PhD physicist do such work would be of value either to the physicist or to society? He mentions the cost that society has incurred in educating a man to the level of a PhD. That man has also incurred some costs in time, effort and lost income. Both he and society are entitled to the return that can be had only if he can do the work he was trained for.

I am particularly concerned by the implication that the editorial is speaking for society to young PhDs. Society will render its own judgment on the need for physicists; PHYSICS TODAY can not presume to speak for it. What it can do is offer something more constructive to those of us about to enter the profession than the chilling thought that unemployment is the price of freedom. Other professions find it not only possible but desirable for their publications to speak for them as well as to them. Like it or not, others will regard PHYSICS TODAY as a voice of the physics profession in any case. If we continue to present ourselves as martyrs to our work, totally unconcerned about our position, reputation or security, society will treat us that way, and we will have only ourselves to blame.

ROBERT LEVINE
University of Pennsylvania

Who accepts responsibility?

I am aghast that R. Hobart Ellis Jr could take such an irresponsible position in the June editorial, "Who Finds the Job?" His primary attitude seems to be (correct me if I err in my interpretation) that competent physicists who cannot find jobs within physics should leave and undertake other vocations (he suggests specifically high-school teaching, the Peace Corps and advising the poor and underprivileged). Furthermore he suggests that the universities and the American Institute of Physics have no responsibility for the physicist after graduation beyond providing employment data and job placement services.

My position, as a graduate student, is this: If colleges and universities throughout this country are preparing thousands of physicists only to face careers in some other field, they are propagating a cruel joke. Do not misunderstand me on this point. I am not knocking the Peace Corps or high-school teachers or any other worthy occupation Ellis would suggest. I am merely stating that I have not *chosen* any of these avenues as a career and I do not care to be *forced* to choose them. I am not asking for a handout when I graduate. I simply want to find a productive job within physics.

I can not offer any concrete proposals to alleviate the present situation except to suggest that Ellis's attitude of passive acceptance is precisely what is *not* needed. I sincerely hope that Ellis's position does not represent the official attitude of AIP. After encouraging students to major in and study physics, I feel that it would be inherently dishonest for AIP to turn around and disown them after they graduate and can not find a job. I certainly could not support a professional society that is not interested in the welfare of its members.

WILLIAM M. GREENBERG
San Diego State College

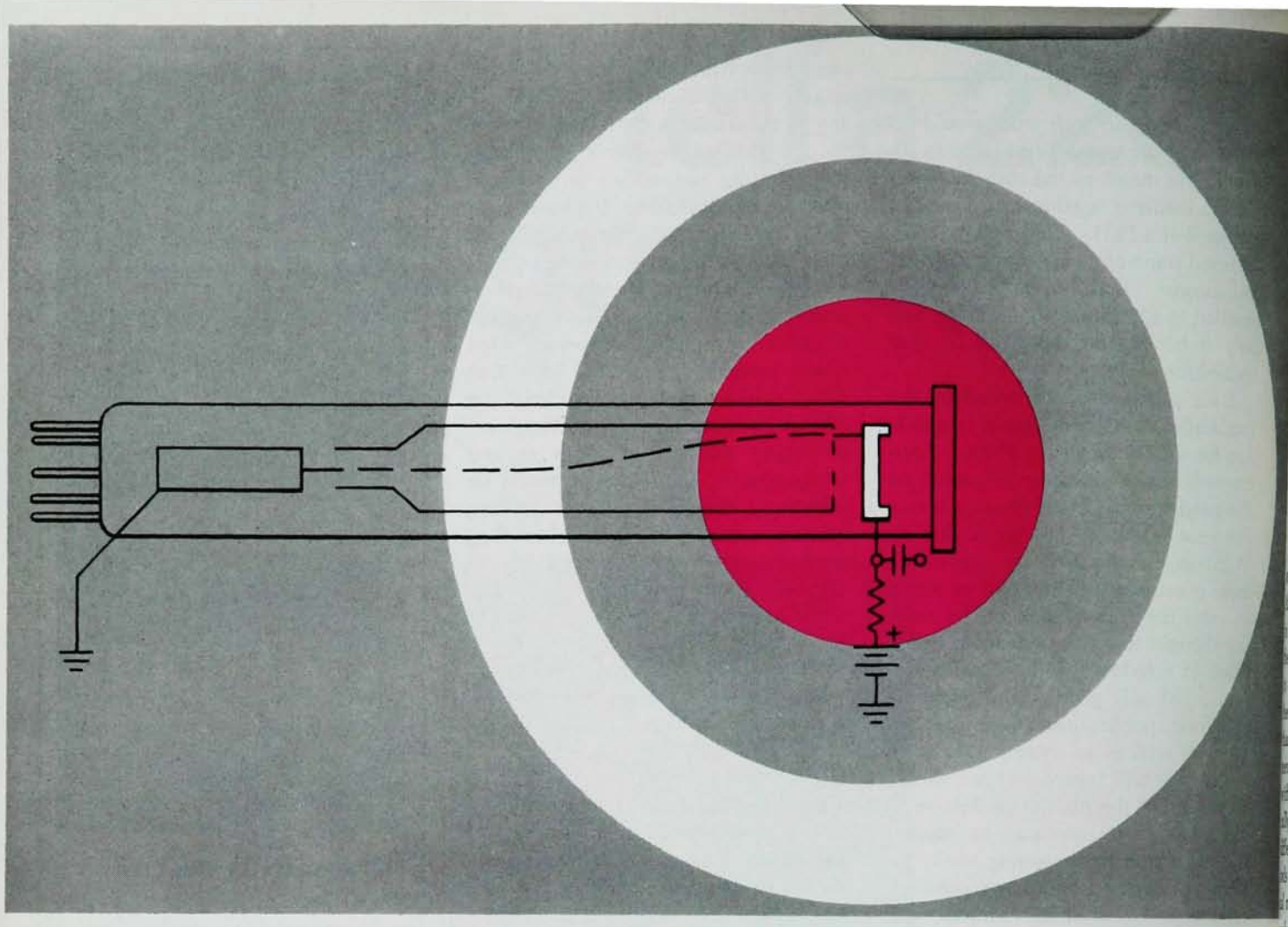
How far to quasar?

I am sure that Fred Hoyle and Geoffrey Burbidge would not wish to take the entire discredit of being the *only* devil's advocates in the matter of quasar distances as suggested in Bernard Burke's excellent article on radio telescopes (PHYSICS TODAY, July, page 54). A good part of the opprobrium for suggesting that quasars may be at less than cosmological distance must rest on me (J. Terrell, *Science* **145**, 918, 1964; **154**, 1281, 1966; **156**, 265, 1967; *Astrophys. J.* **147**, 827, 1967; *Science* **159**, 291, 1968; *Phys. Rev. Lett.* **21**, 637, 1968).

The question of quasar distance is still open as there are a number of puzzling observations apparently inconsistent with cosmological distance, such as anisotropy of quasar distribution, apparent cutoff of red shifts at $z \approx 2.4$ ($z =$ change of wavelength divided by laboratory wavelength) and lack of association with distant galaxies. Origin of the observed quasars in a gravitational collapse at the center of our galaxy would appear to be more consistent with these and other observations. JAMES TERRELL
Los Alamos Scientific Laboratory

Subjective relativity

The February article in PHYSICS TODAY, "Space, Time and Elementary Interactions in Relativity" by Mendel Sachs, is certainly a most valuable report on modern developments in rela-



A solid state target in a camera tube

Bell System PICTUREPHONE® service will need small, reliable TV camera tubes for use in offices and homes, where lighting ranges from dim to very bright. Conventional vidicon tubes are unsuitable, so Bell Labs developed a new kind.

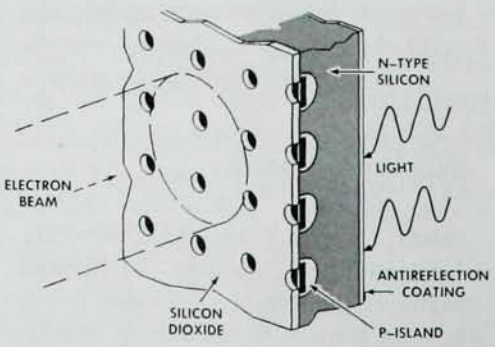
The heart of the new tube is a light-sensitive target containing nearly 700,000 silicon photodiodes in an area less than a half inch square. They are made by diffusing boron, a p-type impurity, through a silicon-dioxide mask into n-type silicon.

A scanning electron beam charges the p material negatively, reverse-biasing the diodes. Holes, created by incident light, are collected by the electric field at the p-n junctions, and individual diodes discharge by an amount proportional to the local light intensity. Recharging of the diodes by the scanning electron beam produces a varying current . . . the output signal.

Among the tube's advantages:

Its target tolerates high-temperature baking . . . a processing step to improve reliability. Conventional vidicon targets cannot stand this.

Silicon's high thermal conductivity and chemical stability help make the new tube immune to "burn-in" (degradation of performance from continuous exposure to a fixed image, very bright light, or a strong scanning electron beam).



The time between a change in target illumination and a like change in output is much shorter with the new target. This improves response to fast-changing scenes.

The light-sensitive face of the new target is optically flat. So, a multilayer antireflection coating can be applied for better sensitivity and minimum received-picture "halo".

Silicon targets have relatively uniform response through the visible and near-infrared—from 4,000 to 9,000 A. Quantum efficiency (electrons per photon) exceeds 0.5. So, these targets have at least 10 times the sensitivity of a standard vidicon camera tube in incandescent light.

This new camera tube is in the latest model PICTUREPHONE set, now undergoing field trials.

From the Research and Development Unit of the Bell System—

Bell Lab

tivity. With reluctance, therefore, I object to a certain passage, inconspicuous within the article itself, only because it has been given prominence by its boldface quotation on page 53. The prominence makes it seem that it contains the very essence of relativity:

> ". . . the theory of relativity introduced a revolutionary concept into physics by relegating space and time to the subjective role of the elements of a language that one observer or another may use to describe the natural laws."

"Subjective," "language" and "observer" as "revolutionary concepts"? After having wrought so much havoc in the interpretation of quantum theory, why should they be transferred to relativity, which is an objective theory? It is even more objective than quantum theory—if this statement is possible. Relativity begins with the positions of hands of identically constructed clocks mounted in different reference systems and with rods at rest not coinciding with identical rods in motion. The rods are described quantitatively by a metric that rests on the successful (but far from expendable) assumption or convention that light has the same vacuum velocity in all inertial reference systems. (This statement is, of course, in contrast with Sachs's assertion on the speed of light in the box on page 52.) The data $xyzt$ are coördinated with $x'y'z't'$ by the Lorentz transformation.

Of course one can illustrate the process of realizing those objective data as "readings" by human "observers" riding on trains who communicate in the "language" of the Lorentz transformation if this is meant *not* as a "revolutionary concept" but figuratively. Sachs himself deflates this symbolic poetry by the fitting remark (page 54) that "the observer could be identified with a star, a man or a proton" and that he "has no anthropomorphic denotation." Why then introduce an anthropomorphic terminology at all? And why present just this linguistic trap in boldface as the quintessence of relativity thereby intriguing superficial readers, deluding serious students and elating popular writers who will pounce on it?

This subjectivism is suggested by a misleading though much beloved terminology in quantum theory. There we are told also of a refined (Copenhagen) *language* to accommodate lack of consistent explanation of the *observer* having *subjective* pictures translated by complementarity, of nonexistence of *qp* pairs, of *subjective* wave packets of expectation and so forth. All this terminology renders the conceptual content of quantum physics indeed very hard to understand as Sachs says on page 51. At least it is hard for those who are not fed enough Danish pastry baked 40 years ago and based on an erroneous interpretation (duality) of fundamental quantum experiments (matter diffraction). Today the way out of the subjective quantum jungle has been found in a self-consistent unitary theory based on a few postulates of symmetry and invariance. The postulates answer the question, "Why is the world a quantum world?" by deriving their formalism from a probabilistic but nonquantal basis without subjective ingredients. If this procedure is possible in the quantum realm, why then becloud the conceptual content of relativity with an equally misguided and unnecessary subjectivism in which data of clocks and rods become words in the language of the observer presented to the reader as the essence of a "revolutionary concept in physics"?

ALFRED LANDÉ
Ohio State University

SACHS REPLIES: I certainly agree with Landé's misgivings about the Copenhagen interpretation of quantum mechanics. In fact I indicated these objections in my PHYSICS TODAY article (in contrast with some of the comments in Landé's letter that imply an agreement with the Copenhagen view). I say in the article that a full exploitation of relativity theory *requires* a philosophic stand (realism) that is contrary to the Copenhagen interpretation (positivism). This contention is contained in the section "Elementarity of Interaction" on page 54. An underlying objective reality here implies particulars (which are testable by experiment), but in contrast with atomistic theories this objective reality has to do with a single closed system, not with the sum of distinguishable parts. (It makes no difference whether the parts are described in classical or quantum mechanics, with or without hidden variables, determinism, discreteness and such features.) The *elementary* aspect of this single closed system is the

fundamental relation that I call "elementary interaction." With this view, only in certain asymptotic limits does the elementary interaction approach the *appearance* of (almost) disconnected parts. These can include a part that might be claiming to make a measurement on the rest of the apparently disconnected system. But it is important that *in principle* this fundamental relation can not be entirely broken within this approach. For this reason I call the relation "elementary."

The implication about space and time that follows is in fact the boldface quotation on page 53. (I did not select the boldface quotations nor see them in proof, but I do feel that this one as well as the others is an important point regarding the argument in the article and that they were wisely chosen.) If one takes this quotation out of context (that is, without fitting it into the logical structure of the rest of the article) and if one continues to insist (in contrast with the view in the article) that the world is made of independent, distinguishable parts, the subjective view of space–time would imply a philosophic stand of positivism in physics. Such a subjective view would be in terms of the trajectories of the parts. But I am not doing this. I am contending that the full logical exploitation of relativity theory implies the *replacement* of space–time (and atomism) with mutual relation (and continuity) as the underlying reality.

In this case the whole is not the sum of its parts; it is one! And probability plays no intrinsic role here; it is only a tool that is useful in certain approximations to the general theory. These are situations in which there is insufficient information about the underlying closed system to complete its description. But the completely determined system does exist. It is described explicitly in the mathematical structure in terms of coupled nonlinear field equations—with all fields mapped in one space–time rather than n space–times for an n-particle system of an atomistic theory. And the complete description is independent of any macroscopic measurement.

Thus this is a deterministic description, not in the Newtonian particle view of classical mechanics but rather in the view that is based on the continuous-field concept originally introduced by Michael Faraday. The space and time parameters are here only a convenient language that is used to facilitate a mapping of the (nonlinear, coupled) field equations. This view is quite contrary to the Copenhagen view as well as to Landé's (and that of David Bohm, Louis de Broglie and others who have been considering deterministic interpretations of quantum mechanics). The reason is that the field view that I have been describing is not quantum mechanics. It contains the formalism of quantum mechanics only in the nonrelativistic limit of small enough momentum transfer within a closed system. Then the nonlinear coupled-field formalism of this theory *approaches* the uncoupled formalism that the Schrödinger or Heisenberg formalism uses to describe "particles." It has a linear eigenvalue structure. But here the Schrödinger–Heisenberg formalism is not correct in principle even though it gives very good mathematical approximation for the matter field equations in the domain of low-energy physics.

There are also a few other comments in the Landé letter that I can not agree with:

- I do not agree that "relativity begins with the positions of hands of clocks and . . . " It appears to me that relativity theory rather begins with a fundamental principle—the principle of relativity. The implication of this axiomatic starting point is assignment of a *passive* rather than an active role to the space and time parameters in the expression of the laws of nature.
- I do not agree that the actual data are $xyzt$. Rather the data are numbers that are associated with conserved quantities of transferred energy, momentum, angular momentum, etc., among the interacting parts of a system. After these reactions are recorded (by a star, a man or a proton!) they are correlated with a language (which a man might call $xyzt$ or, more accurately, some continuous function of $xyzt$) and then checked with the equations that relate to the particular observed phenomena. The phenomena are expressed in terms of the functions of $xyzt$ (the continuous field variables in a field theory) or in terms of $x'y'z't'$ in other frames of reference. The test of relativity theory is whether the forms of deduced (or postulated) laws of nature are the same to all relatively moving frames of reference.
- I did not say in my article that the universality of the vacuum velocity of light is expendable. I did argue that

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the assertion (which I maintain is intrinsic in relativity theory) is a *logical implication* of the underlying axiom of the theory (the principle of relativity) and therefore need not be postulated as a separate axiom. (If one chose to use a language other than *xyzt*, it would then follow that a new universal constant would appear that would correspond to the *c* of the *xyzt* language.)

• Toward the end of his letter Landé implies that the formalism of quantum mechanics is eminently successful and must only be rederived from "a probabilistic but nonquantal basis without subjective ingredients." I agree that the general form of the basic formulation must not have subjective ingredients. But I do not agree that the present formulation (which "works" in the nonrelativistic domain) has yet proven itself correct in principle. If it were so, it would have to predict correctly the behavior of matter in the microscopic domain in *all* experimental conditions. It is well known that there is no mathematically consistent formulation of quantum theory of energy-momentum transfer in the relativistic domain. Despite numerical success of the renormalization program (for a limited number of effects) nobody has ever shown that any relativistic formulation yields a mathematically consistent description with (at least) solutions—let alone solutions that correspond with any observation.

Thus if one is to insist on incorporation of relativity theory to explain high-energy phenomena, it appears to me that quantum mechanics can not yet claim to be anything more than a good mathematical approximation (for some general relativistic theory) in the low-energy limit. No one can claim that the general theory has any of the features of quantum mechanics except in this asymptotic limit. To assume that the general theory does have the formal features of quantum mechanics (for example, linear superposition, homogeneous eigenvalue equations) appears to me at this stage to be purely speculative. I have argued in my own writing that if one fully exploited relativity theory, the formal structure of quantum mechanics would not emerge in fact in the general theory! Nor could atomism remain as a basic conceptual notion.

MENDEL SACHS
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