The Search For Angle *x*

We have tried to show that pyramidal ice crystals are real and that their pyramid faces are normally the simplest possible, namely, the $\{10\bar{1}1\}$ faces. Theoretically these faces are expected to make an angle of 28° with the c-axis of the crystal. Photographs of ice crystals show pyramid faces that appear to be the $\{10\bar{1}1\}$ faces, and photographs of odd radius halos generally show halos that look like those theoretically expected from crystals with $\{10\bar{1}1\}$ faces. So we think we are making some headway in our effort to understand pyramidal ice crystals and odd radius halos. Getting this far, however, occupied the better part of two centuries, and it was not a smooth ride. We find this struggle for understanding to be fascinating in its own right, but we also think there are lessons to be learned from it. In this chapter we therefore look at it in some detail. It is a matter of fleshing out the history outlined in Chapter 7.

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Ice has long been thought, correctly, to have hexagonal, or perhaps trigonal, symmetry. Johannes Kepler, for example, in 1611 in *The Six-Cornered Snowflake* [35] remarked on the hexagonal symmetry of ice and speculated on the internal structure that might be responsible for it. Because of the hexagonal or trigonal symmetry, people could reasonably conjecture the existence of pyramidal or rhombohedral crystals of ice, even if nobody had ever seen one. Although other crystal shapes are conceivable, a pyramidal crystal became the standard crystal model used by people studying odd radius halos. When complete, the crystal would have two basal faces, six prism faces, and twelve pyramid faces, as in, say, Figure 9.5. But the value of angle x—the inclination of the pyramid faces to the c-axis—does not follow from the symmetry, and, as we have seen, it is crucial

for halo theory, since it determines the wedge angles on the crystal and hence the radii of any resulting circular halos. So a major part of the effort to understand odd radius halos was directed at finding the most plausible value for x. This was not so easy, given the meager record of pyramidal crystal observations and the confused record of odd radius halo observations.

Before Bravais

We mentioned in Chapter 7 that Mariotte in the seventeenth century had briefly entertained pyramidal crystals as a possible explanation for the 22° halo. Each of his crystals was a triangular pyramid in the strict sense: it had a triangular basal face and three pyramid faces, nothing more. Mariotte took $x = 30°$.

Venturi [83], in the early nineteenth century, also used pyramidal crystals to explain various halos, most notably the tangent arc. (He was not treating odd radius halos.) In fact, all of Venturi's columnar crystals had pyramidal terminations, with no basal faces. Venturi took $x = 60°$.

Most of Venturi's explanations of individual halos did not pan out, and reading Venturi's book makes one appreciate how difficult the subject of halos was for its pioneers. We mention Venturi here mainly to show that pyramidal crystals, if only hypothetical, were indeed part of the halo conversation at this time.

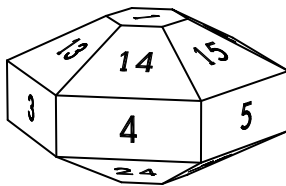Galle [18] in 1840 used pyramidal crystals to explain the 22° halo, though not in the same way as Mariotte. Galle had made many careful measurements

FIGURE 11.1   Pyramidal crystal of Galle, with $x = 60.32°$. Compare with Figures 9.5 or 9.6.

of halos and had concluded that the average measured radius of the 22° halo was slightly less than what he calculated theoretically for a wedge angle of 60°. He reasoned that the 22° halo was actually a mix of two overlapping halos, one made by the usual 60° wedge, and one made by a slightly smaller wedge and thus having a slightly smaller radius. He calculated that a pyramidal crystal with inclination $x = 60.32°$ would give the desired smaller radius. In Figure 11.1 it is wedge 13 23, or 14 24, etc., that has wedge angle $\alpha = 180 - 2x = 59.35°$, which was the angle that Galle wanted.

If you use our Table 9.1 to calculate all of the wedge angles on Galle's pyramidal crystal, you will find three wedge angles all very close to 60°. Each would give a halo with radius close to 22°, so for Galle the 22° halo was actually a mix of three overlapping halos.

But more to the point: When you compute the wedge angles, you will also find
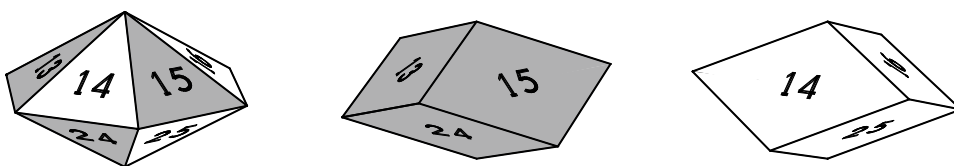
FIGURE 11.2   Pyramidal crystal and two rhombohedral crystals chosen to illustrate the close relation between the (di-)pyramidal and rhombohedral forms. The dark faces on the pyramidal crystal correspond to the faces on the dark rhombohedron, the light faces correspond to the faces on the light rhombohedron. The two rhombohedra differ from each other only by a 60° rotation about the *c*-axis (vertical). The rhombohedra shown here are shaped like those reported in 1821 by Clarke, who measured the interfacial angles and found them to be 60° and 120°. From these values Bravais calculated the inclination *x* between any face and the *c*-axis to be 54.7°.

angles that theoretically give rise to halos having radii of about 9°, 18°, and 31°. Galle noted that there were a number of reports of halos having radii more or less agreeing with these three theoretical radii. He also noted, however, that there were other reported radii as well, radii that were not explained by his hypothetical crystal, and he made no claim to have explained odd radius halos.

Galle's x-value did not catch on. Instead it was a value proposed by Bravais, in 1847, that came to be accepted and then long retained. The appeal of Bravais' x-value was that it was supposed to be based on a real crystal observation. And, in Bravais' hands, it led to passable explanations for many odd radius halos. To explain Bravais' derivation of x, we first need to say more about the symmetry of ice.

The symmetry of ice

Ice, like the mineral beryl, for example, turns out to have full hexagonal symmetry, technically described by the Hermann–Maugin notation $6/m\,2/m\,2/m$. The simplest non-prismatic crystal having $6/m\,2/m\,2/m$ symmetry should be a hexagonal dipyramid (Figure 11.2, left). For a long time, however, it seemed possible that ice had a lower symmetry, perhaps more like quartz, say, which has symmetry technically described by the Hermann–Maugin notation 32. In that case the simplest non-prismatic crystal would be a rhombohedron (Figure 11.2, middle or right), or perhaps a trigonal dipyramid.

Though not necessary for what follows, you can get an inkling of what is meant by low symmetry by taking the lattice of Figure 9.3 and replacing each lattice point by a little upright equilateral triangular prism, with all of the prisms having the same orientation. The resulting configuration of prisms has some of

the symmetry of the lattice, but not all; it has "lower" symmetry. For example, you can rotate the original lattice 180° about the c-axis, and the lattice appears unchanged, but you cannot do the same for the configuration of prisms.

The question of the correct symmetry for ice goes well beyond its marginal relevance to halo theory, and a considerable effort during the twentieth century was made to answer it. The difficulty in determining the symmetry is that, first, the external form of ice crystals gives nothing away. Or rather, their external form can at times even argue *against* $6/m\,2/m\,2/m$ symmetry; examples would be the triangular ice crystals in the left-hand photo of Figure 2.2, or the blatantly asymmetric crystal at the upper right in Figure 10.4.[1] Secondly, the hydrogen atoms in ice are nearly invisible to X-rays, so that X-ray diffraction does not give a complete picture of the internal structure of ice. But in spite of these and other difficulties, a consensus eventually emerged in favor of $6/m\,2/m\,2/m$ symmetry. Much more about the symmetry of ice can be found in a recent book by Petrenko and Whitworth [58]. Older but still very thoughtful discussions can be found in articles by Dobrowolski [15], Barnes [3], and Steinmetz [71].

Bravais

For the purposes of halo theory, the question of the true symmetry of ice is not critical. In Figure 11.2 it makes no difference whether the upper left crystal is considered to be a hexagonal dipyramid with $6/m\,2/m\,2/m$ symmetry or a combination of two rhombohedra with $3\,2$ symmetry. So when Clarke (Chapter 7) saw rhombohedral crystals and apparently succeeded in measuring their interfacial angles, it appeared to be the key to finding angle x and therefore the likely shapes of any pyramidal or rhombohedral crystals. Clarke had found the angle between faces 13 and 15 to be 120°, and from it Bravais computed $x = \tan^{-1}\sqrt{2} = 54.7°$, essentially using the 13 15 line of our Table 9.1. The crystals then would be shaped like those in Figures 11.2 or 11.3. If, as might reasonably be guessed, Clarke was seeing the $\{10\bar{1}1\}$ faces, then the Bravais value for c/a would be $\sqrt{3/8} = 0.612$, from Eq. (9.6). But c/a was not crucial at this stage, as we will see.

This value $x = 54.7°$ pervaded the theory of odd radius halos for over a century. It seems to have originated with Bravais and then been picked up by others, such as Pernter and Exner [57] in the early twentieth century and Visser [84] in the mid-twentieth century. For Bravais in 1847 the choice of 54.7° was reasonable, based as it was on Clarke's observation, which had been made only a couple of decades earlier. (Rhombohedral ice crystals had also been reported by David Brewster [10],

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[1] One can argue that the crystal in Figure 10.4 might have fallen with its c-axis vertical, in which case the two ends of the crystal would have been exposed to different conditions for growth, thus explaining the asymmetry. But it is much harder to reconcile the triangular crystals of Figure 2.2 with $6/m\,2/m\,2/m$ symmetry.
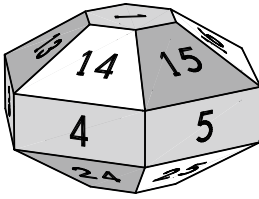
FIGURE 11.3 Pyramidal crystal inferred from Clarke's rhombohedral crystals by Bravais. The crystal is like the one at the left in Figure 11.2 but with prism and basal faces in addition to the pyramid faces. Angle *x* is 54.7°. Several generations of halo theorists regarded crystals like these—either the one here or those in Figure 11.2—as plausible models for real ice crystals, but no crystals with *x* = 54.7° have been reported since Clarke.

TABLE 11.1 Wedge angles and halo radii for the Bravais-Clarke crystal of Figure 11.3.

Wedge	Wedge angle α	Halo radius Δ_{min}
13 25	33.6°	10.9°
13 2	35.3	11.5
13 6	54.7	19.3
3 5	60.0	21.8
13 23	70.5	27.8
13 5	73.2	29.5
1 3	90.0	45.7

in 1834, but Brewster had not been able to measure the interfacial angles.) But as years passed and nobody was able to duplicate Clarke's measurement, the choice $x = 54.7°$ should have drawn more skepticism than it did. Actually, it did draw some, eventually, from A. B. Dobrowolski, who in his *Les cristaux de glace* [15] in 1916 dismissed Clarke's observation and stressed that the c/a ratio of ice was still unknown. But Dobrowolski's caution did not deter subsequent halo theorists, who continued to take Clarke's observation at face value.

At any rate, Bravais in 1847 had little else to go on. Taking $x = 54.7°$, he computed the wedge angles on the resulting pyramidal crystal, essentially evaluating the last column in our Table 9.1. The results are given in Table 11.1, along with the corresponding halo radii.

Bravais treated seven circular halos. Their measured radii were 14.2° (Heiden's halo), 20° (Burney's halo), 25°–28° (Scheiner's halo), 36° (Feuillée's halo), 90° (Hevel's halo), and of course 22° and 46° as usual. As explained in connection with Eq. (8.2), the radius of 90° is far too large to be explained by a ray path that simply enters one crystal face and then directly exits another, as we are assuming, and so Hevel's halo is a separate problem. As can be seen from Table 11.1, the radii 20°, 22°, and 46° are close to halo radii predicted from the Bravais-Clarke pyramidal crystal. That still left Bravais with radii of 14.2°, 25–28°, and 36° to account for. He resorted to a new crystal for each.

What would a reasonable crystal be? Probably it would be another pyramidal crystal, but with x to be determined. Some candidates for x are given by the following principle, which we will call the Rational Tangents Principle.

Rational Tangents Principle *If* $\tan x/\tan x_0 = v/u$ *for some small positive integers* u *and* v, *and if* x_0 *is a crystallographically likely inclination angle, then so is x. (See Appendix E.)*

Taking $x_0 = 54.7°$ from the Clarke rhombohedron, Bravais must have tried various fractions v/u until he came up with the desired halo radii. For Heiden's halo he chose $v/u = 2/3$, which gives $x = 43.3°$. The wedge angles and corresponding halo radii for the resulting pyramidal crystal can be calculated from our Table 9.1 and Eq. (8.1); they are listed in Table 11.2. Two of the resulting halo radii—the ones for the wedges 13 25 and 13 6—are indeed close to 14.2°, the value measured by Heiden. Similarly, Bravais chose $v/u = 2/1$ to produce Scheiner's halo, and $v/u = 4/1$ to produce Feuillée's halo. In all, Bravais postulated four essentially different pyramidal crystals.

In the Rational Tangents Principle it turns out that if the $\{h\,k\,i\,l\}$ faces are the faces with inclination x_0, then the $\{uh\ uk\ ui\ vl\}$ faces are the faces with inclination x. If we assume that the pyramid faces on the Bravais-Clarke crystal are the $\{10\bar{1}1\}$ faces, then the pyramid faces on the other three crystals are the $\{30\bar{3}2\}$ faces ($u = 3$, $v = 2$), the $\{10\bar{1}2\}$ faces, and the $\{10\bar{1}4\}$ faces.

Things were getting complicated. To explain Heiden's halo, for example, Bravais not only had to come up with the $\{30\bar{3}2\}$ faces. Once he had done so and essentially constructed our Table 11.2, he then had to explain why some of the predicted halos in the table did not seem to occur. (In fact, here Bravais seems to have been using a crystal with only faces 13, 15, 17, 23, 25, 27; this eliminated all but the 14.3° and 51.4° halos from Table 11.2.) He also had to explain why there were so many potential crystal faces that were theoretically more likely than the $\{30\bar{3}2\}$ faces (Table E.3 of Appendix E) but which did not seem to appear in reality. (He didn't.) Analogous excuses had to be made for the $\{10\bar{1}2\}$ and $\{10\bar{1}4\}$ faces that he had invoked.

The party line

Neither the c/a ratio nor Miller indices appear explicitly in the Rational Tangents Principle. Thus in finding angle x for the crystal that was supposed to make Heiden's halo, Bravais did not need to know c/a and he did not need to assume that the pyramid faces of the Bravais-Clarke crystal were the $\{10\bar{1}1\}$ faces. Renaming these faces to be, say, the $\{20\bar{2}1\}$ faces would change the indices of the other pyramid faces and it would change c/a, but it would not change the

TABLE 11.2   Wedge angles and halo radii for the pyramidal crystal used by Bravais to explain Heiden's halo. Angle *x* for this crystal is 43.3°.

Wedge	Wedge angle α	Halo radius Δ_{min}
13 25	42.7°	14.3°
13 6	43.3	14.5
13 2	46.7	15.9
3 5	60.0	21.8
13 5	68.7	26.6
13 24	78.1	33.2
13 16	86.6	41.3
1 3	90.0	45.7
13 23	93.4	51.4

inclination angles x_0 and x, and it would not change the crystal shapes themselves. The pyramid faces of the Bravais-Clarke crystal could even be renamed to be second order faces—the $\{11\bar{2}1\}$ faces, for example—so long as the prism faces were renamed as well.

If you try to read the literature on odd radius halos, you will find that the Rational Tangents Principle was a sort of party line, the fundamental tool for incorporating the crystallography. It allows you to generate plausible inclination angles x from a known inclination angle x_0 even if you do not know c/a and even if you do not know the Miller indices. However, the principle does not generate all such angles x. If x_0 is associated with a first order face then so is x, and if x_0 is associated with a second order face then, again, so is x. The principle also has the disadvantage that, unless it is applied conservatively, that is, unless the integers u and v are taken to be very small indeed, you quickly end up with faces that are highly unlikely.

Besson

After Bravais in 1847 the next significant attacks on the problem of determining angle x did not come until the early twentieth century, when Louis Besson and W. J. Humphreys each took a new look at the problem. They ended up nearly at each other's throats.

Besson's [5–7] idea was to select the halos whose radii had been most reliably measured and then to choose angle x so as to best fit the measured radii. Besson constructed a set of curves like those in Figure 11.4, where for each wedge there is a curve that gives the halo radius as a function of x. That is, he essentially
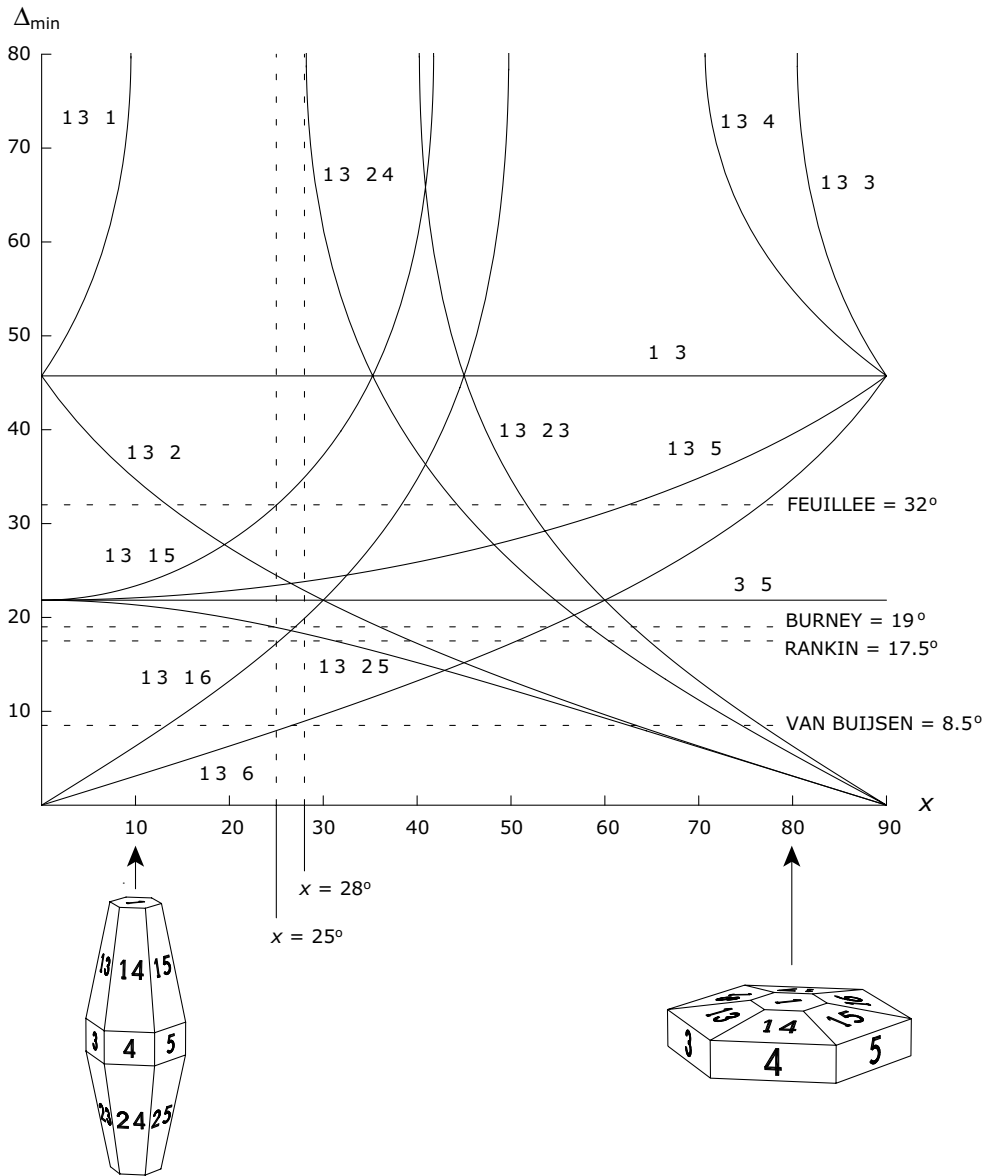
FIGURE 11.4   Besson's method of inferring *x* from measured halo radii. As was usual, the crystal was assumed to consist of six prism faces, two basal faces, and a hexagonal dipyramid determined by *x*. Each curve in the diagram gives the theoretical halo radius Δ_{min} as a function of *x* for a particular wedge of the crystal. The three lowermost horizontal dashed lines correspond to the measured radii of van Buijsen's, Rankin's, and Burney's halo at the time of Besson. Besson looked for *x*-values for which three of the theoretical halo radii would coincide with the three measured radii. The value *x* = 25° was best, but *x* = 28° was nearly as good, as can be seen from the figure. The two crystals shown are for *x* = 10° and *x* = 80° and are included just to give a feeling for the range of crystal shapes under consideration.

used Table 9.1 and Eq. (8.1) to compute the wedge angles and then the halo radii $\Delta_{\min}$ as functions of x. Once he had constructed all of the curves giving the halo radii, he then looked for the value of x that gave halo radii closest to his measured radii. The halos that he selected to analyze were the halos of van Buijsen, Rankin, and Burney, which had measured radii of 8.5°, 17.5°, and 19°—presumably our 9°, 18°, and 20° halos.

Besson found two candidates for x, namely, angles near 25° and angles near 28°. Then, as Besson put it,

> The inclination in the vicinity of 25° is in better accord with the observations of halos and it is also in harmony with the value 54°44′ which Bravais has given of a crystallographic observation of Clarke, which is $3\tan 25°14.4′ = \tan 54°44.1′$, and this has led me finally to maintain as particularly probable, the value 25°14.4′. (Besson [7, page 254])

In other words, Besson was using the Rational Tangents Principle with the Bravais-Clarke value $x_0 = 54.7°$ and with $\tan x/\tan x_0 = v/u = 1/3$. The pyramid faces on the Besson crystal would therefore have been the $\{30\bar{3}1\}$ faces. Or you can take the point of view that Besson's pyramid faces are the $\{10\bar{1}1\}$ faces and that the faces on Clarke's rhombs are the $\{10\bar{1}3\}$ faces.

As can be seen from Figure 11.4, Besson got good agreement between his predicted halo radii and the measured radii that he had chosen to analyze—the supposedly most reliable. The halo that might have tipped the scales away from $x = 25°$ and toward $x = 28°$ is the halo of Feuillée—presumably our 35° halo[2]—which Besson did consider, but as something of an afterthought. Besson knew of four observations of Feuillée's halo, the reported radii being about 32°, 33°, 35°, and 36°. He took the 32° measurement to be most reliable, and this favored $x = 25°$ over $x = 28°$.

Our Figure 8.3 would have been decisive in the other direction. For Feuillée's halo the value $x = 28°$ gives a theoretical radius $\Delta_{\min} = 34.9°$ (Table 8.1), whereas $x = 25°$ gives $\Delta_{\min} = 31.9°$.[3] The red dots in Figure 8.3, at 34.9° from the sun, appear to be right at the inner edge of the halo, whereas the yellow dot, 32° from the sun, is nowhere close. Had Besson had the luxury of access to this figure, he surely would have gone with 35° rather than 32° for the radius of Feuillée's halo, and he would have concluded $x = 28°$. In general, Besson perhaps got a bit lucky regarding his choice of halos to analyze, but his luck ran out—we think—when he came to Feuillée's halo. We should add, however, that even today we have very few good measurements of the radius of this halo.

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[2] The halo seen by Feuillée [17] himself was almost certainly *not* our 35° halo, and it is an accident that his name has been attached to the 35° halo. The halo display that Feuillée reported is a mystery to us.

[3] Use the 13 15 line of Table 9.1 together with Eq. (8.1).

Humphreys

D. M. Dennison [13], using X-rays even before Barnes, had found $c/a = 1.62$. Beginning about 1922, Humphreys [29–31] took Dennison's c/a-value and tried to derive x:

> …by X-ray analysis it has been shown that… the axial ratio (longitudinal to lateral) is almost exactly 1.62. Clearly, then, from the laws of crystallography, the ratio of the height of the pyramidal end of an ice crystal to the inner radius of its base (a lateral axis), must also be 1.62, or some multiple thereof, expressible in either a small whole number or a fraction whose numerator and denominator both are small whole numbers. If, now, we multiply 1.62 by 4/3, a factor entirely allowable, we obtain a pyramid whose sides are inclined 24°51′ to the longitudinal axis. (Humphreys [31, page 528])

From angle x he then calculated the wedge angles and the corresponding halo radii. His resulting radii were of course close to those of Besson, since the Humphreys and Besson x-values were themselves close.

We think Humphreys made a mistake. The inner radius that he talks about in the quote is not a crystallographic a-axis, at least not if he is referring to first order pyramids. For first order pyramids the ratio of height to inner radius is, in our notation, $\cot x$, which is, from Figure 9.9,

$$\frac{2}{\sqrt{3}} \frac{h}{l} \frac{c}{a} \qquad \text{(first order faces)}$$

But $2h/(\sqrt{3}l)$ is not a quotient of small whole numbers. For second order pyramids the ratio of height to inner radius is, from Figure 9.10,

$$\frac{2h}{l} \frac{c}{a} \qquad \text{(second order faces)}$$

The ratio would therefore be c/a if the pyramid faces were the $\{11\bar{2}2\}$ faces (i.e., $h=1$ and $l=2$). And the ratio would be $(4/3)c/a$, giving $x = 24.8°$ as Humphreys claimed, if the pyramid faces were the $\{22\bar{4}3\}$ faces.

We are guessing that Humphreys thought he was talking about the $\{40\bar{4}3\}$ faces when he found $x = 24.8°$, but in one sense it does not matter much, because neither the $\{22\bar{4}3\}$ faces nor the $\{40\bar{4}3\}$ faces are at all likely, contrary to his remark that 4/3 is an entirely allowable factor. Table E.2 gives an inkling of just how unlikely these faces are. In the table there are 31 essentially different crystal faces (forms) that are more likely than the $\{22\bar{4}3\}$ faces. The $\{40\bar{4}3\}$ faces are even less likely.

Thus Humphreys anticipated the approach of Steinmetz and Weickmann. But although his intention of exploiting the known c/a-value was sound, we think that he did so incorrectly and that there was no basis for his conclusion $x = 24.8°$.

This value $x = 24.8°$ has nevertheless enjoyed a long life in the halo literature, where it remained quite healthy into the 1970's and where even today it is not completely dead.

Humphreys may have been more concerned with Hevel's halo (Chapter 18) than with any of the halos that we have been discussing. He had devised an explanation for Hevel's halo that required x to be close to 25°, and this may have colored his assessment of Feuillée's halo. The measured radius of Feuillée's halo, as we saw in connection with Besson, is a good test for the theory; a radius of 32° is consistent with $x = 25°$, a radius of 35° is not. In his *Monthly Weather Review* article [29] of 1922 Humphreys was appropriately cautious about Feuillée's halo:

> The radii of these are, roughly, 8°, 17°, 19°, and, perhaps, 32°. The last of these values is based on various crude estimates ranging from about 28° to 33°, or more.

But in the version in his book, *Physics of the Air* [31], the crucial qualification about the large uncertainty in the Feuillée halo radius was missing. Humphreys took the measured radius to be 32°, and as a result his x-value looked better than it really was.

Besson and Humphreys argued with each other over their two approaches and over the virtues of $x = 24.8°$ versus $x = 25.2°$. Today, of course, we think neither of their x-values was correct. When you read about circular halos whose radii are theoretically supposed to be 8°, 17°, or 32°, you are encountering the legacy of Humphreys and Besson; the value $x = 25°$ gives 8°, 17°, and 32° halos, whereas $x = 28°$ gives 9°, 18°, and 35° halos (Figure 11.4).

Steinmetz and Weickmann

We think Steinmetz and Weickmann got it right, in 1947. As already explained, they took Barnes' value of $c/a = 1.63$, assumed that the pyramid faces were the $\{10\,\bar{1}\,1\}$ faces, and then calculated $x = 28°$. The wedge angles and corresponding halo radii are then as in Table 8.1.

Visser

In a long chapter in *Handbuch der Geophysik* in 1961, S. W. Visser [84] gave a solid summary of halo theory to that date. His treatment of odd radius halos, however, only serves to convey the confusion that surrounded them. His treatment was basically an elaboration of that of Bravais, but with more reported halo radii to contend with. Even after paring down the many reported radii to those that were supposedly most reliable, Visser had to resort to at least seven essentially different pyramidal crystal forms, as opposed to the four pyramidal forms of Bravais or the

single pyramidal form of Steinmetz and Weickmann. Although he equivocated, Visser in the end stayed with the Bravais-Clarke convention $x = 54.7°$ (for his simplest pyramidal form). He was aware of the work of Steinmetz and Weickmann but was apparently unconvinced by it.

Tricker

In 1976 E. C. W. Goldie, G. T. Meaden, and R. White [20] analyzed an odd radius display that had occurred on Easter of 1974 in England and Holland. They used the Besson approach, trying to find an x-value that would best match the halo radii that they had measured from photographs of the display. They concluded $x = 28°$, but they gave few details, not even their values for the measured radii.

The same display was discussed by R. A. R. Tricker [79] in 1979. Tricker had rediscovered the approach of Steinmetz and Weickmann and thus found $x = 28°$. Tricker's article served to bring the Steinmetz and Weickmann point of view to the attention of English speaking readers. With the appearance of Tricker's article, the Steinmetz and Weickmann approach was squarely on the table. We will make this a stopping point and move on to the history of crystal observations.

Pyramidal crystal observations

We stress again the distinction between bullet crystals (Figure 2.7) and true pyramidal crystals (Chapter 10). Most people who report seeing pyramidal crystals are seeing bullets. Bullet crystals are common, whereas pyramidal crystals—at least large ones—are relatively rare.

There do exist a handful of old observations that cannot be dismissed as bullet crystals. Clarke and Brewster, as already mentioned, each reported seeing rhombohedral crystals. H. Schlagintweit [64] also reported rhombohedral ice crystals, in 1854, found high on Monte Rosa. Héricart de Thury [25] reported seeing pyramidal ice crystals during a cave exploration in France in 1805. His description is magical, but we have no idea what he was seeing. Then there are intriguing microscope observations of pyramidal crystals by A. E. Nordenskjöld [54] in 1860. Nordenskjöld saw three different pyramidal forms on the same crystal, measured the x-values for each, and came up with two possible values for c/a. To our knowledge nobody since then has for sure seen an atmospheric ice crystal having more than one pyramidal form. We would dismiss Nordenskjöld's observation entirely, except that, first, he sounds like a cautious observer, and, second, his three measured x-values turn out to be consistent with the theoretical x-values for the $\{10\bar{1}1\}$, $\{10\bar{1}2\}$, and $\{30\bar{3}1\}$ faces computed from the modern $c/a = 1.63$. Much later,

in 1922, Steinmetz [70] measured $x = 27.97°$ on a large ice crystal in a cave. His x-value is correct, according to modern thinking, but most of the crystals in the cave were, from the sounds of his description, just ordinary frost cups without true pyramid faces.

Good ice crystal photographs began to appear about the beginning of the twentieth century [4, 22, 53, 88], but the first photos that unequivocally show pyramidal crystals seem to be those of Weickmann [86, 87] in the 1940's. His photos are good enough that the interfacial angles on some of the crystals can be measured. Weickmann did so and found angles roughly in agreement with those theoretically expected for $\{10\bar{1}1\}$ faces (and $c/a = 1.63$). The agreement was less than spectacular, however, and Steinmetz and Weickmann did not make a point of it when they wrote their *Zusammenhänge* article [72], the fundamental article on odd radius halos.

The point, instead, was made by Teisaku Kobayashi and Keiji Higuchi [38], who in the 1950's succeeded in growing pyramidal ice crystals in the laboratory. The crystals were grown under slightly peculiar conditions, which raised the question of whether the crystals were truly representative of pyramidal crystals in the atmosphere, but Kobayashi and Higuchi had nevertheless made a major advance. They measured interfacial angles on many of the crystals and concluded that the pyramid faces were indeed the $\{10\bar{1}1\}$ faces. Their published work, as well as that of Kobayashi [37], contains many fine crystal photographs that can be measured relatively easily. By this time, then, there was evidence from the crystals themselves that the $\{10\bar{1}1\}$ faces might indeed be the norm for pyramid faces.

In 1969 Takeshi Ohtake [55] photographed natural pyramidal crystals in Fairbanks, Alaska. The pyramid faces look qualitatively like the $\{10\bar{1}1\}$ faces, but the crystals do not happen to be oriented so that the interfacial angles can be measured.

We conclude this section with what is probably only a curiosity and not significant. In the 1950's K. Itoo [34] photographed ice crystals that on first glance appear to have peculiar faces—they are neither basal faces, first order prism faces, nor $\{10\bar{1}1\}$ pyramid faces. In his photos he identified $\{11\bar{2}0\}$, $\{11\bar{2}1\}$, $\{11\bar{2}2\}$, $\{22\bar{4}1\}$, and even higher index faces. The peculiar faces were nearly always flush against the glass of the collecting dish, and it looks to us like the crystals grew on the glass, or perhaps melted or evaporated where they contacted the glass, so that the peculiar faces are not crystallographic faces at all. Weickmann [86, Plates 45 and 46] has photographs of similar crystals, but Weickmann did not claim to see any exotic crystallographic faces in them. We, too, have photographed such crystals, and we do not think the peculiar faces are true crystallographic faces, but we are not positive.

Odd radius halo photographs

In Chapter 7 we mentioned some of the first published photographs of odd radius halos. Like the pyramidal crystal photos, they came too late to help the early halo theorists.

There is another photo that needs to be mentioned, because it has muddied the waters somewhat. The photo was taken by O. M. Ashford in 1956 and interpreted by R. S. Scorer [65] as showing an 8° circular halo. Because the temperature at the time was near freezing, the inference was drawn that pyramidal crystals form under warm conditions. We ourselves see in the photo only a circular lens artifact, the kind that often results when a wide angle camera lens is aimed at the unblocked sun.

20–20 hindsight

The preceding is a summary of the struggle to determine angle x or, in plainer language, the struggle to figure out what a pyramidal ice crystal might look like. The summary is incomplete, but perhaps it is enough. What are we to make of it?

If you study the old work, you will find many mistakes. There was carelessness and there was self-deception. There was far too much uncritical reliance on previous literature, so that mistakes got passed on from author to author. *Caveat lector.*

Many of the mistakes were just computational and must have been nearly inevitable among the massive hand calculations that were required. They caused no lasting harm. But there were substantive mistakes or misconceptions as well, and a couple of them did have long-term implications for the theory. The uncritical reliance on Clarke's rhombohedral crystal observation and on the inferred value $x = 54.7°$, even when the observation had not been reproduced for over a century, corrupted the theory. So did Humphreys' conclusion $x = 25°$. Perhaps it is bad manners for us to be calling attention to these misconceptions, especially with our unfair advantage of 20–20 hindsight, but we think they have proved sufficiently resilient and damaging that they deserve some exposure.

In any case, these mistakes might have been caught, had there been a decent observational record either of pyramidal crystals or of odd radius halos. But there was neither. Halo theorists ended up trying to explain facts that were not facts at all.

What lessons can we take for ourselves from our reading of halo history? In general, it is easy to make mistakes, and we are undoubtedly making our share. We need to be careful.

But more specifically: Among the small circle of halo enthusiasts there has been a shift in orthodoxy regarding the odd radius halos. The shift has been

away from something like the Visser chaos and toward the cleaner and simpler Steinmetz and Weickmann theory. The shift happened in the last few decades, and it happened without a lot of fuss. Rather, it happened without a lot of thought. Most of the people who now accept the Steinmetz and Weickmann theory have never heard of Steinmetz and Weickmann. Until recently there were few photographs of odd radius halos and few photographs of pyramidal crystals. Although we now have ample numbers of photographs, there are not so many for which the radii of the halos or the interfacial angles of the crystals have been accurately measured. So although we think we are on the right track, we have no reason to be complacent.