunders, L. L. Campbell, J. R. Postgate, *J. Bacteriol.* **87**, 1073 (1964); N. Sigal, J. C. Senez, J. L. Gall, M. Sebald, *ibid.* **85**, 1315 (1963).
22. E. Canale-Parola, Z. Udris, M. Mandel, *Arch. Microbiol.* **63**, 385 (1968).
23. M. Mandel and R. A. Lewin, *J. Gen. Microbiol.* **58**, 171 (1969).
24. M. Mandel, O. B. Weeks, R. R. Colwell, *J. Bacteriol.* **90**, 1492 (1965); A. J. Hansen, O. B. Weeks, R. R. Colwell, *ibid.* **89**, 752 (1965).
25. J. Boháček, M. Kocur, T. Martinec, *J. Appl. Bacteriol.* **31**, 215 (1968).
26. J. De Ley and J. van Muylem, *Antonie van Leeuwenhoek J. Microbiol. Serol.* **29**, 344 (1963).
27. R. V. Citarella and R. R. Colwell, *J. Bacteriol.* **104**, 434 (1970); R. R. Colwell, V. F. Adeyemo, H. H. Kirtland, *J. Appl. Bacteriol.* **31**, 323 (1968); R. R. Colwell and M. Mandel, *J. Bacteriol.* **88**, 1816 (1964).
28. E. Leifson and M. Mandel, *Int. J. Syst. Bacteriol.* **19**, 127 (1969).
29. T. D. Brock and M. Mandel, *J. Bacteriol.* **91**, 1659 (1966).
30. We thank N. O. Kjeldgaard and A. Stenderup for their critical review of the manuscript. J.F.A. is supported by a fellowship from the European Molecular Biology Organization.

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We have previously proposed (*1*) that bacterial species exposed to sunlight evolved high G + C contents to avoid thymine specific damage from the ultraviolet radiation in sunlight, and this could be one of the explanations for the wide variation of G + C ratios in bacteria. We presented evidence that there was a good correlation between the amount of sunlight to which bacterial "genera" were normally exposed and their G + C content. We also presented calculations on the amount of ultraviolet that bacteria were exposed to and its effect. Leth Bak *et al.* (*2*) take issue with us in that they find a number of exceptions to our generalization. They consider a fairly small number of individual species, whereas we considered "genera" so as to reduce the influence of minor fluctuations (due to misclassification, poorly understood habitats, and so forth) on individual species, and we considered all 73 "genera" whose G + C contents were known at that time. We still think that the overall correlation is striking and that some individual exceptions are to be expected, in part because of the difficulties in classification and the uncertainties of determining the normal habitats of bacteria in nature.

The alternate explanation of Leth Bak *et al.* that the variation in G + C contents is random (and therefore not otherwise explicable) was one that we discussed (*1*, reference 26), and we pointed out that a *truly random* distribution of G + C contents would leave virtually all bacteria within 1 percent of the mean. This implies that G + C content must reflect underlying, evolutionary forces. We still hold that ultraviolet damage is a tremendous force in the life of microorganisms, that organisms with a high G + C content would be more resistant to damage by ultraviolet, and that this seems the most likely evolutionary force to explain the variation in G + C ratios. Further work on the normal life habitats and evolution of microorganisms will presumably clarify this question.

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References

1. C. E. Singer and B. N. Ames, *Science* **170**, 882 (1970).
2. A. Leth Bak, J. F. Atkins, S. A. Meyer, *ibid.* **175**, 1391 (1972).

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Bohr Atom: A Remark on the Early History

In his excellent book (*1*) *The Conceptual Development of Quantum Mechanics*, Max Jammer discusses a paper (*2*) by my teacher, F. Hasenöhrl, and one of mine (*3*), based on it. He says (p. 75):

> In March 1912 Herzfeld proposed a modification of Thomson's model by assuming circular electronic orbits and a non-uniform charge density of the positive sphere and derived from these assumptions the Balmer series by a quantization of energy in accordance with a rule formulated by Hasenöhrl as generalization of Planck's prescription for the quantization of the harmonic oscillator. But all these and similar calculations . . . lost their validity with the abandonment of the Thomson model on which they are based.

While this description is historically correct, the last sentence misses, in my opinion, an essential point. Hasenöhrl used no model whatever, while the model I used is sufficiently general.

The matter might seem too trivial to discuss further, but a point which is quite important in my opinion and has not been emphasized elsewhere is closely connected with it. Since I am the only survivor of the three involved—Bohr, Hasenöhrl, and myself—I may be permitted to dwell on the matter; I will talk from hindsight, not from what was known at the time.

In 1911, the only material system quantized was the harmonic oscillator [(*4*); Planck, solid state], for which the quantum condition could be written

$$\epsilon_n/\omega = \hbar n \qquad (1)$$

where ϵ_n is the energy of quantum state n, ω is the angular frequency, and $\hbar$ is Planck's constant. (I will ignore the problem of zero-point energy.) It is characteristic for this case that the frequency is independent of the energy. Hasenöhrl looked for a generalization applicable to cases where ω depends on the energy.

He started out with the equations of classical mechanics in the form they take if action and angle variables are used [(*5*); periodic, one-dimensional system].

$$dE/dJ = \omega \qquad (2)$$

where E is the total energy and J, the action variable, is an adiabatic invariant.

From Eq. 2 follows

$$dE/\omega = dJ \qquad (3a)$$

or

$$\int_{E'}^{E} dE/\omega = J - J' \qquad (3b)$$

Hasenöhrl then introduced the quantum condition

$$J - J' = (n - n')\hbar \qquad (4a)$$

or

$$\int_{E'}^{E} dE/\omega = (n - n')\hbar \qquad (4b)$$

He then evaluated, as an example, the connection between E and ω when the relation between ω and n is given by the Balmer formula. No model was used or mentioned.

My paper, based on Hasenöhrl, introduced a modified Thomson model. The original Thomson model contained an electron moving in a sphere of positive electrical density ρ, ρ being constant. This made the electron behave as a simple harmonic oscillator, with a frequency independent of the energy, and, in reaction to an incoming wave, in accordance with classical dispersion theory. I modified this model by having the electron move on a circle around the origin and allowing the density to depend on the radius. The density can then be determined so as to give any prescribed connection between E and ω. This model could, of course, not give the correct dispersion theory, but only quantum mechanics can do that.

Then why did our papers not lead to the correct result? Why did I not find the Rutherford model, which is, of course, a "modified Thomson model" with the positive density a (three-dimensional) delta function?

The answer is that we—like everybody before us and everybody after us until Bohr's papers—identified the frequency of the mechanical motion with that of the emitted light, that is, coupled the mechanical system to the radiation field electromagnetically.

In my opinion, the most fundamental innovation Bohr introduced, which made a theory of atoms and molecules and their spectra possible, was the fact that he applied quantization twice, once to the mechanical system and once to the radiation field, and coupled the two not electromagnetically but by the principle of conservation of energy (*6*). In the case of the Planck oscillator the mechanical system and the radiation field have Hamiltonians of the same form, so that the relation between energy and frequency is the same (*7*), and it does not matter for the result whether the coupling is electromagnetic or by energy conservation; in every other case, the result is different for the two.

Let us see what would have followed if Hasenöhrl had known this principle (together with the Ritz intercombination principle). He would then have concluded that the energy levels were given by

$$\epsilon_n = - R\,c\,2\pi\hbar/n^2 \qquad (5)$$

where R is the Rydberg constant and c is the velocity of light.

Let us see what one can conclude if

$$\epsilon_n = -f(n^2)$$

is given and one makes as the only model assumption that of circular orbits in a central field of potential energy $V(r)$. One gets for the centrifugal force

$$mv^2/r = dV/dr \text{ or } mv^2 = r\,dV/dr \qquad (6)$$

where m is mass, v is velocity, and r is radius.

$$n^2 = \frac{J^2}{\hbar^2} = \frac{m^2v^2r^2}{\hbar^2} = \frac{m}{\hbar^2}\,r^3\frac{dV}{dr} \qquad (7)$$

Therefore one has the differential equation (*8*) from Eq. 5

$$\frac{r}{2}\frac{dV}{dr} + V = -f\left(\frac{m}{\hbar^2}\,r^3\frac{dV}{dr}\right) \qquad (8)$$

This is a first-order differential equation for V. In particular, in the Balmer case (Eq. 5)

$$r^3\frac{dV}{dr}\left(\frac{r}{2}\frac{dV}{dr} + V\right) = -\frac{2\,\pi\hbar^3cR}{m} \qquad (9)$$

which has the solution (*9*)

$$V = -\frac{A}{r}\,;\, A^2 = \frac{4\,\pi\,cR\hbar^3}{m} \qquad (10)$$

and one immediately concludes that the Rutherford atom is the correct "modified" Thomson model for this case.

From the description in Jammer it does not seem as if Bohr, at the time of his first papers in 1913, was aware of the fundamentally new procedure he had introduced; he seemed to look for connections between the mechanical frequency and that of the emitted light in a very indirect manner; however, this might have been pedagogical (*10*).

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References and Notes

1. M. Jammer, *The Conceptual Development of Quantum Mechanics* (McGraw-Hill, New York, 1966).
2. F. Hasenöhrl, *Phys. Z.* **12**, 931 (1911).
3. K. F. Herzfeld, *Wien. Sitzungsber.* **A 121**, 593 (1912).
4. Jammer says correctly on p. 75 (*1*): "All previous applications of Planck's constant to atomic models referred to the Thomson atom, and *were generally based on the assumption of harmonic oscillations*" (emphasis mine).
5. For example, see H. Goldstein, *Classical Mechanics* (Addison-Wesley, Cambridge, Mass., 1950), p. 292, equations 9–38.
6. Indications of this independent quantization of the field may be found in Einstein's light-quantum idea [*Ann. Phys.* **17**, 132 (1905); *ibid.* **20**, 199 (1907); *Phys. Z.* **10**, 185, 817 (1909)] but there is no explicit statement about independent quantization of the mechanical system. See also Einstein's remark quoted by M. Klein [*Paul Ehrenfest* (American Elsevier, New York, 1970), vol. 1, p. 278].
7. F. Hund [*Phys. Today*, August 1966, p. 23] has pointed out that in the case of large quantum numbers n with small changes in n the mechanical frequency and the light frequency approach each other asymptotically. But in 1911–1912 such cases were unknown experimentally [A. H. Pfund found the transition 5 → 6 only in 1924 (*J. Opt. Soc. Amer.* **9**, 133)], and the theoretical basis, the correspondence principle, also had not been discovered.
8. This implies the assumption that the relation $\epsilon = -f(J^2/\hbar^2)$ is valid, not only for $J = n\hbar$, but for all values of J.
9. This is a singular solution, without integration constant. The sign is determined by Eq. 6. Equation 9 also has a regular solution (integration constant a)
$$V = -\left(\frac{\pi}{m}c\,R\,\hbar^3\right)^{1/2}\left(\frac{1}{a} + \frac{a}{r^2}\right)$$
which, however, does not satisfy Eq. 7, the square of the angular momentum having a value fixed by the potential $(\pi\ c\ R\ \hbar^3\ m)^{\frac{1}{2}}\ 2a$. In addition the total charge is infinite, $\rho \sim 1/r^4$.
10. See J. L. Heilbron and T. S. Kuhn, "The genesis of the Bohr atom," in *Historical Studies in the Physical Sciences*, R. M. McCormmach, Ed. (Univ. of Pennsylvania Press, Philadelphia, 1969), vol. 1, p. 266.

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