on which they seek to draw. In several dozen speeches to technical professional groups, from national society gatherings on down, I found that less than 10% of my audience had even an approximate feel for the gross national product, the Federal budget, this year's deficit, the debt, the foreign trade deficit and so on.

Next we could make our processes much more democratic, easily and at no cost. I propose what I proposed to former National Academy of Sciences President Philip Handler in the 1970s—that every NAS committee be required to incorporate at least one radical dissenter: a Karl Morgan on radiation committees, a Henry Kendall on nuclear power committees, a Frank von Hippel on energy committees and so on. If we cannot deal with the views of those very responsible scientists in committee, how will we deal with them in the real world of society? And how will we deal with those elements of the public who are not believers in the same "religion"? (And there they are gaining the upper hand.)

Your roundtable group lacked someone—and there are dozens—who would have articulated the positions that science in total, far from hurting, is gloriously overfunded by the public purse compared with many other investment goals; that the only studies made[1] have shown that Nobel Prize-winning science is negatively correlated with growth of gross domestic product; that, for example, particle physics—a noble calling for any individual or private foundation—has been overfunded by an order of magnitude from public funds. Let us apply the Weinberg criteria.[2] As Alvin Weinberg put it: "The criteria can be divided into two kinds: internal and external.

"... Internal: 1) Is the field ready for exploitation? 2) Are the scientists in the field really competent?

"... Three external criteria can be recognized: technological merit, scientific merit and social merit.

"... Relevance to neighboring fields of science is therefore a valid measure of a field of basic science."

Can anyone challenge my assertion that had particle physics been funded at the lower level, not 1 citizen in 1000 would have noticed, nor would any neighboring field of physics have missed it?

I respectfully propose that in the future this kind of helpful editorial exercise embrace a spectrum of viewpoints to approximate intellectually the *national* spectrum, and not just the circle within which there is so much agreement to start with.

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GLORIA B. LUBKIN AND IRWIN GOODWIN OF *PHYSICS TODAY* REPLY: Rustum Roy makes an important and instructive point—though he is not right in every detail. At least one of the seats at our "science under stress" roundtable was occupied by an articulate critic of unrestricted big science: Erich Bloch. Nor is Congressman George E. Brown Jr an easy mark for scientific entitlements and megabuck projects. He represents a largely nonscientific constituency with an agenda that contains some, but not all, of the needs and wants of the rest of the panelists.

Contrary to Roy's observation, Brown was not alone in arguing that science is only one aspect of our society, which we know to be under stress. Daniel Kleppner said this right at the start, and Bloch expressed a similar thought a short while later. As if in anticipation of Roy's criticism, Alvin Trivelpiece related an anecdote from one of his experiences before an appropriations committee of Congress in the mid-1980s that makes the exact impression Roy would have wanted someone outside the physics community to have created at the roundtable. Trivelpiece recollected that a prominent member of Congress cautioned him about seeking more funds for physics exotica, no matter how priceless the opportunity, in dire fiscal times.

Bardeen, Shockley and Transistor Firsts

It was a pleasure to read your April 1992 special issue on that great physicist and gentleman John Bardeen. His contributions are outstanding enough that it is not necessary to overstate them at the expense of William Shockley to ensure him a major place in history. Nick Holonyak Jr, in his article on page 36, quotes a passage from the preface to Shockley's book *Electrons and Holes in Semiconductors* to show that he himself attributed the invention of the transistor to Bardeen and Walter Brattain. That statement, however, is an example of undue modesty on Shockley's part—a vice that he appar-

continued on page 117

Cryo

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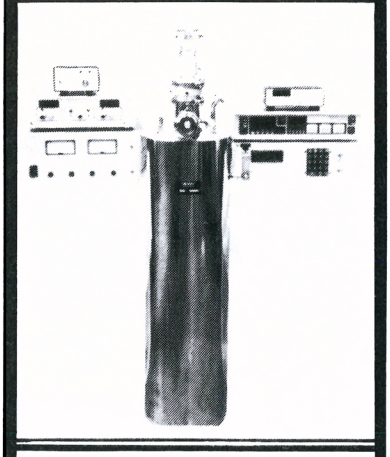

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continued from page 15

ently did not often display.

I got into the transistor business a decade after the events described by Holonyak and therefore do not have the benefits of some of his personal insights. However, in reading the literature without reference to any personal knowledge of the authors, it seemed to me that the record was made crystal clear by two publications on the transistor in 1949: the first by Bardeen and Brattain in April,[1] the second by Shockley in July.[2] They are as different as night and day.

In the first paper, all is still murky. It states that "a complete quantitative theory is still not available." In fact, the theory given does not even qualitatively describe the essential transistor effects. For example, Bardeen and Brattain assign a significant role to electric field rather than diffusion in transport across the base, attribute current flow in the collector to electrons, and fail to dismiss surface conduction as a diversion and not an essential part of the real thing. As Holonyak emphasizes, they did recognize the importance of hole injection from the emitter, but they evidently did not understand, and certainly did not explain, its real role in transistor action.

In Shockley's paper, daylight streams in. He sees clearly the essential features of the original discovery of the transistor effect by Bardeen and Brattain, and he introduces the necessary simplifying assumptions for a satisfactory theory that leads to notions of how a bipolar transistor should really be implemented. These ideas have stood the test of time, and present bipolar-transistor theory and structure stem from that paper rather than the first. As if that were not enough, Shockley went on to provide us with a basic unipolar-transistor theory,[3] again as sole author. It is not overstating things to say that most if not all of modern transistor theory has roots in those two seminal papers.

Why Shockley's ideas were not shared with or by his colleagues, and they instead put into the record a publication that showed that they just didn't have the insights he brought to bear on the problem, is perhaps best understood in the context of Conyers Herring's remarks in another article in your special issue (page 26). These, together with accounts of Shockley's dealings with his colleagues in his transistor company, and his grossly "politically incorrect" social theories, suggest that Shockley was not exactly interested in winning popularity contests. He may or may not have been lovable to those around him, but it is clear that he showed even an outstanding physicist like John Bardeen a clean pair of intellectual heels on the transistor project, and that we owe him a great debt for the brilliance of the insights he gave us.

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2. W. Shockley, Bell Syst. Tech. J. **28**, 435 (1949).
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4/92

HOLONYAK REPLIES: My article on John Bardeen is not concerned with William Shockley or whether junction transistors are better understood than point-contact transistors. The article, I think, makes clear the real issue: How and when did the idea of carrier injection with a current, and as a consequence the transistor, occur? In my 40-year association with Bardeen, I had ample opportunity to learn the answer. Nevertheless, I interrupted Bardeen in his June 1990 interview with NHK, the Japanese television company, when he stated that it was known in 1947 that increasing the temperature of or shining light on a semiconductor increased the electron–hole population. I asked specifically if anyone knew at the time that this could be done with a current. He shook his head and told us that the Bardeen and Brattain experiments revealed this on 16 December 1947, and a demonstration of the effect to the Bell Labs "brass" occurred on 23 December 1947, which is taken as the official date for the "birth" of the transistor. The point is, a new principle had been established for an amplifying device. The rest follows: the semiconductor electronics around us.

Is it necessary to say more? Is it necessary to mention that the point-contact device, because of how it functioned, gave rise to the name "transistor"? Is it necessary to repeat that Bardeen and Brattain recognized that a forward-biased "emitter" (John's terminology), a hole injection current into n-type germanium, gave rise to a current change in a reverse-biased "collector"? Who would have believed that the Ge band structure, then unknown, and carrier lifetime would have permitted carrier injection and collection, even if the idea existed? Accordingly, is it not correct that the point-contact device, whether complicated or not, or even still mysterious in its detailed behavior, is a bipolar transistor—in fact, the original bipolar transistor?

Perhaps I should quote directly from Bardeen and Brattain's US patent 2 524 035, filed on 17 June 1948 and granted on 3 October 1950: "When operated as an amplifier, the emitter is normally biased in the direction of easy current flow with respect to the body of the semiconductor block. The nature of the emitter electrode and of that portion of the semiconductor which is in the immediate neighborhood of the electrode contact is such that a substantial fraction of the current from this electrode is carried by charges whose signs are opposite to the signs of the mobile charges normally in excess in the body of the semiconductor. The collector is biased in the reverse, or high resistance direction relative to the body of the semiconductor."

No matter what we think of the junction transistor, there is no way we can pretend it had an origin apart from or independent of the point-contact transistor, which came first, revealed the phenomenon of minority-carrier injection (which Bardeen identified) and, as I already said, was the original bipolar transistor. Whether we like it or not, the point-contact transistor was the prototype for all succeeding bipolar (injection) devices.

There was no predecessor to Bardeen on the transistor, which is not true of others. All that Bernard T. Murphy's letter does is to make it apparent why I wrote about John Bardeen in the context of the point-contact transistor.

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6/92

What Accelerates Auroral Particles?

In their appropriately enthusiastic account of several very welcome new initiatives in NASA's Small Explorer Program (December 1991, page 44), Daniel N. Baker, Gordon Chin and Robert F. Pfaff Jr include as their figure 5 a drawing (adapted from reference 1) depicting in one plane a set of electrostatic equipotentials apparently hovering above the Earth's auroral zone. The lines, representing several different levels of potential, seem, inconsistently, to be joined together at one end and enigmatically left open at the other. While it is true that there is still a school of thought that holds that the electrons accelerated toward the Earth's auroral regions gain their energy from a poten-