B1913+16. Orbital parameters of the system are determined in a purely phenomenological way, without assuming any specific theory of gravity. The combination of phenomenological parameters can then be compared with the predictions of any specific relativistic theory of gravity, such as general relativity. The key point is that timing observations give access to more phenomenological parameters than the number of independent dynamical degrees of freedom. This redundancy allows one to extract one or more tests of any theory of gravitation. In this way Einstein's theory passes several independent tests, including the crucial one involving radiation damping effects—currently at the 0.3% level—thereby verifying the predicted existence, quadrupolar nature and propagation speed of gravitational waves.

We have no reason to think that general relativity necessarily comprises the last valid words to be written about gravity, not least because the theory is not perturbatively quantizable. Perhaps most importantly, binary-pulsar timing experiments offer a means of probing the nature of gravity under strong-field conditions that do not exist anywhere within the solar system. With such experimental tools, large regions of gravitational "theory space" can be summarily rejected. In the future, one might even hope to isolate a region in which the "true" theory of gravity departs from general relativity. In the meantime, Einstein's theory of gravity remains the best thing going in the nonquantum regime, in full accord with all high-precision experimental tests, *including* gravitational-radiation losses from binary pulsar systems.

References

1. J. H. Taylor, J. M. Weisberg, Astrophys. J. **345**, 434 (1989). J. H. Taylor, A. Wolszczan, T. Damour, J. M. Weisberg, Nature **355**, 132 (1992). J. H. Taylor, in *General Relativity and Gravitation 1992*, R. J. Gleiser, C. N. Kozameh, O. M. Moreschi, eds., Institute of Physics, Bristol, UK (1993), p. 287.
2. T. Damour, J. H. Taylor, Phys. Rev. D **45**, 1840 (1992), and refs. therein.
3. T. Damour, in *Gravitational Radiation*, N. Deruelle, T. Piran, eds., North Holland, Amsterdam (1983), p. 59. T. Damour, in *300 Years of Gravitation*, S. W. Hawking, W. Israel, Cambridge U. P., Cambridge, UK (1987), p. 128.

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New Data Advance Retarded Forces

We read with great interest the Search and Discovery story "New Evidence Confirms Old Predictions of Retarded Forces," by Barbara Goss Levi (April 1993, page 18). We wish to mention two other kinds of experiments that are very pertinent to the issue of retardation. One experiment was a pioneering study by Ed Sabisky and Charles Anderson,[1] who demonstrated that liquid helium films possess a thickness dependence on gas pressure that agrees remarkably well with the retardation theory of Igor E. Dzyaloshinskii, Evgenii M. Lifshitz and Lev P. Pitaevskii[2] (although more recent data are not so consistent). The second experiment is an ongoing project in the group of Thomas Greytak and Daniel Kleppner.[3] This experiment measures the sticking at ultralow energy of a hydrogen atom to a cold liquid He surface. Calculations[4] indicate that the sticking coefficient is reduced by a factor of two when retardation is taken into account! The latest data[5] are consistent with this prediction. We are optimistic therefore about the prospect for further experimental validation of this fundamental concept.

References

1. E. S. Sabisky, C. H. Anderson, Phys. Rev. A **7**, 790 (1973).
2. I. E. Dzyaloshinskii, E. M. Lifshitz, L. P. Pitaevskii, Adv. Phys. **10**, 165 (1961).
3. J. M. Doyle, J. C. Sandberg, I. A. Yu, C. L. Cesar, D. Kleppner, T. J. Greytak, Phys. Rev. Lett. **67**, 603 (1991).
4. C. Carraro, M. W. Cole, Phys. Rev. B **45**, 12 930 (1992).
5. I. A. Yu, J. M. Doyle, J. C. Sandberg, C. L. Cesar, D. Kleppner, T. J. Greytak, Phys. Rev. Lett. **71**, 1589 (1993).

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NASA, Congress and Gomory's 'Goals'

Much of what Ralph Gomory says about Federally funded science in his article "Goals for the Federal Role in Science and Technology" (May 1993, page 42) is indisputable, but his comments on "the huge bill the government pays NASA" call for a personal reply on some specific points.

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Gomory criticizes the $2 billion per year spent on "space probes," arguing that support of individual investigators would be a better way to spend the money. As it happens, much of the NASA space science budget is spent on individuals. The 1991 directory of research projects for NASA's planetary geology and geophysics program, for example, shows that grants between $40 000 and $80 000 were made to 82 individual investigators from various universities, in addition to those from nonprofit private organizations and government laboratories.

Gomory compares (unfavorably) the money spent on space probes with the total spent on individual investigators by NSF. But it should be remembered that NASA research support is, after all, an *additional* funding source for individual proposals.

The very term "space probes" suggests a misunderstanding of space research. The first discoveries made from space concerned the *Earth*: its shape, its gravity field and its radiation belts.[1] The first direct verification of plate movements was accomplished with satellite laser ranging (confirmed by very-long-baseline interferometry), an accomplishment simply impossible with conventional surveying methods. It should also be pointed out that NASA's "huge bill" has over the decades included development of space applications whose value is unquestioned: weather, communications and Earth resources satellites.

And true "space probes" have produced discoveries of fundamental importance, inherently unapproachable by Earth-bound research: the structure of the Big Bang, now expressed as the cosmic background radiation; radar mapping of an Earth-sized but cloud-covered planet; and many others often described in PHYSICS TODAY. These accomplishments are the result of teams of "individual investigators," to use Gomory's phrase. Finally, many "space probes" are hardly "megaprojects." The Cosmic Background Explorer, for example, would fit into a large pickup truck; Vanguard I was compared to a grapefruit; Sputnik 1, to a basketball.

A final point: Gomory suggests that "we need to ask some questions" about the NASA space budget. Congress spends months every year asking exactly such questions. The Space Exploration Initiative has just been abandoned because of Congressional opposition, and the Office of Exploration abolished. Furthermore, NASA research proposals are peer-reviewed by outside scientists who also "ask some questions."

Reference

1. P. D. Lowman Jr, Endeavour **16**, 50 (1992).

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Ralph Gomory's article "Goals for the Federal Role in Science and Technology" was such a sensible and informative analysis of what the nation needs to do in this area that I sent a copy of it to my senators and representative in Congress, an act that I hope is consistent with the "fair use" principle of your copyright statement. If so, I urge that a person from each Congressional district in the country do the same thing.

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Scientists Supporting SSC Quoted Unfairly?

I find it interesting that in his letter in the March 1993 issue (page 13), Lon Hocker placed quotation marks around the word "scientists" when applying it to supporters of the SSC—the implication being, I suppose, that supporters of the SSC are not legitimate members of the scientific priesthood. Such tactics seem out of place in a purportedly rational debate.

As for the question of whether big or small science gives greater economic and technological payoffs, it seems largely irrelevant. Aside from members of Congress, few people try to justify the SSC in terms of spinoffs. The issue is, bluntly, Is it worth doing, and can we afford it? Hocker's judgment is no on both counts. Others may disagree without necessarily being scoundrels.

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CO_2's Greenhouse Contribution Debated

Jocelyn Tomkin (December 1992, page 13) berates Alison Campbell (February 1992, page 15) for stating that "were it not for atmospheric CO_2, the mean temperature at the Earth's surface would be substantially below zero." Tomkin estimates that the complete removal of CO_2 from the atmosphere would decrease the surface temperature by only about 1 °C.

Tomkin's estimate is essentially based on the assumption that the magnitude of the decrease of the outgoing long-wave irradiance at the top of the atmosphere that would be produced by instantaneously doubling the CO_2 concentration is the same as that of the increase that would be produced by instantaneously removing the CO_2 entirely.

This is incorrect. At some wavelengths (near the center of the 15-μm CO_2 band, for example) there is already so much CO_2 that the atmosphere is opaque over short distances, so adding more CO_2 is ineffective; an equivalent reduction has a much greater effect. Using a tested radiative transfer model,[1] one finds that for a midlatitude clear-sky atmosphere, doubling the CO_2 concentration from 350 to 700 parts per million by volume decreases the irradiance at the top of the atmosphere by 3.5 W/m^2; removing the CO_2 entirely increases it by 32 W/m^2.

Corresponding estimates of the effect on the mean surface temperature of the Earth are much more complicated, as both Campbell and Tomkin say. But using a simple radiative convective model,[1] with no other change, one finds that doubling the CO_2 produces a 1.5 °C warming and removing it a 12 °C cooling. Including a simple relative humidity feedback (but no ice-albedo feedback) changes these values to 2.4 °C warming and 17 °C cooling.

Such a cooling would represent a huge change to the present climate, so 17 °C must be taken as a crude estimate. Including an ice-albedo feedback might well justify Campbell's statement. Tomkin's estimate of only 1 °C cooling is much too small, and his assertion that Campbell had overlooked the importance of water vapor is unjustified.

We were sorry to learn that more than half of Tomkin's professional colleagues thought CO_2 rather than H_2O the major greenhouse gas. Meteorologists have become accustomed to the fact that most physicists have little interest in atmospheric processes. Surely not astronomers too?

Reference

1. K. P. Shine, A. Sinha, Nature **354**, 382 (1991).

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Alison Campbell may be mistaken in her assertion that global mean surface temperature would be below