Insert
Fig. 47

The stations ran all the way from the mouth of Oyster Bay to Station 9. Changes in the larvae counts (1 ft. samples, just below the surface) are shown in Figure 48 .

(INSERT Fig. 48)

Each point in the curves indicates one sample. It is clear that the Larvae Mass ebbed down the bay until low slack tide, but went no farther than off the Patterson grounds on this -2.1 foot run-out. Hence at a very low tide the mass of larvae still do not move more than half way down the bay and so are conserved within this body of water. Combining this survey with others, we may say that the Larvae Mass moves back and forth from above Station 9 to just around Deepwater Point.

4) <u>Tidal Cycle, Oyster Bay, Aug. 7, 1945.</u>

Four stations were sampled regularly throughout a tide; Stations 9, 9A off Burns Point, Station 8, and Station 8A ~~off Bowman's grounds~~ about half way between Stations 8 and 9 (Fig. 47). The data are represented in Table 50 P. 177 , and, graphically, in Figure 49 .

(INSERT Fig. 49)

Studying Figure 49 from left to right we note the following:

a) At low tide the larvae are not in the region of these stations but are down-bay from them, as shown in the studies above.

b) Maximum larvae counts were again obtained at about 3 hours before high tide.

c) All stations showed maxima at this stage of the tide. For this there is no apparent explanation, but the general unevenness of the Station 9 curve may possibly indicate a curious serpentine swirling of the Larvae Mass on this tide.

d) Mid-bay stations are lowest at high tide and low tide, thus showing the tidal movement of the mass passed them.

e) Highest counts were obtained at just past mid-ebb tide. This is unusual as compared with the other cycles and may be explained on the basis that the low tide to come had a run-out to only 5.9 feet (ie. was a "high" low-tide) so the ebbing tidal current velocity could not have been strong and the larvae were not churned out of the surface layer.

During this cycle Tollefson operated a current meter at Station 9 in order to determine actual velocities of warer movement during the flooding tide. The curve of tidal current velocity is shown ~~in~~ (Figure 49), ~~above,~~ smoothed by a moving average of threes. Note that the maximum current at Station 9 during flooding occurs soon after slack water. It would be interesting to extend such studies to ebbing tides and to check whether rapid currents do in fact mix surface with deeper layers and so dilute the larvae by spreading them vertically.

The results of this survey suggest that a location Like Station 8A ~~off the Bowman grounds~~ may be a more satisfactory sampling station than our usual Station 9, possibly because the location is farther away from channels in which the tide runs swiftly. At the former locus the larvae counts showed a beautifully uniform behavior, while the "jumpyness" of counts at Station

9, if typical, is not conducive to reliable results in ordinary sampling. It is also to be noted that Station 9A at no time gave an adequate indication of the maximum density of the Larvae Mass and so is indicated as unsatisfactory. Modification of sampling procedures accordingly might result in a better picture in the curves of larvae abundance. in the future.

5) <u>Tidal Cycle and Horizontal Section in Oyster Bay, Aug. 23, 1945</u>.

The results of this survey are given in ~~TableXXXXX,XXFXXXXXX,and~~ Figure 50 . Stations were the same as shown in Figure 47 , and were sampled at 1 foot depth.

(INSERT Fig. 50

It is fairly well indicated in this survey that the Larvae Mass moves down the bay with ebbing tide, though the picture is somewhat irregular, possibly due to churning effect of the swifter ebb-tide currents. But is is obvious that the mass moves up the bay on the flood, maxima following from one station to another progressively up the bay. Highest count was obtained at Station 9 at 2 hours and 20 minutes before high tide. Again we see that this station should be sampled at near mid-flood to give an adequate measure of the maximum density of the Larvae Mass.

6) <u>Tidal Cycle Study of Oyster Bay, July 1, 1946</u>.

In this study by Glud, Tollefson and Lindsay, we have a fine series of samples during a big tidal run-out in Oyster Bay, extending in location all the way from the mouth of the bay up to its highest reaches above Station 9. The stations are designated in Fig. 51 .

(INSERT Fig. 51)

The data are assembled in Table 51 , P. 178, and set out pictorially in Fig. 52 . Subsurface samples, usually at 6 foot depth, are available for down-bay stations.

(INSERT Fig. 52)

This survey locates the Larvae Mass as moving between Station B and Station I during the course of a tide. At high tide it appears that the center of the mass is at Station C, just above Station 9, while at low tide it has drifted down to between Station H and I, off the Patterson grounds. Maximum larvae count was obtained in samples at Station C, at 1 3/4 hours before high tide and larvae abundance at this location was possibly still increasing at the time samples were discontinued. As before, this flood maximum was higher than any maximum during ebb tide. At Station 9 (D) maximum counts were obtained at 5:37 PM or 3 hours before high tide, as also noted from the other surveys previously discussed.

Although we have no complete series of sub-surface samples for all stations, if the trend of those taken at Stations F and G are indicative, then the larvae of up-bay stations at mid- and high-tide are concentrated near the surface. At down-bay stations at low tide, however, the mass of the larvae is not to be found at the surface but deeper. This finding seems to contradict our hypothesis that during the slack tide [illegible] currents the larvae are found predominantly at the surface [illegible] layers of water, for at low-slack tide they are definately not to be found in abundance at the surface down-bay. Yet it is still possible that the mixing effect of the ebb tide current may persist during low-slack water. Only further studies can clear up this point and explain why the larvae are sub-surface at low tide down-bay if this is in fact always the case.

7) Mud Bay Tidal Cycle, July 30, 1950.

On this date Lindsay, McMillin, Wicksten and Sayce surveyed the larvae picture in Mud Bay during a fair tidal run-out and return. A good stock of larvae of straight-hinged to near-setting size was present in the bay at the time. Samples were taken periodically at the stations shown in Fig. 53 , and Table, 52 P. 179 , and Fig. 54 are to be consulted for the findings.

(INSERT Fig. 53
Fig. 54)

The mass of the larvae were found at up-bay stations at high tide and disappeared from there at low tide. Maximum count and a true indication of the abundance of larvae was obtained only at Station A off Ellison's plant on the west or channel side of the bay at full high tide. Hence it is clear on the basis of present surveys that field trips should be planned to sample Oyster Bay at ~~[illegible]~~ Station 8A 3 hours before high tide and at Mud Bay off Ellison's at high tide.

At low tide the mass of the larvae were found in no surface samples at any of the stations. Considering the Oyster Bay cycle just detailed (Fig. 52), one may guess that in Mud Bay also the larvae are for some reason yet unknown below the surface layer at low tide. A further investigation on this point would be worth while, to determine the location of the Larva Mass throughout the whole excursion of the tide.

Of particular importance is the fact that in this bay as in Oyster Bay, the larvae are retained within the upper extent of the inlet. Since the samples contained a fair portion of near-setting size larvae, this confinement *of the mass is seen to apply also to* older larvae which have been drifting about for 20 to 30 days. Thus the maximum sample (A-3 at 7:00 PM) had 2% setting size larvae as also did D-1 at 3:05 PM. Therefore we may discard the hypothesis once advanced that set failures in Mud Bay are due to "leakage" of larvae out of the bay on low low-tide run-outs. This suggestion was made on the basis that Mud Bay has a smaller water volume than Oyster Bay, in the proportion roughly of 209 to 337, and so might be expected to flush out more extensively. Now we must look for reasons for the disappearance of the older larvae during "off years" in Mud Bay, possibly in the direction of unfavorable salinity changes as discussed previously (P. ~~113~~ 114) which gradually deteriorate the larvae, finally effecting their demise after they are half grown.

8) Tidal Cycle of Bottom Samples, North Bay, June 6, 1944.

North Bay has presented a special problem in that usual procedures sometimes failed to show an abundance of larvae commensurate with the high rate of spatfall which later appeared. We have data on a tidal cycle for this bay taken at from just before a low low-tide to high tide. Unlike the other cycles, this was an early-season survey, taken before the larvae population had yet attained its maximum abundance (see the bay-year graph of Fig. 11). Sampling stations are shown in Fig. ~~X~~ 55A . and findings are given in Fig. 55.

(INSERT FIG. 55)

~~and findings are given in Table , P. , and in Fig.~~

In this bay as in others it is seen that the Larvae Mass moves up the bay at flood tide and presumably drifts to somewhere below Sunburn Point with the ebbing tide. Maximum count was obtained off Victor at about 1 1/2 hours before high tide. and greatest abundance at Station 12 was found an hour before high tide. The Larvae Mass enters North Bay proper from Case Inlet about 4 hours before high tide, and there is not much difference between Victor, Allyn and Station 12 in sampling except that the latter shows consistently higher counts. It is indicated that at high tide the mass of larvae is above the power-line towers in the region of the Sargent oyster grounds.

Now this survey contained only bottom samples, of varying depth depending on the stage of tide. It is unfortunate that we have no comparable surface samples at one foot depth on this date. Counts of North Bay surface samples on dates before and after June 6th were as follows:

	June 2	June 12
Station 12	348	16, 36 (two samples)
Station 11	164	360
Station 10	568	336

We shall therefore consider 400 larvae per 20-gallon sample as being a reasonable estimate of surface counts on the date of the tidal cycle.

Sampling was poor from the start (1 1/2 hours before low tide) until 2:30 PM (4 hours before high tide) after which the larvae abundance encountered was near the estimated surface sample value. But at their maxima, the bottom samples exceeded by more than twice the probable count at the surface.

In explanation of this important indication from the data available we return again to an interesting laboratory observation namely, that when

O. lurida larvae are placed in an aquarium and a layer of fresh water placed on top the sea-water, the larvae rise to and remain at the interface but do not enter the fresh water layer and are not seriously affected by its presence. This of course duplicates in miniature the Norwegian oyster "pollen" in which oyster-seed production at low air temperatures is made possible by the thermal insulation of confined ponds with a surface layer of fresh water.

Now in North Bay proper, at the head of Case Inlet, we have a relatively small area into which empty two large streams, one below (Sherwood Creek) and one above (Coulter Creek) the oyster grounds. It is therefore possible that a significant sheet of fresh water may be prevalent in this bay and indeed certain of our chlorinity tests seem to bear this out. If so, then the normal negative geotropism of the larvae may be counteracted by their avoidance of fresher water with the result that they remain most abundant at layers below the surface.

All this is conjecture to be sure, and further studies will be needed to clarify the larvae picture in North Bay. But a practical result has been gained, namely, the prescription that for adequate sampling in this bay the stations should be visited at about one hour before high tide and sub-surface samples taken.

The concept of a moving Larvae Mass which resulted from the tidal cycle surveys has been our guide in plankton studies. At first it was thought that one might apply a correction-factor by which larvae counts at a given location at any stage of tide might be converted to the "maximum available larvae", but we soon saw that this could not be done without very complete series of plankton-tidal cycle studies so that we pursued another alternative. We have been careful to sample when and where the pelagic larvae would be found in maximum abundance. It remains only to discuss why we have employed maximum larvae counts rather than averages as the ~~proper~~ best measure of the effective larval population.

If the larvae were spottily distributed throughout the bay and if they set all at once, at the firing of a gun so to speak, then and only then would average larval counts be the best measure of magnitude of spatfall to be expected. Neither of these assumptions are fulfilled. Instead, the larvae form a whole mass which moves together, is densest centrally and fades out at the periphery; while setting draws on this reserve of potential oysters over a pretracted period of spatfall. Theoretically the population of the entire mass could be determined but this is not practicable in the time available on field trips. Hence if one is restricted to taking a few samples on any day there are only 2 end-points attainable: zero count at the periphery of the Larvae Mass or maximum counts near its center. It is clear that one has to choose the latter and that maximum samples are the best indices of the potential setting population available in a bay. In practice several samples are always taken and the maximum taken as the index of the larvae population on a given date. Larval curves in the bay-year graphs are all based on maximum larvae counts obtained.

IMPORTANCE OF THE EARLY SET AND INSIGNIFICANCE OF THE LATER

The Setting Index or rate of spatfall on fresh cultch from week to week is one thing and the spat accumulated throughout the season on cultch put out on a certain date is another. Still another consideration, and of course the most important one from the practical viewpoint, is how much of the accumulated spat survives through the summer and winter and therefore effectively adds to the recruitment of new seed oysters.

It is apparently the experience of the oystermen that the first wave of setting in the season is the "good"one" and that failure to catch this set cannot be made up during secondary or later surges of spatfall. This conclusion is amply confirmed and quantitatively evaluated by our studies in Oyster Bay.

Let us begin by refering to our data on seasonal cultch strings put out in Dike 5, Oyster Bay, on successive dates in the summer of 1946. All strings were brought into the Laboratory in the fall and large and xxx small spat noted and tabulated as follows:

Date Cultch string put into bay	No. Live spat per shell when string removed	
	Large: 8mm diam. & over	Small: Under 8 mm diameter
June 18	135	63
25	115	62
July 2	93	85
9	0	158
15	3	209
23	0	210
30	0	243
Aug. 6	0	242
13	0	210
20	0	77
28	0	92

By refering to the graphical presentation of this reproductive season (Fig. 19), it is clear that the earliest cultch, of June 18th, caught the maximum of spat which had time to grow to about one centimeter in diameter by the end of the season; while the cultch strings of July 30th and August 6th, put in just as the second wave of setting was beginning, caught the maximum number of spat which, because of their tardy setting, did not have time to achieve considerable growth by the end of the season. During this same year (1946) Tollefson made a comprehensive investigation of the matter which is now summarized.

On 9 different dates throught the setting season three sets of 12-shell cultch strings were put out in each of 4 adjacent dikes at Burns Point in Oyster Bay. One set was taken up in early fall on September 11th, another was removed from the dikes on January 6th, and the third was allowed to remain out until April 10th or early Spring of the year following the catch. Surviving spat on the shells was counted and averaged for each string. The results are presented in Tables 53 through 55 and depicted in Figure 56 which also shows the week to week average spatfall in the four dikes during the season (from Table 40, P. 66).

(INSERT Fig. 56)

Spat counts at Burns Point ran somewhat lower than in Dike 5, as noted above, but the same two marked peaks of spatting are evident in the strings taken out of the water on Sept. 11th. At this time most all spat on the cultch put out from June 25 to July 23rd was large while strings set out after that date showed only small spat.

The graphical summary of the results of this study is most instructive. It shows very dramatically that only cultch which was put out in time to catch the first peak of spatfall came through the winter with a substantial surviving set. Although later cultch caught great numbers of spat during a second wave of spatting, the mortality of this spat was around 93% by the following spring. It is also clear that almost all the spat which survived to early January continued to live until April and probably would have continued to survive from then on with what might be called normal mortality, And this was the case even though the month of January in 1947 was unusually cold, the average air temperature at Olympia falling 3.6°F below normal. (Comparison with seasonal floating strings, Table 2, P. 33, is not apropos since there is an unusual fouling of such cultch during the later months of the summer which is not found in dike cultching.)

The critical time for spat mortality therefore fell somewhere between early September and early January and the spats most affected were those caught later in the season. We do not know the reason for this high mortality of young spat but it is easy to surmize, for it is especially clear in the culture of the Japanese, the Eastern and the European flat oyster that mature larvae are very susceptable to cold water, so it is reasonable to suspect that the young spat share something of this sensitivity and are often killed by Fall weather if they have just recently set. With regard to the larvae and the initial spatting ~~of our~~ early in the season, we find that water temperatures in the bays of lower Puget Sound are always favorable. It would be interesting to test ~~this~~ the hypothesis here offered regarding demise of late-caught sp by comparing with appropriate controls the survival of late summer spat kept through fall and winter at summer water temperature in laboratory tanks.

Hence at Burns Point in 1946 shell put out on June 25, before the first setting peak, carried on the following spring a catch of about 75 large spat per shell, but any cultch set out after the first setting peak bore only about 10 spat. These results therefore emphasize the importance of properly timing the cultching operations and the necessity for setting out cultch *just at the beginning of the first spatfall of the season*. It is the catch of *surviving* seed oysters that matters, and the sharp drop in the survival curve with lateness of summer set shows that it is as important to cultch early enough as it is not to cultch too early.

So clear-cut are the results from this study of 1946 spat survival that one believes they will bear generalization, especially since experienced oystermen themselves are of the opinion that late-caught spat are of little importance and that considerable "slippage" of late spat occurs. If so, our results show that the Olympia oyster in some of our bays "over-produces" larvae late in the summer which can have no chance of surviving as spat and that it is perhaps never worth while to catch the later spatfalls. Since Hopkins did not take into consideration the survival of spat, he was led to the erroneous assumption that his demonstration of important secondary ~~[illegible]~~ setting peaks besides the initial early-summer spatfall provided oystermen with additional opportunities for getting seed oysters. Thus he says (1937, P. 499): "of practical importance is the very prolific late setting period, which follows the first on the next third and fourth spring tide periods; for oyster growers are able to plant catch at this time, also, therby improving their chance of obtaining a satisfactory catch of seeds". Our study makes this conclusion highly questionable. ~~[illegible]~~

The larger early-caught spat on seasonal test cultch are usually definitely set off from spat from later waves of setting, as one would expect. Hence only the larger seed oysters are included in the counts for the seasonal strings given in the bay-year graphs. A review of these seasonal cultch strings for all years in which the information in available confirms the conclusion that the beginning of the first wave of spatfall is the optimum time for cultching. Hence reference to the bay-year graphs shows the following:

SEASON OF 1944

OYSTER BAY	Best catch on cultch put out half-way toward first setting peak; seasonal catch drops off soon after setting peak.
MUD BAY	Poor set. Best catch on shell put out at beginning of set, decreasing gradually thereafter.
NORTH BAY	Best catch from beginning significant set to first setting peak, decreasing rapidly thereafter.
SOUTH BAY	Poor catch on shell put out one week before beginning set; maximum catch on cultch put out at beginning of set, decreasing gradually thereafter.
OAKLAND BAY	Very long, flat setting curve; best set on cultch put out after Setting Index over 50, decreasing rapidly thereafter.

1945

OYSTER BAY	Best catch on shell put out at beginning of set, decreasing thereafter.
MUD BAY	Best catch on cultch put out at first sign of set, decreasing to low at first setting peak.
NORTH BAY	Precipitious setting peak; best catch at beginning set and first setting peak.
SOUTH BAY	Best catch on shell put out at first sign of set, decreasing rapidly thereafter.
OAKLAND BAY	Poor set; best catch on shell laid out just before setting peak.

1946

OYSTER BAY	Best catch at beginning set, decreasing gradually to time of first setting peak and falling off rapidly thereafter.
MUD BAY	Poor set; best catch on shell put out one week before beginning set.
NORTH BAY	Best set on cultch put out at beginning set, decreasing rapidly after first setting peak.

<u>SOUTH BAY</u> Poor set; best catch on cultch put out a week before set begins, decreasing gradually thereafter.

1948

<u>MUD BAY</u> Best catch on shell put out at beginning set, (S.I. = 500); only half as much caught on shell placed out 12 days earlier.

<u>SOUTH BAY</u> (Data inadequate, but indicate major spatfall occured after our records ceased.)

1950

<u>OYSTER BAY</u> Best catch at just before first setting peak, falling off very rapidly thereafter; poor catch on cultch put out 6 days before setting peak.

<u>MUD BAY</u> Best catch on cultch placed at beginning of set, decreasing rapidly from first setting peak on; cultch placed 7 days before beginning significant spatfall caught only 65% of best catch.

<u>NORTH BAY</u> Best catch on cultch placed out at beginning set, decreasing gradually to setting peak and falling off very rapidly thereafter.

<u>SOUTH BAY</u> (Spatfall data inadequate.)

From this survey we can conclude that in all years the optimum time for cultching is not at the crest of a wave of setting but before, at the beginning of significant spatfall which is rising towards the <u>first</u> setting peak. Hence maximum surviving catch is assured by cultching at the time determined by the prediction method herein developed which establishes the date when the spatfall may be expected to be rising to its initial, early-summer peak.

HOW TIME OF OYSTER SETS CAN BE PREDICTED*

One of the most interesting and valuable results of these investigations has been the revelation that by following the air temperatures during the first months of the year it is possible by the end of April to foretell the date in June or July on which cultch should be in place for maximum catch of oyster seed. How this method of set-predictions was developed will now be discussed in detail.

We begin with the fact that the rate of chemical reations and therefore of biological processes is greatly influenced by temperature, usually rising rapidly with and in direct relation to increasing temperatures. This means that at warmer water temperatures the oysters should spawn earlier in the year and the period of pelagic larval life be run through in briefer course, while at lower temperature the whole reproductive cycle will be correspondingly retarded. The clue to timing the set must therefore lie in determining the quantitative relationships between temperature and the rate of the aggregate of biological processes which result in setting larvae. Since oysters are cold-blooded animals, the body temperature at all stages in their life-history is that of the surrounding medium and the rate of their internal processes is determined accordingly.

To discover a relationship between the last stage of the reproductive cycle (the beginning of spatfall) and temperature we need to know precisely the dates at which setting began in the various bays over several

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*This method of predicting sets was announced in the Puget Sound Oyster Bulletin of May 24, 1951 and its anticipations for that year were fully confirmed.

years as well as the effective water temperatures experienced by the spawning oyster ~~and the larvae~~ during those years. Our studies with test cultch put out and examined twice weekly have given us the former information, but we lack complete data on water temperatures. This shortcoming however turns out to have been an advantage in the end since, having to rely on air temperature reports of the U. S. Weather Bureau for certain years, it was found that they were adequate to our needs and so there was opened the possibility of circumventing the tedious and expensive gathering of water temperature data.

Our next step is therefore to discuss the relationship between air temperature and water temperature in the oyster bays. For a year and a half thermographs were kept operating in Oyster Bay, North Bay and Oakland Bay with the sensitive bulb at the level of the oysters themselves thereby giving continuous records of the temperatures experienced by spawning oyster stock in the dikes. In addition, a considerable series of determinations of water temperatures at various depths was made by boat trips to all the bays. Data on dike and open-water temperatures for several years and bays are also available in Hopkins' report (1937).

When the water temperature data which we have is compared with monthly air temperature at Olympia or at Grapeview, Washington, the outstanding fact emerges that average water and air temperatures run very close together throughout most of the year. This relationship is shown graphically in Figures 57,58 and 59 .

Granting the close correspondence between water temperatures and air temperatures, we now turn our attention to the differences. For the years figured, in which we have adequate records, the major differences appear to be as follows:

1) Average dike water temperatures follow along generally a degree or two above average air temperatures during spring and fall.

2) Open water temperatures are 4 to 5 degrees cooler than dike water temperatures in the mid-summer since they do not reflect the effect of heating confined water during exposure by low daylight tides as do the dike water temperatures, (Fig. 57).

(INSERT FIGS. 57
58
59
59a)

3) During the cold winter months (December through February) average water temperatures are considerably higher than air temperatures. This is doubtless explained by the high specific heat of water which acts as a brake against extremes of temperature.

4) For the same reason, open water temperatures are somewhat lower than air temperatures during the warmest months of the year. (Fig. 57)

5) Warm or cold early spring air temperatures are directly reflected in correspondingly warm or ~~[illegible]~~ cooler water temperatures (Figs. 58 / 59 & 59a).

The conclusion ~~that we draw~~ drawn from the relation of air to water temperatures is simple: namely, that since water temperatures follow air temperatures the latter may in themselves give us all we need for the practical purpose of predicting the proper time to put out cultch for the greatest effective seasonal catch.

The next question is, How many months of early spring air temperatures shall we take into consideration as relevant to guiding the prediction of set? Here we are guided by three considerations, the first of which is that we must not have to rely on air temperature data of the later months of May, June and July if we want to be able to predict spatting-time well in advance. The second is that we shall use the minimum number of months record which gives us what we need; and the third is that we can expect that gonadal ripening requires several months to bring the sex products to fruition, probably beginning in January if water temperatures are sufficiently above freezing. Although we have yet no study of gonadal ripening in relation to temperature in any oysters with which to check this supposition, we do know that winter oysters put immediately into warm-water aquaria require a whole month at early summer temperature before they begin to spawn, and it is therefore reasonable to suppose that in the bays gametogenesis extends from a stand-still in our coldest month January through February and March and into April.

Now the effective temperature is the measured temperature multiplied by the number of days it acts upon the oysters (time-caloric factor). This means that we cannot jump from January temperatures to April temperatures and take the average as acting over the entire period. Instead we should break down the period into successive increments of temperature multiplied by the number of days during which it acted on the oysters. We will thus give due weight to the effective temperature of each successive month by treating monthly averages as ~~x~~ separate factors in obtaining a cumulative characterization of the over-all trend of early spring temperatures.

For this purpose we can choose almost any accurate index of the absolute or of the relative warmth of coolness of any given month. In practice it is simplest to use the deviations from normal of the average monthly air temperatures as calculated and published by the U. S. Weather Bureau. Records from the weather bureau station at Olympia (Priest Point park, at 69 feet elevation, on Budd Inlet) should be and are the most relevant for events in Oyster Bay, Mud Bay, and South Bay; while the data of Grapeview (20 feet elevation above mean low water on Case Inlet) are most appropriate for corresponding events in North Bay (see Fig. 1 for location of these stations).

To obtain an index of the cumulative trend of early spring air temperatures we therefore take the algebraic sum of the deviations from normal of average air temperatures for January, February, March and April. Thus for 1949 Olympia station air temperatures we have -9.4, -4.0, +0.1, +1.3 as the deviation values. Adding the negative and subtracting the positives, we obtain an index, which we shall call the early spring Thermal Trend, of -12.0. This procedure is followed for all the years with which we are concerned, using the Weather Bureau data reproduced here in Tables 56 & 57, Pp. 183,184/

Next we turn to our graphs and determine the number of days after April 30th at which setting begins. On good setting years this is approximately the date on which a setting index of 500 is first achieved on an increasing spatfall, but in bays with low setting rates or off years we note our maximal seasonal string catches and measure the period from April 30th to that time at which cultch should have been put out to obtain the maximum surviving set. The two sets of figures are given in tabular form below (Table 58 & 58a)

(INSERT Table 58)
58a)

In order to test whether there is a reliable relationship between air temperatures and time of beginning set we plot Thermal Trend indices against hastening or delay of setting as measured by the time between the end of April and initial significant spatfall. (Figs. 39 - 42). It will be noted at once that the points fall in line in a very beautiful manner indeed. A "best line" can be drawn "through" the points on the graphs and it is a straight line. The mathematical significance of our being able to draw such a line is that a simple and regular relationship is shown to exist between early spring air temperatures and the time of oyster setting. Having the lines, we can note slope and Y-intercept and write the equations of the lines. We also remark the scatter of the actual points with reference to the "ideal" line and understand the variation

TABLE 58 : TIME OF BEGINNING SPATFALL IN RELATION TO EARLY SPRING TEMPERATURES

YEAR	THERMAL TREND*	NUMBER OF DAYS AFTER APRIL 30th ON WHICH FIRST WAVE OF SPATFALL BEGAN		
		OYSTER BAY	MUD BAY	SOUTH BAY
1931	+7.2°F	51	54	
1932	-2.5	55		
1933	-9.1	64	86?	
1934	+17.8	37	41	
1935	-5.8	59	69	
1936	-6.1	66		
1937	-11.4	65		
1938	+5.5	55		
1939	-2.5	60		
1940	+14.1	38		
1941	+20.1			
1942	+1.6	50	45	
1943	-1.4	45		
1944	+2.4	55	55	68
1945	+1.3	52	63	58
1946	+5.2	49	63?	56
1947	+6.1	48	50	
1948	-6.7	62	73	73
1949	-12.0	71	71	
1950	-19.4	73	80	84

* Summated deviations from normal air termperatures, January through April, at Priest Point Park, Olympia,

TABLE 58a: TIME OF BEGINNING SPATFALL IN NORTH BAY IN RELATION TO EARLY SPRING TEMPERATURES

YEAR	THERMAL TREND*	NUMBER OF DAYS AFTER APRIL 30th ON WHICH FIRST WAVE OF SPATFALL BEGAN
1944	+2.6°F	58
1945	+2.1	58
1946	+1.9	53
1947	+6.8	46
1948	-3.1	60
1949	-8.7	66
1950	-15.9	71

*Summated deviations from normal air temperatures, January through April at Grapeview, Wn.

and error involved in each case.

Proceeding in this manner we find the following formulae for predicting the proper time of cultching, on the basis of early spring temperatures, where------

D is the number of days after April 30th that cultch should be in the water to gather maximum set, and

X is the Thermal Trend index or the algebraic sum of the deviations from normal of monthly average air temperatures, January through April, using Grapeview station for North Bay and Olympia (Priest Point Park) for all other bays.

Oyster Bay:

$D = 1.04\ (53.5 - X)$ give the proper date to ± 3 days.

Mud Bay:

$D = 1.16\ (53 - X)$ gives the date to ± 4 days.

North Bay:

$D = 1.1\ (52 - X)$ gives the date to ± 4 days (Grapeview temperature data).

South Bay

$D = 0.97\ (67 - X)$ gives the date to ± 5 1/2 days.

Oakland Bay is omitted in our consideration because Olympia oyster production is now negligible in this area; but it is clear that if conditions returned to favorable and oystering were successfully resumed, a similar formula could be worked out for this bay.

Thus the relation we sought between early spring air temperatures and time of setting has been discovered and is expressed quantitatively and mathematically in the formulae given. The significance of these equations is that all that oystermen need in timing their cultching operations is the formula for their bay and the temperature data which are already carefully and systematically gathered for ~~them~~ all citizens by the U. S. Weather Bureau and its associates. At the end of April the Thermal Trend for the year so far can be calculated, substituted in the equation, and the probable date of beginning set determined.

Thus in Oyster Bay for example, where the error is + 3 days, the highest probability is that the set will begin on the date predicted, somewhat less probable that the actual set will begin one day earlier or one day later, still less that it will commence two days earlier or two days later, and so on to the situation that it is highly improbable, on the basis of known previous seasons, that spatfall will begin four days earlier or four days later than the date predicted. Consider then the extremes of possible error, plus or minus three days. If cultch is put out three days too early it will still not have time to foul before rising spatfall begins. If on the other hand, the actual setting begins three days before the predicted date for cultching, the first peak of setting will still be "hit" since the prediction date is for the *beginning* of the initial wave of spatfall. Hence even on the outer limits of error of the method the predictions will assure maximum catches.

At this point it is well to remark that these formulae for timing cultch are based solely on the experience of the years 1944 through 1950. *Had they been available during this period, timing of cultching operations could have been successfully made for every one of these years on the basis of these equations alone.* Our reason for believing that they will prove to be accurate in future years is that the period on which they are based embraces near-average as well as the extremes exemplified by the extraordinarily warm spring of 1947 when spawning occurred at the end of April and by the record cold spring of 1950. The record of years to come may however serve to refine the formulae by modifying their constants somewhat. Also as the "normal temperature" is recalculated from decade to decade as Weather Bureau data accumulates it may be necessary to change the constants slightly. As they stand the equations are adequate to current practical needs.

Since we cannot foresee the future, one may test the formulae by applying them to the data of earlier years which did not fall under our study.

Hopkins (1937) chronicled the spatfall for several years in Oyster Bay, and Mud Bay, and one of the oystermen, Mr. W. J. Waldrip, has put at our disposal careful records of set on test shell in Oyster Bay for another period of years. Therefore we may calculate from the formulae at what time cultch should have been put out during the years in question and then turn to the actual setting record to see whether this timing would have proved to be the most propitious.

Let us begin by considering the season of 1934 in Oyster Bay and Mud Bay. This was an extraordinarily warm spring. Weather Bureau reports on file enable us to calculate the Spring Thermal Trend Index of +17.8, far above the value for the warmest spring of our own records (1947, + 6.1). Applying the formulae for Oyster Bay and Mud Bay we find that cultch should have been out on June 6 and June 10 respectively, according to our calculations. Now we turn to Hopkins' paper (1937, Fig. 26, and Table 25, PP 482, 483) and note that on June 6, 1934, in Oyster Bay the spatfall was just beginning at about 500 spat per bag of cultch shells per day, attaining a peak of 6761 spat 6 days later. And on June 10 in Mud Bay (Hopkins, 1937, Fig. 31, Table 30, PP 485, 486), spatfall was also just beginning at 100 spat per bag of shells per day, rising to a peak of 305 spat 5 days later. In short, had our formulae been available in 1934 they would have set the date precisely for the very best time for cultching which is just before the first setting peak.

The season of 1935 on the contrary was unusually cold, having a spring Thermal Trend of - 5.8. Calculating the timing of cultch as before, we find predicted dates to be July 1st and July 8th for Oyster Bay and Mud Bay respectively. Refering now to Hopkins' data (1937, Fig. 33, Table 33, PP 488 and 489; Fig. 32, Table 31, P. 486) we find that July 1 marked the crest of the first set in Oyster Bay, while on July 8th in Mud Bay the set was well started and rising to a peak 8 days later. Hence if the dates for cultching had been followed, an ample set could have been obtained in both bays, within the limits of (1) the possible destructive effects of pulp mill liquor and (2) the low spatting rate achieved in Mud Bay during that year.

Turning now to Waldrip's records (Tables 26 - 32, Pp. 155-161) we may select for checking our Oyster Bay formula the year 1941 when the spring Thermal Trend was + 20.1, the highest of all years spatfall of record. This figure gives us by calculation June 4th as the proper time for cultching. Now on June 4th, 1941 no spatfall was occuring at Burns Point or on Waldrip's home dike in Oyster Bay. First weak spatting was picked up on June 23rd, dribbling along until a low setting peak of Setting Index = 580 on July 20th. The formula appears to have failed in this case, but it is more reasonable to believe that it was the spatfall that did so instead; for there is little doubt that the first set of this phenomenally warm season should have come long before July 20th. Spatting climax on July 20th then very likely corresponded to the usual second wave of setting. Since the 1941 season was at the height of a "depression" due to pulp mill pollution we may surmise that this factor caused the failure of the first setting peak.

The 1936 season was interesting in that February was an unusually cold month having an average air temperature of 7.4°F below even that of the preceeding month of January. The corresponding deviation from normal February temperatures was - 7.1°F, the lowest for any February during the years of available spatting records. Does the formula prove equal to this abnormal circumstance? Employing the equation for Oyster Bay we calculate the proper cultching date for 1936 to be July 1st or 62 days after April 30th. On that date Waldrip's records (Table 26) show that spatfall had recently begun and was at a rate of 100 spat per 100 shells per day

(S. I. = 100), gradually increasing to a peak of 4000 twelve days later. Cultching on July 1st would therefore have yielded the maximum seasonal catch!

Finally we can check our formulae, against all the years of Hopkins' and Waldrip's records by plotting the actual dates of beginning spatfall against the Thermal Trend of these years. This is done in Figs. 60 and 61 from the data of Table 58 .

(INSERT Fig. 60
Fig. 61)

Examination of Figs 60 and 61 shows excellent agreement when our formulae are used to "predict" the beginning set during the years 1931 to 1940, for the points ~~fax~~ all fall close to the identical line drawn for the 1944 to 1950 data. Time for beginning spatfall in Mud Bay, 1933, alone appears to be far out of line. Turning to Hopkins' Table 29, footnote 1 (1937, P. 485) however, we see that this author supposed that his test cultch placed out on July 18th of that year did not begin catching spats until the last day it was out (July 25th) whereupon it suddenly picked up a catch of 1494 spat! This assumption seems rather dubious and we may reasonably expect that, as in other years generally, the rate of spatfall began at a slower pace and probably actually commenced nearer the 11th of July than the 25th.

The formulae for timing cultch therefore pass the crucial test of applicability to fresh data which they were not originally designed to explain. We should hence expect that future setting seasons to be confirmatory and have good reason to employ the formulae with confidence.

The method here proposed for long range prediction of the date on which spatfall may be expected to begin is unique. In the case of the European flat oyster, O. edulis, only short-term anticipations have been possible, in part owning to the fact that chance cooling of the waters may on occassion largely destroy a once-promising abundance of oyster larvae. The same is true of the Japanese or Pacific oyster; both in Japan and on our Pacific coast sets are predicted only on short notice largely from the character of the larvae picture. Ostrea virginica, the Eastern oyster has a rather sharp threshold temperature for spawning and these oysters also mutually stimulate each other to spawning via their sex products with the result that spawning occurs simultaneously and a population of larvae all of the same age is developed. Knowing the average duration of larval life one can in some areas therefore predict from the spawning date or from the date on which threshold temperatures is reached approximately when setting should occur.

Hopkins also worked out a method for forecasting the date of beginning spatfall for the Olympia oyster from the date when gravid oysters are first found. His rule was that "Setting of larvae begins in the third tidal period following that during which spawning starts". A "tidal period" was taken to be a period of low low-tides. When this rule is applied to our own bay-year graphs (in which the low low-tides are indicated on the base-line) it is found that for those instances in which the data are sufficiently complete to permit a clear-cut decision (24 bay-years) the rule holds good 63% of the time, whether one considers all the bays of our study or only those which Hopkins studied (Oyster Bay and Mud Bay, plus Little Skookum and Oakland Bay for one season). Probably Hopkins did not strive after a more accurate or a longer-range prediction method since he considered, as already noted, that the second wave of spatfall could profitably be cultched. The use of seasonal cultch has however shown that the ~~[illegible]~~ later-caught spat do not contribute importantly to the recruitment of seed oysters because of the high mortality rate to which they are subject(see pp. 87 - 92).

The dependability of the early summer weather in lower Puget Sound and the adaptation of the oyster larvae to such variations as obtain, together with discovery of the quantitative relationship between air temperature and the tempo of the reproductive cycle of the Olympia oyster has therefore made possible a method for timing beginning spatfall which for ease of determination, accuracy of forecast, and extent of anticipation is without parallel in the prediction of oyster sets.

HOW BEGINNING SPAWNING IS PREDICTED

The method for predicting time of spatfall described above does not even require our determining when the oysters begin to spawn; yet it may conceivably be of practical value for marketing purposes to be able to forecast when oysters will become spawny. To do so we procede in a manner similar to that of predicting the time of beginning set. But in this case we use Grapeview air temperature records for all bays and we omit, in calculating the cumulative deviations from normal during January through April, certain extremely low or unusually high deviations. Whether this procedure be too arbitrary will be discussed in a moment, but first we will show that it does yield workable relationships within the years of record at our disposal.

Thus when the number of days from April 30th to the beginning of significant spawning (5% gravid oysters in our samples) for each bay-year is plotted against the Thermal Trend we obtain the correlations shown in Figs. 62 through 65. Drawning the best straight line through the points of each graph and determining the equations for these lines, we achieve the following formulae, in which

Dsp = number days after April 30th that first significant spawning begins, and

X = the algebraic sum of the deviations from normal of average mean air temperatures at Grapeview for January through April with monthly deviation values of - 4 and less and + 5 and greater omitted from the calculations. (See Table 57, P. 184).

Oyster Bay:

$Dsp = 3.4 (X - 4.8)$ gives date of beginning of significant spawning ± 7 days.

Mud Bay:

$Dsp = -2.63 (X - 5.8)$ gives date ± 7 days.

North Bay:

$Dsp = -2.63\ (X - 7.0)$ gives date ± 4 days

South Bay:

$Dsp = -3.3\ (X - 8.5)$ gives date, of accuracy undetermined because of insufficient years of data.

The data of Hopkins' years of observation are added to the graphs of Oyster Bay and Mud Bay and they agree reasonably well with the trend of the later years under our own surveillance.

Of course if "Dsp" is negative, on the basis of temperature records for January 1 through April 30th, spawning will have already commenced in April. Hence for unusually warm years we need a foretelling from March 31st. On this score we can say that if the index of the Thermal Trend for January through March is + 4 or greater, spawning may be expected to begin during April in most bays. For such unusually warm years one can look for spawning sometime after the middle of April and before the first of May.

In order to obtain the measure of correlation between air temperature and time of spawning shown in the graphs we have had simply to omit the excessive deviations of February (-4.9) for 1933, of April (+5.4) for 1934, of March (-4.2) for 1935, of January (-7.1) for 1949 and January (-10.3) for 1950 when calculating the Thermal Trend for these years. I can hear my scientific friends screaming in horror! How arbitrary! What a ruthless and biased manipulation of the data! But note what has been gained thereby: we have formulae which, had they been available in 1932-1935 and 1944-1950, would have told us the probable date of beginning spawning using only the Grapeview air temperature records for January through March and April. And since these equations were applicable in those disparate years, when spawning was as much as a month earlier in some years than in others, we have good reason to hope that they will hold also for future years.

Now to reply to possible objections to our rather arbitrary handling of the temperature data. In the first place, it is obviously the water temperature and not the air temperatures that affect the oysters directly and determine the rate of the reproductive processes. *If* we had representative annual water temperature records for all our bays over a *considerable* number of years one should, I am sure, be able to make very neat correlations between these temperatures and the rate of progression of the reproductive cycle. And *after* one had thus kept weekly thermograph records in 4 bays for a dozen years he should also be able to work out a more accurate mathematical relation between air temperatures and water temperatures characteristic for each bay whereby he could then dispense with reading further water temperatures and obtain close predictions by following air temperatures alone. But we simply do not have this data on water temperatures over a long period of years so we have to do the best we can with the available air temperature data. Fortunately, it turns out that air temperature records are adequate for the practical objective.

In handling this problem we try to use to full advantage what air vs. water temperature records we have. These are shown in Figs. 57 through 59 (PP.94 a-c) which should be consulted in connection with the following remarks.

We are justified in omitting the extreme low deviations in air temperature when they occur during the beginning months of the year because water temperatures do not "follow them down" but remain much higher, due to the high specific heat or "thermal conservatism" of water.

We are similarly justified in omitting positive deviations of +5 or higher from normal air temperature either for the same reason of thermal lag or because generation of the spawn requires a certain minimum period of time and probably cannot in nature be hastened further by increases in temperature above certain values.

Finally, ~~we~~ one may ask, Why switch to Grapeview temperature data when the unmanipulated Olympia records gave such good correlations for timing of initial spatfall? The answer is simply that we use them because they work better. How can that be? Quite possibly because the recording station at Grapeview is at 20 ft. above mean low water while that at Olympia ~~it~~ is 69ft. Also, Grapeview is surrounded by the waters of Case Inlet. The result is that the air mass thermally tested at Grapeview is more moderated by water temperatures than at Olympia, January and February normal temperatures being somewhat higher at the former station. Grapeview air temperatures therefore correspond more closely during these months to water temperatures in the region generally and therefore probably reflect more closely the effective temperatures involved in initiating the production of spawn. From the fact that such temperature data may be used satisfactorily in this manner for practical results we should expect that a comprehensive study of water temperatures in the bays themselves would show that excluding the excessive variations in plus or minus direction give the best reflection of *water* temperatures during the development of spawn.

It was stated by Korringa in 1940 that "No investigator in Europe has succeeded so far in deducing a reliable mathematical formula, exclusively built up of easily observable factors, such as water temperatures, for the purpose of forecasting swarming [liberation of spawn]. Such a formula would render the time-consuming plankton-investigations superfluous". The procedure just detailed allows us to predict from data much easier to obtain than water temperatures, namely, air temperatures alone, not only the probable date of spawning but also the date of beginning spatfall.

The derivations given for the empirical formulae by which beginning spawning time is determined may seem in certain of their steps to be quite arbitrary and the predicted dates have an accuracy of only plus or minus 7 days in certain bays. All that need be said on this score is that if one should desire a more precise forecast, such could undoubtedly be obtained by more extensive spawning surveys of the bays (not just at one location in each bay) together with an investigation of actual over-all water temperatures. A direct study of the water temperatures themselves would eliminate the need for manipulating air temperatures but it would prove a very tedious study indeed. The even more successful prediction of time of beginning spatfall renders such increased accuracy of spawning prediction unnecessary in relation to the problem of cultching.

The formulae herein offered will, however, allow us to predict within a period of not greater than two weeks when spawning will begin in the various ~~[illegible]~~ oystering bays, and for most years and bays the foretelling may be expected to be a good deal closer than this. ~~The~~ This prediction may be of considerable value in anticipating at what time each year marketable oysters will become"spawny".

The circumstance that each bay has its own characteristic time of spawning and of setting in relation to air temperatures is explainable in terms of the differences from bay to bay in all those topographical features which contribute to the rate of seasonal change in water temperature. Hence, in particular, the less the volume of a bay and the closer its mouth to the main tidal channels from central Puget Sound the greater will be its tidal flushing and the tempering of its thermal change by "outside" waters.

One can get a rough idea of the volume of the bays by integrating (i.e. adding up) all the rather evenly distributed soundings figures provided for each bay in the U. S. Coast and Geodetic Survey chart of the region. The values so calculated, which represent the relative volumes of the oystering bays, are as follows:

~~[illegible]~~