

HERMAN ZANSTRA, DONALD H. MENZEL, AND THE ZANSTRA METHOD OF NEBULAR ASTROPHYSICS

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1. *Introduction*

Two theorists went west in the 1920s, and started theoretical nebular astrophysics in America, the land of clear skies and big telescopes. One went from Holland to the California Institute of Technology, the University of Washington in Seattle, and the Dominion Astrophysical Observatory at Victoria, B.C. in Canada; the other from Princeton, New Jersey to Lick Observatory on Mount Hamilton, California. One was a theoretical physicist with almost no technical background in astronomy; the other had been steeped in astronomy from boyhood and was trained by America's great theoretical astrophysicist, Henry Norris Russell. One was a quiet, shy, other-worldly European; the other was a noisy, brash Westerner from Colorado. One was an outsider with no network of friends or former teachers in astronomy to look out for him; the other had a highly supportive thesis adviser who was perhaps just a little distrusted by safe, sane, observatory directors. Both of them understood quantum mechanics very well in the days of its infancy (1926–31), and both were excellent mathematicians, the Dutchman perhaps more so than the Coloradan.

2. *Gaseous Nebulae*

By the 1920s gaseous nebulae were ripe for theoretical interpretation. They had been studied observationally with the big American telescopes, following the early work of Angelo Secchi, Heinrich Vogel, and William Huggins in Europe, who had found they have emission-line spectra characteristic of a hot gas, including lines of H I. James E. Keeler's precision wavelength measurements of other strong nebular lines with the 36-inch Lick refractor showed that they are not emitted by known elements under laboratory conditions that had been used on Earth, and his direct photographs of nebulae with the "big" (36-inch), "fast" (F/5.7) Crossley reflector showed the forms of several planetary and diffuse nebulae. Keeler's (at Allegheny Observatory) and W. W. Campbell's rapid identification of several He I emission lines in nebulae soon after helium had been isolated by William Ramsay added another known element in them.¹ William H. Wright's slitless spectra showed the projected distributions of the brighter line emission within selected planetary nebulae, and his precision wavelengths of more lines added more puzzles and only very few identifications.² Campbell and Joseph H. Moore discovered on their high-resolution spectrograms that the emission lines were split marginally in a few

planetaries, indicating that they were expanding or contracting.³ Heber D. Curtis's direct photographs of many planetary nebulae revealed that they invariably had faint, blue stars at their centres (if searched to a sufficiently faint level).⁴ With the 100-inch reflector at Mount Wilson Observatory Edwin Hubble discovered that diffuse emission-line nebulae (such as Orion, M 8, and M 16), all have one or more O or early B 'exciting star(s)', while later-type (cooler) B stars are present in nebulae with continuous spectra.⁵ At Lowell Observatory Vesto M. Slipher found that the nebulae with 'continuous' spectra, on better exposed, higher-resolution spectrograms, actually turned out to have the same absorption-line spectra as the brightest stars within them, showing that these nebulae shine by 'reflected' or scattered light.⁶

In 1922 Russell, the very perceptive American astrophysicist, deduced on the basis of these observational results that the mechanism by which the gas in nebulae is excited to emit its line spectrum is radiation from the hot star or stars involved in a nebula. He suggested the radiation might be electromagnetic (light, visual and ultraviolet) or corpuscles (fast particles).⁷

3. *Herman Zanstra*

Herman Zanstra was the theorist who first published the explanation of the way in which this occurs, though he had not read Russell's paper or known about the American observers' results before 1924, when Walter Baade told him about them in Hamburg, Germany.

Zanstra, the shy Dutch theoretical physicist of the Introduction, born in 1894, had not been educated in Amsterdam, Leiden, or Groningen, the leading centres of astronomical research in the Netherlands, but in the Delft Technical University (Hochschule) from which he graduated in 1917 with a Chemical Engineer's degree.⁸ He was much more interested in theoretical physics than in chemical engineering, and he studied and began research on his own while holding a job first as a "technical assistant" (probably a laboratory teaching assistant) at his alma mater for two years, and then as a physics teacher at a city high school in Delft for two more years. Zanstra's own tastes in physics were much more esoteric than the subjects he was teaching. Working alone, he produced a highly theoretical and mathematical paper on relative motion in a universe in which the total angular momentum about the centre of mass is zero, which he called an hypothesis of Abraham Föppl (though Henri Poincaré had also speculated and written extensively on this idea). Zanstra succeeded in getting this paper published in Dutch (in 1921), and then republished two years later in German.⁹

While he was still working on this paper, he apparently sent a draft of it to William F. G. Swann, an unconventional English-born theorist who was then teaching physics in America. Zanstra wanted to go on to a Ph.D. in physics; that path seemed closed to him in Holland, but Swann, then on the faculty of the University of Minnesota, arranged for him to be admitted there. Zanstra earned his doctoral degree

at Minnesota in two years, finishing in 1923 with an expanded, revised version of this paper as his thesis on “relative motion in classical mechanics”. It seems to have made no predictions that could be tested experimentally in the laboratory, and no one seems ever to have adopted it or followed it up.¹⁰

4. *Post-doctoral Fellowship*

But Zanstra applied for, and got, undoubtedly largely on the recommendations of Swann and his other professors at Minnesota, one of the quite new National Research Council post-doctoral fellowships. It provided support for one year initially, but was renewable for two more years successively, if he continued to show progress. He spent his first year at the University of Chicago with Swann, who had moved there from Minnesota. Zanstra’s stated research topic was “correlation of electromagnetic phenomena in terms of moving electrons only”. His synopsis of it was long on goals but short on specifics (except that Swann had suggested it), but basically the idea was to develop a theory of electromagnetism without fields, couched entirely in terms of interactions of moving electrons. Since neither his bibliography nor his reports to the International Education Board, the arm of the Rockefeller Foundation that administered the fellowships, show any sign of results from it, probably what Zanstra actually did was to complete a written version of his thesis (which most likely had been speeded up for Swann’s departure from Minnesota, and accepted in unfinished form) for publication in the *Physical review*.¹¹

Before the 1923–24 academic year had ended, Swann had decided to move on again, this time to Yale University. He took with him Ernest O. Lawrence, who had entered Minnesota as a graduate student one year after Zanstra (Lawrence got a Ph.D. in 1925 after one year each at Minnesota, Chicago, and Yale), but he did not take his older Dutch postdoc with him. Instead he recommended that Zanstra spend a year in Europe, working with theorists there.

Zanstra was very good in mathematics, and in spite of his not having accomplished anything meaningful at Chicago, his fellowship was renewed for a second year. By now he wished to develop what was then called a “fundamental theory” (the equivalent of a “grand unified theory” of today), which would include gravitational and electromagnetic phenomena on equal terms, as both Albert Einstein and Wolfgang Pauli were then trying to do. Apparently Zanstra wanted to work under Arnold Sommerfeld in Munich, but without a powerful sponsor to recommend him, he was not invited there. Instead, Sommerfeld sent him on to his own former student, the brilliant, sarcastic Pauli, who had just become a docent at Hamburg University. Evidently they did not hit it off well, for Pauli, when asked by the Rockefeller Foundation several years later confidentially to rank order all the NRC fellows he had known, replied giving only the names of the best three (two of them were Robert Oppenheimer and Max Delbrück), and the worst, Zanstra, who he said was “very poor”. All the rest, between these extremes, Pauli said,

were “entirely satisfactory”.¹²

One of the predictions of Zanstra’s theory was that there could be negative mass as well as positive, and he hoped to explain the high radial velocities that were just then being measured for a handful of galaxies as a consequence of gravitational repulsion between positive- and negative-mass ‘spiral nebulae’ (more or less similar to the ‘cosmological constant’ of Einstein’s general theory of relativity). Pauli did not support these ideas of Zanstra, but put him in touch with Baade, his close friend, then a staff member at Hamburg Observatory, and with Rudolph Minkowski, in the physics department. Baade became Zanstra’s friend and adviser on astronomical topics like spiral ‘nebulae’ and real ones. Undoubtedly Baade recognized the great mathematical powers of the Dutch theorist (who was just one year younger than him), and certainly he turned Zanstra from his grandiose ideas of founding a new physics to working much more productively on real astrophysical problems. Baade suggested he look into the question of how the hot stars might ‘excite’ the emission-line spectra of gaseous nebulae.

As part of his NRC fellowship, Zanstra travelled in Holland and Germany, met many leading physicists of the 1920s, and spent two months at Niels Bohr’s Institute in Copenhagen, then in the ferment of the new quantum mechanics, which he quickly picked up. Augustus Trowbridge, the senior Princeton experimental physicist then stationed in Paris as the Rockefeller Foundation’s “director of science” for Europe, also counselled Zanstra to work on more limited theoretical problems. Before publishing, Trowbridge advised Zanstra, he should consider carefully “the physical consequences of some of his rather extraordinary mathematical conclusions”. Zanstra had submitted a long paper on his theory of “the correlation of gravity and electromagnetic phenomena in a symmetrical scheme” to the *Philosophical magazine* (a respected English physics journal devoted mostly to theoretical or ‘mathematical physics’ papers) in January, and as it was delayed for months by the editors, and ultimately rejected, Zanstra took Baade and Trowbridge’s advice more seriously. The second year of his fellowship would end in June, and he had no results to show for it. Earlier he had hoped to get a teaching job for the next year back in the United States, preferably at a university near one of the larger astronomical observatories, so that he could work on a second paper on his new theory, applying it specifically to spiral ‘nebulae’. By spring he realized there was no hope of that, and following Baade’s advice, he was working on “an application of the quantum theory to the emission of light by diffuse nebulae”.¹³

In it, he considered only the hydrogen Balmer lines, $H\alpha$, $H\beta$, $H\gamma$, for they and a few He I lines had been identified in gaseous nebulae, but the origin of the rest of the observed nebular emission lines was still a mystery. Zanstra knew that these H I lines could be excited by absorption of ultraviolet continuum radiation in the higher-energy Lyman lines $L\beta$, $L\gamma$, $L\delta$, which, absorbed by neutral H atoms in its ground state with principal quantum number $n = 1$, excite them to levels with $n \geq 3$, leading to emission of the Balmer series. (The excitations to $n = 2$ lead only to scattering of $L\alpha$.) The hotter a star is, the stronger its ultraviolet continuum

would be in the regions of $L\beta$, $L\gamma$, etc., and hence the stronger the resulting Balmer emission lines would be, compared with the continuous spectrum of the nebula (a fraction of the starlight in the photographic region, scattered by an unspecified process that was later found to be dust). With his knowledge of quantum mechanics, using roughly estimated values of transition probabilities (which had not yet been calculated in detail then, even for H I) and estimates of stellar and nebular parameters provided by Baade and Minkowski, Zanstra was able to work out this problem quantitatively. The result he found, that nebulae with hotter stars within them ($T > 20,000$ K) would show mainly H I emission-line spectra, while nebulae with cooler ‘exciting stars’ would show mainly continuous spectra, matched Hubble’s observational result fairly well.

Zanstra rushed a draft of this paper to Trowbridge in early July 1925; he had applied for a renewal of his NRC fellowship by telegram at the last moment in mid-April, and it had been granted, though in mathematics rather than physics, indicating that the physicists who advised the NRC had little confidence in his “symmetrical theory”. But by then Zanstra was visiting astronomers at Potsdam, to get better values for the temperatures of B stars, and was planning to move to an American institution, preferably the California Institute of Technology, where he hoped the mathematical physicist Paul Epstein would sponsor him.¹⁴

Zanstra sailed for America in late August, and was soon at Caltech, where he reworked his nebular theory. He had naturally used an ‘idealized case’ (or model) of a nebula, and only by stretching all the parameters had he calculated strong enough H I line intensities to agree with Hubble’s estimates of the observed values. Baade, who had originally suggested the problem, now conceived a second idea which solved it much more satisfactorily. As Zanstra stated in a paragraph added at the end of his draft paper just before he sent it to Trowbridge, the German observer suggested that he should consider recombination of hydrogen ions (H^+) with free electrons to the upper levels ($n \geq 3$) of H I, following photoionization by the ultraviolet continuous spectrum of the star, as an additional mechanism. Baade was led to this idea by Hubble’s published report that he had seen the Balmer continuum (emitted in recombination to the level $n = 2$) in the near-ultraviolet spectrum of one nebula. As Baade and Zanstra both realized, the same process must be occurring for all the higher levels $n \geq 3$, emitting similar continua in the then unobservable infrared.¹⁵

Working at Caltech, Zanstra quickly found that recombination is by far the more important mechanism, because in hot stars many more photons are available in the ultraviolet Lyman continuum than in the Lyman lines. Epstein was his adviser, and at the Mount Wilson Observatory offices in Pasadena he occasionally saw Hubble, who discussed his spectra and their meaning with him. The whole picture fitted together much better in terms of recombination. Zanstra saw that the number of ionization processes, measured by the strength of the Balmer H I emission lines, depended directly on the luminosity of the star in the far ultraviolet ($\lambda \leq 912$ Å, the Lyman series limit); comparing it with the observed luminosity of the star in the

photographic region ($3648 < \lambda < 5000 \text{ \AA}$) provided a direct means of determining the temperatures of O and B stars, and the central stars of planetary nebulae (all idealized as black bodies).

Zanstra presented a paper on these research results at a meeting of the American Physical Society at Stanford University on 6 March 1926. None of the other twenty-six papers was on astrophysics, and it is doubtful if any other astronomers were there to hear Zanstra's ideas. Among those listed as present was Fritz Zwicky, then a young Swiss theoretical physicist who was a Rockefeller postdoctoral fellow at Caltech; he later became a well known observational astrophysicist there.¹⁶ No doubt Zanstra continued touching up and polishing his paper, and then submitted it to the *Astrophysical Journal*, where it was published in 1927. It was a very important paper, and the abstract from the Stanford meeting had described its main results well.¹⁷

5. *University of Washington and Dominion Astrophysical Observatory*

By then Zanstra was teaching mathematics and astronomy at the University of Washington, where he had begun as an assistant professor in September 1926. Probably he had obtained the job on Epstein's recommendation, and perhaps after being interviewed at the Stanford meeting, although there is no documented evidence of either. Because of his heavy teaching load at Washington (fourteen hours of instruction each quarter) he had little time for research, but Seattle was close to the Dominion Astrophysical Observatory at Victoria, B.C.¹⁸ Its relatively new 72-inch reflector had briefly been the largest telescope in the world, from 1917 to 1919. The DAO director, John S. Plaskett, was sympathetic to Zanstra, who spent the three-month summer vacation summer of 1927 there (at his own expense), getting observational data to test his theory (with a lot of help at the telescope from Harry H. Plaskett, the director's son and an astronomer also).

That same year Ira S. Bowen, whom Zanstra must have known at Caltech, finally solved the riddle of the previously unidentified strong nebular emission lines. Using laboratory data that he and Robert A. Millikan had secured there on the ultraviolet spectra of lines like O II, O III, N II, and Ne III, Bowen showed that the 'nebulium' lines were emitted in 'forbidden' transitions of these ions, which because of their small transition probabilities are extremely weak under normal laboratory conditions, but are strong in the low-density gaseous nebulae. These lines carried off the excess energy released in each photoionization process as kinetic energy, later transformed to excitation energy, and then radiated in the forbidden transitions.¹⁹ Zanstra incorporated them in his theoretical treatment, and thus had an additional method for estimating the temperature of the exciting stars, to supplement his original hydrogen-ionization energy method, and his newer, similar helium-ionization energy method. Zanstra published the main results for the three planetary nebulae he had studied at the DAO in a short 'letter' in *Nature* in 1928, and completed writing his very important long paper on the subject at Imperial

College London in 1929.²⁰ This paper was delayed in publication at DAO for two years, however, probably largely because of the many photographic illustrations included in it of spectra and monochromatic images of planetary nebulae, printed on fine glossy paper.²¹

6. Europe

Zanstra had left Washington for a temporary, one-year appointment as an assistant professor in mathematics at Imperial College, where he completed writing his long DAO publication. His teaching in the beginning courses was considered unsatisfactory, although he did well with a relativity course for advanced students.²² He was not reappointed for a second year. With no paid position in science (it was the time of the Great Depression) Zanstra returned to Germany in 1929. Baade, who had spent a year in the United States, half of it at Mount Wilson Observatory (and who probably had further discussions with Zanstra in Pasadena in the late summer of 1926), was in Hamburg again. They had both attended a meeting of the German Astronomical Society in Heidelberg in 1928, where Zanstra gave an oral paper on his nebular results (and Baade one on a cluster of galaxies in Ursa Major that he had found and observed with the Hamburg 1-metre reflector); perhaps they had discussed another visit then. At any rate Zanstra worked on nebular research at Hamburg as a “voluntary collaborator” for eight months in 1930, mostly along theoretical lines, but also with Baade at the telescope, getting more data on planetary nebulae.²³ He wrote another long paper there, published in the *Zeitschrift für Astrophysik*, rounding out his work on the emission processes and the temperatures of the central stars in nebulae.²⁴ Soon after he published a second paper, this one on the expansion of planetary nebulae, based on high-dispersion spectra of a few bright objects published by Campbell and Joseph H. Moore, which showed the emission lines marginally split by the Doppler effect. They had not made this interpretation themselves, but simply reported the observation, and concentrated on what they thought was spectroscopic evidence for rotation of the nebulae.²⁵ These two papers, together with Zanstra’s earlier ones, and a third, which he published in England in 1932, strengthening the evidence for expansion and discussing its probable origin, laid out the basis of much of the nebular research that was carried out, mostly by others, in the next twenty years.²⁶

7. An Independent Co-discovery?

In June 1931, more than five years after Zanstra had given at the Stanford APS meeting his paper on the conversion of ultraviolet continuum photons radiated by the central star to Balmer emission lines by photoionization followed by recombination, Donald H. Menzel, in an oral paper presented at a meeting of the Astronomical Society of the Pacific, said that he and Zanstra had independently shown that this mechanism was important, and that the typical effective temperatures of these stars



FIG. 1. Herman Zanstra around the time of his retirement (author's collection).

was 40,000 to 50,000 K. Menzel included this statement in the written form of the paper which he published that same year.²⁷ Furthermore, Zanstra in his long DAO paper, also published in 1931 (but supposedly completed in 1929) *agreed* that he and Menzel had “independently conceived of this mechanism, and had calculated the order of magnitude of the brightness of the nebulae derived from it in the Balmer lines” nearly simultaneously.²⁸ For the rest of his life, Menzel continued to claim that the method of estimating the temperature of the ‘exciting star’, which most other nebular astrophysicists referred to as the “Zanstra method” should be called instead the “Menzel-Zanstra method”! What are the facts?

8. Donald H. Menzel

Menzel, born in Florence, Colorado, seven years after Zanstra, was the brash, young Westerner of the Introduction. Bright and ambitious from his early youth,



FIG. 2. Donald H. Menzel in 1927 (author's collection).

he was an outstanding student of chemistry, astronomy, mathematics, and physics at the University of Denver, where he completed his A.B. in chemistry at the age of nineteen. His mentor in astronomy was Herbert A. Howe, a teacher and visual observer of the old school, who let the eager young student observe with the 20-inch refractor of Chamberlin Observatory, on the campus. Menzel was a great reader, became an amateur radio operator, dominated the science club, taught school on the side, and had a good time in everything he did. He stayed at Denver for one more year to get a master's degree in astronomy, but by then he wanted to be an astrophysicist, not a teacher in a small Western college or university. Menzel went to Princeton as a graduate student on a fellowship, and with Russell as his teacher and mentor learned atomic theory and how to apply it to the stars. He did his Ph.D. thesis on interpreting the spectra of cool stars in terms of temperature, excitation, and ionization, while Cecilia Payne (later Payne-Gaposchkin) did hers

on hot stars as a Radcliffe graduate student, both using Harvard spectrograms under Russell's guidance.²⁹

After getting his Ph.D. in 1924, Menzel taught one year at the University of Iowa, and the next at Ohio State University, before going to Lick Observatory in 1926, just after his marriage. Though his two earlier jobs were full-time teaching positions, he had made time to do a surprising amount of research, mostly interpreting published observational data on planets, especially Mars, in physical terms. Menzel was very pleased to obtain the research position at Lick; he was the first theorist appointed to its staff, although the associate director who hired him, Robert G. Aitken, made it clear that he would be expected to take his turn observing at the telescope on the long-time Lick radial-velocity program.³⁰

Very soon after his arrival at Mount Hamilton, Menzel published a brief, interesting review of planetary nebulae, summarizing the little that was known about them and their central stars.³¹ He did not mention this paper then in any of his letters to Russell that have survived, nor did he write that he was working on planetary nebulae. Probably the paper was based on studies Menzel had begun while a student at Princeton, perhaps as assignments by Russell. Almost certainly Menzel had prepared his review to show Wright and discuss with him. No doubt he hoped to collaborate with the Lick observer, just as earlier he had collaborated with W. W. Coblentz and C. O. Lampland at Lowell Observatory, interpreting their radiometric (far infrared) measurements of planets.³² If so, he had misjudged his man, for the older Wright was fiercely independent, and had almost no knowledge of astrophysical theory. He took any criticism of his scientific work as a personal affront, and Menzel had, in a previous paper, politely but mercilessly criticized Wright's analysis of his multicolour photography of Mars in terms of its atmosphere.³³ During Menzel's entire six years at Lick, Wright was personally friendly with him, listened to what he said, but never shared any data with him, or offered to collaborate with him, or even acted on any of his suggestions. Wright just continued observing in his own, quite successful way. It was a pity, for together they would have made a powerful team, but Wright never was a team player.³⁴

At any rate, it was in that 1926 paper that Menzel later claimed he had independently published the idea of the "Menzel-Zanstra" method. In fact the paper does not say that at all. In a short section of it about the H I emission lines, Menzel wrote that a fraction of the energy in the ultraviolet continuum of the star might reappear in the Balmer lines, following recombination, but there is no quantitative calculation at all comparable to Zanstra's of just how large this fraction would be. On the basis of his qualitative statements, Menzel then concluded that a star with $T = 20,000$ K would not give nearly the strengths of the observed Balmer lines, though a temperature $T = 40,000$ K perhaps might; but he seemed to believe that this was much too hot for a real planetary-nebula central star to be. In the published paper, Menzel concluded that his rough calculation had proved that ultraviolet radiation could *not* be the mechanism that produces the observed H I emission lines by photoionization followed by recombination. "It is necessary to fall back on the

theory of corpuscular radiation”, Menzel stated in the paper, and went on to analyse Wright’s published monochromatic images of planetaries in those terms. At the end of this discussion he concluded that the problem is much more complicated than previously supposed, and that more data were needed.

It is impossible to reconcile what is published in this 1926 paper with what Menzel later claimed for it. There is no sign he had worked seriously on the problem earlier, or that he went back to it afterward, until his 1931 paper, another review of the observational status of planetary nebulae, in which he made his claim. Furthermore, it is very difficult to believe that Menzel had not read the published abstract of Zanstra’s paper at the Stanford meeting, soon after it came out in the May 1926 issue of the *Physical review*.³⁵ Russell’s prize student read everything he could find about astrophysics, and he was still in Columbus then, teaching and awaiting his marriage but surely not neglecting the library. Perhaps he had forgotten that he had read that abstract by 1931, and probably he had not studied it deeply, but it is hard to imagine that he had missed it.

9. What Happened to Menzel?

Menzel published his 1926 paper on planetary nebulae soon after arriving at Lick Observatory. He spent six very productive years doing research there as the first theoretical astrophysicist at what was then a very observationally oriented observatory. Menzel’s most important work was reducing, analysing, and interpreting all of Campbell’s observational data secured at total solar eclipses from 1898 to 1908 in a path-breaking astrophysical study of the solar chromosphere; he also worked on the astrophysics of the planets, his first love, and began his long-lasting research on gaseous nebulae with the 1931 review paper discussed in Section 8. Then in 1932 he left Lick Observatory when Aitken, by then director, was even more inhospitable to Menzel’s theoretical ideas than he had been earlier as associate director, with Campbell in the background supporting the young Princeton Ph.D.³⁶ In his new job at Harvard, which he was very glad to get, Menzel had a long, successful research career in solar physics, gaseous nebulae, and theoretical astrophysics. With several collaborators, including Leo Goldberg, Lawrence H. Aller, and James G. Baker, all then graduate students, he published an extremely important series of papers on physical processes in gaseous nebulae. He became acting director of Harvard College Observatory in 1952 and director in 1954, retired in 1966, and died in 1976.

10. What Happened to Zanstra?

Zanstra received deliverance of a sort from life as an unpaid, voluntary researcher, living on whatever resources he had, in January 1931, when he was appointed an assistant in physics at the University of Amsterdam, back in his own country. He became a member of the cosmic-ray group headed by Jacob Clay; Zanstra was its “house theorist”, interpreting their experimental results in terms of the penetration

of charged particles ('corpuscular radiation') through the atmosphere, and their orbits in the magnetic field of the Earth. He had some time for astrophysical research on nebulae and on the solar chromosphere, and on comets (all objects in which low-density atomic physics reigns), and continued to publish his results in those fields in astronomical journals.³⁷ In 1937 when Oxford University began to move its Radcliffe Observatory from England to the superior climate of South Africa, and set up a Radcliffe travelling fellowship, Zanstra left Holland to become its first holder. His duties were to do research, for the first two years (1937–39) in Oxford, and the last two (1939–41) in Pretoria. Zanstra's research flourished, and he published many more astrophysical papers. But just as he was leaving Britain for the final two years of his fellowship, the Second World War broke out, and he could not return to Europe in 1941, nor get any kind of a research position in South Africa. He wrote to colleagues at Minnesota, Harvard, Yerkes Observatory, and doubtless other research institutions in America, but in the end had to settle for a teaching post in Durban, South Africa.³⁸

In 1946, after the end of the war, he at last became the professor of astronomy at the University of Amsterdam, and director of its Astronomical Institute. There he continued to do nebular and solar theoretical research until he retired in 1959. Soon after Caltech inaugurated its astrophysics graduate teaching program in 1948, Baade, by then a senior staff member at Mount Wilson and Palomar Observatories, hoped to bring Zanstra there for a year, to do nebular research, but that visit never came about.³⁹ After his retirement, Zanstra went to the University of Michigan (where Leo Goldberg and Lawrence Aller, two of Menzel's best students and collaborators in solar and nebular research, were professors) for one year as a visiting professor. He returned to Holland, but visited Oxford and Cambridge every summer. He died in 1972.

11. *Why?*

Why did Menzel claim he had independently discovered the "Menzel-Zanstra" method, and why did Zanstra more than half agree that he had? In 1972 Menzel revealed some of his reasons at a symposium on planetary nebulae in Liège, to which Zanstra had been invited but did not come. (He was ill, and died three months after the symposium.) Menzel in his "concluding remarks" at the symposium, partly a summary of what all the other speakers had said, partly an autobiographical account of his own nebular research, referred to his 1926 paper. This time, however, he said that in it he had pointed out that "*if* [my italics] ultraviolet radiation from the the central stars is responsible for the nebular luminosity, by the photo-ionization of hydrogen, the temperature of the star would have to be of order 40,000 [K]". But, Menzel said, he wanted to add an "historical footnote"; he had "qualified this high temperature, terming it 'unreasonable'". He had done so, he said, at the insistence of his new, senior colleagues at Lick Observatory, especially Wright. Menzel went on to say that he had published a second paper on planetary nebulae in 1931, and in

that year Zanstra had independently published one based on the same fundamental physics.⁴⁰ Here Menzel ignored not only Zanstra's abstract, published in 1926, but also his 1927 *ApJ* paper and his 1928 letter to *Nature*. However, by 1972 Menzel himself was old (he died four years later); he had not prepared these concluding remarks, and he may have been forgetful. But basically this explanation rings true. Menzel probably had derived the temperature 40,000 K for planetary nebula central stars much as Zanstra had, but he had not published it because Wright and others at Lick did not believe the result. Menzel felt he could not afford to cross them at the beginning of his stay at Mount Hamilton.

From that point on it was easy for Menzel to remember his original correct result, and to 'forget' that he had not published it. If he had read or heard about Zanstra's published abstract of May 1926, as seems likely, he could conceivably have forgotten it by 1931, but it is not very probable that he did. By then Menzel was desperate to get out of Lick Observatory; he knew that he had done all he could there, and he was tired of Aitken's unwillingness to let him publish theoretical papers, or to give him leave to travel to other, more exciting observatories for discussions and research. Menzel tried hard to get a faculty position at Columbia University (this instead went to Jan Schilt), was offered an associate professorship at Minnesota but turned it down on Russell's advice that it would be all teaching and administration with no time for research, and then, in June 1932, was offered a position at Harvard, which he immediately accepted.⁴¹ For all these positions, Menzel's list of publications was important, but a strong recommendation from Russell was even more so.

Menzel, with his active, inquiring mind, and his eye on success, had already published a speculative, wide-ranging paper on the possible energy sources of the stars, and it was important for him to be an expert on nebulae as well. Thus when Russell published a note in *Nature* in May 1931 on white dwarfs and the central stars of planetary nebulae, and mentioned Zanstra's first paper in the *Zeitschrift für Astrophysik* on his observational results on the temperatures of the latter stars (his longer *DAO publication* was still in press), Menzel immediately wrote to his former teacher, insisting that he himself deserved the credit, not Zanstra. "Note in my 1926 paper that the method there given for determining temperature of planetary nuclei is essentially that followed later by Zanstra", he wrote. "With this exception. I used integrated light where he used monochromatic radiation."⁴² In his 1931 review paper on planetary nebulae, Menzel backed off a little, saying he and Zanstra had "independently" shown that the luminosity of the nebula was derived from the photoionizing radiation of the star, but neither in this paper nor in his letter to Russell did he mention that he had concluded that this could *not* be the process. Nor did he mention that Zanstra's 1926 abstract had appeared in print two months after his oral paper at Stanford, and two months *before* Menzel submitted his review to the PASP, where he knew it would get quick publication. It is hard to escape the conclusion that Menzel was playing fast and loose with "the truth, the whole truth, and nothing but the truth".

It is more difficult to understand why Zanstra agreed that he and Menzel had “independently conceived of the mechanism of ionization and recombination the order of magnitude, and had calculated the order of magnitude of the brightness of the nebula from it” in the Balmer lines, as he did in his important *DAO publication* published in 1931. Whether or not they met in person and discussed this point before Zanstra published his statement is not a matter of record, but their paths do not appear to have crossed in the years 1926 to 1931. Zanstra must have looked at Menzel’s 1926 paper, and what he wrote, quoted above, is technically correct (except probably the word ‘independently’, but no doubt he was too inhibited to bring this up). But Zanstra did not write, as he could have, that his 1926 abstract gave a numerical value for the temperature of O stars, as his full paper of 1927 did in much more detail, while Menzel’s paper actually rejected the value his order-of-magnitude estimate predicted. Menzel was active, upwardly mobile with his eye on the main chance, and hungry; he seized the prize and hung on to it. Zanstra was passive, not a fighter, and drifted with the tide.

However it was obvious to anyone who was interested in nebular astrophysics how much more analysis Zanstra had put into his work, backed up by observational data it suggested which he had obtained at the telescope (with Baade’s and Plaskett’s help). All papers and books on nebular astrophysics (except Menzel’s own, and a few early ones by his students) simply refer to Zanstra as the discoverer of the method.

12. Conclusion

Zanstra opened the field of theoretical nebular astrophysics in America, and Menzel followed after him and, with his students, greatly extended it. Zanstra was only in the United States and Canada as an active research astrophysicist for three years; Menzel and most of his students stayed there all their lives. This curious episode illustrates that outstanding astrophysical theorists can be very different from one another, just as observers are. Astronomers and astrophysicists are human beings. New theories, like this basic way of understanding gaseous nebulae in terms of photoionization, are quantitative, physical interpretations led by observational findings, often qualitative and incomplete. Besides having a good theory, it often helps a theorist to be pushy and to have a widely known, successful teacher as an advocate.

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