ally rejected by those who had built and used it (*8*). One might use this argument as one testable hypothesis to explain the abandonment of the structure, and future research at the site should perhaps be directed toward this aspect of the problem, as well as toward other issues which have been raised.

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9 September 1974

The intended point of my article was to present evidence that the cairns of the Big Horn Medicine Wheel are astronomically aligned with the rising and setting azimuths of the summer solstice sun and with three bright stars of summer dawn that could have been used for heliacal reference. This is all that archaeo*astronomy* can say, and the limit of what is really testable. Anything beyond this is speculation.

All of Reyman's comments deal with this second realm and hence lie outside the reach of useful comparison or test. In general, I think that, in proposing more elaborate explanations, he has overlooked the description of the site and its setting; it is remote, crude, and not a part of any lasting settlement. It is unlikely that the site was abandoned as a consequence of celestial precession. To this day Aldebaran, Rigel, and Sirius still rise in rough alignment with the cairns. The precessional change of these stars is chiefly in the right ascension coordinate; their rise-set points are fixed by their declinations, which have changed only negligibly in the past centuries.

We apparently disagree on the more substantive and crucial issue of the current health of archaeoastronomy. Reyman states that much of the current research in this cross-disciplinary field suffers from a lack of hypotheses. I would agree that the patient is sick, but my diagnosis is precisely the opposite.

For a hundred years and more archaeoastronomy has been a weak field, not because of a lack of speculation but from an overindulgence in it, and a concurrent deficiency of painstaking measurement and skeptical remeasurement. Investigators have seldom taken the time to establish each case factually and firmly and have too often fallen into the trap of never announcing a measurement until they had a theory to back it up. Sir Norman Lockyer's 19th-century findings on the celestial alignments of Stonehenge and the Egyptian monuments were lost in this way, when he unfortunately shifted emphasis from fact to fancy and was caught in a web of overelaboration.

We have always been too quick to leave the harder work and jump to the conclusions, to avoid the more difficult tests and extensions of measurement and to debate instead the hypotheses. Some of each is surely necessary. But if archaeoastronomy is ever to win the attention and respect of the other fields it serves, it must first establish its credibility and its credentials. In a doubting world this is usually done by exposing observational evidence to the rigors of test and cross-examination. This requires that we publish more quantitative claims and that we write and argue more about measurements than about hypotheses. I am personally encouraged that the trend of current research in archaeoastronomy is in this direction.

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Irrigation Increases Rainfall?

Fowler and Helvey (*1*) investigated the effect of irrigation on climate in the Columbia Basin, Washington, and concluded that there was no effect and that my 1967 claim (*2*) of a significant increase in precipitation "does not appear sound." To help resolve the question, I have tested the data available for 7 years since 1966, and I find that my original claim is strengthened.

July and August rainfall in the Columbia Basin was 50 percent higher during the period 1955 through 1973 compared with the period 1931 through 1950. For the region outside the basin but within 150 miles (240 km) of its center the increase was 23 percent. Between the two periods, from 1950 through 1955, irrigation water from the Grand Coulee Dam became available and was applied to 400 square miles (approximately 100,000 hectares) of formerly near-desert land in the center of the basin. The inference is that irrigation increased the summer rainfall.

In 1967, using 12 years of postirrigation data, I was able to show a significant increase in July and August rainfall in the basin. Four of the 12 years were used to explore the problem

Table 1. Tests for significance of precipitation increase. The number of stations is given for the start and end of the period. Abbreviations: T and C, percentage increases for target and control gages, respectively; S.D., standard deviation $= [\overline{(T-C)^2} - (\overline{T-C})^2]^{1/2}$; $t = (T - C)(n-1)^{1/2}/\text{S.D.}$; n, number of years (successive years are assumed independent); and t_{05} and t_{99} are values from the one-tailed t-test at $P = .95$ and $P = .99$, respectively, for $n - 1$ degrees of freedom.

Period, stations	Number of stations	Precipitation (inches)		Mean/normal (%)	$T-C$ (%)	S.D. (%)	t	t_{95}	t_{99}
		Mean July + August	Normal (1931–1950)						
1959–1966									
Target	55–55	0.99	0.545	181.7					
Control	47–47	2.37	1.624	145.9	35.8	43.6	2.18*	1.895	2.998
1967–1973									
Target	52–46	0.625	0.537	116.4					
Control	44–41	1.54	1.59	96.9	19.5	28.1	1.73	1.94	3.14
1959–1973									
Target	55–46	0.81	0.539	150.3					
Control	47–41	1.99	1.62	122.8	27.5	37.9	2.71**	1.76	2.62

* Significant at $P = .95$. ** Significant at $P = .99$.

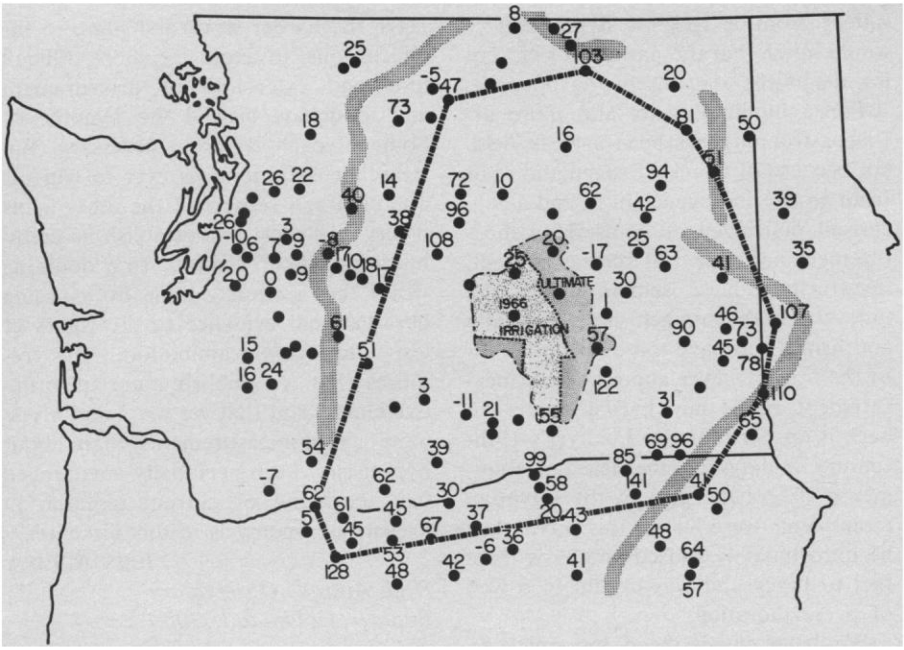

Fig. 1. Percentage increases in July and August rainfall for the period 1955 through 1973 compared to 1931 through 1950. The selected target area is outlined by dashed straight lines. Control stations are those outside the lines. Data used in selection were not used in testing. Shaded lines indicate the natural watershed boundaries of the Columbia Basin. Shaded areas indicate the extent of irrigation in 1966 and the projected ultimate area to be irrigated. All stations within 150 miles of the center of the irrigated area are shown; only those with fairly complete records were used.

—to determine the likely size of the affected area so that appropriate target and control areas might be defined for statistical testing with the remaining 8 years of data. The exploration suggested that almost the entire basin was affected and that the greatest effect appeared in the foothills just inside the natural boundaries of the basin. A target was drawn (Fig. 1) bordered by straight line segments connecting stations of high 4-year response in the boundary areas.

In the initial exploration of the problem, all rain gage records within 150 miles of the center of the basin were examined. Of the 102 gages with usable records, 55 were found inside the designated target area and 47 were outside. The gages inside were designated "target," and the gages outside, but within the 150-mile radius, were designated "control." Many of these control gages are in the high mountains or in the heavy rainfall zone west of the Cascade Mountains so that the control zone has a much wetter climate than the target zone, but no more comparable control area could be selected.

The basic statistic to be tested was the percentage of normal of each year's July plus August rainfall averaged over all target stations. This statistic was compared with the percentage of normal for the average of all control stations. "Normal" was taken to be the rainfall of the period 1931 through 1950, before the irrigation became extensive. Percentage of normal was chosen as the basic statistic in order to put the target and control areas on a comparable basis.

Since 4 of the 12 years were used to define a target, eight pairs of data remained for statistical testing. The test results published in 1967 are shown in Table 1 along with results for the seven more recent years. The statistical inference that the two sets of data are drawn from the same population was rejected at the 5 percent level in 1967. For the seven recent years by themselves the increase does not quite reach a level of significance, but when both periods are combined the 1 percent level is reached.

The fundamental difference between Fowler and Helvey's investigation and my own is that they assumed that the effect of irrigation should show up as an increase in rainfall on or immediately adjacent to the irrigated area. On this basis they chose their control area within the basin itself: it was a small group of gages from a relatively small area of land that had been irrigated for many years prior to the start of the Columbia Basin irrigation project. Perhaps they found no increase because they examined the problem on too small a scale.

The increase is real, but as I pointed out originally it could be due to a climatic trend, not of total rainfall, but of the rainfall ratio between coastal and inland areas. This possible explanation seems less likely, however, than the following: The moisture added by irrigation is evaporated and must eventually return to the earth's surface as precipitation. The question is, Where and when? The basin is nearly surrounded by mountains. The surface layer of air in the basin will eventually be carried over the mountains, and if additional moisture has been added to the surface layer of air, we would expect additional precipitation in the foothills. The earlier report (*2*) indicated that this appears to be what happens during the 2 months when additional evaporation is greatest. The study which includes the data of the more recent years seems to confirm this. The gages showing the higher percentage increases in Fig. 1 are in the foothills inside the basin. In the center of the basin the increases are small and in some cases negative.

The traditional opinion has been that irrigation is not likely to affect climate. This opinion is based on a study by Holzman (*3*) published in 1937, and it was ably defended in 1962 by McDonald (*4*) in an effort to counter extravagant claims that man might be able, by artificial means, to adjust the climate to suit himself. No such extravagant claim is intended here. In my earlier paper I discussed mechanisms that had not been considered by Holzman and McDonald; I implied that the irrigation water in the Columbia Basin was recycled at least once as rainfall; and I discussed the implications of this precipitation increase with respect to the rich wheat lands in the foothill areas. The original conclusions seem to be strengthened by the subsequent 7 years of data.

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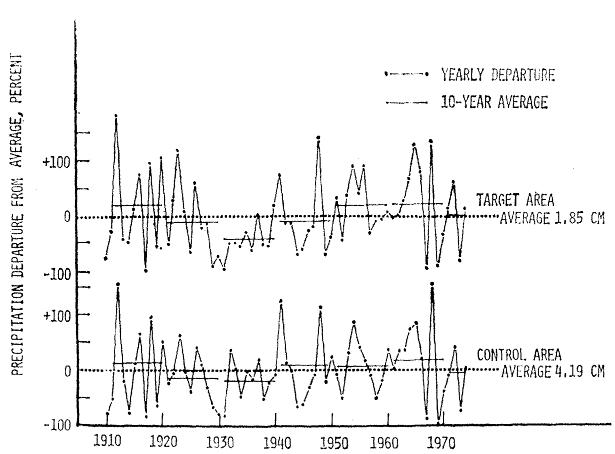

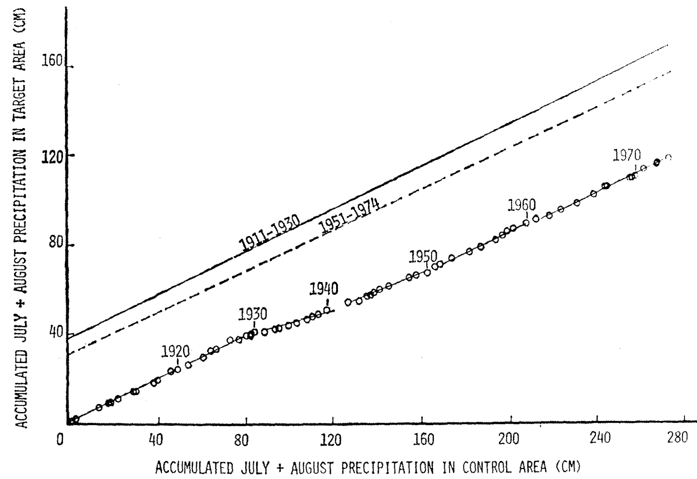

Fig. 1 (left). Yearly and 10-year departures from 65-year average July-August precipitation. Fig. 2 (right). Double mass plot of accumulated July-August precipitation for control and target stations. Differences in slopes between 1911–1920, 1921–1930, 1951–1960, and 1961–1974 are nonsignificant. The slope for 1911–1930 is significantly higher ($P = .01$) than that for 1951–1974.

Among climatic elements of the semiarid Columbia Basin and its surrounds, Stidd correctly isolates July-August precipitation as a critical factor. If continuing irrigation causes increases in precipitation, as Stidd claims, it would have far-reaching consequences on (i) land use planning, especially in floodplain areas, (ii) the necessity of maintaining quantity and quality of irrigation water from contributory watersheds, and (iii) the desirability of further increasing the irrigated area. However, his conclusion that precipitation increased because of irrigation cannot be verified with available precipitation records for this area. The differential response of stations in several climatic types to a period of widespread drought is identified by Stidd as a change in precipitation due to irrigation within the Columbia Basin Project.

The historical perspective provided by Fig. 1 indicates the primary source of error, the abnormality of July-August precipitation during the period 1931 to 1950, selected by Stidd as the climatic "normal" describing "nonirrigation conditions" (*1*). Nonirrigated, in fact, describes only the Columbia Basin Project itself. Within Stidd's target area in Washington alone, there were at least 187,000 hectares of irrigated land in 1949 (*2*). The precipitation abnormality is most evident at target stations. Stations comprising the control group show the onset of a period of below-average precipitation in the late 1920's, but return to more average conditions after only 3 to 5 years of decreased July-August precipitation. Target stations, on the other hand, do not recover to predrought values for about 10 years. An examination of the decades comprising the normal period indicates the inconsistency. The likelihood that target precipitation is of the same population for the decades 1931–1940 and 1941–1950 is less than 1 in 1000. (Precipitation in the target area in 1931–1940 was 66 percent of that in 1941–1950; t statistic = 5.242; 106 d.f.). Control stations also showed a lower (77 percent) mean precipitation in 1931–1940 than in 1941–1950. The difference, however, is nonsignificant at $P = .05$ (t statistic = 1.758; 90 d.f.). Comparisons based on these data lead to a conclusion that stations in the target area have shown a greater response than control stations over the years since the drought.

Uniqueness of climate during the 1930's and 1940's and even into the 1960's in other areas of the Northern Hemisphere has been discussed by Bryson (*3*).

Relative precipitation in the two areas can be more realistically evaluated with a double mass plot, which requires no decision as to the normality of any period. Droughts or excesses of precipitation have no effect on the slope of the plot as long as both control and target areas respond proportionally. A double mass plot of accumulated July-August precipitation for Stidd's control and target areas is shown in Fig. 2 (*4*). Slopes of the regressions relating control to target precipitation for the five decades from 1911 to 1960 and the period 1961 to 1974 are ranked as follows: 1931–1940, 0.3062; 1941–1950, 0.4048; 1921–1930, 0.4723; 1911–1920, 0.4769; 1961–1974, 0.4784; and 1951–1960, 0.4966. Testing for common slope for the regressions reveals that the last four slopes are not significantly different at $P = .05$. The combined slopes for 1911–1930 and 1951–1974 (0.4881 and 0.4662, respectively), although nearly identical visually (Fig. 2), are significantly different at $P = .01$, with target area precipitation less for the postirrigation period. These considerations add further support to our original statement (*5*) that "the claim of a significant increase in July-August precipitation because of basin irrigation does not appear sound."

Finally, the irrigated developments within the upper Columbia River Basin predating the Columbia Basin Project were equal in area to 180,000 hectares (*6*) at the inception of the drought of the 1930's, nearly equal to the present size of the Columbia Basin Project. Their effectiveness in ameliorating the regional drought of the 1930's cannot be documented.

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