

High Power—High Frequency Pulsed or C.W. Oscillator



PG-656-C, Mod VI

Power Output C.W. 100 watts
Frequency Range 0.500 — 100 MHZ

Continuously Tunable
Harmonic Content (Mostly Odd) <13%

Pulse Performance—Partial list

Peak Power >300 watts
Frequency Range Standard .200 — 177 MHZ
PRF — Max (Ext. gate) 300 KHZ
Duty Cycle 30%
Frequency Range Extended 12 KHZ
Pulse Length 1–30 usec

Additional Features Available

Use as Gated Amplifier Separate Pulse Width and Amplitude Controls



Note: PG-655-C, Mod V has the above features except for C.W. Power Limit of 20 watts



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Input—Separately tunable to accommodate 10–100 pfd effective source.

Band Width 7 MHZ
R.F. output (93 ohms) 1 v p to p
Noise figure 3–4 db
Gain Max 80 db
Gain Range 40 db
Recovery Time 2 usec
Weight 14 lbs
Chassis—Rack Mountable 19 × 5¼ × 8½

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letters

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group and SLAC's group A have collaborated in a series of electron scattering experiments, including the deep inelastic experiments.

Richard E. Taylor
SLAC
Stanford University

Geophysics disagreement

Having read the recent article on "Opportunities in Geophysics" (February, page 23) by my senior University of Michigan colleague H. Richard Crane, I find myself compelled to state my disagreement with both the specific comments contained therein on the optimum education of a physical oceanographer, and with the general spirit of complacency expressed most explicitly in the concluding paragraphs of his article.

I am moved to write by the vivid impressions left with me by the succession of physics graduate students who have come to inquire of me as to their chances of obtaining a research position in oceanography upon completing their PhD in physics. I saw this same concern mirrored in the questions put to me by a group of physics seniors when I met with them earlier this winter for an evening seminar.

The section of Crane's article dealing with oceanography begins with the statement: "Oceanography is best entered by someone who has completed an MS or even a PhD in one of the basic disciplines such as physics." This statement should be inscribed in the same ledger as the following: "Physics is best entered by someone who has completed an MA or even a PhD in applied mathematics." I suggest that both these statements may reflect the vested interests of certain departments, but neither of them is responsive to the legitimate needs of the majority of students. Our students require (and, what is more, expect) to be able to embark on a coordinated (may I say efficient?) curriculum that will, if they are diligent, take them from freshman matriculation to the PhD in something like seven or eight years. I believe that we should devote our efforts to turning out effective, contributing, scientists in this time span. If there is no apparent effort on our part to do this then we will with justification be accused of being unresponsive academic mandarins. Unless a student wishes a protracted formal education I would strongly recommend that he not defer his entry into oceanography beyond the beginning of his graduate years.

The deficiencies that exist in today's undergraduate physics curriculum in preparing a student for graduate study

in physical oceanography are essentially those that Crane cites Landsberg as identifying in discussing preparation for graduate study of atmospheric physics. I wish to suggest to Crane and others who recognize that "recultivation may require changes in the curriculum and research experience, perhaps reaching down into the physics major program" that they need not look far in seeking informed opinion as to what these changes might entail.

Lest the reader come to suspect that I harbor resentments against the physics community, let me hasten to say that I hold the science of physics in high esteem. Indeed, although I received my PhD in oceanography and have for the past three years taught courses in that field, I was schooled through my MS in physics, and spent three fruitful years as an assistant professor of physics. I chose to forsake the teaching of physics for the teaching of oceanography because I felt that I could more closely relate the insights gained in my research to my classroom presentations. This has indeed proved to be the case.

Edward C. Monahan
University of Michigan
Ann Arbor

The author comments: As I stated clearly at the beginning of the article, my only sources of information and opinion were several of the elder statesman in the geophysics area. The only exception was that one of the respondents included a quote from a young faculty member. My job was reporting and editing, except for the concluding remark that was termed "complacent." That the views on the ways of entering oceanography and the chances of employment expressed in the foregoing letter differ from those I got from my sources is par for the course and not of my doing; however I can, and probably should, be faulted for not directing my inquiries to a more diverse sample. This is a point that might well be observed by the next author who makes a similar survey of a field. It is to be hoped that on balance such articles on neighboring fields have a plus effect in broadening the horizon for physics students and new PhD's. *physics today* has run several articles in this vein, and I am trying to assist the editor in his effort to bring others into being. They may be better for Edward Monahan's criticism.

H. Richard Crane
University of Michigan

More chemical-bond debate

The recent exchange of letters between Linus Pauling and J. C. Phillips (February, page 9) was interesting in that it demonstrated three points: (1) articles

in physics today should be better reviewed; (2) your editorial "A New Breed of PhD" is thereby refuted, since Phillips has demonstrated the inability of the average PhD to apply principles in a field with which he is unfamiliar, and (3) PhD's need a more rounded education than they are getting. The persistent ignoring of the concept of radius ratio by Phillips, although it is an adequate explanation of the types of structural ordering that he considered a mystery (his article, February, 1970, page 23) is one such demonstration. A second is his complete lack of familiarity with chemical valence, not only the examples cited by Pauling, but also the glaring error in Phillips's reply ("Silver is commonly divalent...").

William R. Cook Jr
Cleveland Heights, Ohio

The author comments: I should explain, in reply to William Cook, that I neglected the concept of radius ratio in discussing the covalent-ionic transition because its relevance was disproved many decades ago. To enlarge the discussion, consider the following quotes (comments in brackets added by me):

"The atomic [billiard-ball] concept [of crystal structure] so popular in ancient Athens still enjoys an intuitive appeal to many physicists in situations where they ought to know better."¹

"Broad indications on the relative stability of different crystal structures for series of compounds can be obtained by a very crude model, which represents the ions as rigid spheres of a given radii . . . The predictions of the rigid-ion model have, however, little quantitative value . . . The differences between the theoretical values of the cohesive energy and these experimental values, are consistently large and positive . . . The discrepancy points to the presence of a sizable covalent contribution to the binding. It is noteworthy that the discrepancy is very large for the salts which crystallize in the zincblende structure."²

"It is known that silver can form two covalent bonds, as, for example, with carbon in the $[NC-Ag-CN]^-$ complex ion, each having nearly as much covalent character as for a bond to univalent silver . . .; and resonance of two bonds among the available positions in the silver halide crystals would double the calculated amount of covalent character of the crystal bonds, giving . . . 46% for AgI."³

Finally, what about the notion that one can explain either covalent-ionic or covalent-metallic transitions by assigning radii to atoms, one for each kind of atom in each kind of structure? The answer to this was given long ago:

"The Parts of all homogeneous hard bodies which fully touch one another,

stick together very strongly. And for explaining how this may be, some have invented hooked Atoms, which is begging the Question."⁴

I hope this answers Cook's questions. From this discussion, one may judge that old ideas die hard, especially when the new ones contain a substantial portion of originality.

References

1. J. C. Phillips, *Rev. Mod. Phys.* **42**, 355 (1970)
2. M. P. Tosi, *Advances in Solid State Physics* **16**, 1 (1964). (This was ref. 6 in the original article.)
3. L. Pauling, *The Nature of the Chemical Bond*, Cornell U. P., Ithaca (1940), page 73.
4. I. Newton, *Optics* (1704), quoted by A. N. Holden in *The Nature of Solids*, Columbia U. P., New York (1965).

J. C. Phillips
Bell Telephone Laboratories
Murray Hill, N. J.

Objectivity defined

One aspect of Jean Loiseau's criticism of relativity and relativists (November 1970, page 13) is, I think, wrongly interpreted by Mendel Sachs in his reply to the letter. When discussing measurements of the velocity of light, Loiseau uses the term "objectivity" to refer to the proper relation between theory and observation. This is a legitimate use of the term as it is applied to persons (such as relativists), whereas Sachs's definition is appropriate to the term as it applies to theories (such as relativity).

Observation in science is necessarily influenced to a certain extent by theory, insofar as theory suggests what experiments are to be done and how they are to be interpreted. However, the objectivity of science is violated when this influence is either too weak, so that possible experiments that could confirm or deny the theory are not performed, or when this influence is too strong, so that certain experimental results are ignored or misinterpreted when they conflict with the theory.

Loiseau's specific criticisms of relativity in this regard may be debated, but he does not misuse the term "objectivity" as Sachs implies.

David R. Lyzenga
Ann Arbor, Michigan

Corrections

April, page 71—Ronald Rau was erroneously identified as formerly of Brookhaven National Laboratories. He is still Associate Director of High Energy Physics at this laboratory.

April, page 57—The publisher of the new book "Solid State Spectroscopy Supplement to Optics and Spectroscopy" is the Optical Society of America, not Vance Weaver. □

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