

with their estimated evolutionary or spin-down ages of several hundred million years.

Which evidence is more compelling? The kinetic youth of the millisecond pulsars—now measured via their proper motions—was initially believed to be a selection effect: All the reported millisecond pulsars in the Galactic disk are very near their expected birth sites, inside the molecular-cloud layer. Purely by selection? No counterexample has been published so far (by May 1990), after three years of intense search, and the (large) proper motions of the three above-mentioned pulsars close the argument.

There is, therefore, compelling evidence that the millisecond pulsars are extremely young, that they have lifetimes shorter by an order of magnitude than the nonmillisecond pulsars and, consequently, that they are almost as abundant as the latter. They may turn off young, by being quenched. How were they formed?

It is generally agreed that the fast pulsars are spun up, or "recycled," by mass accretion from a nearby companion. This opinion has so far only been formed on white paper, under the assumption of unhampered accretion. It contrasts with all the known pulsing x-ray sources, in which accretional spin-up is compensated by magnetic spin-down. (The Alfvén radius, which is used in such considerations, may be inapplicable, because the pressure of the confined, diamagnetic disk plasma will adjust to the magnetic pressure.)

In any case, if the millisecond pulsars are young, they cannot be recycled. Their magnetic moments are apparently lower than those of "ordinary pulsars" by a factor of order 10^2 , not necessarily 10^4 . No field decay need have taken place: They may simply be the product of less efficient supernova explosions, in which a weaker field implies a weaker coupling to the ejected shell during the explosions.

There remains, of course, the unexplained overabundance of globular-cluster pulsars. Are they all old? A possibility that I have not seen discussed in the literature is that instead of being spun up or formed from white dwarfs by accretion-induced collapse, the globular-cluster pulsars are reactivated neutron stars, converted by accretion from field-aligned rotators to (mildly) oblique rotators. There would thus be long time intervals of turnoff (by alignment or quenching) interrupted by short epochs of activity.

Another controversial statement is

represented by the narrow emission cone shown in figure 3. If it were as narrow as drawn and stated, we would have many more pulsars than progenitor stars. In fact, Adrian Blaauw's estimate² for a 0.5-kiloparsec neighborhood of the Sun implies that all stars more massive than 14 solar masses would be required as progenitor stars of neutron stars, both pulsars and close binary ones, if the beaming factor were as small as for a circular beam—in conflict with evolutionary calculations, which predict much higher critical masses.

References

1. S. R. Kulkarni, in *Neutron Stars and Their Birth Events*, NATO Adv. Stud. Inst. C, vol. 300, W. Kundt, ed., Kluwer, Dordrecht, The Netherlands (1990), p. 59.
2. A. Blaauw, in *Birth and Evolution of Massive Stars and Stellar Groups*, W. Boland, H. van Woerden, eds., Reidel, Dordrecht, The Netherlands (1985), p. 211.

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BACKER AND KULKARNI REPLY: Wolfgang Kundt's letter appears to consist of one major point, two minor points and a radical suggestion. We will respond in that order.

The major point is Kundt's assertion that millisecond and binary pulsars are young objects, and he quotes an article based on a review lecture¹ to bolster this claim. However, even a casual reading of that article would make it clear that the contrary is being claimed. For example, on page 74 it is asserted that "the ages of the recycled pulsars are inferred to be $\geq 10^8$ y... whereas most field pulsars are believed to be young objects ($< 10^7$ y)." The review article has a whole section, amounting to three pages, devoted to an extensive and critical discussion of the observations that led to this conclusion. The same article explicitly notes that this conclusion is inconsistent with the low $|z|$ heights of recycled pulsars and concludes that this must be a selection effect. Indeed, as the article points out, both pulsar 1855 + 09 and pulsar 1957 + 20, the only millisecond pulsars found in systematic surveys at the time the review was concluded, are at low latitudes simply because the searches were *directed* at low Galactic latitudes. Additionally, the article points out that pulsars 1937 + 214 and 1953 + 29 (the other two pulsars not found by systematic surveys) were found be-

cause "the considerations leading to the discovery... were their close location to the Galactic equator." The article continues, "Indeed, so convinced I am that this is a selection effect that our group has initiated a millisecond pulsar survey at Arecibo towards intermediate latitude." Our groups are engaged in two surveys looking for nearby pulsars at the Arecibo Observatory. We are glad to announce that pulsars away from the Galactic plane, as argued in the review article, have been now found by Alexander Wolszczan² in a survey conducted at high latitudes. We are somewhat surprised that Kundt, the organizer and the editor of the lecture series, failed to read the rest of the review article and confined his attention to one table.

With regard to the first minor point, we are at a loss to understand Kundt's confusion and objection about the use of millisecond and binary pulsars for fundamental tests of physics. The tests we talked about in our PHYSICS TODAY article take advantage of the demonstrated temporal stability of these pulsars, and how they originated is irrelevant.

Second, with regard to the illustration of a pulsar in figure 3, Kundt fails to read the text on page 28, where we explicitly state that pulsar beams increase with the square root of the rotation rate.

Finally, we come to the issue of the origin of pulsars in globular clusters. Kundt suggests a new mechanism to reactivate old pulsars by accretion, and we are faulted for not discussing this hypothesis. Regardless of what our opinion may be of the new mechanism, is it not a bit too much to expect us to include in our article unpublished mechanisms?

References

1. S. R. Kulkarni, in *Neutron Stars and Their Birth Events*, NATO Adv. Studies Inst. C, vol. 300, W. Kundt, ed., Kluwer, Dordrecht, The Netherlands (1990), p. 59.
2. A. Wolszczan, IAU Circ. 5073 (1990).

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Resolving Some Hole-Burning Issues

Dietrich Haarer and Robert Silbey (May 1990, page 58) state that "A. Szabo performed the first optical hole-burning experiment."¹ Although Szabo may have done the first such

experiment in solids, as he claims,¹ my own work on optical spectral hole burning and that which I did with my colleagues predates Szabo's by many years.

Specifically, this work included:

- ▷ the first observation of optical hole-burning effects in a helium-neon laser²
- ▷ the first qualitative interpretation³ and quantitative explanation⁴ in terms of optical hole burning of the single-mode power tuning dip (or "Lamb dip") as the single-frequency standing-wave gas laser is tuned through line center. Observation of this dip was first reported by Ross McFarlane, Willis Lamb and myself and by A. Szoke and Ali Javan.⁵
- ▷ the quantitative evaluation in terms of hole-burning effects⁴ and direct observation in a beating experiment between two single-mode lasers⁶ of the mode pushing (or hole repulsion) effect on the frequency shift in a single-frequency helium-neon laser as it is tuned near line center
- ▷ the direct observation of the spectral hole shape produced by a single-frequency tunable running wave in a separate helium-neon discharge tube⁷
- ▷ the first interpretation of the role of hole burning in mode suppression and frequency stabilization in gas lasers containing saturable absorbers.⁸

A long review article⁹ that I wrote in 1969 summarizes these and other optical hole-burning studies prior to the work by Szabo.

References

1. A. Szabo, *Phys. Rev. B* **11**, 4512 (1975).
2. W. R. Bennett Jr, *Phys. Rev.* **126**, 580 (1962).
3. W. R. Bennett Jr, in *Optical Masers*, O. Heavens, ed., Opt. Soc. Am., Washington, D. C. (1962), p. 24.
4. W. R. Bennett Jr, in *Quantum Electronics III*, vol. II, P. Grivet, N. Bloembergen, eds., Columbia U.P., New York (1963), p. 441.
5. R. A. McFarlane, W. R. Bennett Jr, W. E. Lamb Jr, *Appl. Phys. Lett.* **2**, 189 (1963). A. Szoke, A. Javan, *Phys. Rev. Lett.* **10**, 521 (1963).
6. W. R. Bennett Jr, S. F. Jacobs, J. T. La-Tourette, P. Rabinowitz, *Appl. Phys. Lett.* **5**, 56 (1964).
7. W. R. Bennett Jr, in *Atomic Physics*, V. W. Hughes et al., eds., Plenum, New York (1969), p. 435. See also W. R. Bennett Jr, V. P. Chebotayev, J. W. Knutson Jr, *IEEE J. Quantum Electron.* **4**, 384 (1968).
8. W. R. Bennett Jr, *Comments At. Mol. Phys.* **2**, 10 (1970); **3**, 63 (1972).
9. W. R. Bennett Jr, in *Brandeis University Summer Institute in Theoretical Physics*, 1969: *Atomic Physics and Mo-*

lecular Physics, M. Chretien, E. Lipworth, eds., Gordon and Breach, New York (1973), p. 5; later published as *The Physics of Gas Lasers*, Gordon and Breach, New York (1979).

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HAARER AND SILBEY REPLY: In our article on hole burning in glasses, we stated that A. Szabo had performed the first hole-burning experiments. William R. Bennett Jr states that his experiments on hole burning in gas-phase systems predate those of Szabo. He is correct. Szabo performed the first hole-burning experiments in a condensed-phase system—the subject of our article.

Note that there is a great difference between Bennett's spectral hole burning in the velocity distribution in gas-phase molecules and spectral hole burning in the inhomogeneous distribution of sites in a condensed-phase system. In Bennett's experiments, gas-phase hole-burning spectroscopy created a center dip at zero velocity only, while hole burning in condensed phases creates a dip at any frequency and so samples any part of the inhomogeneous distribution.

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Kramers's 'Influence' Was a Coincidence

The exchange between Max Dresden and D. ter Haar in the October 1989 issue (page 15) concerning Hendrik A. Kramers's continued interest in the Ising problem during the late 1940s brought back memories of my Leiden years. I became Kramers's assistant in 1946 and had the privilege of witnessing him at work. It is from this vantage point that I am puzzled when ter Haar writes, with reference to R. M. F. Houtappel's thesis, "On reading this paper one comes across yet another example of Kramers's facility in producing elegant mathematical methods." This is amplified by Dresden: "The thesis by R. M. F. Houtappel . . . was clearly strongly influenced by Kramers. The elegant mathematics, the ingenious way in which group theory is avoided in a clever adaptation of Bruria Kaufman's method—these are as characteristic of Kramers as his signature."

The inference is based on the assumption that in Leiden a PhD candi-

date received a thesis topic and the method for approaching it from his adviser. I find it inconceivable that this scenario applied to Houtappel. He was the most fiercely independent and original of our graduate students, and as his best friend I witnessed from close by the realization of his thesis. Just in case I missed something, I contacted him in Leiden, where he enjoys his retirement. In his reply, he wrote that it was Ralph Kronig in Delft who, early in 1944, got him interested in the so-called chessboard problem, in connection with Kramers and Gregory Wannier's paper of 1941. (Kronig had tried his hand at this already in the late 1930s, without much result.) During the "hongerwinter" of 1944–45, Houtappel worked on the problem and studied, against Kronig's wishes, group theory and hypercomplex systems for this purpose. When, at the end of the war, Lars Onsager's paper became known, Houtappel had little difficulty understanding it. He gave a few lectures on the subject.

His letter to me continues: "Early in January 1950 Kramers asked me to select a topic to work on. I proposed then to search for a solution of the triangular and hexagonal variants of the chessboard problem. Kramers replied in no uncertain terms that this was much too difficult and that a number of 'heavies' had tried this in vain. When I insisted that he would let me try during the next weeks, when he was to visit Bohr in Denmark, he agreed: 'That is a good idea. So you will see for yourself that it is impossible and then we can look for a suitable topic.'"

Upon Kramers's return in the first week of February, I was able to report to him that Houtappel was succeeding, using Kaufman's formalism. Two weeks later he explained his solution to Kramers and got the go-ahead for his PhD. After that, Kramers gave tremendous support by suggesting additional calculations and helping to formulate the conclusions. But the basics, including the idea of avoiding group theory and the method for doing so, are all Houtappel's. Any similarity to Kramers's "signature" must be a coincidence.

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Developing Countries' Scientific Woes

I was delighted by Mano Singham's Opinion column "Science for Developing Countries" (August 1989, page 61). I work in a field, hydrology, that