letters

What happened to Einstein's papers?

A rather large number of letters[1] in the correspondence columns of your journal under the heading of "Relativity Debate . . ." reflect the difficulties encountered when one seeks to resolve the limitations of the special theory without taking recourse to the general theory. The contemporary textbook versions of the general theory further compound the problem by the modern trend of using non-integrable local Lorentz frames of reference. Without an easily accessible complete collection of Einstein's original papers it will become harder and harder to track down the discrepancies between source and contemporary renditions. Permit me to cite a few later Einstein papers relevant to some of the questions with which your correspondents have been concerned.

At the end of section 2 of his article on the foundations of the general theory, Einstein[2] writes: "The principle of the constancy of the vacuum speed of light requires a modification." At the time, Max Abraham took Einstein to task (in a rather unfriendly manner) about this deviation from his earlier stance. Abraham's objections have now been forgotten, because they were an attempt at curtailing a researcher's privilege to change or even modify his earlier views of nature.

How far Einstein was willing to go in abandoning the constant vacuum speed of light is apparent from an article he published in 1914.[3] It is a response to an article by Harzer,[4] and in this article (in discussing the Sagnac effect) Einstein recognizes the need for distinguishing between two distinct vacuum speeds of light in certain noninertial systems. An overview of these matters and their relation to the older literature can be found in a more recent review article.[5]

Few libraries have these precious old journals, and even then the appropriate references are hard to find. Clearly much confusion could be avoided if Einstein's complete works were available in this country, either in German original or in translation. All of which prompts the question: What has prevented this publication in the quarter century that has gone by since Einstein passed away?

A Russian version has been available for many years. Yet no Western country, nor Israel has been able to take a similar initiative. Presumably they are prevented from doing so by US estate law. Surely, it is not the first time that the common interest of science and technology is being held hostage by litigation!

The present example though is utterly distasteful, because legalistic bickering has been able to delay for more than twenty years the accessibility of the intellectual inheritance of one of the great minds of this century. It is an extremely embarrassing state of affairs for a world that prides itself to champion the cause of intellectual freedom. Crackpot books about Einstein, yes, but Einstein's own writings, no: only in Russia!

At this late date a government-sponsored publication, by act of Congress, is called for. I, for one, hate to contribute to the profits of private endeavor that, through all those years, has shown such shameful disregard for the common good.

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Velikovsky again

C. Leroy Ellenberger in his letter (April 81 page 72) responding to S. F. Kogan's letter (September 80), mentions that Sagan overlooked such factors as specific heat, mass and surface area in his calculation of cooling regarding Veli-

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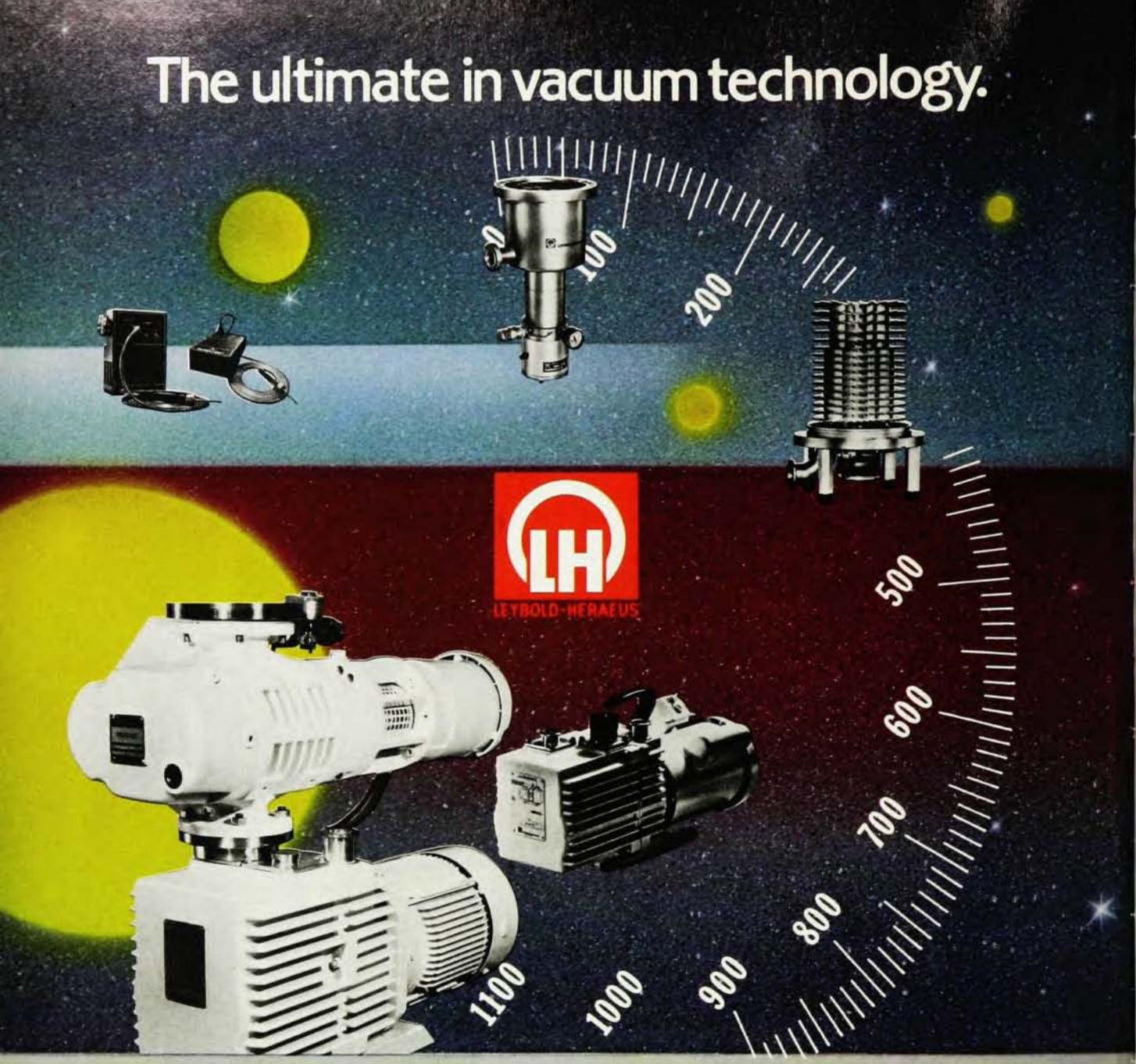

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kovsky's theory in *Worlds in Collision* [Ellenberger was refering to George R. Talbot's article in *Kronos* (Fall 1981)]. In this letter, Ellenberger asked, "Why has not one scientist in the thirty years since *Worlds in Collision* appeared formulated a *valid* refutation?" The purpose of this letter is to attempt to fill this void by correctly treating the cooling problem.

Kogan's original letter (September 1980) admonishes Sagan for only addressing the contribution to the heating of Venus due to the Sun, pointing out that Velikovsky mentions several other mechanisms. In fact, Kogan states that Sagan's own calculation regarding the heating due to ejection from Jupiter supports Velikovsky's work. It is interesting that Kogan only scrutinizes Sagan's faulty calculations when they tend to refute Velikovsky, but is mute when they support him.

In his calculation of the heating caused by ejection, Sagan claims that at least ten percent of a comet's kinetic energy would appear as heat. Unfortunately, there is no obvious mechanism by which an ejected object would attain this rather arbitrary value, and Sagan offers no proof.

The surface temperature of Jupiter is only about 200 K. It is almost certain that any object ejected from this planet would have to originate at or near the surface, as objects ejected from the central regions would tend to explode under the decrease of pressure. (Even in the center, the temperatures are not very large. Current models place the temperature at the center at less than 35 000 K.) Objects ejected from near the surface would tend, naturally, to be at a temperature close to the surface value. The only question that remains to be decided is the extent to which an object of Venus's size would be heated by atmospheric friction during the ejection process. The value obtained for this would, of course, be closely related to the time required for the ejection process itself.

To obtain an upper bound, the object may be assumed to start at the center. It is easy to calculate how long an object could take to pass through the body of Jupiter at the escape velocity (any higher velocities would require even less time). Assuming all of Jupiter's mass to be concentrated at its center, and that an object at the center is given the appropriate escape velocity, it would require less than 14 minutes for this object to reach the limits of Jupiter's atmosphere! This is clearly insufficient time to heat an object of Venus' mass to any significant extent. In any case, this heating would primarily be a *surface* effect. The majority of material significantly heated would probably be atomized and torn off the surface by friction as it passed through the atmosphere. After the heat had uniformly distributed throughout the planetary body that managed to escape, its overall temperature would not be significantly different than the surface of Jupiter.

Let us next consider the heating caused by the collisions between Venus and the Earth and Mars. Sagan's calculation on this matter, referred to by Kogan, seems reasonable. (At least Kogan seems willing to accept Sagan's result, noting that it should probably be considerably lower for the Earth. I treat it as an upper bound for the temperature increase for Venus.) The temperature increase should not exceed about 100 K for the Earth. It will be assumed that this figure it also reasonable for Venus, considering its similarity to Earth.

The only heating mechanism presented by Velikovsky not discussed to this point is that due to solar heating. This will next be calculated in some detail.

It is first necessary to obtain the differential equation governing the time dependence of Venus' temperature due to this process. The equation relating the heat added to a substance to the corresponding increase in temperature is:

$$dQ/dT = C_v M(dT/dt) \tag{1}$$

Where dQ is the heat added, C_v is the specific heat at constant volume, M is mass, and dT is temperature increase.

In the present problem there are two contributions to dQ/dt: Heat added by the absorption of solar radiation and heat lost by reradiation of Venus into space. These may be represented as . . .

$$dT/dt = (1/C_v M)(P_0 - \sigma A T^4) \tag{2}$$

where P_0 is the total solar power received by Venus over its surface, A is Venus's surface area and σ is the Stefan–Boltzmann constant. The solution to equation 2 may be represented as

$$\tau = \frac{C_v M}{4\alpha}\left(\frac{\alpha}{\beta}\right)^4\left\{\ln\left|\frac{T+(\alpha/\beta)^{1/4}}{T-(\alpha/\beta)^{1/4}}\right| + 2\tan^{-1/2}[(\beta/\alpha)^{1/4}T]\right\}_{T_0}^{T_F} \tag{3}$$

In which τ is the time required for Venus to change from temperature T_0 to temperature T_f, $\alpha = P_0$, $\beta = \sigma A$.

The most uncertain term in equation 3 is the specific heat of Venus. Limiting cases for this parameter are considered below. A value of 500 K for T_0 seems generous based on the arguments given regarding the other heating mechanisms, and 2 calories/gram K may be chosen as an absolute upper bound for C_v. For Velikovsky's theory to be correct the remaining tempera-

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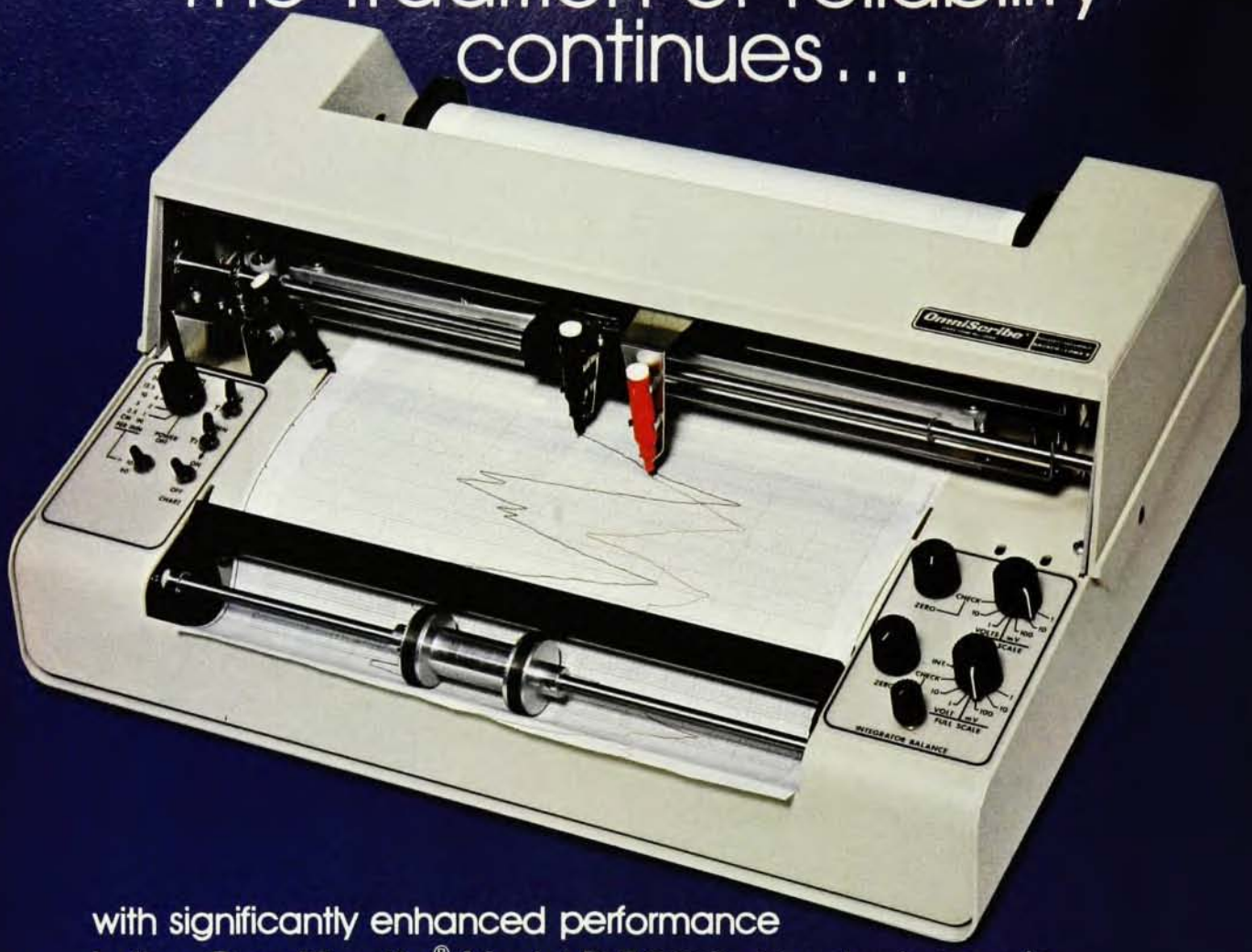

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ture increase to the present-day value of 750 K would have to be obtained by solar heating. Letting P_0 correspond to the power Venus would receive by solar radiation if it were located at the Sun's surface (neglecting the relatively small contribution due to heat conduction), equation 3 gives a time of about 51 years for this temperature increase to occur! This is clearly too large to be consistent with Velikovsky's theory.

It is clear that if Velikovsky's theory is to have any chance of being correct a far lower value for C_v must be chosen. If a value of 0.1 calories/gram K is used, still allowing P_0 to correspond to radiation received by Venus if at the Sun's surface, equation 3 gives about 2.5 years to raise the temperature of Venus from 500 K to 750 K. It is clear from these calculations that solar heating is also a very weak process in accounting for the high temperature of Venus (neglecting, of course, the greenhouse effect). This last calculation is not so obviously inconsistent with Velikovsky's theory as is the first, and requires some further consideration.

Assuming 2.5 years to be a reasonable heating time, then, it is now necessary to calculate to what temperature the planet would have decreased having a specific heat of 0.1 calories/gram K and cooling for approximately 3500 years. Equation 3 may still be used for this purpose, replacing P_0 with the total power received by Venus over its surface at its present distance from the Sun. It gives a value of about 550 K. This is far from the known value.

It should be clear that no value for C_v can be chosen to make Velikovsky's theory consistent with the facts. If a low enough value is chosen so that the planet can heat up quickly enough, it will then cool off too quickly. If a large value is chosen so that it will cool off slowly enough, it will heat up too slowly as well.

It would appear, then, when specific heat, and so on, are accounted for properly there is no way to square Velikovsky's theory with the facts. I get the impression, however, from the tone of the letters supporting Velikovsky that their writers will remain unconvinced. Nevertheless, the exercise of correcting Sagan's cooling calculation was quite interesting and, hence, worthwhile.

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4/81

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While I enjoy reading articles and letters on both sides of the Velikovsky debate, they always leave me a little empty, like the Chinese dinner joke. I think the reason why is that the debate is less like a scientific one than a political one. Each side stresses the partial evidence in its favor, ignores any potentially damaging weaknesses, and attacks the other side with generalities.

The most recent correspondence (April, page 15) is no exception. Scientific inquiry is not served by personally directed questions like "how does [R. R.] Newton explain the high proportion of Argon 36 found on Venus, its retrograde rotation, and the fact that more heat is radiated up from its surface than down . . . ?"[1]

These questions have already been explained: S. Singer[2] and R. Phillips *et al.*[3] utilize dynamical analyses and Venerian measurements to explain the concentration of primordial argon, the retrorotation, and other differences between Venus and Earth, based on an object being captured by, and then colliding with, Venus.

With regard to heat, S. F. Kogan argues against Venus' temperature being due to the greenhouse effect, by quoting from a general survey article by R. Kerr.[4] If Kogan had checked references more deeply, she would have found that P. Thaddeus[5] proposed, more than sixteen years ago, that the infrared "windows" in the CO_2 absorption spectrum (which were believed to transmit more energy from a surface near 750 K than the solar input) were closed by a combination of collision broadening, pressure-induced rotational absorptions, and induction of normally forbidden infrared transitions.

Under Thaddeus' direction, pressure-induced rotational absorption beyond 20 microns was demonstrated experimentally. This was followed by experimental demonstration of pressure-induced absorption in the 2.3-micron region.[7] The CO_2 window in this region is near the peak of the 750K black-body curve and is thus critical to the greenhouse argument. If not closed, it alone would radiate over 5×10^5 ergs cm^{-2} s^{-1}, compared to the cloud-top emission and solar equilibrium values of 2.25×10^5 erg cm^{-2} s^{-1} for the entire spectrum.

This new absorption, corresponding to transitions between ground and the Fermi pair at 4248 and 4391 cm^{-1}, together with earlier data[8] on pressure-induced transitions from Welsh and others was entered in a computer model of the Venus atmosphere. With the pressure-induced absorptions in CO_2 alone, the total upward flux into the clouds was calculated to be 2.3×10^5 erg cm^{-2}s^{-1} for V. Auduevsky's estimate[9] of a 768 K surface. More recent temperature determinations (such as quoted by Kogan) are lower, giving an upward flux of 2.2×10^5 erg cm^{-2} s^{-1}. The greenhouse effect in CO_2 is thus

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sufficient to explain the high surface temperature.

Kogan's second quote from Kerr, that the Pioneer probes saw "more energy being radiated up . . . than enters as sunlight" is not a strike against the greenhouse effect, but corroboration, as any greenhouse converts incoming short-wave radiation to long-wave. The infrared upward flux balances the net downward flux in the visible region. Surely neither Kogan nor Kerr believe that *visible* upward radiation exceeds downward–if it did, the Venera photographs would have shown ground brighter than sky!

However, both sides should be tested by prediction. I would welcome a review of total upward and downward radiation at *all* wavelengths from 0.1 to 100 microns. Tests can also be applied to changes in the Moon's orbit. We can examine tidal records for the past 4000 years, both in sediments and on the growth bands of oceanic shellfish. Because both daily and monthly periodicities can be identified, we should be able to see a discontinuous change in the number of days per month.

Furthermore, a change in the month from 36 to 29.5 days would have reduced the Moon's semimajor axis from a previous value 14.2% greater than today's, with a substantial effect on tidal amplitudes. If the encounter occurred near apogee, the pre-encounter apogee lunar tide would have been only 0.63 as great as today, or very little more than the solar tide. Twice a year, this would have resulted in neap tides less than 10% of the mean tide today. If the encounter occurred at perigee, there would have been a predictable annual pattern of intervals with no tides at all. Encounters at other positions in the moon's orbit are intermediate between these two cases. Thus early tidal records containing regular intervals with much lower amplitudes than today's will reinforce Velikovsky's arguments. I hope that PHYSICS TODAY will publish replies with relevant observational data.

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THE AUTHOR RESPONDS: Jean Piquette should easily find a few more interesting exercises for correcting Sagan's mistakes. There is a growing list of them.

However, it seems unnecessary to point them all out because most of them are irrelevant when compared to other aspects of Sagan's criticism which are much more serious and deserve the attention of every scientist who cares about scientific integrity (whether or not he is interested in this particular debate).

One of these aspects—the one emphasized in my criticism (September 1980, page 97)—is the very serious one of misrepresenting the book analyzed, claiming it did not say what it obviously did, and the other way around; calculating the probability of six *impact* collisions when only *near* collisions were described, and so on.

Another interesting aspect is well illustrated in the problem Piquette chose. It is the logic behind Sagan's arguments that I have noted before. Sagan said in effect: Velikovsky's hypothesis that Venus was ejected from Jupiter cannot claim the heat due to such ejection because Velikovsky did not suggest it first. This sort of "Alice in Wonderland" logic of "If someone else said it first it cannot be used in support of your argument" went unnoticed and unprotested. Piquette actually applies a similar type of logic when he writes parenthetically "neglecting, of course, the greenhouse." The greenhouse effect according to Piquette should not be applied to a model of Venus as reconstructed in *Worlds in Collision* because Velikovsky did not suggest it. Hence Venus did not receive the Sun's radiation during the last 3500 years. During that time, according to Piquette it would have only dissipated its heat, and that without benefit of its massive envelope to keep its heat in. It is true that Velikovsky claimed that the greenhouse effect would not by itself explain the high temperature of Venus. It is also true that the greenhouse effect apparently cannot explain all of it. But has certainly contributed some of it.

And now to Piquette's "facts": While Piquette correctly points out that Sa-

gan bases his heat of ejection on an arbitrary assumption and offers no proof, Piquette is no less arbitrary nor offers any proof either when "generously" choosing 500 K for the same T_0, conveniently ignoring the simple possibility that Venus could have erupted from some intermediate depth.

Thus Piquette's calculation is as irrelevant as is Sagan's third appendix, for they both elaborate on a relatively unimportant source of heat while neglecting one of the most important ones ("birth and expulsion"). In addition if Piquette had studied Velikovsky's work before trying "to fill the void" the following interesting exercises should have been considered:

▶ To calculate the heat due to tidal fricition of a molten Venus. In 1969 Velikovsky wrote[1] "Approaching the sun on an elliptical orbit, as I have claimed it did as a protoplanet, it had some of its energy of motion converted by tidal friction into heat. This tended 1) to keep the body plastic or molten and 2) to decrease the elongation of its orbit with each passage around the sun resulting in an almost circular orbit."[2]

▶ To calculate the heat that would result from the formation of eddy currents on a Venus moving through Jupiter's enormous electromagnetic field and on a few subsequent approaches.

John Moore's welcome suggestion to devise tests for Velikovsky's thesis should be an invitation to many PHYSICS TODAY readers, who are familiar with Velikovsky's work, to offer pertinent tests in their different fields. Before turning to this important part of his letter, some points deserve comment. The reports Moore cites on pressure-induced infrared absorption of CO_2, are all pre-1971 material. They should have been familiar to such prolific workers in the field as J. Pollack, one of the staunchest supporters of the greenhouse effect. If Moore had checked more recent references, he would have found that Boese, Pollack and Silvaggio as recently as 1979 looked for H_2O and "a significant assist form the cloud particles" to close "the major CO_2 window in the infrared and achieve the greenhouse effect."

However he would also find that both theoretical cases: H_2O plus cloud particles (Pollack) or pressure-induced CO_2 absorption (Thaddeus, Moore) were simply shown by Pioneer Venus not to be doing the job there. Pioneer Venus reports are replete with different "surprise" and "puzzling" results that counter indicate the greenhouse explanation of Venus's heat. Here are some examples, all taken from Science, 6 July 1979, which was devoted to Pioneer Venus reports:

▶ Several *surprising* SNFR results[3] appear at the bottom layer where specific expectations (Boese, Pollack[8] Silvaggio, Science, 23 February 1979) are contradicted by the SNFR flux data . . . In addition the north probe flux is several times what the model predicts, even with as little as 0.01% H_2O, thus indicating that the lower atmosphere *has less opacity* (is perhaps drier) *than expected*. (V. E. Suomi *et al.*, page 84)

▶ Below 27 km the shape of the *night* profile " . . . begins to deviate with an indication of *heating*" (*ibid.* page 85) this was so unexpected that instrument failure was considered.

▶ "Below 18 km the atmosphere at the *night* probe site is shown to be about 2°k *warmer* than it is at the day probe site, a somewhat *puzzling* finding." (A Sieffetal, page 46)

▶ F. W. Taylor *et al* reported the "*unexpected* equator-to-pole temperature increase" of 20 K (!) at altitudes below 80 km. (page 66) (emphasis added).

It is hard to understand Moore's misunderstanding of Kerr's very plain statement that "when Pioneer Venus probes looked at the amount of radiant energy passing through the atmosphere, each one found more energy being radiated up from the lower atmosphere than enters it as sunlight."

Obviously both "energy" and "sunlight" here include all radiations from uv to far ir. (. . . Suomi's remark that the SNFR "spectrally integrate radiation in a wide bandpass, from ultraviolet to far infrared."[3]) . . .

When the abundance of Ar^{36} was discovered by pioneer Venus, Donahue was quoted exclaiming that there was " . . . something unexpected and different about Venus, pointing scientists toward a major discovery. . . " and that the implications were "staggering." Yet Moore brushes it aside claiming that Singer and Phillips *et al.* have already explained it all.

Phillips *et al.* do not even attempt to explain Pioneer's Ar^{36} findings, nor retrograde motion. At the end of a long article on Venus's tectonics, they mention in passing that these two anomalies should be considered "the two significant constraints on the formation of Venus," reluctantly citing a suggestion by another worker that these anomalies could possibly be explained if Venus had an impact collision with a hypothetical innermost planet, which would have been heavily irradiated with Ar^{36} because of proximity to the sun. They add; "Appeal to a fifth former planet, in the inner solar system seems unaesthetic and ad-hoc . . . "[4]

Singer's article having been written before the Ar^{36} discovery, of course does not even mention it. To explain Venus' retrorotation he postulated (eight years after it was discovered) an ad-hoc "moon-like object from an originally retrograde orbit" that had collided with Venus, despinned it and would have "disappeared by crashing into" its surface.[5] The only "analysis" he applies is to show that solar tidal dissipation, cannot be called upon to explain Venus's retrorotation.

I wonder why scientists like Moore should find such ad-hoc catastrophist explanations any more acceptable than that of *Worlds in Collision* (1950) and *Earth in Upheaval* (1955) where Velikovsky proposed a reconstruction of events on the basis of hundreds of pages of evidence—human and geological—from all around the globe, from which he deduced far reaching conclusions which were heretical at the time, and yet since, unexpectedly found correct.

That Venus would have an anomalous rotation was one of Velikovsky's expectations. It is ironical that, while in 1950 *Worlds in Collision* drew most of the criticism because it postulated near collisions (between Venus and other planets), scientists now conjure hypothetical retrograde moons and inner planets, to collide with Venus, and conveniently disappear to evade considering Velikovsky's thesis which had expected such findings.

And so we come to Moore's suggestion that "both sides should be tested by prediction," which is very welcome.

In fact Velikovsky's conclusions, "predictions," or advance claims have had so many unexpected substantiations, that they have brought many scientists to consider him and his work ever more seriously. For more detail and examples see reference 6.

In 1962 v. Bargmann and L. Motz wrote in Science (21 December) "to establish Velikovsky's priority of prediction" of Venus's high surface temperature' Jupiter's radio signals; and Earth's magnetosphere.

To show that Venus's surface temperature was unexpected, they quoted "F. D. Drake (PHYSICS TODAY, April 1961, page 30) described the discovery as a surprise . . . in a field in which the fewest surprises were expected we would have expected a temperature only slightly greater than that of Earth. . . " About the magnetosphere they wrote: "On 5 December 1956 . . . Velikovsky submitted a memorandum . . . for the planned IGY (International Geophysical Year) in which he suggested the existence of a terrestrial magnetosphere reaching the moon. . . . The magnetosphere was discovered in 1958 by Van Allen."

These last two are specific electromagnetic phenomena which Velikovsky claimed would be found. However they are part of a wider concept, which also anticipated Jupiter's electromagnetic fields. . . .

When Velikovsky wrote of interplanetary discharges, it seemed unthinkable, now electrical discharges

between Io and Jupiter have been discovered by Voyager. . . .

From geology I point out two examples out of many.[6] In *Worlds in Collision* Velikovsky maintained that some of the petroleum was of recent extraterrestrial origin. This was criticized for petroleum was thought to be many millions of years old, of biogenic origin and never to be found in recent sediments. However, already in 1952 P. V. Smith reported[7] the "surprising" fact that Gulf of Mexico oil was found in recent sediments, and must have been deposited, according to C^{14} tests, during the last 9200 ± 1000 years. In 1962 A. T. Wilson postulated a nonbiogenic cosmic origin for all the oil deposits (with minor dissolved biogenic constituents).[8]

When Velikovsky attributed the sudden mass extinctions of different species to extraterrestrial near collisions (*Worlds in Collision, Earth in Upheaval*) it was a heresy against evolution.

Last year L. W. Alvarez *et al.*, on the basis of high amounts of extraterrestrial iridium found in the Cretaceous–Tertiary boundary layer, came to the conclusion that the sudden extinction of many species at that time was caused by an impact collision with an extraterrestrial object, about 10 miles in diameter.[9] However, as was pointed out by scientists from different fields[10], such an object would be far to small to account for (1) the very high percentage of the extraterrestrial iridium found, (2) its global distribution at the Cretaceous–Tertiary boundary—from Denmark to New Zealand, and (3) the great extent of the extinctions of different species. Therefore a close approach (rather than impact) of a massive comet with a sweeping tail, as Velikovsky had suggested, seems worth considering.

Alvarez *et al.* conclude: "... if the C–T extinctions were caused by an impact event, the same could be true of the earlier extinctions . . . five . . . since the end of the Precambrian . . ."

Why then exclude the extinctions found at the end of the pleistocene, only thousands of years ago?

So many more diverse and unexpected substantiations came from astronomy and geology that it is impossible to even list them here[6,11] and certainly impossible to ascribe them all to chance. Therefore the theory behind all these advance claims needs to be seriously tested.[12] When establishment Science has concluded that "Venus was formed differently," that it probably had a collision with a planet, and that five different major extinctions on earth were caused by extraterrestrial agents they have almost arrived at the story of *Worlds in Collision* . . .

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Solar powers from satellites

The October issue included the article "OTA and NAS Evaluate Solar Power Satellite Problems" (page 53). I would like to make additional comments on the subject of solar power satellites (SPS). The OTA and NAS reports are important contributions to considerations of the SPS concept. Of the two assessments, the OTA report (OTA-E-144, August 1981, for sale by the Superintendent of Documents, US Government Printing Office, Washington, DC 20402), represents a more comprehensive and in-depth treatment, and is an excellent reference for those interested in learning more about this concept.

The NAS report focuses on the SPS reference system based on 1960s technology, and a design which could generate 5 GW at the receiving antenna on Earth. The NASA SPS reference scenario assumes that 60 satellites would be constructed between 2000 and 2030. This scenario served as a basis for studies pertaining to space transportation, materials resources, and manufacturing requirements, and is not "a 300-GW source of power."

The NAS conclusion that the SPS costs are prohibitive is primarily based on the assumption that single crystal silicon solar cell arrays would be at least 10 or more likely 50 times the cost goals assumed for the SPS reference