# COPERNICUS, PTOLEMY AND PARTICLE THEORY

The following text pretends to be written by a second-century Alexandrian astronomer. But it is not:

"The standard model of *planetary motions—the epicycle theory*—has enjoyed an enormous amount of success. Indeed, it appears to be consistent with all established *astronomical observations*. This being the case, the first question one should ask is why one should even be looking for something better. Most criticisms of the standard model are based on the fact that it requires a number of arbitrary choices and fine-tuning adjustments of parameters. These features do not prove that it is wrong or even incomplete. However, given the history of successes in *mathematical astronomy*, it is natural to seek a deeper underlying theory that can account for many of the arbitrary choices of parameters. These include *the choice of equans, the number of epicycle generations*, and the specific values of various parameters."

In fact, this is the second paragraph of a recent essay[1] by a particle physicist, in which I substituted the italicized passages for "electroweak and strong forces," "particle physics experiments," "elementary-particle physics" and "gauge groups and representations, the number of families of quarks and leptons, the origins of the Higgs symmetry-breaking mechanism," respectively.

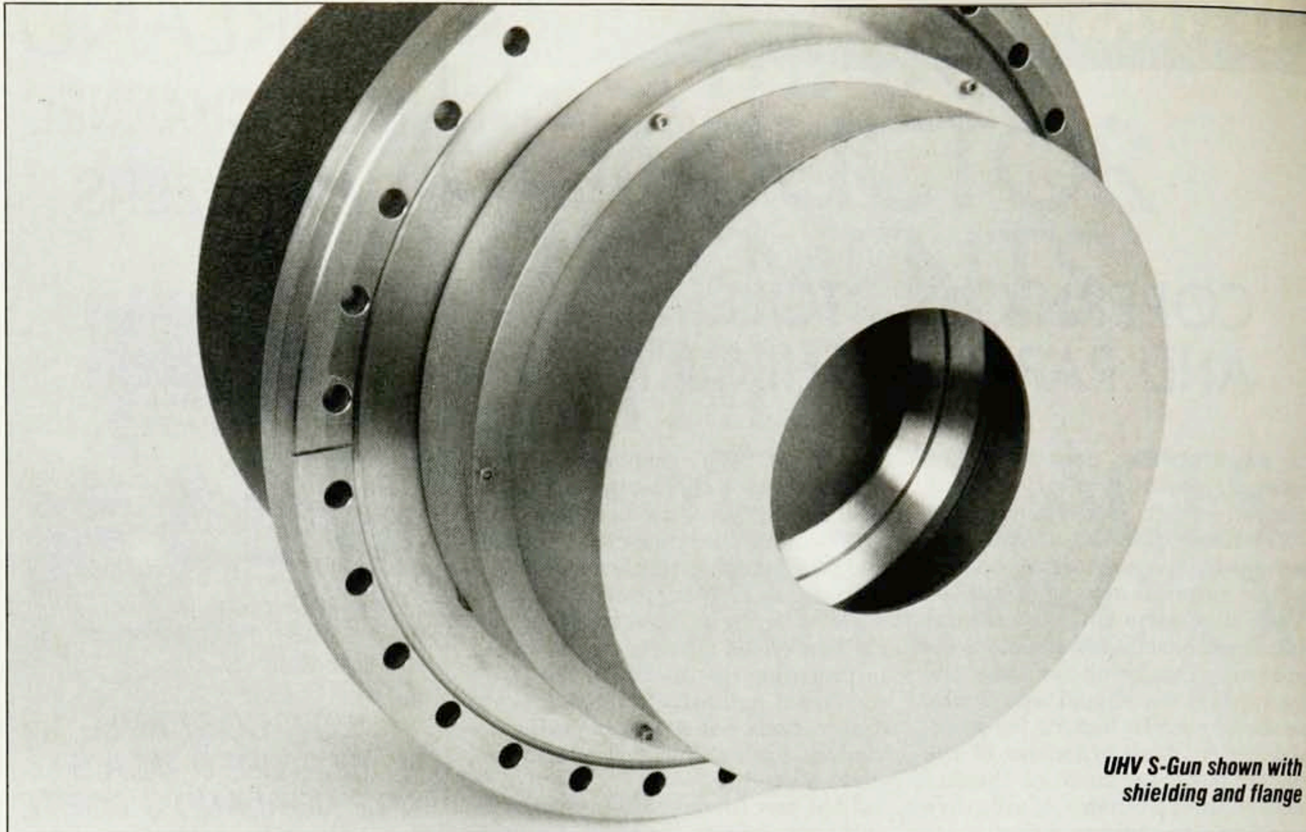
It is interesting that even as it stands above, the text makes sense. Ptolemy's epicycle theory could, within the accuracy then obtainable, correctly reproduce and predict the results of observations. What is more important, it could even be improved by adding another generation of epicycles now and then and fine-tuning the parameters. From a positivist's point of view the theory was quite a success.

And yet it was fundamentally in error—not because of its aesthetic shortcomings, but because it could not serve as the first link in the chain that led to Newton's laws. Copernicus's theory, on the other hand, could, despite all its residual epicycles. Without the Copernican world picture, Kepler would not have been able to calculate the trajectory of Mars and formulate his laws, even if he had used today's observational data instead of Brahe's. Note that Copernicus himself did not need any new data in proposing his theory; he was even somewhat hampered by too accurate observations, which were showing deviations from uniform circular movement about the Sun.

That new fundamental theories result from unconventional thinking rather than from abundance and accuracy of experimental results can be illustrated by at least one more example. Einstein did not propose his relativity theory on the basis of experimental detection of deviations from Newton's laws. Had such experimental results accumulated, we might have seen a different development. In 1894 it was proposed[2] that the perihelion shift of Mercury could be explained by changing Newton's $1/r^2$ law to $1/r^{2.00000016}$. Better observations would certainly have improved the last figures of the exponent.

There is, unfortunately, no *a priori* way of telling Copernicus-type theories from Ptolemy-type ones. As physical theories cannot be proved, the only evidence we can have for any of them is in events compatible with it. The strength of such evidence varies and there is no objectivity in assessing it. Probably no one would go so far as to declare that observations of black crows constitute evidence for the existence of white crows just because the observation is compatible with the theory. However, things are not always so easy. A "compatible event" that for one physicist is proof of the existence of gluons can remain unconvincing for another. Since the observations of apparent planetary motions were compatible with the epicycle theory, they were generally taken for its confirmation. Today we may be doing the same with our "standard theory."

The point I want to make is that



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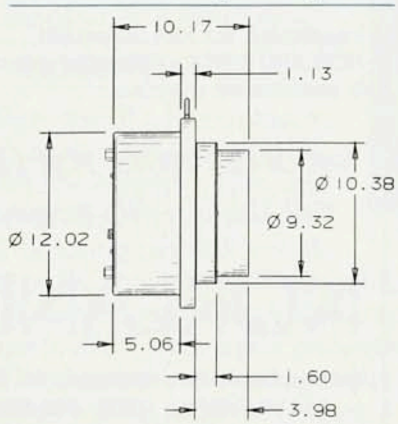


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more experiments with lots of accurate data do not necessarily mean progress in physics. They could, instead, perpetuate old theories, just refining their free parameters and adding new generations of something now and then. This danger is even greater today, when the data are screened by computers; these are looking for what we want to find and tend to mask the rest. Ptolemy's theory would be disproved by observing the crescent of Venus; would, however, the imaginary Alexandrian observer include in his computer program a code that could recognize such a crescent?

In the continuing debate about SSC funding we hear arguments about what physics can contribute to society, with obvious references to Faraday's research. There is another question, whose answer is with much less obvious: What will the SSC contribute to physics?

## References

1. J. H. Schwarz, in *300 Years of Gravitation*, S. W. Hawking, W. Israel, eds., Cambridge U. P., Cambridge, England (1987), p. 652.
2. A. Hall, Astron. J. **14**, 49 (1894).

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## The Computer as Tutor

I read with great interest Leo Kadanoff's remarks on "Interactive Computation for Undergraduates" in the December 1988 issue (page 9). The opening paragraph about the computer revolution's resulting "in some revision in the style and content of physics instruction" particularly caught my attention. So did the references to Halliday and Resnick and to Goldstein.

I studied physics in the 1960s, and graduated in 1970 with a PhD in theoretical nuclear physics. As the economic climate at that time was not good for physicists, I made a career switch and went into the computer field. I spent six years in the Federal government and then moved into private industry. Since 1976, I have been with an aerospace corporation, doing systems analysis. My specialty is software quality assurance.

I have lost contact with academia. However, Kadanoff's remarks spurred me to recall my past as a physics student. I too got my introduction to physics with Halliday and Resnick and learned my classical mechanics from Goldstein. About this I have no regrets because I think they are fine texts. But Kadanoff's references to these texts together with his comments about the computer revolution raised the following questions in my mind:

▷ Is physics still being taught the way it was in the 1960s? In particular, are concepts such as the harmonic oscillator and the square-well potential emphasized?

▷ Have computer programs like MathView Professional, Eureka, PowerMath, Milo and Mathematica (I am heavily into Macintosh software) reshaped physics instruction? In particular, now that mathematics no longer stands in the way of solving "realistic" physics problems, are students shown how to deal with oscillators other than harmonic and potentials other than square wells?

▷ The technology embodied in personal computers (not to mention powerful pocket calculators, like the HP-28S with its 32K of memory and symbolic math capability) is, of course, a two-edged sword. Their ready availability increases the potential for misapplication of mathematics to physics. What is being done to train physicists (and engineers) in the proper use of these tools? How are students being sensitized to distinguish nonsense from physically meaningful results when they look at the output from these tools?

Over the past few years, I have noticed the growing impact that computers have had within my company. Things that used to take weeks now take hours. Now that many of us are becoming more acclimated to this technology, we increasingly integrate it into our planning. The effect on productivity is often quite significant. But not all engineers are in step with what this technology can offer. Some still prefer to do things the old way—and some do it the old way quite well. My question is, Does the old way still dominate physics education? If so, why? If not, how is personal computer technology being used to improve the breadth and depth of physics education?

You may be asking yourself why an ex-physicist is so concerned about the way physics is being taught in colleges and universities. My answer is the following: I left physics in 1970 with mixed emotions. There were few job opportunities where I could directly apply physics. (Even academic positions were in short supply.) Since that time, I believe, physics has failed to attract sufficient numbers of students to meet the demands of industry. Certainly within my own com-

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